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Childhood Family Structure and Schooling Outcomes: Evidence for Germany

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Abstract

Using multiple methods and samples, we analyse the impact on schooling outcomes of growing up in a non-intact family. We find that having lived in a disrupted household in Germany is associated with worse outcomes according to estimates from models that do not control for possible correlations between common unobserved determinants of family structure and educational performance. But when endogeneity is accounted for, using sibling-difference estimators and two quasi-experimental estimators, there is little conclusive evidence of a causal impact of family structure on schooling outcomes, particularly for individuals who grew up in Western Germany, whether from native or guestworker families. For young adults who grew up in Eastern Germany, there is some evidence that experience of life in a non-intact family during childhood reduces subsequent educational attainment.

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1. Introduction

No parent wishes to see their child do badly at school and the support for public policies directed at improving children's attainments and avoiding disadvantageous outcomes is widespread. At the same time, the debate about the extent to which family breakdown during childhood affects individual's life chances is intense in many countries. Given the substantial interest in these topics 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 educational attainments. 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 unobserved factors. Unlike many previous studies, we seek a robust picture of the impact of childhood family structure by contrasting results from different estimation methods, each of which relies on different but complementary identification strategies.¹ We estimate level and propensity score matching models, mother fixed-effects and quasi experimental models, and models based on comparisons between individuals whose fathers died, divorced, or remained married. The main schooling outcome analysed is whether an individual has educational qualifications to university entrance level or higher, but we also consider other measures of schooling performance. We use various definitions of childhood family structure, and check robustness in several other aspects.

Our second contribution is substantive. This is one of the first 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.

¹ Two recent examples of a similar approach are Levine and Zimmerman (2005) and Currie and Tekin (2006). They use a variety of statistical methods to measure the effects of a child's exposure to welfare benefit receipt on developmental outcomes, and child maltreatment on crime, respectively.

Knowing that growing up in a disrupted family has causal effects is crucial for underpinning a wide range of public policies. Children may alter their attitudes toward education or receive special support from schools while living in a non-intact household. Alternatively, the observed correlations may reflect nothing more than correlated unobservables that affect both parents' family structure and children's later attainment. For example, parents and children may live in communities with poor schools and few job prospects. Germany is an extremely interesting case for this analysis, not only because this study has never been performed on Germany before, but also because the German government is committed to a huge public education programme (Hanushek 2002) and large welfare expenditures on families with children (Garfinkel 2005). In our context, another interesting feature is that 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 2005). 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 is 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.

For the West German and Guestworker samples, we find that growing up in a non-intact family (especially if parents divorced) has an adverse effect on schooling outcomes. But once the potential endogeneity of educational attainment and childhood family structure is accounted for, the detrimental impact of non-intactness loses its statistical significance. Put another way, the confidence intervals around the estimates are wide enough that the data are consistent with the impact of family structure being zero as well as negative. By contrast, the estimates from the East German sample suggest that the adverse association between lower school achievement and family non-intactness may have a causal interpretation. This result is robust to a number of alternative model specifications and data selections (though not across all our estimators).

The rest of the paper is organized as follows. Section 2 reviews relevant previous literature and outlines our empirical strategy. Section 3 presents the data, and the definitions of schooling outcomes, family structure, and other control variables. Section 4 discusses our main findings and Section 5 provides a summary and conclusions.

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

A. Related Literature

An extensive body of research, mostly based on US data, has identified childhood family structure as a key determinant of children's later achievements (Haveman and Wolfe 1995). Most studies have found that growing up without a biological parent is negatively associated with schooling attainments and 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, the same mother. Recent studies of educational outcomes using these methods are Case et al. (2001), Ermisch and Francesconi (2001), Ginther and Pollak (2004), and Gennetian (2005).

A number of studies have compared children's attainments before and after the divorce of their parents (e.g. Cherlin et al. 1991; Painter and Levine 2000; Piketty 2003). 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).

There are also quasi-experimental studies. One type has used parental death as an exogenous source of parental absence (see, among others, Biblarz and Gottainer 2000; Corak 2001; Lang and Zagorski 2001). A second type has used comparisons of educational outcomes for children who were exposed to different divorce laws during childhood. Examples include Gruber (2004) who exploited variation across US states and over time in changes in divorce regulation and Piketty (2003) who exploited the increase in separation rates following a divorce law change in France.

A distinctive feature of our research is that we seek a robust picture of the impact of childhood family structure on schooling outcomes, derived by contrasting estimates from different methods (each of which impose different identifying assumptions), studying several

outcomes, and employing several different definitions of family structure (e.g. not only the occurrence of non-intactness but also its duration).

Another distinctive feature is that we study Germany, a country for which the evidence available about the association between family structure and child’s education is limited.² Mahler and Winkelmann (2004) report that growing up in a lone-mother family slightly reduced the probability of being in the *Gymnasium* secondary school track, but also argued 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 evidence for Germany is that it is based on “cross-section” (or “level”) estimators which assume that all family background variables, including family structure, are uncorrelated with family- and child-specific unobservables. But a weak correlation between family structure and child’s education obtained from levels estimators cannot be taken as conclusive evidence that family structure is determined independently of child or family unobservables. By supplementing level estimators with models that make weaker identifying assumptions, and exploring robustness in many 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

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; Page and Stevens 2004; Ruhm 2004):

$$S_{ij} = F_{ij}\mathbf{b} + X_{ij}\mathbf{g} + \mathbf{a}_j + u_{ij}, \quad (1)$$

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 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, \mathbf{a}_j , and a random idiosyncratic component, u_{ij} .

We use four econometric methods.³ First, we estimate “level” regressions based on the assumption that observed determinants of attainment (F_{ij} and X_{ij}) are uncorrelated with unobservable determinants \mathbf{a}_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 \mathbf{a}_j (e.g. ability and motivation) which are partly inherited in the form of genetic and cultural endowments.⁴ 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.

Second, we estimate propensity score matching models. For consistent estimation of \mathbf{b} , this method – as well as the previous one – requires that there be no unobservable differences between children who grew up in a non-intact family (“treatment” group) and children who grew up in an intact family (“control” group) after conditioning on X_{ij} (the “conditional independence” or “selection on observables” assumption). However, compared to the level-type regressions, with their assumptions on linearity and additivity that can only unsatisfactorily be relaxed in full, matching provides a method by which no functional form restrictions on the relation between S_{ij} , F_{ij} , and X_{ij} need be made (Moffitt 2004).

Third, we estimate mother fixed-effect (FE) or 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 \mathbf{b} 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 and propensity score matching models. Mother FE 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 in Section 4.

Fourth, we estimate variants of equation (1) using two quasi-experimental methods, each relying on a different source of exogenous variation in family structure. The first involves individuals whose father died when they were a child. 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

² Most of the evidence discussed above is for the USA. Exceptions include Jonsson and Gähler (1997) and Björklund et al. (2007) for Sweden, Cherlin et al. (1995) and Ermisch and Francesconi (2001) for Britain, Corak (2001) for Canada, and Piketty (2003) for France.

³ In addition, we also computed Manski bounds using a subset of variables in X_{ij} (such as age, sex, and mother’s education) to create subgroups of respondents (Manski 1990). All the point estimates presented in Section 4 fell within the bounds and so, for brevity, the bounds are not shown.

⁴ See Behrman et al. (1994), Rosenzweig and Wolpin (1995), and Ermisch et al. (2004).

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 including 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-FE models, the sample sizes are small, since paternal death during childhood is a relatively rare event in contemporary Germany.

The second quasi-experimental method involves individuals whose childhood spanned the mid-1970s, the period when changes to West German divorce law eliminated “fault” grounds for divorce. We exploit 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. After World War II, the two Germanies 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).

To implement this second method, we model school outcomes using a before-after design, with the treatment effect characterized by the coefficient on the interaction between an indicator for having experienced parental divorce in the FRG and time. The variants of equation (1) that we estimate take the form:

$$S_{it} = \mathbf{d}_0 + \mathbf{d}_1 d_{1i} + \mathbf{d}_2 d_{2i} + (\mathbf{d}_3 + \mathbf{d}_4 W_{it}) \mathbf{I}_t + \mathbf{b}_1 T_{1it} + \mathbf{b}_2 T_{2it} + \mathbf{Z}_{it} \mathbf{g} + \mathbf{e}_{it}, \quad (2)$$

where i indexes children and t indexes survey years. The term d_{1i} is a dummy variable equal to one for individuals who ever lived with a divorced mother in the FRG during childhood and zero otherwise; d_{2i} is the corresponding variable for the GDR; W_{it} is equal to one if i lived in the FRG at time t , and zero otherwise; \mathbf{I}_t is a full set of year dummies; T_{1it} is a dummy variable equal to one if parental divorce for child i in the FRG occurred during the post-reform period and zero otherwise; T_{2it} is the corresponding variable for the GDR; vector \mathbf{Z}_{it}

contains child/family characteristics (potentially different from those included earlier in X_{it} in equation (2)); and e_{it} is an i.i.d. disturbance term.⁵ The parameter of prime interest is \mathbf{b}_1 which measures the effect on S_{it} of parental divorce under the post-1976 unilateral divorce regime for individuals from the FRG sample (i.e. the difference-in-differences between individuals with divorced and married parents). The key assumption required for the identification of this treatment effect is that child endowments (subsumed in e_{it}) do not depend on the specific divorce law in force.

It is worth emphasizing that the economic and statistical assumptions required to justify the use of each of the previous models are different. One set of assumptions neither implies nor is implied by the other. In general, however, level and matching models assume that conditioning on observables eliminates selection bias. A benefit of this strong assumption is weaker assumptions about other features of the underlying economic model: matching (but not level) models do not require exogeneity of regressors or exclusion restrictions, provided valid conditioning sets are known (Heckman and Vytlacil 2004). On the other hand, sibling difference and quasi experimental methods model selection bias directly. But the estimates from each of these methods are likely to identify different local average treatment effects, in the sense that they may be valid for different individuals located in different points of the distribution of e_{it} . We do not intend to impose strong priors as to what method should be preferred but, as part of our robustness checking exercise, we wish to explore whether estimates differ substantially across methods.

3. Data

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

Our data come from the German Socio-Economic Panel Study (SOEP), combining information from the first twenty-two annual interview waves (1984–2005) and the retrospective lifetime employment, marital and fertility histories (which span the pre-panel period for most respondents).⁶ 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 FRG. In June 1990, the SOEP was expanded to the territory of the former GDR, including nearly 2,200 new households.

Our analysis is based on three distinct samples. The first consists of individuals who belonged to households that were part of the original SOEP West German sample ('West

⁵ Further details of the empirical specification of equation (2) are provided in Section 4.

German sample'). The second sample includes individuals who belonged to households that were part of the original SOEP West German Guestworker sample ('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 GDR before 1990 and whose head was a GDR citizen ('East German sample'). Panel data are available for the West German and Guestworker samples from 1984 and for the East German sample from 1990 onwards.⁷

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 2005; (c) were not disabled; and (d) had mothers who provided complete family and employment histories over the first ten years of the child's life.⁸

Condition (a) was imposed to avoid overrepresentation in the sample of individuals who left their parents' home at a relatively late age. Although, in principle, the condition may lead to sample selection bias if educational outcomes and co-residence with one's mother share unobserved determinants, we believe the problem is not serious. By age 18, only six 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, e.g. her age, education, and income sources. Condition (c) reduces problems arising if parents choose family structure patterns (and other behaviour, such as employment) on the basis of their child's health. Mother fixed-effects models identify the parameter of interest by assuming that there are no intra-family responses and this would be harder to justify if disabled children were 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 reason for stopping family structure and maternal employment histories at age 10 of the child is related to institutional conditions concerning school track choice (see

⁶ The SOEP is documented at <http://www.diw.de/english/sop/service/index.html>.

⁷ 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.

below). This guarantees that family disruptions are always measured *before* the child outcome of interest is determined.

The sample selection criteria resulted in a sample of approximately 1,500 individuals for the West German sample, 800 for the Guestworker sample, and 700 for the East German sample. Sample sizes are smaller than those derived from administrative sources (e.g. Corak 2001; Piketty 2003; Björklund et al. 2007) but compare favourably to earlier studies that use SOEP data (e.g. Jenkins and Schuter 2002; Mahler and Winkelmann 2004) or survey data from other countries (e.g. Painter and Levine 2000; Ermisch and Francesconi 2001; Ginther and Pollak 2004).

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

Before introducing our measures of schooling outcomes, we need to explain the structure of the German school system.⁹ 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. Around the age of 10, pupils are channelled into three main types of secondary school: general school (*Hauptschule*), intermediate school (*Realschule*), and grammar 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 vocational training 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 usually followed by attendance at a further education college combined with an apprenticeship or, rarely, a move to a *Gymnasium*. *Gymnasium* is the most academic and prestigious school 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.¹⁰ Since education is a responsibility of each federal state, 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

⁸ Father-only families were excluded from the sample: only 75 children (or 2 percent of individuals in our final sample) were dropped.

⁹ See Dustmann (2004) for further details.

¹⁰ This discussion refers to West Germany. After reunification, East Germany adopted the educational system of West Germany (Jeschek 2000). 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 maternal education, but this does not drive any of the differences in results for the East German and West German samples. They are irrelevant for the estimation of mother FE models.

school track, and are not large. State dummies are included in almost all regressions, in any case.

We use three 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 the SOEP. Almost 36 percent of the West German sample and 35 percent of the East German sample have qualifications to *Abitur* or higher, but only 21 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 39 percent of the former group had *Abitur* or higher qualifications but only 25 percent of the latter group. The differentials are even higher for the Guestworker sample (22 percent compared with 11 percent) and the East German sample (39 percent and 22 percent).

The other outcomes analyzed are the secondary school track followed at age 14, and secondary school test scores in Mathematics, German, and first foreign language.¹¹ We examine the probability of *Gymnasium* attendance because *Gymnasium* 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 (e.g. 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 because we require valid information about school track attended at age 14.¹² In the West German sample, 40 percent of individuals from an intact family had attended *Gymnasium*, whereas only 26 percent of individuals from a non-intact family had. For the East German sample, there was a slightly smaller differential, and no differential for the Guestworker sample.

The SOEP Youth Questionnaire (first administered in 2000) and the 'BIOSOC' supplement to the main questionnaire contain information about the scores obtained in

¹¹ We also analyzed the probability that a child repeated a grade (i.e. whether he/she was ever held back in school) during primary school years. The results of this analysis were similar to those found for test scores and are therefore not reported.

¹² Information on secondary school track at 14 was obtained from 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*).

secondary school for Mathematics, German, and the first foreign language. The data refer to the final year at school, and so scores are measured at different ages depending on school track. Assessments are on a six-point scale on which a score of 1 represents the highest mark and a score of 6 is the lowest mark. The outcomes modelled are the probability of achieving a high score (1 or 2), and the probability of achieving a low score (5 or 6). Due to small sample sizes, analysis of these outcomes had to be restricted to individuals in the West German sample ($N = 911$). The differences in scores between children from intact families and non-intact families are not statistically significant for German and Mathematics, whether we look at the top or the bottom of the distribution. For the first foreign language, children from non-intact families are significantly less likely to have a high score and more likely to have a low score.

D. Measures of Family Structure During Childhood

We use five different family structure measures, each of which was constructed from the mother's marital histories. The first measure takes the value zero if the individual lived continuously with both biological (or adoptive) parents up to his/her tenth birthday, and one otherwise.¹³ Thus, an individual 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 tenth birthday either because of a partnership dissolution (through divorce or father's death) or because the person was born outside of marriage and the mother did not subsequently marry the biological father.¹⁴ A number of earlier studies have reported different impacts of the experience of a non-intact family depending on how old the child was when the dissolution occurred (Wojtkiewicz 1993). Our second measure therefore breaks down the first measure into two, each corresponding to a childhood stage: early childhood (birth to age 5), and middle childhood (ages 6–10).¹⁵

Our third measure distinguishes between individuals whose mother was unmarried at their birth from individuals who ever lived with a separated/divorced mother and individuals

¹³ Ginther and Pollak (2004) distinguished children reared in "blended" families – stepchildren and their half-siblings who are the biological children of both parents – from children reared in traditional intact families as well as from children reared in other family structures. Because of small sample sizes, however, our measures cannot make this distinction.

¹⁴ 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 (less than 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. Similarly, divorces before 1983 refer to breakdowns of legal marriages only, whereas during the panel years they cover both legal marriage and cohabitation disruptions.

who experienced the death of their father during childhood.¹⁶ This measure is used in our first quasi-experiment: the experience of individuals who experienced the death of their father during childhood provides a benchmark from which to judge the endogeneity of divorce.

The fourth and fifth measures focus on the duration of non-intactness rather than simply its occurrence (as in the first three measures): the proportion of childhood years (ages 0–10) that an individual 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. Slightly less than one in six individuals in the West German sample experienced life in a non-intact family during childhood, which is about 40 percent fewer than in the East German sample and more than 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 route into non-intactness in the West German sample but, in the Guestworker and East German samples, unmarried motherhood was more prevalent. About 70 percent of family disruptions in the West German sample and 85 percent in the East German sample occurred between ages 0–5, mainly because of the substantial fraction of unmarried mothers. The proportions of years spent in a non-intact family shown in Table 2 were computed using the whole sample (i.e. including those who always lived with both biological parents). On average, individuals 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 individuals who lived in a non-intact family, the proportions become 39, 35 and 42 percent.

E. Additional Control Variables

We use an extensive set of control variables corresponding to those that have been used in previous research: 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 federal state of residence. We also include controls for the individual's mother's characteristics: her age when the individual was born, highest educational attainment, the number of years worked part time and full time during the individual's first ten years of life, and childhood

¹⁵ In earlier work (Francesconi, Jenkins, and Siedler 2005), we defined childhood as spanning years 0–16 rather than 0–10 as here. Changing the definition did not change any of our substantive conclusions.

¹⁶ 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 estimates 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. But the results are qualitatively similar to those shown below.

family income (post-tax post-transfer household income averaged over all years for which income information was available between the ages of 0 and 16).

Descriptive statistics for the control variables are provided in Table 3. There are roughly 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 children, the most common maternal education level is the lowest one, and only 5–7 percent have mothers with university degrees. In contrast, among East German children, about 45 percent have mothers with intermediate school qualifications and 24 percent with university degrees.¹⁷ East German mothers also have the strongest labour market attachment, with 8 years of full-time experience and nearly two years of part-time experience, as opposed to two and three years respectively among West German mothers and four and one years respectively among Guestworker mothers. Average childhood family income was greatest for the West German sample, around €35,000 per year, which was about 18 percent and 12 percent greater than for individuals in the Guestworker and East German samples.

4. 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. (For brevity, the estimates for the other explanatory variables are not listed.) The first three columns show estimates from three level logit regressions as progressively more control variables were included. The next column reports estimates from propensity score models which use biweight kernel matching on the full set of controls as in specification [3] of the logit model, and the last column presents mother fixed-effects estimates obtained from linear probability models.¹⁸ To ease interpretation, all estimates are expressed as marginal effects.

¹⁷ The maternal education variable has four categories, in ascending order: general secondary school qualification or less, intermediate school qualification, *Abitur*, technical college or university degree. To simplify cross-sample comparisons, we used the same broad categories for each sample, 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, i.e. 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.

¹⁸ We also performed this analysis using local linear regression matching models and Chamberlain conditional logit models. Because results obtained were similar to those shown in Table 4, they are not reported.

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 estimate ($\mathbf{b} = -0.148$) is obtained from level regression specification [1]. 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. Hence non-intactness has a large effect through parental income and employment. After controlling for these two mechanisms in specification [3], we find that having experienced life in a non-intact family is still associated with a reduction of the chances of achieving *Abitur* or higher qualifications by 8 percentage points. Although this is a substantial impact, it is almost half of the effect measured in specification [1] and it is statistically significant only at the 10 percent level. Of course, a causal interpretation of the estimate is questionable, because the selection-on-observables assumption is hard to justify. The propensity score matching estimate, which also relies on such an assumption, shows a reduction too but of only 4 percentage points and this is insignificant at any conventional level.

As argued in Section 2, the mother FE estimator relies on weaker assumptions for identification. The point estimate from this model is again negative and of the same magnitude as the level estimate from specification [1] ($\mathbf{b} = -0.155$), but it is not statistically significant at conventional levels (p -value = 0.078). That statistical significance is smaller for sibling-difference estimates than for corresponding level estimates has also been reported in other related studies (e.g. Björklund et al. 2007). Small sample size may be a concern here (although it cannot be the reason for the lack of significance in the Björklund and colleagues' study for Sweden, since they use large samples drawn from population registers). We return to this issue in the robustness checks below.

Arguably an explanation for the large standard errors is that they arise from differential and offsetting effects associated with different types of non-intactness. Panel B shows our results of checking this. The difference between the estimates on 'Parents divorced' and on 'Father died' can also be given a causal interpretation assuming the father's death provides exogenous variation in parental loss. Level estimates indicate a significantly lower probability of achieving *Abitur* or higher qualification for children of divorced mothers, even after controlling for family income and maternal employment (specification [3]), by 12 percentage points. Although the matching estimator estimate is of comparable magnitude and is statistically significant, the FE model reveals that having experienced a

parental divorce reduces the chances of achieving *Abitur* or higher qualifications by almost 19 percentage points, but this effect is not statistically significant at conventional levels (p -value = 0.107). Similar considerations hold for individuals who were born to unmarried mothers. Interestingly, it is divorce (and not paternal loss through death) that leads to lower schooling attainment: the difference between the estimate on ‘Parents divorced’ and that on ‘Father died’ is large but never significantly different from zero. This is in line with the results reported by Corak (2001) and Lang and Zagorsky (2001), although their analyses covered differently-defined outcomes.

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. In all cases, the effects in middle childhood are more adverse than those measured in early childhood. But, with the exception of the level logit results from specification [1], the estimates are always imprecisely estimated. As a result, we cannot reject the hypothesis that the estimated effects in the two childhood stages do not differ from each other.

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. There are no statistically significant estimates when duration is broken down by family type (panel E). The same conclusions can be drawn from the FE estimates.

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 (especially in the case of divorce) is suggested by estimates that rely on selection-on-observable type assumption but, once correlated unobserved background characteristics are accounted for, the effect becomes imprecisely estimated.

B. Guestworker Sample and East German Sample

We repeated the whole analysis for the Guestworker and East German samples but, for brevity, Table 5 we only reports estimates for two measures of family structure.

Despite the differences in family structure and educational attainment between Guestworkers and West Germans, the results shown in Table 5 for young adults in the

Guestworker sample are qualitatively similar to those found for individuals in the West German sample in Table 4. Growing up in a non-intact family reduces the probability of achieving *Abitur* or higher qualifications, but this effect is never statistically significant and becomes negligible when fixed unobserved factors shared by siblings are accounted for.¹⁹ The three models tell different stories about which type of family structure leads to worst schooling outcomes, with the level estimates pointing at being born to an unmarried mother, the matching estimator results pointing at divorce, and the mother FE estimates at father's absence through death. Besides this disagreement, none of the estimates is statistically significant. So too is the difference between the estimate on 'Parents divorced' and that on 'Father died', implying that paternal loss (whether by divorce or father's death) does not lead to significantly lower chances of achieving *Abitur* or higher qualifications.

The picture for East German sample members is rather different. Irrespective of the estimation method, the estimates on 'Ever lived in a non-intact family' are always negative and significant. The mother FE estimate indicates a reduction in the probability of *Abitur* or greater qualifications of about 26 percentage points, and it is twice as large as the estimates found with matching and level logit regressions after controlling for family income and maternal employment. The association between childhood family disruption and lower schooling attainment of East German youth, therefore, may not be driven by a disproportionate selection of low achievers in non-intact households; it appears to be causal. Most of this impact is linked to an early family disruption, when children were aged 0–5 (not shown). There is, however, disagreement across methods as to which type of family disruption drives this relationship. According to the level logit and matching models, the largest negative association is estimated for children of unmarried mothers, while with the FE model, the largest effect is found for children of divorced parents. But we cannot confirm that divorce has a detrimental impact on schooling when father's loss by death is used as an exogenous change in childhood family structure: the difference between the estimates on 'Parents divorced' and 'Father died' is generally small and always insignificant.

Despite these ambiguous results, the earlier estimates clearly point to an adverse effect of family non-intactness. East German young adults who grew up in a non-intact family might suffer a non-negligible handicap in their schooling careers, having to face substantially lower chances of achieving *Abitur* or higher qualifications than their

¹⁹ As a robustness check, we reestimated the model for this 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.

counterparts who lived with both biological parents during childhood. Family non-intactness is a more common experience for East German youth than for the other children in our data. And it might add to a number of other disadvantages that seem to characterise the environment in which East German children grew up, such as greater risks of living with unemployed parents (Siedler 2006), and in poor housing and high crime neighbourhoods (ZUMA 2005).

C. Other Schooling Outcomes

Family structure effects for West German and Guestworker sample members might be statistically insignificant because educational qualifications are measured many years after the family disruption. 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 inferior outcomes will be concentrated at (early) stages of life closer to the time of family breakdown. For this reason, we consider other schooling outcomes which are observed at younger ages, such as *Gymnasium* attendance at age 14 and secondary school scores.

The estimated effects of 'Ever lived in a non-intact family' 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 FE estimates. Models for secondary school scores could be estimated only 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 FE estimate reveals that experience of life in a non-intact family does not affect the probability of attending *Gymnasium* at age 14. This emerges also from the matching regression and the model that uses father's death as the instrument (not shown). The level logit estimate instead implies an eight percentage point reduction, but this is significant only at the 10 percent level. Exactly the same qualitative results are found for children in the Guestworker and East German samples, with the estimated effects being always small and insignificant.

From these results, therefore, we cannot find any evidence that the negative effect of family non-intactness on the probability of reaching *Abitur* or higher qualifications for East German children is preceded by a lower probability of being in the *Gymnasium* track at age 14. One reason for this lack of effect is that young East Germans who attended *Hauptschule* or *Realschule* and obtained *Abitur*-level qualifications were disproportionately from non-

intact families. We explored this possibility but found no evidence in its support. Another explanation is that, in contrast with the hypothesis of a gradual psychological adjustment, family non-intactness requires time before its effects on individuals' educational outcomes become apparent. While *Gymnasium* enrolment might be relatively unaffected, success in the *Gymnasium* track (that is, acquisition of the *Abitur* degree) could be seriously hampered by experience of life in a disrupted family. This interpretation ties in well with the point that it is the long-term influence of family background that best explains the observed relationship between early parental behaviour and later child outcomes (Heckman and Cameron 2001; Cunha et al. 2005).

D. Difference-in-Difference Estimates from 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. (Estimation with them included in the treatment group 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 defined the post-reform period as 1977 and afterwards, and the pre-reform period as the years up to and including 1976, since the June 1976 family law reform was implemented only in June 1977 and because dates for divorces that occurred before the start of the SOEP (1984) are recorded in years.²¹ The vector Z_{it} 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 and time dummies identifying either when the divorce occurred (if the divorce occurred after 1984) or at the first wave the mother is observed (if the divorce occurred before 1984), plus a full set of year-region interactions.²²

Linear probability estimates for five different subsamples are summarised in Table 7. The five subsamples account in different ways for the fact that, since 1990, there has been one uniform family law for the whole of Germany, and hence \mathbf{b}_1 is identified only through

²⁰ Before 1990 migration between the former GDR and FRG was virtually inexistent. Since then migration is allowed but there is one uniform legal family code applied to the whole of Germany. Hence our results are unlikely 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. four 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.

variation over time in divorce law rather than variation over time and across states. In addition, two specifications are shown corresponding to whether or not state-specific (FRG and GDR) time trends were excluded, i.e. whether \mathbf{d}_3 and \mathbf{d}_4 and the year-region interactions in Z_{it} were set to zero or not. With the introduction of state-specific time trends we try to control for the possibility that changes in the divorce law could be correlated with other time-varying state-specific characteristics that affect child outcomes.

Regardless of specification and subsample, there was no statistically 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 \mathbf{b}_1 in the equation for *Gymnasium* attendance at age 14 are negative and range between 3 and 8 percentage points in absolute terms. They are somewhat lower than the level logit estimate for the West German sample shown in Table 6, and are always 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 adverse effect of family non-intactness on schooling outcomes.

E. Additional Robustness Checks

We made a number of additional robustness checks.²³ First, because mother fixed-effects models can only be estimated using data from families with at least two siblings and these families could be a nonrandom subgroup of the population of families with children, we re-estimated the level logit and propensity score matching regressions for the probability of achieving *Abitur* or more using data for two subsamples. The first subsample included individuals with siblings (i.e. only-children were excluded) and the second 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 for the FE regressions). In the absence of any bias, we expect the results based on these two new subsamples to be comparable to the level estimates in Tables 4 and 5. Indeed, the point estimates from the two new subsamples were quite similar to our previous results.²⁴

²² The different timing of the region variables is because the SOEP does not ask respondents about housing and residential location prior to their joining the panel.

²³ For brevity, the estimates are not shown, but are available from the authors.

²⁴ The smaller size of these subsamples reduced the precision of some of such estimates, however.

Second, arguably the non-significance of our fixed-effects regressions may have arisen because they were based on small samples, especially in the case of the East German and Guestworker samples. To investigate this, we combined the three original samples into one. With this new sample, we re-estimated the probability of having *Abitur* or higher qualifications using level and fixed-effects regressions, while also including sample dummies and interactions between family structure variables and sample dummies. As before, the propensity score matching and FE estimates indicated a significant negative association between measures of family non-intactness and attainment probabilities only for the East German sample members. For the other two groups of children, again, the estimates provided scant evidence that childhood family structure significantly affects children's schooling outcomes.

Third, because the SOEP does not collect a full history of housing tenure and residential mobility before the panel began, we cannot fully control for geographic location during childhood years for a large number of individuals 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 may be affected is undetected (Pribesh and Downey 1999). The only reliable information that we can use is the number of years individuals have lived in their current address during childhood. In level and matching regressions for the probability of achieving *Abitur* or higher qualifications, adding this variable (expressed as a proportion of childhood years) to the set of regressors did not alter the results shown in Tables 4 and 5, while this variable was never statistically significant. In particular, the negative effect of family non-intactness for East German sample members was still highly significant and did not seem to operate through more unstable residential patterns of non-intact households.

Fourth, one might be concerned 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 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 individuals in the West German and Guestworker samples in more recent years. Alternatively, if divorce has become more prevalent and this is associated with an average reduction in schooling attainment, we may expect to observe stronger negative effects among West German and Guestworker sample members. To address this issue, we re-estimated our level, matching and FE regression models for the probability of attaining *Abitur* or more

using individuals from the West German and Guestworker samples from 1990 onwards, i.e. the same time period covered by the East German sample. This strategy did not change any of our findings.

5. Summary and Conclusions

Does experience of life in a non-intact family during childhood affect schooling outcomes in Germany? The answer is not straightforward. For West German and Guestworker children, the answer is yes according to level regression models. Most (albeit not all) of this association operates through lower family income and lower maternal labour market attachment. But when the endogeneity of family structure is accounted for, using a variety of estimators, there is little evidence that growing up in a non-intact family significantly affects schooling outcomes. These results hold true regardless of how old the child was when the family disruption occurred, regardless of the type of non-intactness, and regardless of which schooling outcome measure is considered, whether the probability of having *Abitur* or higher qualifications, of being at the top or the bottom of the distributions of secondary school scores, or of repeating a grade during primary school.

The answer for East German children differs. With the exception of the estimator that uses father's death as an exogenous change in childhood family structure, all our estimators lead to the conclusion that growing up in a non-intact family does reduce the chances of achieving *Abitur* or higher qualifications. This result is robust to a number of model specifications and data selections. We find no evidence, however, that this impact is anticipated by a lower probability of being in the *Gymnasium* track at age 14. One explanation for this result is that family non-intactness may require time before its effects on individuals' educational outcomes become apparent. This explanation ties in well with the established notion that family background has long-term influences on child outcomes which, in our context, will require further analysis.

Further analysis should also be devoted to the reasons why East German children's schooling attainment is more vulnerable to family disruptions than that of West German children or children of immigrants. Individuals who grew up in a family from the former East Germany headed by a citizen of the former German Democratic Republic were more likely to experience life in a non-intact family and were also more likely to face other forms of an adverse environment after re-unification (such as greater risks of local unemployment and higher crime rates). The extent to which such disadvantages compounded the effect of family non-intactness or were driven by it is an issue that goes beyond the scope of this study, but its

understanding is of clear policy salience. Finally, additional research is also to explore whether there are causal pathways linking childhood family structure to other later-life outcomes, such as earnings, unemployment, and health.

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Table 1
Means of the outcome variables by sample and childhood family structure

Outcome	West German sample		Guestworker sample		East German sample	
	Non-intact family	Intact family	Non-intact family	Intact family	Non-intact family	Intact family
<i>Abitur</i> or higher qualification	0.249	0.385	0.111	0.220	0.217	0.394
<i>N</i>	241	1,263	54	709	180	487
<i>Gymnasium</i> attendance at age 14	0.256	0.400	0.145	0.163	0.337	0.453
<i>N</i>	297	1,476	83	845	279	602
Secondary school scores						
Proportion with high scores						
German	0.224	0.290				
Mathematics	0.328	0.322				
First foreign language	0.230	0.303				
Proportion with low scores						
German	0.034	0.027				
Mathematics	0.057	0.061				
First foreign language	0.080	0.054				
<i>N</i>	174	737				

Note: *N* is the number of individuals.

Table 2
Childhood family structure by sample

Measure of childhood family structure	West German sample	Guestworker sample	East German sample
Ever lived in a non-intact family	0.160	0.071	0.270
Born to unmarried mother	0.056	0.035	0.162
Parents divorced	0.080	0.028	0.102
Father died	0.024	0.008	0.006
Ever lived in a non-intact family at ages:			
0–5	0.110	0.052	0.229
6–10	0.050	0.019	0.041
Proportion of childhood years lived in a non-intact family	0.056 (0.179)	0.025 (0.124)	0.110 (0.249)
Proportion of childhood years lived with unmarried mother	0.031 (0.151)	0.017 (0.108)	0.079 (0.226)
Proportion of childhood years lived with divorced mother	0.020 (0.096)	0.007 (0.063)	0.031 (0.127)
Proportion of childhood years lived with widowed mother	0.005 (0.045)	0.001 (0.009)	0.001 (0.008)
<i>N</i>	1,504	763	667

Note: *N* is the number of individuals. Standard deviations are in parentheses. Childhood refers to ages 0–10.

Table 3
Summary statistics by sample

Variable	West German sample	Guestworker sample	East German sample
Age	26.20 (5.81)	25.60 (5.30)	23.40 (3.27)
Age < 22	0.269	0.262	0.321
Age 22–25	0.275	0.334	0.405
Age > 25	0.457	0.404	0.274
Year of birth	1974.64	1974.02	1980.03
Female	0.497	0.474	0.502
Mother's highest educational attainment			
Secondary general school certificate or less	0.633	0.887	0.136
Intermediate school qualification	0.264	0.021	0.445
<i>Abitur</i> (or equivalent)	0.030	0.005	0.178
Technical college or university degree	0.067	0.045	0.235
Mother's age at birth	27.03 (5.50)	26.36 (6.01)	24.43 (4.39)
Only child	0.123	0.047	0.157
Number of brothers ^a	0.818	1.231	0.658
Number of sisters ^a	0.791	1.184	0.591
Birth order ^{a,b}			
First child	0.384	0.315	0.477
Second child	0.393	0.316	0.422
Third child or more	0.223	0.369	0.101
Average post-government household income during childhood years ^c	34,504 (15,844)	29,200 (8,796)	30,853 (10,534)
Mother's employment during childhood years:			
Number of years full-time employed	1.961 (3.272)	4.013 (4.393)	8.166 (3.564)
Number of years part-time employed	2.538 (3.501)	1.201 (2.634)	1.715 (2.879)
<i>N</i>	1,504	763	667

Notes: Figures are sample means with standard deviations in parentheses.

^a Includes adopted and foster children.

^b Computed for children with siblings only.

^c Average computed using all positive household annual income values available for years 0–16. Household income was deflated using the Consumer Price Index and is expressed in Euros (year 2000 prices).

Table 4
Childhood family structure and the probability of achieving *Abitur* or higher qualification:
West German sample

Measure of childhood family structure	Logit ^a			Matching ^b	Mother FE ^c
	[1]	[2]	[3]		
<i>Panel A</i>					
Ever lived in a non-intact family	-0.148 (0.045)	-0.105 (0.047)	-0.079 (0.047)	-0.037 (0.043)	-0.155 (0.086)
<i>Panel B</i>					
Born to unmarried mother	-0.150 (0.063)	-0.117 (0.065)	-0.088 (0.067)	-0.051 (0.053)	-0.185 (0.119)
Parents divorced	-0.203 (0.059)	-0.148 (0.060)	-0.124 (0.059)	-0.122 (0.060)	-0.187 (0.114)
Father died	0.004 (0.126)	0.041 (0.132)	0.061 (0.134)	-0.007 (0.070)	0.297 (0.275)
<i>Panel C</i>					
Ever lived in a non-intact family at ages:					
0–5	-0.136 (0.053)	-0.100 (0.054)	-0.070 (0.055)	-0.031 (0.045)	-0.151 (0.088)
6–10	-0.174 (0.069)	-0.116 (0.072)	-0.098 (0.071)	-0.105 (0.056)	-0.170 (0.117)
<i>Panel D</i>					
Proportion of childhood years lived in a non-intact family	-0.170 (0.083)	-0.120 (0.086)	-0.070 (0.087)		-0.070 (0.181)
<i>Panel E</i>					
Proportion of childhood years lived with unmarried mother	-0.112 (0.095)	-0.071 (0.099)	-0.019 (0.102)		-0.437 (0.293)
Proportion of childhood years lived with divorced mother	-0.439 (0.162)	-0.347 (0.161)	-0.289 (0.150)		0.098 (0.241)
Proportion of childhood years lived with widowed mother	0.034 (0.268)	0.093 (0.296)	0.121 (0.292)		0.407 (0.593)

Notes: Standard errors in parentheses. Childhood years refer to ages 0–10. Estimators defined in the main text.

^a 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 average household 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.

^b Figures are average treatment effects of the treated from biweight kernel regressions. All variables as in specification [3] of the level estimates are used to estimate the propensity score matching models.

^c Figures are marginal effects from linear probability models computed at average values of all the variables used. FE = fixed effects. Other regressors used were 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 higher qualification:
Guestworker and East German samples

Measure of childhood family structure	Logit			Matching	Mother FE
	[1]	[2]	[3]		
<i>Guestworker sample</i>					
Ever lived in a non-intact family	-0.115 (0.068)	-0.110 (0.068)	-0.085 (0.062)	-0.082 (0.057)	-0.017 (0.096)
Born to unmarried mother	-0.132 (0.113)	-0.127 (0.112)	-0.124 (0.110)	-0.045 (0.085)	0.032 (0.137)
Parents divorced	-0.114 (0.103)	-0.111 (0.099)	-0.060 (0.083)	-0.143 (0.098)	-0.057 (0.141)
Father died ^a	-0.074 (0.160)	-0.063 (0.161)	-0.046 (0.141)	-0.042 (0.240)	-0.110 (0.332)
<i>East German sample</i>					
Ever lived in a non-intact family	-0.167 (0.050)	-0.143 (0.051)	-0.129 (0.051)	-0.115 (0.048)	-0.262 (0.127)
Born to unmarried mother	-0.184 (0.061)	-0.162 (0.062)	-0.150 (0.063)	-0.149 (0.061)	-0.260 (0.153)
Parents divorced	-0.145 (0.078)	-0.120 (0.079)	-0.100 (0.080)	-0.123 (0.065)	-0.324 (0.201)
Father died	-0.081 (0.147)	-0.046 (0.143)	-0.087 (0.148)	-0.150 (0.224)	0.037 (0.387)

Notes: Standard errors in parentheses. For definitions and comments, see notes to Table 4.

Table 6**The impact of ‘ever lived in a non-intact family’ on additional educational outcomes, by sample**

Outcome measure	West German sample		Guestworker sample		East German sample	
	Logit	Mother FE	Logit	Mother FE	Logit	Mother FE
<i>Gymnasium</i> attendance at age 14	-0.080 (0.043)	0.010 (0.060)	-0.074 (0.045)	-0.014 (0.086)	-0.035 (0.045)	-0.018 (0.081)
High scores ^a						
German	-0.022 (0.045)					
Mathematics	0.060 (0.043)					
First foreign language	-0.035 (0.045)					
Low scores ^a						
German	0.006 (0.007)					
Mathematics	-0.006 (0.017)					
First foreign language	0.002					

Note: The control variables are as in Table 4 with the exception of age dummies.

^a Each regression also controls for school track attended.

Table 7
Difference-in-difference estimates

	<i>Abitur</i> or higher qualification	<i>Gymnasium</i> attendance at age 14
<i>Panel A (all sample)</i>		
No time trends	0.051 (0.075)	-0.067 (0.072)
With time trends	0.067 (0.077)	-0.082 (0.072)
<i>N</i>	2,043	2,326
<i>Panel B (divorces in 1990–1992 in East Germany dropped)</i>		
No time trends	0.051 (0.075)	-0.067 (0.072)
With time trends	0.068 (0.077)	-0.083 (0.071)
<i>N</i>	2,041	2,321
<i>Panel C (all divorces in 1990–1992 dropped)</i>		
No time trends	0.061 (0.066)	-0.029 (0.073)
With time trends	0.078 (0.078)	-0.041 (0.073)
<i>N</i>	2,031	2,299
<i>Panel D (divorces after 1990 in East Germany dropped)</i>		
No time trends	0.061 (0.076)	-0.028 (0.073)
With time trends	0.078 (0.078)	-0.040 (0.073)
<i>N</i>	2,028	2,278
<i>Panel E (all divorces after 1990 dropped)</i>		
No time trends	0.056 (0.078)	-0.030 (0.079)
With time trends	0.073 (0.080)	-0.029 (0.081)
<i>N</i>	2,020	2,230

Notes: Table shows estimates of b_1 (equation (2)) obtained from separate linear probability model regressions.

Standard errors allowing for arbitrary serial correlation are shown 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.

When *Abitur* or higher qualification was the dependent variable, the regressors also included child's age.

The East and West German samples each contain children with a German-born mother only.