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# Family dynamics through childhood: A sibling model of behavior problems☆

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**Abstract**

This article draws upon theories of the life course and child development to examine how structural changes in the family and parenting practices affect child behavior problems in middle childhood. Our analysis improves upon prior research by simultaneously examining the effects of poverty, single-motherhood, welfare, and kin co-residence, distinguishing between early and current exposure to changes of these family conditions, and controlling for unobserved, preexisting family differences. We estimate fixed-effects sibling models using the matched mother–child data of NLSY79. We find two robust relationships: child behavior problems are shaped by early childhood poverty, which is not mediated by current parenting nor contaminated by family selection, and mothers' use of physical punishment, which is not contaminated by family selection. The findings support the early childhood exposure hypothesis applied to poverty, a parenting hypothesis applied to mother's physical punishment, and a family selection hypothesis applied to positive parenting, father's time, and cultural activities.


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## 1. Introduction

Sociologists have become increasingly concerned with the ways in which families shape children's development and overall well-being. This growth of interest has been fueled by rapid transformations in family structure, deepening poverty among children, and the deteriorating well-being of disadvantaged children. Children's behavior problems in middle childhood may be implicated in later adult outcomes. For example, life course research finds that child temper tantrums are associated with problems in adulthood, such as downward occupational mobility, erratic work lives, and divorce and separation (Caspi et al., 1987).

An important theme in life course and child development research focuses on the timing of changes in the family through the lives of children. This literature has examined how the effects of structural changes in the family, such as divorce or descent into poverty, depend on the developmental stage of the child. For example, some studies find that children's well-being is reduced by early childhood poverty, but not early childhood divorce (McLeod and Shanahan, 1993; Wu and Martinson, 1993). Others caution that the negative effects of family change could be due to pre-existing factors that select for family change (e.g., Cherlin et al., 1998; McLanahan and Bumpass, 1988).

This paper draws on life course and child development literatures to examine child behavior problems in middle childhood. First, we examine the hypothesis of family selection in which observed associations between family change and child outcomes are spurious due to unobserved pre-existing conditions in the family. We use fixed-effects sibling models to control for family selection. Second, we model the dynamics of structural changes in the family and test three hypotheses: Are child behavior problems determined by early childhood exposure to family change, current exposure, or family instability? Unlike most previous research, which typically examines only a single dimension of the family conditions, we examine four dimensions, including single-motherhood, grandparent co-residence, welfare dependency, and poverty. Third, we examine the effects of current parenting practices on child behavior problems and test the extent to which parenting mediates the effects of changes in family structure.

## 2. Family selection

The effects of the timing and changes in family structure on child outcomes can be due to family selectivity—that is, preexisting differences between families that experience the structural change and those that do not (McLanahan and Bumpass, 1988). Our family selection hypothesis specifies that the association between family structure and child outcomes is spurious due to stable preexisting differences between families that affect child outcomes and are correlated with family structure. These preexisting differences can include marital discord, parent's personality, neighborhood differences, genetic predispositions, and the like—in short, virtually anything that varies across families and affects both family structural change and child

Table 1
Hypothesized effects of family dynamics and parenting on child behavior problems

| Hypothesis | Explanatory concept | Effect |
|---|---|---|
| Early childhood exposure | Years of exposure in early childhood | Continuity |
| Current childhood exposure | Current state | Contemporaneous |
| Family instability | Spell of current state | Cumulative |
| Parenting | Positive vs. punitive parenting | Mediating |
| Family selection | Unobserved family heterogeneity | Renders other effects spurious |

outcomes. For example, parents' personalities, temperament, and competence can contribute to divorce, poverty, and welfare dependency, as well as child behavior problems. Research suggests that preexisting differences help to account for the association between divorce and child behavior problems. Twin studies suggest that genetic predispositions affect a person's likelihood of maintaining a lifelong marriage (McGue and Lykken, 1992), and that genetics are involved in child behavior problems (Scarr, 1992).

Because research on effects of family dynamics on child outcomes uses non-experimental designs, we cannot rule out the possibility of family selection (family preconditions and shared genetic tendencies) affect child behavior problems (McLanahan and Bumpass, 1988). Recent multivariate research addresses the problem of family selection by controlling for *observable* family variables, such as demographic and parental characteristics, when estimating the effects of family change on children's outcomes. But given the limits of measured variables available to researchers, it is likely that family selection operates on *unobserved*, unmeasured family factors. Indeed recent research has gone beyond this to try to control for unmeasured family effects using methods such as sibling models or instrumental variables models (e.g., Duncan et al., 1998; Guo and Van Wey, 1999; Mayer, 1997). We adopt a fixed-effects sibling model, which controls for all unobserved preexisting family differences that are invariant with respect to time, and compare it to a random-effects model, which assumes no bias due to family selection. Comparing the results of a fixed-effects model and a random-effects model, we test the hypothesis that family selection will render the effects of family dynamics and parenting practice spurious (see Table 1).

## 3. Family dynamics

Life course perspectives emphasize that lives are interdependent and linked through time, and focus attention on the timing and sequencing of events such as divorce, moving in with grandparents, welfare, and poverty (Elder, 1985). Recent research has examined how child outcomes are affected by such events considered separately. Research on poverty distinguishes permanent, chronic, persistent, recurrent, occasional, and transient forms of poverty (Ashworth et al., 1994), finding that early childhood exposure to poverty impedes child development (Duncan et al., 1994,

1998; Pagani et al., 1997), exposure to poverty in middle childhood is less important than early childhood (e.g., Duncan and Brooks-Gunn, 1997), and that current poverty affects behavior problems indirectly through parenting (McLeod and Shanahan, 1993). Research on effects of welfare participation finds modest negative effects of AFDC among non-working mothers on parenting (Kalil and Eccles, 1998) and on child outcomes (Smith et al., 2000). Levine and Zimmerman (2000), however, find that significant correlations between AFDC and child problems were spurious when unobserved characteristics of the mother are controlled.

A large research body examines the timing of changes in family structure. Amato and Booth (1997) found that divorces occurring early in the child's life exerted stronger effects on child well-being in families experiencing marital discord; this effect operated indirectly through the lessening of parental support. Wu and Martinson (1993), however, found little effect of early childhood exposure to non-intact families on teenage pregnancy. Research on current single-mother families is inconsistent: some researchers find negative effects of current single-mother families (Hanson et al., 1997; Lindner et al., 1992), while others do not (Wu and Martinson, 1993). Research on the role of grandparents finds positive effects on cognitive and emotional development of preschoolers (Baydar and Brooks-Gunn, 1991) and negative effects on school performance of school-age children (Furstenberg et al., 1987).

Wu and Martinson (1993), in particular, have lent some conceptual clarity to this literature by distinguishing the effects of early versus current family structure, and stable versus unstable family structure. We extend their approach in three ways. First, we examine the timing and duration of four dimensions of family conditions—family structure, grandparent co-residence, welfare recipiency, and poverty. Second, we examine a *family selection* hypothesis, in which preexisting stable differences across families select for family conditions and also affect child behavior problems, thereby rendering the effects of family conditions spurious (McLanahan and Bumpass, 1988). Third, we consider the potential mediating effects of parenting practices.

We hypothesize that *early* childhood exposure to hardship affects behavior problems in middle childhood, even net of later exposure. In the transition from infancy to early childhood parents shift from a caretaker role to a role of socializing children. At this crucial period, prolonged exposure to hardships, such as divorce, welfare dependence, and poverty can undermine parental socialization, whereas the presence of grandparents can improve matters (Duncan and Brooks-Gunn, 1997). Such effects could operate indirectly on later behavior problems through one of two mechanisms. First, early child exposure could lead to behavior problems that persist into middle childhood by selecting negative environments that sustain maladaptive behaviors (Caspi et al., 1987). Second, early childhood exposure to hardship can undermine later parenting practices, which affects behavior problems in middle-childhood, even net of prior behavior.

We hypothesize that children's *current* exposure to disadvantaged family conditions affect child behavior problems, net of early child exposure. During middle childhood—between the ages of 6 and 14—effective parenting moves beyond direct reinforcement and modeling to the use of inductive reasoning, social approval, and monitoring to control behavior. The literature has documented the negative

association between current disadvantaged family conditions and children's social development (e.g., Matsueda and Heimer, 1987; Sampson and Laub, 1993). Absence of the father in single-mother families, limited resources due to poverty, and stigma attached to welfare dependency make it difficult to supervise and control children, increase current stress, and undermine current parenting. Conversely, the presence of extended kin may offset some of these deficits, increasing supervision, reducing stress, and easing parenting. We examine whether contemporary family conditions affect child behavior problems directly or indirectly through parenting practices, such as using physical punishment, spending time with children, and using positive inductive reasoning.

Finally, we hypothesize that longer durations of exposure to current disadvantaged family conditions can have a particularly negative effect on children's outcomes (Wu and Martinson, 1993). In his work on children of the great depression, Elder argues that the event of a change in family circumstances challenges families, disrupting old habits and routines, producing disequilibrium in the family system, and creating stress on parents and children. The effects of a family change may vary by the amount of time passed since the change (Wu and Martinson, 1993). Most families will suffer in the short run, as they are overwhelmed by new sources of stress. Over time, however, some families learn to adapt to changing circumstances by developing new routines, changing expectations, and finding new sources of self-esteem (Elder, 1974). This suggests that longer durations since family change could lead to smoother family functioning and greater child well-being. In contrast, mental health researchers studying the negative effects of stress have emphasized chronic stress as the major problem. Pearlin (1989) argues that changing life events do not necessarily produce stress unless they are undesired or non-normative; enduring chronic strains, such as living in poverty, are key producers of stress; and life events and chronic strains act jointly. This line of theorizing suggests that long durations in an undesired status will produce chronic stress, weaken family functioning, and increase child behavior problems. We will test for both mechanisms and test whether chronic strains affect child outcomes indirectly by altering parenting practices.

## 4. Parenting practices

A long history of research has emphasized the importance of parenting for child development. Research has identified two important dimensions of parenting: (1) positive parenting, including warmth, affection, and praise; and (2) parental control, including rule formation, discipline, and punishment (e.g., Amato and Booth, 1997; Baumrind, 1978). We hypothesize that positive parenting—praising a child, showing physical affection, and saying positive things—will reduce problem behaviors by teaching children the boundaries of behavior in a non-threatening way (Baumrind, 1978; Patterson et al., 1992). In contrast, the use of physical punishment may increase aggression by legitimizing the use of violent and aggressive behavior to solve problems (Larzelere, 1986; McLeod and Shanahan, 1993; Olweus, 1980; Straus, 1991).

Research has found that fathers' involvement in child rearing has a positive effect on children's well-being (e.g, Lamb, 1987; Snarey, 1993). Therefore, we examine whether the amount of time fathers spend with their children reduces child behavior problems, net of other dimensions of parenting. Furthermore, it may be that the kinds of activities that parents share with their children are important for children's adjustment. Thus, parents who invest in their children's cultural capital, such as taking them to museums, reading with them, and encouraging them to read at home will increase their children's prosocial behaviors and decrease their antisocial behaviors.

We examine the effects of parenting on child behavior problems, and also evaluate the extent to which parenting mediates the effects of family structural changes. If family conditions, such as poverty or single-motherhood, shape or undermine parenting practices, which in turn affect child outcomes, the effects of family conditions could operate indirectly through parenting. We will also examine whether effects of parenting are rendered spurious due to family selection.

## 5. Data and measures

### *5.1. The national longitudinal survey of youth*

We use data from the 1979 National Longitudinal Survey of Youth (NLSY79). NLSY79 is based on a national probability sample of 12,686 American youths who were aged 14–21 in January 1979, and who have been re-interviewed annually through 1994 and biennially afterward. In 1996, 80% of the female respondents interviewed were mothers of at least one child. From 1986, the NLSY79 has assessed child development of children born to these female respondents. By 1996, six waves of assessments had been completed with 9282 children and 17 waves of interviews had been completed with the 4716 mothers. We take advantage of this survey design to estimate sibling models.

We confine our analysis to children aged 6–14 during 1986–1996, excluding non-school-age children, older teenagers, and children born prior to the beginning of NLSY79. Children are included if they were 6–14 in any of the years from 1986 to 1996. If a child is age eligible in more than 1 year, we randomly select 1 year to be included in the sample so that only one observation per child is included. For example, suppose a child was 6 in 1986, 8 in 1988, 10 in 1990, 12 in 1992, and 14 in 1996, and we randomly chose the year 1988. We use the assessment of behavior problems in 1988 (age 8) as the outcome, and her life course experiences from age 0 to age 8 to construct explanatory variables. We use two samples for analysis. The first sample, called the "complete sample," includes all eligible children with valid behavior problem assessments (5808 children born to 3259 mothers), and is used to estimate random-effects models. The second sample, called the "sibling sample," excludes families with only one child observed, leaving 4354 children born to 1805 mothers. Among the 1805 mothers, 1236 had two children, 431 had three children, and 138 had more than three children. We use the sibling sample to estimate fixed-effects sibling models, which require two or more siblings per family.

### 5.2. Measurement

#### 5.2.1. Child behavior problems

NLSY79 administered to mothers a checklist of their children's behavior problems derived from the Achenbach Behavior Problems Checklist (Achenbach and Edelbrock, 1981). We estimated confirmatory factor models with two latent constructs—internalizing and externalizing symptoms. The specification is derived from Achenbach and Edelbrock's (1981) initial factor analysis, and replications by Zill (1985), Parcel and Menaghan (1988), McLeod and Shanahan (1993), and Lizotte et al. (1992), and is consistent with psychological diagnostic classifications. We create (standardized) composites for internalizing and externalizing, using factor score regression weights from our confirmatory factor models.

#### 5.2.2. At-birth conditions

We measure several variables at the time of birth of a given child. Maternal age at birth of each child differs across (non-twin) siblings. Mother's educational level at birth of each child indicates the human capital available to the family when the child was born. Mothers' smoking and drinking during pregnancy captures prenatal health conditions.

#### 5.2.3. Family dynamics

We measure childhood exposure to changes in family structure, grandparent co-residence, AFDC participation, and poverty. For family structure, we focus on whether a family is intact or not for a given child.[1] We define a family as intact if the child has been living continuously with both biological father and mother since birth. Grandparents' co-residence is defined as the mother and child living in the grandparents' home. AFDC participant refers to mothers receiving AFDC payments in a given year. Poverty refers to family income falling below the poverty line for a given year.

We use three sets of measures to distinguish between early childhood exposure, current exposure, and family instability. We use the example of poverty to illustrate these distinctions. *Early childhood exposure* (0–5 years) measures the total number of years the child spent in poverty during the first 6 years of life. *Current exposure* to poverty is measured with a dummy variable indicating poverty vs. non-poverty in the year of assessment of behavior problems. *Duration of the current spell* in poverty is measured, for those currently impoverished, as the number of consecutive years they have been impoverished. We address missing data on life course experiences by imputing the missing value from an adjacent wave of the same child, and include a dummy variable to adjust for potential non-random missing values.

[1] We also examine a more detailed classification of family structure, distinguishing stepfamilies and cohabitation from single status. Multivariate analyses reveal that neither detailed nor simple family structure explains children's behavior problems. Therefore we use the simple classification in the analysis.

*5.2.4. Parenting practices*

NLSY79 administered a short form of the Home Observation Measurement of the Environment (HOME). We disaggregate the index and examine whether distinct sub-dimensions affect behavior problems distinctly. We specify five major dimensions of parenting. The dimensions are positive parenting by the mother, physical punishment (spanking) by the mother, cultural activities attended by parent and child, father's time spent with the child, and home reading activities. We estimated a confirmatory factor model, tested its goodness of fit, and then computed factor scores for each construct.

Positive parenting is a composite of three items, including the number of times a parent praised a child, showed physical affection, and said positive things in the past week. Physical punishment is the number of times in the past week the mother spanked the child. We top-coded physical punishment at eight times spanked a week to reduce the potential influence of outliers (only 0.6% of children experienced spanking greater than eight times in the past week). Cultural activities measure how often the child was taken to museums and performances in the past year. Father's time spent with the child is based on the frequency that father and child participated in outdoor activities, ate meals together, and otherwise spent time together. Home reading activities include the frequency of mothers' reading to children and children's own reading at home for enjoyment. Both father's time and reading activity refer to general frequency without a specific time frame (e.g., past week or past year).

*5.2.5. Child characteristics*

We characterize children by their sex, birth order, age, current health status, and current sibling situation. Child development research demonstrates that, compared to girls, boys exhibit higher levels of externalizing symptoms and lower levels of internalizing symptoms. Children's current health status is measured by health conditions that limit school attendance, schoolwork, and physical activity, and that require medicine or drugs, special equipment, or attention from a doctor. Research suggests that birth order, number of siblings, and the age of the youngest sibling may induce differential parental treatments to siblings and thus affect children's behavior (Ernst and Angst, 1983).

*5.2.6. Background variables*

We include three background variables shared by siblings of a family, including race, teen-mother status, and mother's cognitive ability. Race matters because black mothers tend to report fewer behavior problems of their children (Hao, 1995). Teenage motherhood is measured by whether the mother gave birth to the first child during her teenage years. This variable takes the same value across siblings within families, whereas the maternal age at birth differs by sibling. Teenage motherhood may indicate a lack of maturity and preparation for motherhood and thus may trigger a cumulative spiral of disadvantages (Furstenberg et al., 1987). Finally, mother's ability is measured by the Armed Forces Qualification Test (AFQT). We choose a subset of tests that are important for women such as word knowledge, paragraph

comprehension, numerical operations, and coding speed. Since the scores are not normed by educational levels, we construct percentile ranks of the selected AFQT scores among all female NLSY79 respondents with the same level of schooling. The percentile ranks should then represent ability net of education effects.

## 6. Model specification

Our modeling strategy proceeds in two steps. First, we examine the effects of family background and childhood experience of structural changes in the family on child behavior problems, testing our early childhood, social control, and family instability hypotheses. Second, we examine the effects of parenting on child behavior problems, controlling for family background and childhood experience of structural changes in the family, testing our parenting hypotheses. To determine whether these relationships are spurious, due to selection on unobserved family effects, we use fixed-effects sibling models to control for pre-existing stable family conditions.

### *6.1. Models of family background and structural changes in the family*

Our models of family background and structural changes in the family focus on the effects of the timing of life course events on child behavior problems. Let $y_{ij}(t)$ be one of the measures (scalars) of behavior problems for child $j$ in family $i$, assessed at time $t$. Because we choose one time per child, $t$ is not a subscript indicating multiple time periods, but simply indicates the time of assessment, which varies across siblings. (There is only one record per child.) $Z_i$ is a vector of background variables, including race, mother's teen-mother status, and mother's ability for family $i$. $X_{ij}(t)$ is a vector of the child's characteristics as controls (sex, age, health conditions, and sibling situation at the time of assessment). The vector $B_{ij}$ denotes at-birth conditions, including the $i$th mother's health, human capital, and age at the time of birth of the $j$th child. The vector $C_{ij}(t)$ denotes variables describing childhood exposure to parents' life course transitions up to time $t$, the year of assessment

$$y_{ij}(t) = \beta_1' Z_i + \beta_2' X_{ij}(t) + \beta_3' B_{ij} + \beta_4' C_{ij}(t) + \alpha_i + u_{ij}(t), \tag{1}$$

where $\alpha_i$ represents the family-specific intercepts, capturing all other *enduring* family characteristics omitted from $Z_i$ that affect child behavior problems, including parents' genes, personality, values, skills, mental health—in short everything that is the same for siblings, varies across families, and does not change over time. If we assume that $\alpha_i$ is a random variable and uncorrelated with $Z_i$, $B_{ij}$, $C_{ij}(t)$, and $X_{ij}(t)$ we have a random-effects (RE) model, which can be estimated by GLS to obtain consistent and asymptotically efficient estimates and unbiased standard errors (e.g., Greene, 2003).[2] The coefficient vector, $\beta_4$, can be used to test the early childhood,

[2] Note that OLS estimates applied to Eq. (1) would provide unbiased but inefficient estimates, as well as biased estimates of standard errors unless there is no between-family effect ($\alpha_i = 0$).

current state, and duration of current state hypotheses under the assumption that mother's selectivity into a given life course event, $C_{ij}(t)$, is not related to unmeasured family effects, $\alpha_i$. Instead, the model assumes that such selectivity is captured by our stable covariates, $Z_i$, and our other time-varying covariates, $C_{ik}(t)$ $(k \neq j)$, and $B_{ij}$. In other words, the random-effects model cannot control for possible spuriousness due to $\alpha_i$. Recall that for a confounding variable (e.g., $\alpha_i$) to render a variable's effect—e.g, $C_{ij}(t)$—spurious, it must be correlated with the variable as well as have an effect on the outcome. The random-effects model assumes no correlation between the unobserved family effect and observed covariates.

It could be, however, that $\alpha_i$ *is* correlated with $C_{ij}(t)$ because unmeasured family characteristics affect selection into life course events. This would bias the estimates of our parameters, and in particular, our estimates of $\beta_4$. To address this possibility we relax the assumption that family effects, $\alpha_i$, are orthogonal to observed time-varying regressors, $B_{ij}$, $C_{ij}(t)$, and $X_{ij}(t)$, using a fixed-effects (FE) model:

$$y_{ij}(t) = \beta_1' X_{ij}(t) + \beta_2' B_{ij} + \beta_3' C_{ij}(t) + \alpha_i + u_{ij}(t), \tag{2}$$

where $\alpha_i$ is assumed to be fixed rather than random, and $Z_i$ is absorbed in $\alpha_i$.[3] Thus, even if unmeasured parental and family characteristics influenced mothers' selection into life course statuses, our fixed-effects model will provide consistent and asymptotic efficient estimates of parameters. Eq. (2) controls for unobserved selection into values of $B_{ij}$, $C_{ij}(t)$, and $X_{ij}(t)$ by controlling for $\alpha_i$.[4]

To provide an intuitive idea of how a fixed-effects model controls for unobserved selection effects, we can show how it can be estimated using first differences. Using a simplified model for illustration, suppose our model is correctly specified for each of two siblings, and $\mathrm{FAM}_i$, is a vector of preexisting family characteristics that select into life course structures (where $\alpha_i = \beta_3' \mathrm{FAM}_i$)

$$\text{sibling 1} \quad y_{i1}(t) = \beta_1' X_{i1}(t) + \beta_2' B_{i1} + \beta_3' \mathrm{FAM}_i + u_{i1}(t),$$

$$\text{sibling 2} \quad y_{i2}(t) = \beta_1' X_{i2}(t) + \beta_2' B_{i2} + \beta_3' \mathrm{FAM}_i + u_{i2}(t).$$

If we difference across the siblings by subtracting the equation for sibling 2 from that of sibling 1 (sibling 2 − sibling 1) we obtain:

$$y_{i2}(t) - y_{i1}(t) = \beta_1'[X_{i2}(t) - X_{i1}(t)] + \beta_2'(B_{i2} - B_{i1}) + \beta_3'(\mathrm{FAM}_i - \mathrm{FAM}_i) + u_{i2}(t) - u_{i1}(t).$$

[3] Because the estimates of our fixed-effects model are conditional on the sample in that $\alpha_i$ are treated as fixed (and estimable) rather than random and drawn from a probability distribution, we are limited in our ability to make out-of-sample predictions beyond the sample values of $\alpha_i$ (Chamberlain, 1982).

[4] A fixed-effects model also controls for persistent measurement errors in mothers' reports on child behavior problems, which are likely in panel data. Such persistent errors are absorbed in our unobserved family heterogeneity component. Note, however, that it does not control for other more complicated forms of measurement error.

The third term on the right-hand side is zero, and therefore this model yields consistent estimates of $\beta_1$ and $\beta_2$. Unobserved preexisting characteristics that select for life course structures, $\mathrm{FAM}_i$, are eliminated by subtraction, rather than through estimation of $\beta_3$.[5] In other words, all between-family variance in behavior problems has been eliminated. By comparing Eq. (1) with Eq. (2), we can examine whether traditional research on the timing of family structural changes on child outcomes may have yielded misleading results for failing to control for selection on unobserved family characteristics. This provides a strong test of our life course hypotheses and family selection hypothesis.

Although the fixed-effects sibling model controls for potential bias due to unmeasured family characteristics, it also has at least four limitations. First, our fixed-effects sibling models (and random effects models) do not control for unmeasured time-varying family effects. Fortunately, however, we control for a host of observed time-varying family variables, while examining the effects of a given life course variable. Second, the fixed-effects (and random effects) model does not control for unmeasured child characteristics that differ across siblings, such as sibling-specific genes, temperament, or mothers' attention. All of our models must rely on measured child-specific characteristics, $B_{ij}$, to rule out such effects. Third, all of our sibling models fail to control for the possibility that the behavior problems of the first child may have affected the decision to have a second child, which could bias the effects of the effect of number of siblings or teen child bearing (Phillips, 1999).[6] Fourth, sibling fixed-effects models eliminate between-family variance in independent variables, resulting in more sampling variability in estimates and less power of statistical tests relative to random effects models. Therefore, we will examine the relative percentage of within- and between-family variance in our predictor variables, and carefully assess the trade-offs between bias and efficiency in assessing RE vs. FE estimates.

### 6.2. Models of parenting practices

Our hypotheses about parenting practices include a parenting effect hypothesis, in which parenting directly affects child behavior problems and a family selection hypothesis, in which preexisting family characteristics select for parenting and child outcomes. Our parenting model adds parenting practices, $P_{ij}(t)$, to Eq. (1), which gives us a random effects model:

$$y_{ij}(t) = \beta_1' Z_i + \beta_2' X_{ij}(t) + \beta_3' B_{ij} + \beta_4' C_{ij}(t) + \beta_5' P_{ij}(t) + \alpha_i + u_{ij}(t), \tag{3}$$

where $\alpha_i$ is again assumed to be orthogonal to all other right-hand side variables. Thus, parents' selection of certain parenting practices is assumed to be captured by observed covariates, and is orthogonal to unmeasured family characteristics.

[5] Our data include families with more than two siblings, and therefore, rather than differencing the data, we deviate each sibling from his or her family-specific mean for each variable. Like differencing, this estimation procedure eliminates between-family variance in our variables and yields consistent estimates.

[6] We have examined this issue using a covariance structure model and found biases to be modest.

We can relax the latter assumption by adding parenting to Eq. (2), which gives us a fixed-effects model:

$$y_{ij}(t) = \beta_1' X_{ij}(t) + \beta_2' B_{ij} + \beta_3' C_{ij}(t) + \beta_4' P_{ij}(t) + \alpha_i + u_{ij}(t). \quad (4)$$

Eq. (4) offers a strong test of our parenting hypothesis. By comparing Eq. (3) with Eq. (4), we can test the parenting selection hypothesis, which implies that previous estimates of parenting effects may have been biased by failing to control for unobserved preexisting family characteristics. Also, by comparing our models with and without parenting practices, we can assess whether parenting mediates the effects of structural changes in the family on child outcomes.

## 7. Results

Table 2 describes the unweighted distributions of our analysis variables for the complete sample, which includes 3259 families with a total of 5808 children, and the sibling sample, which includes 1805 families of two or more siblings with a total of 4354 children. The two measures of behavior problems are very similar between the two samples. Most of the explanatory variables also have similar distributions across the two samples. As expected, the one variable that differs across samples is the percentage of first-borns, which should be lower in the sibling sample because it eliminates families with only one child (who necessarily are first born).

Both the complete and sibling *unweighted* samples over-represent disadvantaged and black families. Almost one-third of the children are black, the mothers of nearly 20% of the children were teenagers at time of first birth, and the mothers have relatively low ability (3.3 deciles in AFQT). At the time of birth, children faced relatively disadvantaged family conditions, including mothers with few years of education and relatively high incidences of drinking and smoking during pregnancy. These disadvantages persisted through childhood. About half of the children lived in non-intact families and 30% of the children were living in poverty at the time of assessment. Out of their first 6 years, children on average spent about 2 years in non-intact families as well as in poverty. The reason for this overrepresentation of disadvantaged families lies in the NLSY79 sampling design, which over-sampled minority families, who are on average less advantaged compared to whites. Recall also that NLSY79 includes all children born to female respondents who were born between 1957 and 1964 and our samples include children aged 6–14 during 1986–1996. Therefore, our samples include a disproportionate number of children of younger mothers (particularly for older children), who are on average more disadvantaged than children as a whole. For example, in our sample, children aged 14 were necessarily born to mothers 16–25 years old. The minority over-sample can be corrected by controlling for race in multivariate models. Such an option is not available to correct for our sample's omission of older mothers. We therefore exercise caution generalizing our results, even while controlling for race, low SES, and age. Although NLSY79 is the best dataset currently available to address our research questions, we emphasize that our findings do not generalize to the entire population of children.

Table 2
Descriptive statistics of variables used in analysis

| Variable | Complete sample | | Sibling sample | | % within variance |
|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | |
| *Behavior problems* | | | | | |
| Internalizing | .85 | .19 | .85 | .19 | 38.3 |
| Externalizing | 1.07 | .26 | 1.07 | .26 | 37.7 |
| *Child characteristics* | | | | | |
| Male | .51 | .50 | .52 | .50 | 59.9 |
| Age | 8.49 | 2.24 | 8.62 | 2.26 | 56.0 |
| Poor health | .23 | .75 | .23 | .75 | 53.1 |
| First born | .45 | .50 | .35 | .48 | 84.3 |
| Number of siblings | 2.59 | 1.17 | 2.89 | 1.12 | 10.4 |
| Age of youngest sibling | 5.40 | 3.11 | 5.29 | 2.97 | 36.9 |
| Missing number of siblings | .00 | .03 | .00 | .03 | 55.6 |
| Missing age of youngest sibling | .01 | .10 | .01 | .10 | 48.9 |
| *Background* | | | | | |
| Black | .31 | .46 | .32 | .47 | 0 |
| Mother even been a teen-mom | .18 | .38 | .19 | .39 | 0 |
| Mother's AFQT | 3.37 | 2.62 | 3.29 | 2.63 | 0 |
| Missing AFQT | .05 | .22 | .05 | .22 | 0 |
| *At-birth conditions* | | | | | |
| Mother was <18 at birth | .03 | .17 | .03 | .17 | 58.8 |
| Mother's education at birth | 11.76 | 2.26 | 11.67 | 2.21 | 3.5 |
| Mother drank during pregnancy | .09 | .28 | .09 | .27 | 40.8 |
| Mother smoked during pregnancy | .08 | .27 | .08 | .27 | 26.9 |
| Missing drinking or smoking | .05 | .21 | .05 | .21 | 54.4 |
| *Early childhood exposure* (years) | | | | | |
| Intact family | 3.78 | 2.78 | 3.83 | 2.78 | 22.7 |
| Living in grandparent home | .76 | 1.47 | .71 | 1.38 | 27.2 |
| Receiving AFDC | 1.25 | 2.03 | 1.35 | 2.11 | 9.4 |
| Living in poverty | 1.97 | 2.29 | 2.11 | 2.34 | 8.4 |
| *Current condition* | | | | | |
| Non-intact family | .48 | .50 | .46 | .50 | 19.4 |
| Living in grandparent home | .04 | .20 | .04 | .19 | 33.7 |
| Receiving AFDC | .18 | .39 | .20 | .40 | 17.3 |
| Living in poverty | .29 | .45 | .32 | .47 | 17.6 |
| *Duration of current spell* (years) | | | | | |
| Living in non-intact family | 2.02 | 3.41 | 1.95 | 3.37 | 24.6 |
| Living in grandparent home | .13 | .84 | .10 | .69 | 32.1 |
| Receiving AFDC | 1.02 | 2.58 | 1.13 | 2.72 | 15.1 |
| Living in poverty | 1.71 | 3.26 | 1.89 | 3.40 | 12.8 |
| *Missing experience* | | | | | |
| Missing intact family | .01 | .09 | .01 | .10 | 8.0 |
| Missing living arrangement or welfare receipt | .10 | .30 | .10 | .30 | 17.9 |
| Missing living in poverty | .58 | .49 | .59 | .49 | 20.4 |

Table 2 (*continued*)

| Variable | Complete sample | | Sibling sample | | % within variance |
|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | |
| *Parenting practice* | | | | | |
| Physical punishment (spanking) | .69 | 1.30 | .71 | 1.35 | 38.4 |
| Cultural activities | 1.78 | .67 | 1.76 | .68 | 31.5 |
| Positive parenting | 5.25 | 4.14 | 5.11 | 4.19 | 27.1 |
| Father's time | 3.20 | 1.22 | 3.21 | 1.23 | 27.3 |
| Reading activities | 3.63 | .92 | 3.59 | .93 | 39.7 |
| Missing physical punishment | .08 | .28 | .07 | .26 | 53.3 |
| Missing reading | .06 | .23 | .05 | .22 | 57.5 |
| Missing other parenting | .17 | .37 | .15 | .36 | 54.1 |
| Number of Children | 5808 | | 4354 | | |
| Number of Families | 3259 | | 1805 | | |

Source: National Longitudinal Survey of Youth, youths 1979–1996, children 1986–1996. *Note.* Complete sample refers to a sample of all children with one observation per child randomly selected. Sibling sample refers to a sample of more than one sibling per family. We report the distribution of internalizing and externalizing symptoms before standardization used in model estimation.

The use of sibling data to control for unobserved family heterogeneity requires that variables used in the analysis exhibit sufficient variation within families. The last column of Table 2 presents within-family variance of each variable as a percentage of total variance for the sibling sample. The percentage of within-family variance for internalizing and externalizing symptoms is almost 40%. The percentage for the right-hand side variables ranges from 3.5% for mother's education at birth to 84.3% for the first-born status. Childhood exposure to parents' life course transitions varies between 8.4 and 33.7%. We exercise caution in drawing conclusions from variables with small within-family variances by examining the changes in sampling variability (estimated by standard errors) in moving from random effects to fixed-effects models.

Turning to multivariate analysis, we present bivariate, random-effects, and fixed-effects estimates estimates from the same model without parenting mediating effects in two tables—Table 3 for child characteristics, family background, and at-birth conditions Table 4 for childhood exposure and family instability. Comparison of bivariate and random-effects estimates allows us to examine potential spuriousness and hidden antecedent or mediating effects in the bivariate estimates, as well as assess the potential effects of multicollinearity in the random-effects estimates.[7] Comparison of the random- and fixed-effects allows us to examine potential bias in our random-effects estimates due to unmeasured family selection, as well as a potential

[7] Multicollinearity can be a problem because it increases sampling variability in estimates and causes a loss of power in statistical tests. It is directly analogous to having too small a sample size (see Goldberger, 1991; Matsueda and Bielby, 1986). Therefore, to assess potential problems, we examine the behavior of estimated standard errors (our measure of sampling variability) when potentially collinear variables are estimated separately (table not shown), versus jointly (see also Belsley, 1982).

Table 3
Child characteristics, family background, at-birth conditions, and child behavior problems

| | Internalizing | | | Externalizing | | |
|---|---|---|---|---|---|---|
| | Bivariate | RE | FE | Bivariate | RE | FE |
| *Child characteristics* | | | | | | |
| Male | .071** | .053* | .037 | .275*** | .258*** | .258*** |
| | (.026) | (.024) | (.031) | (.026) | (.024) | (.032) |
| Age | .052*** | .058*** | .067*** | .028*** | .034*** | .053*** |
| | (.006) | (.007) | (.010) | (.006) | (.007) | (.010) |
| Poor health | .200*** | .158*** | .096*** | .189*** | .149*** | .102*** |
| | (.017) | (.016) | (.022) | (.018) | (.016) | (.022) |
| First born | .054* | .063* | .061 | −.077** | −.054* | −.095* |
| | (.026) | (.030) | (.038) | (.027) | (.031) | (.038) |
| Number of siblings | −.010 | −.062* | −.035 | .022 | .049* | −.046 |
| | (.011) | (.017) | (.038) | (.011) | (.017) | (.039) |
| Age of youngest sibling | .014** | −.013* | −.012 | .006 | −.018** | −.026** |
| | (.004) | (.006) | (.009) | (.004) | (.006) | (.009) |
| *Background* | | | | | | |
| Black | .018 | −.055 | — | .060* | −.026 | — |
| | (.028) | (.039) | | (.029) | (.038) | |
| Mother was a teen-mom | .106** | .018 | — | .201*** | −.021 | — |
| | (.034) | (.045) | | (.034) | (.045) | |
| Mother's AFQT | −.033*** | −.004 | — | −.046*** | .001 | — |
| | (.005) | (.008) | | (.005) | (.007) | |
| *At-birth conditions* | | | | | | |
| Mother was <18 at birth | −.007 | −.251*** | −.300** | .116 | −.112 | −.140 |
| | (.077) | (.079) | (.105) | (.078) | (.080) | (.107) |
| Mother's education at birth | −.054*** | −.031** | −.050 | −.081*** | −.054*** | −.084* |
| | (.006) | (.009) | (.033) | (.006) | (.009) | (.034) |
| Mother drank during pregnancy | .138** | .065 | −.002 | .231*** | .126** | .017 |
| | (.046) | (.045) | (.069) | (.047) | (.045) | (.070) |
| Mother smoked during pregnancy | .279*** | .160** | .080 | .426*** | .249*** | .026 |
| | (.048) | (.050) | (.087) | (.049) | (.050) | (.089) |
| $R^2$ | | .069 | .054 | | .099 | .059 |
| Number of children | | 5808 | 4354 | | 5808 | 4354 |
| Number of families | | 3259 | 1805 | | 3259 | 1805 |

Source: National Longitudinal Survey of Youth, youths 1979–1996, children 1986–1996. *Note.* All models include childhood experience of mother's life course transitions and dummy variables indicating missing values. Internalizing and externalizing are standardized. Standard errors are in parentheses. The *R*-squared reported is the overall *R*-squared for RE and the within *R*-squared for FE.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

increase in sampling variability in our fixed-effects models. The dependent variables are standardized to allow us to compare the relative magnitudes of effects between internalizing and externalizing symptoms. Overall, the $R^2$s of our RE models are

moderate in size (.07–.10), similar or somewhat larger than previous research using these data (e.g., McLeod and Shanahan, 1993; Parcel and Menaghan, 1994).[8]

### 7.1. Child characteristics, background variables, and conditions at birth

Table 3 presents effects of child characteristics, mothers' background, and mother's conditions at birth. Consistent with previous research, males exhibit substantially more externalizing symptoms, but only slightly more internalizing problems. Age has strong positive effects on both internalizing and externalizing problems. According to the FE estimates, each year of age is associated with an increase of about 7% of a standard deviation of internalizing and 5% of externalizing. We find that poor health of the child has a strong effect on behavior problems, and this effect survives controls for selection on unobserved variables. According to our FE model, an increase in health conditions that limit a child's activities are associated with an increase of 10% of a standard deviation of internalizing and externalizing. Furthermore, health interacts with sex: poor health is associated with boy's behavior problems but not that of girls (effect not shown).

Among our background variables, teenage mother and mother's AFQT score show significant bivariate effects, which no longer have a direct effect on behavior problems in the random-effects model that includes child characteristics and at-birth conditions. This suggests that child characteristics and at-birth conditions mediate the effects of teenage mother and AFQT. Note that these background variables are identical for siblings and therefore, are absorbed in the fixed-effects of the FE models.

With two exceptions, the effects of variables measured at birth fail to survive controls for unobserved heterogeneity. One exception is mother's education at birth—a well-established factor in determining child's outcomes—which has a significant negative effect on externalizing problems. Indeed, mother's education is the strongest predictor of externalizing in our FE models (standardized coefficient of −.19). The effect of mother's education on internalizing is significant in our RE models, but not in our FE models, despite the point estimate being relatively large (standardized coefficient of −.11). The lack of significance could be due to increased sampling variability of the FE estimate (the standard error is nearly four times as large). The other exception is maternal age at birth of the child, which, net of other variables, significantly *reduces* internalizing symptoms in FE.[9] Mother's drinking and smoking during pregnancy have small effects in our RE models, which disappear in our FE models. The latter finding is unlikely to be due to sampling variability, as a standardized coefficient of less than .05 would be significant in the FE model.[10]

[8] Note that the $R^2$s for the FE models indicate only the percentage of variation explained by the sibling-specific predictors and do not include those explained by the family fixed-effects.

[9] The effect of teen maternal age on internalizing problems persists when we further classify maternal age into four categories: <18, 18–19, 20–24, and 25 and above. In addition, other estimates remain unchanged.

[10] A simple approximation of how large a coefficient must be to reach significance given our estimated standard error can be computed by $\beta^* = s_\beta\,(1.96)$, where $s_\beta$ is the standard error, and 1.96 is the $t$ value corresponding to the 5% significance level. The standardized counterpart would be $P^* = \beta^*\sigma_x$ when the dependent variable is standardized.

### 7.2. Early, current childhood exposure and family instability

Table 4 presents estimates of the effects of mother's life course transitions on child behavior problems. Table 4 also gives the Hausman test of the null hypothesis, assumed by our random-effects model, that unmeasured family effects are orthogo-

Table 4
Child exposure to family dynamics and child behavior problems

| Variable | Internalizing | | | Externalizing | | |
|---|---|---|---|---|---|---|
| | Bivariate | RE | FE | Bivariate | RE | FE |
| *Early childhood exposure (years)* | | | | | | |
| Intact family | −.024*** | .010 | .016 | −.038*** | .003 | −.001 |
| | (.005) | (.010) | (.016) | (.005) | (.010) | (.016) |
| Living in grandparent home | .015 | −.024* | −.017 | .026** | −.016 | −.001 |
| | (.009) | (.011) | (.020) | (.009) | (.011) | (.020) |
| Receiving AFDC | .048*** | .008 | .010 | .071*** | .007 | −.020 |
| | (.006) | (.011) | (.020) | (.006) | (.011) | (.020) |
| Living in poverty | .054*** | .031** | .044* | .078*** | .041*** | .049* |
| | (.006) | (.010) | (.019) | (.006) | (.010) | (.019) |
| *Current condition* | | | | | | |
| Non-intact family | .166*** | .080 | −.006 | .237*** | .136* | −.013 |
| | (.026) | (.055) | (.101) | (.026) | (.055) | (.104) |
| Living in grandparent home | .324** | .178 | −.068 | .076 | .033 | −.227 |
| | (.103) | (.092) | (.139) | (.105) | (.093) | (.142) |
| Receiving AFDC | .214** | .074 | .008 | .078 | .024 | −.065 |
| | (.072) | (.064) | (.100) | (.073) | (.064) | (.102) |
| Living in poverty | .070 | .063 | .126 | .014 | .088 | .007 |
| | (.060) | (.051) | (.078) | (.061) | (.051) | (.080) |
| *Duration of current spell* (years) | | | | | | |
| Non-intact family | .021*** | −.003 | −.005 | .026*** | .006 | −.013 |
| | (.003) | (.007) | (.010) | (.003) | (.007) | (.010) |
| Living in grandparent home | −.039 | −.023 | .054 | −.006 | .006 | .091* |
| | (.023) | (.022) | (.039) | (.023) | (.022) | (.040) |
| Receiving AFDC | .001 | .001 | .024 | .010 | .006 | .016 |
| | (.009) | (.010) | (.015) | (.009) | (.010) | (.016) |
| Living in poverty | .018** | .001 | −.023 | .011 | −.011* | −.022 |
| | (.007) | (.008) | (.013) | (.007) | (.008) | (.013) |
| Hausman test: $\chi^2$ | | 65.29** | | | 67.95** | |
| (*df*) | | (28) | | | (28) | |
| $R^2$ | | .069 | .054 | | .099 | .059 |
| Number of children | | 5808 | 4354 | | 5808 | 4354 |
| Number of families | | 3259 | 1805 | | 3259 | 1805 |

Source: National Longitudinal Survey of Youth, youths 1979–1996, children 1986–1996. *Note.* All models include childhood experience of mother's life course transitions and dummy variables indicating missing values. Internalizing and externalizing are standardized. Standard errors are in parentheses. The *R*-squared reported is the overall *R*-squared for RE and the within *R*-squared for FE.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

nal to observed regressors. The tests reject the RE assumption in favor of the FE assumption that family effects are correlated with regressors. This finding supports the family selection hypothesis that some RE estimates are spurious due to unmeasured family effects. We turn to the individual coefficients to determine where selectivity renders specific effects spurious.

The early childhood exposure hypothesis implies that early childhood exposure has long-term consequences for child behavior problems assessed during middle childhood. Consistent with this hypothesis, we find that early childhood exposure to poverty is associated with greater behavior problems and this survives the test of family selectivity in the FE models (see the first panel of Table 4). According to the FE estimates, when compared to a child who never lived in poverty during early childhood (46% of our sampled children), a child who lived in poverty throughout the 6 years of early childhood (14% of our sampled children) will exhibit about 26% ($.044 \times 6$ years) of a standard deviation more internalizing problems and 29% of a standard deviation more externalizing problems.

We do not, however, find evidence for the effects of early childhood experience of family structure, AFDC, and grandparent co-residence on child behavior problems. The bivariate effects of early childhood exposure to non-intact family structure and AFDC are no longer significant in the RE model. It is possible that at-birth conditions are antecedents of early childhood exposure.

Turning to current childhood exposure, the bivariate relationships show some small effects on internalizing, but these effects disappear in the random effects model (and sampling variability is small). When externalizing problems are concerned, non-intact family has a positive bivariate effect, which persists in the RE model. The FE estimate of this variable is not significant, but the large standard error suggests we cannot rule out sampling variability with our data. That is, the non-significant effect of current exposure to non-intact family could be due to family selection or sampling variability. In sum, the current exposure hypothesis receives little support from our data.

In testing the family instability hypothesis, we examine the effects on child behavior problems during middle childhood of continuous duration following the most recent change in family circumstance. We find little evidence for this hypothesis. Non-intact family has a very small bivariate effect on behavior problems, which disappears in our multivariate models. All other effects of duration in current state are negligible in size and significance (including the effect of duration of living with grandparents, which is only .06 standardized, although significant at the .05 level).

We noted that some previous research has operationalized the family instability hypothesis using the frequency of transitions the child experienced (e.g., Wu and Martinson, 1993). Therefore, we re-estimated our models substituting the current duration measures with the frequency of transitions. Here, several of the RE estimates, while modest in size, reach statistical significance. The FE estimates, however, are small and non-significant, meaning we cannot rule out family selection. Other research on family instability emphasizes the recency of changes. Any recent change in family circumstances can produce disequilibrium and create stress on children (Elder, 1974), but downward changes, in particular, may be linked to chronic strain,

a key producer of stress (Pearlin, 1989). We use a dummy variable to indicate any change in family circumstances within the past 2 years and another dummy to indicate only recent downward changes. Substituting the current duration measures with either indicator, we find that a positive correlation between behavior problems and a downward change disappears in both RE and FE estimates. Any change, however, is positively related to externalizing (but not internalizing) in FE estimates, but strangely it is due to upward and not downward changes. Taken together, these results do not support the hypothesis that recent changes are responsible for children's behavior problems.

We also explored the possibility that chronic exposure to family deprivation throughout childhood is the key variable, rather than early childhood exposure (Pearlin, 1989). We measured chronic deprivation as 75% in the family condition throughout childhood, and found no significant effects in our FE models. We then measured chronic deprivation as the number of cumulative years in the family condition from birth to the age of assessment and found that chronic poverty had modest effects, which are difficult to disentangle from early child poverty. Thus, it appears that early childhood poverty is driving any effects of chronic poverty throughout childhood.

Finally, to test hypotheses about early and current childhood exposure and family instability our models have included three measures for each of the four dimensions of family conditions (poverty, welfare, family structure, and kin co-residence). Given that only early child poverty shows a robust effect, might these trios of variables be too highly related to disentangle their separate effects or somehow cancelled each other out? To investigate this possibility, we conducted joint tests of the trio of coefficients in our fixed-effects models and found that the poverty variables, driven by early child poverty, were highly significant as a group, but welfare, family structure, and kin co-residence were not. We also estimated each variable within a set separately, dropping the other two, and failed to find differences in our overall story.[11]

### 7.3. Parenting practices

Table 5 presents the results from the model adding the parenting practices. We also report coefficients for our family structural condition variables to examine whether our parenting practices mediate their effects. If these coefficients in Table 5 are diminished from the corresponding coefficients of Table 4, we would conclude that parenting is mediating effects of family conditions. We find little support for this hypothesis. The RE and FE estimates for family dynamic variables in Tables 4 and 5 are remarkably similar in sign, magnitude, and significance level. Of most importance, the effects of early child poverty, the strongest variable, are unchanged in both

[11] Another possibility might be that our early childhood variables have indirect effects that operate through later life course variables, which mask their effects. We estimated a reduced-form FE model for early childhood effects excluding all later life course effects, and again found robust effects of early childhood poverty only.

Table 5
Child exposure to family dynamics, parenting, and child behavior problems

| Variable | Internalizing | | | Externalizing | | |
|---|---|---|---|---|---|---|
| | Bivariate | RE | FE | Bivariate | RE | FE |
| *Early childhood exposure* (years) | | | | | | |
| Intact family | −.024*** | .006 | .013 | −.038*** | −.000 | −.004 |
| | (.005) | (.010) | (.016) | (.005) | (.010) | (.016) |
| Living in grandparent home | .015 | −.022* | −.009 | .026** | −.017 | .000 |
| | (.009) | (.011) | (.020) | (.009) | (.010) | (.020) |
| Receiving AFDC | .048*** | .006 | .006 | .071*** | .005 | −.022 |
| | (.006) | (.010) | (.019) | (.006) | (.010) | (.020) |
| Living in poverty | .054*** | .030** | .044* | .078*** | .037*** | .049* |
| | (.006) | (.010) | (.019) | (.006) | (.010) | (.019) |
| *Current condition* | | | | | | |
| Non-intact family | .166*** | .042 | −.024 | .237*** | .099 | −.033 |
| | (.026) | (.053) | (.099) | (.026) | (.053) | (.101) |
| Living in grandparent home | .324** | .152 | −.027 | .076 | .018 | −.238 |
| | (.103) | (.090) | (.137) | (.105) | (.090) | (.139) |
| Receiving AFDC | .214** | .050 | −.014 | .078 | .005 | −.086 |
| | (.072) | (.062) | (.098) | (.073) | (.062) | (.100) |
| Living in poverty | .070 | .037 | .076 | .014 | .043 | −.042 |
| | (.060) | (.049) | (.077) | (.061) | (.049) | (.078) |
| *Duration of current spell* (years) | | | | | | |
| Non-intact family | .021*** | −.003 | −.005 | .026*** | −.005 | −.012 |
| | (.003) | (.006) | (.010) | (.003) | (.006) | (.010) |
| Living in grandparent home | −.039 | −.016 | .055 | −.006 | .011 | .094* |
| | (.023) | (.022) | (.038) | (.023) | (.022) | (.039) |
| Receiving AFDC | .001 | −.000 | .024 | .010 | .003 | .016 |
| | (.009) | (.010) | (.015) | (.009) | (.010) | (.015) |
| Living in poverty | .018** | .000 | −.019 | .011 | −.009* | −.019 |
| | (.007) | (.008) | (.013) | (.007) | (.008) | (.013) |
| *Parenting practice* | | | | | | |
| Physical punishment | .134*** | .129*** | .095*** | .205*** | .182*** | .154*** |
| | (.010) | (.010) | (.015) | (.010) | (.010) | (.015) |
| Cultural activities | −.095*** | −.020 | −.010 | −.186*** | −.061** | −.046 |
| | (.019) | (.019) | (.031) | (.020) | (.019) | (.032) |
| Positive parenting | −.017*** | −.008* | −.004 | −.026*** | −.014*** | −.009 |
| | (.003) | (.003) | (.005) | (.003) | (.003) | (.006) |
| Father's time | −.090*** | −.044*** | −.023 | −.084*** | −.027* | −.014 |
| | (.011) | (.012) | (.019) | (.011) | (.012) | (.019) |
| Reading activities | −.126*** | −.067*** | −.038 | −.234*** | −.120*** | −.068** |
| | (.014) | (.014) | (.021) | (.014) | (.014) | (.021) |
| Hausman test: $\chi^2$ | | 81.78*** | | | 94.47*** | |
| ($df$) | | (36) | | | (36) | |
| $R^2$ | | .133 | .095 | | .177 | .106 |
| Number of children | | 5808 | 4354 | | 5808 | 4354 |
| Number of families | | 3259 | 1805 | | 3259 | 1805 |

Source: National Longitudinal Survey of Youth, youths 1979–1996, children 1986–1996. *Note.* All models include individual characteristics, family background, at-birth conditions, early childhood duration, current state and the spell of current state, as well as the dummy variables indicating missing values. Internalizing and externalizing are standardized. Standard errors are in parentheses. The *R*-squared reported is the overall *R*-squared for RE and the within *R*-squared for FE.

* $p < .05$.
** $p < .01$.
*** $p < .001$.

RE and FE estimates. Clearly, child poverty affects behavior problems directly, and not indirectly through current parenting practices. This does not mean, however, that parenting is not important.

Indeed, turning to the effects of parenting practices themselves, we find that mother's physical punishment is positively associated with both internalizing and externalizing symptoms in our RE models. Moreover, these effects persist in our FE models, so we can rule out the family selection hypothesis in favor of the parenting effects hypothesis. The FE results reveal that physical punishment has a particularly strong effect on externalizing behaviors. Compared to children who are not spanked, children who are spanked an average of eight times a week score 1.23 standard deviations higher on externalizing symptoms and 0.76 higher on internalizing. Reading activities exert a small but significant effect on externalizing problems, which persists in the FE model, but its effect on internalizing is about half the size and is not quite significant in the FE model. The other parenting variables have very small effects that appear to be spurious due to family selection. The small but significant effects of cultural activities, positive parenting, and father's time in RE models are roughly cut in half in the FE models, which explains their lack of statistical significance. Thus, unobserved family characteristics, such as parent's unmeasured endowments, cultural background, or genes, render these parenting effects spurious.

These estimates of parenting effects, however, are subject to possible bias if children's behavior influences parental behavior. According to the child psychological literature, children contribute to their development by eliciting parental responses. Thus, we would expect that submissive, conforming children would tend to elicit positive responses from parents, while aggressive, temperamental children might elicit aggressive or punitive responses. This implies that any estimate of parenting effects could be overestimated if it does not account for the potential child effect. We re-estimated our models using lagged parenting variables to predict our child behavior problems. Using the 2-year lagged variable available in the dataset, we found that physical punishment exerted significant and strong effects on both internalizing and externalizing problems, which persist in FE models. As expected, the effect is smaller using a 2-year lag. We also find significant effects of reading activities in our RE models, but they are no longer significant in our FE models.[12]

## 8. Conclusions

This article has examined the effects of structural changes in the family and parenting practice on child behavior problems during middle childhood. Our models draw upon theories of the life course and child development and improve on prior research by examining multiple structural changes in families simultaneously.

[12] We also investigated whether our parenting variables mediated the effects of our life course variables, and did not find evidence to support the hypothesis. In particular, neither physical punishment nor reading activities mediated the effects of early childhood poverty on child behavior problems.

In particular they distinguish among early and current childhood exposure and family instability, controlling for family selection by controlling for unobserved, preexisting family differences, and considering the potential mediating effect of parenting. Our models yield three robust findings.

Our first finding is support for the early childhood exposure hypothesis for poverty. We find that longer exposure to poverty in early childhood is associated with greater child behavior problems in middle childhood. This effect persists when controlling for early exposure to other structural changes, current exposure to all structural changes, family stability, and unmeasured preexisting family differences. This result provides stronger evidence for an early child poverty hypothesis than found previously (e.g., Duncan et al., 1994; Duncan and Brooks-Gunn, 1997; Lichter, 1997). In addition, we find that the early childhood exposure effect is not mediated by current parenting practices. All this implies that policies designed to ameliorate early childhood poverty may be particularly effective in improving child development.

Second, we find support for the detrimental effect of physical punishment, which is substantially associated with children's behavior problems. Our evidence here is stronger than previous studies (see Straus, 1991) because we have ruled out family selectivity and we consider the potential child feedback to parenting using a lagged predictor variable.

Third, we find some support for the family selection hypothesis, which posits that significant effects found in random-effects regressions are spurious due to the correlation between unobserved preexisting family differences and observed regressors. Several parenting variables identified as important in previous studies show small but significant effects in our random effects models, but turn out to be spurious when preexisting family effects are controlled. These include positive parenting, father's time, and cultural activities. In addition, mother's drinking and smoking during pregnancy also appears to be spurious. These findings underscore the importance of controlling for unmeasured preexisting family conditions.

While our results are based on a stronger research design than previous studies of these issues, we should nevertheless point out several important caveats. First, we have not controlled for unmeasured child characteristics that differ across siblings, such as child-specific genes, temperament, or parents' specific treatment of one sibling versus the others. This could be done using individual fixed-effects models for longitudinal data, but would prevent us from estimating effects of family conditions that differ across siblings. Second, although we have controlled for a large number of measured time-varying effects, there could be others, that we have excluded which could exert a biasing effect. Third, our model using fixed-effects and controlling for a large number of related variables substantially reduces bias in estimates, but also increases sampling variability. We have been careful in evaluating our statistical tests and generally find adequate power to detect meaningful effects. For example, we generally have sufficient power to detect standardized coefficients as small as .09. Indeed, perhaps a problem with nearly all analyses of NLSY79 is finding many significant coefficients but explaining little overall variance because the coefficients are very small (e.g., McLeod and Shanahan, 1993; Parcel and Menaghan, 1994). Finally,

as noted earlier, the sampling design of NLSY79 children does not permit us to generalize to the population of all mothers and children.

Future research can extend our analyses to examine issues we have been unable to address fully. First, longitudinal data on children's behavior problems would allow us to examine trajectories of children's behavior problems, while controlling for unobserved heterogeneity across families and across siblings. Second, covariance structure models would allow one to model dynamic processes in which parents make decisions about investing in their children. For example, we could estimate a model in which the behavior problems of the first-born child would influence parents' decisions about investing in subsequent children. In this way, sibling outcomes would be dynamically interrelated (e.g., Becker, 1991; Rosenzweig and Wolpin, 1995).

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