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Family structure and children's achievements

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Abstract. In this paper we estimate the relationships between several outcomes in early adulthood (educational attainment, economic inactivity, early childbearing, distress and smoking) and experience of life in a single-parent family during childhood. The analysis is performed using a special sample of young adults, who are selected from the first five waves of the British Household Panel Survey (1991–95) and can be matched with at least one sibling over the same period. We also perform level (logit) estimation using another sample of young adults from the BHPS. We find that: (i) experience of life in a single-parent family is usually associated with disadvantageous outcomes for young adults; (ii) most of the unfavourable outcomes are linked to an early family disruption, when the child was aged 0–5; and (iii) level estimates, whose causal interpretation relies on stronger assumptions, confirm the previous results and show that, for most outcomes, the adverse family structure effect persists even after controlling for the economic conditions of the family of origin.

JEL classification: J12, I20, I30

Key words: Family structure, intergenerational links, siblings estimators

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1. Introduction

This paper asks whether a number of outcomes in early adulthood are associated with experience of life in a single-parent family during childhood. Economists have long identified the family as one the most important institutions in a market economy that fosters the transmission of income inequality over time (Knight 1935; Becker 1981) and may act as a potential income equaliser across family members (Griliches 1979). A large literature has extensively studied the effect of children on marital instability and other household behaviour (e.g., Becker et al. 1977; Browning 1992 for a survey). Only in recent years, however, has the intergenerational transmission of human and social capital received empirical attention, with studies that examine the correlation between children's outcomes as young adults and a variety of parental circumstances and events during childhood, including family structure.[1]

A legitimate concern with much of this new literature is that the estimated effect of childhood family structure on children's achievements might be spurious (Manski et al. 1992; Mayer 1997). This is due to the mutual association that family structure and children's outcomes share with some unmeasured true causal factor. For example, the association between having experienced life in a single-parent family and, say, experiencing difficulties in the labour market may not be necessarily the result of family structure during childhood. Rather, differences in labour market success may simply reflect the characteristics of families in which the children of single mothers are brought up. For a broad set of young people's outcomes (education, inactivity, early childbearing and health), we estimate both level (cross-sectional) models and family-specific fixed-effects models.[2]

Although the use of sibling data has become increasingly common in economics, most of the empirical studies linking parents' behaviour and children's attainments have not addressed the problem of unmeasured heterogeneity with sibling estimators.[3] We show that the effect of family structure on outcomes can be identified with sibling differences if family structure does not respond to "idiosyncratic endowments" of children. On this assumption, our sibling-difference estimates would measure the causal impact of childhood family structure on young adults' achievements. But note that, in addition to inherent differences between siblings (e.g., one born with a disability), differences between siblings in their idiosyncratic endowments include differences over time in parental attitudes and behaviour which may affect both family structure and children's outcomes. For example, the father may develop an alcohol addiction, giving rise to a situation in which an elder sibling spends only a small part of his childhood with an alcoholic father while the youngest has one for most of her childhood. The father's alcohol problem may directly affect investment in the youngest child, and her parents may also divorce because of it, thereby causing correlation between idiosyncratic endowments and family structure. Thus, while the assumption of no such correlation is weaker than the assumptions needed in the cross-sectional model (see the Appendix), it is still a strong one. A causal interpretation of the level estimates relies on even stronger assumptions, but such estimates are useful for comparing our results to those provided in other studies. It is, therefore, safest to interpret both sets of estimates as suggestive associations, with the sibling-difference estimates controlling for more aspects of family background than the level estimates, making them less contaminated by unmeasured factors associated with both family structure and children's outcomes.

Most of what we know of the relationship between family background and children's outcomes is from the United States. Little is known about British cohorts born since 1958. In a recent study, Kiernan (1997) finds that, among members of the 1958 birth cohort, divorce during childhood is associated with outcomes – for educational attainment, economic situation, partnership formation and dissolution and parenthood behaviour in adulthood – which we would generally interpret to be worse than the same outcomes for young adults from intact families. Similarly, the study by Fronstin et al. (forthcoming) suggests that a parental disruption adversely affects labour market outcomes of children at age 33 (in 1991). Their results indicate that the effect occurs primarily through decreased employment for men and decreased wage rates for women. These studies build on earlier research by Elliot and Richards (1991), Ní Bhrolcháin et al. (1994) and Kiernan (1996) on schooling and socio-economic performance, and Kiernan (1992) and Cherlin et al. (1995) on demographic outcomes. All use samples from the National Child Development Study (NCDS) for the 1958 birth cohort.

Our analysis uses, instead, a special sample of young adults from the first five waves (1991–1995) of the British Household Panel Survey (BHPS), who can be matched with at least one sibling over the same period.[4] These young adults are then linked to their mothers' family history collected in the 1992 wave, as well as to other information about the mother from the mother's interviews during the panel,[5] and they are followed over the panel years. We also analyse another sample, in which young adults need not be matched with a sibling, but, as in the Sibling Sample, they must live with their mothers for at least one of the five panel years. The BHPS data have some clear advantages over the NCDS data. They are a better reflection of contemporary impacts of family background, which may be particularly important for that of family structure, because the incidence of single parenthood is now much higher than among the parents of the 1958 cohort. Moreover, they allow for better (although not ideal) measurement of family economic circumstances.[6] More importantly, with the information on siblings, they allow us to control for unobserved additive effects that are shared by children who belong to the same family.[7]

We find that experience of life in a single-parent family is typically associated with unfavourable outcomes for young adults. They achieve lower educational levels, face higher risks of economic inactivity and early birth, and they also have higher chances of smoking and feeling more distressed. Family structure in early childhood (ages 0–5 of the child) has the strongest associations with all the outcomes under analysis (similar to the impact of income on outcomes found by Duncan et al. 1997).[8] All these results also emerge with level estimates, whose causal interpretation relies on even stronger identifying assumptions than in the case of sibling differences. Our level estimates are consistent with most of the available evidence in this literature. As in McLanahan (1997), they confirm that, for almost all the outcomes under analysis, experience in a single-parent family during childhood still matters after taking the economic circumstances of the family of origin into account.

The rest of the paper is organised as follows. Section 2 describes the data and the estimation procedures, Sect. 3 presents our main results, and Sect. 4 concludes. The Appendix outlines the identification problem that is common to all studies of the relationship between family structure during childhood and children's outcomes.

2. Data

2.1. Estimating samples and family background measures

The data come from a special sample selected using the first five waves of the British Household Panel Survey (BHPS). In Autumn 1991, the BHPS interviewed a representative sample of 5,500 households, containing about 10,000 persons. The same individuals are reinterviewed each successive year, and if they leave their original households to form new households, all adult members of these new households are also interviewed. Similarly, children in original households are interviewed when they reach the age of 16. Thus, the sample remains broadly representative of the population of Britain as it changes through the 1990s.[9]

The second wave (1992) of the BHPS contains retrospective information on complete fertility, marital and cohabitation histories for all adult panel members in that year. Our analysis proceeds as if all children lived with their mothers throughout their years of dependency, which we assume to be until their sixteenth birthday.[10] This information provides the basis for our family structure measure: whether or not the young adult spent time in a single-parent family during his/her childhood. A child is defined as being brought up in an intact family if he/she lived continuously with both biological (or adoptive) parents, up to his/her 16-th birthday. Thus, according to our definition, a child would have spent some time in a single-parent family if he/she ever lived with a biological or adoptive mother who was not cohabiting nor married before his/her 16-th birthday, either because of a partnership dissolution or because he/she was born outside of a live-in partnership and the mother did not cohabit or marry within one year of the birth.[11] This measure is also broken down by the timing of the start of a spell in a lone parent family, distinguishing between three different child developmental stages, ages 0–5, 6–10, and 11–16.[12]

By matching young adults with their mothers, we are also able to measure other family background characteristics that would be unavailable otherwise, such as age of the mother at the young person's birth, mother's education, parents' real income when the child was aged 16 (or the youngest age at which we observe the child living with parents), whether or not parents are owner-occupiers, and if so, house value when the child was aged 16, years spent at the address occupied by the child at age 16 and current age of the child. We also obtain information on the smoking behaviour of the mother at the time the mother and her young-adult child lived together, which will be used for one of the health-related outcomes defined below (smoking). In addition, the third wave (1993) of the BHPS contains retrospective information on job histories for all adult panel members.[13] With this information we determine the proportion of months that mothers worked in each of the three developmental stages and over the entire childhood of the young adults.[14]

In our analysis, we use two samples. The first sample (labelled as "Individual Sample") consists of 764 individuals who: (i) were observed living with their mother when aged 16 or 17 during any of the first five waves (1991–

1995) of the BHPS; and (ii) report full information on outcomes and family background measures. The coresidence condition (condition (i)) is imposed in order to match data on family background from the mother's record to her child. Because 95% of the panel members live with their parents when aged 16–17 (Ermisch 1996), our sample is likely to be random. For some of the outcomes (e.g., schooling and early childbearing), we impose additional restrictions that we will describe below.

The second sample (labelled as "Sibling Sample") consists of 411 individuals with full information on outcomes and background measures who: (i) coresided with their mother for at least one year during the first five waves; (ii) were born between 1965 and 1979; and (iii) can be matched with at least one sibling (or half-sibling). Imposing condition (iii) – which allows us to control for unobserved heterogeneity in terms of family- or mother-specific fixed effects – on the Individual Sample would leave us with only 74 sibling pairs, arguably too few to draw reliable inference. At the cost of introducing some selection bias (i.e., correlation of the unobservables associated with the dependent variable(s) and the sample selection process), we impose conditions (i) and (ii), thereby increasing the number of sibling pairs to 252.

Table 1 shows the distribution by age of the young adults in the two samples. All individuals in the Individual Sample are 21 years-old or less (because of our selection, they were born between 1974 and 1979), and evenly distributed by age. About three-fourths of the individuals in the Sibling Sample are 22 or less and only 8% are more than 25 years-old. Table 2 presents the summary statistics of the variables for all individuals in the two samples. These statistics are computed for the last available year in which the indivi-

Table 1. Distribution of individuals by age in the two estimating samples

Age	Individual sample			Sibling sample		
	N	Prop.	Cum. Prop.	*N*	Prop.	Cum. Prop.
16	125	0.164	0.164	55	0.134	0.134
17	137	0.179	0.343	46	0.112	0.246
18	138	0.181	0.524	52	0.127	0.372
19	123	0.161	0.685	36	0.088	0.460
20	125	0.164	0.848	54	0.131	0.591
21	116	0.152	1.000	46	0.112	0.703
22				29	0.071	0.774
23				25	0.061	0.835
24				17	0.041	0.876
25				12	0.029	0.905
26				8	0.020	0.925
27				8	0.020	0.944
28				11	0.027	0.971
29				6	0.015	0.985
30				6	0.015	1.000
All ages	764			411	1.000	

Note: Individual Sample refers to individuals who lived with their mother at least at one interview date when aged 16 or 17 during the first five waves of the BHPS. Observations are at the last available period. Sibling Sample refers to individuals who coresided with their mother at least at one interview date during the first five waves of the BHPS, were born between 1965 and 1979, and are observed in any of the sample years with at least one sibling. For each sibling, observations are at the last available period.

Table 2. Summary statistics of variables used in analysis

Variable	Individual sample		Sibling sample	
	Mean	Std. Dev.	Mean	Std. Dev.
Age 16	0.164			
Age 17	0.179			
Age 18	0.181			
Age 19	0.161			
Age 20	0.164			
Age 21	0.152			
Age	18.437	1.682	20.304	3.543
Year of Birth	1976.4	1.703	1974.6	3.669
Female	0.476		0.418	
Ever in single-parent family	0.325		0.212	
Ever in single-parent family:				
child's age 0–5	0.200		0.090	
child's age 6–10	0.072		0.071	
child's age 11–16	0.052		0.051	
Mother has O level	0.346		0.350	
Mother has A level	0.153		0.124	
Mother has higher qualification	0.109		0.066	
Prop. of mother's time worked	0.434		0.375	
Prop. of mother's time worked:				
child's age 0–5	0.266		0.210	
child's age 6–10	0.422		0.359	
child's age 11–16	0.583		0.522	
Mother's age at birth ≤ 21	0.160		0.151	
Mother's age at birth ≥ 34	0.042		0.054	
Mother's age at birth	26.536	4.659	26.546	4.518
Mother smokes[a]	0.251			
Annual parents' real income (£10,000)	2.523	1.557		
Parents are house owners	0.775			
Current value of parents' house (£10,000)	7.482	7.134		
Years spent at current address	2.602	5.463		
Number of observations (individuals)		764		411

[a] Used in smoking regressions only.

duals are observed in the survey period under analysis. The Table indicates that the average year of birth of the young adults in the Individual Sample is 1976, with a mean age of 18 years. Nearly 48% of the sample are women. About 40% of the mothers of these young adults have no academic qualification, over three-fourths of parents were homeowners by the end of their offspring's childhood, and had an average real (1995) income of £25,000. Almost one-third of the sample experienced life in a single-parent family; either their mother's partnership dissolved before they reached age 16, or they were born outside of a live-in partnership. Of the children who spend some time in a single-parent family, 60% had this family experience below the age of 6.[15] On average, mothers gave birth at ages 26–27: 16% of the young adults in the sample were born when their mother was aged less than 22, and just over 4% of them have mothers aged 34 or more at their birth. Mothers worked almost 84 months, that is, 43% of the first sixteen years of life of their children. Maternal labour supply and child's age are clearly positively related. Approximately 1 in 4 of the young people in the sample have a mother who smokes.

Table 3a. Distribution of siblings (individuals) and sibling pairs in the sibling sample

	Number of:			
	Siblings per household	Households	Individuals	Comparisons (sibling pairs)
	2	165	330	165
	3	23	69	69
	4	3	12	18
Total		191	411	252

Table 3b. Sibling pairs with different experiences of family structure

All childhood		Ages 0–5		Ages 6–10		Ages 11–16	
Freq.	%	Freq.	%	Freq.	%	Freq.	%
30	11.9	31	12.3	22	8.7	15	60

Note: All percentages are computed in terms of total number of comparisons (see Table 3a).

The average figures for the slightly older Sibling Sample are similar, but here we have a smaller proportion of women and a smaller fraction of people that ever lived in a single-parent family. To ease the interpretation of the estimates, Tables 3a and 3b present this sample in greater detail. The 411 young adults come from 191 households: 165 of these households have 2 siblings in our sample, 23 have 3 siblings, and 3 have 4 siblings.[16] A total of 252 comparisons is then obtained from this sample. To identify an association between any variable x and any outcome y, the siblings estimator would require sibling variations in both x and y. Variations across siblings, say, in the proportion of months which the mother worked during each child's childhood are straightforward, simply because of birth order. It is only when the mother never/always worked over both children's childhood that there is no variation. But because our family structure measure is an incidence measure (over either the entire childhood or the three different developmental stages), the nature of sibling variations may be less clear. For example, in a two-child family, one of the half-siblings may have experienced a family break-up while aged 0–5, while the other child, born within a subsequent union of the mother, would never experience a family break-up if the mother and her new partner do not dissolve their union. Or, comparing two full siblings, one may be aged 0–5 when the parents' union dissolves while the other is aged 6–10. Table 3b shows that, of the 252 sibling pairs, 30 of them have a different experience of family structure over the entire childhood. Most of the action occurs at the early stages of child development: 31 sibling pairs live in different family structures when aged 0–5, while only 15 have a different experience when aged 11–16.

2.2. Outcomes

Education. Our measure of educational attainment is achieving an A-level qualification or higher qualification.[17] For each young person, we take the

Table 4. Mean outcomes by sample

	Individual sample	Sibling sample
Education	0.4765	0.4742
N	489	310
Inactivity	0.0781	0.0975
N	2388	1652
Early childbearing	0.0150	0.0195
N	1070	257
Distress	0.2038	0.2008
N	2388	1652
Smoking 10 or more cigarettes a day	0.1792	0.1660
N	2388	1652

Note: *N* is the number of observations (individuals or person-periods) used in estimation.

highest education level as that in the latest year in which we observe him/her in the panel. As it is rare to obtain A levels before the age of 18, we further limit the estimating sample to people who are in the panel at ages 18 or above. Thus, we perform our analysis on 489 and 310 individuals in the Individual Sample and the Sibling Sample, respectively. Table 4 indicates that the percentage of individuals who have achieved at least a highest qualification of A level is similar across samples, and around 47.5%.

Inactivity. This outcome is defined as neither working nor being in school nor looking after children, nor being in government training schemes. The analysis is based on 2,388 and 1,652 person-periods in the Individual Sample and Sibling Sample, respectively. This last sample matches siblings on the year of observation (thus avoiding comparisons at different points of the business cycle). As Table 4 shows, the inactivity rate is slightly larger for individuals in the Sibling Sample: 9.8%, versus 7.8% in the Individual Sample.

Early childbearing. This outcome is defined as having had a first birth at age 21 or less. For the young women in our samples, we estimate the association of the family background measures with the probability of becoming a mother in a given year, conditional on remaining childless up to that point and censoring women when they reach their 21st birthday. We have 1,070 and 257 person-periods in the Individual Sample and Sibling Sample, respectively. Because having a child is inherently age-dependent, the sisters' comparisons are made at common ages. On average, 2% of childless women aged 16–21 have a child each year, but the first birth rate increases with age. Lifetable estimates based on the Individual Sample imply that 13% of young women would become mothers by their 21st birthday, which is less than the one-fifth of women born during 1974–1975 who had a first birth by their 21st birthday indicated by registration statistics (Office for National Statistics (ONS), 1997, Table 10.3). This difference is likely to reflect our sample selection criteria based on coresidence with parents at age 16–17; that is, women who became mothers early are less likely to be observed living with their parents in the BHPS.[18]

Health. We analyse two measures of health-related outcomes. The first measure is defined as having a high level of distress, and it is derived from a set of subjective indicators of well-being.[19] The second measure takes the value of one if an individual smokes more than 10 cigarettes a day, and zero otherwise.[20] The analysis is conducted on 2,387 and 1,652 person-periods in the Individual Sample and Sibling Sample, respectively. In the latter, age enters parametrically, i.e., no age matching is imposed on sibling comparisons. Approximately 1 in 5 young adults reports a high level of distress, and 1 in 6 smokes 10 or more cigarettes a day.

2.3. *Estimation*

We estimate "level" logit regressions with the Individual Sample, and sibling fixed-effects (FE) linear probability models with the Sibling Sample.[21] The coefficient of the family structure variable can be interpreted as the average association of the outcome with family structure in a population in which the family structure impact varies in a random way.[22] Throughout the analysis, we compute robust standard errors that are consistent even if the residuals are not identically and independently distributed, that is, the standard errors are robust to arbitrary forms of heteroskedasticity for individuals over time. In the case of the education outcome, when all variables are measured at the last available year for each individual, the standard errors of the estimates obtained with the Individual Sample are robust to any form of correlation between siblings.

We consider young people's outcomes and their experiences of life in a single-parent family both during their entire childhood (Table 5) and at three developmental stages, ages 0–5, 6–10 and 11–16 (Table 6). All regressions include age, gender, and family structure during childhood. In the Individual Sample we further control for year of birth and mother's education, plus an indicator of whether or not the mother smokes for the smoking outcome only[23]. In an alternative specification of the logit regressions with the Individual Sample, we also include mother's employment patterns (proportion of months worked) during childhood, mother's age at child's birth,[24] and a set of variables that control for the economic circumstances of the family of origin. They are family income, whether parents are homeowners, value of the house if owners, and length of time spent at current (parental) address. These "economic" variables can only be measured when the child is aged 16, or in the first year in which we observe the child living with parent(s), and not during childhood. To the extent that there are persistent components of income or wealth, however, these variables should be indicative of the financial and economic environment of the family of origin.

The identification of an "effect" of childhood family structure on children's later achievements relies on assumptions about parents' (or mother's) and individual's behaviour as well as about the processes that determine cultural and genetic transmission of endowments across generations. In the Appendix we present a simple empirical model to clarify the identification problem inherent in all models of intergenerational links. The within-mother (of family) FE estimator applied to the Sibling Sample identifies the family structure effect under the assumption that family structure does not respond to, and is not correlated with, children's "idiosyncratic endowments". For the reasons discussed in the Introduction and the Appendix, this is a strong assumption, and we do not know the degree to which it is violated. If it is not true, the sibling-difference estimator only indicates suggestive associations between children's outcomes and family structure.

With the level (logit) estimator, this condition is necessary but not sufficient. Identification can be achieved only if one of the three following restrictions is further imposed: (a) there is no degree of "inheritability" of endowments across generations; (b) all the family background variables are independent of family endowments; (c) parents do not respond to child endowments or they only respond to differences in siblings' endowments. Dearden et al. (1997) find evidence of large and significant intergenerational correlations in earnings and years of schooling, thereby making it hard to accept condition (a). As pointed out above, we estimate two specifications of the logit regressions. The variables included in the main specification are likely to be independent of the children's idiosyncratic endowments, but mother's education and family structure may be correlated with the family endowment, thus violating condition (b). The variables in the alternative specification (such as mother's employment and family income) are likely to be correlated with both family and children's idiosyncratic endowments. Nevertheless, they are used in most of the studies in this literature, and our estimates can then be more easily compared to those currently available. Finally, if we believe that parents respond to child endowments, then also condition (c) is untenable. Behrman et al. (1982) and Ermisch and Francesconi (2000) formulate an optimising model of educational choice in which parents only respond to differences between their children's endowments, but identification with the level estimators still requires additional orthogonality assumptions (see Appendix).

3. Results

The estimation results are reported in Tables 5 and 6. A causal interpretation can only be given to the sibling-difference estimates if family structure does not respond to, and is not correlated with, idiosyncratic children's endowments. Both tables also contain the estimates from the logit regressions, whose causal interpretation relies on even stronger identifying assumptions. These estimates, expressed in the form of marginal effects for a young adult with average characteristics, offer however a useful benchmark for comparison.

3.1. Education

Having spent time with a single mother during childhood is associated with a significantly lower probability of achieving A level or more: 13.7% and 14.6% lower in the Individual Sample (main specification (i)) and the Sibling Sample, respectively (Table 5).[25] This association becomes weaker and less precisely estimated in the alternative specification (ii), thereby suggesting that the negative impact of single parenthood on young people's schooling operates partly through lower incomes and wealth in single-parent families (for a similar finding for the US, see Boggess 1998). While we cannot reject the hypothesis that the three stage-specific coefficients are equal (see *p*-values in Table 6), it is noteworthy that the FE estimates from the Sibling Sample exhibit their strongest negative association between schooling and experience in a single-parent family when the young adult was aged 0–5 (Table 6). An early

Table 5. Family structure during childhood and young adults' outcomes

	Individual sample					
	(i)		(ii)		Sibling Sample	
Outcome	Marg. eff.	|*t*-ratio|	Marg. eff.	|*t*-ratio|	Coeff.	|*t*-ratio|
Education	−0.137	2.913	−0.083	1.749	−0.146	1.896
Inactivity	0.056	3.901	0.037	2.577	0.018	0.917
Early childbearing[a]	0.018	2.250	0.012	1.531	0.024	1.688
Distress	0.055	2.453	0.056	2.455	0.036	1.412
Smoking 10+ a day[b]	0.073	2.790	0.063	2.325	0.068	2.670

Note: Estimates for the Individual Sample are marginal effects from logit regressions computed at the average values of all variables used. Regressions under specification (i) include: age, gender, year of birth, mother's education, and a constant. Regressions under specification (ii) include those in (i) plus: proportion of mother's time worked, mother's age at child's birth, family income at child's age 16 (or youngest age when child coresided with his/her mother), whether parents are house owners, value of the house if owners, and length of time spent at current address. Estimates in the Sibling Sample are obtained from within-mother fixed-effects (FE) model. All FE regressions include age. Sisters' differences are taken at the same age in the case of the early childbearing outcome; in all other cases, age enters parametrically.
[a] Women only.
[b] Controls for mother's smoking (Individual Sample only).

experience of parental loss, therefore, is more likely to jeopardise the child's subsequent educational career. Contrary to traditional stress theory, which predicts that the impact of a family split is strongest immediately after it occurred (Conger et al. 1993), our estimates suggest that it is a family disruption in *early* childhood (or being born outside of a live-in partnership) that has the most pronounced consequences on later educational achievements, possibly through its effects on salient aspects of the child's cognitive, cultural and social development. This result is in line with some of the US evidence (see Duncan et al. 1997).[26] The level estimates reveal that a family disruption in adolescence exhibits a large and significant negative correlation with educational attainment. Such a correlation becomes weaker after controlling for the family economic environment (specification (ii)).

3.2. *Inactivity*

A family breakdown during childhood is associated with a 5.6% higher probability of economic inactivity (specification (i), Table 5). Controlling for the economic circumstances of the family of origin reduces it only slightly, but the correlation is always positive and strongly significant. This finding is, however, not robust to the presence of mother's fixed effects. Interestingly, as we sort out the associations by developmental stage (Tables 6), the Sibling Sample estimates show that individuals who spent time in a single-parent family in their early childhood (when aged 0–5), have a 14% higher probability of being inactive in their young adulthood than those who did not experience that family structure. Notice also that the hypothesis of equality of the effects by developmental stage is rejected at any conventional level. The two specifications estimated with the Individual Sample detect the same pat-

Table 6. Family structure during childhood and young adults' outcomes by developmental stage

	Individual sample					
	(i)		(ii)		Sibling Sample	
Outcome	Marg. eff.	|t-ratio|	Marg. eff.	|t-ratio|	Coeff.	|t-ratio|
Education						
child's age 0–5	−0.119	2.141	−0.070	1.266	−0.264	2.561
child's age 6–10	−0.127	1.671	−0.062	0.806	−0.113	0.762
child's age 11–16	−0.228	2.291	−0.171	1.620	−0.102	0.611
equality p-value[a]	0.565		0.615		0.310	
Inactivity						
child's age 0–5	0.067	3.976	0.051	3.187	0.140	4.207
child's age 6–10	0.049	2.195	0.027	1.193	−0.078	1.610
child's age 11–16	0.009	0.330	−0.009	0.634	0.010	0.536
equality p-value[a]	0.105		0.069		0.001	
Early childbearing						
child's age 0–5	0.022	2.542	0.016	1.786	0.024	1.683
child's age 6–10	0.007	0.399	0.003	0.223	0.022	1.672
child's age 11–16	0.014	0.859	0.009	0.655	0.018	1.594
equality p-value[a]	0.543		0.632		0.245	
Distress						
child's age 0–5	0.052	2.004	0.055	2.059	0.044	1.309
child's age 6–10	0.072	1.836	0.076	1.919	0.068	1.402
child's age 11–16	0.036	0.778	0.031	0.660	0.040	1.243
equality p-value[a]	0.812		0.729		0.094	
Smoking 10+ a day						
child's age 0–5	0.070	2.282	0.054	1.745	0.049	2.185
child's age 6–10	0.039	0.814	0.037	0.751	0.021	1.478
child's age 11–16	0.134	3.210	0.138	3.183	0.046	0.847
equality p-value[a]	0.226		0.131		0.042	

Note: Estimates for both samples are obtained as explained in the footnotes of Table 5, except that proportion of mother's time worked during childhood (used in specification (ii) only) and family structure during childhood are broken down by the three developmental stages of the child.
[a] Figures are p-values of the test that the estimated coefficients are equal by developmental stage. The p-values are obtained from χ^2-statistic in individual sample and F-statistic in the siblings sample.

tern, but the impact is lower and around 5–7%. Again, although the parental loss occurred at early stages of life, it appears to have long-term consequences on young people's chances of being economically active (either in the labour market or in school).[27] Thus growing up in a disrupted family affects individuals' later success in at least two different ways, which may be related: it reduces young people's chances of obtaining higher levels of education and it increases their risk of inactivity (see also McLanahan and Sandefur, 1994, Chap. 3).

3.3. Early childbearing

Experience of life in a single-parent family during childhood is associated with significantly higher chances of an early birth: a young woman who had such an experience has a 1.8% per annum higher chance of early childbearing than a woman who did not (specification (i), Table 5). This association diminishes when the economic variables are included in estimation, but it is still quite large. Better economic circumstances appear, therefore, to play a role in reducing the risk of an early birth for women who grew up in a single-parent family but do not eliminate it.[28] A young woman's lower education and employment expectations (two characteristics that are common to women with experience in a single-parent family during their childhood) are likely to reduce her perception of the costs of early childbearing, and a birth may reduce her educational attainments. This illustrates the value of analysing young people's outcomes jointly. The estimates from the Sibling Sample are large and confirm that the positive association between family structure and early birth persists even when we control for mother's fixed effects. Having experienced a family breakdown increases the probability of early motherhood by 2.4% per annum (t-ratio = 1.69). While an early family disruption (when the girl was in pre-school years) exhibits a stronger association (both samples, Tables 6), the three stage-specific estimates from either sample are not statistically different from each other. While restating the importance of an early parental loss, this finding also suggests that family disruptions during school years and adolescence are likely to lessen monitoring of children's activities, and those are the times when parental supervision could prevent behaviour that leads to early childbearing (Thornton and Camburn 1987; Hill et al. 1998).

3.4. Distress

The level estimates show a positive and significant association between family structure and level of distress before and after including the economic variables in estimation: having spent time in a single-parent family during childhood increases the probability of having a high level of distress by 5.5%. This result is consistent with the large psychological literature in this area, which has shown that children and adults in non-intact families are at greater risk for psychological adjustment problems compared to those in families with both biological parents (e.g., Amato and Keith 1991; Bruce and Kim 1992). Family disruptions that occurred either in pre-school (ages 0–5) or in primary school years (ages 6–10) exhibit the strongest associations: for example, young adults whose parents separated when they were between 6 and 10 years of age are about 7% more likely to report high levels of distress than young adults who lived in an intact family during their entire childhood. After controlling for mother's fixed effects, however, the relationship between distress and family structure (at any developmental stage) is not statistically significant. The fact that this correlation is imprecisely measured when we account for unobserved heterogeneity may, however, reflect measurement error in our measure of distress rather than the absence of a robust relationship. It is well known that the presence of modest errors in variables can wipe out most of the associations of interest and that sibling differences exacerbate this effect of measurement error (Griliches 1979). The information on distress is, in fact, elicited in the self-completion questionnaire of the BHPS by a series of questions regarding the way the respondent has been feeling over the last few weeks. The exact phrasing is: "Have you recently ... (felt under strain, depressed, etc.)?". Given the subjective measure of the answer and the relatively short reference period (the last few weeks), this measure is likely to pick up a lot of noise and other (possible temporary) aspects of psychological well-being.[29]

3.5. Smoking

The association between family structure and smoking 10 or more cigarettes a day is always strong and well determined: having spent time with a single mother during childhood increases the probability of heavy smoking by about 7%. This association persists even after controlling for the economic conditions of the family of origin and falls only to 6.3%. Accounting for unobservable factors that are shared by children who belong to the same family (born to the same mother) confirms this finding, with the effect being 6.8%. Family disruptions that occurred either during pre-school years (both samples) or during adolescence (Individual Sample only) seem to have a stronger relationship with heavy smoking.[30] In the case of the Sibling Sample, we reject the hypothesis that the estimated coefficients are equal across the three developmental stages at conventional levels of significance. But this hypothesis cannot be rejected for the estimates obtained with the Individual Sample. In general, our findings confirm the evidence, documented in many social medicine studies, that children of single mothers have an increased risk of being smokers regardless of whether or not the mother smokes (Green et al. 1990; Turner-Warwick 1992). This suggests that, beside mother's smoking behaviour, other characteristics of single-parent families (such as lower parental control or lower expected human capital) foster children's smoking.

4. Conclusions

In this paper we estimate the relationship between several outcomes in early adulthood (educational attainment, economic inactivity, early childbearing, distress and smoking) and the experience of life in a single-parent family during childhood. We use a sample of young adults, selected from the first five waves of the BHPS (1991–1995), who can be matched with at least one sibling over the same period. This sample allows us to estimate the relationships of interest by sibling differences. We also perform our analysis on another sample which we estimate using a level (logit) model. These estimates are useful for comparison with the existing evidence.

We draw attention to four aspects of our findings. First, we show that sibling differences require a weaker assumption (as compared to the assumptions imposed by standard level estimators) for the identification of the family structure effect, namely that family structure is not correlated with children's idiosyncratic endowments, but it is still a strong one. Second, using such sibling differences, we find that experience of life in a single-parent family during childhood is usually associated with negative outcomes for children as young adults: lower educational attainments, higher risks of inactivity and early birth, and higher chances of being a heavy smoker and experiencing higher levels of distress in early adulthood. Third, family structure in early childhood (when the child was between the ages of 0 and 5) appears to be more important for shaping achievement, behaviour and mental health than does family structure during primary school years or adolescence. Fourth, the level estimates, whose causal interpretation relies on even stronger assumptions, confirm our results and are consistent with much of the evidence available in this literature. In addition, they allow us to show that the adverse family structure association generally persists even after controlling for the economic circumstances of the family of origin.[31]

Although we are only able to interpret these results as suggestive associations, the sibling-difference estimates control for more family background factors than has been usual in the literature. Identification of a causal impact must, however, await data which contain sufficiently convincing instruments that allow family structure to be modelled as an endogenous variable.

Appendix: The identification problem

Let j index family and i index individuals (or, interchangeably, young adults and children). For convenience, assume that the relationship that we estimate is given by the following linear probability model (see Angrist and Lavy 1996):

$$p_{ij} = \beta X_{ij} + u_{ij}, \tag{1}$$

where p_{ij} is a dichotomous variable indicating one of the outcomes, taking the value of 1 if the outcome under study occurs and 0 otherwise; X_{ij} is a vector of explanatory variables, such as age, family structure during childhood, mother's education and parental income (Sect. 2 gives a complete list of the variables used in estimation); u_{ij} is a random shock with zero mean. In this formulation, the parameters β are assumed to be the same for all individuals. Arguably, the effect of family structure is heterogeneous (i.e., some children might be better off in a non-intact family, while others might be worse off). Notice, however, that the methodology proposed here would apply even if one specifies a random-coefficients model in which $\beta_j = \beta + \phi_j$, and $E(\phi_j u_{ij}) = E(\phi_j X_{ij}) = 0$.

Our objective is to provide consistent estimates of the "effect" of family structure (contained in X) on the probability of various children's outcomes, p. Consistent estimation of β in (1) requires that the variables measuring mother's (or parents') behaviour during childhood contained in X be uncorrelated with the disturbance term u. We investigate this issue using a framework suggested by Behrman et al. (1994) and Rosenzweig and Wolpin (1995). Consider a two-child family. For the i-th child in family j with sibling k,

$$u_{ij} = \delta_1 \varepsilon_{ij} + \delta_2 \varepsilon_{kj} + \alpha_j + \mu_{ij} \tag{2}$$

$$\varepsilon_{ij} = \rho \varepsilon_j + v_{ij} \tag{3}$$

$$X_{ij} = \Pi \varepsilon_j + \gamma_1 \eta_{ij} + \gamma_2 \eta_{kj} + \theta_j \tag{4}$$

Equation (2) decomposes u_{ij} into four elements: a mother-specific fixed effect common to both siblings, α_j; two distinct stochastic components that depend on the endowments of each sibling, ε_{ij} and ε_{kj}; and measurement error, μ_{ij}. The parameters δ_1 and δ_2 capture the parental (or own) response to child endowments that are observable to all family members but are not observed by the econometrician. We assume that $E(\varepsilon_{ij}) = E(\mu_{ij}) = E(\alpha_j) = E(\varepsilon_{ij}\mu_{ij}) = E(\varepsilon_{kj}\mu_{ij}) = E(\alpha_j\mu_{ij}) = 0$, for all i, k, and j.

Equation (3) is a type of Galton's law of heritability of endowments (see Becker and Tomes 1986), with regression to the mean across generations ($0 \leq \rho < 1$); ε_j is the zero-mean parents' (or mother's) endowment and v_{ij} is an idiosyncratic disturbance with zero mean, and uncorrelated with ε_j and with v_{kj} (the analogous disturbance for sibling k).

Equation (4) relates the variables in X_{ij} to the parental endowment, ε_j, a mother-specific fixed effect, θ_j, and the idiosyncratic endowments of the children, η_{ij} and η_{kj}, where Π, γ_1 and γ_2 are conformable vectors of parameters. The parameters in Π capture the mother's (or parents') response to her (their) own endowment, while γ_1 and γ_2 measure the parental response to child-specific idiosyncratic endowments. Equation (4) allows for the possibility that aspects of family environment, including family structure, may be influenced by the family's as well as the children's endowments. We assume that $E(\eta_{ij}) = E(\eta_{kj}) = E(\theta_j) = E(\eta_{ij}\eta_{kj}) = E(\eta_{ij}\theta_j) = E(\eta_{ij}\varepsilon_j) = 0$, for all i, k, and j. We further assume that $E(\eta_{ij}\alpha_j) = E(\eta_{kj}\alpha_j) = E(\eta_{ij}\mu_{ij}) = E(\eta_{kj}\mu_{ij}) = E(v_{ij}\theta_j) = E(v_{kj}\theta_j) = E(v_{ij}v_{kj}) = E(\mu_{ij}\theta_j) = 0$.

This formulation introduces three different sources of family-specific heterogeneity: in Eq. (3), ε_j is transmitted through the endowments, ε_{ij} and ε_{kj}, to which parents' or individuals' behaviour (via δ_1 and δ_2) can respond; another component, θ_j in Eq. (4), affects children's outcomes indirectly through the parental behaviour measured by X_{ij}; the last source of heterogeneity, α_j, affects the outcome p_{ij} directly (through Eq. (2)), regardless of individual earnings endowments.

Substituting (3) in (2) yields $u_{ij} = \delta_1 v_{ij} + \delta_2 v_{kj} + (\delta_1 + \delta_2)\rho\varepsilon_j + \mu_{ij} + \alpha_j$. The level estimates of β in (1) are consistent if the covariance (vector) between X_{ij} and the error term u_{ij} is zero. However, from (4),

$$\begin{aligned} \operatorname{cov}(X_{ij}, u_{ij}) = {} & \Pi(\delta_1 + \delta_2)\rho\sigma_\varepsilon^2 + \Pi\sigma_{\alpha\varepsilon} + (\delta_1 + \delta_2)\rho\sigma_{\varepsilon\theta} \\ & + \sigma_{\alpha\theta} + (\gamma_1\delta_1 + \gamma_2\delta_2)\sigma_{\eta v}, \end{aligned} \tag{5}$$

where $\sigma_\varepsilon^2 = \operatorname{var}(\varepsilon_j)$, $\sigma_{yz} = \operatorname{cov}(y, z)$, for $y, z = \alpha_j, \varepsilon_j, \theta_j, \mu_{ij}, \eta_{ij}$, and v_{ij}. In general (5) is not zero, in which case estimates of β are inconsistent. Even after introducing other orthogonality assumptions, i.e., $\sigma_{\alpha\varepsilon} = \sigma_{\varepsilon\theta} = \sigma_{\alpha\theta}$, we still find that $\operatorname{cov}(X_{ij}, u_{ij}) = \Pi(\delta_1 + \delta_2)\rho\sigma_\varepsilon^2 + (\gamma_1\delta_1 + \gamma_2\delta_2)\sigma_{\eta v}$, and this covariance disappears only if: a) either $\Pi = 0$, or $\delta_1 = -\delta_2$, or $\rho = 0$; *and* b) $\sigma_{\eta v} = 0$, or $\delta_1 = -\delta_2$ and $\gamma_1 = \gamma_2$, or $\gamma_1 = \gamma_2 = 0$. It is not implausible that at least some of the variables in X_{ij} depend on the family endowment ε_j, that is, $\Pi \neq 0$. For example, mother's education depends on ε_j. If we believe that there exists a positive degree of "inheritability", through genetic and cultural transmission of endowments, then also ρ is non-zero. Furthermore, if we believe that parents respond to child earnings endowments, then $\delta_1 + \delta_2 \neq 0$. Behrman et al. (1982) and Ermisch and Francesconi (2000) do, however, present a family model of endogenous education in which $\delta_1 = -\delta_2$. For consistent estimates of β in this model, however, it is also necessary to assume that the variables in

X do not respond to child idiosyncratic endowments, so that $\gamma_1 = \gamma_2$.[32] That is, when $\delta_1 = -\delta_2$ is coupled with the orthogonality assumption $\sigma_{\alpha\varepsilon} = 0$, $\gamma_1 = \gamma_2$ is sufficient for identification of the parameter β.

Many of the problems of the level estimates have to do with the presence of mother-specific fixed effects.[33] A siblings estimator of the components of β for which the elements of X_{ij} differ between siblings is based on the differences between siblings, and such fixed effects would be eliminated via differencing. In our two-child family case, this is

$$\begin{aligned} \Delta p &= \beta \Delta X + (\delta_1 - \delta_2)\Delta\varepsilon + \Delta\mu \\ &= \beta \Delta X + \Delta\xi, \end{aligned} \tag{6}$$

where $\Delta z = z_{ij} - z_{kj}$, for any term z, and $\Delta\xi = (\delta_1 - \delta_2)\Delta\varepsilon + \Delta\mu$. From Eq. (4), it follows that $\Delta X = (\gamma_1 - \gamma_2)\Delta\eta$. Thus, the covariance between ΔX and the disturbance term in (6) is given by

$$\mathrm{cov}(\Delta X, \Delta\xi) = (\gamma_1 - \gamma_2)(\delta_1 - \delta_2)E(\Delta\eta\Delta v) + (\gamma_1 - \gamma_2)E(\Delta\eta\Delta\mu). \tag{7}$$

Our previous assumptions that $E(\mu_{ij}) = E(\mu_{ik}) = E(\eta_{ij}) = E(\eta_{kj}) = E(\eta_{ij}\mu_{ij}) = E(\eta_{kj}\mu_{ij}) = 0$ guarantee that the second term of (7) is always zero. We then only need to assume that either $\sigma_{\eta v} = 0$, or $\delta_1 = \delta_2$, or $\gamma_1 = \gamma_2$ to identify β. The conditions $\sigma_{\eta v} = 0$ (or, if $\eta_{ij} = v_{ij}$ as in Rosenzweig and Wolpin (1995) and Ermisch and Francesconi (1997), $\sigma^2_\eta = 0$) and $\delta_1 = \delta_2$ are difficult to justify by theoretical arguments. It may, however, be plausible that many aspects of the family environment do not respond to children's idiosyncratic attributes, so that $\gamma_1 = \gamma_2$. This latter condition may be met by some family background variables used in the empirical analysis, like mother's education, because the children's idiosyncratic endowments are only apparent at their birth or afterwards. But other family behaviour, like mother's work patterns, may instead be the *result* of child-specific idiosyncratic attributes rather than the *cause* of the young adult's achievements. For this reason, with the Sibling Sample we estimate a model that contains only (sibling differences in) age, gender, and family structure during childhood. In other words, identification of the family structure "effect" with our data requires that family structure does not respond to, and is not correlated with, children's idiosyncratic endowments. The level (logit) regressions performed with the Individual Sample in specification (i) also contain year of birth and mother's education, variables that are likely to be insensitive to the child's idiosyncratic attributes. As most of the studies in this literature, in specification (ii) we add mother's employment patterns during childhood, mother's age at child's birth, family income, and other "economic" variables that control for the economic circumstances of the family of origin. This allows for a comparison of our estimates with those currently available in the literature, but imposes stronger identifying assumptions.

In sum the identification of the family structure effect on children's achievements rests both on the availability of "prior information" about the process generating parental behaviour and children's outcomes, and on the researchers' willingness to make specific assumptions on such a process. We share Manski et al. (1992) view that "as long as social scientists are heterogeneous in their beliefs about this process, their estimates of family structure effects may vary" (p. 36).

Endnotes

[1] For a detailed overview of existing studies, see McLanahan and Sandefur (1994), Haveman and Wolfe (1995) and Mayer (1997).

[2] Throughout the paper, the terms "mother-specific fixed effects" and "family-specific fixed effects" are used interchangeably, because of the data that we analyse. But it should be emphasized that our fixed-effects estimator eliminates the influence of all unmeasured persistent mother, family and community characteristics that do not differ by siblings.

[3] Recent exceptions are the longitudinal studies by Duncan et al. (1997) and Blau (1999) on the effect of family income on schooling and child development, and by Grogger and Ronan (1996) on the effect of fatherlessness on education and wages.

[4] Recent studies using BHPS data similar to those employed here include Ermisch and Francesconi (1997), who investigate the association of several family structure measures on educational attainments, and Ermisch and Francesconi (2000), who analyse the relationship between family background and young people's earnings.

[5] For data on household income, we use information from fathers, stepfathers or other adults, if present in the household.

[6] On the other hand, the NCDS does have larger sample sizes and more measures of non-economic background factors.

[7] By collecting information on individuals born in the first week of March 1958, the NCDS data are instead not suitable for this estimation procedure. By 1991 in the NCDS sample used in Dearden (1998), there are only 27 pairs of twins.

[8] This finding has never been emphasised before in the British context, partly because the NCDS data could not reliably identify family breakdowns from birth up until age 5. As a result, most of the studies with NCDS data use samples that are typically restricted to individuals whose parents were in an intact family at age 7. An exception is Frostin et al. (forthcoming). Some of their regressions are estimated for a sample of NCDS children aged 33 in 1991, which included those whose parents were not in an intact family either at birth or at age 7 of the child.

[9] Of those interviewed in wave 1 (1991), 88% were re-interviewed in wave 2. The wave-on-wave response rates from the third wave onwards have been consistently above 95%. The BHPS data are therefore unlikely to suffer from serious attrition bias.

[10] The first five waves of the BHPS indicate that 93% of single-parent families are headed by the mother and that 86% of dependent children living with a step-parent lived with their natural mother.

[11] If the birth occurred outside of a partnership and the mother partnered within one year, we assumed that the mother had moved in with the biological father (as assumed in Bumpass et al. (1995) and Ermisch and Francesconi (forthcoming)). For adopted children, we use information on the year in which they were adopted to match in the mother's family history appropriately. In 96% of the cases, the children are natural children.

[12] Ermisch and Francesconi (1997) experiment with other, more detailed measures of family structure, e.g., step-families and durations of different family structures. But this simple dichotomy by developmental stage performs as well in predicting educational attainment as more complex measures. In addition, Ermisch and Francesconi (forthcoming) find that only 242 women had a pre-partnership birth as of 1992 (wave 2), representing 0.05% of all women in that survey year. Since we cannot determine whether or not they subsequently lived with the child's father, we assume that the women who formed a union within one year of the birth did so with the father. Of the 242 women who had a pre-partnership birth, 77 (32%) lived with the child's father. Because of the small sample sizes, therefore, we cannot explore the distinction between children who experienced a family disruption when age 0–5 and children born into a single-parent family.

[13] That is, the third wave contains retrospective information on jobs held by all adults since they left full-time education, including their current work if it started before September 1990, up to

September 1990. For jobs started after that date, we use information collected in the panel wave-on-wave work history.

[14] We performed a similar analysis using more complex measures of mother's employment patterns over her child's childhood, including the proportions of months worked in full-time and part-time jobs and the proportion of months worked in broad occupation groups. Our main results are unchanged.

[15] While the overall figure may appear high, it is consistent with life table estimates by Ermisch and Francesconi (forthcoming), which indicate that 40% of mothers in the BHPS will spend some time as the only parent. The stage-specific figures may also appear large in the light of divorce registration statistics which indicate that about 40% of dependent children of divorcing parents are aged under 6. Recall, however, that single-parent families are also formed by the dissolution of cohabitations and births outside of a partnership. In the BHPS data, these last two categories account for 35% of instances of single parenthood. If, in conjunction with the divorce registration statistics, we assume that all dissolving cohabitations involve children under 6, then 60% of instances of single parenthood would start when the child is aged under 6.

[16] The 165 two-sibling households give rise to 165 comparisons, one per siblings' pair; the 23 three-sibling households produce 69 comparisons, 3 in each household; and the 3 four-sibling households give rise to 18 comparisons, 6 in each household.

[17] For readers unfamiliar with the British education system, "A(Advanced) level" corresponds to education beyond high school, but short of a university degree. At least one A level is necessary to be admitted to a university.

[18] In line with official statistics, data from the original representative BHPS sample show that approximately 20% of women have a baby by age 21. Registration statistics also indicate that, for the most recent cohorts of women for which we have data (women born in the late 1960s), the median age at first birth was 27 (ONS 1997, Table 10.3). The original BHPS data produce a similar figure.

[19] Distress is measured in comparison with "usual" conditions. The subjective indicators are: (i) loss of concentration; (ii) loss of sleep; (iii) playing a useful role; (iv) capable of making decisions; (v) constantly under strain; (vi) problem overcoming difficulties; (vii) enjoy day-to-day activities; (viii) ability to face problems; (ix) unhappy or depressed; (x) losing confidence; (xi) believe in self-worth; (xii) general happiness. Each indicator is measured over a scale that runs from 1 to 4. Recording 1 and 2 values on individual indicators to 0, and 3 and 4 values to 1, and then summing over all indicators gives a new scale running from 0 (the least distressed) to 12 (the most distressed). The scale is known in the health literature as *caseness.* For each young adult, our mental health measure takes the value of one if his/her caseness variable has a value of 3 or more, and zero otherwise. See Cox et al. (1994). Because collapsing the distress scale to a dichotomous variable may reduce the outcome variability, we also perform our analysis treating it as a continuous measure.

[20] The cut-off choice for the number of cigarettes smoked in a day is arbitrary. We also performed the analysis with a variable taking value of one if the respondent smokes and zero otherwise. The results are qualitatively identical to those reported below.

[21] A well-known problem inherent to all linear probability models is that the predicted outcomes are not constrained to lie between zero and one. This may obviously compromise the interpretation of the estimates as the probability that the event under study will occur. To gauge how serious this problem can be with our data, we first estimate our five outcomes with the Individual Sample using a linear probability model. We then use the estimated coefficients to compute the conditional expectation $E(y|x)$, where y is an outcome and x is the appropriate vector of explanatory variables. We predict, at most, 2 cases outside the unit interval for the education outcome (that is, 0.41% of the 489 observations used in this estimation), and 6 cases outside the unit interval for the inactivity outcome (0.25% of the observations). The maximum number of predictions that do not lie between zero and one is always lower for the other three outcomes. Therefore, the prediction problem of the linear probability model is arguably inconsequential for the samples used in this study.

[22] For expositional purposes, the model in the Appendix does not allow for heterogeneity in the family structure effect.

[23] There are six age dummies (age 16 is the base) in the Individual Sample. Mother's education is grouped in four categories: no qualification (base), O level, A level, and higher qualification. In the Sibling Sample, age enters non-parametrically only in the case of early childbearing, i.e., when sisters' comparisons are made at a common age.

[24] We distinguish three stages of mother's age at birth: young (maternal age less than or equal to 21), middle (maternal age between 22 and 33, base); and old (maternal age greater than or equal to 34).

[25] As a way of checking whether the differences between individual-based and sibling-based estimates arise from the different age distributions in the two samples (see Table 2), we have also estimated level models using the Sibling Sample for all outcomes. In general the point estimates are very close to those found with the Individual Sample while the standard errors are somewhat larger (presumably due to the smaller sample sizes). This suggests that the differences in results across samples do not systematically depend on their different age distributions. For example, the level estimates (in terms of marginal effects) of the education outcome found with the Sibling Sample are -0.138 (t-ratio $= -2.187$) and -0.122 (t-ratio $= 2.042$) for specification (i) and (ii), respectively.

[26] Duncan et al. (1998) find that "early childhood appears to be the stage in which family economic conditions matter most" (p. 420). Our findings are consistent with their result, given that the impact of family structure on educational attainment may operate partly through family income.

[27] For the estimates obtained from specification (ii) of the Individual Sample, we can reject the hypothesis of equality of the estimated stage-specific coefficients at the 10% level but not at the 5% level. For the estimates from specification (i), we instead never reject the equality hypothesis.

[28] The results from specification (ii) also reveal that if the mother herself was aged 21 or less when her daughter was born, then the odds that the daughter has an early birth are higher. Thus, there is evidence of a recurrence of early motherhood across generations. This association becomes slightly weaker when the economic circumstances of the family of origin are controlled for. On the other hand, if mothers were aged 34 or more at birth, the chance of their daughters having an early birth are lower.

[29] When the distress measure is treated as a continuous variable, we again find positive and well-measured estimates of family structure using the Individual Sample. For example, in comparison with the results reported in Table 5, having spent some time in a single-parent family during childhood increases the distress level (which on average equals 1.364) by 0.384 points (t-ratio $= 2.864$) in specification (i) and by 0.388 points (t-ratio $= 2.843$) in specification (ii). The estimates obtained from the Sibling Sample are always positive, but somewhat smaller and never statistically significant. In sum, these results provide evidence comparable to that found with our dichotomous measure.

[30] We also find a high persistence in smoking behaviour across generations. In the Individual Sample, a young adult whose mother smokes has approximately 15% higher chances of being a heavy smoker than a young adult (with similar characteristics) whose mother does not smoke. This association is always significant at any conventional level. The inclusion of mother's smoking behaviour, however, may be problematic for identification of the family structure effect (see Sect. 2.3 and the Appendix). Its exclusion from the level regressions does not alter our results.

[31] Discussing the findings from twelve different papers, McLanahan (1997) argues that growing up in a single-parent family has negative consequences for children's well-being across several domains (e.g., test scores, education, behavioural and psychological problems, jobs and income) even after taking family income into account.

[32] We discount the unlikely case that $\sigma_{\eta v} = 0$.

[33] Given the data analysed in this study and the setup of the model presented here, the definition of "mother-specific fixed effect" is equivalent to that of "family-specific fixed effect". In particular, our fixed-effects estimator captures all persistent mother, family and community effects that do not differ by siblings. The two labels are therefore equivalent and used interchangeably throughout the paper.

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