Childhood Parental Behaviour and Young People's Outcomes

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Summary

This paper estimates the relationship between several outcomes in early adulthood (education, inactivity, early birth, distress and smoking) and experiences of life in a single-parent family and with jobless parent(s) during childhood. The analysis is performed using a special sample of young adults, who are selected from the first nine waves of the British Household Panel Survey (1991-1999) and can be matched with at least one parent *and* one sibling over the same period. This sample allows us to estimate the relationship of interest using sibling differences. We also use another sample of young adults from the BHPS, matched to at least one parent, to estimate more conventional level models and compute nonparametric bounds and point estimates. The estimates based on sibling differences require weaker assumptions (as compared to the assumptions imposed by nonparametric estimators under conditional independence and level estimators) for the identification of the effects of family structure and parental joblessness on the outcomes under analysis. We find that: (i) experiences of life in a single-parent family and with jobless parents during childhood are usually associated with disadvantageous outcomes for young adults; (ii) the effect of family structure is in general significantly greater (in absolute value) than the effect of parental worklessness; (iii) most of the unfavourable outcomes are linked to an early family disruption, when the child was aged 0-5, whereas the timing of parental joblessness during childhood has more complex effects, with different outcomes being more strongly influenced by parental worklessness at different developmental stages.

Keywords: Intergenerational processes; Treatment effects; Selection and Identification; Nonparametric bounds; Family endowments; Sibling estimators

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1 Introduction

The central question of this paper is whether a number of outcomes in early adulthood are associated with experience of life either in a single-parent family or in a family with jobless parents during childhood (i.e., from birth to the sixteenth birthday of the child). An extensive body of research has identified childhood family structure as a key factor associated with later achievements (McLanahan and Sandefur, 1994; Haveman and Wolfe, 1995; Cherlin *et al.*, 1995; Kiernan, 1997), and parental joblessness is known to be a strong indicator of dependence on social security benefits, especially among families with children (Iacovou and Berthoud, 2000). Therefore the focus of this paper is on the importance of childhood poverty relative to childhood family structure in influencing outcomes later in life. Given the current emphasis on family issues and child poverty, this is a theme of relevance for social research and contemporary public policy (e.g., Department of Social Security, 2001).

A fundamental concern for this research is that the estimated effects of childhood family structure and parental joblessness might be spurious (Manski *et al.*, 1992). This is due to the mutual association that family structure (or parents' non-employment patterns) and young people's outcomes may share with some unmeasured or unmeasurable factors. For example, behavioural and medical problems such as alcoholism, depression or drug addiction may make a parent less likely to work (and more likely to divorce) and affect his/her child's life. Another possibility is that parents who are strongly committed to their job may not only be less likely to lose it but also provide more support to their children. In general, therefore, the association between experience of life in a poor family or with a lone parent and later outcomes may not necessarily be the result of childhood family poverty or family structure *per se*. Rather, differences in outcomes may simply reflect the (sometimes unobserved, other times unobservable) characteristics of families in which the children of poor/single-parent families are brought up.

As in most studies in this literature, the objective of this paper is to determine the distribution of a specific outcome, say highest educational achievement, which would be realised if all individuals with a given background were brought up in a non-intact family (or in a poor family), and compare this with the educational distribution that would be realised if the same individuals were instead brought up in an intact family (or non-poor family). As shown in Section 2, this comparison is unidentified in non-experimental survey data, such as those used here. Borrowing from Manski (1989, 1990, 1994 and 1995), we show that true "treatment effects" can be bounded, but the bounds are generally too wide, and cannot be used to sign the effects of interest. Point estimates of the treatment effects can also be determined nonparametrically, but at the cost of imposing a rather stringent exogeneity assumption.

We also present estimates of the effect of family structure and parental joblessness on child outcomes obtained from parametric models. Indeed, our current knowledge about those effects is almost exclusively based on such models (for surveys of mainly American studies, see McLanahan and Sandefur, 1994; Haveman and Wolfe, 1995; Duncan and Brooks-Gunn, 1997). Standard parametric models are useful because they usually specify a relatively simple process that links treatments to outcomes, but rely on questionable assumptions about parents' and individual's behaviour as well as about the mechanisms of intergenerational transmission. Because of these assumptions, we additionally estimate parametric models using sibling differences. We show that the effects of family structure and parental joblessness on outcomes can be identified with sibling differences provided that neither family structure nor parental joblessness patterns respond to "idiosyncratic endowments" of children. On this assumption, our sibling-difference estimates would measure the causal impact of childhood family structure and poverty on young adults' achievements. But note that, in addition to inherent differences between siblings (e.g., one born with a disability), sibling differences in idiosyncratic endowments include time differences in parental attitudes and behaviour which may affect both outcomes and family structure and/or parents' non-employment patterns. For example, a father may become alcoholic, giving rise to a situation in which his oldest child spends only a small part of his/her childhood with an alcoholic father while his youngest child has an alcoholic father for most of his/her childhood. The father's alcohol problem may directly affect his investment in the youngest child's education. In addition, the father may lose his job or have a less stable attachment to the labour market. Similarly, the parents may divorce because of the father's alcohol addiction. In each of these situations we are likely to observe a systematic correlation between idiosyncratic endowments and childhood family structure or poverty. Thus, while the assumption of no such correlation is weaker than the assumptions needed in standard paramtetric models, it is still a strong one.

Our analysis uses a special sample of young adults from the first nine waves (1991-9) of the British Household Panel Survey (BHPS), who can be matched with at least one sibling over the same time period. These young adults are then linked to their parents' family history collected in the second (1992) wave and to their parents' job history collected in the 1993 wave of data, and are followed over the nine available years of the survey. Additional information about their parents (e.g., education and region of residence) is obtained from the parents' own interviews. The family and job histories allow us to date the family structure and parental joblessness measures over the entire childhood of each young adult in our sample. We also analyse another sample, in which young adults need not be matched with a sibling, but they must live with their mothers (and fathers) for at least one of the nine panel years. As in other previous research (Duncan and Brooks-Gunn, 1997; Ermisch and Francesconi, 2001a and 2001b), we distinguish between three different child developmental stages, that is, ages 0-5, 6-10, and 11-15.

By using data from the BHPS, this paper fills a gap between other studies on child outcomes in Britain. In fact, most of the previous research on intergenerational links uses data from the National Child Development Study (NCDS) of British children born in a week in March 1958 and, to a lesser extent, from the 1970 British Cohort Study (BCS). Kiernan (1992 and 1997), Ní Bhrolcháin *et al.*, (1994), and Cherlin *et al.*, (1995) have used samples from the NCDS to study the relationship between childhood family structure and well-being as adults. These studies find that family disruption during childhood is correlated with outcomes for educational attainment, economic situation, partnership formation, relationship breakdown and parenthood behaviour in adulthood, which are generally worse than those for children from intact families. Gregg and Machin (1999) use NCDS data to uncover some of the main factors associated with childhood disadvantage. They find that economic and social disadvantages faced during childhood display a persistent association with subsequent economic failure in the labour market. Using the same data source, Hobcraft and Kiernan (1999) show that any experience of childhood poverty is clearly associated with a number of adverse outcomes in adulthood, such as lone parenthood, lack of a telephone in the home, lower educational achievement and worse health. Joshi and Verropoulou (2000) use data from both the NCDS and the BCS to examine the association of parental employment, family structure and poverty with a number of outcomes (e.g., cognitive development, educational attainment and early childbearing). They find that poor circumstances in the family of origin are the most important predictor of child outcomes, over and above the effects of childhood parental employment and family structure. It should be emphasised, though, that these last two variables are not well measured in either the NCDS or the BCS.

Indeed, the BHPS has some advantages over the birth cohort data. First, the data are more recent and a better reflection of contemporary family life. The households tracked by the BHPS have been interviewed annually since 1991, and the young adults that are the focus of this study were born between 1970 and 1983. Lone parenthood is much more common among BHPS families than when the 1958 NCDS cohort was growing up. Second, although the NCDS and other cohort studies have large sample sizes and include more measures of non-economic background factors and children's early achievements, the BHPS yields more detailed information on parents' income and employment (and non-employment) patterns. For example, it is possible to measure parents' joblessness throughout their children's upbringing, rather than at particular points in time. Third, because the BHPS follows whole households, including siblings, it is possible to control for any unobserved influences in family background characteristics that are shared by children from the same family.

The next section of the paper highlights the identification problem that is common to all studies involving the relationship between childhood parental behaviour and later child outcomes. It also outlines how the effects of family structure and parental non-employment patterns on the outcome of interest can be bounded using the method suggested by Manski (1989 and 1990), and presents a parametric model for which standard (or level) estimates and sibling-difference estimates are derived. The third section describes the data used in the estimation. The fourth reports our main results, which are separately obtained for young men and young women, and examines the possibility of differential effects by broad developmental stages during childhood. Section 5 concludes.

2 Identification issues and methodology

2.1 The identification issue

The central problem in determining the causal impact of family structure and parental joblessness on children's outcomes is eloquently presented in Manski (1989, 1990, 1994 and 1995) and Manski *et al.*, (1992). Here we briefly review the identification issue and relate it to our estimation procedure.

We assume that for each of the BHPS respondents in our sample, who are drawn from a population of youth, we observe the set $\{y_1, y_0, Z, X\}$. Here X is the vector of observed (discrete and continuous) covariates describing the person's individual and family characteristics (e.g., age and parents' education). The vector Z contains the binary variables we are most interested in — family structure and parental joblessness — which are defined as "treatment" variables. In what follows, for ease of exposition, we restrict Z to a single variable, z, which takes the value of one if the person receives the treatment (i.e., experienced life in a single-parent family or lived with jobless parents during his/her childhood), and zero otherwise. Let y_1 be a dichotomous outcome (e.g., having achieved A level qualifications or more, being economically inactive, and having had an early birth), which is equal to one if the outcome is realised and zero otherwise, when the treatment occurs ($z = 1$). Similarly, y_0 indicates the outcome if the treatment event does not happen ($z = 0$). Consider $P(y_1 = 1 \mid X)$ and $P(y_0 = 1 \mid X)$, the probabilities that a young person with covariates X would achieve the outcome if he/she were or were not to receive the treatment, respectively. In most empirical analyses, the difference

$$\tau(X) = P(y_1 = 1 \mid X) - P(y_0 = 1 \mid X) \tag{1}$$

is defined as the (treatment) effect of z on y. (For a discussion of other treatment effects, see Manski *et al.*, 1992; Heckman, 2001). By the Law of Total Probability, we can re-write each of these probabilities as

$$\begin{aligned} P(y_1 = 1 \mid X) &= P(y_1 = 1 \mid z = 0, X)P(z = 0 \mid X) \\ &\quad + P(y_1 = 1 \mid z = 1, X)P(z = 1 \mid X) \end{aligned} \tag{2}$$

and

$$\begin{aligned} P(y_0 = 1 \mid X) &= P(y_0 = 1 \mid z = 0, X)P(z = 0 \mid X) \\ &\quad + P(y_0 = 1 \mid z = 1, X)P(z = 1 \mid X). \end{aligned} \tag{3}$$

Non-experimental data (such as those from the BHPS, the NCDS or the BCS) allow us to recover all the expressions on the right-hand sides of (2) and (3), except for $P(y_1 = 1 \mid z = 0, X)$ and $P(y_0 = 1 \mid z = 1, X)$. This is simply because we cannot observe y_1 if $z = 0$ or y_0 if $z = 1$. Thus, the treatment effect (1) cannot be identified.

2.2 Manski's bounds

In various contributions, Manski (1989, 1990, 1994, 1995) has proposed a simple and convenient way to bound the probabilities of interest under the assumption that no prior information is available. Because y is a dichotomous outcome, the unidentified conditional probabilities must lie in the unit interval. Thus, from (2) and (3) it follows that

$$\begin{aligned} P(y_1 &= 1 \mid z = 1, X)P(z = 1 \mid X) \leq P(y_1 = 1 \mid X) \\ &\leq P(y_1 = 1 \mid z = 1, X)P(z = 1 \mid X) + P(z = 0 \mid X) \end{aligned} \tag{4}$$

and

$$\begin{aligned} P(y_0 &= 1 \mid z = 0, X)P(z = 0 \mid X) \leq P(y_0 = 1 \mid X) \\ &\leq P(y_0 = 1 \mid z = 0, X)P(z = 0 \mid X) + P(z = 1 \mid X). \end{aligned} \tag{5}$$

It is easy to verify that the bounds (4) and (5) imply lower and upper bounds on the treatment effect $\tau(X)$ in (1) of the form:

$$D(X) - P(z = 1 \mid X) \leq \tau(X) \leq D(X) + P(z = 0 \mid X), \tag{6}$$

where $D(X) = P(y_1 = 1 \mid z = 1, X)P(z = 1 \mid X) - P(y_0 = 1 \mid z = 0, X)P(z = 0 \mid X)$. Notice that the lower bound in (6) is obtained by subtracting the upper bound in (5) from the lower bound in (4), and the upper bound is obtained in a similar way. In principle, the effect of z on y can lie in the interval [-1, 1], which has width 2, whereas the bounds in (6) have clearly width 1, thus reducing the range of the possible values of $\tau(X)$. The bounds in (6), however, always contain zero, and thus they do not bound the sign of the treatment effect. In the absence of additional information or assumptions, the interval in (6) is all that can be identified with the data.

A number of studies on intergenerational links, however, have offered at least two approaches to identify $\tau(X)$ more tightly. The first approach is based on the rather stringent condition that the treatment is randomly assigned (Manski *et al.*, 1992). This means that, conditional on X, the outcomes y_0 and y_1 are mean-independent of both family structure and parental joblessness. That is:

$$P(y_1 = 1 \mid z = 1, X) = P(y_1 = 1 \mid z = 0, X)$$

and

$$P(y_0 = 1 \mid z = 1, X) = P(y_0 = 1 \mid z = 0, X).$$

Under conditional independence, a nonparametric point estimate of $\tau(X)$ will be given by

$$\Delta(X) = P(y_1 = 1 \mid z = 1, X) - P(y_0 = 1 \mid z = 0, X). \tag{7}$$

Clearly, the assumption that both family structure and parental joblessness are exogenous is likely to be untenable if we believe that unobserved/unobservable factors, which affect family structure and parental joblessness, are systematically correlated to child outcomes. The estimates (7) are, however, easy to compute and represent a useful benchmark for the estimates obtained from the second approach that is described in the following subsection.

2.3 Parametric models

The effect of z on child outcomes is identified if we have some prior information about the probability distribution of (y, z) conditional on X, where $y = y_1 z + y_0(1-z)$ is the realised (observed) outcome. This prior information is typically expressed in terms of generalised linear models (Manski, 1995) of the form:

$$g\{E(y)\} = Z'\beta + X'b, \tag{8}$$

and $y \sim F$, where $g\{\cdot\}$ is the link function, F is a (parametric) distributional family, $E(\cdot)$ is the mathematical expectation operator, and β and b are conformable parameter vectors to be estimated. In most applications, F is assumed to be Bernoulli and $g\{\cdot\}$ is the logit function, which imply that (8) is a logistic regression. Instead, if F is Gaussian and $g\{\cdot\}$ is the identity function, then (8) becomes a linear regression.

We present (and, in a subsequent section, estimate) two classes of such models, which differ not by the specification of F or $g\{\cdot\}$, but by the way in which they handle the unobservables. Before doing so, we need to lay out the relationships between child outcomes and different sources of unobservables that may be relevant in the intergenerational transmission process. To simplify our exposition, we assume that F is Bernoulli and $g\{\cdot\}$ is the identity function, so that (8) becomes a linear probability model. The X variables have also been partialled out of the model for convenience. (In our empirical analysis, however, we will estimate logit regressions with the inclusion of the covariates X described in Section 3). Under these assumptions, we can re-write (8) as follows:

$$y_{ij} = \beta_1 z_{1ij} + \beta_2 z_{2ij} + u_{ij}, \tag{9}$$

where the subscripts i index individuals (or, interchangeably, young adults and children) and j index families, respectively, and z_1 and z_2 denote childhood family structure and parental joblessness, respectively. The term y_{ij} is the realised outcome for individual i in family j, and takes the value of 1 if the outcome under study occurs and 0 otherwise, and u_{ij} is a random shock with zero mean. In this formulation, the parameters β are the same for all individuals. Arguably, the effect of Z is heterogenous (e.g., some children might be better off in a lone-parent family, while others might be worse off). Our framework, however, would apply even if one specifies a random-coefficients model in which $\beta_j = \beta + \eta_j$, and $E(\eta_j u_{ij}) = E(\eta_j z_{nij}) = 0$ for n=1,2. (However, it may not be feasible to estimate such a model, because repeated observations within each family j are needed, whereas most of the families in our sample consist of only two or three siblings. See Section 3 below).

Our objective is to provide consistent estimates of the effects of z_1 and z_2 on the probability of various child outcomes, y. Consistent estimation of β in (9) requires that the variables in Z (and in X if they were included) be uncorrelated with the disturbance term u. We investigate this issue with the framework suggested by Behrman et al. (1994), Rosenzweig and Wolpin (1995) and Ermisch and Francesconi (2001c). Consider a two-child family. For the i-th child in family j with sibling k, we have

$$u_{ij} = \delta_1 \epsilon_{ij} + \delta_2 \epsilon_{kj} + \alpha_j + \mu_{ij} \tag{10}$$

$$\epsilon_{ij} = \rho \epsilon_j + \nu_{ij} \tag{11}$$

$$z_{nij} = \pi_n \epsilon_j + \gamma_{n1} \phi_{ij} + \gamma_{n2} \phi_{kj} + \theta_{nj} + e_{nij}, \quad \text{for} \quad n = 1, 2. \tag{12}$$

Equation (10) decomposes u_{ij} into four unobserved (or unobservable) elements: a family- or mother-specific fixed effect common to both siblings, α_j (e.g., affection, motivation and work ethics); two different stochastic components that depend on the endowments of each sibling, ϵ_{ij} and ϵ_{kj} (e.g., innate academic ability); and another shock that captures 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 analyst. We assume that $E(\epsilon_{ij}) = E(\alpha_j) = E(\mu_{ij}) = E(\epsilon_{ij}\mu_{ij}) = E(\epsilon_{kj}\mu_{ij}) = E(\alpha_j \mu_{ij}) = 0$, for all i, k, and j.

Equation (11) is a type of Galton's law of heritability of endowments (see Galton, 1886; Becker and Tomes, 1986), with regression to the mean across generations (i.e., $0 \leq \rho < 1$). The term ϵ_j represents the zero-mean parents' (or mothers') endowment that is passed on to the children, and ν_{ij} is the child-specific idiosyncratic disturbance with zero mean and uncorrelated to ϵ_j and ν_{kj} (the analogous disturbance for sibling k). Finally, equation (12) relates the treatment variables z_1 and z_2 to the parental endowment, ϵ_j; a zero-mean family- and treatment-specific fixed effect, θ_{nj} (which captures the different propensities of parents to live together or split up and to work hard or lose their job); the zero-mean idiosyncratic endowments of the children, ϕ_{ij} and ϕ_{kj}, which may be associated with parental (non)-employment patterns or family structure during childhood (e.g., sport or musical talents, mental retardation and, again, academic ability); and a white noise e_{nij} that is uncorrelated to the other stochastic components in (10)-(12). The parameters π_n captures the parents' response to their own endowments, while the parameters γ_{n1} and γ_{n2} measure the parental response to the child-specific endowments in terms of the treatment z_n, n=1,2. Therefore, in (12) we allow for the possibility that some aspects of the family environment, in particular family structure and joblessness, are influenced by both the family and the children's endowments. (In a more general formulation, equation (12) may apply to all the variables included in X). We assume that $E(\phi_{ij}\phi_{kj}) = E(\phi_{ij}\theta_{nj}) = E(\phi_{ij}\epsilon_j) = 0$, for all i, k, j and n. For simplicity, we also assume that $E(\alpha_j\phi_{ij}) = E(\alpha_j\phi_{kj}) = E(\phi_{ij}\mu_{ij}) = E(\phi_{kj}\mu_{ij}) = E(\nu_{ij}\theta_{nj}) = E(\nu_{kj}\theta_{nj}) = E(\nu_{ij}\nu_{kj}) = E(\theta_{nj}\mu_{ij}) = 0$, for all i, k, j and n.

In sum, this framework introduces three different influences of family-specific heterogeneity on child outcomes. First, ϵ_j in equation (11) is transmitted through the endowments, ϵ_{ij} and ϵ_{kj}, to which parents' or individuals' behaviour can respond (via δ_1 and δ_2). Second, θ_j in (12) affects child outcomes indirectly through the parental behaviour measured by z_{1ij} and z_{2ij} (and eventually by X_{ij}). The third source of heterogeneity, α_j, determines the outcome y_{ij} directly through equation (10), regardless of child or family endowments. We now turn to the two types of models to be estimated.

2.3.1 Level models

The first class of models is, at present, perhaps the most popular in applied social research (e.g., McLanahan and Sandefur, 1994; Haveman and Wolfe, 1995; Duncan and Brooks-Gunn, 1997; Kiernan, 1992, 1996 and 1997; Gregg and Machin, 1999; Hobcraft and Kiernan, 1999; Joshi and Verropoulou, 2000). We label these "level models" simply because they (or the data they are applied to) do not distinguish among young adults within the same family (i.e., siblings and half-siblings), treating them as unrelated individuals. This means that, after accounting for the specification of the unobservables (10)-(11) and the processes (12) that determine treatments (and covariates), one tries to recover the parameters β by estimating (9) directly with the variables Z (and X) in "levels", rather than in differences or other forms.

Substituting (11) in (10) yields $u_{ij} = (\delta_1 + \delta_2)\rho\epsilon_j + \delta_1\nu_{ij} + \delta_2\nu_{kj} + \alpha_j + \mu_{ij}$. The level estimates of β in (9) are consistent if the covariance between z_{nij} and the new error term is zero. Using (12), it is clear however that

$$\begin{aligned} \mathrm{cov}(z_{nij}, u_{ij}) &= \pi_n(\delta_1 + \delta_2)\rho\sigma_\epsilon^2 + \pi_n\sigma_{\alpha\epsilon} + \pi_n(\delta_1 + \delta_2)\rho\sigma_\epsilon^2 + \sigma_{\alpha\theta_n} \\ &\quad +(\gamma_{n1}\delta_1 + \gamma_{n2}\delta_2)\sigma_{\phi\nu}, \end{aligned} \tag{13}$$

where $\sigma_\epsilon^2 = \mathrm{var}(\epsilon_j)$, $\sigma_{rs} = \mathrm{cov}(r, s)$, for $r, s = \alpha_j, \epsilon_j, \theta_{nj}, \mu_{ij}, \phi_{ij}$, and ν_{ij}, for n=1,2. In general, therefore, even under all the assumptions introduced so far, the covariance (13) is not zero, implying that the level estimates of β are inconsistent. Introducing further orthogonality conditions, which cannot be easily justifiable on theoretical grounds, i.e., $\sigma_{\alpha\epsilon} = \sigma_{\epsilon\theta_n} = \sigma_{\alpha\theta_n} = 0$, we still find that $\mathrm{cov}(z_{nij}, u_{ij}) = \pi_n(\delta_1+\delta_2)\rho\sigma_\epsilon^2 + (\gamma_{n1}\delta_1+\gamma_{n2}\delta_2)\sigma_{\phi\nu}$. Clearly, this covariance disappears only if:

$$\pi_n = 0; \quad \text{or} \quad \delta_1 = -\delta_2; \quad \text{or} \quad \rho = 0; \tag{14}$$

and

$$\sigma_{\phi\nu} = 0; \quad \text{or} \quad \delta_1 = -\delta_2 \ \text{ and } \ \gamma_{n1} = \gamma_{n2}; \quad \text{or} \quad \gamma_{n1} = \gamma_{n2} = 0. \tag{15}$$

It is not implausible that either family structure or parental joblessness or both (or any of the covariates in X that are usually included in level models) depend on the family endowment ϵ_j, that is, $\pi_n \neq 0$. For example, parental education depends on ϵ_j. Futhermore, if we believe that there exists some degree of inheritability, through genetic and cultural transmission of endowments, then also ρ is non-zero. Finally, if parents respond to child endowments that directly affect child outcomes (that is, ϵ_{ij} and ϵ_{kj}), then $\delta_1 + \delta_2$ would in general differ from zero. To the extent that parents reinforce or compensate for differences in their children's endowments, then $\delta_1 + \delta_2 \neq 0$ (see Behrman *et al.*, 1982; Behrman et al., 1994; Ermisch and Francesconi, 2000a). With these considerations, condition (14) is unlikely to hold. So, even if condition (15) is satisfied, the level estimates will not identify β, because the assumptions needed to justify (14) are arguably untenable.

2.3.2 Sibling-difference models

The second class of models, which we label "sibling-difference models", do acknowledge the fact that siblings or half-siblings share many family-specific characteristics (e.g., biological or social parents, their parenting style, parents' social and cultural environments, housing and, to a large extent, neighbourhoods and schools). The estimation is meant to eliminate most of such common observed and unobserved (or unobervable) factors by relating differences in outcomes between siblings to differences in their experience of life with poor or single-parent families (as well as differences in other time-varying covariates). In our two-child family case, a sibling-difference estimator is computed on

$$\begin{aligned} \Delta y &= \beta_1 \Delta z_1 + \beta_2 \Delta z_2 + (\delta_1 - \delta_2)\Delta\epsilon + \Delta\mu \\ &= \beta_1 \Delta z_1 + \beta_2 \Delta z_2 + \Delta\psi, \end{aligned} \tag{16}$$

where $\Delta r = r_{ij} - r_{kj}$, for any term r in (16), and $\Delta\psi = (\delta_1 - \delta_2)\Delta\epsilon + \Delta\mu$. From equation (12), it follows that $\Delta z_n = (\gamma_{n1} - \gamma_{n2})\Delta\phi$. Thus, the covariance between Δz_n and the disturbance term in (16) is given by

$$\text{cov}(\Delta z_n, \Delta\psi) = (\delta_1 - \delta_2)(\gamma_{n1} - \gamma_{n2})E(\Delta\phi\nu) + (\gamma_{n1} - \gamma_{n2})E(\Delta\phi\Delta\mu). \tag{17}$$

Our previous assumptions that $E(\mu_{ij}) = E(\mu_{kj}) = E(\phi_{ij}) = E(\phi_{kj}) = E(\phi_{ij}\mu_{ij}) = E(\phi_{kj}\mu_{ij}) = 0$ for all i, k and j guarantee that the second term on the right-hand side of (17) is always zero. Thus, the covariance (17) would vanish if

$$\sigma_{\phi\nu} = 0; \quad \text{or} \quad \delta_1 = \delta_2; \quad \text{or} \quad \gamma_{n1} = \gamma_{n2}. \tag{18}$$

It is hard to assume that the covariance between child idiosyncratic components would vanish, because some of the components that are *not* fully determined through heritability of endowments may affect childhood family structure and parental (non)employment patterns, such as mental retardation or musical talents (hence $\sigma_{\phi\nu} \neq 0$. Similarly hard is to justify that parents equally respond to their children endowments regardless of their relative differences, i.e., $\delta_1 = \delta_2$ (see the discussion in 2.3.1). If, however, we believe that many aspects of the family environment and behaviour (and, in particular, family structure and parental joblessness during childhood) do not respond to children's idiosyncratic endowments, then $\gamma_{n1} = \gamma_{n2}$ for n=1,2. This condition will then be sufficient to identify β.

There may be good reasons to believe that this identifying assumption is sensible, especially if we accept that parents have only a limited knowledge of their children's idiosyncratic endowments at least up to a certain age, and therefore may not respond to them in terms of their observed Z (and X). For example, this can be true for some types of mental and physical disorders (e.g., autism), mathematical geniouses and sport or musical talents, which tend to be revealed slowly over time. A large body of the developmental psychology literature documents that parents' ability to assess their own children's endowments is partly related to the feedback received from children themselves. Parents seem to be most accurate when their children's performance falls at an extreme, either very high or very low, because of the clarity of the feedback that they receive (Heriot and Schmickel 1967). But parents of children that fall in between such extremes are likely to be less accurate in their assessments (Frankenburg *et al.*, 1976; Knobloch *et al.*, 1979; Hunt and Paraskevopoulos, 1980). Parents, however, accumulate information about their children as they grow older. This process is likely to reduce parental inaccuracies about their children's endowments (Entwisle and Hayduk, 1981). In particular, parents are likely to adjust their behaviour (hence, Z and X), once they have sufficiently accurate information about ϕ_{ij} and ϕ_{kj}, and this may be in general well before the end of childhood for i and k. So, even though the assumption that $\gamma_{n1} = \gamma_{n2}$ is tenable at young ages, it may not be so if one considers parents' behaviour over the entire childhood of their children. Because of the increasing disclosure of information about child endowments, this assumption will be almost certainly violated when children pass a given age, e.g., the age at school entry. This is why in the empirical analysis we will pay special attention to the results obtained for the early developmental stages (e.g., ages 0-5).

In sum, the estimator based on the sibling differences (16) identifies β but at some cost. We must be willing to assume that parents have (at least initially) only a limited knowledge about their children's idiosyncratic attributes, and, consequently, do not systematically respond to them by adjusting their behaviour through (12). But the estimates of β obtained from widely used level models are consistent only if we are willing to impose even stronger identifying assumptions (see condition (14)). Prior information about and/or willingness to make specific assumptions on the process linking parental behaviour to child outcomes are fundamental for the identification of β. As Manski *et al.*, (1992) observe, "as long as social scientists are heterogeneous in their beliefs about this process" (p. 36), their estimates of any treatment effect will continue to vary.

3 Data

3.1 Estimating samples and treatment variables

The data come from a special sample selected using the first nine waves of the British Household Panel Study (BHPS). In Autumn 1991, the BHPS interviewed a representative sample of 5500 households, containing about 10000 persons. The same individuals are re-interviewed 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. Some 88% of the original BHPS sample were re-interviewed for the second wave (1992) and the response rates have been consistently higher than 95% from the third wave onwards. Thus, the sample remains broadly representative of the population of Britain as it changes through the 1990s. (Futher information on the BHPS can be obtained at `http://www.iser.essex.ac.uk/bhps`).

To estimate the effects of parental joblessness and family structure during childhood on young adults' outcomes, we first match young adults (aged 16 or above) to one or both of their biological/adoptive parents interviewed in at least one of the nine waves. We then use the information that parents provide about their family and work histories to determine the measures of family structure and parental worklessness that applied when their children were growing up (from birth to age 16). This sampling strategy yields a sample (labelled "Individual Sample") of 948 men and 839 women (a total of 1787 individuals) who: (a) were aged 16 or over and were born between 1970 and 1983; (b) did not have any serious health problems or disabilities; (c) were living with their biological, adoptive or step-parent(s) for at least one year during the first nine waves of the survey (1991-9) and their mothers were interviewed in the second (1992) wave; and (d) had mothers from whom complete employment histories could be obtained covering their entire childhood (i.e., from birth to their sixteenth birthday) as well as information on other variables (see subsection 3.3). Condition (a) is imposed in order to have a relatively homogeneous group of young adults, who went through a relatively similar educational system, yet allowing for a sufficiently large number of birth cohorts. We introduce condition (b) to reduce the problem of parents' obviously choosing their employment (and family structure) patterns during the child's childhood based on health considerations for the child (this refers to equation (12) and the identifcation problem illustrated in subsection 2.3). Condition (c) allows us to recover a precise measure of family structure. Finally, we impose condition (d) so that, by construction, we would have full information on at least one of the variables that is crucial to construct our measure of parental joblessness during childhood.

Of the 1789 individuals in the Individual Sample, 604 (or 34%) could not be matched to their siblings in any of the panel years (either because their siblings were not interviewed between 1991 and 1999 or because they are only children). The remaining 1183 individuals are matched with at least one sibling (or half-sibling), and constitute our "Sibling Sample". To ease the interpretation of the statistics and the estimates, Table 1 presents the distribution of the individuals in this sample by household and number of siblings per household and the number of sibling pairs. The 1183 young adults come from 524 households: 408 of these households have two siblings in our sample, 98 have three siblings, 17 have four siblings, and one have five siblings. A total of 814 comparisions can then be computed. (In fact, the 408 two-sibling households give rise to 408 comparisons, one per sibling pair; the 98 three-sibling households produce 294 comparisons, three in each household; the 17 four-sibling households give rise to 102 comparions, six in each households; and the only five-sibling household in the sample yields 10 sibling comparisons, as there are 10 possible comparisons between five siblings). This sample allows us to estimate the treatment effects of interest here using sibling differences (814 sibling pairs), whereas the Manski's bounds, nonparametric estimates under conditional independence and level estimates are obtained from the Individual Sample. We have also computed the Manski's bounds and nonparametric estimates on both the Sibling Sample (treating each observation separately) and on the sibling differences. Because the results are similar to those presented here, we do not report them for brevity.

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. This information provides the basis for one of our treatment variables, i.e., childhood family structure, which is defined as whether or not the young adult spent time in a single-parent (lone-mother) family during his/her childhood. (Note that only a very small proportion of children in the BHPS has grown up in a single-father family.) 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 sixteenth 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 sixteenth birthday, either because of 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. (For a similar definition, see Bumpass *et al.*, 1995; Ermisch and Francesconi, 2000b). Because of both substantive issues and the identification arguments discussed in Section 2, 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.

The third wave (1993) of the BHPS contains retrospective information on job histories for all adult panel members interviewed that year. This included information about all jobs they held between the time they left full-time education and September 1990, when the first wave (1991) of the BHPS began collecting information. Young people are included in our analysis only if there is complete information about their mothers' employment patterns during childhood (as well as other background measures, such as maternal education and date of birth). If fathers are successfully matched to their natural, adoptive or step-children, but we could not obtain a complete record of their employment histories, we assume they always worked over the months for which this information is missing. By doing so, we are likely to reduce the incidence of parental joblessness, but its effect on youth outcomes would be unaltered if the father's information is missing at random (see Ermisch *et al.*, 2001, for a discussion of this issue). Parental joblessness at a given age (between birth and sixteen) is defined to occur when both the father and the mother were not in paid work for at least one month during the twelve months the child is at that age. Notice that mother and father are not required to be both out of the labour force or unemployed in the *same* month. This would reduce the incidence of parental worklessness quite dramatically and, as a consequence, our multivariate analyses would suffer from small ($X \times Z$) cells. In addition, for all those individuals who do not have a "father-figure" (i.e., biological/adoptive father or stepfather) present during the nine years of the panel who could be interviewed, the joblessness measure refers only to the non-employment patterns of mothers (see below).

Table 2 reports the means of the treatment variables for both the Individual Sample and the Sibling Sample. In this last case, we distinguish between the means computed on the sample of individuals ("levels") and the means of the sibling differences ("differences"). The column labelled "All ages" considers the value of the two treatment variables over the entire childhood of all young adults. We observe that almost 25% of the young adults in the Individual Sample had an experience of life in a non-intact family. This proportion is only slightly smaller for the people in the Sibling Sample (23%). The sibling difference figures, instead, reveal that only 5.2% of sibling comparisons have a different family structure over the entire childhood, the remaining 94.8% of sibling pairs have either always lived in an intact family or both siblings experienced a non-intact family during their childhood. Notice that, in the Individual Sample and the Sibling Sample (levels), the family structure measure is constructed in a way that the proportions in each of the developmental stages sum up to the total proportion. Of the young people who lived in a non-intact family in those two samples, almost 45% did so when they were under the age of six, while only 26-28% when they were adolescent (aged 11-15). The figures in the bottom panel of Table 2 are not directly comparable because they are obtained from sibling differences. For example, in a two-child family, one of the siblings may be aged 0-5 when the parents' union dissolves whereas the other is aged 6-10. Comparing those two siblings over their entire childhood would produce no difference in the family structure measure (all ages). But comparing them at the 0-5 stage, we find that the older was in an intact family, whereas the younger was not. Almost 10% of the sibling comparisons reveal a different experience of family structure when both siblings were aged 0-5, another 11% live in different family structures when aged 6-10 and about 6% have a different experience when aged 11-15.

Table 2 also reports the proportion of young adults whose parents have been observed workless sometime during their entire childhood and by developmental stage. This measure is not mutually exclusive by developmental stage: that is, having lived with jobless parents when aged, say, 0-5 does not imply having lived with jobless parents in the other developmental stages, not does it exclude such a possibility. (This is why the three figures by developmental stage do not add up to the figure for all ages.) The table shows that almost 47% of the young adults in the Individual Sample have lived at least one year of their childhood in which both parents were jobless. Interestingly, the largest incidence of parental joblessness (almost 40%) is observed when the child was a pre-schooler, the lowest (around 18%) when the child was an adolescent. The figures are similar when we consider the Sibling Sample in levels. The bottom panel shows that only 2.6% of the siblings have different patterns of parental joblessness over their childhood taken as a whole. But partitioning by developmental stage reveals greater within-family variation: indeed, 19% of the sibling pairs have experienced different parental worklessness patterns when both siblings were pre-schoolers, another 22% did so when they were in primary school (ages 6-10), and another 14% when they were aged 11-15.

3.2 Child outcome measures

Education—Our measure of educational attainment is achieving an A-level or higher qualification. (For readers unfamiliar with the British educational system, "(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.) For each young person, we take the 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 individuals who are in the panel at ages 18 or above. We therefore perform our analysis on 1489 young adults in the Individual Sample and 603 sibling comparisons in the Sibling Sample, respectively. Table 3 indicates that about 62% of the people in the Individual Sample have achieved a highest qualification of at least A-level. It also indicates that 34% of the sibling pairs report a different educational outcome. (Means computed on levels of the Sibling Sample are not reported both because they are close to those found for the Individual Sample and because they are not used in estimation).

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 9513 person-periods in the Individual Sample, and 6169 sibling-pair observations in the Sibling Sample. This last sample matches siblings on the year of observation (thus avoiding comparisons at different points of the business cycle). Table 3 shows that the inactivity rate is 7.2% for people in the Individual Sample, whereas the incidence of inactivity differs for about 13% of the sibling pairs under study.

Early childbearing—This outcome is defined as having a first birth at age 21 or less for women only. (Relatively few men in the sample had become fathers before their twenty-first birthday *and* lived with their children.) For the young women in our samples, we estimate the relationship between treatment variables (and other family background measures) and 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 twenty-first birthday. We have 2942 person-periods in the Individual Sample and 507 sibling pairs in the Sibilng Sample. Because having a child is inherently age-dependent, the sister comparisons are made at common ages. On average 2.6% of childless women aged 16-21 in the Individual Sample have a child each year, but the first birth rate increases with age. Life table estimates imply that 11% of young women would become mothers by their twenty-first birthday, which is less than the one-fifth of women born in 1975-1976 who have a first birth by their twenty-first birthday indicated by registration statistics (Office for National Statistics, 2000, Table 10.3). This difference is likely to reflect our sample selection criteria based on coresidence with parents: that is, women who became mothers early are less likely to be observed living with their parents in the BHPS. (In line with official statistics, data from the entire BHPS sample show that about 20% of women have a child by age 21.) Table 3 also shows that 3.4% of the sisters in the sample have a different early chilbearing behaviour.

Health—We analyse two measures of health-related outcomes. The first measure is defined as having pyschological distress, and it is derived from from the 12-item General Health Questionnaire (GHQ-12). The items relate to: (i) loss of concentration; (ii) loss of sleep; (iii) playing a useful role; (iv) ability to make decisions; (v) feeling constantly under strain; (vi) problems overcoming difficulties; (vii) enjoyment of day-to-day activities; (viii) ability to face problems; (ix) unhappiness or feeling depressed; (x) loss of confidence; (xi) belief in self-worth; (xii) general happiness (Goldberg, 1972). It has been shown that the most appropriate and efficient threshold to detect symptoms of psychological distress is four or more on the GHQ-12 (Goldberg *et al.*, 1998). For this reason, we use the GHQ-12 as a dichotomous indicator with a cut-off point at a score of four. Our second measure of health takes the value of one if an individual regularly smokes, and zero otherwise. (We performed the analysis using also another measure, based on whether an individual smokes 10 or more cigarettes a day, and obtained the same qualitative results. In what follows, therefore, we only present the estimates for the simple measure of smoking). The analysis on the health outcomes is conducted on 9513 person-periods in the Individual Sample, and 6169 sibling-pair observations in the Sibling Sample. In the latter, age enters parametrically, i.e., no age matching is imposed on sibling comparisons. Table 3 indicates that one in five young adults report a high level of psychological distress, and more than two in five smoke (Individual Sample). Nearly 27% and 36% of the sibling comparisons show a difference in the distress and smoking outcomes, respectively.

3.3 Other variables

By matching young adults with their parents, we are also able to measure other family background characteristics that would be unavailable otherwise, such as age of the mother and father at the young person's birth and parental education. We also obtain information on the smoking behaviour of the parent(s) at the time they lived with their young-adult child, which will be used for the smoking outcome. The Appendix Table A1 presents the means of the x variables included in the analysis. The figures in the two samples are computed for the last available year in which the young adults or the siblings are observed in the survey period under analysis. The level statistics on the Sibling Sample are similar to those of the Individual Sample and, therefore, are not reported.

Looking at the figures in the Individual Sample, the table indicates that 47% of the young adults are women and were born on average in 1975-1976. Their age ranges between 16 and 29, with a mean just above 22. Individuals are evenly spread across age groups, with 60 percent of the sample being 22 or younger. About 30% of the mothers of these young adults have no academic qualification, while almost 40% hold A level or higher qualifications. The educational distribution of fathers reflects the fact that we include fathers with no information on education in the base category (no qualification). (The statistics computed only on individuals who have father's information available reveal that 24% of young adults have fathers with no qualification and more than 50% have fathers with A level of higher education). On average, mothers gave birth at ages 26-27: 12% of the young adults were born when their mother was aged 21 or less, and almost 5% of them have mothers aged 35 or more at their birth. The age at birth of fathers is about 2 years higher, and this is also capture by the higher proportion of young people having a father aged 35 or more at birth (9%). Family size may be an important determinant of children's success, because parents' resources (time and money) are likely to be spread more thinly as the number of children increases (Stafford, 1987). Similarly, birth order is generally assumed to have a relationship with the way in which parents allocate their resources across children (Behrman and Taubman, 1986). In view of this, our analysis takes account of the number of brothers and sisters that each young adult has. It also controls for the possibility that the respondent is an only child and he/she is the firstborn in the family. Table A1 shows that about 7% of the young people in the sample are only children, while 36% are firstborn. Most of them have one or two siblings, on average 1.7

brothers and sisters (and a maximum of seven). Just under one in four of these young people have a mother who smokes, and 57% of them have a father who smokes. Notice that approximately 17% of the individuals in the sample do not have information about their father's working histories. An additional 13% do not have a father-figure present during the nine panel years who could be interviewed. So a total of almost 30% of the individuals have missing father's information. For them, as explained in subsection 3.1, the joblessness measure refers only to the non-employment patterns of the mothers.

The means for the Siblings Sample are quite different. This is because they refer to the average (absolute) differences between sibling pairs rather than the means of the variables of interest over all the individuals. Table A1 indicates that nearly 46% of the sibling differences are between a brother and a sister, the remaining 54% refer to same-sex siblings (either between brothers or between sisters). The mean age difference among siblings is slightly over 3 years (the median age difference is 2.8 years, while 75% of the siblings in the sample have just over 4 years of difference in age). More than two-thirds of the differences are taken between firstborn and higher-order siblings, the remaining differences are between higher-order siblings only. Almost 15% of the sibling differences involve one sibling born to a mother aged 21 or less at birth and another sibling born to the mother at an older age, another 4% are between one sibling born to a mother aged 35 or more at birth and another sibling born to the mother at a younger age, and the remaining 81% are between siblings whose mother was between 22 and 34 years of age at birth. The corresponding figures for father's age at birth are 5%, 7%, and 88%, respectively. Because the other family-specific variables in our data (e.g., parental education) are common to all siblings in the same household, they do not vary between siblings and cannot contribute to our estimation based on sibling differences.

4 Results

For each outcome, we report both Manski's bounds and nonparametric estimates determined under the assumption of conditional independence (or exogenous assignment) of family structure and parental joblessness. For bounds and nonparametric estimates, we compute bootstrap standard errors that are obtained with 500 replications. We also report the estimates from level and sibling difference logit regressions. For the sibling comparisons, the dependent variable takes the value of unity if one of the siblings (sorted first) reaches the outcome of interest (e.g., has an A level or above, smokes or is inactive) and the other does not, and zero otherwise. (Because of the non-random sorting of siblings, a constant term is included in all the sibling difference regressions. See Ashenfelter and Rouse, 1998). Throughout the parametric 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 over time. In the case of the education outcome, when all variables are measure at the last available year for each individual, the standard errors of the estimates obtained with the Individual Sample are instead robust to any form of correlation between siblings. To ease interpretation, the logit estimates are expressed in the form of a marginal effect for a young adult with average characteristics.

4.1 Benchmark estimates

Table 4 reports our basic results. In the case of the level and sib-difference models, it only contains the estimates of family structure (i.e., ever lived in a non-intact family during childhood) and parental joblessness (i.e., ever lived with jobless parents during childhood). The estimates of the other variables are not reported for convenience and can be obtained from the authors upon request.

The Manski's bounds and nonparametric estimates in Table 4 are computed on the entire Individual Sample without conditioning on X. The bootstrap standard errors indicate that, without exceptions, the bound estimates are precise. For the family structure treatment, inspection of Table 4 shows that the estimated lower bound is greater (in absolute terms) than the estimated upper bound in the case of education, while the opposite is true for the other four outcomes. In particular, the lower bound for education is -0.622 (with a bootstrap standard error of 0.012), and the largest upper bound, which occurs in the case of early childbearing, is 0.681 (s.e.=0.005). This suggests that having spent some time during childhood in a non-intact family may reduce the chance of achieving A-level or higher qualifications and increase the chances of inactivity, early childbearing, psychological distress and smoking in early adulthood. Indeed, the nonparametric estimates obtained under the exogenous assignment assumption confirm this expectation. Notice, however, that the estimate for early childbearing is not statistically significant at conventional levels (t-ratio=1.912).

The nonparametric estimates of the parental joblessness treatment reveal the same pattern of associations, that is, experience of life with jobless parents during childhood reduces the probability of achieving A-level or more and increases the chances of the other outcomes. The effect on education is again the largest (in absolute value) with a reduction of 15 percentage points, and the smallest is the effect on early childbearing with an increase of 2.6 percentage points (this last effect is, however, quite sizeable given the average probability of having a birth before age 21 is only 2.6%, see Table 3). But now the estimated bounds are generally more centered around zero than in the case of the family structure treatment. Therefore, unless we are willing to assume conditional independence, we cannot have strong expectations on the sign of the effect of parental joblessness on the outcomes of interest.

Before turning to the results from the parametric models, it is useful to consider the issue of the tightness of the Manski's bounds. With a width of one, they may not be informative, particularly if they are centered around zero, as it is in the case of parental joblessness. To tighten the bounds further, we estimate them after conditioning on some of the variables contained in x. Specifically, we partition x in 144 cells defined on: sex, individual's age (3 groups); whether the individual is firstborn; mother's education (2 groups); father's education (2 groups); and mother's age at child's birth (3 groups). We first compute lower and upper bounds for both treatment variables in each of the cells, $L(x)$ and $U(x)$, respectively. We then select the smallest and largest values for both the lower and upper bounds, $L_{\mathcal{S}}(x)$, $L_{\mathcal{L}}(x)$, $U_{\mathcal{S}}(x)$ and $U_{\mathcal{L}}(x)$. The difference between the largest upper bound and the smallest lower bound $(U_{\mathcal{L}}(x) - L_{\mathcal{S}}(x))$, which is greater than unity but at most equal to 2, provides a measure of the support of the treatment effect conditional on x. The difference between the smallest upper bound and the largest lower bound, $U_{\mathcal{S}}(x) - L_{\mathcal{L}}(x)$, which is at most equal to one, narrows the range of $\tau(x)$ down, and therefore should provide us with a better indication of where the treatment effect is likely to be.

The results of this exercise are reported in Table 5. The bounds around the family structure effect are precisely measured for all five outcomes. Furthermore, the difference $U_{\mathcal{S}}(x) - L_{\mathcal{L}}(x)$ is slanted towards negative values in the case of education (with the bounds being [-0.25, 0.11]), and towards positive values in the case of the other outcomes, the widest of which occurs for early childbearing and is $[-0.076, 0.571]$. Upholding our previous findings, these estimates suggest that ever residing in a non-intact family during childhood is likely to have adverse consequences on young adults' outcomes by decreasing their probability of achieving A-level or higher qualifications and increasing their probabilities of being inactive, having an early birth, feeling psychologically distressed and smoking. The results for the parental joblessness treatment are somewhat different. First, the sampling precision of the $U_{\mathcal{S}}(x)$ and $L_{\mathcal{L}}(x)$ bounds is low in the case of education. Second, the difference $U_{\mathcal{S}}(x) - L_{\mathcal{L}}(x)$ is slanted towards negative values for education (albeit not statitically significant) and early childbearing, with bounds being $[-0.223, 0.143]$ and $[-0.317, 0.164]$ respectively. For the other outcomes, this difference does restrict the range of possible effects but is not informative as to their sign (possibly with the exclusion of smoking). Having lived in a family with workless parents, therefore, seems to reduce young people's chances of achieving A-level or more and, somewhat surprisingly, those of early childbearing, and increase the probability of smoking. The effects on the probability of being inactive and feeling distressed cannot be signed.

To have a fuller understanding of the treatment effects on the outcomes under study, we turn to the point estimates obtained from the parametric level and sibling-difference models. These estimates are reported in the last two columns of Table 4. They always fall within both the bounds presented in Table 4 and the tighter $[L_{\mathcal{L}}(x), U_{\mathcal{S}}(x)]$ bounds of Table 5. This suggests that both models produce estimates that are compatible with the data. Notice also that, although the two models rely on different identifying assumptions, the treatment effect estimates are, in general, not statistically different from each other at standard levels of significance.

Having spent time with a single mother during childhood is associated with a significantly lower probability of achieving A-level or more. Given a baseline average probability of 62% (Table 3), the reductions by 7 and 17 percentage points (according to the level and sibling difference models respectively) are quite substantial. The 10-point difference betweeen the estimates of the two models is also the largest, but is not statistically significant. A family breakdown during childhood is also associated with: an increase of 3-5 percentage points in the probability of economic inactivity, an increase of 4-5 percentage points in the probability of an early birth (according to the sibling difference estimates only), an increase of 3-4 points in the probability of psychological distress (level estimates only), and an increase of 14-17 points in the probability of smoking. Interestingly, the strong impact of family structure on early childbearing found with the sibling difference model is the only significant departure from the estimates obtained with the nonparametric model under the exogenous assignment assumption. All the other effects that are determined with either the level model or the sibling difference model are consistent with our simple nonparametric approach, even without conditioning on X.

The parametric estimates of the parental joblessness treatment are qualitatively remarkably similar to each other. For each outcome, they are also similar in sign to the estimates of the family structure treatment, but their magnitudes differ quite considerably. Experience of life with jobless parents during childhood is associated with a reduction of 5-6 percentage points in the probability of achieving A-level or more: this effect is about 12 points smaller than the corresponding effect of family structure. Parental joblessness in the first sixteen years of life is also associated with an increase of 3 points in the probability of being inactive, 3-4 points in the chance of an early birth, 2-8 points in the probability of psychological distress, and 3-5 points in the probability of smoking. Again, most of these estimates are in line with those found with the nonparametric model, although the magnitude for some of them is statistically different, as in the case of education and psychological distress.

To summarise our results so far, we draw attention to three points. First, the way in which family structure and parental joblessness operate on young people's outcomes is strikingly similar. Experience of life with a single mother or with jobless parents 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 smoking and experiencing psychological distress. Second, despite this similarity, the size of the effects varies markedly. In particular, looking at the sibling difference estimates only—as they do not assume conditional independence, and impose relatively mild identifying restrictions—the effect of family structure is always significantly greater (in absolute value) than the effect of parental worklessness, regardless of the outcome. This suggests that family structure may be more important than parental joblessness in determining the young people's outcomes under study (McLanahan, 1997). Third, using the sibling difference estimates as the reference, the exogeneity assumption is arguably not far off the mark for both treatments. Therefore, within our sample, the nonparametric estimates based on exogenous assignment and unconditional on X are not only easy to estimate but also are a good marker for the relationship between treatments and outcomes. Whether this is true for other data sources or other treatments and outcomes remains to be seen. (For a similar finding, see Manski *et al.*, 1992).

4.2 Estimates by developmental stage

Not only are the estimates by developmental stage interesting in their own right and important for policy purposes, but they are also critical to the identification of the treatment effects in the case of the sibling difference models (see subsection 2.3.2). A causal interpretation can be given to the sibling difference model as long as we are willing to assume that the treatment-effect variables do not respond to, and are not correlated with, idiosyncratic differences in children's endowments. This seems to be more plausible when parents have less information about their children's endowments rather than more, which, in turn, is more likely to happen when children are very young, say before they start school. The sibling difference estimates when the child's age is between 0 and 5 are, therefore, our closest approximation to the causal effects of parental joblessness and family structure in our data. Nevertheless, we present results for three developmental stages over the entire childhood, ages 0-5, 6-10 and 11-15.

Tables 6-9 contain estimates of the bounds and the nonparametric, level and sibling difference models by developmental stage. Bounds and nonparametric estimates are again obtained without conditioning on X. The tables report also the p-values of χ^2 tests that the estimated coefficients in the are equal across developmental stages. Focusing on the results from the sibling difference model, the hypothesis that the three stage-specific coefficients are equal is rejected (at conventional statistical levels) in three of the five outcomes for parental joblessness (i.e., inactivity, early childbearing and psychological distress) and in two outcomes for family structure (inactivity and psychological distress). Thus, for such outcomes, the timing of parental worklessness and family disruption is potentially important. In all the other cases, however, the hypothesis of equality cannot be rejected.

Focusing mainly again on the sibling difference estimates, we sort our discussion on Tables 6-9 into three points. First, despite our previous comments on coefficient equality, the sibling difference estimates usually identify the strongest link between outcomes and family structure when the young adult was aged 0-5. An early experience of parental loss is, therefore, more likely to jeopardise children's subsequent educational career (Table 6), their chances of being economically inactive (Table 7), and their probabilities of experiencing psychological distress and smoking later in life (Table 9). (In the case of early childbearing, the effect of family disruption when girls are aged between 11 and 15 is larger, albeit not significantly, than the effect when girls are aged 0-5). Our estimates suggest that a family disruption in *early* childhood (or being born outside a live-in partnership) has the most pronounced consequences on later achievements, possibly through its effects on salient aspects of child's cognitive, cultural and social development (see also Duncan and Brooks-Gunn, 1997; Duncan *et al.*, 1997; Duncan *et al.*, 1998).

Second, the timing of parental joblessness during childhood is more complex. Parental joblessness when the child is aged 11-15 turns out to be especially important in increasing the chances of both psychological distress and smoking (Table 9). The chance of an early birth is highest and around 4 percentage points if a young woman spent some time with workless parents during her primary school years, ages 6-10 (Table 8). This suggests the importance of both role models that parents may have on their children—especially when they start their formal secondary socialisation process in school—and economic hardship (Hill and Duncan, 1987; Haveman *et al.*, 1997). On the other hand, young people's risk of economic inactivity is mostly influenced by experience of life with workless parents during pre-school years, indicative of the fact that economic hardship may be operative once again (Duncan and Brooks-Gunn, 1997). Finally, parental joblessness when children are either in pre-school years or in early adolescence seem to have identical negative effects on higher educational attainments. The possibility that the timing of parental joblessness during childhood has different impacts on different outcomes has implications for public policy vis-à-vis the timing of income supports (or cash transfers) to poor families.

Third, contrary to the results of Table 4, the sibling difference estimates by developmental stage reveal some meaningful and significant departures from the level and nonparametric estimates. For example, inspection of the level and sibling difference estimates for parental joblessness shows that the level estimates either are smaller in absolute value (as in the case of education, early childbearing and psychological distress) or reveal a different timing (inactivity, smoking) or both (early childbearing). Similar considerations emerge after comparing the sibling difference estimates to the nonparametric estimates. Notice that the nonparametric estimates are also not precisely measured in a few important cases, e.g., for parental joblessness on distress and education (across all age groups), and for family structure (ages 6-10 and 11-15) on education. So, although unconditional nonparametric estimates give us a good picture of the relationship between treatments and outcomes over the entire childhood, they perform less satisfactorily when we break up the effects by developmental stage. This is also the case for the level estimates. Our discussion in subsection 2.3 stresses the revelance of the timing of the treatments for a causal interpretation of the point estimates. In particular, the assumptions needed for identification of the treatment effects seem to be less restrictive if we estimate sibling difference models *and* the treatments are measured when children were aged 0-5 (i.e., when parents have only a limited amount of information on their children's idiosyncratic endowments). Perhaps more importantly, such assumptions are arguably less restrictive than those required by nonparametric models with exogenous assignment or by level models.

4.3 Gender differences

In the attempt to discover whether treatments and outcomes are differently correlated by gender, we performed the entire analysis presented so far for men and women separately. The results of this analysis are not shown for expositional convenience but can be obtained from the authors upon request. We again focus on the sibling difference estimates. Experience of life in a single-parent family during childhood is systematically associated with significantly worse outcomes for young men. Their probability of achieving A-level or higher qualifications is reduced by approximately 18 percentage points, while their risks of inactivity, distress and smoking are increased by about 6, 4 and 15 percentage points respectively. In the case of young women, having spent time with a single parent significantly increases the chances of inactivity and smoking, but does not significantly modify the probabilities of achieving high levels of formal education and experiencing psychological distress.

On the other hand, the effects of parental joblessness generally lose statistical significance in both samples, perhaps as a result of the smaller size of the two separate male and female samples. The negative effect on education is still large in magnitude (6.4% and 4.8% lower probabilities for men and women, respectively), but is no longer significant. The only exception occurs in the case of distress, in which case having lived with workless parents during childhood increases the risk by 8.3 and 6.9 percentage points for young men and young women respectively. For the other outcomes, the effects by gender, albeit insignificant, are close to each other and similar to those shown in Table 4, suggesting that the differences by gender are likely to be negligible.

4.4 Influence of other covariates

Among the many other variables included in the analysis (especially in the level models), we focus only on a few. Again, the results are not reported for brevity. The level models show that, as might be expected, the likelihood that young people hold an A-level or higher qualification increases with age. So, in the case of daughters, does the chance of giving birth for the first time by age 21. The risk of smoking is also greater as people become older up to age 22, and then tends to decline. The risks of inactivity and psychological distress are greatest at the ages of 19 and 20 respectively, and then taper off. However, from the sibling difference models, we find no evidence of age effects on any of the outcomes under analysis (except in the case of early childbearing, for which sisters are matched at the same age).

The relationship between parents' educational attainment and outcomes for their children can only be measured using the level models. In any case, the findings are interesting. The impact of mother's and father's schooling is large and statistically significant in the case of young people's education and economic inactivity. That is, more educated parents have children with a greater probability of higher educational achievements and lower risk of inactivity. The cultural milieu in which people grow up is therefore likely to be consequential to some of their outcomes as young adults, over and above the effects of parental non-employment patterns and family structure. Parents' education has instead a small and insignificant effect on early childbearing and the two health-related outcomes. The inclusion of parents' education, however, may be problematic for identification purposes. Indeed, in the context of the model presented in subsection 2.3.1, it is plausible that parental schooling is correlated with, say, family endowment ϵ_j, which implies $\pi \neq 0$, thus violating one of the conditions in (14).

Parents' own age when their children were born appears to play only a small part in shaping young people's later achievements. There is little evidence of any systematic and significant relationship between father's age at the time his child was born and any of the outcomes under study. However, having a young mother (aged 21 or less at the time of birth) increases the odds that her daughter has an early birth. This result is robust to family-specific fixed effects, as it also emerges from the sibling difference model. There is therefore evidence of a recurrence of early motherhood across generations. On the other hand, if mothers were aged 35 or more at birth, the chances of their daughters having an early birth are lower. The estimates from the level model also reveal a high persistence in smoking behaviour across generations. Young adults whose parents (mother or father) smoke experience a significant increase in their risk of smoking of about 15 percentage points as compared to young adults with similar characteristics whose parents do not smoke. As in the case of parents' education, the inclusion of (current) parental smoking behaviour may generate identification problems. Again, the correlation between parents' smoking and family endowment is arguably plausible.

4.5 Restricting the analysis on families with father present

As noted in Section 3, for about 30% of the young adults we do not have father's employment patterns, either because his work history is missing or because the father is not present in the household (see also Table A1). In the analysis so far, we have followed the common practice of retaining all individuals, substituting mean or reference values when father's information is missing, and indicating missing father or missing father's work information with two dummy variables. This approach maximises the size of the sample under study, but produces biased estimates for the treatment effects (especially in the case of parental joblessness), if there are unobserved or unobservable characteristics that affect child outcomes and are systematically correlated with the lack of information on father's non-employment patterns or father's absence. The size of the bias, however, is small as long as the covariances between mother's and father's joblessness are small, and the difference between the means of the father's worklessness variable in the missing and non-missing samples is small. Nonetheless, to reduce the potential for such a bias, we performed our analysis on another subsample that, from the Individual Sample, excludes cases with missing father's information. We end up with a subsample of 673 men and 585 women, a total of 1258 individuals.

The first column of Table 10 shows the marginal effects estimated from the sibling difference model applied to this subsample. For brevity, we only report the results that are obtained when the treatment variables are measured over the entire childhood (rather than by developmental stage). All the effects are similar in sign and magnitude to those found for the Individual Sample reported in Table 4, with the exception of the parental joblessness effects on education and psychological distress, which are now not significantly different from zero. This similarity suggests that the bias which affects the estimates of the Individual Sample is likely to be small. Notice also that the positive family structure effects on the probabilities of inactivity, early childbearing and distress are now slightly larger than the original effects shown in Table 4. Therefore, experience of life in a single-mother family during childhood may turn out to have even stronger consequences on later outcomes once the bias induced by father's information missing not at random is removed or, at least, limited.

4.6 Is there a sample selection bias?

The condition that children must coreside with their biological, adoptive or step-parent(s) in at least one interview during the panel years is imposed so that data on family background and family structure from the parents' records could be reliably matched with data on their children (see condition (c) in subsection 3.1). However, such a condition would create the potential for sample selection bias if there are unobserved factors affecting young adult outcomes that also affect the chances that children would be living with their parents. For this reason, from the Individual Sample we also constructed a restricted sample, consisting only of individuals who were living with at least one parent when aged 16-17. The justification for doing this is that, over the 1990s, 95% of all young people aged 16-17 live at home with their parents (Ermisch, 1999). This restricted sample, which consists of 693 men and 623 women (a total of 1316 individuals), is therefore likely to be a random sample, representative of the whole population of that age, from which any selection bias is largely attenuated.

The second column of Table 10 contains the estimates from this restricted sample. Again, these are sibling difference marginal effects with the treatments being measured over the entire childhood. With the exception of the effect of parental worklessness on inactivity (which now becomes small and not significant), the new estimates are remarkably similar to those reported in Table 4 for the whole Individual Sample. This implies that the sample selection bias induced by the coresidence condition may be inconsequential. The smaller (absolute) effect of family structure on education (a reduction of 10.8 versus 17.2 percentage points in Tables 10 and 4 respectively) is accompanied by a smaller baseline probability of achieving A-level or more in the younger restricted sample (57% versus 62%, see Table 3).

5 Conclusions

In this paper we estimate the relationship between several outcomes in early adulthood (educational attainment, economic inactivity, early childbearing, psychological distress and smoking) and two policy-relevant "treatments", that is, experience of life in a single-parent family and experience of life with jobless parents during childhood. Both treatments are strongly correlated with child poverty (Iacovou and Berthoud, 2000; Jenkins *et al.*, 2001) and highly pertinent to recent public policy initiatives in Britain (Department for Work and Pensions, 2001). We use a sample of young adults, selected from the first nine waves of the BHPS (1991-1999), who can be matched with at least one parent *and* one sibling over the same period. This sample allows us to estimate the relationships of interest using sibling differences. We also perform our analysis on another sample, in which young adults are matched with at least one parent, which we estimate using parametric level (logit) models. These estimates are useful for comparison with those available in the existing literature. Furthermore, we use this sample to compute Manski's bounds and nonparametric estimates of the treatment effects. Even after controlling for a large set of covariates, the bounds—which have a width of unity—are generally not tight enough to narrow the possible values of the treatment effects within a reasonable range. These are, however, the only estimates that can be derived from the data without imposing any identifying restriction.

We draw attention to six aspects of our findings. The first two are methodological, whereas the last four are substantive. First, we show that the estimates based on sibling differences require weaker assumptions (as compared to the assumptions imposed by commonly used nonparametric estimators under exogenous assignment and level estimators) for the identification of the two treatments under study, namely that family structure and parental joblessness during childhood do not respond to the differences in children's idiosyncratic endowments. Second, some of the estimates of the parametric models that do not impose the exogeneity assumption (both level and sibling difference estimates) suggest that this assumption is not far off the mark (see Manski *et al.*, 1992). On the other hand, the nonparametric bounds tests do not reject either of the parametric model specifications. Some other parametric estimates, however, do reveal significant departures from the nonparametric estimates, especially the sibling differences when the treatment effects are broken down by developmental stage. This is unfortunate because the identifying assumption of the treatment effects in sibling difference models is more likely to be met when the information that parents have on their children's endowments is low, that is, when children are young (e.g., ages 0-5).

Third, using such sibling differences, we find that experiences of life in a single-parent family and with jobless parents during childhood are usually associated with negative outcomes of children as young adults: lower education attainments, higher risks of inactivity and early birth, and higher chances of smoking and experiencing psychological distress. The effects of parental joblessness are similar in magnitude for young men and young women, but the detrimental effects of family structure appear to be greater for men than for women. Fourth, regardless of the outcome, the effect of family structure is in general significantly greater (in absolute value) than the effect of parental worklessness, suggesting that the intergenerational transmission of attainments and behaviours may operate more strongly through family structure than through parental joblessness (McLanahan, 1997). Fifth, family structure in early childhood (when the child was between the ages of 0 and 5) appears to be more important for shaping the five outcomes under analysis than does family structure during primary school years or early adolescence. Conversely, the timing of parental joblessness during childhood has more complex effects, with some outcomes more strongly influenced by parental worklessness during pre-school years (inactivity and education), others by worklessness during primary school years (childbearing), and others by worklessness during early adolescence (health and education). If the evidence about family structure effects has straighforward implications on the timing of monetary and non-monetary support to poor families (i.e., single-parent families with young children), the evidence on the timing of parental joblessness has more ambiguous consequences for policy interventions. Sixth, these results hold even when we account for father's absence or for sample selection based on coresidence with parents, that is, the biases that both missing father's information and coresidence with parents generate seem to be negligible in terms of the estimated relationship between our treatments and outcomes.

A number of extensions of this work would be desirable. For example, it may be important to see if our results hold even when a more standard measure of poverty based on household income (e.g., 60% of current household median net income) is used (Jenkins *et al.*, 2001). Another extension is to model and estimate the joint interplay of family structure and family poverty during childhood. This would help better underpin which of these two processes explains most of the variation in the outcomes of interest. Finally, including information on child care during pre-school years might either reverse or, if the results reported in NICHD Child Care Research Network (2000 and 2002) for the United States are also valid for Britain, reinforce our results. Such extensions require data that are not currently available in the BHPS for the cohorts of young people analysed here. But as the number of BHPS waves increases and children born in the panel grow older and are followed into their early adulthood, these extensions would open perhaps the most promising avenue for deepening our research on intergenerational links in the years to come.

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Table 1. Distribution of siblings (individuals) and sibling pairs in the Sibling Sample

	Number of:			
	Siblings per household	Households	Individuals	Comparisons (sibling pairs)
	2	408	816	408
	3	98	294	294
	4	17	68	102
	5	1	5	10
Total		524	1183	814

Table 2. Treatment Variables by Sample and Developmental Stage

Sample and treatment	All ages	Developmental stage (child's age) 0-5	6-10	11-15
Individual Sample [N=1787]				
Parental joblessness	0.469	0.390	0.251	0.175
Family structure	0.249	0.107	0.078	0.064
Sibling Sample (levels) [N=1183]				
Parental joblessness	0.475	0.389	0.244	0.189
Family structure	0.226	0.095	0.067	0.064
Sibling Sample (differences) [N=814]				
Parental joblessness	0.026	0.193	0.216	0.135
Family structure	0.052	0.095	0.108	0.062

Note: N is the number of young adults in the Individual Sample and Sibling Sample (levels) and the number of sibling differences in the Sibling Sample (differences), respectively.

Table 3. Mean Outcomes by Sample

	Individual Sample	Sibling Sample (differences)
Education	0.623	0.337
N	1489	603
Inactivity	0.072	0.131
N	9513	6169
Early childbearing[a]	0.026	0.034
N	2942	507
Distress	0.192	0.268
N	9513	6169
Smoking	0.425	0.355
N	9513	6169

Note: N is the number of observations (individuals or person-periods) used in estimation.
[a] Women only.

TABLE 4. Parental Joblessness and Family Structure during Childhood and Young People's Outcomes

Outcome and treatment	Manski's bounds Lower	Manski's bounds Upper	Nonparametric estimates	Level estimates	Sib. diff. estimates
Education					
Parental joblessness	-0.541	0.459	-0.152	-0.058	-0.051
	(0.013)	(0.013)	(0.025)	(0.027)	(0.024)
Family structure	-0.622	0.378	-0.158	-0.074	-0.172
	(0.012)	(0.014)	(0.031)	(0.031)	(0.061)
Inactivity					
Parental joblessness	-0.578	0.422	0.039	0.032	0.025
	(0.005)	(0.005)	(0.005)	(0.013)	(0.013)
Family structure	-0.256	0.744	0.045	0.030	0.052
	(0.005)	(0.005)	(0.007)	(0.014)	(0.011)
Early childbearing[a]					
Parental joblessness	-0.599	0.401	0.026	0.035	0.034
	(0.009)	(0.009)	(0.005)	(0.008)	(0.004)
Family structure	-0.237	0.763	0.015	0.006	0.046
	(0.008)	(0.008)	(0.008)	(0.004)	(0.005)
Distress					
Parental joblessness	-0.554	0.446	0.033	0.024	0.078
	(0.005)	(0.005)	(0.008)	(0.013)	(0.016)
Family structure	-0.319	0.681	0.048	0.042	0.031
	(0.005)	(0.005)	(0.010)	(0.015)	(0.020)
Smoking[b]					
Parental joblessness	-0.492	0.508	0.052	0.034	0.047
	(0.005)	(0.005)	(0.010)	(0.033)	(0.023)
Family structure	-0.410	0.590	0.139	0.138	0.169
	(0.005)	(0.005)	(0.013)	(0.036)	(0.018)

Note: Manski's bounds and nonparametric estimates are computed on the entire sample (without conditioning on X). Level and sibling difference estimates are marginal effects from logit regressions computed at average values of all variables used. Level regressions also include: age groups, gender, year of birth, mother's and father's education, mother's and father's age at child's birth (using the three groups in Table A1), number of brothers and sisters, whether respondent is only child, whether respondent is firstborn, and a constant. Sibling difference models also include differences in: gender, age, age of mother's and father's at child's birth (three groups of Table A1), whether respondent is firstborn, and a constant. Sister differences are taken at the same age in the case of the early childbearing outcome; in all other cases, age enters parametrically. Standard errors are given in parentheses. Bootstrap standard errors for Manksi's bounds and nonparametric estimates are obtained with 500 bootstrap replications. Standard errors for level and sibling difference estimates are robust to arbitrary forms of heteroskedasticity.

[a] Women only.

[b] Controls for mother's and father's smoking (level regression only).

TABLE 5. Smallest and Largest Manski's Bounds for Treatment Effects on Young People's Outcomes

Outcome and treatment	Lower bound Smallest ($L_{\mathcal{S}}(x)$)	Lower bound Largest ($L_{\mathcal{L}}(x)$)	Upper bound Smallest ($U_{\mathcal{S}}(x)$)	Upper bound Largest ($U_{\mathcal{L}}(x)$)
Education				
Parental joblessness	-0.857	-0.223	0.143	0.777
	(0.130)	(0.148)	(0.131)	(0.154)
Family structure	-0.889	-0.250	0.111	0.750
	(0.046)	(0.161)	(0.047)	(0.149)
Inactivity				
Parental joblessness	-0.866	-0.136	0.134	0.864
	(0.041)	(0.036)	(0.042)	(0.036)
Family structure	-0.433	-0.066	0.567	0.934
	(0.041)	(0.015)	(0.038)	(0.016)
Early childbearing				
Parental joblessness	-0.836	-0.317	0.164	0.683
	(0.048)	(0.059)	(0.048)	(0.057)
Family structure	-0.429	-0.076	0.571	0.924
	(0.037)	(0.021)	(0.039)	(0.022)
Distress				
Parental joblessness	-0.728	-0.284	0.272	0.716
	(0.030)	(0.052)	(0.031)	(0.055)
Family structure	-0.509	-0.152	0.491	0.848
	(0.033)	(0.042)	(0.034)	(0.043)
Smoking				
Parental joblessness	-0.629	-0.294	0.371	0.706
	(0.081)	(0.046)	(0.082)	(0.046)
Family structure	-0.600	-0.230	0.400	0.770
	(0.083)	(0.045)	(0.080)	(0.047)

Note: Bounds are estimated for 144 groups based on: individual's age (3 groups: age$\leq$19; 20$\leq$age$\leq$22; and age$\geq$23); individual is firstborn (2 groups); sex (2 groups); mother's education (2 groups: mother has less than A level qualification; mother has A level or higher qualifications); father's education (2 groups defined similarly to mother's education); mother's age at child's birth (3 groups: mother aged less than 22; mother aged between 22 and 34; mother aged 35 or more). Bootstrap standard errors (obtained with 500 replications) are given in parentheses.

TABLE 6. Parental Joblessness and Family Structure during Childhood and Young People's Educational Attainment by Developmental Stage

Treatment and developmental stage	Manski's bounds Lower	Manski's bounds Upper	Nonparametric estimates	Level estimates	Sib. diff. estimates
Parental joblessness:					
child's age 0-5	-0.624	0.376	-0.154	-0.009	-0.072
	(0.013)	(0.013)	(0.133)	(0.016)	(0.042)
child's age 6-10	-0.621	0.379	-0.023	-0.049	-0.010
	(0.013)	(0.013)	(0.131)	(0.025)	(0.052)
child's age 11-15	-0.624	0.376	-0.158	-0.046	-0.071
	(0.013)	(0.012)	(0.137)	(0.030)	(0.054)
Test of equality (p-value)[a]				0.561	0.491
Family structure:					
child's age 0-5	-0.634	0.366	-0.196	-0.101	-0.115
	(0.012)	(0.013)	(0.044)	(0.036)	(0.047)
child's age 6-10	-0.617	0.383	-0.086	-0.060	-0.033
	(0.012)	(0.013)	(0.051)	(0.027)	(0.017)
child's age 11-15	-0.617	0.383	-0.084	-0.047	-0.047
	(0.013)	(0.013)	(0.050)	(0.039)	(0.046)
Test of equality (p-value)[a]				0.125	0.089

Note: See note of Table 4.

[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 with 2 degrees of freedom.

TABLE 7. Parental Joblessness and Family Structure during Childhood and Young People's Inactivity by Developmental Stage

Treatment and developmental stage	Manski's bounds Lower	Manski's bounds Upper	Nonparametric estimates	Level estimates	Sib. diff. estimates
Parental joblessness:					
child's age 0-5	-0.076	0.924	0.104	0.023	0.036
	(0.003)	(0.003)	(0.049)	(0.010)	(0.008)
child's age 6-10	-0.077	0.923	0.041	-0.004	0.005
	(0.003)	(0.003)	(0.044)	(0.009)	(0.008)
child's age 11-15	-0.075	0.925	0.133	0.037	0.007
	(0.003)	(0.003)	(0.061)	(0.009)	(0.009)
Test of equality (p-value)				0.020	0.041
Family structure:					
child's age 0-5	-0.147	0.853	0.047	0.024	0.074
	(0.003)	(0.003)	(0.011)	(0.011)	(0.015)
child's age 6-10	-0.127	0.873	0.052	0.025	0.033
	(0.003)	(0.003)	(0.013)	(0.012)	(0.014)
child's age 11-15	-0.127	0.873	0.008	-0.004	-0.037
	(0.004)	(0.003)	(0.011)	(0.014)	(0.036)
Test of equality (p-value)				0.148	0.022

Note: See notes of Tables 4 and 6.

TABLE 8. Parental Joblessness and Family Structure during Childhood and Young Women's Early Childbearing by Developmental Stage — Women Only

Treatment and developmental stage	Manski's bounds Lower	Manski's bounds Upper	Nonparametric estimates	Level estimates	Sib. diff. estimates
Parental joblessness:					
child's age 0-5	-0.030	0.970	0.025	0.010	0.023
	(0.003)	(0.003)	(0.014)	(0.008)	(0.009)
child's age 6-10	-0.031	0.969	0.026	0.006	0.041
	(0.003)	(0.003)	(0.035)	(0.007)	(0.015)
child's age 11-15	-0.032	0.968	0.013	0.027	0.015
	(0.003)	(0.003)	(0.027)	(0.006)	(0.025)
Test of equality (p-value)				0.084	0.018
Family structure:					
child's age 0-5	-0.113	0.887	0.037	0.027	0.046
	(0.006)	(0.006)	(0.014)	(0.012)	(0.022)
child's age 6-10	-0.088	0.912	0.010	-0.013	0.038
	(0.005)	(0.005)	(0.009)	(0.010)	(0.024)
child's age 11-15	-0.087	0.913	0.001	0.001	0.059
	(0.005)	(0.005)	(0.012)	(0.011)	(0.008)
Test of equality (p-value)				0.103	0.214

Note: See notes of Tables 4 and 6.

Table 9. Parental Joblessness and Family Structure during Childhood and Young People's Health by Developmental Stage

Treatment and developmental stage	Manski's bounds Lower	Manski's bounds Upper	Nonparametric estimates	Level estimates	Sib. diff. estimates
Distress					
Parental joblessness:					
child's age 0-5	-0.197	0.803	-0.070	-0.005	-0.006
	(0.004)	(0.004)	(0.044)	(0.014)	(0.037)
child's age 6-10	-0.196	0.804	0.015	0.014	0.042
	(0.004)	(0.004)	(0.056)	(0.016)	(0.014)
child's age 11-15	-0.194	0.806	0.114	0.024	0.089
	(0.004)	(0.004)	(0.066)	(0.015)	(0.022)
Test of equality (p-value)				0.410	0.036
Family structure:					
child's age 0-5	-0.246	0.754	0.034	0.034	0.060
	(0.004)	(0.004)	(0.015)	(0.020)	(0.011)
child's age 6-10	-0.232	0.768	0.035	0.041	-0.036
	(0.004)	(0.004)	(0.016)	(0.025)	(0.037)
child's age 11-15	-0.226	0.775	0.053	0.053	0.017
	(0.004)	(0.004)	(0.018)	(0.023)	(0.010)
Test of equality (p-value)				0.786	0.019
Smoking					
Parental joblessness:					
child's age 0-5	-0.428	0.572	0.047	0.065	0.037
	(0.003)	(0.003)	(0.025)	(0.029)	(0.011)
child's age 6-10	-0.429	0.571	0.065	-0.028	0.004
	(0.004)	(0.005)	(0.012)	(0.021)	(0.010)
child's age 11-15	-0.427	0.573	0.032	0.023	0.077
	(0.003)	(0.005)	(0.021)	(0.034)	(0.012)
Test of equality (p-value)				0.078	0.057
Family structure:					
child's age 0-5	-0.407	0.593	0.182	0.176	0.193
	(0.004)	(0.005)	(0.017)	(0.046)	(0.076)
child's age 6-10	-0.422	0.578	0.101	0.122	0.145
	(0.003)	(0.005)	(0.019)	(0.053)	(0.069)
child's age 11-15	-0.430	0.570	0.036	0.069	0.086
	(0.004)	(0.006)	(0.021)	(0.057)	(0.049)
Test of equality (p-value)				0.258	0.230

Note: See notes of Tables 4 and 6.

Table 10. Treatment Effects in Two Special Subsamples — Sibling Difference Estimates

Outcome, treatment and sample size	Families with father present	Restricted sample
Education		
Parental joblessness	-0.028	-0.051
	(0.044)	(0.025)
Family structure	-0.162	-0.108
	(0.069)	(0.048)
N	476	435
Inactivity		
Parental joblessness	0.036	0.008
	(0.016)	(0.030)
Family structure	0.058	0.048
	(0.014)	(0.020)
N	4318	3784
Early childbearing[a]		
Parental joblessness	0.039	0.037
	(0.011)	(0.015)
Family structure	0.057	0.038
	(0.015)	(0.016)
N	442	387
Distress		
Parental joblessness	0.021	0.071
	(0.020)	(0.023)
Family structure	0.079	0.050
	(0.024)	(0.028)
N	4318	3784
Smoking		
Parental joblessness	0.032	0.035
	(0.029)	(0.037)
Family structure	0.147	0.126
	(0.036)	(0.031)
N	4318	3784

Note: N is the number of observations (sibling pairs or sibling-pairs per period) used in estimation. For the description of the subsamples, see text. For other details, see note of Table 4. Standard errors are given in parentheses.

[a] Women only.

TABLE A1. Means of the Other Variables Used in the Analysis

	Individual Sample (N=1787)	Sibling Sample (differences) (N=814)*
Female	0.470	0.458
Age (years)	22.062	3.331
Age group:		
16 (base)	0.078	
17	0.089	
18	0.083	
19	0.091	
20	0.091	
21	0.089	
22	0.082	
23 and more	0.0397	
Year of birth	1975.6	
Age of mother at birth (years)	26.528	
≤ 21	0.118	0.148
22-34 (base)	0.833	0.811
≥ 35	0.049	0.041
Age of father at birth (years)[a]	29.068	
≤ 21	0.036	0.048
22-34 (base)[b]	0.873	0.883
≥ 35	0.091	0.069
Mother's education:		
No qualification (base)	0.301	
Less than O level (or equivalent)	0.103	
O level (or equivalent)	0.202	
A level (or equivalent)	0.071	
Higher vocational qualification	0.245	
University and higher degrees	0.078	
Father's education:		
No qualification (base)[b]	0.473	
Less than O level (or equivalent)	0.053	
O level (or equivalent)	0.116	
A level (or equivalent)	0.076	
Higher vocational qualification	0.205	
University and higher degrees	0.077	
Number of brothers	0.903	
Number of sisters	0.804	
Only child	0.072	
Firstborn	0.360	0.674
Mother smokes[c]	0.224	
Father smokes[c,d]	0.565	
Missing father's work history information	0.167	
Missing father's information	0.297	

* Reports absolute value of differences between sibling pairs.
[a] Computed on cases with non-missing father's information only.
[b] Includes cases with missing father's information.
[c] Used in smoking regressions only.
[d] Excludes cases with missing father's information (which are included in the base category).