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IZA DP No. 604

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October 2002

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

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IZA Discussion Paper No. 604
October 2002

ABSTRACT

The Effect of Family Income During Childhood on Later-Life Attainment: Evidence from Germany*

We examine the impact of family income during childhood on the type of secondary school that German children attend, a good indicator of their lifetime socioeconomic attainment. By contrast with several US child outcome studies, we find that late-childhood income is a more important determinant of outcomes than early-childhood income, and income effects are not greater for poor households compared to rich households, other things equal. The income effects are small for native-born German children and non-existent for children from guestworker households. Income effects are also small relative to the impact of differences in parental educational qualifications or institutional factors related to the federal state of residence.

JEL Classification: I21, I32, J13, D31

Keywords: child poverty, educational achievement, schooling, family background

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* This paper builds on research originally funded by the Anglo-German Foundation. We also benefited from ISER's core funding from the UK Economic and Social Research Council and the University of Essex, and CASE's core funding from the ESRC. We thank colleagues (especially Marco Francesconi), audiences at seminars, and the ESPE-2002 and SOEP-2002 conferences for their helpful comments and suggestions.

1. Introduction and motivation

Child poverty has become a major policy issue in a number of countries. The relationships between family income during childhood, parental background more generally, and outcomes later in life have therefore come under increased scrutiny. In this paper we examine the type of secondary school that German children attend – a good indicator of their subsequent lifetime socio-economic achievement. We provide new evidence about the effects of differences in childhood family income while controlling for other potential effects, also considering whether income effects vary according to childhood stage, and whether they are non-linear.

There are two main competing hypotheses about the role of income in the intergenerational transmission mechanism – the investment theory and the good parent theory (Mayer, 1997). The former holds that income has a direct effect on outcomes; the second maintains that is has an indirect effect. The investment theory emphasises that parents invest time and money in their children, where those investments may be in education, health, or a good home environment (not only housing quality and neighbourhood, but also items such as books and educational toys). See, for example, Becker (1981), Becker and Tomes (1986), and more recently Acemoglou and Pischke (2001) or Shea (2000), in whose models higher incomes ease credit constraints in the financing of a child's education. Intrahousehold aspects are surveyed by Behrman (1997).

The good parent theory says that low income induces greater parental stress, and thence poor parenting. It is stress rather than low income per se which harms children's social and emotional development and, in turn, harms future success. Another version of the good parent theory, the role model theory, holds that 'low income parents develop values, norms, and behaviours that are "dysfunctional" for success in the dominant culture' (Mayer, 2002, p. 14). The driving factor is dysfunction rather income itself.

The timing of low-income spells or other success-inhibiting factors during childhood may matter, according to both theories. If there are periods early in childhood that are crucial for child development, then a lack of resources at that time may have a cumulative effect on later progress. On the other hand, resources to promote success during adolescence may be more expensive than those for young children. Credit constraints are likely to be more relevant for secondary and tertiary education choices than primary school ones.

Discriminating between the investment and good parent theories is difficult given the character of most data sets, ours included. Our contribution is of a different nature. We aim to shed light on how far existing evidence about the effects of income on child outcomes are generalisable, rather than country- or institution-specific.

Most analyses of intergenerational transmission, income, and educational outcomes have used US data, and modelled cognitive test scores, high school graduation, or years of schooling: see the studies and surveys of, inter alia, Blau (1999), Duncan et al. (1997), Duncan et al. (1998), Haveman and Wolfe (1995), and Mayer (1997). The main findings about income effects may be summarised as follows:

1. 'Permanent' income has a greater effect on outcomes than 'current income' (the effect of income measured as an average over all (or parts) of childhood is greater than the effect of income measured at the same time as the outcome);
2. Conditional income effects are much smaller than unconditional income effects (the effect of income becomes much smaller when additional explanatory variables are added into child outcome regression models);
3. Conditional income effects are small relative to the effects of a number of other factors associated with differences in outcomes, for example race and parental education;
4. Income effects differ by childhood stage: differences in early-childhood income tend to be greater than the effects of late-childhood income; and
5. Income effects are non-linear: a given change in income has a bigger impact on the outcomes of children from poor families than for children of rich families.

Like all summaries, this one is subject to caveats. Not all the findings hold in the same way for every child outcome. For example, 'family income has much stronger associations with achievement and ability-related outcomes … than with measures of health and behavior' (Duncan et al., 1998, p. 420). And whereas evidence for stage-specific income effects is adduced by leading studies such as Duncan et al. (1998) and Duncan and Brooks-Gunn (1997), Mayer's review is more agnostic: 'Parental income may be more or less important at different ages for child outcomes, but we will need more research to demonstrate this across the full range of child outcomes' (2002, p. 52).

Non-US studies of the links between family income and education-related child outcomes are rare. Ermisch and Francesconi (2001) analysed British young adults' educational attainment, defined in terms of the highest qualification achieved (from no qualifications through to university degree). The family background variables focused on were maternal and paternal educational qualifications, but one of their ordered logit regression models also included household income as an explanatory variable. Income was measured when the child was aged 16 or 17, and so childhood-stage effects were not examined. Non-linear income effects were found: young adults from the poorest fourth of the income distribution had much lower educational attainments than those from higher income groups.

For Germany, Büchel et al. (2001) analysed the probability of attending a *Gymnasium* (the top secondary school track – see below).[1] They found that household income averaged over ages 6–13 had a greater impact on *Gymnasium* attendance probabilities than income at age 13. However, income effects were said to be small, and 'controlling for various non-monetary family characteristics, the chances of poor children being able to attend *Gymnasium* are not significantly lower than those for children living in households with intermediate income' (2001, p. 165).

We examine income effects on German children's educational pathways in much greater detail in this paper. We consider all three secondary school tracks, and our measures of income and other variables cover the whole of childhood (rather than only ages 6–13), so we are able to examine potential childhood-stage effects. Also, by contrast with Büchel et al. (2001), we control for father's and mother's educational qualifications, examine the impact of money income rather than needs-adjusted income, and explore whether income effects differ for native German children and children from households headed by a foreigner ('guestworker').

We argue that, for this German child outcome, it is late-childhood income that matters (not early-childhood income as in several leading US studies), and that income effects are linear rather than non-linear. Moreover the income effects that do exist are small, in absolute terms and relative to the effects of other determinants, and only apply to native German children. They are non-existent for children of guestworker households.

We describe Germany's distinctive three-track secondary school system in Section 2, and argue that the track followed is a good indicator of later life socioeconomic success. Our findings about income effects are derived from regression models estimated using data from the German Socio-Economic Panel. Econometric models and data are discussed in Sections 3 and 4. The empirical results are presented in Section 5, and Section 6 provides a summary and conclusions.

[1] Dustmann's (2001) study of educational pathways in Germany also examined the transition to secondary school, but he did not include income in his models. Büchel and Duncan (1998) modelled the probability of *Gymnasium* attendance at age 14. The focus was on the impact of measures of parental social activities rather than income. A single income regressor was used: household income averaged over as many years between ages 9 and 14 as the data permitted.

2. Secondary school pathways in Germany, and their importance for later-life outcomes

The three-track secondary school system

Germany has three main types of secondary school: *Gymnasium*, *Hauptschule*, and *Realschule*. Successful completion of *Gymnasium* education leads to the *Abitur* qualification (after 13 years of schooling, at age 18 or 19), which entitles holders to enter university. Pupils at a *Hauptschule* leave school at age 16 and then typically proceed on a vocational training track combining a three- or four-year apprenticeship (*Lehre*) with attendance at a trade or technical training college. The *Realschule* provides more academically demanding schooling than *Hauptschule*, and is more orientated towards preparation for white-collar careers. Formal schooling also finishes at age 16, and is typically followed by attendance at a further education college combined with an apprenticeship, or (rarely) a move to a *Gymnasium*. Whatever the track followed, education is compulsory up to age 18. German schools are publicly funded, do not charge fees, and are typically well-resourced. Private schools are rare. In 1995, 23 percent of German thirteen-year-olds attended a *Hauptschule*, 23 percent attended a *Realschule*, and 31 percent attended a *Gymnasium*. (Expressed as percentages of the number of children following these three tracks only, the proportions were 30 percent, 30 percent, and 40 percent, respectively.) The remaining 23 percent attended a diversity of other school types (Statistisches Bundesamt, 1997).[2]

The transition from primary school to secondary school does not depend on scores in formal entrance tests; rather it depends on a mixture of parental preferences and recommendations from primary school teachers (based on primary school performance). There are some differences between federal states (*Bundesländer*); for example, in Baden-Württemburg and Bayern, primary school teachers' recommendations are decisive.

Choice of secondary school type, and progress within the system, are widely perceived to be meritocratic rather than depend on financial resources. However there is clearly scope for differences in family financial (and other) resources to affect the secondary school track followed, for several reasons. Although primary education provision is homogeneous, early- and middle-childhood differences in parental resources and the home environment can affect children's readiness and ability to learn and thence primary school performance. Parental preferences for their child's secondary school track may also vary with

[2] The most common of these were the comprehensive *Gesamtschule* (9 percent) and schools combining

income: for example, higher-income parents may aspire to higher-earning jobs for their children (and hence the *Gymnasium* school track – see below). The nature of the German education system suggests that late-childhood income may also play a significant role. Following the *Gymnasium* track – most likely followed by university attendance (up to six years for a first degree) – entails a longer commitment to learning rather than earning than does following the other two tracks. The time of transition to secondary school is when financial considerations and constraints first become most explicitly apparent in the German educational system, and parents and children may base their decisions on household income around that time, i.e. late childhood rather than early childhood.

Our empirical work focuses on the type of school that was attended at age 14 (as previous researchers have done). There is some diversity in the age at which children make the transition to secondary school. In a few cases they may move as early as age 10, but the vast majority of transitions are made later, and by age 14 at the latest. Switches between tracks are rare. In sum, by focusing on age 14, we ensure that we correctly measure the track followed.

School type and later-life earnings

The type of secondary school that a German child attends has long-term consequences for his or her socioeconomic attainment. This point is illustrated in Table 1, which shows how earnings for prime-aged men and women working full-time in West Germany in 1994 varied with the type of secondary school leaving certificate (*Abschluss*) obtained.[3] The table has three main categories, corresponding to the three school tracks identified earlier. (We grouped *Fachschule* (vocational college) and *Gymnasium* certificates together because the former allows entry to vocational tertiary education.) The 'other' category includes some men and women who attended secondary schools other than the three main tracks, but mostly comprises guestworkers and other foreigners whose schooling was completed outside Germany.

An earnings gradient is clearly visible, and for both men and women. On average, *Gymnasium* graduates earn more than *Realschule* graduates who, in turn, earn more than *Hauptschule* graduates. The *Gymnasium* premium is particularly striking: among men, average earnings for *Gymnasium* graduates were some 43 percent higher than for

Hauptschule and *Realschule* classes (7 percent).

[3] Year 1994 is when the oldest children in our sample turned 14. We repeated the Table 1 calculations for 1993, 1995, and 1996, and found the same earnings-education gradients in each case.

Hauptschule graduates and, among women, even higher – 82 percent. The differential between *Realschule* and *Hauptschule* average earnings is smaller, but still notable, and sufficient to caution against pooling these two groups. The three secondary school tracks are naturally ordered from lowest (*Hauptschule*) to highest (*Gymnasium*), where the ordering is associated with a progressively more challenging academic education and increasingly better career prospects.

<Table 1 near here>

3. The econometric model

We estimated ordered probit regression models in which the latent variable, secondary school quality, is related to the three observed secondary school types. Let the latent index, x_{ji}*, describe secondary school quality for child *j* in family *i*:

$$x_{ji}^* = x_{ji} + e_{ji} \tag{1}$$

where the observable index, x_{ji}, is described by

$$x_{ji} = b_1' Y_{ji} + b_2' Z_{ji}. \tag{2}$$

The Y_{ji} is a single measure or a vector of measures of household income during childhood, and the Z_{ji} are other covariates (discussed in detail in the next section). The conditional income effect is encapsulated in the estimate of b_1.

If there were an unobserved family-specific effect, α_i, for example summarising parental 'ability', the true latent index would be given by $x_{ji}^* = a_i + x_{ji} + e_{ji}$ instead of (1), and estimates of the conditional income effect based on (1) and (2) could be biased upwards. To investigate the role of family-specific effects, we considered fixed effect (sibling difference) models and random effects models. Estimation of sibling difference models was infeasible because our sample of 522 children contained too few siblings (only 80 pairs and 10 triples) and, among these, there was insufficient variation in outcomes and explanatory variables. Our strategy was to estimate ordered probit models with and without random effects. For each random effect model estimated, the variance of the random effect was statistically significant, but the coefficient estimates and their statistical significance were remarkably similar to those of the corresponding standard ordered probit model with Huber-White estimates of standard errors (to account for repeated observations on siblings from the same household).[4] Given this

[4] The random effects ordered probit estimates were made comparable with the standard ordered probit estimates by applying the formulae given by Arulampalam (1999).

robustness, and for brevity's sake, we do not report the random effects ordered probit results (they are available from the authors on request).[5]

4. The data set and variables

Our analysis is based on the German Socio-Economic Panel (GSOEP), using data for a cohort of West German children born 1980–84 and observed for up to 14 years (GSOEP waves 1–14).[6] We use children from GSOEP samples A (native German households) and B (guestworker households). Sample B is an oversample of foreigners – individuals and their households mostly of Turkish, Greek, Italian, Yugoslav, or Spanish nationality who had been recruited from abroad during the economic booms of the 1960s and 1970s.[7]

Children of guestworker households are important for the current study for two reasons.[8] First, from a statistical methods perspective, oversampling raises the issue of when to use sample weights. Second, from a substantive point of view, one may hypothesise that cultural or ethnic differences may lead the intergenerational transmission process for these children to differ from those of native German children. For example, as a selected immigrant sample, parental motivation, hopes for their children, and attitudes to education, may be greater than for otherwise equal native German children.

We used sample weights when deriving descriptive summary statistics, but not for the regression results that are reported. Instead we addressed the two guestworker issues simultaneously, by investigating whether income effects varied with guestworker status (and other variables) using models with interaction effects, and also estimating separate models for native and guestworker children. This strategy was inspired by DuMouchel and Duncan (1983) who discuss the rationale for using sample survey weights in (least squares) regression analysis. In the spirit of their work, we are arguing that a model suitably augmented with interaction effects characterises the data satisfactorily, in which case weights are not required. Nor are they required in the separate regressions case.[9]

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[5] Unlike the fixed effect models, the random effects ones assume that there is no correlation between a family-specific effect and income (or any other covariate). Since the likely correlation is positive, our estimates of income coefficients may be interpreted as upper bounds.

[6] See http://www.diw-berlin.de/soep/soepe.htm for GSOEP documentation.

[7] Because we want to use data covering the whole of childhood, we did not use data from subsequent new samples, in particular the East German sample (beginning 1990) and the recent immigrant sample (1994/5).

[8] This is in addition to the usual reason for interest, namely the commonly-held belief of their economic vulnerability – one of the reasons why the over-sample was drawn in the first place.

[9] In addition we compared the results of weighted and unweighted regressions (also motivated by DuMouchel and Duncan, 1983). Estimates for corresponding models were quite similar when interactions between income

We divided childhood into three stages, broadly corresponding to key developmental and schooling stages: early childhood (when aged 0–5 years), middle childhood (ages 6–10), and late childhood (ages 11–14). In order to maximise sample size, we selected children with income observed at least once during each of the three childhood stages, where income was measured by household annual net (post-tax post-benefit) income in 10,000s of 1996 Deutschmarks (DM).[10] Income was not adjusted ('equivalised') for differences in household size and composition, as we did not wish to impose the strong ratio functional form that this would entail. Instead we used separate regressor variables for money income and household composition.[11] Income during each stage was summarised for each child by the mean of his or her within-stage incomes. Similarly, whole-childhood income was calculated as the mean of incomes from ages 0 to 14, using as many observations as were available. Income was observed for all 14 years for 51 percent of the sample, 74 percent had incomes for at least 13 years, and 94 percent had incomes for at least 12 years.[12]

Our portfolio of control variables was modelled on those used in previous research. Arguably, regressions should not include any explanatory variables that are subject to the choice of the parent or the child: 'A specification that includes inputs or jointly chosen variables yields estimates of income effects that are not useful for policy purposes, because they hold constant variables that will actually change in response to changes in income' (Blau, 1999, p. 262). However, as Blau also pointed out, deciding which of the various control variables should be treated as jointly-chosen cannot be settled by a priori reasoning. His strategy was to check the sensitivity of income effect estimates to the inclusion of different sets of controls. This was our approach too.

The core set of control variables included the highest educational qualification of the child's mother and father (university degree or equivalent, apprenticeship, school leaving certificate (*Abschluss*), or no qualifications), the child's sex, mother's age in the year the child was born, the child's birth order, the number of children in the household at age 14, whether the child belonged to a guestworker household, and federal state of residence at age 14.

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and guestworker status were included. When interactions were not included, estimates of the magnitude of income effects differed (unsystematically) in corresponding specifications, but patterns of statistical significance were similar.

[10] We used the derived variable provided as part of the Cross-National Equivalent File: see Burkhauser et al. (2001) for details.

[11] This follows the practice of e.g. Blau (1999) and Duncan et al. (1998).

[12] The vast majority of children had at least three income observations per childhood stage. The fewest observations on average were for early childhood but, even in this case, 88 percent had at least two income observations.

We also estimated models with additional control variables supplementing those in the core set. These extra variables were childhood stage-specific measures of maternal labour supply (number of years worked), experience of parental marital dissolution and re-partnering,[13] experience of a residential move, and whether the home was owned rather than rented.[14] These variables were never statistically significant in our preliminary analyses and their exclusion did not change the coefficient estimates for income or core control variables. Moreover, the supplementary variables are those most likely to be subject to the endogeneity critique. For these reasons, and brevity's sake, we do not report the results from the regressions with the supplementary controls.

Selection of children on the basis of availability of incomes for each childhood stage resulted in an initial sample of 542, and the main estimation sample consisted of 522 children.[15] Paternal education information was missing for 20 children who were born into a household headed by a lone mother. Preliminary analyses confirmed that their exclusion did not affect our results. For the robustness check, we ran regressions including all 542 children, set the paternal education variable equal to 'no qualification' where it was missing, and included lone parenthood variables (as described in the last paragraph) among the explanatory variables. The regression coefficients on the variables of interest changed hardly at all.

Thirty-nine percent of the analysis sample attended *Gymnasium* at age 14, 30 percent attended *Realschule*, and 31 percent attended *Hauptschule*. These proportions match the national percentages (reported earlier) closely.

Summary statistics for the explanatory variables are provided in Appendix Table 1. The table shows, for example, that household income increased by about a third between early- and late-childhood (see also Table 1). Apprenticeships were the most common parental educational qualification, held by 70 percent of fathers and 67 percent of mothers. The average age of each mother in the child's birth year was 27 years. Few mothers worked during childhood: the average was only about three months in early childhood and almost six months in late childhood. Family breakdown and re-partnering was also rare (each experienced by fewer than 5 percent at any childhood stage). The average birth order was

[13] Because there were so few children who experienced parental re-partnering when aged 0–5, we combined this category with the corresponding middle-childhood one. Preliminary analyses also experimented with alternative lone parenthood variables – whether ever lived in a lone parent household (by childhood stage), or the total number of years spent in a lone parent household – but these were also statistically insignificant.

[14] In preliminary analysis, we also experimented with parental wage rates as regressors (following Blau, 1999), but estimated coefficients were not statistically significant.

[15] The sample size was reduced to 519 when we measured income using household income at age 14.

below two, and the average number of children per household when the child was 14 was two. Nine percent of the children came from guest-worker households. Overall, the characteristics of this sample of West German children differ markedly from those of the samples of US children used in previous research.[16] Compared to the US, fewer mothers in West Germany work, fewer children experience lone parenthood, the ethnic dimension is of a different nature, and the education system has a different structure. On these grounds, one might expect the determinants of child outcomes, and income effects in particular, also to differ between Germany and the USA.

5. Results

Income mobility during childhood

Identification of differences between the impact of permanent income and current income, and between income at different childhood stages, depends on there being sufficient income mobility over childhood. We therefore began by examining how household income varied across childhood and how it compared with income measured at age 14. Table 2 shows that the correlation between early-childhood and middle-childhood averaged income, and between middle-childhood and late-childhood averaged income was high, around 0.8, but lower for early- and late-childhood incomes, 0.63. Correlations between each childhood stage average income and whole-childhood average income were higher still, ranging from 0.85 to 0.91. (The correlations are remarkably similar to those reported by Duncan et al., 1998, for US children.) Although these statistics suggest that income mobility is low, they are in fact consistent with a substantial degree of income movement over time (as Duncan et al., 1998, also report). Table 3 classifies children according to quartile group of early- and late-childhood income. Of those in the poorest fourth of the early-childhood income distribution 46 percent were in a different fourth of the late-childhood income distribution. For the second quartile group, the percentage of movers was 57 percent, and for the third and fourth quantile groups, 60 percent and 30 percent.

<Table 2 and Table 3 near here>

[16] For example, compare the summary statistics reported by Duncan et al. (1998, Appendix A).

Unconditional versus conditional income effects

Echoing the US studies, we found a strong (unconditional) association between childhood income and secondary school quality, and conditional income effects were substantially smaller than unconditional ones (cf. summary points 1 and 2 in the Introduction). We compared unconditional income effects (derived from ordered probit regressions with income variables as the only regressors) and conditional income effects (derived from regressions also including the core control variables), for a range of income measures. For example, in the unconditional case, the coefficient on income at age 14 was 0.107 (asymptotic *t*-ratio 5.01), compared with a coefficient of 0.204 (7.61) on whole-childhood income (the average from ages 0–14). When the core controls were also included as explanatory variables, the coefficients fell to 0.065 (2.97) and 0.149 (4.36) respectively.

Whole-childhood and stage-specific income effects

We now focus on the estimates of conditional income effects, for whole-childhood and stage-specific incomes. (The substantive implications of the coefficients are discussed later in terms of predicted school type probabilities.) Results are reported for three specifications of the relationship between income and latent school quality: linear, loglinear, and a linear spline allowing effects to differ for incomes above and below the (weighted) sample median income.[17] See Table 4. The estimated coefficients for the control variables were very similar in all three specifications (see the results for Model 1 in Appendix Table 2), so we may focus on the income results.

<Table 4 near here>

According to each of models (1)–(3), there are statistically significant and positive effects of whole-childhood income on school quality. The log-linear specification fits the data the least well of the three according to the Akaike Information Criterion (AIC). The point estimates of the linear spline model suggest that income effects are larger at above-median levels than at below-median levels, i.e. contrary to the hypothesis that income effects are greater for low-income families than high-income families (cf. summary point in the Introduction). (This explains why the loglinear specification fitted worse – it assumes income effects are greater at lower incomes.) However a Wald test of the hypothesis that the slopes

[17] We also estimated linear spline specifications with knot points at the sample quartiles, and at the sample deciles. The general patterns of coefficient estimates were consistent with those reported for the single-knot spline, except that they were estimated more imprecisely. We also explored non-linearities using a set of dummy variables corresponding to income quantile groups. The results were broadly similar to those for the three-knot linear spline.

of the two spline segments were equal could not be rejected ($\chi^2(1)$ test statistic = 0.37, p = 0.54). In addition a likelihood-ratio test of Model 1 against Model 3 yielded a $\chi^2(1)$ test statistic = 0.49 (p = 0.48). We therefore favour the linear whole-childhood income specification. A DM10,000 increase in income has the same impact on school quality for a poor child as for a rich one.

But are there stage-specific income effects? Again we explored linear, loglinear, and linear spline specifications. See the results for Models 4–6 in Table 4. (Model 4's estimates for the control variables are shown in Appendix Table 2.) When comparing the stage-specific income results with those for whole-childhood income, note that whole-childhood income for each child (the average from ages 0–14) does not equal the sum of his or her stage-specific incomes (averages within each stage). And so, in general, Model 1 is not nested within Model 4. An exception would be if there were no stage-specific income effects and household income were constant throughout childhood, in which case we would also expect that each stage-specific income coefficient in a linear specification would be roughly one third the size of the corresponding whole-childhood income coefficient.[18] The coefficients are indeed smaller, though not by that amount.

The results provide mixed evidence about whether timing matters. Judged by the AIC, the choice is again between the linear and linear spline specifications. Each suggests that both early-childhood and late childhood income may play a role, but coefficients are estimated imprecisely – at best around the eight percent significance level (for late childhood income in Model 4). For the linear spline model, we could not reject the hypothesis that the three below-median income coefficients all equalled zero (Wald $\chi^2(3)$ test statistic = 0.77, p = 0.86), or the hypothesis that the above-median income coefficients for early- and late-childhood income were equal and non-zero (Wald $\chi^2(1)$ test statistic = 0.20, p = 0.66). A likelihood-ratio test of the linear model (4) against the spline model (6) – imposing the contraints that income effects were the same at below- and above-median income levels for each childhood stage – produced a $\chi^2(3)$ test statistic of 2.52, with p = 0.47. So again we favour the linear income specification. And in this model, we could not reject the hypothesis that effects of early- and late-childhood income were the same (Wald $\chi^2(1)$ test statistic = 0.01, p = 0.93).

[18] The reason is that 'a given five-year average income level produces one-third the total childhood income of that same income level averaged over 15 years' Duncan et al. (1998, p. 165). The argument requires some modification here because our income stages are not each five-year intervals.

Overall, the clearest evidence so far about stage-specific effects is that middle-childhood income is less relevant than either early- or late-childhood income. However, we have not yet investigated potentially important interaction effects.

Stage-specific income effects with guest-worker interactions

To explore whether income effects differed between native German children and those from guestworker households, we re-estimated our whole-childhood and stage-specific income models, first interacting guestworker status with income and, second, estimating separate regressions for each group. Results for linear income specifications are shown in Table 5.[19]

There are clear – and statistically significant – guestworker interaction effects apparent in all the results shown. According to likelihood ratio tests, Model 7 is preferred to Model 4 ($\chi^2(1)$ test statistic = 6.0, p = 0.01), and Model 8 is preferred to Model 4 ($\chi^2(3)$ test statistic = 13.6, p = 0.00). Using whole-childhood income as the income measure (Model 7), the coefficient on income for native German children was 0.18 (i.e. higher than in Model 1), but virtually zero (0.183–0.188) for guestworker children. The hypothesis that the income and interaction coefficients were of equal size and opposite sign could not be rejected (Wald $\chi^2(1)$ test statistic = 0.00, p = 0.95). All the other coefficients changed hardly at all (see Appendix Table 2), with one notable exception. The guestworker status variable changed from being statistically insignificant to being strongly positive and significant, as shown in Table 5. The stratified sample estimates (Models 9a, 9b) provided corroborative evidence for guestworker interactions: the coefficient on whole-childhood income did not differ significantly from zero, but was significantly positive for native German children.[20]

<Table 5 near here>

Turning to the results from the stage-specific income models, we found clear evidence that timing of low income spells matters. There was a statistically significant income effect for late-childhood income – but only for native German children. The estimate for them is 0.12 (asymptotic t-ratio = 2.96), twice the size of the corresponding coefficient in Model 4, but –0.14 (0.121–0.262) for guestworker children. For other childhood stages, there appeared to be no interaction effects. The coefficient on early childhood income was much the same

[19] In preliminary analysis, we also investigated potential interactions between income and father's education, but these were not statistically significant. We also estimated linear spline models, and they suggested similar results to those reported.

[20] The stratified models are not fully comparable with the interactions models, because to estimate the former we had to combine some parental education and federal state categories in order to maintain cell sizes.

magnitude as in the model without interactions (about 0.06), but imprecisely estimated. These findings are echoed by those from the stratified sample estimates (Models 10a, 10b). The coefficient on late-childhood income in the regression for native German children, 0.14, was statistically significant (asymptotic *t*-ratio = 3.2), but –0.09 for guestworker children and with a large standard error. Coefficients on early-childhood income are very similar for both groups, around 0.08, but with asymptotic *t*-ratios of only 1.2 and 1.3.

The conclusion that can be drawn from Table 5 is that there are significant income effects but only for native German children, and the difference is apparently driven by income in late-childhood rather than other childhood stages. But, at the same time, at low levels of income, guestworker children achieve higher school quality levels than their incomes would otherwise imply. Put another way, a graph of the school quality index against income would show a horizontal line for guestworker children, whereas the line for native German children would have a lower intercept but a positive slope. We illustrate this further below in terms of predicted probabilities of *Gymnasium* attendance, in the context of a more general discussion of the substantive magnitude of the income effects, in absolute terms and relative to the effects of other factors that determine child outcomes.

Are the income effects large or small?

To assess the magnitude of income effects, we calculated the probabilities of *Gymnasium* attendance for individuals with different income levels and combinations of other characteristics. The effects of income were compared with the effects associated with guestworker status, family background (represented by father's highest educational qualification), and institutional variations in the education system (associated with federal state of residence). In all the regressions we estimated, higher school quality was associated with higher levels of paternal and maternal educational qualifications. There were significant differences in the school quality index by federal state, with markedly lower values for Bayern and Baden-Württemberg (where primary school preferences outweigh those of parents in the choice of secondary school track).[21]

[21] Although these states are also distinctive because they are affluent, southern, politically conservative, and catholic, it is the differences in secondary school entry rules that are most likely driving our results. (For example, one would expect more affluent states to have higher school quality, other things equal, rather than lower as we observe.) Alternatively it may be that in these regions, the lifetime income prospects for graduates of the *Gymnasium* track are lower and of the vocational tracks higher, than elsewhere in Germany. We found no strong evidence for this in versions of Table 1 calculated separately for these states (for 1993–96). In both states, there was some evidence of higher relative earnings for men with a *Realschule* leaving certificate (compared to the national average differential). But, at the same time, the *Gymnasium* earnings premium (relative to *Hauptschule*) was also greater than the national average premium. Similar breakdowns for women were

Predicted probabilities were derived using the point estimates from Model 8 (stage-specific incomes with guestworker interactions, linear specification). Groups of (hypothetical) children were characterised according to whether they were from a native German or guestworker household, by paternal education level, whether they lived in Berlin or Bayern and of course household income during childhood. We assumed that each child remained in the same quartile group of the income distribution throughout childhood, but income levels rose in absolute terms over time (i.e. reflecting the experience of our sample) More specifically, we assumed that the annual household incomes of a child in the poorest quartile group were (in 10,000s of 1996 DM) 2 during early childhood, 3 during middle childhood, and 4 during late childhood. For children in the second quartile group, the income sequence by childhood stage was 4, 5, 6; for the third quartile group, the sequence was 6, 7, 8; and for the richest quartile group, 8, 9, and 10. Other characteristics were assumed to be the same for every child (and close to sample mean values): mother's highest educational qualification was set equal to apprenticeship, the age of the mother at the child's birth was 27, the number of children in the household when the child aged 14 was 2, and the child's birth order was 2.[22] We considered boys rather than girls (but note that differences between the sexes were not statistically significant).

The predicted probabilities are reported in Table 6. They show, for example, that a native German child in the second poorest income quartile group, living in Berlin, and whose father has completed an apprenticeship, has a probability of attending *Gymnasium* of 40 per cent. If instead, the child were in the poorest income quartile – i.e. annual incomes were DM20,000 lower during each childhood stage – the probability is one-third smaller, 0.27. Being in the richest fourth rather than the poorest fourth – a fourfold increase in annual childhood incomes, other things equal – raises the *Gymnasium* probability by a factor of two and a half, to 0.67. Observe that these income effects are not present for guestworker children – for them, *Gymnasium* probabilities do not vary with income. For guestworker children, probabilities are higher than for otherwise similar native German children in the poorest fourth of the childhood income distribution, much the same in the second poorest fourth, and then fall below in the top half of the distribution. Comparing two children in the richest fourth of the distribution, each living in Berlin and whose fathers have apprenticeships, the

unreliable because of small cell sizes.

[22] Higher school quality was associated with being an elder child (i.e. lower birth order) and smaller numbers of children in the household at age 14. The regressions also suggested that school quality was higher for girls than boys, and increased the older the age of the mother when the child was born but, for both these variables, the coefficients were imprecisely estimated. See Appendix Table 2 for full details.

guestworker child has a probability of attending *Gymnasium* of 0.42 compared to the native German child's probability of 0.67. If their fathers both had a tertiary qualification instead, the difference in probabilities is still high: 0.73 compared with 0.90.

<Table 6 near here>

The income effects for native German children may be described as small, because it is only with large income changes that there are large changes in *Gymnasium* probabilities. A difference of DM20,000 in post-tax post-benefit income per year (sufficient to move individuals between quantile groups) represents a large income change. If incomes were DM1000 per year higher than previously assumed (2.1, 3.1, 4.1 for a child in Q1 rather than 2, 3, 4), then predicted probabilities for children in Q1 to Q4 would be at most one percentage point higher than the counterpart probability shown in the table.

Income effects are small too because it is only large income changes that generate differences in probabilities corresponding to those produced by changes in some other factors. Differences in educational structure and parental background have impacts on *Gymnasium* attendance probabilities similar or greater to the impact of a native German child changing quartile group of the childhood income distribution. Table 6 shows that living in Bayern rather than Berlin is associated with a very large fall in *Gymnasium* attendance probabilities. For example, focussing again on the case of the native German child whose father has an apprenticeship (the modal case), the probability for a child in the poorest fourth of the childhood income distribution was 0.06 rather than 0.27 and, for a child in the richest fourth, 0.30 rather than 0.67. Thus changing federal state has a larger effect than moving from the richest fourth to the poorest fourth of the childhood income distribution. Guestworker children living in Bayern rather than Berlin also have substantially lower *Gymnasium* attendance probabilities, other things equal.

Different levels of paternal education are also associated with substantial differences in *Gymnasium* attendance probabilities. For example, for a native German child resident in Berlin and in the second poorest quartile group, the probability almost doubles if the father has a tertiary educational qualification rather than an apprenticeship, to 0.72 from 0.40. The probability halves, to 0.19, if instead he had no qualifications. The same patterns exist for guestworker children. For a boy resident in Berlin and in the second poorest quartile group, the probability of *Gymnasium* attendance was 0.20 if his father had no qualifications, 0.42 if he had an apprenticeship, and 0.74 if he had a tertiary qualification.

6. Summary and conclusions

We have examined the effects of childhood family on German children's secondary school type, with a specific aim of seeing whether our results were consistent with previous findings, most of which refer to the USA. Comparisons with the five-point summary given in the Introduction show that there are some similarities with earlier findings but also some important differences.

Income averaged over all childhood had a larger impact on our child outcome than did current income, and conditional income effects were much smaller than unconditional ones (points 1 and 2). Income effects are also relatively small (point 3), but there is particularly German twist to this. They exist only for native German children, and not at all for children of guestworker households.

There are differences from the earlier literature concerning childhood stage-specific income effects (point 4) and non-linearities (point 5). For secondary school type in Germany, the evidence about timing suggests that it is late-childhood income that matters most. The results are consistent with the arguments made earlier that it is first at this stage that there is an explicit choice to be made with clear financial consequences (in terms of foregone earnings rather than costs and fees). Evidence that early-childhood income matters is weak, and for middle-childhood income, non-existent. Finally, it appears that income effects are not greater for children from poorer households than those from richer households – the effect of income was captured satisfactorily using linear specifications.

Büchel et al.'s (2001) study also reported small income effects, and stated that they were 'an indication of the success of Germany's socially aware education policy, which offers publicly financed access to all schools to achieve equal opportunities for all children (2001, p.151). Our results suggest that this claim needs to be modified, in particular to take account of the experience of children from guestworker households. For those in the poorest fourth of the income distribution, and compared to native German children, the probabilities of *Gymnasium* attendance are higher than their incomes would otherwise suggest. We are not aware of any special measures targeted at this group that would explain this, however. A more plausible story is that low-income guestworkers are more motivated and forward-looking for their children than otherwise comparable native German parents – the guestworkers are a selected sample who originally came to Germany to better themselves. Contrary to the equal opportunities ideal is our finding that among those in the richest half of the income distribution, guestworker children have lower probabilities of *Gymnasium* attendance than do native German children.[23] Understanding the sources of these differences is an important task for future research.

More generally, the results indicate that differences in institutions and societies do matter. Compared to the USA, Germany has a distinctive three-track secondary school system and, related to this, there is much greater emphasis on formal qualifications in the labour market. Fewer mothers with young children work, lone parenthood is rare, and the status of guestworker households in German society differs from that of African-American and Hispanic households in the USA.

[23] The chances of being in the richest half are lower for individuals in guestworker households than for native Germans too, but that is a different issue.

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Table 1. The *Gymnasium* premium
(mean earnings in 1994 of men and women aged 26–59 working full-time, by highest school-leaving certificate)

School-leaving certificate (*Abschluss*)	Men		Women	
	Mean (DM per year)	Mean, as percentage of *Hauptschule* mean	Mean (DM per year)	Mean, as percentage of *Hauptschule* mean
Other	45,833	87	28,919	112
Hauptschule	52,635	100	25,860	100
Realschule	56,163	107	33,520	129
Gymnasium (or *Fachschule*)	75,459	142	46,984	182

N = 3682 (men), 2820 (women). Source: GSOEP.

Table 2. Correlations, means and standard deviations of household income variables

	Pearson correlation coefficients						
Household income variable	(a)	(b)	(c)	(d)	(e)	Mean	S.D.
(a) Average, ages 0–5	1.00	–	–	–	–	5.59	2.28
(b) Average, ages 6–10	0.84	1.00	–	–	–	6.69	3.00
(c) Average, ages 11–14	0.63	0.78	1.00	–	–	7.43	3.11
(d) Average, ages 0–14	0.85	0.95	0.91	1.00	–	6.70	2.63
(e) At age 14	0.54	0.67	0.91	0.81	1.00	7.71	3.57

Note: Household income is annual household income in DM10,000s (1996 DM).
Weighted data. Unweighted N = 522 (519 for income at age 14).

Table 3. Mobility in the distribution of income over childhood: late-childhood destinations by early-childhood origins (row percentages)

Quartile group of household income: average, ages 0–5	Quartile group of household income: average, ages 11–14				
	Q1 (poorest)	Q2	Q3	Q4	All
Q1 (poorest)	54.0	31.8	10.9	3.3	100.0
Q2	23.5	42.5	29.0	5.1	100.0
Q3	17.8	20.9	39.5	21.8	100.0
Q4	4.2	5.4	21.3	69.1	100.0
All	25.0	25.0	25.0	25.0	100.0

Note: Weighted data. Unweighted N = 522.

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Table 4. Whole-childhood and stage-specific income effects (ordered probit estimates)

	Whole-childhood income			Stage-specific income		
	Linear	Log-linear	Spline	Linear	Log-linear	Spline
Household income variable (in 1996 DM10,000s)	(1)	(2)	(3)	(4)	(5)	(6)
Average, ages 0–14	0.149					
	[4.36]					
Log(average, ages 0–14)		0.757				
		[3.14]				
Below median(average, ages 0–14)			0.108			
			[1.43]			
Above median(average, ages 0–14)			0.173			
			[3.32]			
Average, ages 0–5				0.067		
				[1.55]		
Average, ages 6–10				0.019		
				[0.35]		
Average, ages 11–14				0.062		
				[1.75]		
Log(average, ages 0–5)					0.153	
					[0.64]	
Log(average, ages 6–10)					–0.073	
					[0.23]	
Log(average, ages 11–14)					0.552	
					[2.18]	
Below median(average, ages 0–5)						–0.021
						[0.22]
Above median(average, ages 0–5)						0.115
						[1.71]
Below median(average, ages 6–10)						0.021
						[0.22]
Above median(average, ages 6–10)						0.021
						[0.28]
Below median(average, ages 11–14)						0.039
						[0.53]
Above median(average, ages 11–14)						0.078
						[1.54]
Pseudo-R^2	0.17	0.16	0.17	0.16	0.17	0.17
Log-likelihood	–475.9	–478.5	–475.7	–476.9	–480.2	–475.6

Note: Asymptotic *t*-ratios shown in brackets, derived using Huber-White standard errors adjusting for repeated observations on siblings from the same household. Models also include the core set of regressors described in text: estimates for models (1) and (4) are shown in Appendix Table 2; estimates for models (2), (3), (5), and (6) are available on request from the authors. *N* = 522.

Table 5. Income effects with guest-worker interactions (ordered probit estimates)

Household income variable (in 1996 DM10,000s)	Guest-worker status interacted with income variables: Whole-childhood income	Guest-worker status interacted with income variables: Stage-specific incomes	Separate models for native German and guestworker children: Whole-childhood income, Guest-worker	Whole-childhood income, Native	Stage-specific incomes, Guest-worker	Stage-specific incomes, Native
	(7)	(8)	(9a)	(9b)	(10a)	(10b)
Average, ages 0–14	0.183		0.070	0.244		
	[4.65]		[0.92]	[6.56]		
Average, ages 0–5		0.062			0.081	0.074
		[1.10]			[1.23]	[1.34]
Average, ages 6–10		–0.010			0.093	0.028
		[0.17]			[0.86]	[0.47]
Average, ages 11–14		0.121			–0.094	0.137
		[2.96]			[1.12]	[3.21]
Interaction between guestworker status and income:						
Average, ages 0–14	–0.188					
	[2.37]					
Average, ages 0–5		–0.003				
		[0.03]				
Average, ages 6–10		0.088				
		[0.69]				
Average, ages 11–14		–0.262				
		[3.08]				
Guestworker household	1.087	1.189				
	[2.30]	[2.44]				
Pseudo-R^2	0.17	0.18	0.08	0.14	0.09	0.14
Log-likelihood	–472.9	–470.1	–160.6	–324.9	–158.7	–325.4

Note: Asymptotic *t*-ratios shown in brackets, derived using Huber-White standard errors adjusting for repeated observations on siblings from the same household. Models also include the core set of regressors described in text. Estimates for models (7) and (8) are shown in Appendix Table 2; estimates for models (9) and (10) are available on request from the authors. N = 522 (173 children in guestworker households, 349 children in native German households).

Table 6. Predicted probabilities of *Gymnasium* attendance at age 14, by income group, guestworker status, father's highest educational qualification, and federal state.

	Native German		Guest-worker	
Father's educational qualifications and household income quartile group	Berlin	Bayern	Berlin	Bayern
Father has no qualifications				
Q1 (poorest)	0.11	0.01	0.20	0.04
Q2	0.19	0.03	0.20	0.04
Q3	0.29	0.07	0.20	0.04
Q4	0.42	0.13	0.20	0.04
Father has apprenticeship				
Q1 (poorest)	0.27	0.06	0.42	0.13
Q2	0.40	0.11	0.41	0.12
Q3	0.53	0.20	0.41	0.12
Q4	0.67	0.30	0.41	0.12
Father has tertiary qualification				
Q1 (poorest)	0.59	0.24	0.74	0.38
Q2	0.72	0.36	0.74	0.38
Q3	0.82	0.49	0.73	0.37
Q4	0.90	0.63	0.73	0.37

Predicted probabilities derived from the point estimates of Model 8 (see Table 5). Predictions assume that the child stays in the same income quartile group at each life-cycle stage. The assumed early-, middle-, and late-childhood incomes (in 1996 DM10,000s) were for Q1, 2, 3, 4; for Q2, 4, 5, 6; for Q3, 6, 7, 8; and for Q4, 8, 9, 10. In addition, the predictions assume that the child was a boy, mother's highest educational qualification was apprenticeship; age of mother at child's birth was 27; number of children in household when child aged 14 was 2; child's birth order was 2.

Appendix Table 1: Summary Statistics

Variable	Unweighted		Weighted	
	Mean	S.D.	Mean	S.D.
School type at age 14				
Hauptschule	0.38	0.49	0.31	0.46
Realschule	0.29	0.45	0.30	0.46
Gymnasium	0.33	0.47	0.39	0.49
Household income (in 1996 DM10,000s)				
Average, ages 0–5	5.31	2.03	5.59	2.28
Average, ages 6–10	6.33	2.53	6.69	3.00
Average, ages 11–14	7.16	2.96	7.43	3.11
Average, ages 0–14	6.40	2.37	6.70	2.63
At age 14	7.40	3.37	7.71	3.57
Father's highest educational qualification				
None	0.07	0.26	0.02	0.14
School leaving certificate	0.20	0.40	0.10	0.30
Apprenticeship	0.60	0.49	0.71	0.46
Tertiary	0.13	0.33	0.17	0.38
Mother's highest educational qualification				
None	0.12	0.32	0.04	0.20
School leaving certificate	0.27	0.44	0.23	0.42
Apprenticeship	0.57	0.50	0.67	0.47
Tertiary	0.05	0.22	0.06	0.24
Other child and household characteristics				
Mother's age, year of child's birth	26.90	5.30	27.14	4.88
Child is female	0.49	0.50	0.51	0.50
Number of children in household at age 14	2.03	1.01	1.96	1.00
Birth order	1.84	0.791	1.69	0.78
Guestworker household	0.33	0.47	0.09	0.29
Federal State (Bundesland) at age 14				
Berlin	0.02	0.15	0.03	0.16
Schleswig-Holstein	0.04	0.19	0.04	0.20
Hamburg	0.01	0.09	0.01	0.08
Niedersachsen	0.11	0.31	0.12	0.32
Bremen	0.01	0.09	0.01	0.12
Nordrhein-Westfalen	0.24	0.43	0.24	0.43
Hessen	0.09	0.29	0.07	0.27
Rheinland-Pfalz, Saarland	0.06	0.24	0.06	0.24
Baden-Württemberg	0.23	0.42	0.17	0.38
Bayern	0.20	0.40	0.25	0.43
Mother's labour supply				
Years worked, child aged 0–5	0.33	0.87	0.23	0.73
Years worked, child aged 6–10	0.65	1.43	0.43	1.17
Years worked, child aged 11–14	0.68	1.31	0.49	1.14
Family breakdown and re-partnering				
Breakdown, child aged 0–5	0.02	0.15	0.03	0.17
Breakdown, child aged 6–10	0.05	0.21	0.04	0.20
Breakdown, child aged 11–14	0.03	0.17	0.04	0.19
Repartnering, child aged 0–10	0.02	0.15	0.03	0.15
Repartnering, child aged 11–14	0.02	0.13	0.02	0.13
Home ownership and residential mobility				
Home owned, child aged 0–5	0.35	0.48	0.47	0.50
Home owned, child aged 6–10	0.18	0.39	0.23	0.42
Home owned, child aged 11–14	0.08	0.28	0.08	0.28
Moved home, child aged 0–5	0.12	0.32	0.14	0.34
Moved home, child aged 6–10	0.06	0.24	0.05	0.21
Moved home, child aged 11–14	0.04	0.20	0.04	0.19

Note: Unweighted N = 522.

Appendix Table 2. Non-income parameter estimates for selected regression models

Regressor	Whole-childhood income: No interaction	Whole-childhood income: With guestworker interaction	Stage-specific incomes: No interaction	Stage-specific incomes: With guestworker interaction
(Regression model)	(1)	(7)	(4)	(8)
Father's highest educational qualification (reference category: none)				
School leaving certificate	0.267	0.270	0.268	0.297
	[0.97]	[0.98]	[0.98]	[1.10]
Apprenticeship	0.553	0.579	0.556	0.631
	[2.13]	[2.20]	[2.16]	[2.45]
Tertiary	1.367	1.351	1.403	1.474
	[4.24]	[4.13]	[4.37]	[4.49]
Mother's highest educational qualification (reference category: none)				
School leaving certificate	0.474	0.476	0.463	0.476
	[2.06]	[2.16]	[2.03]	[2.12]
Apprenticeship	0.457	0.422	0.455	0.415
	[2.00]	[1.90]	[2.00]	[1.83]
Tertiary	0.826	0.741	0.847	0.774
	[2.11]	[1.89]	[2.17]	[1.90]
Other child and household characteristics				
Mother's age, year of child's birth	0.015	0.013	0.015	0.014
	[1.14]	[1.01]	[1.13]	[1.06]
Child is female	0.121	0.133	0.123	0.123
	[1.15]	[1.26]	[1.17]	[1.16]
Number of children in household at age 14	–0.116	–0.132	–0.111	–0.131
	[1.77]	[2.03]	[1.70]	[1.99]
Birth order	–0.290	–0.275	–0.287	–0.272
	[4.37]	[4.14]	[4.36]	[4.09]
Guestworker household	0.056	1.087	0.052	1.189
	[0.35]	[2.30]	[0.33]	[2.44]
Federal State (Bundesland) at age 14 (reference category = Berlin)				
Schleswig-Holstein	–0.317	–0.385	–0.297	–0.328
	[0.70]	[0.85]	[0.64]	[0.71]
Hamburg	–0.358	–0.581	–0.336	–0.582
	[0.60]	[1.01]	[0.56]	[1.01]
Niedersachsen	–0.597	–0.656	–0.570	–0.611
	[1.99]	[2.22]	[1.86]	[1.93]
Bremen	–0.671	–0.723	–0.665	–0.686
	[1.54]	[1.64]	[1.52]	[1.47]
Nordrhein-Westfalen	–0.396	–0.458	–0.377	–0.451
	[1.44]	[1.68]	[1.34]	[1.53]
Hessen	–0.075	–0.115	–0.056	–0.100
	[0.23]	[0.35]	[0.17]	[0.29]
Rheinland-Pfalz, Saarland	–0.352	–0.358	–0.345	–0.328
	[1.10]	[1.13]	[1.06]	[0.98]
Baden-Württemberg	–0.766	–0.754	–0.737	–0.713
	[2.77]	[2.75]	[2.63]	[2.43]
Bayern	–0.943	–0.994	–0.916	–0.940
	[3.38]	[3.58]	[3.22]	[3.18]
κ_1 (lower cutpoint)	0.66	0.78	0.66	0.85
	[1.11]	[1.31]	[1.10]	[1.37]
κ_2 (upper cutpoint)	1.60	1.72	1.60	1.80
	[2.67]	[2.86]	[2.64]	[2.90]
Pseudo R^2	0.17	0.17	0.16	0.18
Log likelihood	–475.9	–472.9	–476.9	–470.1

Note: Asymptotic *t*-ratios shown in brackets, derived using Huber-White standard errors adjusting for repeated observations on siblings from the same household. N = 522. Regressions also included income regressors: see Tables 4 and 5 for estimates.

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