Parental Disruption and Adult Well-Being: A Cross Cohort Comparison

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ABSTRACT:

Although many studies examine the link between parental divorce and child well-being, some theories of the effects of divorce suggest that the negative associations should have declined over time. However, few studies have examined the extent to which the associations have remained stable over time. Using data from two British cohorts, we analyse both shorter- and longer-term outcomes of children who experienced a parental divorce and the extent to which the associations have changed over time. Estimating similar models for both cohorts, we find little evidence of any change in the size of the relationship as divorce became more commonplace.

INTRODUCTION

Amongst British children born since the 1950s divorce has replaced death as the main cause of family disruption and rising divorce rates have led to increases in the proportions of children that have experienced the break up of their parents' marriage. There is now a substantial body of research that has demonstrated for a range of nations that children whose parents divorce are more likely to be disadvantaged on a range of childhood, adolescent and adult outcomes (for reviews see Amato and Keith 1991a, 1992b; McLanahan and Sandefur 1994; Rogers and Pryor 1998; and Sigle-Rushton and McLanahan 2004). In the British context, statistically significant associations of poor outcomes with parental divorce have been reported using studies of individuals from three different birth cohorts born in 1946, 1958 and 1970 (see for example, Wadsworth and MacLean 1986; Kiernan 1992; Cherlin Kiernan and Chase-Lansdale 1995; Kiernan 1997). While results for individual cohorts are well-documented, very few studies have examined the extent to which the associations have remained stable over time.

This is an important issue to explore because some well-cited theories of the effects of divorce suggest that the negative associations should have declined over time. For instance, as divorce becomes more commonplace, the selection hypothesis would suggest that the average child of a divorced family would come from less problematic families. As a result, the bias due to omitted variables (third variables that are associated with both parental divorce and subsequent disadvantage) should be lessened. In addition, the liberalisation of divorce law and a greater reliance on mediation are both likely to have reduced family conflict and stress. Family conflict and stress have both been strongly implicated as correlates of both childhood and adolescent problems (Amato and Keith 1991a; Morrison and

Cherlin 1995). Taking a wider perspective, as alternative family structures have become more widely accepted, divorce has been accompanied by less stigma and any negative effects of community disapproval should have lessened over time. Finally, with increased family breakdown came increasing levels of information on the effects of divorce on children. Consequently, parents may have become more aware of the potential difficulties and tried to mitigate the effects of separation on their children. Motivated by these considerations, we seek to determine whether there is evidence for the hypothesis that parental divorce has become less strongly associated with poor child outcomes during childhood and adulthood as divorce has become more commonplace.

To date there is mixed evidence on whether the impact of divorce on children has changed over time. An early review of the literature reported that divorce effect sizes had decreased when early and older studies were compared (Amato and Keith 1991), and more recent evidence suggests that the positive relationship between parental divorce and own divorce have both attenuated over time (Wolfinger 1999). But other recent studies find no changes over time in the associations of father absence with educational attainment and occupational status. Indeed, recent studies suggest that the effects of family structure on, for example, educational attainment have remained fairly constant in both the United States and the United Kingdom from the 1960s to the 1990s (Ely et al. 1999; Biblarz and Raftery 1999).

In this study we make use of data from two British cohorts, born in 1958 and 1970 and followed up from birth into adulthood, to analyse the life experiences and chances of children who experienced a parental divorce and the extent to which the negative effects of parental divorce have remained stable or not over time. Although born only twelve years apart, the social contexts in which the two cohorts grew up were very different. Most

pertinent for this study, the divorce rate climbed dramatically throughout the 1970s and the cohort born in 1970 were far more likely to see their parents divorce and to grow up in a social environment where divorce was more common and alternative family structures increasingly condoned.

The prospective nature of the data allows us to examine whether the impact of divorce on behavioural and academic attainment during childhood has changed over time; as well as whether the size of the effects for adult well-being (at age 30 in the case of the 1970 cohort and age 33 in the case of the 1958) in terms of educational qualifications attained, being on welfare benefits and emotional well-being have changed. Additionally, these longitudinal data allow us to start with a sample of children whose parents were still together at the first childhood follow-up and control for characteristics that predated family disruption and thus control for some selection effects.

METHODS

Data

This study uses data from the National Child Development Survey (NCDS) and the British Cohort Study (BCS), two nationally representative, longitudinal studies of birth cohorts in Great Britain. Both studies are similar in design; the NCDS study follows the lives of a cohort of children born in one week of March in 1958, and the BCS study follows another cohort born in one week of April in 1970. Both original samples provide information on over 17,000 births, and the data collected at the baseline provides detailed information on the provision of ante- and post-natal services for the study of perinatal and infant mortality. Both studies have followed the cohorts over time and later waves, although not always interviewing children at the same ages, are similarly designed and include a broad range of

socio-economic, demographic, health and attitudinal measures (Despotiduou and Shepherd 1998). The first two follow-up interviews took place when the NCDS children were aged seven and 11 and the BCS children were aged five and 10.[1] Although the studies do not use exactly the same instruments, there is a good deal of overlap in the information provided. At both waves, detailed information on the child's behaviour was obtained from the mother (and from teachers at age seven and 11 in the NCDS and at age 10 in the BCS), and information on the child's academic ability was assessed using standardised tests. Parent and household information was also collected at these childhood waves. In addition, both cohorts were interviewed as adults; the fifth NCDS follow-up interview was conducted when cohort members were aged 33, and the fifth BCS follow-up interview was conducted in 2000 when the cohort members were aged 30.[2] These data provide a unique opportunity to research two groups of children born twelve years apart and to explore changes in the association of family disruption with subsequent disadvantage over a period when family breakdown was becoming more commonplace.

The length of time between interviews means that sample attrition, particularly at older ages, is inevitable. The age 30 BCS response rate -- defined as the number of achieved interviews divided by the initial sample[3] of cohort members -- was 69.9% (Collins et al

[1] A third childhood follow-up interview was conducted when both cohorts were aged 16.
[2] NCDS cohort members were also interviewed at age 23 and 42, and BCS cohort members were interviewed via a short postal questionnaire at age 26.

[3] By the age 30 interview, 1.6% of cohort members were classified as permanent or proxy refusals, 1.2% had emigrated so were not contacted for interview, and 0.6% had died. These individuals (along with a small number who were found to have a birthday outside of the

2001). When NCDS cohort members were interviewed at age 33, the response rate was similar. Out of an initial sample of 16,455 cohort members known and thought to be alive, 11,363 were successfully traced and interviewed. Comparisons show that the achieved samples do not differ a great deal from other survey samples of the British population although there is a slight under-representation of the most disadvantaged groups (Fogelman 1983; Shepherd 1997).

Sample Selection

For this analysis we restrict each sample to include only those children who were living with both biological parents at the time of their first follow-up interview (wave 1). For our examination of age 10 or 11 outcomes, we further restrict the sample to those whose parents provided information on their family structure at the second follow-up interview (wave 2). Similarly, when we examine adult outcomes, we restrict the sample to those individuals for whom we can determine whether or not there was a parental divorce or separation before the age of 17. The latter information was collected for both cohorts at the third follow-up interview, administered at age 16 for both samples. In addition, members of the BCS cohort were asked, at the age 30 interview, about any family disruptions that occurred during childhood. These additional questions are used when the age 16 information is not available. Members of the NCDS cohort were asked to provide only limited information on family disruptions at age 33, but at their sixth follow-up at age 42, they were asked to provide detailed information. In order to maximise our samples, when information on family structure at age 16 is missing, we use information collected at ages 33 and 42 to determine

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survey reference week) are not included as part of the initial sample in the calculation of the response rate (Collins et al 2001).

whether or not a family disruption occurred. Because we are interested in parental dissolution and its association with indicators of disadvantage, we eliminate from the sample those individuals who experienced a parental death.

The restrictions outlined above do not reduce the achieved samples appreciably. For the NCDS sample we have 14,707 children with information on their family structure at age 7, 13,520 (91.9%) of whom are living with both natural parents. Of these, 1,998 children are missing information on their family structure at the second follow-up at age 11. This leaves 11,522 children with information on their family structure. After removing from the sample those children who are living in foster care, who experienced a parental death and who are missing information on some of the control variables (we remove from the sample the small number of individuals who were missing information on their father's occupational class and their parent's housing tenure at age 7), the NCDS sample totals 11,225 children. For the first BCS follow-up at age five, there are 13,135 children with information on their family structure, 11,849 (90.2%) of whom are living with both natural parents. Of these, 1,499 children are missing information on their family structure at the second follow-up at age 10. Most of these 1,499 are missing because their families were not interviewed at wave 2. There are few missing cases as a result of non-response. This leaves 10,350 children with information on their family structure. After removing from the sample those children who were living in foster care, who experienced a parental death or who were missing information on some of the control variables, our BCS sample includes 10,162 children.

Of the 13,520 cases in the NCDS sample that were identified as living with both natural parents at the first follow-up wave, 9,503 (about 70%) have information on family disruptions up to age 16, and are not eliminated as result of parental death or missing

information at wave 1. For the BCS sample, the percentages are somewhat higher. Of the 11,849 individuals who were reported at age five to be living with both natural parents, 9,242 (about 78%) have information on parental separation or death up to the age of 16 and are not eliminated because of a parental death or because they lack information on some of the wave 1 control variables. Unfortunately, for both sub-samples, not all cases have complete information for all of the outcome variables that we consider. Rather than further restrict our samples, we opt to include as many valid observations as possible. This means that our sample sizes fluctuate and are not entirely comparable, but the gains from using as much valid information as possible most probably exceed the benefits of using entirely identical but smaller (and potentially less representative) samples.

Restricting the samples to those children who were living with both natural parents at wave 1 allows us to obtain baseline information on child and family characteristics for a group of children from non-disrupted families, some of whom later experienced a family dissolution and some of whom did not. Because we are interested in both the short and long term associations, we examine both short-term, pre-adolescent outcomes (measured at age 11 for the NCDS sample and at age 10 for the BCS sample), and longer term adult outcomes. Using logistic regression, we examine the associations of parental disruption, controlling for a range of pre-disruption antecedents.

Dependent variables

Child well-being. In order to assess the well-being of children, we use measures of the child's temperament and academic success at age 11 for the sample drawn from the NCDS cohort and at age 10 for the sample drawn from the BCS cohort. For temperament, we examine three measures each of which is constructed using parental responses to a series

of questions concerning their children's behaviour. A battery of questions, devised by Rutter and colleagues (1970), was asked at the first and second follow-ups of both studies. In most cases, parents were provided with a series of descriptions and asked to report whether each description certainly applied, somewhat applied, or did not apply to their child. Although both the wording and coding of the inventory was somewhat different for the BCS cohort at age 10, we have attempted to define our categories at age 10 in as meaningful and consistent a way as possible.[4] Nonetheless, the differences between the two inventories are substantial enough that readers should interpret differences across samples with some caution.

Following Hobcraft (1998), we group 11 items into three categories. We use parental assessments of how often the child fights with other children, is irritable, is destructive, and is disobedient to construct a measure of "aggression". We use parental reports of the extent to which their child is a worrier, a loner, miserable or tearful, and afraid of new situations to construct an "anxiety" measure. Finally, characterisations of the child as being squirmy or fidgety, having twitches or mannerisms, and having difficulties concentrating are used to construct a "restlessness measure".

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[4] At age 10, parents were given, for each description, a line with "certainly applies" at one end and "does not apply" on the other. They were then asked to "…make a vertical mark though the line…to indicate the extent to which the statement applies to your child's behaviour". Where the mark fell on the line was then coded into a scale from 1- 99 with 99 being the most extreme agreement with the statement and 1 being the most extreme disagreement with the statement. We have divided the 1 - 99 scale into thirds corresponding to the certainly applies, somewhat applies and does not apply categories.

Each item was coded on a scale of 0 to 2 with 0 meaning not applies, 1 meaning somewhat applies, and 2 meaning certainly applies. Within each group, the items were summed together to create three overall scores ranging from 0 - 8 for the aggression and anxiety scores and 0 - 6 for the restlessness score. We then classified each sum as low, moderate, high or missing. For aggression and anxiety, a sum total of 0 or 1 was coded as low, 2 or 3 was coded as moderate, and greater than or equal to 4 was coded as high. In the case of restlessness, a sum of zero was coded as low, 1 or 2 was coded as medium, and greater than 3 was coded as high. Our three temperament outcomes are constructed as indicator variables that equal one if the child scores high on aggression, anxiety or restlessness, respectively. The distributions of these outcomes are presented for both samples in Table 1.

Although the distributions are similar across cohorts, the NCDS sample has a slightly higher percentage of cases with high scores for anxiety and restlessness. In both samples, 12% of the cases are coded as having high aggression scores and high anxiety score is the most common of the three outcomes (26% of the NCDS sample and 20% of the BCS sample have a high anxiety score).

We use scores on standardised academic tests to measure academic achievement at the second follow-up interviews. For both cohorts, the outcome is constructed by combining a verbal test score and a quantitative test score, but because the students were not administered the same test, cohort differences should be interpreted with caution. Each test score was standardized to have a mean of zero and a variance of 1, and the two standardized test scores were then added together. Poor academic achievement is measured with an indicator variable that equals one for those children whose test scores place them in the

bottom quartile of the distribution of scores (for the full sample of children with test scores at age 11 or 10, depending on the sample). Frequencies for this outcome are presented in Table 1. For both cohorts, our restricted sample is slightly less likely to have had low scores suggesting a small amount of positive selection on academic performance as a result of our sample restrictions.

Longer-term adult outcomes. We measure socio-economic and psychological well-being in adulthood using three indicator variables, the distributions of which are presented in the lower half of Table 1. To measure poor academic success and labour market preparation, we use an indicator variable that is set equal to one for those cohort members who lack any academic or vocational qualifications at age 33 or 30 for the NCDS and the BCS samples, respectively. In Table 1, we see that, across cohorts, a lack of educational qualifications became less common over time (Bynner and Joshi 2002). In 1991, 12% of the NCDS sample reported having no qualifications, and in 2000, just 7% of the BCS sample reported having no academic or vocational qualifications.

Economic well-being and instability is measured using an indicator that equals one if the cohort member reports being in receipt of non-universal (means tested or targeted) benefits when they were interviewed in the fifth follow-up survey. Benefit receipt is relatively common in both samples and the frequencies are similar across the two cohorts. About 16 percent of the NCDS sample reported receipt of non-universal benefits at age 33 and 14 percent of the BCS sample reported receipt of non-universal benefits at age 30.

Mental health and well-being is measured using the Malaise Inventory, a 24-item battery of questions designed to identify those individuals at high risk of depression (Rutter et al 1970). The items cover a range of symptoms associated with depression, and, similar to

previous work, we classify those individuals answering yes to at least seven of the 24 items as being at high risk of depression (Richman 1978; Rutter et al 1976). As documented elsewhere, high malaise has become more common in the younger cohort (Bynner et al 2002). Individuals in the BCS sample are much more likely to have a high malaise score at age 30 than were individuals in the NCDS sample at a similar age.

Control variables

Family disruption. Our models compare children who experienced a parental divorce or separation with those children who did not. For the childhood outcomes, our parental divorce variable identifies those children who experienced a parental divorce or separation sometime between the first and second follow-up waves. At the second follow-up, information was collected on the relationship between the cohort member and their mother figure and father figure. Choices include natural parent, adoptive parent, stepparent, foster parent, grandparent, elder sibling, natural parent's cohabiting partner, other, and no mother/father figure. When the father figure or mother figure is not the natural parent, interviewers were instructed to determine why a separation had occurred. Responses to these questions allowed us to identify whether a disruption was due to death or separation. We drop from the sample those children who experienced a parental death. For a small number of cases, we were not able to identify the reason for a parental separation, and these children were coded as having experienced divorce or separation.[5]

For the adult outcomes we identify those individuals who experienced a parental divorce or separations sometime between the first and third wave (conducted at age 16), or

[5] This was a small number of children and our results do not change substantively if they are omitted from the sample.

when supplemental information is utilised, before the age of 17. At age 16, similar information on family structure and reasons for changes in family structure were also collected. Because many of the individuals in our samples were not interviewed at age 16, we use information collected at later waves when data is missing. At the time of their age 30 interview, members of the BCS cohort were asked whether or not their parents had ever permanently separated and, if so, how old they were when it happened. At age 33, the NCDS cohort was also asked whether or not their parents had ever permanently separated and if so how old they were when that happened. This additional information collected at later ages helps to augment our older sample, increasing the number of valid cases substantially.

In Table 2, we see that most of the individuals in our samples lived with both natural parents during childhood, but there are some noticeable differences across cohorts. Parental divorce or separation was more common in the BCS sample. Between ages seven and 11, only 2.3% of the NCDS sample had experienced a parental divorce or separation, compared to 6.3% of the BCS sample between ages five and 10. By age 16, 7.7% of the NCDS sample had experienced a parental divorce after age seven and 16.4% of the BCS sample had after age five. Inter-cohort comparisons are made more difficult by the differing lengths of exposure, but although divorce was more common in the later sample, parental divorce was relatively more likely to occur at older ages in the NCDS sample, reflecting the increasing incidence of divorce over time.

Wave 1 control variables. To estimate our full models, we control for a variety of child and parent characteristics, all measured in the first follow-up wave prior to any family disruption. Child characteristics include the child's sex, along with measures of temperament and academic test scores. Parent and household characteristics include whether the family

lives in public (local authority) housing, the father's occupational class, and whether one or both of the parents reads to the child. In both samples, the numbers with missing information on parental housing tenure were small (less than 30 cases per sample) once we restricted the sample to those who were identified as living with both natural parents at wave 1. For this reason, the sample does not include those individuals with missing information on this variable. Although the numbers were somewhat higher, we also removed from the sample those cases where information on the occupational class of the father figure was missing. To maximise sample size, all remaining controls explicitly identify those individuals with missing information, and a missing indicator is included in our models. The distributions of these variables for our restricted sample are presented for each cohort in Table 2.

The wave 1 temperament scores for aggression, anxiety and restlessness are constructed using the same questions and scoring methods that were described above for the wave 2 outcomes. For the control variables we identify those children with high scores – at least four out eight for aggression and restlessness and at least two out of six for restlessness. In addition, we also identify a moderate group. Those individuals with scores of two or three out of eight are identified as having moderately high aggression or anxiety scores. Those with scores of one or two out of six are identified as having moderate restlessness. The group with the lowest scores forms our reference category.

In Table 2, we see that the temperament scores are distributed similarly across cohorts. The distribution of anxiety scores seems to differ most across cohorts, and comparing with the data in Table 1 it differs most by age as well. Low wave 1 anxiety is more common in the BCS sample than in the NCDS sample, although nearly all of the difference is driven by a larger percentage of NCDS cases falling into the moderate category

as opposed to the high. Unlike the other temperament scores, the frequency of high anxiety increases substantially between waves 1 and 2 – from 11% to 25% in the NCDS sample and from 11% to 20% in the BCS sample. Restlessness and aggression scores are similarly distributed both across cohorts and, within each sample, over time. Missing information on these scores is much more common in the later cohort than in the earlier.

Academic test scores are constructed similarly to the wave 2 academic test scores described above, but differences in age at the time of the first follow-up mean that the tests used to measure cognitive ability at wave 1 differ according to cohort. For the NCDS cohort, our tests are more similar to those used at wave 2. We combine the scores from two general ability tests: one focusing on reading skills and the other on mathematics skills. For the BCS sample, at age five, we use tests of the child's vocabulary and of the child's ability to copy designs. Because the age five tests do not capture mathematical abilities as well as we would like, it is possible that we will find a stronger correlation between early and later academic performance for the NCDS sample simply because the tests we use are more similar. Despite the differences in content, the variables are constructed the same way. We identify those individuals who scored in the top quartile of the distribution and those who scored in the bottom quartile. As with the temperament scores, we identify those individuals with missing test scores, and include the missing indicator in the analysis. Those whose scores placed them in the middle two quartiles form the reference group. Similar to what we saw in Table 1, our restricted samples have a slightly smaller percentage of bottom quartile scores than the full sample, but the differentials are not large. In addition, missing information on academic test scores is more common in the BCS sample than in the NCDS sample.

The data in Table 2 show that, although local authority housing was very common in both cohorts, it was more common in the earlier cohort than in the later. With a 10 percentage point differential, this is one of the largest differences we see across cohorts for any of our controls. About 40% of seven year olds in our NCDS sample were reported to be living in local authority housing. Ten years later, around 30% of 5 year olds were identified as living in the same sort of housing.

Father's social class is broadly defined and identifies those men whose current or last job was in a non-manual, skilled manual, or semi- or unskilled manual occupation. Our controls include indicators for skill manual and semi- or unskilled manual, with non-manual forming the reference group. Skilled manual is the most common occupational class in both samples, although when we compare the samples, non-manual occupations are slightly more common and unskilled manual slightly less common in the BCS sample.

Our final wave 1 control is an indicator that is set equal to one if one or both parents were reported to read with the child. The questions used to construct this variable are slightly different, however. The NCDS sample contains information how often the mother reads to the child and how often the father reads to the child. Responses are frequently, occasionally, or rarely or not at all. The BCS sample contains information on whether the mother and/or father read to the child in the last seven days. For the earlier cohort, we identify those individuals who are read to occasionally or more often by at least one parent, and for the later cohort those who were read to in the last seven days by at least one parent. This information is not entirely comparable but resulted in the most similar frequencies across cohorts (all other coding strategies resulted in differentials that seemed implausibly large). Differences in the way the variable is measured probably account for the greater

percentage of parents who are coded as not reading to their child in the BCS sample, and the reading indicator is probably picking up a higher level of reading activity in the BCS sample than it is in the NCDS.

We cannot argue that our set of control variables is exhaustive; indeed we were limited to information included in both samples. Nonetheless, we would maintain that our controls are comprehensive in the sense that they represent multiple theoretical and disciplinary perspectives. We include pre-disruption proxies for academic ability (academic test scores), parental engagement (reading with the child), socio-economic characteristics (occupational class of the father, housing tenure), and individual temperament (aggression, anxiety, and restlessness scores). Our inability to control for parental conflict is the most glaring omission in our models because there is good evidence in the literature that conflict substantially mediates the effects of disruption (see, for example, Jekielek 1998). Ideally, we would have liked to include measures of the parent's mental health and alcohol problems or financial problems – indicators of conflict if not direct measures of conflict -- but these measures were only available in one data set and not the other.[6] When we introduced additional indicators that we thought might be important mediators but were only available for one sample (results not shown here but available from the authors on request), they were

[6] Mental health is the only possible exception. The NCDS data have information on whether or not there are any mental health problems in the family at wave 1 and the BCS data have information on the mother's wave 1 malaise score at wave 1. Because our aim was to estimate models as similar as possible in order to assess cohort differences in the effects of disruption, these variables did not seem similar enough to justify their inclusion. Neither one changed substantially the parameter estimates for divorce once they were included.

often significantly associated with the outcomes but the family disruption parameters were very little changed.

RESULTS

For each outcome in Tables 3-5 we present two models. The first model includes only the divorce indicator as a control, and the second includes our set of pre-disruption characteristics. This allows us to assess the extent to which controlling for a range of pre-disruption characteristics affects the disruption parameter estimates. We also report whether or not the estimated parameters differ significantly across cohorts. We are particularly interested in whether or not the size or significance of the disruption parameters has changed over a time period when disruption was becoming increasingly common. The estimated parameters for all the logistic models are presented as odds ratios – the proportional change in the odds of an event occurring due to a change from zero to one of the corresponding control variable. When an odds ratio exceeds one, there is a positive association between the control variable and the outcome, and when an odds ratio is less than one, there is a negative association between the control variable and the outcome.

In the results presented here, we have controlled for the sex of the child in our final models, but have not shown the results when the models are estimated for males and females separately. Although previous research suggests that in some cases, the parameter estimate for disruption may differ significantly by gender (see, for example, Cherlin et al 1991; Cherlin et al 1995; Jekielek 1998), we, in fact, found no significant gender differences in the parameters for disruption when we estimated the fully interacted models. We also estimated models in which only the behavioural scores were interacted with sex and found no significant interactions. In order to present and discuss any cohort differences in a more

accessible way, we decided not to present the results pertaining to the sex specific models or interactions in the main body of the paper, but they are available on request from the corresponding author.

Child well-being

In Table 3, we present results pertaining to wave 2 (age 11 for the NCDS sample and age 10 for the BCS sample) aggression, anxiety and restlessness. In the first and third columns, we see that in both samples, those children who experienced a parental divorce or separation are more likely to have a high aggression score, but the parameter for divorce is only significant at conventional levels for the BCS sample. When pre-disruption characteristics are added, the odds ratios for divorce and disruption fall – from 1.31 to 1.08 for the NCDS sample and from 1.55 to 1.28 in the BCS sample. Even with the inclusion of the pre-disruption controls, the parameter for parental divorce or separation remains significant for the BCS sample and insignificant for the NCDS sample. Nonetheless, cohort differences in the effect of divorce are not large enough to be statistically significant.

In columns 4 and 6 of Table 3, we see that, compared to children with no disruption, children who experienced a parental divorce are significantly more likely to have a high anxiety score at age 11 or 10. The odds ratios for parental divorce or separation are very similar across samples. As we observed in the results for aggression, when we introduce the set of pre-disruption controls (columns 5 and 7), the parameter estimates for divorce fall, but more for the NCDS sample. Indeed, for the NCDS sample, parental divorce or separation is no longer significantly associated with high anxiety at age 11, but for the BCS sample, the

parameter remains significant at conventional levels with an odds ratio of 1.30. Once again, the divorce or separation parameters do not differ significantly across cohorts, however.

Parental divorce is not significantly associated with high restlessness in either model or for either cohort. This is the only outcome for which there is no evidence in either sample of an association of parental divorce and disadvantage even at the bivariate level. In both full models, the parameter for parental divorce changes from borderline significant to insignificant.

Putting these results together, where there is evidence of a link between divorce and behavioural problems, it is in the BCS sample only, and then for two out of the three outcomes. In the NCDS sample, children who experienced a parental divorce are no more likely to have high scores on any of the temperament measures we consider, although in the case of anxiety the association only becomes insignificant when we introduce the set of pre-disruption controls. Contrary to the hypothesis that the associations might decline over time, we see in these tables, that if anything, there is evidence for an emerging relationship – at least when it comes to aggressive or anxious behaviour. Substantial differences in the measurement of the wave 2 outcomes means, however, that our conclusion of cohort differences is tentative at best.

Several pre-disruption characteristics are significantly associated with high wave 2 behavioural scores, but there are few significant cohort differences. Not surprisingly, children with evidence of a behavioural problem at wave 1 were more likely to have a high score on that same outcome at the second follow-up wave. In addition, children identified as restless in wave 1 were more likely to have high scores on all three outcomes in wave 2. Aggressive children in the BCS sample are more likely to have high scores on all three

outcomes as well. There is also, for the NCDS sample, a significant link between high wave 1 aggression and subsequent restlessness. For all three outcomes, children with low (high) test scores at wave 1 are more (less) likely to have evidence of behavioural problems at wave 2, and the odds ratios are similarly sized across samples. High academic test scores are negatively associated with high restlessness, in both samples, but are not significantly associated with aggression or anxiety. In both samples, children living in local authority housing at age seven or five are more likely to have high aggression scores at ages 11 and 10, respectively, and the odds ratios are similarly sized in both samples. Finally, in the BCS sample only, the social class of the father at wave 1 is associated with subsequent aggression and anxiety and the odds ratios differ significantly from those estimated using the NCDS sample. Despite the fact that many factors appear to be significantly associated with behavioural problems, and often with large odds ratios, they do not appear to eliminate the significant bivariate effects of a parental divorce.

Table 4 presents the odds ratios pertaining to low academic test scores at age 11 or 10 in the NCDS and BCS samples, respectively. Although model 1 shows a significant association of parental divorce or separation with low test scores, once the pre-disruption characteristics are introduced, these parameters are no longer significant, suggesting that pre-disruption differences in families that divorce and do not divorce account for observed differences in academic achievement in the short term. There are no significant differences between children who experienced a parental disruption and those who did not in their wave 2, academic performance, and similar to the results presented in Table 3, there are no significant cohort differences in the associations in either model. Also similar to the results for behavioural outcomes, there are few significant cohort differences in the parameters for

the control variables. The most noticeable difference is that males performed better than similar females in the older cohort and worse than similar females in the younger cohort. Moreover, the association between early and later academic performance is stronger for the older sample than for the younger sample reflecting perhaps the greater similarities in the age seven and 11 tests than the age five and 10 tests. In both samples, having a father whose job was classified as skilled, semi-skilled or manual in wave 1 is positively associated with having low academic test scores in wave 2, and there is some evidence that the relationship is stronger for the NCDS cohort, at least for the unskilled category.

Longer-term adult outcomes

The results in Table 5 suggest that although the odds ratios fall when pre-disruption characteristics are introduced, for all three adult outcomes we consider, parental divorce or separation (between wave 1 and age 16) is positively and significantly linked, in both samples, to a lack of academic or vocational qualifications, receipt of non-universal benefits, and having a high malaise score. Moreover, there is no evidence that the parameters differ significantly across cohorts.

Despite the differences in educational attainment across cohorts, parental divorce has an odds ratio that is similarly sized for both samples (odds ratio 1.80:1 for the NCDS sample and 1.86:1 for the BCS sample). The introduction of wave 1 controls does reduce the odds ratios, to 1.46:1 and 1.52:1 for the NCDS and BCS samples, respectively, but the parameters remain significant. For receipt of non-universal benefits, the pattern is similar. The inclusion of pre-disruption controls reduces the odds ratios, but they remain significant and very similarly sized. Malaise is the only outcome for which the pattern is slightly different. In the first models, it appears that parental divorce by age 17 is more strongly associated with

adult malaise in the NCDS sample than in the BCS sample. Although the odds ratios of 1.71:1 and 1.56:1 are not significantly different from one another, the difference is large relative to what we find for the other outcomes. The inclusion of the pre-disruption characteristics reduces both odds ratios, and narrows the cohort difference considerably, however. For all three adult outcomes, the odds ratios are remarkably similar across cohorts and although there is some evidence that pre-disruption controls mediate the divorce effects, they do not eliminate the significant associations entirely.

Cohort differences in the parameters for the control variables are rarely significant. Academic test scores appear more strongly correlated with a lack of qualifications in the NCDS sample. Consistent with rapid increases in women's academic qualifications relative to men's, a trend that began in the 1970s, 33 year old males in the NCDS sample are less likely to have no qualifications whereas 30 year old men in the BCS sample are more likely to lack qualifications than their female counterparts (West and Pennell 2003). Although men are less likely to have high malaise in both samples, the gender gap is significantly more narrowed in the BCS sample. Having a father who was employed in a semi-skilled or unskilled occupation is more strongly correlated with no qualifications in the older than in the younger sample, while parental housing tenure is less strongly correlated with receipt of benefits in the NCDS sample.

DISCUSSION

The analysis conducted in this paper was motivated by two central research questions. First, we were interested in whether or not the inclusion of pre-disruption controls altered the associations of divorce and disadvantage appreciably or in different ways across cohorts. Second, we wished to determine whether or not the association of parental dissolution and

poor outcomes had declined in recent years. As divorce became more commonplace, it is also likely that it become less selective of troubled families. If this is the case, we would expect to find a weaker association when we compare younger cohorts to older cohorts. Of course, our research questions rested on the assumption that there was a correlation to be explained.

Our findings confirm that there is an association to be explained when the focus of the analysis is on adult outcomes. For the childhood outcomes, however, the results are more varied. High restlessness and, for the NCDS sample, high aggression are not significantly associated with parental dissolution even prior to adding wave 1 controls. So for child outcomes, there is an association to be explained, but only for some outcomes -- two out of the four outcomes (anxiety and academic test scores) for the NCDS sample and three out of four outcomes (all but restlessness) for the BCS sample.

The inclusion of pre-disruption characteristics confirmed that there were important differences between more and less disadvantaged children, but, by and large, these differences did not account for the effects of a parental divorce. The evidence for this findings is, however, not entirely conclusive. Parental divorce or separation remains significantly correlated with all three adult outcomes and for both samples even after a range of wave 1 antecedents are included in the models. These results are consistent with some previous research on adult outcomes carried out using the NCDS sample (Kiernan 1992; Cherlin et al 1995; Hobcraft 1998). But once again, the results for the childhood outcomes are more ambiguous. In both samples, the addition of wave 1 controls reduces the association of parental divorce with academic test scores and the odds ratios become insignificant. After the addition of age 7 control variables, the significant association of parental divorce with anxiety becomes insignificant for the NCDS sample. So for the NCDS

sample, where there is an association to be explained, pre-disruption controls seem to matter. In contrast, for the BCS sample, wave 2 anxiety and aggression are associated with parental divorce or separation even after the introduction of the age 5 controls. In the short-term then, there is some evidence of a persistent link between parental divorce and poor behaviour, but only for the younger cohort. In the long-term, the odds ratios attenuate when pre-disruption controls are added but remain significant in both samples. The pre-disruption factors are frequently associated with indicators of adult disadvantage, but they do not mediate to any great extent the relationship between divorce and those indicators.

The answer to our second research question – whether the parameters linking divorce to disadvantage show evidence of change over time -- is unambiguously no, at least in the models that we estimate. Despite rapid changes in the frequency and acceptability of parental divorce beginning in the 1970s, it is striking that the parameters linking family disruption to child and adult outcomes are so similar across these two samples. Indeed, there are no significant cohort differences in the effects of divorce or separation for either short or long-term outcomes. If the increasing prevalence of parental divorce made it a less selective or stigmatising experience, this effect is not obvious when we compare our two cohorts. On the other hand, the experience of a parental divorce or separation was still fairly rare even for the younger cohort. Perhaps an analysis that compared samples drawn from cohorts over a wider time span would be more able to identify cohort changes due to declines in selectivity or stigma.

While not central to our main research questions, some noticeable cohort differences in the estimated parameters of some of the control variables raise some intriguing questions. For instance, there are significant gender gaps for all of the outcomes we consider, but in

some cases, there are substantial changes in the size or direction of those gaps across cohorts as well. Men in the BCS sample have odds ratios for restlessness at wave 2 and for malaise in their early 30s that are significantly larger than those that obtain for the NCDS sample. For both academic outcomes (Table 4 and Table 5), the gender gaps not only change size but change direction. Being male reduced the odds of low test scores for the older cohort, but increased the odds of low test scores in the younger one. Similarly, men drawn from the older cohort were less likely than comparable women to have no qualifications by age 33. Less than a decade later, 30 years old males in the BCS sample were more likely than females to have no qualifications. Although this pattern is consistent with an emerging gender gap in educational achievement that began to emerge in the 1970s and has been growing larger ever since, it is not clear that the dramatic differences can be explained entirely by this trend (West and Pennell 2003). Moreover, there is some evidence that, at certain ages, girls have historically out-performed boys on academic tests in the United Kingdom. Gallagher (1997) mentions that grammar schools (selective publicly funded schools) historically capped places for girls because they performed better than boys on the age 11 tests used select students. His work suggests that the male advantage we find at age 11 in our NCDS data may need further scrutiny.

Performance on academic tests, even when administered at relatively young ages, is persistently associated with a range of outcomes in both samples. Associations are particularly strong for subsequent academic performance – academic tests at wave 2 and lack of qualifications in adulthood. Where significant cohort differences are identified, the relationships are stronger for the NCDS sample. Because the children in our samples were administered the tests at different ages, it is unclear whether the differences are genuine

cohort differences or attributable to the fact that the children in the NCDS sample were somewhat older and had already been in school when they were tested or because of the differences in the material covered in the tests. Unfortunately, the use of different tests makes any definitive explanation of the observed cohort differences impossible with these data.

Parental housing tenure at age seven or age five is associated with four of the seven outcomes for the NCDS sample and with five of the seven outcomes for the BCS sample. Where cohort differences are significant, they indicate a stronger association of local authority housing with indicators of disadvantage for those born in 1970. The increased strength of the correlation of parental housing tenure with adult outcomes may be due to increasing levels of residualisation in council housing where families that could afford to buy left behind those who were more disadvantaged, a trend that began in the 1960s and accelerated in the 1980s (after the second childhood wave of the BCS70) with right-to-buy (Lee and Murray 1997; Burrows 1997). Although the right-to-buy would have had an impact on the later childhood years only, by the 1970s, there was already clear evidence that lower income households were increasingly over-represented within council housing as higher income groups moved towards home-ownership (Murie, Niner, and Watson 1976). This means that the average child living in local authority housing would be more disadvantaged. In addition, housing tenure might reflect increasing neighbourhood disadvantage as well. The same sort of residualisation argument might also explain the significantly weaker link between father's occupational class and disadvantage in academic indicators for the BCS sample. As the employment conditions and stability of unskilled jobs

deteriorated over time, the group of children in this category may have become relatively more disadvantaged as well.

While our results are suggestive of both stability and change in the relationships between childhood background factors and subsequent disadvantage, there are several questions we have not addressed in this analysis that may merit more detailed consideration. We wish to mention the limitations of the current results and outline briefly the directions in which our analysis could be fruitfully extended. The issues we would like to consider further fall into three broad categories: sample selection, issues of timing, and intermediate pathways.

Restricting our samples allows us to control for a range of characteristics, all of which were measured before any disruption occurred, the restricted samples may be biased as a result of our study design. In a paper that applied the same restriction to the NCDS data, Cherlin et al (1995) found that when they considered age 23 demographic outcomes, there was evidence of positive sample selection bias, but the bias did not alter the parameter estimates appreciably. Moreover, it is difficult to identify and defend instruments that are associated with being included in the sample (an intact family at age seven or five) and that are not associated with the outcomes. Given that different sets of instruments can yield different and sometimes contradictory results, and that some research suggests that corrections for sample selection can, in cases, do more harm than good (Stolzenberg and Relles 1990), we have opted, for the more straightforward approach. The fact that we have left this issue unaddressed means that the results must be interpreted with some caution, however.

There are three issues of timing that we believe require more consideration. First, we include in our restricted sample, all children who were living with both natural parents at age seven in the NCDS sample or age five in the BCS sample, even if they experienced a family disruption a short time later. If our aim is to identify a set of baseline controls, this restriction may be too lenient. Because divorce and separation are not discrete events but processes, some of the children in our samples may have already been negatively affected by the onset of a divorce despite the fact that they were still living with both their parents at the time of interview. If this were the case, the baseline child characteristics may be "over-correlated" with the outcomes, picking up some of the association that should be attributed to disruption. Moreover it is probable that there would be a closer link between similar measures in the two childhood waves than for the adult outcomes, meaning that the degree of over-control would be greater for the child outcomes. We have run some additional models that drop children with evidence of divorce within two years of the wave 1 interview and the results do not change substantively when we impose this additional restriction, but more examination of this issue is warranted.

Another issue of timing relates to the child's age at the time of disruption. Although for our childhood outcomes, disruption occurred over a relatively narrow age range, for our adult outcomes, inattention to the timing of the disruption may be more problematic. If disruption has more deleterious effects at earlier ages or when it occurs at times of transition (in school, for example), this restriction may be problematic. As a preliminary step, we have run additional models for the adult outcomes which allow the disruption parameters to differ when the disruption occurred before or after the second follow-up interview. The results suggest that the parameters do not differ significantly, but it is not clear that the age groups

we use are the most theoretically defensible, and, in future research, we hope to explore this timing issue in greater detail.

A third timing issue is related to the other two and pertains to our construction of the divorce variables. To maximise the number of valid cases in our samples, we chose to identify as disrupted only those children who experienced a disruption through age 16. Research by Kiernan and Furstenberg (2001) suggests that later disruptions may be important as well. Moreover, because children in the British system are making important educational transitions at age 16 and because, as we have already mentioned, divorce is a process rather than a discrete event, it is possible that children who were still living with both natural parents at age16 may nonetheless have been affected by an disruption that would occur in the near future. For this reason, our cut-off point is not fortuitous and further research is needed to assess the sensitivity of our results to different specifications of these variables.

One further puzzle is why parental divorce appears to have weaker effect in the short term than in the long term. Our prior expectations would have been that the behavioural measures would show significant indications of an impact of parental divorce a short time afterwards. Yet the effects are not that large, even before the introduction of the pre-disruption controls (whatever their limitations, as indicated above). It is possible that parents (or mostly mothers) are adjusting their reports of the child's behaviour to allow for perceived reactions to the divorce. One possibility would be to use teacher reports rather than parent reports. Unfortunately, since the BCS children were only aged five at the first wave we cannot introduce teachers' ratings of behaviour pre-disruption for both samples. Moreover, for the BCS sample, the behavioral questions asked of the teachers are different from those asked of the parents making comparability across respondents difficult to achieve.

Finally, although our results show persistent associations of divorce with subsequent disadvantage, particularly in adulthood, our research does not shed light on the processes that lead to that disadvantage. We had thought that short term disadvantage might set off a chain of events that lead to longer term disadvantage, but because the associations with short term outcomes are less consistent, our results do not unambiguously support this hypothesis. Moreover, we need to include more intermediate measures in order to understand the continuity (or discontinuities) of pathways towards disadvantage. Research carried out using the NCDS sample suggests that disruption is associated with adolescent and early adult transitions. Whether these interrupt academic and career progression and lead to subsequent disadvantage is an intriguing hypothesis that we hope to explore in greater detail.

CONCLUSION

This study sought to examine the relationship between parental divorce childhood and adult disadvantage by first restricting the sample to children who had not experienced a disruption and second by introducing a set of explanatory variables, all of which were measured prior to any disruption. To compare the two cohorts and the effects of disruption over time, we have constructed a set of outcome and explanatory variables that are as similar as possible. For each of the cohorts, we estimated the effects of parental divorce on several measures of disadvantage in late childhood and adulthood. These include early behavioural and academic problems, early school leaving, receipt of means-tested benefits, and poor mental health. In addition to measures of family structure, we include a wide range of family and childhood antecedents as controls.

Contrary to what might be expected, we found little evidence for the hypothesis that divorce has become less selective over time. Parameter estimates across cohorts are

surprisingly similar and not significantly different in any of the models we estimate. Although there are many other antecedents we would like to have included in our models, it is noteworthy that the significant parameter estimates are often little changed once pre-disruption characteristics are introduced. We also find that there is no evidence of a correlation between parental divorce and short-term pre-adolescent outcomes in the NCDS models, but significant associations remain for two behavioural outcomes in the BCS sample, even after the inclusion of pre-disruption control variables. Although our results are largely contrary to our expectations, and raise many issues we would like to pursue further, our most conclusive finding raises the most intriguing question. Why is it that the associations of family disruption and subsequent indicators of disadvantage are so remarkably stable over the time period we consider?

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TABLE 1. MEANS OF THE OUTCOME VARIABLES: SAMPLES OF CHILDREN DRAWN FROM THE NCDS AND BCS DATA

	NCDS		BCS	
	Valid Cases	Frequency	Valid Cases	Frequency
Age 11/10				
High Aggression	11178	0.12	9864	0.12
High Anxiety	11023	0.26	9858	0.20
High Restlessness	11159	0.13	9873	0.11
Low Test Scores	10499	0.23	8419	0.22
Age 33/30				
No Qualifications	7326	0.11	8122	0.07
Benefit Receipt	7392	0.16	7670	0.14
Malaise	7457	0.09	7596	0.16

TABLE 2: MEANS OF THE CONTROL VARIABLES: SAMPLES OF CHILDREN DRAWN FROM The NCDS AND BCS DATA

	NCDS		BCS	
	Valid Cases	Frequency	Valid Cases	Frequency
Marital History between waves 1 and 2	Ages 7 and 11, 1965-1969		Ages 5 and 10, 1975-1980	
Not divorced, separated or disrupted	10970	0.98	9519	0.94
Divorced or separated	255	0.02	643	0.06
Sex				
Female	5492	0.49	4880	0.48
Male	5733	0.51	5282	0.52
Behavioral Scores at wave 1				
Aggresion score <2	4424	0.39	4320	0.43
Aggression 2/3 out of 8	5075	0.45	4036	0.40
Aggression 4 or higher	1704	0.15	1659	0.16
Anxiety score <2	4745	0.42	5347	0.53
Anxiety 2/3 out of 8	5191	0.46	3537	0.35
Anxiety 4 or higher	1256	0.11	1093	0.11
Restless score =0	4964	0.44	4360	0.43
Restlessness 1/2 out of 6	4924	0.44	4548	0.45
Restlessness 3 or higher	1321	0.12	1059	0.10
Missing Behavioral Scores	64	0.57	361	0.04
Academic Test Scores at wave 1				
Bottom Quartile	2484	0.22	2169	0.21
Second Quartile	2689	0.24	2364	0.23
Third Quartile	2907	0.26	2446	0.24
Top Quartile	2782	0.25	2500	0.25
Missing	363	0.03	683	0.07
Housing Tenure at wave 1				
Local Authority	4481	0.40	3027	0.30
Other	6744	0.60	7135	0.70
Social Class of Father at wave 1				
Non-manual	3619	0.32	3632	0.36
Skilled Manual	5096	0.45	4757	0.47
Semi- or Unskilled Manual	2510	0.22	1773	0.17
Reading with Child at Wave 1				
Neither parent reads	1245	0.11	1936	0.19
At least one parent reads	9841	0.88	7986	0.79
Missing	139	0.01	240	0.02

Marital History between waves 1 and age 16				
Not divorced, separated or disrupted	8776	0.92	7724	0.84
Divorced or separated	727	0.08	1518	0.16

TABLE 3: ODDS RATIOS FOR BEHAVIORAL OUTCOMES AT AGE 11 FOR THE NCDS SAMPLE AND AGE 10 FOR THE BCS SAMPLE

	High Aggression at Wave 2				High Anxiety at Wave 2				High Restlessness at Wave 2			
	NCDS		BCS[a]		NCDS		BCS[a]		NCDS		BCS[a]	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Family Change Waves 1-2												
divorce	1.31	1.08	1.55 ***	1.28 *	1.35 *	1.24	1.35 **	1.30 *	1.37 +	1.21	1.27 +	1.10
Wave 1 Controls												
male		1.44 ***		1.28 ***		0.89 *		0.87 *		1.33 ***		**1.75** ***
mod. aggression		3.05 ***		2.66 ***		0.98		**1.15** *		1.06		**1.69** ***
high aggression		11.79 ***		**7.99** ***		0.96		**1.27** **		1.92 ***		2.13 ***
mod. anxiety		0.94		1.05		1.96 ***		*2.25* ***		1.02		0.90
high anxiety		1.16		0.97		4.56 ***		**6.14** ***		1.19 +		0.97
mod. restlessness		1.25 **		1.19 *		1.17 **		1.08		2.83 ***		2.74 ***
high restlessness		1.40 **		**1.88** ***		1.59 ***		**1.22** *		8.70 ***		9.47 ***
1+ missing score		3.79 ***		*2.04* ***		1.42		1.38 *		2.71 **		1.54 *
bottom test quartile		1.38 ***		1.36 ***		1.20 **		1.15 *		1.40 ***		1.31 ***
top test quartile		0.87 +		0.82 +		0.92		0.97		0.78 **		0.76 **
missing test scores		1.05		*1.54* **		1.28 *		1.06		1.07		1.14
local authority housing		1.25 **		1.29 **		1.00		0.92		0.98		0.98
skilled manual father		1.11		**1.56** ***		1.04		**1.26** ***		1.16 *		1.00
semi- or unskilled father		1.15		**1.79** ***		0.90		**1.37** ***		1.13		1.13
reads with child		0.86		0.71 ***		1.24 **		*1.02*		0.86		0.92
missing		0.68		0.71		1.23		1.22		1.05		0.78
Log-Likelihood	-4103.68	-3495.82	-3524.63	-2986.06	-6258.53	-5927.70	-4884.40	-4514.84	-4228.31	-3688.08	-3343.41	-2895.75
N	11178		9864		11023		9858		11159		9873	

Notes: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a]Parameters in italics are significantly different from the NCDS cohort at a 10 percent level, those in boldface are significantly different at a 5 percent level

TABLE 4. ODDS RATIOS FOR ACADEMIC OUTCOMES AT AGE 11 FOR THE NCDS SAMPLE AND AGE 10 FOR THE BCS SAMPLE

	Low Test Scores at Wave 2			
	NCDS		BCS[a]	
	Model 1	Model 2	Model 1	Model 2
Family Change Waves 1-2				
divorce	1.53 **	1.16	1.42 ***	1.16
Wave 1 Controls				
male		0.80 ***		**1.30** ***
mod. aggression		1.16 *		1.04
high aggression		1.43 ***		1.31 **
mod. anxiety		0.93		0.98
high anxiety		0.98		0.96
mod. restlessness		1.01		1.02
high restlessness		1.45 ***		1.27 *
1+ missing score		2.63 **		1.72 ***
bottom test quartile		9.02 ***		**3.22** ***
top test quartile		0.17 ***		**0.26** ***
missing test scores		2.12 ***		1.28 *
local authority housing		1.48 ***		1.57 ***
skilled manual father		2.15 ***		2.01 ***
semi- or unskilled father		2.91 ***		**2.21** ***

reads with child		0.77 **		0.71 ***
missing		1.04		1.24
Log-Likelihood	5634.80	-4028.61	-4475.18	-3757.11
N		10499		8419

Notes: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a]Parameters in italics are significantly different from the NCDS cohort at a 10-percent significance level, those in boldface are significantly different at a 5 percent level.

TABLE 5: ODDS RATIOS FOR OUTCOMES AT AGE 33 FOR THE NCDS SAMPLE AND AT AGE 30 FOR THE BCS SAMPLE

	No Qualifications				Receipt of Non-Universal Benefits				High Malaise Inventory Score			
	NCDS		BCS[a]		NCDS		BCS[a]		NCDS		BCS[a]	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Family Change Waves 1- Age 16												
divorce	1.80 ***	1.46 **	1.86 ***	1.52 ***	1.67 ***	1.50 ***	1.72 ***	1.49 ***	1.71 ***	1.51 ***	1.56 ***	1.44 ***
wave 1 controls												
male		0.67 ***		**1.23** *		0.62 ***		0.55 ***		0.44 ***		**0.63** ***
mod. aggression		1.27 *		1.13		1.04		1.16 +		1.26 *		1.19 *
high aggression		1.61 ***		1.30 +		1.27 *		1.38 **		1.46 **		1.41 ***
mod. anxiety		0.84 +		0.97		0.94		1.04		1.02		1.07
high anxiety		0.84		0.75 +		1.05		0.94		0.90		*1.28* *
mod. restlessness		0.99		1.15		0.85 *		**1.26** **		0.94		1.11
high restlessness		1.31*		1.15		1.15		1.30 *		1.37 *		1.38 **
1+ missing score		1.83		1.08		1.33		1.43		0.44		1.51 *
bottom test quartile		3.83 ***		**2.66** ***		1.71 ***		1.65 ***		1.79 ***		**1.19** *
top test quartile		0.36 ***		*0.54* ***		0.78 **		*0.62* ***		0.78 *		0.84 *
missing test scores		1.72*		*1.04*		1.14		1.12		1.10		1.19
local authority housing		1.65 ***		*2.02* ***		1.31 ***		**1.79** ***		1.15		1.34 ***
skilled manual father		2.27 ***		1.69 ***		1.48 ***		1.62 ***		1.27 *		1.11
semi- or unskilled father		4.01 ***		**2.28** ***		1.98 ***		1.86 ***		1.38 **		1.21 +
reads with child		0.55 ***		0.68 ***		0.94		0.78 **		0.89		0.93
missing		0.80		1.10		0.75		1.20		0.73		0.86
Log-Likelihood	-2532.03	-2100.67	-2055.03	-1829.37	-3221.10	-3079.18	-3104.03	-2856.57	-2220.15	-2122.95	-3330.79	-3244.79
N	7326		8122		7392		7670		7457		7596	

Notes: + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a]Parameters in italics are significantly different from the NCDS cohort at a 10 percent level, those in boldface are significantly different at a 5 percent level.