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# The Impact of Complex Family Structure on Child Well-being: Evidence From Siblings

*Evidence from the United Kingdom Millennium Cohort on children at ages 3 and 5 with older siblings addresses the questions of whether those living with both biological parents and only full siblings have better emotional and behavior outcomes than other children, and whether nonfull siblings affect children's outcomes independently of parents' partnership status. Adjusting for measured family circumstances and resources in cross-sectional regressions accounted for much of the adverse association of family complexity with child outcomes. Controlling for unobserved family and child fixed effects did not, however, attenuate all estimates further. Fixed unobservable factors appeared to be masking underlying associations. Allowing for them intensified some, albeit modest, estimates. These revealed excess externalizing behavior problems for boys with single or stepparents but only full siblings. For girls with single mothers, the chances of internalizing problems were raised. Whether siblings were full or not made little difference to outcomes in general.*

In recent decades, patterns of partnership and parenthood have changed profoundly in many industrialized countries, with implications for the living arrangements in which children are brought up (Lesthaeghe, 1995; McLanahan, 2004). Children living with both biological parents tend to have better developmental outcomes than children who do not. This empirical regularity is found in the United States (Carlson & Corcoran, 2001; Cooksey, 1997; McLanahan & Sandefur, 1994) as well as other industrialized countries (Björklund, Ginther, & Sundström, 2007; Ermisch & Francesconi, 2001). As family forms have diversified, the literature has not only contrasted the experiences of children in one- and two-adult families but also distinguished between married and cohabiting couples and the

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formation of stepfamilies (Artis, 2007; Biblarz & Raftery, 1999; Brown, 2004; Case, Lin, & McLanahan, 2001; Hofferth, 2006; Hofferth & Anderson, 2003; Sweeney, 2010). Increasing partnership instability and multipartner fertility mean that children are less likely to live with both biological parents and are also more likely to be brought up sharing a home with half- or stepsiblings (Brown, Stykes, & Manning, 2016; Carlson & Furstenberg, 2006; Guzzo, 2014). Yet family structure research has focused more on parent–child relationships, paying less attention to the configuration of sibling relationships in the family.

Siblings form another dimension to a child's experience of family life, which is likely to have implications for child development (Brody, 1998; Dunn, 2005). Siblings can support each other during stressful events or be an additional source of maladjustment. Although psychological literature on the quality of siblings' relationships downplays the importance of biological relatedness (Gass, Jenkins, & Dunn, 2007), there is emerging evidence from family structure research showing that children who live with nonfull siblings fare worse than those who do not (Halpern-Meekin & Tach, 2008; Tillman, 2008).

In this article, we suggest that the relationship between family structure and child development is best understood when families are studied holistically and sibling constellations are taken into account alongside parent–child relationships. Drawing on research on stepfamilies and on the few studies distinguishing nontraditional siblings (Fomby, Goode, & Mollborn, 2016; Gennetian, 2005; Halpern-Meekin & Tach, 2008; Tillman, 2008), we distinguish between biological two-parent, stepparent, and single-parent families as well as families with full and nonfull siblings. This provides a more complete picture of the family structure than usually made in the literature. We use the term *family complexity* to indicate this way of conceptualizing children's living arrangement, which integrates the traditional approach based on child–parent relationships with information on children's relationships to their coresiding siblings (Brown, Manning, & Stykes, 2015). Our contribution is based on previously unanalyzed evidence about children's behavioral adjustment in families with more than one sibling in the United Kingdom, where data of the appropriate structure are relatively rare.

## Why Family Complexity May Matter

There are only a few studies, all from the United States, that expand the traditional family structure classification to investigate the impact of living with half- or stepsiblings, but this emerging line of research points to adverse effects for children and youth. Adolescents living with half- or stepsiblings were found to have poorer academic achievement and higher levels of depression, school-related behavioral difficulties, and delinquency than children living with full siblings only (Halpern-Meekin & Tach, 2008; Tillman, 2008). These associations cut across different parental structures and remained after controlling for family background characteristics, family instability, quality of interpersonal relationships within the family, and parental investment in children. Moreover, the negative association of family complexity with youth academic achievement was much stronger for males than females (Tillman, 2008), in line with the finding that girls forge more positive relationships with their siblings of any sort than do boys (Anderson & Rice, 1992). Ginther and Pollak (2004) found little difference in the educational outcomes of joint children and stepchildren from the same complex families, but both groups of children had worse academic outcomes than children who were residing with full siblings only. Similarly, Gennetian (2005) reported the possibility of a small negative effect of living with half- and stepsiblings on the cognitive scores of children aged 5 to 10. The results on children's emotional and behavioral adjustment are similar. Fomby et al. (2016) showed that among kindergarten children, those living with half- or stepsiblings displayed worse behavioral scores when compared with their peers with full siblings only and whose parents had the same union status, even after extensive controls on parental financial, material, and emotional resources. Hofferth (2006) also found more emotional and behavioral difficulties among children aged 3 to 12 years with complex sibships.

There are a number of explanations of why these negative associations might be effects of family complexities on child development, which broadly draw on theories about stress and parental investment (Tillman, 2008). Stress theory asserts that major or highly disruptive events create strain and psychological distress (Amato, 2000; Fomby & Cherlin, 2007; Thoits, 1995). Changes in parental partnership may put children at greater risk of stresses from ruptured relationships and the upheaval of family life. Stressful changes may also affect parents, leading to more punitive and less responsive parenting practices (Cooper, McLanahan, Meadows, & Brooks-Gunn, 2009). Other disruptive life events, such as home moves or loss of contact with a familiar social network, often accompany partnership breakups, reinforcing its negative effect on family members' emotion, behavior, and health (Adam & Chase-Lansdale, 2002).

Stepfamily formation, although varied, often entails a series of events, from partnership breakup to the introduction of a new parental figure. The departure or arrival of parental figures can be bewildering for children, especially younger ones (Amato, 2005). There are relatively few legal and social norms on the kind of involvement and investment expected from stepparents, and such ambiguity increases adjustment problems (Cherlin, 1978). The presence of half- and stepsiblings can reinforce the ambiguity, making it more difficult to define not only individual family members' roles (Fine, 1996) but also who belongs to the family and who does not (Brown & Manning, 2009; Stewart, 2005). For example, the arrival of a half-sibling may trigger feelings of displacement in the older child, who may feel excluded from the new intact family being formed, as found by Bernstein (1997), particularly for boys. In other words, relative to other families, complex families are likely to experience more stressful changes and be characterized by more ambiguous family roles, resulting in lower parental effectiveness as well as children's maladjustment.

The explanations referring to parental investment focus on differences across family forms in economic, emotional, and time resources devoted to children. Lower financial and material resources explain much of the disadvantage, particularly in cognitive and educational terms of growing up with a single parent. Single parents are less able to buy goods and services that enrich child development and experience chronic stress associated with economic hardship (Chase-Lansdale & Pittman, 2002). Complex families may have fewer resources than intact families if the adults have additional parenting obligations outside the household. The greater fluidity in family arrangements that characterizes complex families with multipartner fertility also means that children may cumulatively receive fewer parental resources during their childhood and adolescence, as they may experience spells with only one parent or very short-term relationships with a parent figure (Cancian, Meyer, & Cook, 2011).

Researchers have also paid particular attention to the distribution of parental resources, which is thought to be more contentious in complex families than in intact families. The weaker biological ties can lead to lower emotional and material investment in nonbiological children and to discrimination between siblings with different nonresident parents (Evenhouse & Reilly, 2004), although other studies find little support for the biological ties hypothesis (Hofferth & Anderson, 2003). Children who receive less favorable parental treatment than their siblings are also more likely to have behavioral problems and internalizing symptoms (McHale, Updegraff, Jackson-Newsom, Tucker, & Crouter, 2000).

There is also a long, but distinct, literature focusing on the number of children in a family. This builds on the idea that the more children there are in a family the more likely will be competition for material resources and parental attention, and that parents of large families trade off quality for quantity (Becker & Tomes, 1976). To the extent that complex families have more children than simple families, there would be more resource dilution. However, Black, Devereux, and Salvanes (2005) failed to find confirmation of a penalty to family size in Norwegian data on educational attainment. There is evidence that in stepfamilies and intact families alike, the arrival of a joint child reduces the parental involvement with existing children (Stewart, 2005). Yet this shift of attention may be more problematic for the cohesion of stepfamilies where the ties between stepparent and stepchild are still being formed. In sum, research examining parental resources has suggested that children in complex families may be at a disadvantage relative to children in traditional nuclear families because of lower parental financial, emotional, and time investment, with the presence of nontraditional siblings possibly reinforcing this pattern. Children in nontraditional families may be at greater risk from stresses on resources or relationships, but not inevitably. How far they succumb can vary by outcome, circumstances, and their own and family members' abilities to cope, also known as resilience (Patterson, 2002).

### Explaining the Empirical Regularities

The associations between nontraditional family forms and adverse outcomes for children are well documented empirically, and there are many plausible causal mechanisms. Nevertheless, they may result from differences in the characteristics of parents selecting into different family forms rather than family structure per se. In the U.S. context in particular, patterns of partnership and fertility differ starkly across socioeconomic groups, and complex families are much more common among those with fewer resources (McLanahan, 2004). Within this pattern there may also be some positive selection of stepparents with good parenting skills into complex families, as detected by Hofferth (2006). Studies on the association between family form and child outcomes usually estimate a series of ordinary least squares (OLS) models that progressively incorporate additional variables on respondents' background. The problem remains that observed explanatory variables may not fully account for selection on unobserved child or family characteristics.

A common solution in family structure research is to employ fixed effects with longitudinal data. Several studies on divorce and child development were able to account for much of the apparent impact of divorce by adjusting their estimates for preexisting differences between children. For example, Cherlin et al. (1991) studied the impact of divorce on children's behavioral and cognitive outcomes in the United States and United Kingdom. By adjusting their estimates for preexisting differences in behavior and achievement between the children of parents who would later divorce and the children whose parents would remain together, they were able to account for much of the apparent impact of divorce, particularly on boys' behavior problems. Likewise, in Li's (2007) analyses of the behavior problems of girls and boys separately, the significance of parental divorce disappears on the introduction of fixed effects. Other research in the United States has applied fixed effects to isolate the impacts of family structure on various outcomes (e.g., Crosnoe, Prickett, Smith, & Cavanagh, 2014; Gibson-Davis, 2008). The technique eliminates unvarying influences, observed as well as unobserved.

Data on multiple siblings to address unmeasured family heterogeneity have been used in studies of outcomes in adulthood (Björklund et al., 2007; Ermisch & Francesconi, 2001). Taking sibling differences tends to remove adverse associations with nontraditional family structure in childhood. Sibling fixed effects have also been used to assess whether growing up with one stepparent and one biological parent rather than both biological parents is consequential for children. Case et al. (2001) found that children in blended families who are the biological offspring of the mother had better educational attainment than children in the same family raised by a stepmother, but Ginther and Pollak (2004), looking at stepfamilies with mainly stepfathers, found little difference in the educational outcomes of joint children and stepchildren within a family. Evenhouse and Reilly (2004), on the other hand, analyzed 33 outcomes in adolescence and early adulthood from the U.S. National Longitudinal Survey of Adolescent Health. After differencing across siblings, they found that outcomes related to child interactions with parents retained their sensitivity to family structure, suggesting genuine stresses as a result of living with a stepparent.

Only one study, Gennetian (2005), used both family and child fixed effect models to account for unobserved confounders in both family and child. Her study, using the offspring of the U.S. National Longitudinal Survey of Youth, 1979, focused on the association between family complexity, specifying the presence of half- and stepsiblings, and the cognitive outcomes of 5- to 10-year-old children. The inclusion of family fixed effects eliminated the significance of the adverse association for children living in a blended family (i.e. where children did not all share the same parents). The further control for child fixed effects showed a borderline significance for blended families and still significant deficits for children who were or had previously been in single-parent families.

Fixed effects estimation has thus proved a promising approach to address the problem of selection in research on the possible effects of family structure on children's outcomes. However, it cannot account for unobservable factors that vary over time within families or children. It also relies on a subset of observations displaying variation over time or within families, a point to which we return later. Intergenerational data on parents' antecedent attributes are also suitable to address selection (Fomby & Cherlin, 2007), but are rarely available. Alternative approaches include the use of hierarchical linear modeling (random effects; Gibson-Davis & Gassman-Pines, 2010) or propensity score matching (Frisco, Muller, & Frank, 2007). We chose the approach of fixed effects rather than hierarchical modeling, despite the latter making more use of observed information across the whole sample, because the specification of fixed effects is more likely to eliminate selection, although of course it may not do so entirely. Propensity score matching is also not perfect at eliminating selection bias, and insofar as it requires a binary treatment it would not be suitable to compare multiple family forms.

## The Present Study

In this article, we explore the fixed effect approach as applied to the emotional and behavioral outcomes in mid childhood of children in the United Kingdom in the early 21st century, extending literature that is almost exclusively on the United States. With greater family stability and a more generous welfare system in the United Kingdom, it cannot be assumed that the outcomes of family complexity are similar in the two contexts. We investigate the following three research questions:

1. Did children who lived with both biological parents and only full siblings have better emotional and behavior outcomes than children who lived with siblings in other family forms?
2. Does the presence of nonfull siblings have adverse consequences for children's emotional and behavior outcomes beyond those of parents' union status?
3. Do any identifiable impacts affect girls and boys alike?

We look for independent effects both of parents' union status and sibling composition on well-being at a range of ages in mid childhood. Complex sibling relationships cut across different family structures: Not all stepfamilies include a shared child, and children not sharing parents may be living with a single parent. Therefore, we distinguish between families with two biological parents, stepparents, and single parents and between full and nonfull siblings. By classifying children on the basis of their relationships to both parents and siblings, we contribute to an emerging literature seeking to account more fully for family complexity (Brown et al., 2015). Our methodological contribution also includes the exploitation of an underused element of the U.K. Millennium Cohort Study: a repeated measurement of outcomes at two points for multiple children in the same family. This allows using family and child fixed effect models to account for unobserved heterogeneity at both levels. This approach gets us closer to placing a causal explanation on the results, but does not guarantee it.

Although we only have follow-up data after 2 years, our focus on mainly primary school-age children and their social–emotional adjustment is of interest given that these early outcomes are predictive of children's future success (Layard, Clark, Cornaglia, Powdthavee, & Vernoit, 2014; Ram & Hou, 2005). It also complements prior research, which has paid more attention to adolescents than to younger children and, with the exception of Tillman (2008), has seldom had a large enough sample to explore the differences between boys and girls, despite their different patterns of behavior difficulties and relationships with siblings.

## Method

### *Data and Sample*

We use data from the U.K. Millennium Cohort Study (MCS; http://www.cls.ioe.ac.uk/mcs). The MCS is a longitudinal survey following a nationally representative sample of 19,000 children born in the United Kingdom in 2000 to 2002 (Joshi & Fitzsimons, 2016). MCS has been tracking cohort members (CMs) since the age of 9 months. Survey data have been collected throughout the United Kingdom on six occasions up to the age of 14 so far. At the second and third waves (mainly in 2004 and 2006, respectively), when the CMs were aged 3 and 5 years, the data were collected on one or two of their coresident older siblings younger than the age of 15 years, in the context of a study of school-aged children (Plewis, 2007). This included information on older siblings' social and emotional adjustment, which was measured by the same questions as used for the CMs. Hence data on multiple siblings per family are available from two occasions, allowing us to estimate models with family and child fixed effects to account for unobserved heterogeneity both in families and individual children.

Our analytic sample includes 14,833 children nested in 6,464 families and observed at two points in time, resulting in 29,666 child–occasion observations. The children are divided between 6,435 CMs and 8,398 older siblings; 4,530 CMs have only one older sibling in the data set and 1,905 have two, whereas 58 older siblings (29 pairs) are included without any data on the CM. By construction, children included in the analytical sample are not evenly distributed across ages. CMs are within a few months of their third and fifth birthdays, whereas their older siblings are spread across a wider age range, although siblings aged 2 to 3 years older than the CM are prevalent.

To be included in our analytical sample, children needed (a) to have valid measures of either emotional or behavioral problems at both waves and (b) to be in sibling pairs or triads, so that two or three children per family are analyzed. Thus, although children from the MCS are representative of their birth cohort, our sample is not because it excludes CMs who did not have older siblings. However, although the sample could include only two or three siblings per family, our measure of sibling structure takes into account all coresident children in the family. Further exclusions from the analytical sample are children for whom information on any of the covariates was missing ($n = 593$), those children who were the twin or triplet of a CM included in the analysis ($n = 77$), and those children who were not reported by the biological mother of the cohort child at both surveys ($n = 255$). This last restriction is common to other studies in this field (such as Ram & Hou, 2005) and helps simplify interpretation. It excludes a small number of highly atypical families and ensures that the dependent variable is reported by the same person on both occasions. The sample sizes reported in Tables 1–4 and the supplementary material vary slightly according to which outcome is analyzed.

### *Variables*

*Children's well-being.* We investigate children's emotional (internalizing) and behavioral (externalizing) problems. Internalizing problems include being withdrawn, feeling fearful or too dependent, or being bullied. Externalizing behavior reflects how far children turn problems outward ("act out") and includes difficulties in interacting with other people. Internalizing and externalizing problems are not mutually exclusive, as children can exhibit both.

At each wave, these outcomes were measured by the mother's report on the Strengths and Difficulties (SDQ) questionnaire (Goodman, 1997; http://www.sdqinfo.com). Designed as a screening tool, high scores on the SDQ scale are predictive of clinically identified mental health disorders (Goodman, Ford, Simmons, Gatward, & Meltzer, 2000). The SDQ internalizing scale sums responses to five items each for emotional problems (e.g., "nervous or clingy in new situations") and for peer problems (e.g., "tends to play alone"). The SDQ externalizing scale sums responses to five items each for conduct problems (e.g., "often has temper tantrums") and for hyperactivity (e.g., "constantly fidgeting"). Each item is marked on the following three-point scoring system: 0 = "not true," 1 = "somewhat true," 2 = "very true." Each scale ranges between 0 and 20, with higher values indicating greater problems. The two scales are internally consistent and demonstrate good reliability; Cronbach alphas range between .61 and .83.

*Family type classification.* The main independent variable of interest is family type, defined from the perspective of each child included in the sample. We expand the definition of family type commonly used in studies of family structure and take into account the child's relationships both with parents and with coresident siblings. We classify a child's family type by the usual threefold classification of family structure: two biological parents, stepparents, single parent. We label any two-parent family where the biological mother lives with a partner other than the child's father as a *stepfamily*, whether or not the respondents use this terminology. Given our sample restriction, all single parents are single mothers and almost all stepparents are stepfathers. We further split each of these three categories into the following two configurations of coresident siblings: all full siblings and any nonfull sibling. As there are too few stepsiblings among coresident children to treat them separately from half-siblings, the resulting classification has six categories.

We derive the family type classification using the matrix of relationships in each of the two waves of MCS before restricting the data set to our analytical sample. This matrix includes all people living in the household and provides information on their gender, age, and relation to each other. This shows how each child is related to the parental adults and to each other child in the household. The family complexity variable

Table 1. *Family Type: Prevalence and Internalizing and Externalizing Problems by Wave*

| | Unweighted frequency | | Weighted percentage | | Average internalizing | | Average externalizing | |
|---|---|---|---|---|---|---|---|---|
| Family complexity | Wave 2 (1) | Wave 3 (2) | Wave 2 (3) | Wave 3 (4) | Wave 2 (5) | Wave 3 (6) | Wave 2 (7) | Wave 3 (8) |
| Two biological parents, only full siblings | 10,464 | 10,018 | 69.6% | 66.6% | **2.7** | **2.4** | **5.0** | **4.0** |
| Two biological parents, any nonfull siblings | 1,094 | 1,009 | 7.7% | 7.0% | **2.8** | 2.4 | **6.8** | **4.8** |
| Stepparent, only full siblings | 136 | 162 | 1.0% | 1.2% | **3.6** | **3.3** | **6.9** | **5.7** |
| Stepparent, any nonfull siblings | 1,100 | 1,216 | 7.9% | 8.8% | **3.5** | **3.4** | **6.3** | **5.7** |
| Single mother, only full siblings | 1,204 | 1,479 | 8.1% | 9.9% | **3.6** | **3.4** | **6.7** | **5.5** |
| Single mother, any nonfull siblings | 835 | 949 | 5.7% | 6.6% | **4.1** | **3.9** | **7.5** | **6.3** |
| *n* | 14,833 | 14,833 | 14,833 | 14,833 | 14,681 | 14,681 | 14,727 | 14,727 |

Data in columns 1 to 4 are weighted means and unweighted frequencies, and the sample is the analytical sample. Data in columns 5 to 8 are average internalizing and externalizing scores, with data in bold indicating a significant difference (at $p < .05$) against the first family complexity category within each column. Sample restricted to observations with valid internalizing and externalizing outcomes, respectively.

takes into account the presence of all coresident siblings at the two points in time, irrespective of whether the individual child is included in the analysis. For example, a pair of full siblings living with their lone mother would be assigned to the "single parent, any nonfull sibling" category if there is also a younger half-sibling, who would not, however, be included in the analytical sample. Among all siblings present in the family matrix, contributing to sibling structure of the analytical sample, there were approximately 1,600 nonfull siblings of the CM. They were mostly half-siblings, with only 15 stepsiblings, foster siblings, or adoptive siblings at Wave 2 and 13 at Wave 3.

This family typology does not differentiate between married and cohabitating couples. Such a distinction is of interest and importance in the U.S. context (Hofferth, 2006; Manning & Brown, 2006; Raley & Wildsmith, 2004), but is arguably less salient in the United Kingdom. Although in the United States cohabiting mothers are similar to single mothers, in the United Kingdom they resemble married mothers more nearly, both in term of relationship stability and level of socioeconomic resources (Crawford, Goodman, Greaves, & Joyce, 2012; Kiernan, McLanahan, Holmes, & Wright, 2011).

*Control variables.* Our analysis includes several controls, the choice of which was informed by previous research on behavioral and emotional adjustment using MCS data (Joshi & Fitzsimons, 2016). Child's age was measured in years as a continuous variable, along with its square to account for nonlinearity in children's developmental trajectories. Child's gender was coded 0 for girls and 1 for boys. Number of siblings counts all resident siblings younger than the age of 16 years, a continuous variable ranging from 1 to 12. Similarly, age order was calculated among all resident siblings younger than the age of 16 years and was capped at 5. Ethnicity is that of the mother, coded into one of the following six categories: (a) White (reference), (b) mixed, (c) Indian, (d) Pakistani or Bangladeshi, (e) Black Caribbean or Black African, (f) other. Family income was measured as the log of the joint equivalized net weekly income of the mother and the partner in each wave. Mother's depression was measured with the Kessler K6 scale (http://www.hcp.med.harvard.edu/ncs/k6_scales.php), a scale of six symptoms potentially ranging from 0 to 24 with mean of 3. Employment status of parents in the household was measured in the following four categories at each wave: (a) no one in work (reference category), (b) one of two in work, (c) two in work, (d) one of one in work. A dummy variable captured residential mobility between Waves 2 and 3 (0 = "no move," 1 = "moved home"). Maternal education was coded in six categories along the standard U.K. national classification of educational or vocational qualifications (0 = *no qualification* to 5 = *postgraduate degree*). Maternal age, at interview, was also included as a continuous variable in years, allowing for nonlinearity by including its square.

### *Analytical Strategy*

Taking the sample of children in families where there were at least two children with measured outcomes, we ran descriptive analyses of the children's emotional and behavioral outcomes by family type separately by wave to register change over time. We then estimated the effect of family complexity on child outcomes using two types of regression analyses: OLS models and fixed effects models. The first OLS model (Model 1) includes only family complexity and the following child characteristics: age, age squared, and gender. The second OLS model (Model 2) adds all other covariates to capture family characteristics and control for differences in resources across family types. A wave dummy variable is included in both models to control for any changes in the economic environment and any wave-specific features of the data collection.

We also estimated fixed effects models to account more fully for unmeasured time-invariant factors. The family fixed effects, estimated in Model 3, subtract the values for each child and observation from the family mean for four to six observations. This model abstracts from time-invariant characteristics at the family level. Around 5% of the families had siblings with differing values on the complexity variable. Such differences stem partly from the asymmetric situations of half-siblings, with some children living with both biological parents while their half-siblings live with one biological parent and one stepparent. They also stem from changes in parents' partnership between the two waves. It is these differences that permit us to estimate the different associations of different family states with the outcomes.

Both child and family and child fixed effects are incorporated in Model 4, whereby each child's values net of the family mean are also subtracted from their own mean across the two surveys. The child fixed effects model relies on the changes of family complexity within the child. Such changes occurred to 9% of children and were a result of changes between waves in the presence of parents (either an arrival or departure) or occasionally of siblings. Although the estimates are produced by exploiting change over time, they capture the effect of being in a specific family type, ignoring the trajectory producing it (Gibson-Davis, 2008). Accounting for unobserved family and child characteristics in this way greatly reduces the influence of unmeasured factors that jointly determine family type and child outcomes, making a causal interpretation of the effect of family complexity more plausible (Andrews, Schank, & Upward, 2006). Selection bias might still arise if there are time-varying unobserved characteristics at the child and family levels that are correlated with family complexity. Given the relatively large size of our sample compared to many used in the literature, we investigated our third research question, about gender differences, by estimating Model 4 separately for boys and girls.

The OLS estimation offers evidence about the observable characteristics accounting for the raw association between family forms and child outcomes. Comparing them to the fixed effects models helps reveal the role of unobservable characteristics. The OLS models are estimated using the "regress" command in Stata 13 (StataCorp., 2013). The commands "areg" and "reghdfe" estimate the family fixed effects and the family–child fixed effects models, respectively. All regressions account for both sample design and attrition (Ketende & Jones, 2011). Variables that are constant over waves drop out in the fixed effects models. In our case, mother's ethnicity and sampling stratum drop out in Models 3 and 4, and the child's gender drops out in Model 4. Ages of child and mother remain, as the interval between interviews varies slightly across respondents.

## Findings

### *Descriptive Statistics*

Table 1 reports the distribution of different family types in the analytic sample at Waves 2 and 3, along with mean internalizing and externalizing scores for each family type. A nonnegligible proportion of children were in single or stepparent families. When the cohort children were aged 3, nearly 70% of those who were in this sample lived in nuclear families with full siblings only; 8% each lived with two biological parents and at least one nonfull sibling, with stepparents and at least one nonfull sibling, and with single parents with only full siblings; 6% were with single parents and at least one nonfull sibling; a few (1%) lived in stepparent families with only full siblings. The proportion living with both biological parents was similar to the cohort as a whole, and along with it, declined between waves. The four categories without the biological father increased modestly, by four percentage points, reflecting the separation and repartnering of parents during the 2 years. It is also worth noting that by differentiating among siblings, we were able to identify children in blended families who would otherwise be considered to be living in traditional families with two biological parents (7.7% in Wave 2 and 7.1% in Wave 3).

When it comes to internalizing and externalizing problems, the descriptive results showed children in complex families exhibiting more problems of both sorts, in both waves, than those in the nuclear families. The difference was significant in all cases, except for children with natural parents and nonfull siblings for internalizing problems in Wave 3. The highest scores were found among children living with a single parent and nonfull siblings. Across all family types, mean externalizing problems were higher than internalizing at each wave. The gap between the two shrank over time; whereas internalizing problems were more or less stable, externalizing problems fell towards Wave 3, reflecting a generally higher prevalence in early than later childhood.

Additional cross tabulation of family complexity by key control variables (shown in the supplementary material, Tables A and B) indicated that sampled children in traditional, nonblended nuclear families were the most advantaged group. Single- and stepparent families had relatively lower income, higher levels of reported maternal depression, and lower levels of parental education and employment. Furthermore, internalizing and externalizing problems were higher among children living in disadvantaged circumstances, suggesting that families with fewer resources, economic and psychological, will also invest materially and emotionally less in these children.

### *Regression Results*

*Internalizing problems.* The first OLS model in Table 2 confirmed that children in complex families exhibited more internalizing symptoms than the traditional reference group. The difference (minimally adjusted) was greatest for children with single parents followed by those with stepparents. Moreover, children in families with nonfull siblings tended to fare worse than their counterparts in families with full siblings (except in stepparent families). This pattern, although intuitive, cannot be interpreted as causal given the need to account for the drivers of family complexity.

The association of family complexity with socioeconomic disadvantage and maternal depression suggests that child outcomes might have common causes with these factors, in line with previous work on MCS (Kiernan & Mensah, 2010). The inclusion of family characteristics and circumstances in Model 2 reduced the estimates for family complexity. However, despite this attenuation, the association between family complexity and children's problems remained positive and significant for those living with nonfull siblings in step- and single-parent families.

Moving to the fixed effects specifications, the results with family fixed effects (Model 3) showed that children with single parents had significantly raised internalizing problems, particularly if there were nonfull siblings. The coefficients for these two categories were larger in Model 3 compared to the fully controlled OLS regression. The estimate for children living in stepparent families with nonfull siblings became nonsignificant. When child fixed effects were included along with family fixed effects (Model 4), the estimates for single-parent families increased, and the estimate for stepparent families with nonfull siblings, somewhat unexpectedly, regained borderline significance. If expressed as a percentage of the standard deviation on the internalizing score, the effect size for children in single-parent families was 0.31 of a standard deviation for those with nonfull siblings and 0.18 of a standard deviation for those with only full siblings. In general, the effect sizes for the internalizing outcome were relatively small.

*Externalizing problems.* Turning to the results on the more prevalent externalizing problems in Table 3, Model 1 showed that children in all five nontraditional family categories fared worse than the reference group. The higher scores were for children living with nonfull siblings (compared to those living with full siblings given the same type of parents). Children of single parents living with nonfull siblings exhibited the highest problem score. These associations were stronger in magnitude than for internalizing problems.

The introduction of the controls in Model 2, as with internalizing problems, attenuated but did not eliminate the adverse estimates of family complexity. In other words, some of the impact

Table 2. *Regression Results for Internalizing Problems Strengths and Difficulties Scores*

| Internalizing problems | OLS, Model 1 | | OLS, Model 2 | | Family FE, Model 3 | | Family–child FE, Model 4 | |
|---|---|---|---|---|---|---|---|---|
| Family complexity; reference: 2 natural parents, only full siblings | | | | | | | | |
| Two biological parents, any nonfull siblings | 0.21*** | (0.069) | 0.047 | (0.069) | 0.11 | (0.265) | 0.34 | (0.264) |
| Stepparent, only full siblings | 0.80*** | (0.249) | 0.32 | (0.240) | 0.55 | (0.363) | 0.64* | (0.337) |
| Stepparent, any nonfull siblings | 0.56*** | (0.087) | 0.22** | (0.087) | 0.32 | (0.269) | 0.36 | (0.262) |
| Single mother, only full siblings | 0.84*** | (0.072) | 0.091 | (0.112) | 0.45** | (0.189) | 0.52*** | (0.162) |
| Single mother, any nonfull siblings | 1.25*** | (0.096) | 0.36*** | (0.125) | 0.68** | (0.310) | 0.87*** | (0.272) |
| Boy | 0.16*** | (0.037) | 0.17*** | (0.036) | 0.24*** | (0.041) | – | – |
| Child's age | 0.26*** | (0.031) | 0.12*** | (0.037) | 0.040 | (0.036) | −0.29*** | (0.066) |
| Child's age squared | −0.011*** | (0.002) | −0.0042** | (0.002) | −0.0015 | (0.002) | 0.0099*** | (0.002) |
| Child age order based on relationship matrix; reference: oldest in household | | | | | | | | |
| Second | – | – | −0.44*** | (0.055) | −0.57*** | (0.060) | −0.40 | (0.279) |
| Third | – | – | −0.49*** | (0.085) | −0.81*** | (0.114) | −0.61 | (0.409) |
| Fourth | – | – | −0.47*** | (0.129) | −0.76*** | (0.172) | −0.81 | (0.566) |
| Fifth or more | – | – | −0.45** | (0.221) | −1.25*** | (0.321) | −0.26 | (0.734) |
| Number of coresident siblings | – | – | 0.071** | (0.032) | 0.076 | (0.084) | 0.043 | (0.078) |
| *n* | 29,362 | | 29,362 | | 29,362 | | 29,362 | |
| $R^2$ | 0.051 | | 0.135 | | 0.514 | | 0.788 | |

*Notes.* Standard errors in parentheses. Models 2 to 4 also include parental education, mother's age, ethnicity and depression, family income and employment, mobility, sampling stratum, and a wave dummy (see Table C in the appendix for full results). FE = fixed effects; OLS = ordinary least squares.

$^{*}p < .10$. $^{**}p < .05$. $^{***}p < .01$.

of family complexity was accounted for by the adverse circumstances of nontraditional families (i.e., low parental education, low employment, and high maternal depression).

The inclusion of family fixed effects in Model 3 largely eliminated the estimated association with family complexity. Most of its impact was accounted for by these unmeasured time-constant factors in family circumstances or behaviors. When fixed effects within child as well as family were included, Model 4, two of the five estimates went up and regained significance, again unexpectedly. Children in stepparent families with full siblings were more affected (0.24 of a standard deviation) than those living in single-parent families with full siblings (0.13 of a standard deviation)—each relative to the traditional reference category. The interpretation of this counterintuitive result is discussed later.

*Controls.* When it comes to the controls, the full results of Model 2 (reported in the supplementary material) showed that boys, children living in families with less education, higher levels of maternal depression, less income, or less employment exhibited higher problems.

Patterns by child age were nonlinear. Internalizing problems rose until reaching a peak around age 15, and externalizing problems fell until reaching a minimum around age 9 and then started to rise. The rank by age of children in the household (normally birth order) showed significant differentials, in opposite directions for each outcome, independently from the child's age. The senior children in the household showed more internalizing and fewer externalizing problems. Before controlling for family heterogeneity, internalizing problems increased with the number of siblings, whereas externalizing problems declined.

Children living in families where the main respondent was Indian, Pakistani, or Bangladeshi exhibited higher levels of internalizing problems, whereas those in families with mixed-race, Pakistani, Bangladeshi, or Black respondents exhibited lower externalizing problems than the White majority group. Both internalizing and externalizing problems were high for young mothers and dropped until reaching a minimum at current maternal age around 37 and 41 years, respectively. Home moves in the period preceding the survey, included as a potential measure of family stress, did not

Table 3. *Regression Results for Externalizing Problems Strengths and Difficulties Scores*

| Externalizing problems | OLS, Model 1 | | OLS, Model 2 | | Family FE, Model 3 | | Family–child FE, Model 4 | |
|---|---|---|---|---|---|---|---|---|
| Family complexity; reference: 2 biological parents, only full siblings | | | | | | | | |
| Two biological parents, any nonfull siblings | 0.86*** | (0.103) | 0.40*** | (0.101) | −0.14 | (0.390) | 0.24 | (0.335) |
| Stepparent, only full siblings | 1.67*** | (0.305) | 0.78*** | (0.298) | 0.74 | (0.491) | 0.95*** | (0.364) |
| Stepparent, any nonfull siblings | 1.88*** | (0.117) | 1.13*** | (0.115) | 0.31 | (0.399) | 0.46 | (0.333) |
| Single mother, only full siblings | 1.45*** | (0.099) | 0.21 | (0.150) | 0.49* | (0.277) | 0.52*** | (0.199) |
| Single mother, any nonfull siblings | 2.25*** | (0.125) | 0.72*** | (0.165) | 0.25 | (0.470) | 0.27 | (0.360) |
| Boy | 1.27*** | (0.050) | 1.28*** | (0.048) | 1.43*** | (0.057) | – | – |
| Child's age | −0.71*** | (0.042) | −0.54*** | (0.048) | −0.55*** | (0.048) | −1.05*** | (0.103) |
| Child's age squared | 0.034*** | (0.002) | 0.029*** | (0.003) | 0.032*** | (0.003) | 0.046*** | (0.002) |
| Child age order based on relationship matrix; reference: oldest in household | | | | | | | | |
| Second | – | – | 0.63*** | (0.072) | 0.73*** | (0.080) | −0.029 | (0.364) |
| Third | – | – | 0.45*** | (0.113) | 0.74*** | (0.154) | −0.17 | (0.483) |
| Fourth | – | – | 0.82*** | (0.182) | 1.32*** | (0.235) | 0.039 | (0.626) |
| Fifth or more | – | – | 0.96*** | (0.291) | 0.95** | (0.394) | 1.30 | (0.888) |
| Number of coresident siblings | – | – | −0.10** | (0.041) | 0.094 | (0.119) | 0.11 | (0.094) |
| $n$ | 29,454 | | 29,454 | | 29,454 | | 29,454 | |
| $R^2$ | 0.112 | | 0.187 | | 0.519 | | 0.839 | |

*Notes.* Standard errors in parentheses. Models 2 to 4 also include parental education, mother's age, ethnicity and depression, family income and employment, mobility, sampling stratum, and a wave dummy (see Table D in the appendix for full results). FE = fixed effects; OLS = ordinary least squares.

$^{*}p < .10$. $^{**}p < .05$. $^{***}p < .01$.

affect the outcomes when other controls were considered.

Note that most family- and child-level controls lost significance in the fixed effects models. This happened because most of these variables had limited variations just during the 2 years separating Waves 2 and 3. The only control variable to retain significance, in the fixed effects models, although attenuated, was maternal depression, as measured by the Kessler scale score. Here there was some variation over time.

*Disaggregation by gender.* Table 4 reports further investigations, repeating Model 4 separately for boys and girls. The findings showed that the estimated effects of single-parent families on internalizing behavior (apparent in Table 2) were confined to girls (0.29 of a standard deviation for girls living with full siblings, and 0.47 for those living with nonfull siblings). Although girls' internalizing problems appeared not to be affected by living with a stepfather, boys had a significant estimate in the small group of stepfamilies with only full siblings. Further analysis of the subscales (not shown) suggested that the main impact for daughters of single mothers was in peer problems.

The impact on externalizing problems seen in Table 3 was driven by variations within boys (0.25 of a standard deviation for boys living in stepparent families with full siblings, and 0.11 for boys living in single-parent families with full siblings), with no significant estimate for girls. The subscale most affected for boys was conduct problems (not shown). Although family complexity effects varied by gender, the estimates for birth order (also not shown) did not.

## Discussion

Our aim was to contribute to an emerging literature on family complexity, going beyond traditional classification of family structure and bringing siblings' composition into focus. Unlike previous studies, our evidence is based on the United Kingdom, where evidence on family complexity has not been fully exploited. The association of nontraditional family situations with mid childhood problems recorded for our sample of siblings was, as expected, greatly reduced by controlling for observed and unobserved family and child fixed effects, but the attenuation is not complete. Some aspects of family complexity still had statistically

Table 4. *Results for Internalizing and Externalizing Problems by Gender (Family–Child FE Model)*

| | Boys | | | | Girls | | | |
|---|---|---|---|---|---|---|---|---|
| Family complexity | Internalizing | | Externalizing | | Internalizing | | Externalizing | |
| Two parents, any nonfull siblings | 0.19 | (0.405) | 0.83* | (0.459) | 0.54 | (0.354) | −0.22 | (0.467) |
| Stepparent, only full siblings | 1.20** | (0.517) | 1.42*** | (0.542) | 0.041 | (0.406) | 0.53 | (0.469) |
| Stepparent, parent, any nonfull siblings | 0.34 | (0.381) | 0.72 | (0.489) | 0.49 | (0.359) | 0.22 | (0.444) |
| Single mother, only full siblings | 0.31 | (0.232) | 0.54** | (0.270) | 0.78*** | (0.223) | 0.46 | (0.296) |
| Single mother, any nonfull siblings | 0.55 | (0.402) | 0.64 | (0.518) | 1.29*** | (0.372) | −0.042 | (0.504) |
| *n* | 14,996 | | 15,054 | | 14,366 | | 14,400 | |

*Notes.* Standard errors in parentheses. Model also includes parental education, mother's age, ethnicity and depression, family income and employment, mobility. FE = fixed effects.

$^{*}p < .10$. $^{**}p < .05$. $^{***}p < .01$.

significant positive estimates. Single parenthood had three of four significant estimates of raised problems in the sample pooling girls and boys when compared with families with both biological parents and only full siblings. The presence of a stepfather appeared to raise both internalizing and externalizing problems, but not where the families were blended (i.e., two of four). There was very little significant contrast, given the parental situation, between blended families and those with only full siblings. A blended, two-parent family that appeared to be intact unless the mixed parentage is noted could be a source of family complexity that is hidden in conventional statistics. Such blended families here had different outcomes from intact families with only full siblings in the unadjusted model, but these were generally accounted for in the adjusted models. The lack of findings for independent effects of sibling structure tells against the hypothesis that parents discriminate between resident children by blood relationship, but may reflect the limitations of our sample, noted later.

The conjecture that results would vary by gender was confirmed in that the effects detected on externalizing problems affected boys, and those on internalizing problems mainly involved girls with single mothers. This is consistent with the idea that girls and boys react differently to stress, but could reflect variations in parenting by the sex of child and of the parent.

The finding of significant estimates in our fixed effects models supports a causal interpretation of at least some of the crude correlation between family complexity and these outcomes. Unless there are omitted time-varying confounders, perhaps in a reciprocal influence of child and family difficulties, we can infer that some features of family complexity act to raise children's internalizing and externalizing problems relative to noncomplex families. Furthermore, against expectations, some estimates for family complexity increased when the fixed effects were included in Models 3 and 4 in comparison to Model 2 (OLS with all controls). Usually the inclusion of fixed effects would attenuate the impact on the dependent variable (as in Gennetian, 2005). This was not the case here.

We offer this interpretation. Family complexity is positively associated with adverse circumstances such as low employment, low education, low income, and maternal depression. In other words, both family complexity and adverse circumstances are associated with the outcome in the same direction. Family fixed effects account for both the measured and unmeasured adverse circumstances of the family. They also account for any unmeasured correlates of complexity that affect the outcome in the opposite direction (which we term *family mitigating factors*). These might include proactive parenting, parental resilience, and good temperaments. If adverse factors dominate, the estimated impact of family complexity will decline in a fixed effect estimation over OLS, but if mitigating factors dominate, it will rise. Some estimated impacts did rise for single parents in the family fixed effects models. Therefore, the family fixed effects on internalizing problems in single-mother families, for example, were dominated by mitigating factors in the family. This finding suggests that some single-mother families might have been drawing on unobservable strengths, allowing them to cope better than expected with their situations.

When the child fixed effects were introduced in Model 4, the estimates for internalizing problems of children living in single-mother families rose even further above Model 2, although not back to the level of the crude association in Model 1. This indicates that some individual children of single mothers display their own mitigating characteristics, strengthening their capacity to overcome family stresses. Some children in such families (and perhaps some stepfamilies) may display personal strengths and rise to the challenges of their situations. For those who do not, the adverse consequences are revealed by these estimates.

The latent capacity to cope in adverse circumstances is difficult if not impossible to measure directly in a survey. One way to infer such resilience in cross-sectional data, cited by Masten (2001), is to estimate moderating influences of measured variables using interactions, but this is not always very successful and does not capture unobserved characteristics. Provided they do not change during the time period concerned, these can be allowed for in data such as ours, which permit the simultaneous use of family and child fixed effects. Most cross-sectional studies, including those with multiple siblings per family, will give an incomplete and possibly inaccurate picture of the impact of family complexity. The findings from Model 4 are the novel contribution of this study because they can be interpreted as estimating the causal impact of family complexity net of family circumstances and net of the relevant unobserved characteristics of the family and child.

### *Limitations*

Our study relies on a subsample of MCS children who have coresident siblings. Our results therefore apply only to multichildren families—a case of only one child is not considered. Moreover, some complex family types are more represented than others. There were too few stepsiblings in the data to distinguish from half-siblings. Our sample was relatively young; stepsiblings may be more common at a later stage. Nevertheless, the response rates to the survey and restrictions on our sample most likely mean that complex relationships are underrepresented even at these ages. The requirement that the children analyzed should be living with their natural mother at two consecutive surveys could have disqualified some of the most disrupted families. The sample nevertheless provides reasonable evidence on family structure during early childhood within a large number of multichild households and sizable absolute sample sizes for cases experiencing change.

Our chosen method of two-level fixed effects has its limitations as well. The variation underlying the estimates is produced by the subset of observations that change family form over time or contain within-family differences. The 2-year time limits the number of changes we can observe. However, in our sample this number is not negligible. With data confined to two snapshots, we cannot see any long-term effects or detect persistent family instability. Fixed effects models do not capture relevant unobservable characteristics that change over time in a family, child, or interpersonal relationship. On the other hand, they may overcontrol for factors that would be better understood by measuring them rather than differencing them out, as in a random effects approach, but our prime concern is to investigate selection into family types.

The scope of our analysis is also limited. We have not attempted any allowance for the marital status of two-parent families or the gender configuration of siblings (although we do investigate gender difference in effects of family complexity). Our evidence on outcomes concerns behavioral and emotional difficulties, not, for example, cognitive attainment. We rely on mother reports of the SDQ, which might include some measurement error despite our inclusion of maternal depression in the regressions. We are not able to explore many aspects of family structure that are likely to be important to children, such as duration, frequency of changes, parenting practice, and intrafamily conflict, or relations with any parents or siblings living elsewhere. These will not be picked up in fixed effects if they change over time, and investigating them would bring some further insights into the mechanisms underpinning the effects of family structure we uncover.

### *Conclusion*

We have asked how the complex structure of modern families affects child well-being. We investigate how parental partnership and the relatedness of siblings may impact on children's internalizing and externalizing behavioral problems. The inclusion of sibling configuration is warranted by increasing multipartner fertility and evidence of effects on children's well-being. We account for children's growing experience of diverse sibling relationships in single- and two-parent families. In doing so, we contribute to an emerging line of research that widens the lens on family structure and relates children's well-being not only to their relationships to parents but also to their siblings. Another novelty resides in addressing the problem of selection into family types by our use of the fixed effect method to account for unobserved family and child heterogeneity. We present evidence for the United Kingdom from a little known data set in which this exercise is possible.

Even in mid childhood, about one third of the children studied lived in complex families with either nonnatural parents or nonfull siblings. Such nontraditional living arrangements were associated with internalizing and externalizing problems. They were also associated with adverse material circumstances and maternal depression. As in other literature, including analyses of the entire Millennium Cohort, these factors appeared to account in major part for the higher level of problems, particularly externalizing problems, in complex families. Although we concur that family resources and family stress formed a major reason for children's poorer scores in nontraditional families, our exploration of fixed effects adds the suggestion of some genuine effects of family complexity on mental health for some more vulnerable children and an ability to cope in others.

When compared with traditional, two-parent families, family complexity appeared to lead to more internalizing problems among children living with single parents, especially girls with nonfull siblings, and some in stepfamilies. For externalizing problems, there appeared to be effects among boys living with full siblings in both step- and single-parent families. The order of magnitude of those effects we have detected is modest.

The inclusion of family and child fixed effects in the regression models made it possible to control for latent family and child predispositions that mitigate against child mental problems, assuming such predispositions do not change over time. Such mitigating factors may include attributes such as good temperament, coping skills, including resilience in face of adversity, intrafamilial affection, and possibly positive selection into nontraditional family types. Parents and children may well be forced to draw on such mitigating factors when exposed to adverse circumstances. However, the presence of these factors has seldom if ever been detected in previous studies, which lacked appropriate data. This study showed that such protection can occur and can mask potentially negative effects of living in some types of complex families.

Although it is data on siblings that enables us to infer these otherwise unobserved processes around family structure, our second research question asked whether the biological relationship between siblings made any difference to child outcomes on average. The evidence about shared parentage of the siblings did not suggest that having siblings who are not full biological relations necessarily increases the adverse effects of family complexity. In general, nonfull siblings seem to present little more trouble than full siblings. These findings from the sibling data set suggest it will be important to follow the development of mental health as the main cohort grows older, even if sibling comparisons cannot be updated. The indications here are that family resources and maternal mental health will be more important determinants of children's well-being than family complexity in itself.

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## Supporting Information

Additional supporting information may be found in the online version of this article:

**Table A:** Average income, maternal depression score, and number of siblings by family complexity.

**Table B:** Cross tabulation of family complexity by key variables in Waves 2 and 3.

**Table C:** Full regression results for internalizing problems SDQ scores.

**Table D:** Full regression results for externalizing problems SDQ scores.

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