Childhood Family Structure and Schooling Outcomes: Evidence for Germany

Marco Francesconi*
Department of Economics,
University of Essex

Stephen P. Jenkins
Institute for Social and
Economic Research,
University of Essex

Thomas Siedler
Institute for Social and
Economic Research,
University of Essex
and DIW Berlin

Abstract

We analyse the impact of growing up in a family headed by a lone mother on schooling outcomes using data from the German Socio-Economic Panel. According to estimates from models that do not control for the possibility that family structure and educational performance share common unobserved determinants, growing up in a non-intact family is associated with worse outcomes. But once endogeneity is accounted for – whether by using estimators comparing siblings, or comparing children who experienced parental loss through death rather than divorce, or comparing children by whether their parents were exposed to a reform that made divorce easier – the evidence that family structure affects children's schooling outcomes is much less conclusive. Point estimates indicate non-intactness has adverse effects, but confidence intervals are large and span zero. One exception concerns the probability of *Gymnasium* attendance at age 14 which is some 40 percent lower for West Germans who experienced life in a non-intact family during childhood. This result however is sensitive to the method used to account for family structure endogeneity, and is not found by instrumental-variables estimators.

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* Corresponding author: Marco Francesconi, Department of Economics, University of Essex, Wivenhoe Park, Colchester CO4 3SQ, UK (mfranc@essex.ac.uk). Financial support from the Anglo-German Foundation and the UK Economic and Social Research Council is gratefully acknowledged.

I. Introduction

No parent wishes to see their child do badly at school, to end up unemployed, stuck in a dead-end job, or poor, and there is widespread support for social policies directed at improving children's attainments and avoiding disadvantageous outcomes. Given the substantial policy interest and the growing availability of intergenerational data sources, it is unsurprising that there is a burgeoning literature about the extent to which growing up in a lone parent family has deleterious consequences for later-life attainment. Most existing evidence concerns the USA, however. In this paper, we provide new evidence about the impact of childhood family structure on schooling outcomes in Germany.

We offer two contributions. The first is methodological. Like many previous studies we recognise that correlations between childhood family structure and child outcomes may reflect the impact of some unobserved factor. Unlike many previous studies, however, we seek a robust picture of the true causal impact of childhood family structure by combining different methods. We estimate models that utilise different identifying strategies: sibling-difference models, instrumental variables models, and models based on comparisons between individuals whose fathers died, divorced, or remained married. The principal schooling outcome analysed is whether an individual has educational qualifications to university entrance level or higher, but we consider several other measures of schooling outcomes as well. We use various definitions of childhood family structure, and make several further sensitivity tests.

Our second contribution is substantive in the sense that it is one of the first systematic studies of the effect of childhood family structure on schooling outcomes for young adults in Germany. A distinctive feature of our research is that we compare results for three samples: individuals who grew up in a family from the former West Germany headed by a native German; individuals who grew up in a family from the former West Germany headed by a guestworker; and individuals who grew up in a family from the former East Germany headed by a citizen of the former German Democratic Republic. The samples provide an opportunity to explore the extent to which the effects of family structure may differ within different social and cultural environments.

Family structure patterns differed between the former East Germany and West Germany: the extra-marital birth rate, the divorce rate and the proportion of lone parent families among all families were all higher in the former (ZUMA 2004). So too were the labour force participation rates of mothers and state support for families (Szydlik 2000). The guestworker sample adds a further contrast. Sample members grew up in the former West

Germany, but their family was headed by someone from Turkey, Greece, the former Yugoslavia, Spain or Italy. In other words, there was substantial ethnic and religious diversity compared to the native German sample members, likely to be reflected in different styles of child-raising and attitudes to the 'family', and thence one might expect the impact of growing up in a non-intact family and schooling outcomes also to differ.

We find that the conclusions to be drawn about the impact of growing up in a non-intact family depend crucially on whether unobservable family background characteristics are controlled for, whereas variations across estimation sample or definitions of outcome or childhood family structure matter rather less. Estimates that ignore the endogeneity associated with family structure suggest that experience of life in a non-intact family during childhood has a relatively large and statistically significant adverse impact on schooling outcomes. In contrast, the various models that accounted for endogeneity also produced point estimates indicating an adverse impact, but the standard error associated with each estimate was also large. Put another way, the confidence intervals for estimated effects were large, so the data were consistent with the impact of family structure being zero as well as adverse.

These conclusions held broadly true for all three samples (West German, East German, or Guestworker), and for each schooling outcome with one exception. According to the sibling-difference estimates, being in the top secondary school track at age 14 is significantly less likely for West Germans who experienced life in a non-intact family during childhood. This result however is sensitive to the method used to account for family structure endogeneity, and is not detected by instrumental-variables estimators that exploit father's death or changes in the divorce law as sources of exogenous variation in family structure.

The rest of the paper is organized as follows. Section II reviews relevant previous literature and outlines our empirical strategy. Section III presents the data, and the definitions of schooling outcomes, family structure, and other control variables. Section IV discusses our main findings and Section V contains a summary and concluding remarks.

II. Identifying the Effect on Attainment of Growing Up in a Lone Parent Family

A. Related Literature

An extensive body of empirical research, mostly based on US data, has identified childhood family structure as a key determinant of children's later achievements (McLanahan and Sandefur 1994; Haveman and Wolfe 1995). Most studies have found that growing up without a biological parent is negatively associated with schooling attainments and also with a number of other indicators of later economic success (such as employment, earnings, income, and wealth). There is disagreement, however, about whether the impact of family structure is causal (Manski et al. 1992). Arguably lone parenthood may be correlated with other socioeconomic disadvantages, and so inferior outcomes may arise from (potentially unobserved) factors other than a parent's absence. Researchers have employed several methods to account for the influence of these other factors.

Sibling difference (fixed effects) models take account of the fixed unobservable endowments that are shared by siblings and half-siblings from the same family (or, more usually, mother). Ginther and Pollak (2004) showed that educational outcomes for children in blended families (i.e. stepchildren and their half-siblings) are similar, and worse than the outcomes for those who grew up in families in which all children are the biological children of both parents. Using fixed-effects estimators also, but with different definitions of family structure and different datasets, Case et al. (2001) and Ermisch and Francesconi (2001) also found that growing up in a non-intact family has a negative and significant effect on schooling achievements. Gennetian (2005) reports that living in a lone-parent family has a negative effect on children's test scores, but also found that living with a step-parent or with half-siblings had no effect on children's cognitive achievements.

A number of studies have compared children's attainments before and after the divorce of their parents. The hypothesis is that the poorer schooling attainment of children from non-intact families does not reflect the lack of investment of both biological parents, rather it reflects pre-existing disadvantages of the family (e.g., higher parental conflict) or youth (e.g., lower ability). Cherlin et al. (1991) report that elementary school children whose parents split up performed poorly in school even before parental separation. Similarly, Piketty (2003) showed that the poorer school performance of French children who later experienced parental separation was comparable with that of children who already lived with only one of their biological parents. In contrast, Painter and Levine (2000) found that most of the detrimental effects on school outcomes of parental absence during adolescence were not due to pre-existing disadvantages and so conclude that the correlation between family structure and child outcomes is causal.

Quasi-experimental studies have used parental death as an exogenous source of parental absence. Lang and Zagorski (2001) and Corak (2001) found little evidence that growing up in a non-intact family had adverse effects on children's educational outcomes and economic wellbeing in adolescence or early adulthood. Biblarz and Gottainer (2000) reported that children of divorced lone mothers have lower educational achievements than do children whose fathers died.

Another example of a quasi-experimental study is based on comparisons of educational outcomes for children who were exposed to different divorce laws during childhood. Gruber (2004) exploited variation across US states and over time in changes in divorce regulation. In particular, he concluded that exposure during childhood to unilateral divorce (i.e. divorce that does not require the explicit consent of both partners) led to lower educational outcomes. Piketty (2003) exploited the increase in separation rates following a divorce law change in France. He found that the difference in school performance between children of lone-parent households and children of two-parent households declined as the separation rate increased, and attributed the lower attainment of lone-parent children to pre-divorce factors such as parental conflict.

The lack of consensus on the effect of childhood family structure can only be partly due to the fact that some studies refer to countries other than the United States (where school and family institutions are likely to differ),[1] or refer to different data sets for the same country. In fact, different conclusions have also been drawn from studies using the same data set for the same country. This has arisen because of variations in econometric approach, sample inclusion criteria, definition of family structure, choice of conditioning variables, or the time period considered. A distinctive feature of our research is that we address this robustness issue directly, by using different methods (employing different identifying assumptions), studying several schooling outcomes and using several different definitions of family structure (e.g. not only the 'occurrence' of non-intactness but also its duration).

For Germany, the evidence available about the association between family structure and child's education is limited. Mahler and Winkelmann (2004) found that growing up in a lone-mother family slightly reduced the probability of being in the *Gymnasium* secondary school track, but also showed that most of this adverse effect was due to lone mothers' lower incomes. Jenkins and Schluter (2002) stated that measures of family breakdown (and re-partnering) had no association with school track, while Bohrhardt (2000) reported that there was no impact of experience of parental marital dissolution on the probability of getting a school-leaving certificate.

One problem with this research for Germany is that it uses "cross-section" (or "level") estimators which assume that every family background variable, including family structure, is uncorrelated with family- and child-specific unobservables. But a weak correlation between

[1] Exceptions include Jonsson and Gähler (1997) and Björklund et al. (2004) for Sweden, Cherlin et al. (1995), Hobcraft and Kiernan (2001) and Ermisch and Francesconi (2001) for Britain, Corak (2001) for Canada, and Piketty (2003) for France.

family structure and child's education obtained from levels estimators cannot necessarily be taken as evidence that family structure is determined independently of child and/or family unobservables. By supplementing level estimators with models that make weaker identifying assumptions, and exploring robustness in additional directions, we aim to understand better whether there is a causal effect running from childhood family structure to children's schooling outcomes in Germany.

B. Econometric Modelling

Our econometric strategy is to apply a number of different methods with different assumptions in order to identify the effects of experience of life in a non-intact family during childhood on later life outcomes. Given data for a sample of individuals about schooling outcomes and parental marital histories, the effects of childhood family structure can be modeled in the following way (Painter and Levine 2000; Lang and Zagorski 2001; Ruhm 2004; Page and Stevens 2004):

$$S_{ij} = F_{ij}\beta + X_{ij}\gamma + \alpha_j + u_{ij}, \quad (1)$$

where S_{ij} represents a schooling outcome for individual i from family j, F_{ij} is a vector of childhood family structure variables, and X_{ij} is a vector of child- and family-specific variables that may be fixed (e.g. mother's education) or vary over time (e.g. the individual's age) and that may be correlated with the schooling performance. (The empirical definitions of the variables included in F_{ij} and X_{ij} are explained in the next section.) The error term has two components, a family-specific fixed effect, α_j, and a random idiosyncratic component, u_{ij}.

We estimate (1) using four methods. First, we compute "worst-case" Manski bounds for treatment effect β, using a subset of variables in X_{ij} (such as age, sex, and mother's education) to create subgroups of respondents, (Manski 1990, 1995). The bounds identify an interval (generally of length less than one) which always contains zero, and so the sign of β cannot be pinned down. This method shows what can be identified from the data without additional information or assumptions – which are employed by the other methods.

Second, we estimate level regressions based on the assumption that observed determinants of attainment (F_{ij} and X_{ij}) are uncorrelated with unobservable determinants α_j and u_{ij}. This assumption is implausible because it is likely that an individual's schooling performance is affected by mother-specific unobserved influences α_j (e.g., ability and motivation) which are inherited in the form of genetic and cultural endowments.[2] Despite this, many of the findings reported in the literature have been obtained from level regressions, and so level estimates provide an important reference point.

Third, we estimate mother fixed-effect (sibling difference) models, which take account of the fact that siblings and half-siblings share many family-specific characteristics that are relevant to the attainment process. Estimation of these models leads to consistent estimates of β if parents respond equally to each of their children's idiosyncratic endowments (Rosenzweig and Wolpin 1995; Ermisch et al. 2004) – arguably a weaker assumption than the 'selection on observables' imposed by the level regression models.[3] Mother fixed-effects models can only be estimated on families for which we observe at least two children and the intergenerational transmission process may differ between single- and multi-child families. We address this issue in our sensitivity analysis (Section IV.F).

Fourth, we estimate variants of equation (1) using instrumental variables methods. Specifically, two quasi-experiments, each relying on different sources of "exogenous" variation in family structure, are used. The first involves individuals whose father died when they were a child (Lang and Zagorski 2001; Corak 2001). If an individual's idiosyncratic endowments do not depend on whether his/her parents split up, or whether his/her father died (i.e., paternal loss via death is exogenous), then schooling outcomes of individuals whose father died during their childhood provide a benchmark against which to assess the endogeneity of parental loss through parental separation or divorce. We implement this method by using as regressors in (1) family structure variables that distinguish individuals who ever lived with a separated or divorced mother from individuals who experienced the death of their father during childhood and individuals whose mother was unmarried when they were born. As with estimation of mother fixed-effects models, the sample sizes are small, since paternal death during childhood is a relatively rare event.

The second quasi-experiment involves individuals whose childhood spanned the mid-1970s, the period when changes to West German divorce law eliminated "fault" grounds for

[2] For formal models of intergenerational transmission in which this is the case, see Behrman et al. (1994), Rosenzweig and Wolpin (1995), and Ermisch et al. (2004).

[3] Equal parental responses imply that there are no differences over time in parental attitudes and behaviour that affect both family structure and child outcomes. There are situations where this is not true. For example, the arrival of a second child may put parents under greater strain giving rise to a situation in which an elder sibling spends only a small part of his childhood with distressed parents while the youngest has distressed parents for most of her childhood. Parental conflict may directly affect investment in the youngest child, and the parents may also divorce because of it, thereby causing correlation between idiosyncratic endowments and family structure. In this case, sibling-difference (and level) estimates are biased. See Ermisch et al. (2004) for further discussion.

divorce. Our model exploits the variation between the former East Germany and West Germany and variation over time in the ease of getting divorced associated with changes in divorce law (cf. Corak 2001; Piketty 2003; and Gruber 2004).[4] School outcomes can be modelled using a before-after design, with the treatment effect given by the coefficient on the interaction between an indicator for having experienced parental divorce in West Germany and time. The variants of equation (1) that we estimate take the form:

$$S_{ij} = \delta_0 + \delta_1 d_{1i} + \delta_2 d_{2i} + (\delta_3 + \delta_4 W_{it})\lambda_t + \beta_1(d_{1it} \times \tau_t) + \beta_2(d_{2it} \times \tau_t) + Z_{it}\gamma + \varepsilon_{it}, \qquad (2)$$

where i indexes children and t indexes survey years. The term d_{1i} is a dummy variable equal to one for West German individuals who lived with a divorced mother during childhood and zero otherwise; d_{2i} is the corresponding variable for East Germany; W_{it} is equal to one if i lived in West Germany at time t, and zero otherwise; λ_t is a full set of year dummies; τ_t is a dummy variable equal to one if parental divorce occurred during the post-reform period and zero otherwise; vector Z contains child/family characteristics (potentially different from those included earlier in X in equation (2)); and ε_{it} is an i.i.d. disturbance term.[5] The parameter of prime interest is β_1 which measures the effect on S of parental divorce under the post-1976 unilateral divorce regime for individuals from the West German sample (i.e. the difference-in-differences between individuals with divorced and married parents).[6] The key assumption here for the identification of β_1 is that child endowments (subsumed in ε_{it}) do not depend on the specific divorce law in force.

III. Data

A. The German Socio-Economic Panel and Our Three Samples

Our data come from the German Socio-Economic Panel Study (SOEP), combining information from the first nineteen annual interview waves (1984–2002) and the retrospective

[4] After World War II, the two German states followed different approaches to family law. In 1955, the German Democratic Republic (GDR) introduced the Family Law Code which regulated divorce on the no-fault principle of irretrievable breakdown of marriage. As a consequence, divorce with the consent of just one rather than both partners became legal, and this law remained unchanged and effective until reunification (Wagner 1997). In contrast, the Federal Republic of Germany (FRG) introduced a law in 1953 that eased consensual (and fault) divorce. This was replaced by the First Marriage Law and Family Law Reform Act in June 1976, implemented one year later. This introduced the concept of irretrievable breakdown of marriage and unilateral divorce became possible. Since October 1990, a uniform family law based on the FRG's 1976 Reform Act has applied to the whole of Germany (Martiny and Schwab 2002).

[5] Further details of the empirical specification of equation (2) are provided in Section IV.E.

[6] A negative value of β_1 would imply an adverse impact on schooling outcomes of making divorce easier. On the other hand, β_1 may be positive: by lowering divorce costs, unilateral divorce lead to lower levels of parental conflict which, in turn, have beneficial effects on schooling outcomes (Piketty 2003). A positive β_1 might also reflect the possibility that unilateral divorce strengthens the bargaining power of mothers who, in turn, devote more resources to their children than they would in a fault-divorce regime (Gruber 2004).

lifetime employment, marital and fertility histories (which span the pre-panel period for most respondents). Each year since 1984, the SOEP has interviewed a sample of nearly 17,000 individuals in approximately 6,000 native German and guestworker households from the former Federal Republic of Germany. In June 1990, the SOEP was expanded to the territory of the former German Democratic Republic, including nearly 2,200 new households. On-going representativeness of the population has been maintained by using a following rule typical of household panel surveys.[7]

Our analysis is based on three different samples. The first consists of individuals who belonged to households that were part of the original SOEP West German sample, i.e. sample 'A', and with a German head of household ('West German sample'). The second sample includes individuals who belonged to households that were part of the original SOEP West German Guestworker sample, i.e. SOEP sample 'B' ('Guestworker sample'). Guestworker households are private households headed by someone who came to Germany under the guestworker programmes of the 1960s and 1970s (Gang and Zimmermann 2000). The third sample comprises individuals belonging to households located in the former German Democratic Republic (GDR) before 1990 and whose head was a GDR citizen ('East German sample'). Panel data is available for the West German and Guestworker samples from 1984 and for the East German sample from 1990 onwards.[8]

B. Sample Selection Criteria

Our analysis dataset consists of individuals who: (a) were aged 18 or less in the first year first observed as SOEP members; (b) were living with their mother for at least one year between 1984 and 2002; (c) were not disabled;[9] and (d) had mothers who provided complete family and employment histories over the individual's entire childhood, i.e., from birth to the child's sixteenth birthday.[10]

Condition (a) was imposed to avoid overrepresentation in the sample of individuals who had left their parents' home at late ages. Although, in principle, the condition may lead

[7] The SOEP is documented at http://www.diw.de/english/sop/service/index.html.

[8] Sample membership refers to the location when the household was originally sampled, and not current location because of subsequent mobility between the former East Germany and West Germany. Foreign children, other than those from Guestworker families, were excluded from the analysis due to small sample sizes: nine children from the West German Sample and one from the East German Sample were dropped.

[9] Disability status is measured prospectively during the survey period. Ideally we would like to measure children's disability status during their childhood, but the SOEP does not contain this retrospective information prior to 1984.

[10] Father-only families were excluded from the sample: only 75 children (or 2 percent of individuals in our final sample) were dropped.

to sample selection bias if educational outcomes and co-residence with one's mother share unobserved factors, we believe the problem is not serious. By age 18, only seven percent of German children have left their parental home (Iacovou 2002). Condition (b) enables us to match children to mothers who are SOEP respondents themselves. This allows us to derive information about the mother (and the family) directly from the mother, such as her age, education, and income sources. Selection condition (c) reduces the problem that parents might choose family structure patterns (and other behaviour, such as employment) on the basis of considerations of their child's health. Maternal fixed-effects models identify the parameter of interest by assuming that there are no intra-family responses, and this would be hard to justify if disabled children had been included in the sample. Condition (d) means that we have full information on our key variable of interest (childhood family structure) and on maternal employment, a family background variable that has been seen as an important determinant of children's attainments (Ruhm 2004).

The sample selection criteria resulted in a sample size of approximately 1,400 for the West German sample, 700 for the Guestworker sample, and 600 for the East German sample.

C. The German School System and Our Measures of Schooling Outcomes

Before introducing our measures of schooling outcomes, we need to explain the structure of the German school system (see Dustmann 2004 for further details). Schooling begins with voluntary pre-school kindergarten. Compulsory school attendance starts at age six, and ends at age 18. Primary school covers the first four years, after which children continue their education in secondary schools. Around the age of 10, pupils are channelled into three main types of secondary schools: secondary general school (*Hauptschule*), intermediate school (*Realschule*), and high school (*Gymnasium*). *Hauptschule* offers the lowest level of secondary education, and ends after five or six years at the age of 15–16, potentially with a formal leaving certificate. *Hauptschule* graduates typically proceed to a vocational training track which combines a three- or four-year apprenticeship with attendance at a technical training college. *Realschule* leads to a formal degree after six years (when students are aged 16), and is generally followed by attendance at a further education college combined with an apprenticeship or, rarely, a move to a *Gymnasium*. *Gymnasium* is the most academic track. Schooling ends at age 18–19 after 13 years of formal schooling and leads to the *Abitur* certificate, the highest secondary-school qualification, and entitles holders to enter universities and other institutions of higher education.[11] Since education is a responsibility of the states, and not of the federal government, details of this description vary from state to state. The differences are mainly related to the age of entering or leaving a specific school track, and are not large. State dummies are included in most regressions, in any case.

We use four measures of schooling outcomes. Our primary measure is a dichotomous variable equal to one if the individual's educational attainment is *Abitur* or higher and zero otherwise. Attainment is measured in the final year in which an individual aged 19 or more was observed in SOEP. Almost 35 percent of the West German sample and 32 percent of the East German sample have qualifications to *Abitur* or higher, but only 20 percent of the Guestworker sample (see Table 1). However, within each sample, there is a clear gap in educational achievement between individuals who spent their entire childhood in an intact family and those individuals who did not. For example, in the West German sample, some 38 percent of the former group had *Abitur* or higher qualifications but only 24 percent of the latter group. The differentials are even higher for the Guestworker sample (20 percent compared with 9 percent) and the East German sample (37 percent and 19 percent).

The other three outcomes are based on information about: school track followed at age 14; secondary school test scores in Mathematics, German, and first foreign language; and whether the child was ever held back in school during primary school years ('grade repetition'). We examine the probability of *Gymnasium* attendance because it is widely seen as the top track; indeed, there are sizeable wage advantages over the life cycle associated with it (Dustmann 2004). The age at which pupils move from primary to secondary school varies between states, from a minimum of 10 (for instance, Baden-Württemberg, Bavaria, Hamburg, and Hesse) to a maximum of 14 (e.g., Berlin, Brandenburg, and Bremen). Thus measuring school track at age 14 gives us a good measure of the route followed. Analysis of this outcome is based on a slightly different sample from that used to analyse the first schooling outcome: we require valid information about school track attended at age 14 as well as childhood family structure variables covering the first 14 years of their lives (rather than 16

[11] This discussion refers to West Germany. After reunification, East Germany adopted the educational system of West Germany (Jeschek 2000). But, even before 1990, the GDR had a similar school system, albeit with some differences in the length of the various secondary school tracks (e.g. completion of the *Gymnasium* track required eight rather than nine years). Such differences are inconsequential for the measurement of our dependent variables. They only marginally affect our measures of parental education, but this does not drive any of the differences in results for the East German and West German samples (see sections III.E and IV.D) and, of course, are irrelevant for the estimation of sibling difference models.

years, as elsewhere in the analysis).[12] In the West Germany sample, some 40 percent of individuals from an intact family had attended *Gymnasium*, whereas only 27 percent of individuals from a non-intact family had. For the East German sample, there was a similar differential, but none for the Guestworker sample.

The SOEP Youth Questionnaire (which was first collected in 2000) and the 'BIOSOC' supplement to the main questionnaire contain information about the scores obtained in secondary school for Mathematics, German, and the first foreign language. The data refers to the last school year, and so measured at different ages depending on the school track. Assessments are on a six-point scale, in which a score of 1 represents the highest mark and a score of 6 is the lowest mark. The outcomes we model are achievement of a high score (1 or 2), and achievement of a low score (5 or 6). Due to small sample sizes, analysis of these outcomes had to be restricted to children in the West German sample (N = 380). The differences in outcomes between children from intact families and non-intact families are not statistically significant for German and Mathematics, irrespective of whether we look at the top or the bottom of the distributions. For the first foreign language, children for non-intact families are significantly more likely to have a high score and to have a low score.

Grade repetition is fairly common among primary-school pupils in Germany (Max Planck Institute 2002). It is generally thought to indicate some kind of problem in school and to be a good predictor of future problems: for instance, several developmental studies have documented that grade repetition is negatively correlated with cognitive achievement and positively associated with dropping out of school (Reynolds 1992). Our grade repetition measure equals one if an individual had ever repeated a grade in primary school and equals zero otherwise. For sample size reasons, analysis is restricted to members of the West German sample (N = 389). Those from non-intact families are about 40 percent more likely to repeat a grade in primary school than their intact-family counterparts, and this difference is statistically significant.

D. Measures of Family Structure During Childhood

We use five different family structure measures, each of which was constructed from the mother's marital history files. The first measure takes value zero if a child lived continuously with both biological (or adoptive) parents up to his/her sixteenth birthday, and one otherwise.

[12] Information on secondary school track at 14 was obtained from the parents. For this outcome, we restricted our analysis to children who were enrolled at one of the three main types of secondary school (*Hauptschule*, *Realschule* and *Gymnasium*).

Thus, a child would have spent time in a non-intact family if he/she ever lived with a biological or adoptive mother who was not married before his/her sixteenth birthday either because of a partnership dissolution (through divorce or father's death) or because the child was born outside of marriage and the mother did not subsequently marry the biological father.[13] A number of earlier studies have reported different impacts of the experience of a non-intact family depending on the age of the child in which the dissolution occurs (Wojtkiewicz 1993; Hill et al. 2001). Our second measure therefore breaks down the first one into three, each corresponding to childhood stages: early childhood (birth to age 5), middle childhood (ages 6–10), and late childhood (ages 11–16).

Our third measure distinguishes between children whose mother was unmarried at their birth from children who ever lived with a separated/divorced mother and children who experienced the death of their father during childhood.[14] This measure is used in our first quasi-experiment: the experience of children who experienced the death of their father during childhood provides a benchmark from which to judge the endogeneity of divorce.

None of the three measures presented so far suffers from the "window" problem discussed by Wolfe et al. (1996) because they are based on information about childhood in its entirety. On the other hand, our first three measures focus on the 'occurrence' of non-intactness rather than its duration. This motivates our fourth and fifth family structure measures: the proportion of childhood years that a child lived in a non-intact family, and the proportion of childhood years that an individual lived with a mother who was unmarried at the individual's birth, with a separated/divorced mother, and with a widowed mother.

The family structure measures are summarised in Table 2. One in five individuals in the West German sample experienced life in a non-intact family at least once during childhood, which is about 30 percent fewer than in the East German sample and twice as many as in the Guestworker sample. The major types of family structure also differ by sample. For example, divorce was the most common reason for non-intactness in the West German and Guestworkers samples (especially the former), with unmarried motherhood and divorce parents equally common in the East German sample. About 50 percent of family

[13] For children born outside of a partnership before 1983 and for the mother's marital histories prior to 1983, we cannot know exactly whether the mother cohabited with or married the biological father. For the 255 children (nine percent of the individuals in the three samples pooled) whose mother partnered within one year, we assumed that she moved in with the biological father. Ermisch and Francesconi (2001) made a similar assumption.

[14] We also experimented with another measure that further distinguished mothers who repartnered after divorce or husband's death from mothers who did not. We do not report the results for such a measure because of the small size of the samples on which this analysis was performed, especially for the East German and Guestworker samples.

disruptions in the West German sample, and 70 percent in the East German sample, occurred when children were aged 0–5, mainly because of the substantial fraction of unmarried mothers in all samples. The proportions of years spent in a non-intact family shown in the table were computed using the whole sample of children (i.e. including those who always lived with both biological parents). Table 2 shows that, on average, children spent 8 percent, 3 percent and 12 percent of their childhood in a non-intact family in the West German, Guestworker, and East German samples respectively. If the samples are restricted to only children who lived in a non-intact family, the proportions become 39, 35 and 42 percent.

E. Additional Control Variables

Given the rich child/mother/household information available in the SOEP, we are able to use an extensive set of control variables corresponding to those that have been used in previous research. We have measures of: the individual's age, year of birth, and sex, whether the individual is an only child or not, measures of birth order, the number of brothers and sisters, and the region of residence (federal states). We also include a set of controls for the individual's mother's characteristics: her age when the individual was born, highest educational attainment, and number of years worked part time and full time during the individuals's childhood (ages 0–16). We also estimated some models using childhood family income as an additional regressor. The income measure was post-government household income averaged over all childhood years for which income information was available.

Descriptive statistics for the additional control variables are provided in Table 3. There are equal numbers of men and women. Members of the West German and Guestworker samples are about 2–3 years older than those in the East German sample, and their mothers also are about 2–3 years older. Guestworker sample members come from larger families, having more brothers and sisters and fewer are only children. For West German and Guestworker sample members, the most common maternal education level is the lowest one, and only 4–6 percent have mothers with university degrees. In contrast, among East German sample members, about 55 percent have mothers with intermediate school qualifications and 26 percent with university degrees.[15] East German mothers also have the strongest labour

[15] The maternal education variable has four categories, in ascending order: general secondary school qualifications or less, intermediate school qualifications, *Abitur*, technical college and university degree. To simplify cross-sample comparisons, we used the same broad categories though qualifications in the former FRG were different from those in the former GDR, and qualifications in Germany differ from those obtained abroad by mothers in the Guestworker Sample. Using an alternative categorisation of educational qualifications for mothers, distinguishing between mothers with engineering and technical college degrees from mothers with university degrees, did not change our key results presented in the next section.

market attachment, with nearly 13 years of full-time experience and three years of part-time experience, as opposed to three and five years respectively among West German mothers and six and two years respectively among Guestworker mothers. Average childhood family income was greatest for the West German sample, around €34,000 per year, which was about 17 percent and 14 percent greater than for children in the Guestworker and East German samples.

IV. Results

A. Basic Estimates for the West German Sample

In Table 4 we show the effect of childhood family structure on the probability of having educational qualifications to *Abitur* or higher for the West German sample. The first two columns report worst-case Manski bounds, the next three columns show estimates from three level regressions with progressively more control variables, and the last two columns present mother fixed-effects estimates obtained from linear probability models and conditional logit models. All regression estimates are expressed as marginal effects, evaluated at sample values of the other regressors.

Panel A indicates that there is a negative association between having lived in a non-intact family during childhood and the probability of attaining *Abitur* or higher qualifications. The largest point estimate ($\beta = -0.133$) is obtained from level regression specification [1]. The estimate falls inside the Manski bounds, so we cannot reject the model. The level estimates become smaller in magnitude as we move from specification [1] to specification [2] which also includes childhood family income as a regressor, to specification [3] which also includes maternal employment. Thus, the effect of non-intactness works also (but not exclusively) through parental income and employment. In fact, even in specification [3], having experienced life in a non-intact family is still associated with a statistically significant reduction of the chances of achieving *Abitur* or higher qualifications by 6 percentage points (but statistically significant only at the 10 percent level). This is a non-negligible effect, corresponding to at least one-sixth of the sample proportion who had *Abitur* or higher qualifications. The estimate is questionable, however, because its consistency relies on a selection-on-observables assumption which is hard to justify (Section II.B).

The sibling-difference estimator relies on a less stringent assumption for identification. The point estimates from these models are again negative, with $\beta = -0.067$ in the linear probability model (column [4]) and –0.049 in the conditional logit model (column

[5]). But both estimates are imprecisely estimated. A 95 percent confidence interval indicates that the data are consistent with there being a large adverse effect of growing up in a non-intact family or with the effect being non-existent. The loss in statistical significance of sibling-difference estimates has also been reported in other related studies and for different countries (Björklund et al. 2004).

We explored whether the imprecise effect arose because of differential and offsetting effects associated with different types of family structure (panel B). The difference between the estimated coefficient on 'Parents divorced' and that on 'Father died' can also be given a causal interpretation assuming the father's death provides exogenous variation in parental separation. Level estimates indicate a significantly lower probability of achieving *Abitur* or higher qualification for individuals whose mothers were not married at their birth and for children of divorced mothers, even after controlling for family income (specification [2]), by 10 percentage points and 8 percentage points respectively. However, once we also control for childhood maternal employment, the estimates become smaller in magnitude and statistically insignificant. The fixed-effects estimates reveal that having been born to an unmarried mother reduced the chances of achieving *Abitur* or higher qualifications by 11–13 percentage points, while death of one's father's *increased* such chances by 2–18 percentage points. Such estimates however never differ significantly from zero. This is in line with the results reported by Corak (2001) and Lang and Zagorsky (2001). The results in panel B suggest that there is some variation in the effects of different types of childhood family structure, with the worst outcomes emerging among children of unmarried mothers. But when we control for childhood maternal employment in level regressions, or account for mother-specific unobservables or the endogeneity of family breakdown, there is no clear-cut evidence that any type of childhood family structure significantly affects children's later educational achievements.

Next we consider whether the impact of non-intactness varied with the age at which it was experienced (panel C). The results here echo those in Panel A. The estimates from sibling-difference models have a negative sign (in all but one case) and are imprecisely estimated. We can never reject the hypothesis that the estimated effect is equal to zero irrespective of the childhood stage in which the non-intactness occurred. Thus timing does not seem to matter.

Finally, we switch from occurrence to duration measures. Panels D and E show the estimates obtained for the proportion of childhood years in any type of non-intact family and also broken down by types of non-intact family. According to level regression [1], panel D, there is a significant negative association between time spent in a non-intact family and the outcome, but the estimates become much smaller in magnitude and statistically insignificant as controls for childhood family income and maternal employment are added in the next two columns. No statistically significant estimates arose when duration was broken down by family type (panel E). The same conclusions can be drawn from the fixed-effects models.

Taken together the estimates in Table 4 suggest that we cannot conclude with confidence that experience of life in a non-intact family during childhood has a detrimental impact on the probability of achieving *Abitur* or higher qualifications for West German young adults. An adverse effect is what the level regressions point to but, once correlated unobserved background characteristics are accounted for, the magnitude of the effect becomes smaller and less precisely estimated.

B. Guestworker Sample and East German Sample

We repeated the analysis presented so far for the Guestworker and East German samples but, for brevity, only report the estimates for two measures of family structure and exclude the conditional logit estimates.[16] The results are in Table 5.

Despite the differences in family structure and educational attainment between three sample groups discussed earlier, the results shown in Table 5 are remarkably similar to those already seen for the West German sample. In particular, the level estimates imply that growing up in a non-intact family has a large and statistically significant adverse effect, reducing the probability of *Abitur* or higher qualifications by about 9 percentage points for both East German sample members and Guestworker sample members, even after controlling for family income and maternal employment. As for the West German sample, the largest negative associations are estimated for children of unmarried mothers.[17] But, again as before, the differences in outcomes for individuals whose father died and children who grew up with a divorced mother are never statistically significant at the 5 percent level. These findings are corroborated by the sibling-difference estimates.

[16] These estimates are typically lower (in absolute value) than those obtained from the fixed-effects linear probability models shown in the table, and are never statistically significant. Similarly, the results for the other three family structure measures do not alter the picture presented here. In general, these patterns about fixed-effects logit regressions and various family structure measures emerge also in our subsequent analyses, even if their specific results are not shown.

[17] As a robustness check, we reestimated the model for the Guestworker sample also including a set of dummy variables for mothers' and fathers' nationality. The estimates on the family structure variables were very similar to those reported in Table 5, while the nationality dummies were jointly statistically insignificant.

C. Other Schooling Outcomes

Family structure effects may be statistically insignificant because educational qualifications are measured potentially many years after when the family disruption occurred. A number of studies by developmental psychologists and sociologists have found that parents and children gradually adjust to divorce, with parents' childrearing skills improving and parental conflict tapering off (Amato 1993). If this is the case, children's well-being after marital dissolution will eventually improve with the passage of time, and we might expect to observe inferior outcomes concentrated at (early) stages of life closer to the time of family breakdown.[18] For this reason, we consider other schooling outcomes which are observed at younger ages, such as *Gymnasium* attendance at age 14, secondary school scores and grade repetition.

Estimates of the 'Ever lived in a non-intact family' variable are shown in Table 6. For brevity, we report only level estimates from the specification that includes childhood family income and maternal employment and mother fixed-effects estimates obtained from linear probability models for the three samples. But the results from conditional logit regressions and other family structure measures are also discussed. Models for scores and grade repetition could only be estimated using level models and for the West German sample because of small sample sizes and insufficient variation between siblings.

For the West German sample, the fixed-effects estimate implies that experience of life in a non-intact family significantly lowers the probability of attending *Gymnasium* at age 14: the marginal effect is a reduction of some 15 percentage points. (The level estimate is a seven percentage point reduction, and significant only at the 10 percent level.) The sibling-difference estimate is well within the Manski bounds, and appears mainly to reflect the adverse effects of divorce when children were aged 6–14 and of father's death when children were aged 11–14 (estimates not shown). Estimating the sibling-difference model with conditional logit regressions leads again to an effect that is still negative and significant, but substantially smaller (β = –0.038, *t*-value = –2.13). However, the probability of attending *Gymnasium* at age 14 for individuals who experienced father's death is not statistically different from that of children who lived in an intact family, and is higher than the probability for children of divorced parents. Thus the finding that family non-intactness has an adverse

[18] These earlier outcomes however may be relevant if they facilitate the productivity of later investments or if the skill acquisition that they imply raises skill attainment at later stages of the life cycle (Cunha et al. 2005). Potentially worse early outcomes might affect other (later) aspects of life (e.g. self-esteem, social networks, marital prospects, and earnings), even if we cannot find evidence of a negative impact of growing up in a non-intact family on the probability of achieving *Abitur* or more. Examining such later outcomes is beyond the scope of this paper.

causal effect on *Gymnasium* attendance is not robust across methods which differently account for family structure endogeneity.

For the Guestworker sample, the estimated effects are negative but are statistically significant only in the level model. There is also no evidence of a significant impact among children in the East German sample. In the models of the probabilities of being at the top or at the bottom of the score distributions in German, Mathematics and first foreign language, and the probability of repeating a grade in primary school, it turns out that estimated family structure impacts are all close to zero, and statistically insignificant.

Although the negative effect for *Gymnasium* attendance is not robust, how can we reconcile it with the insignificant result of the probability of having *Abitur* or higher qualification? One explanation has already been offered: we might expect inferior outcomes to be concentrated at stages of life that are closer to the time of family breakdown, and age 14 is well before the age at which we measure highest educational qualification (after 19 at least, and usually much later). It may be that the *Gymnasium* system (and, afterwards, the university system) equalises opportunities for children of different backgrounds in such a way that any initial disadvantage in terms of *Gymnasium* access may be eroded with the passage of time.

Another explanation is simply related to our measures of schooling outcomes. First, it is possible to get the *Abitur* even if not attending *Gymnasium* at age 14. In the West German sample, about 18 percent of individuals with *Abitur* switched from other school tracks to the *Gymnasium* after age 14. Second, some tertiary degrees can be gained without having *Abitur*, and these are counted as achievement of '*Abitur* or higher qualification': 22 percent of the individuals in the West German sample, who have contributed to the estimation of attainment probabilities, have such degrees. The reconciliation of the family structure effects across outcomes rests on whether the "switchers" between school tracks were more likely to experience living in a non-intact family during childhood. Indeed this was the case, with nearly 16 percent of the switchers coming from a non-intact family as opposed to 12 percent for non-switchers, and the difference being statistically significant.

All in all, we have found no clear-cut evidence that childhood family structure has an adverse impact on schooling outcomes. One exception concerns *Gymnasium* attendance at age 14, which is substantially lower for individuals who lived in a non-intact family during childhood. This effect however is detected only by fixed-effects estimators and not by the instrumental-variables approach based on fathers' death. We return to this point in Section IV.E.

D. The Effect on Schooling Outcomes of Variables Other than Childhood Family Structure

In Table 7 we show level and sibling-difference estimates (from logit and linear probability models, respectively) of the impact on the probability of having achieved *Abitur* or higher qualifications of the various control variables used for the models reported in Tables 4 and 5.

For the West German and Guestworker samples, the probability of achieving Abitur or higher qualifications is higher for those aged 22 or more than for younger individuals. For the East German sample, the opposite is the case according to the level estimates, perhaps reflecting the depreciation among the older groups of skills that were more relevant in the pre-reunification GDR. The West German sample is distinctive because it is the only sample for which gender matters: compared to young men, young women are less likely to have *Abitur* or higher qualifications.

Being an only child appears not to affect attainment probabilities but, among those with siblings, birth order and number of siblings have heterogeneous impacts across the samples. For example, having more brothers and sisters is estimated to have an adverse effect for all samples (the effect is most precisely estimated for the Guestworker sample). On the other hand, birth order has no significant effects on attainment with the exception of the West German sample, for which there is an adverse impact of not being the first born.

The impact of household income is ambiguous. Although the level estimates indicate that having a higher income raises attainment probabilities, only in the West German sample the sibling-difference estimates are statistically significant.

Individuals with older mothers have higher achievement probabilities according to the level estimates but this effect disappears when mother's fixed effects are taken into account. For the West German sample, the more highly qualified the mother, the more likely is her child to have *Abitur* or higher qualifications. For the East German sample, there is a similar association, though the advantage conferred from having some qualifications rather than none is less than for the West German sample. By contrast, for the Guestworker sample, the only distinct advantage is apparently associated with a mother having *Abitur*-level qualifications herself. The mixed results for this sample may reflect genuine heterogeneity in the way in which the educational qualifications that mothers gained (perhaps outside Germany) were treated in Germany. Once we control for maternal education, the longer that a mother was employed during childhood is associated with lower attainment probabilities for children in the West German and Guestworker samples, though this association evaporates with the sibling-difference estimates.[19]

E. Difference-in-Difference Estimates in a Before and After Design

We implemented the before-after design described by equation (2), comparing East German sample members and West German sample members.[20] Guestworker sample members were excluded in order to reduce observed heterogeneity between treatment and control groups. (Their inclusion however did not alter our conclusions.) The analysis concerned the probability of achieving *Abitur* or higher qualifications and the probability of *Gymnasium* attendance at age 14. We define the post-reform period to refer to the post-1977 years and the pre-reform period to refer to the pre-1976 years, since the June 1976 family law reform was implemented only in June 1977, and because the information available in the SOEP for divorces that occurred before the start of the survey (1984) is only available in years.[21] The vector Z in equation (2) contained variables summarizing socio-demographic characteristics such as maternal and paternal education, age of mother at child birth, number of children, and family income, a full set of regional dummies timed either when the divorce occurred (if the divorce occurred after the beginning of the panel) or at the first wave the mother is observed (if the divorce occurred before 1984), and a full set of year-region interactions.[22]

Linear probability estimates obtained from two specifications of equation (2), and for five different subsamples, are summarised in Table 8. The two specifications differ by whether or not they include time trends, i.e. whether δ_3 and δ_4 and the year-region interactions in Z are set to zero or not. The five subsamples differently account for the fact that, since 1990, there has been one uniform family law for the whole of Germany, and thus β_1 is identified only through variation over time in divorce law rather than variation over time and across states.

[19] We also estimated models in which the 'ever in a non-intact family' effect was allowed to differ by birth order. For each sample, the lower attainment probabilities for individuals who experienced life in a non-intact family are further reduced for those who are not the first born. The other results do not differ from those based on Tables 4, 5, and 7.

[20] Before 1990 migration between the former GDR and FRG was virtually inexistent. Since then migration is allowed but there is one uniform legal code applied to the whole of Germany. Our results therefore are likely not to suffer from selective migration bias, whereby migration decisions are related to divorce regimes.

[21] Excluding parents who divorced in 1976 and 1977 from the West German sample meant dropping 13 observations, i.e., 4 percent of all divorced mothers in the sample (or 0.5 percent of all mothers). Importantly, for the estimation of *Gymnasium* attendance, 1976 and 1977 are included as pre-reform years for individuals from the former GDR, otherwise the control group would not have information on the pre-reform period.

[22] The reason of the different timing of the region variables is because the SOEP does not collect retrospective information on housing and residential location.

Regardless of specification and subsample, there is no significant impact of unilateral divorce on the probability of having *Abitur* or higher qualification. (The point estimates are positive suggesting that a potentially lower parental conflict experienced by children whose parents go through unilateral divorce might *improve* their performance on this school outcome.) The point estimates of β_1 in the equation for *Gymnasium* attendance at age 14 are negative and range between 11 and 14 percentage points. They are similar in magnitude to the fixed-effects estimate for the West German sample shown in Table 6, but are now statistically insignificant.

Overall these results reinforce our previous conclusion that once one turns from simple level regression models to models accounting for endogeneity, it is difficult to find any straightforward effect of childhood family structure.

F. Robustness Checks

We made a number of robustness checks.[23] First, because mother fixed-effects models can only be estimated on families with at least two siblings and these families could be a nonrandom subgroup of the population, we reestimated the (level) logit regressions for the probability of achieving *Abitur* or higher using data for two subsamples. The first subsample excluded only children, and so included individuals with siblings, some of whom however may not be observed (because they either left home already or were too young to be selected for the analysis). The second subsample consisted of siblings for whom we have valid information on whether or not they achieved *Abitur* or higher qualifications (i.e., the same sample as that used earlier for the fixed-effects regressions). In the absence of any sizable bias, we expect the results based on these two new subsamples to be comparable to the estimates in Tables 4 and 5. Indeed, the point estimates from the two new subsamples are quite similar to our previous results.[24]

Second, our fixed-effects regressions are run on arguably small samples, especially in the case of the East German and Guestworker samples. We thus pooled our original three samples in one. On this new sample, we then reestimated the probability of having *Abitur* or higher qualifications using level and fixed-effects regressions, after including sample dummies and interactions between family structure variables and sample dummies. As before, the level estimates indicate a significant negative association between measures of

[23] For brevity, the detailed estimates are not reported, but are available from the authors.
[24] The smaller size of these subsamples reduced the precision of some of such estimates, however.

family non-intactness and attainment probabilities. But again, sibling-difference models show little evidence that childhood family structure significantly affects children's later schooling achievements.

Third, because the SOEP does not collect a full history of housing tenure and residential mobility over the years before the panel began, we cannot fully control for geographic location during childhood years for a large group of children in our sample. If the residential patterns of non-intact families are systematically different from those of intact families, one mechanism through which children's lives are affected is undetected (Pribesh and Downey 1999). The only reliable information that we can use for all children in our samples is the number of years they have lived in their current address during childhood. In the level regressions for the probability of achieving *Abitur* or higher qualifications, controlling for this variable (expressed as a proportion of childhood years) does not alter the results shown in Tables 4 and 5, while this variable is never statistically significant. In particular, the point estimates on the family structure measures do not change much in all three samples, but in the Guestworker sample standard errors get larger so that the corresponding estimates tend to lose statistical significance.

Fourth, in spite of the similarity of results across samples, there may be a concern that the relationship between school outcomes and family structure differs between the East German sample on the one hand and the West German and Guestworker samples on the other hand, simply because the data for East and West Germany span different time periods. For example, if the stigma of divorce fell over time, we may expect to see even smaller associations of non-intactness with school outcomes among children in the West German and Guestworker samples. To help address this issue, we reestimated our level and fixed-effects regression models for the probability of attaining *Abitur* or more using individuals from the West German and Guestworker samples from 1990 onwards, which corresponds to the time period of the East German sample. This alternative selection did not change any of our previous findings.

V. Conclusions

Does experience of life in a non-intact family during childhood affect children's school outcomes in Germany? Our analysis shows that there is no simple answer. According to level regression models, which do not control for the possibility that family structure and school outcomes share common unobserved factors, growing up in a non-intact family is generally associated with worse outcomes. However, when endogeneity is accounted for – whether by comparing siblings who experienced different family structures, or by comparing children who experienced parental loss through death or divorce with those from intact families, or by exploiting legislative changes to the former West German divorce law – there is little evidence that family structure significantly affects children's schooling performance.

These conclusions hold true regardless of the age of the child when parental separation occurred, and for all three samples (West German, East German, and Guestworker samples). This is quite remarkable, given the substantial differences in socioeconomic institutions generally (West German versus East German samples) and in social and cultural milieu (West German and Guestworker samples). Moreover our results are consistent across a number of different schooling outcomes: the probability of having *Abitur* or higher qualifications, of being at the top or the bottom of the distributions of secondary school scores, and of repeating a grade during primary school. An exceptional result concerns the probability of *Gymnasium* attendance at age 14 for West German children. In this case, the fixed-effects estimates indicate a large and statistically significant adverse impact of experience of life in a non-intact family at earlier ages. This effect however is not detected by instrumental-variables estimators, and is thus not robust.

In general, therefore, our findings indicate that the evidence for a causal effect running from family breakdown to schooling outcome is weak. The results should not be interpreted to mean that family background has no effect on educational achievements. There are some strong associations of attainment with observable parental characteristics, of which maternal education and family income are prime examples. The extent to which these and other family influences are causal is not known for Germany; establishing this is likely to be a promising avenue for future research.

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Table 1: Means of the Outcome Variables by Sample and Childhood Family Structure

	West German sample		Guestworker sample		East German sample	
	Non-intact Family	Intact Family	Non-intact Family	Intact Family	Non-intact Family	Intact Family
Abitur or higher qualification	0.238	0.377	0.087	0.207	0.193	0.368
N	286	1116	69	673	166	397
Gymnasium attendance at age 14	0.267	0.401	0.152	0.167	0.316	0.454
N	303	1263	79	778	231	518
Scores						
Proportion with high scores (1, 2)						
German	0.312	0.314				
Mathematics	0.325	0.360				
First foreign language	0.351	0.284				
Proportion with low scores (5, 6)						
German	0.013	0.020				
Mathematics	0.052	0.049				
First foreign language	0.091	0.040				
N	77	303				
Grade repetition (primary school)	0.105	0.074				
N	38	351				

Note: *N* is the number of individuals.

Table 2: Childhood Family Structure, by Sample

	West German sample	Guestworker sample	East German sample
Ever lived in a non-intact family	0.204	0.093	0.294
Born to unmarried mother	0.054	0.034	0.147
Parents divorced	0.113	0.046	0.135
Father died	0.037	0.013	0.012
Ever lived in a non-intact family at ages:			
0–5	0.099	0.050	0.206
6–10	0.049	0.019	0.042
11–16	0.056	0.024	0.046
Proportion of childhood years lived in a non-intact family	0.080	0.033	0.120
	(0.204)	(0.131)	(0.242)
Proportion of childhood years lived with unmarried mother	0.022	0.011	0.050
	(0.118)	(0.078)	(0.167)
Proportion of childhood years lived with divorced mother	0.047	0.018	0.062
	(0.159)	(0.089)	(0.166)
Proportion of childhood years lived with widowed mother	0.011	0.004	0.008
	(0.074)	(0.047)	(0.068)
N	1402	742	563

Note: *N* is the number of individuals. Standard deviations are in parentheses.

Table 3: Summary Statistics, by Sample

	West German Sample	Guestworker Sample	East German Sample
Age	25.261	24.631	22.160
	(5.000)	(4.498)	(2.425)
Age < 22	0.314	0.317	0.456
Age 22-25	0.268	0.321	0.423
Age > 25	0.418	0.362	0.121
Year of birth	1973.76	1973.62	1979.01
Female	0.492	0.474	0.502
Mother's highest educational attainment			
No degree or secondary general school certificate	0.654	0.935	0.158
Intermediate school certificate	0.256	0.019	0.552
Grammar school certificate (*Abitur)*	0.027	0.005	0.034
Technical college or university degree	0.063	0.041	0.256
Mother's age at birth	26.934	26.326	24.364
	(5.564)	(5.948)	(4.472)
Only child	0.126	0.047	0.154
Number of brothers[a]	0.809	1.224	0.663
Number of sisters[a]	0.779	1.203	0.595
Birth order[a,b]			
First child	0.386	0.311	0.489
Second child	0.393	0.314	0.418
Third child or more	0.221	0.375	0.092
Average post-government household income during childhood years[c]	34,410	29,320	30,099
	(16,109)	(8,883)	(10,121)
Mother's employment during childhood years:			
Number of years full-time employed	3.181	6.345	12.639
	(4.870)	(6.341)	(4.909)
Number of years part-time employed	4.705	2.315	2.802
	(5.257)	(3.933)	(4.134)
N	1402	742	563

Notes: Figures are sample means with standard deviations in parentheses.
[a] Includes adopted and foster children.
[b] Computed for children with siblings only.
[c] Computed for all childhood years for which positive household income is available. Household income was deflated using the Consumer Price Index and is expressed in constant Euros (base year = 2000).

Table 4: Childhood Family Structure and the Probability of Achieving *Abitur* or Higher Qualifications–West German Sample

	Manski's Bounds[a]		Level (logit) Estimates[b]			Mother FE Estimates[c]	
	Largest Lower	Smallest Upper	[1]	[2]	[3]	Linear Probability	Conditional Logit
Panel A							
Ever lived in a non-intact family	-0.153	0.171	-0.133	-0.090	-0.064	-0.067	-0.049
	(0.099)	(0.063)	(0.031)	(0.035)	(0.037)	(0.087)	(0.147)
Panel B							
Born to unmarried mother	-0.103	0.375	-0.127	-0.104	-0.081	-0.115	-0.127
	(0.056)	(0.175)	(0.046)	(0.051)	(0.056)	(0.128)	(0.348)
Parents divorced	-0.103	0.171	-0.129	-0.082	-0.056	-0.076	-0.068
	(0.058)	(0.064)	(0.038)	(0.044)	(0.046)	(0.111)	(0.220)
Father died	-0.224	0.111	-0.131	-0.092	-0.072	0.182	0.027
	(0.054)	(0.109)	(0.060)	(0.072)	(0.080)	(0.251)	(0.082)
Panel C							
Ever lived in a non-intact family at ages:							
0-5	-0.103	0.200	-0.133	-0.101	-0.073	-0.062	-0.057
	(0.054)	(0.072)	(0.038)	(0.043)	(0.046)	(0.097)	(0.169)
6-10	-0.103	0.167	-0.114	-0.060	-0.034	-0.092	-0.025
	(0.056)	(0.089)	(0.053)	(0.064)	(0.067)	(0.131)	(0.109)
11-16	-0.151	0.111	-0.129	-0.090	-0.069	-0.071	-0.002
	(0.063)	(0.107)	(0.048)	(0.055)	(0.059)	(0.137)	(0.072)
Panel D							
Proportion of childhood years lived in a non-intact family			-0.195	-0.106	-0.051	0.023	-0.015
			(0.078)	(0.082)	(0.081)	(0.157)	(0.063)
Panel E							
Proportion of childhood years lived with unmarried mother			-0.112	-0.059	-0.007	-0.429	-0.137
			(0.117)	(0.126)	(0.132)	(0.540)	(0.402)
Proportion of childhood years lived with divorced mother			-0.256	-0.148	-0.092	-0.083	-0.026
			(0.104)	(0.104)	(0.101)	(0.172)	(0.094)
Proportion of childhood years lived with widowed mother			-0.177	-0.073	-0.014	0.768	0.194
			(0.208)	(0.220)	(0.227)	(0.402)	(0.761)

Notes: Standard errors in parentheses.
[a] Computed using 48 groups based on individual's age (two groups: age ≤ 24 years, aged > 24 years), sex (two groups), mother's highest educational attainment (two groups: mother has at least intermediate school qualifications, mother has less than intermediate school qualification), mother's age at the child's birth (three groups: mother aged ≤ 24 years, aged 25-27, aged ≥ 28 years), year of birth (two groups: born before 1974, born in 1974 and later). Standard errors are obtained with 500 bootstrap replications.
[b] Figures are marginal effects from logit regressions computed at average values of all the variables used. Other variables are age groups, sex, year of birth, mother's highest educational attainment, mother's age at the child's birth, whether the respondent is an only child, number of brothers and sisters, birth order, regional dummy variables, a linear time trend, and a constant. Specifications [2] and [3] also include the average income during childhood years. Specification [3] also includes the number of years of maternal part-time and full-time and part-time employment during the respondent's childhood. Standard errors allow for arbitrary serial correlation.
[c] Figures are marginal effects computed at average values of all the variables used. FE = fixed effects. Other variables are the (sibling) differences in gender, age, mother's age at the child's birth, whether the respondent is the first-born and a constant. Standard errors are robust to any form of correlation between siblings.

Table 5: Childhood Family Structure and the Probability of Achieving *Abitur* or more – Guestworker Sample and East German Sample

	Manski's Bounds		Level (logit) Estimates			Mother FE Estimates
	Largest Lower	Smallest Upper	[1]	[2]	[3]	Linear Probability
A. Guestworker Sample						
Ever lived in a non-intact family	-0.136	0.250	-0.101	-0.098	-0.088	-0.068
	(0.079)	(0.224)	(0.034)	(0.035)	(0.035)	(0.103)
Born to unmarried mother	-0.111	0.250	-0.126	-0.124	-0.124	-0.058
	(0.045)	(0.197)	(0.037)	(0.038)	(0.036)	(0.142)
Parents divorced	-0.156	0.333	-0.103	-0.101	-0.083	-0.021
	(0.054)	(0.273)	(0.042)	(0.042)	(0.045)	(0.158)
Father died	-0.091	0.667	-0.008	-0.002	0.011	-0.513
	(0.048)	(0.064)	(0.101)	(0.105)	(0.111)	(0.324)
B. East German Sample						
Ever lived in a non-intact family	-0.181	0.111	-0.114	-0.093	-0.094	-0.068
	(0.119)	(0.108)	(0.044)	(0.047)	(0.047)	(0.169)
Born to unmarried mother	-0.167	0.250	-0.137	-0.119	-0.119	-0.069
	(0.158)	(0.222)	(0.049)	(0.052)	(0.052)	(0.194)
Parents divorced	-0.167	0.111	-0.078	-0.058	-0.060	-0.068
	(0.144)	(0.103)	(0.056)	(0.065)	(0.065)	(0.279)
Father died[a]	-0.167	0.545	-0.097	-0.072	-0.077	
	(0.154)	(0.156)	(0.146)	(0.142)	(0.135)	

Notes: Standard errors in parentheses. For definitions and comments, see notes to Table 4.
[a] Due to small sample sizes, the reference category in the mother FE regression for the East German Sample includes children whose father died.

Table 6: The Impact of 'Ever Lived in a Non-intact Family' on Other Educational Outcomes by Sample

	West German Sample		Guestworker Sample		East German Sample	
	Level	FE	Level	FE	Level	FE
Gymnasium attendance at age 14	-0.065	-0.154	-0.059	0.048	-0.081	0.008
	(0.041)	(0.072)	(0.030)	(0.078)	(0.047)	(0.094)
Top scores[a]						
German	0.025					
	(0.066)					
Mathematics	-0.008					
	(0.068)					
First foreign language	0.099					
	(0.073)					
Bottom scores[a]						
German	-0.0001					
	(0.012)					

Mathematics	-0.005
	(0.007)
First foreign language	0.016
	(0.019)
Grade repetition (primary school)[b]	-0.015
	(0.025)

Note: The control variables are as in Table 4 with the exception of age dummies.
[a] Each regression also controls for school track attended.
[b] Regression does not include federal state dummies. According to the timing of the transition from primary to secondary school in federal states, we used the first four or six years of schooling when measuring grade repetition in primary school. Federal states in West Germany in which transition from primary school to secondary school occurs after four years of primary education are Baden-Württemberg, Bayern, Hamburg, Hessen, Nordrhein-Westfalen, Rheinland-Pfalz, and Saarland. Transition to secondary school after six years of primary education occurs in Berlin (West), Schleswig-Holstein, Niedersachsen, and Bremen (Sekretariat der Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland 2003).

Table 7: Other Covariates and the Probability of Achieving *Abitur* or More by Sample

	West German Sample		Guestworker Sample		East German Sample	
	Level	FE	Level	FE	Level	FE
Age 22-25	0.293	0.170	0.193	0.215	0.046	0.306
	(0.053)	(0.049)	(0.054)	(0.051)	(0.084)	(0.117)
Age > 25	0.297	0.203	0.206	0.289	-0.034	0.321
	(0.081)	(0.064)	(0.089)	(0.068)	(0.143)	(0.216)
Year of birth	-0.003		0.005		-0.042	
	(0.006)		(0.006)		(0.022)	
Female	-0.026	-0.083	0.007	0.027	0.094	0.090
	(0.028)	(0.036)	(0.025)	(0.038)	(0.041)	(0.078)
Only child	-0.049		-0.035		-0.096	
	(0.050)		(0.054)		(0.062)	
Mother's age at birth	0.014	-0.004	0.004	0.014	0.028	-0.021
	(0.003)	(0.010)	(0.003)	(0.009)	(0.006)	(0.043)
Number of brothers	-0.035		-0.057		-0.098	
	(0.022)		(0.018)		(0.049)	
Number of sisters	-0.067		-0.056		-0.089	
	(0.023)		(0.015)		(0.046)	
Birth order						
Second child	-0.115	-0.104	0.028	0.023	-0.177	0.083
	(0.030)	(0.042)	(0.032)	(0.047)	(0.047)	(0.119)
Third child or more	-0.216	-0.117	-0.015	0.019	-0.085	0.334
	(0.038)	(0.083)	(0.043)	(0.077)	(0.047)	(0.062)
Average post-government household income during child's	0.006	0.012	0.003	0.005	0.007	-0.014
childhood years[a]	(0.001)	(0.004)	(0.002)	(0.006)	(0.002)	(0.012)
Mother's highest educational attainment						
Intermediate school	0.245		-0.029		0.142	
Certificate	(0.039)		(0.051)		(0.071)	
Grammar school	0.344		0.319		0.134	
certificate	(0.089)		(0.163)		(0.153)	
Technical college or	0.514		0.099		0.306	
university degree	(0.055)		(0.099)		(0.091)	
Mother's employment during child's childhood years:						
Number of years in	-0.006	0.001	-0.002	-0.011	0.005	-0.004
part-time employment	(0.003)	(0.012)	(0.003)	(0.017)	(0.009)	(0.047)
Number of years in	-0.012	-0.006	-0.008	-0.002	0.006	0.061
full-time employment	(0.003)	(0.014)	(0.003)	(0.012)	(0.009)	(0.038)

Notes: Reference categories are age<22, mother with general secondary school qualification, male and first born. The estimates correspond to those reported in Tables 4-5 (specification [3] in the case of the level regressions, and linear probability model in the case of the FE regressions). Standard errors are in parentheses. For details see the notes to Table 4.

[a] Household income is divided by 1000.

Table 8: Difference-in-Difference Estimates

	Abitur or higher qualification	*Gymnasium* attendance at age 14
Panel A (all sample)		
No time trends	0.079	-0.139
	(0.093)	(0.111)
With time trends	0.124	-0.138
	(0.097)	(0.110)
N	1735	2041
Panel B (divorces in 1990-1992 in East Germany dropped)		
No time trends	0.078	-0.140
	(0.093)	(0.111)
With time trends	0.123	-0.139
	(0.097)	(0.111)
N	1730	2035
Panel C (all divorces in 1990-1992 dropped)		
No time trends	0.069	-0.120
	(0.094)	(0.112)
With time trends	0.095	-0.116
	(0.092)	(0.112)
N	1713	2011
Panel D (divorces after 1990 in East Germany dropped)		
No time trends	0.068	-0.113
	(0.094)	(0.111)
With time trends	0.111	-0.110
	(0.098)	(0.111)
N	1703	1981
Panel E (all divorces after 1990 dropped)		
No time trends	0.075	-0.117
	(0.093)	(0.119)
With time trends	0.121	-0.118
	(0.098)	(0.120)
N	1694	1929

Notes: Figure are β_1 estimates (see equation (2)) obtained from separate linear probability model regressions. Standard errors that allow for arbitrary serial correlation are in parentheses. The variables in Z are year of birth, sex, birth order, number of brothers, number of sisters, mother's age at birth, mother's highest educational qualification, average childhood family income, number of years of maternal part-time and full-time employment during childhood, and a full set of interaction terms between federal state dummies and year dummies. The regressions where *Abitur* or more is the dependent variable also include child's age. Both East and West German samples contain children with a German-born mother only.