DISCUSSION PAPER SERIES

IZA

IZA DP No. 246

Schooling Family, Background, and Adoption: Does Family Income Matter?

Erik Plug
Wim Vijverberg

January 2001

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

Schooling, Family Background, and Adoption: Does Family Income Matter?

Erik Plug
University of Amsterdam, Wageningen University and IZA, Bonn

Wim Vijverberg
University of Texas at Dallas and IZA, Bonn

Discussion Paper No. 246
January 2001

IZA

P.O. Box 7240
D-53072 Bonn
Germany

Tel.: +49-228-3894-0
Fax: +49-228-3894-210
Email: iza@iza.org

This Discussion Paper is issued within the framework of IZA's research area *The Future of Work.* Any opinions expressed here are those of the author(s) and not those of the institute. Research disseminated by IZA may include views on policy, but the institute itself takes no institutional policy positions.

The Institute for the Study of Labor (IZA) in Bonn is a local and virtual international research center and a place of communication between science, politics and business. IZA is an independent, nonprofit limited liability company (Gesellschaft mit beschränkter Haftung) supported by the Deutsche Post AG. The center is associated with the University of Bonn and offers a stimulating research environment through its research networks, research support, and visitors and doctoral programs. IZA engages in (i) original and internationally competitive research in all fields of labor economics, (ii) development of policy concepts, and (iii) dissemination of research results and concepts to the interested public. The current research program deals with (1) mobility and flexibility of labor markets, (2) internationalization of labor markets and European integration, (3) the welfare state and labor markets, (4) labor markets in transition, (5) the future of work, (6) project evaluation and (7) general labor economics.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character.

IZA Discussion Paper No. 246
January 2001

ABSTRACT
Schooling, Family Background, and Adoption: Does Family Income Matter?*

One would expect that family income is an important positive factor in the school attainment of children. However, evidence on this relationship is often tainted by the lack of control for parental ability, since at least a portion of ability is transferred genetically to children. This paper considers empirical strategies that control for both observed and unobserved parental ability. In the end, family income still has a significant effect, which must therefore be causative. It implies that high-ability children in low-income families face binding credit constraints that society may wish to relieve.

JEL Classification: D31, I21, J13, J24

Keywords: Intergenerational mobility, human capital, family income, adoption

Erik Plug
NWO program "Scholar"
Department of Economics and Econometrics
University of Amsterdam
Roetersstraat 11
1018 WB Amsterdam
The Netherlands
Tel.: +31(0)20 5254311
Fax: +31(0)20 5254310
Email: plug@fee.uva.nl

* This research is part of the NWO priority program on schooling, labour market performance and economic development. Financial support from IZA is gratefully acknowledged. We also thank Hessel Oosterbeek, Jan Rouwendaal, and participants in seminars at IZA (Bonn, 2000), Scholar (Ams-terdam, 2000) and other universities for their valuable comments.

1 Introduction

Many empirical studies find family income to be an important factor in explaining school success of children (Becker and Tomes, 1985; Taubman, 1989; Haveman and Wolfe, 1995). The mechanism economists offer to explain this family relation is that children from poor families are restricted in their pursuit of more and higher quality education because their parents face credit constraints when financing their children's education.

More recently, economists are aware that such conclusions are as yet unwarranted because of flaws in the underlying empirical models (Blau, 1999; Cameron and Heckman, 1998; Cameron and Taber, 2000; Mayer, 1997; Shea, 2000). The problem is that most studies ignore the strong correlation of both family income and educational attainment with (mostly unobserved) ability. Any correlation between family income and children's school success may therefore not be causative at all: parents with high earnings are on average better endowed with ability than parents with low earnings, and they also tend to produce children who do well in school by virtue of superior genes. The fact that these children are successful and come from high income families fails to prove causality.

Without a doubt, the dynamics of the distribution of income hinges critically on the role of family income. If family income has no impact on the production of human capital in their offspring, the distribution of income at any point in time is merely a reflection of the distribution of ability among the then-existing population. On the other hand, if family income does matter, the distribution of income also depends on the income distribution among the previous generation and on the existence of credit constraints. Thus, a better understanding of the role of family income is also required when designing educational policies. If public resources spent on educational systems are believed to alleviate the financial constraints of students from poor families, it is important to know whether family income is the actual mechanism at work. Similarly, income tax policies may, or may not, have a long term impact on the distribution of income of the next generation.

The aim of this paper is to estimate true (causal) family income effects on the educational attainment of children. The following discussion spells out four alternative approaches that attempt to distill this illusive effect, but each strategy has its limitations. The four approaches form the ingredients for the empirical analysis in Section 4.[2]

The first and most convenient approach is to find a rich data set that allows one to control for the effect of ability. Studies that do so mostly use IQ test scores and found that the impact of family income falls but remains significantly positive. Of course, the main problem with this approach is that ability controls

[2]This is the reason we restrict the discussion to these four approaches. We do not claim completeness and are aware that there are other approaches too.

like IQ measures inevitably remain incomplete (Griliches, 1977; Plug, Van Praag and Hartog, 1999). Accordingly, we do not think that the estimated income effects obtained with this approach warrant a causal interpretation.

The second approach is to use data panels to compare the impact of family income on children's educational attainment at different stages of the educational career. Since direct schooling costs are in essence independent of ability but vary over the different schooling stages, the impact of income should vary accordingly. In this vein, Alvin and Thornton (1984) find that the impact of family income is greater when the child is 15 years old than when the child is 4 years of age. Similarly, Mayer (1997) finds that the impact of family income increases over the years, but she also finds that the impact remains positive when the child has already left school. Mayer argues that because family income is generated after the educational outcome is observed, the positive impact of family income rather points to unobserved ability effects than to income effects per se. The main problem with this interpretation is that lifecycle theories of consumption permits temporal income to have an impact on educational attainment even before schooling is commenced or after schooling is completed. For example, if parents borrow money to finance their offspring's education, future income affects current expenditures. Vice versa, if parents saved income to finance their offspring's education, past income affects current educational spending. Altogether, it is difficult to conclude that income matters on grounds of estimates like these.

The ideal approach to establish the causal relation between family income and the educational attainment of children is to use experimental designs where family income and children are randomly connected. Two designs are evidentiary: (i) in the case that children are raised by randomly selected parents, if children brought up in high-income families do better in school, income matters; (ii) if money is given to randomly selected parents and if their children then receive more schooling, income matters. The main problem with social experiments is that they are seldom carried out. Our solution is to imitate these experiments.

The first experiment requires children that are not genetically descended from the family that rears them. Adopted children fulfill this requirement. Thus, if the relationship between family income and educational outcomes is estimated on a sample of adopted children, estimated income effects can be interpreted as causal effects. The idea to use adopted children to measure the difference between the environmental and genetic influence of family background is not new. Studies on family income effects and school success of adopted children, however, are rare and offer mixed results. On the one hand, using a very small and selective sample, Scarr and Weinberg (1978) estimated positive income effects on IQ for biological children and no effects for adopted children. On the other hand, Sacerdote (2000) found substantial income effects on the educational outcomes of adopted children. The problem with such studies is that adoption samples are small in size and that adoption outcomes remain biased to the extent that children and income are not connected at random. This is the case whenever adoption agencies apply recruitment programs to select families that are suited for adoption.

The second experiment imitates a lottery and requires the measurement of an ability-free income component in order to separate income effects from ability effects. Perhaps closest in the spirit of this experiment is the study by Shea (2000). He isolates that part of income that he defines as income luck using instruments like union status, industry, and involuntary job loss due to plant shutdowns. He finds that parental income only matters when the father has less than 12 years of schooling but not in families with low income per se. The main problem is whether his instruments are valid instruments, i.e., variables that are substantially correlated with family income but somehow are not correlated with the unobserved ability factors that influence the educational outcome of children (Bound, Jaeger, and Baker, 1995). For union status or industry choice this is rather questionable. According to mainstream sociological theories of mobility, not only education and income but also occupations are transmitted from father to son.

The empirical analysis below estimates the effect of family income on the educational attainment of children with a strategy inspired by these four approaches. In isolation each approach is rather limited, but in combination, they will offer a better understanding of the role of family income. Data are provided by a unique US dataset, the Wisconsin Longitudinal Survey (WLS), that contains very detailed multigenerational information about households. Among the most important variables are family income measured in 1975 and 1992, IQ of one of the parents measured when (s)he was a high school senior in 1957, educational attainment of the children as of 1992, and information whether these children are their parents' own offspring as opposed to adopted children. The final conclusion is that family income has a causative impact on children's schooling that is independent of parental ability.

The plan of the paper unfolds as follows. Section 2 describes the data from the Wisconsin Longitudinal Survey. Section 3 briefly discusses our empirical strategy. Section 4 presents and discusses the empirical findings. Section 5 summarizes our conclusions.

2 Data

This paper employs the Wisconsin Longitudinal Survey which is an unique American dataset with information on people who were born around 1940.[3] The collection of these data started in 1957 with a questionnaire administered to the complete cohort of students who graduated from a high school in the American state Wisconsin in that year. The information in that first wave relates to the students' social background (parents' education and occupation, numbers of older

[3]For more information on the WLS data, see, among others, Sewell and Hauser (1992), Hauser et al. (1996), and Plug and Vijverberg (2000).

and younger sibling), intelligence (measured as standardized IQ test scores), and aspirations. Subsequently, research was continued on a randomly selected one third of the original cohort. In 1964 and 1975, the respondents was approached again to obtain information about, among others, their schooling and labor market careers. In 1992, the same sample was contacted once more in order to collect new information about their labor market experiences between their late thirties and early fifties. As well, this latest round contained questions about many life events, attitudes, and facets of life.

Of particular interest for the present study, a set of questions targeted the educational attainment of the respondents' children. Respondents were asked to list for each child the highest grade or year of regular school that child ever attended, whether (s)he completed this grade or year, and whether (s)he attended a regular school in the last 12 months. From the information on educational attainment we create the variable "years of schooling." For those children who completed the highest level attended, years of schooling equals the number of years nominally required for that. Children who were still in school constitute censored observations and will be treated accordingly in our empirical analysis; this is the case for about 20 percent of our sample. Note that deleting these censored observations from the analysis would cause the results to be biased. This holds true especially for the age variable because in that case only low achieving young children would be included in the sample.

As the respondents in the sample often have more than one child, we construct sibling information variables for each child. We also use information on the relationship of the child to the respondent. In particular, we distinguish children who are adopted from children who are the biological offspring of their listed parents.

The other explanatory variables are common to all children from a family. These variables can be divided into two groups: human capital variables and financial variables. We discuss each group in turn. Human capital variables are years of schooling of the children's parents (one of whom is a respondent of the original 1957 sample); the respondent's IQ score at age 16; and years of schooling of the respondent's parents. Financial variables included in our analysis are family income measured in 1975 and in 1992, as well as income components that represent random income shocks that are not correlated with observed and unobserved ability. Section 3.1 discusses how we derive these income components.

The number of observations in the 1957 sample equals 10317, but we restrict ourselves basically to the 8500 people who responded to the 1992 questionnaire. Since in this paper we do not want to get involved in complications that arise if children are brought up in incomplete families, we exclude about 1800 childless and one-parent families. After also removing 1350 observations with missing or incomplete information on income in 1975 and 1992 and on their children's age, gender and educational attainment, we have a sample with 5365 families and 13626 children, 549 of whom are adopted. For our sibling model we further restrict the sample to families with at least two children, and if a family has more than two children we randomly select two for the analysis. Thus, we end up with a sample of 6460 siblings from 3230 families.

Summary statistics appear in Table 1. The first column reports statistics on the full sample using all children in the WLS database. The second and third column tabulate the restricted samples consisting of siblings and adopted children.

3 Empirical strategy

3.1 Determining causality

Focusing on the question how family income affects the educational attainment of children, the Introduction already discussed four alternative approaches, each tackling the ability bias in its own (limited) way. The first two approaches are traditional and use additional ability measures and periodic measurement of family income. The other two approaches negate the genetically based correlation between parental income and children's ability with adoption and lottery experiments. We will frame our tests in terms of combinations of these four approaches.

We begin by estimating the usual relationship between family income, other family and individual characteristics and the educational outcomes. We combine the first two approaches by using an additional parental ability measure, as measured by an IQ test score at the age of 16, and two family income measures, measured in 1975 when most children are in primary or lower secondary education and in 1992 when most of the children have left school. We estimate a censored regression model because the analysis uses all children and some children are still in school in 1992. Since the income coefficients can only be interpreted as causal income effects if IQ scores sufficiently capture the genetic link between parents and offspring and if there are no omitted variables that correlate with both family income and educational achievement, we do not wish to conclude causation, for obvious reasons.

We apply the same strategy once more but this time we deal with these omitted ability variables. If we assume that siblings reared together in one family are similarly affected by these unobserved family characteristics we might consider fixed effects estimators: in principle, through differencing the schooling functions of siblings, unobservable components that vary only across families drop out and unobservables that vary across siblings remain. However, differencing also removes family income effects. Instead, therefore, we focus on a model that allows unobserved family-specific characteristics in the schooling function to be correlated across siblings. The model is one with a bivariate random effect and allows for censoring.[4] We estimate this model on a sample of siblings and compare

[4]For the econometric specification of this sibling model, please see Appendix A.

these new estimates with those obtained using a censored regression model on a sample of all children.

Again, we cannot safely say that this sibling model yields estimated income effects that are causal. The problem remains that unobserved ability effects might operate through income as well. Only if family income is randomly assigned to children could one interpret the estimated income coefficient as the causal effect of family income on educational outcomes. The ideal research design would disconnect the family link between children and the family that rears them. Hypothetically, one could achieve this by dropping either children or money on the doorsteps of randomly selected parents, and then follow the children to see how well they do in school. Conceptually, this is precisely the objective of the adoption and lottery experiments.

To examine the outcomes of an adoption experiment we simply estimate a censored regression model on a sample of adopted children. Unfortunately, the sibling model cannot be estimated in this way because the sample of adopted siblings is too small. For the lottery experiment we first compute those parts of income that are arguably generated by luck in both 1975 and 1992 and are independent of observed and unobserved ability (see section 3.2). With these new income measures we then re-estimate income effects in both the censored regression model and the sibling model.

To sum up, each approach is rather limited in obtaining the causal relation between family income and the educational attainment of children. Our combined approach, however, generates a set of results that, with the nuances addressed in the discussion below, makes a compelling case that we can indeed make the step from correlation to causation.

3.2 Identifying random income shocks

Identification of that part of family income that is arguably generated by luck in the market requires family income to be measured at (at least) two different points in time. Define family income measured in 1975 as y_{75} and in 1992 as y_{92}. Both incomes are generated through observed ability variables (a_{75} and a_{92}), unobserved ability variables (e_{75} and e_{92}) and through sheer luck in the market (l_{75} and l_{92}). The latter variables are unobserved as well. Thus:

$$y_{75} = a_{75} + e_{75} + l_{75},\ y_{92} = a_{92} + e_{92} + l_{92} \tag{3.1}$$

All income generators are allowed to vary over time. Since we plan to imitate a lottery where (prize) money is given to randomly selected parents at different points in time, we are interested in finding unobservables l_{75} and l_{92}.

The first step is to predict log family income in both years on the basis of observed ability measures, and to compute residuals for both years. These residuals express family income net of observed ability and therefore measure unobservable ability and market luck:

$$y_{75} - a_{75} = e_{75} + l_{75},\ y_{92} - a_{92} = e_{92} + l_{92} \tag{3.2}$$

Assuming that (i) luck and unobserved ability are unrelated; (ii) luck moves randomly over time; (iii) only unobserved ability is temporally correlated, we have

$$E(y_{75} - a_{75})(y_{92} - a_{92}) = E(e_{75} + l_{75})(e_{92} + l_{92}) = Ee_{75}e_{92} \tag{3.3}$$

Define the residuals as $\hat{u}_{75}$ and $\hat{u}_{92}$. The result in equation (3.3) constitutes the motivation behind the second step which is (i) to regress $\hat{u}_{92}$ on $\hat{u}_{75}$ and to compute the residuals which may be denoted by $\hat{l}_{92}$; and, in reverse, (ii) to regress $\hat{u}_{75}$ on $\hat{u}_{92}$ and to compute the residuals of this regression which may be denoted by $\hat{l}_{75}$. By equation (3.3), the regression purges the left hand side $\hat{u}$ from common unobserved ability factors common with the right hand side $\hat{u}$. Therefore, the remaining residual of this regression proxies that income component that reasonably represents luck.

Two notes are still in order. First, this technique purges any observed or unobserved income determinant that remains constant over at least this portion of the lifecycle, including ethnic factors, personality traits, et cetera. This is fortuitous, since such factors should not be ascribed to luck anyway. Second, the timing of the measurement of income is such that in 1975 children are still in a compulsory education stage, whereas in 1992 most children have finished their schooling career. This feature will be put to good use in the analysis below.

4 Results

To gain insight into how family income affects the educational attainment of the next generations, the empirical results will be presented along the lines set out in Section 3. Section 4.1 evaluates estimates based on standard approaches; Section 4.2 proceeds to the adoption experiment; and Section 4.3 examines the outcomes of the lottery experiment. It should be noted that throughout the analysis we use parental IQ test scores measured when one of the parents was 16 years old as an ability control.[5]

4.1 The role of family income

Table 2 reports estimates of the relation between family income (and other family and individual characteristics) and the educational achievement of children

[5]Since the WLS measures IQ of only one parent (except in infrequent occasions where two high school seniors of the 1957 sample married each other), we assume father and mother to be in same IQ class.

using WLS samples consisting of all children, siblings and adopted children, respectively.[6] First, consider the impact of family income measured in 1975. At this time the respondent is about 34 years old and, on average, his or her children will be in primary or the early years of secondary school. At this stage, schooling is compulsory, implying (at first glance) that family factors should at most have a muted effect. But recall that the dependent variable is completed (or, if so be the case, censored) years of schooling. 1975 family income may have three effects on eventually completed schooling: (i) according to the lifecycle theory of consumption, schooling later on is paid for by savings from income received earlier; (ii) according to the permanent income hypothesis, variations in 1975 income are indicative of, though imperfectly correlated with, permanent income on which parents base their consumption; (iii) according to the theory of household production, early income creates a family environment that is conducive to the child's success in school, which in turn invites further schooling investment when the child has become a young adult. In any case, column 1 of panel A reports a strong positive parental income effect.

Column 2 uses family income of 1992. At this stage of the parental lifecycle, most children have just ended their schooling career, and college expenses may still be taking a big bite out of the parents' budget. Again, one may offer a permanent income and a lifecycle theory argument. If parents anticipate on their future income (which is closely related to permanent income) while funding their children's education, 1992 income will still be important when the children have finished school. Even so, whether we use 1975 or 1992 income the estimated income effects are not substantially different.

To see whether it is income in 1975 or income in 1992 that is most important we include both income measures simultaneously. Compared to the first two columns, both estimates fall but remain significantly positive, with the impact of 1992 income marginally higher than that of 1975 income. Thus, parental income seems important, whether it is obtained when students are in their in early childhood, or when they already left school.

Yet, even if these findings indicate that income matters, they do not reveal that it is indeed parental income itself that has a beneficial impact on children. The problem is that ability is only incompletely measured with IQ test scores and that unobserved ability components also transfers from parent to child and operate through income. The sibling model captures the correlation between the errors of the schooling functions of siblings. The structure of the sibling model allows the correlation (ρ_k) to vary between households. Estimates in panel B of Table 2 indicate income effects that tell the same story as above, but they also imply that ρ_k hovers around 0.29 and is actually quite stable across households.

[6]All regressions include family and individual characteristics as additional controls. In the paper we only report the estimated income coefficients. Appendix B briefly discusses the other estimates.

To the degree that this correlation picks up the unobserved ability component, it appears to be substantial, which implies the potential that our family income estimates are biased. Further research is needed, to which we now turn.

4.2 The adoption experiment

A more sophisticated way to study parental income effects is to look at children that are genetically unrelated to the family they are raised in. We approximate this experiment by regressing the years of schooling of adopted children on the characteristics of adopting families. We observe that both family income measured in 1975 and 1992 create identical and significant income effects. When income in 1975 and income in 1992 are included simultaneously, the parameter estimates are statistically insignificant individually but are jointly significant at a 10 percent level.[7] Since all these income estimates are genetically unbiased we tentatively conclude that income matters.

Closer inspection, however, suggest that one should be careful about drawing inferences. One might expect the impact of family income on the educational achievement to be stronger for children raised by their own biological parents than for adopted children. The reason is that for parents and their biological children the income transfers capture genetic transfers. For adopted children, however, these genetic transfers do not exist with respect to the family of rearing. But this is not what the evidence in Table 2 indicates: the income effects for biological children, siblings and adopted children are about the same.

Is there an explanation for this? Is there something in the process of adoption that might compensate for the lack of genetic transfer between biological and adopted children which would lead to equal income effect estimates? Adoption experiments (like ours) produce biased estimates if adopted children are not randomly assigned to the family of rearing. The descriptive statistics in Table 1 illustrate that adoption families are not randomly drawn from the population at large and that adopted children live in higher-income families with better educated parents who have a higher IQ. This suggests either (i) that adoption agencies use family recruitment programs to sort out families that are suited for adoption or (ii) that adoption families just select themselves. In either case, adoption families have more favorable socio-economic backgrounds, which, in combination with the underlying sample selectivity process, results in family income coefficients that tend to be too high.[8]

Even if these estimated income effects may be tainted by selectivity bias, there is reason to take them seriously. First of all, with different U.S. data where the

[7]The χ^2 test score equals 5.31 with a p-value of 0.07.

[8]Plug and Vijverberg (2000) discuss the potential dangers in much more detail. That study focuses on differences between the educational outcomes of biological and adopted children and discusses how the estimates are affected if there are (unobserved) differences in genetic make-up, environment, matching process, and upbringing.

mechanism of assigning children to adoptive parents is fairly random, Sacerdote (2000) finds income coefficients for adoption and natural families that are statistically identical when explaining the years of schooling of children. Secondly, up till now, we have implicitly assumed that families treat their biological and adopted children equally with respect to the time and money they invest in them. If there are differences in upbringing and these differences are captured by the income coefficient, such an interpretation of income effects is not correct. When parents do treat their adopted children differently, they either invest less in their adopted children and income effects will be smaller, or they invest more and income effects are larger. In fact, previous research by Dawkins (1976) and Case, Fin and McLanahan (2000) has found that parents invest less in their adopted children. They argue that parents feel the urge to protect their own genetic material and therefore underinvest in their genetically unrelated children. An alternative economic motive that explains the same mechanism would be that parents expect closer ties (financially and emotionally) in their old age with their biological children than with their adopted ones and, thus, invest more in the education of their biological children. In either case, the income estimates for adopted children may serve as lower bounds for biological children: if income matters for adopted children, it definitely matters for biological children.

In sum, because adoption does not have the nice randomization characteristic of typical laboratory experiments, the sample of adopted children offers estimates that are subject to contradictory interpretations due to selectivity bias and underinvestment arguments. The parallel findings in Sacerdote (2000) give us a compelling reason to believe that we do have evidence of a true causal effect income effect, but the case for such a conclusion must still be bolstered with further support.

4.3 The lottery experiment

An alternative experimental design is to identify that part of parental income that represents luck. The idea is to imitate a lottery where money is given to randomly selected parents at different points in time, and then to track the subsequent school performance of their children. If income truly matters we should observe at least two things. First, children should do better in school when parents are handed over their prize money while their offspring are still in school. And second, no effects are expected when parents win their lottery prize and children have already left school. Since parents cannot foresee (future) variation in their income when their children have completed their schooling, it is impossible to anticipate on it while funding their children's education.

In this paper we extract random income shocks using information on 1975 and 1992 income, along the lines set out in Section 3.1. The first stage regressions predict the logarithm of family income in both 1975 and 1992 on the basis of observed human capital and ability measures. The parameter estimates are reported in panel A of Table 3; while for the purposes of this paper they are not particularly interesting, the estimates are entirely plausible, and the strong effects of education and IQ are notable. The residuals from these equations consist of two components: unobserved parental ability and a non-structural part (which might be income generated by luck in the market). Regressing the 1992 income residual on the 1975 counterpart should pick up these unobserved parental abilities; the residual of this equation proxies that component of the 1992 family income that is reasonably ability-free. Vice versa, if regressing the 1975 residual on the 1992 measure yields a measure for ability-free income generated in 1975. These equations are found in the second part of Table 3. The R^2-values turn out to be rather low, suggesting that, beyond observable skill factors, it is predominantly luck in the market that generates family income.[9]

Next, both luck components are entered into the children's human capital equation as a parental income measure; see the first two columns in Table 4. Recall that the 1975 income is received when children are still in school, and that most children have left school when parents receive the 1992 income. In the light of the discussion above, we should expect the former to have an impact but the effect of the latter to be negligible. The estimates are in fact rather paradoxical. On the one hand, family income observed in 1975 matters even after one purges the income component that derives from ability transfers between parents and offspring. Not unexpectedly, the estimate is somewhat smaller than those estimated in Table 2. On the other hand, the impact of the 1992 luck component shows similar patterns and does not disappear.

This is surprising. It might suggest to some that the interpretation of this lucky income component is somehow faulty. Should one perhaps conclude that the random income variable measures unobservable traits that drive educational achievement, such as personality? By design, such lifelong traits have been purged away. Could one argue that a 1992 income shock, being uncorrelated with 1975 income, may not have been not foreseeable in 1975 but that it was reflective of (un-)favorable financial events that played out over a period of several years prior to 1992, during which more children were still in school? There is nothing in the data to prevent one from making such a case, but let us see where this argument leads: the 1975 random component affects all children whereas the 1992 component only hits those who are relatively young (and are still in school or left school recently). Let us therefore respecify the model and interact lucky income with the child's age; see columns 3 and 4 of Table 4. Only the 1992 random income generates negative significant interacted age effects, such that younger

[9]The R^2-values of 0.113 and 0.115 indicate a correlation between the two first-stage residuals of about $0.114^{1/2} = 0.34$, which, if this extraction strategy may so be interpreted, reflects unobservable ability factors impinging on the generation of income. This value is strikingly close to the average correlation of 0.29 of the unobservable ability components in the schooling functions of siblings; see Section 4.1. In future research, it will be worthwhile to explore the role of such ability factors in greater depth.

children are more strongly impacted. Interacted 1975 random income effects are smaller and not significant. The only conclusion that is warranted here is that income itself has to be a beneficiary influence on the child's school performance.

Finally, to get an idea of the magnitude of the income effect, the income elasticities are about 0.02 to 0.03 (Table 4, columns 1 and 2). Comparable estimates taken from existing literature, ranging from 0.01 to 0.04,[10] are biased because there is hardly any study that corrects for genetic and environmental transmissions. The literature also shows that by far the most fundamental economic factor describing the educational attainment of children is the human capital of parents, typically measured by the number of years of schooling (Haveman and Wolfe, 1995). Our model yields the same result, with elasticities of about 0.22 for both parents (0.23 to 0.28 in Sacerdote, 2000; 0.11 to 0.13 in Case, Lin and McLanahan, 2000). In addition, the parental IQ elasticity is about 0.09.[11] Thus, income effects are less important, but they are not unimportant. Consider a random income shock in 1992 of two standard deviations, which would lift a household from a relatively disadvantaged (one standard deviation below the mean) to a relatively advantaged position (one standard deviation above the mean). The family's 10 years old child would complete 0.77 year of additional schooling; their 20 years old young adult would stay 0.55 year longer (on the basis of Table 4, column 4). Given a private rate of return to education of 8 percent, such an income shock would raise the children's lifetime earnings by 4.4 to 6.2 percent.

5 Concluding remarks

This paper examines how family income affects the educational attainment of children. It tackles the problem that estimated income effects are potentially biased by the fact that high ability parents not only generate more income but produce high ability children as well.

To summarize our explorations, we use three strategies to test the idea that a better access to financial resources improves the children's educational achievement, namely (i) the relation between observed family in 1975 and 1992 in combination with IQ as an explicit ability measure and the educational outcomes of children; (ii) the relation between observed family income in 1975 and 1992 and the educational outcomes of adopted children (who are by definition genetically unrelated to the family that rears them); (iii) the relation between those

[10] Table 2 yields elasticities to these biased values.

[11] However, one must be aware of the many complexities that are encountered when comparing elasticities across studies, such as large variation in estimation techniques, small overlap of the variables used in models, different variable specifications, design of the sample, and so forth. For example, in our model the number of siblings has a significantly negative effect in all specifications and captures a portion of an indirect income effect.

components of 1975 and 1992 family income that reasonably represent random income shocks and the educational outcomes of children. In isolation each test demonstrates that family income is a significant factor. Taken together and after careful consideration of the meaning of each estimate, the results strongly suggest that the positive relation between family income and school success is causal and quantitatively not unimportant.

This implies that children living in poor families where resources are lacking are restricted in their educational career. From an economic point of view, this is not at all surprising. To overcome their financial difficulties while financing their children's education, parents have two options. The first option is to borrow. This is not likely to happen since capital markets are unwilling to let (poor) parents borrow against the expected human capital of their children. The second option is to save. That is, parents who foresee difficulties in financing their children's higher education have incentives to save money when their children are very young. This seems unlikely as well, since young parents mostly consume all of their income.[12] In the end financial constraints turn out to be rather decisive in explaining the school success of children.

This conclusion is a motivator for designing educational policies that benefit the poor. Provided that one has a reliable method to measure children's ability, society benefits from alleviating the financial constraints that keep able but low-income students from seeking a more advanced education. Such short-run assistance will also have long-term benefits: these program beneficiaries will not only earn higher incomes but also, given the intergenerational genetic transfer of ability, tend to have higher-ability children, who, because of their parents' income, will be able pursue their desired level of education on their own.

References

Alvin, Duane and Arland Thornton. (1984). Family origins and the schooling process; Early versus late influence of parental characteristics, *American Sociological Review,* vol. 49, pp. 784-802.

Becker, Gary S. and Nigel Tomes. (1986). Human capital and the rise and fall of families. *Journal of Labor Economics*, vol. 4, pp. 1-39.

Blau, David. (1999). The effect of income on child development. *The Review of Economics and Statistics*, vol. 81, pp. 261-167.

[12]Suppose parents are forward-looking and optimize over a long horizon, and suppose also that earnings profiles rise toward middle age and decline in old age. Under such conditions, young parents want to consume more than their resources allow for. But since they cannot use their future earnings as a collateral to borrow, they will, as a matter of optimizing lifecycle behavior, consume all of their income when they are young.

Bound, John, Jaeger, David A. and Regina Baker. (1995). Problems with instrumental variables when the correlation between the instruments and the endogenous explanatory variable is weak. *Journal of the American Statistical Association*, vol. 90, pp. 443-450.

Cameron, Stephen and James Heckman. (1998). Life cycle schooling and dynamic selection bias: Models and evidence for five cohorts of American males. *Journal of Political Economy,* vol. 106, pp. 263-333.

Cameron, Stephen and Christopher Taber. (2000). Borrowing constraints and the returns to schooling. NBER Working Paper 7761. Cambridge, Massachusetts.

Case, Anne, Fen I Lin and Sara McLanahan. (2000). How hungry is the selfish gene? *Economic Journal,* vol. 110, pp. 781-804.

Case, Anne, Fen I Lin and Sara McLanahan. (2000). Educational attainment in blended families. NBER Working Paper 7874. Cambridge, Massachusetts.

Dawkins, Richard. (1976). *The Selfish Gene.* Oxford University Press, New York.

Ermisch, John and Marco Francesconi. (2000). Family matters: Impacts of family background on educational attainments. *Economica*, forthcoming.

Griliches, Zvi. (1977). Estimating the returns to schooling: some econometric problems, *Econometrica*, vol. 45, pp. 1-22.

Hauser, Robert M., Taissa S. Hauser, Deborah Carr, Jennifer Sheridan. (1996). Wisconsin's class of 1957 and their families in the 1990s. CDE Working Paper 96-12. Center for Demography and Ecology, University of Wisconsin-Madison.

Haveman, Robert and Barbara Wolfe. (1995). The determinants of children attainments: A review of methods and findings. *Journal of Economic Literature*, vol. 33, pp. 1829-1878.

Mayer, Susan E. (1997). *What Money Can't Buy: Family Income and Children's Life Changes.* Harvard University Press, Cambridge.

Plug, Erik J. S., Bernard M.S. van Praag and Joop Hartog. (1999). If we knew ability, how would we tax individuals? *Journal of Public Economics*, vol. 72, pp. 183-211.

Plug, Erik J.S. and Wim P.M. Vijverberg. (2000). Schooling, family background, and adoption: Is it nature or is it nurture? Scholar working paper series WP161000.

Sacerdote, Bruce. (2000). The nature and nurture of economic outcomes. NBER Working Paper 7949. Cambridge, Massachusetts.

Scarr, Sandra and Richard A. Weinberg. (1978). The influence of family background on intellectual attainment. *American Sociological Review*, vol. 43, pp. 674-692.

Sewell, William H., and Robert M. Hauser. (1992). A review of the Wisconsin longitudinal study of social and psychological factors in aspiration and achievement, 1963-1993. CDE Working Paper 92-01. Center for Demography and Ecology, University of Wisconsin-Madison.

Shea, John. (2000). Does parent's money matter? *Journal of Public Economics*, vol. 77, pp. 155-184.

Taubman, Paul. (1989). Role of parental income in educational attainment. *The American Economic Review,* vol. 79, pp. 57-61.

Vijverberg, Wim P.M. 1997. Monte Carlo evaluation of multivariate normal probabilities. *Journal of Econometrics*, vol. 76, pp. 281-307.

A The sibling model

The children's educational achievement is measured as years of initial schooling. Schooling depends on observables that vary within and across families, $x_{ik} = [z'_{ik}, z'_k]'$, and unobservables that vary within and across families η_{ik}, where i and k indexes individuals and families, respectively.[13] In our sibling model we view heterogeneity due to unobserved family characteristics in the context of a random coefficient model. If the unobservable family components vary stochastically across families we write down

$$h_{ik} = \alpha' z_{ik} + \beta'_k z_k + \eta_{ik} \tag{A.1}$$

where

$$\beta_k = \beta + \eta_k \tag{A.2}$$

[13]Attributes that vary across members within a family are, for example, age of the child or gender. Examples of attributes that vary across families are family income, parental ability and education levels of parents.

Substitution of (A.2) in (A.1) gives a linear schooling function

$$h_{ik} = \alpha' z_{ik} + \beta' z_k + \epsilon_{ik} \tag{A.3}$$

where $\epsilon_{ik} = \eta_{ik} + \eta_k' z_k$. The disturbance terms are normally distributed with means equal to 0 and variances denoted as $Var[\eta_{ik}] = \tau_{ik}^2$ and $Var[\eta_k] = \Gamma$. This implies that the distribution of ϵ_{ik} is normal; its mean is equal to

$$E[\epsilon_{ik}] = E[\eta_{ik} + \eta_k' z_k] = 0 \tag{A.4}$$

and variance is defined by

$$Var[\epsilon_{ik}] = E[\epsilon_{ik}^2] = \tau_{ik}^2 + z_k' \Gamma z_k = \sigma_{ik}^2 \tag{A.5}$$

ϵ_{ik} is independent between households but correlates across members of the same household. The covariance between members i and j of family k is

$$Cov[\epsilon_{ik}, \epsilon_{jk}] = E[\epsilon_{ik}\epsilon_{jk}] = z_k' \Gamma z_k \tag{A.6}$$

Hence, we will estimate a linear schooling function that allows for familywise heteroscedasticity and covariance.

The distribution of ϵ_{ik} in (A.4)-(A.6) is indeed richly parameterized. This represents a drawback for the iterative maximization of the log-likelihood function defined below, as there is a distinct possibility that the iterated value of σ_k^2 (not to mention the final estimate) becomes negative for at least some k. This derails the maximization procedure. For this reason, we respecify the distributional assumption by allowing for familywise heteroscedasticity in the following manner:[14]

$$\sigma_{ik}^2 = \exp(\gamma_i) + \exp(\gamma' z_k) \tag{A.7}$$

The variance depends on individual characteristics through γ_i, which in our analysis differs only by the child's gender. The component of the variance that owes to the heterogeneity in unobserved family characteristics (η_k above) is given by $\exp(\gamma' z_k)$. Consequently the within-family correlation ρ_k between family members i and j may be defined as

$$\rho_k = \frac{\exp(\gamma' z_k)}{[\exp(\gamma_i) + \exp(\gamma' z_k)]^{1/2}[\exp(\gamma_j) + \exp(\gamma' z_k)]^{1/2}} \tag{A.8}$$

The use of exponentiation ensures positive values both for the variance σ_{ik}^2 and the correlation ρ_k.[15]

[14] The vector z_k does not include a constant. This constant would be only weakly identified, as γ_i already anchors the average variance.

[15] Individual characteristics determine the variance but not the correlation coefficient because the latter is driven by family variables that are common across siblings. Overall, one might wish to simplify the model by omitting this complicated covariance structure. The estimation results strongly suggest that the heteroskedasticity and correlation characteristics of the covariance structure are empirically meaningful. Thus, a simpler model with an i.i.d. assumption would not yield consistent parameter estimates, owing to the frequent censoring on years of schooling.

We now turn to the derivation of the likelihood function. For reasons explained below, we consider a family with two children. Children who are still in school constitute censored observations and will be treated accordingly in our empirical analysis. Based on this information, we must make a distinction between three types of families: (i) those where all children have completed their school career; (ii) families where one of the children is still in school; and (iii) families where all children are still in school. For the first group the contribution to the likelihood function is

$$L_k^{(1)} = f(\epsilon_{ik}, \epsilon_{jk}) = \phi_2(\epsilon_{ik}/\sigma_{ik}, \epsilon_{jk}/\sigma_{jk}, \rho_k)/\sigma_{ik}\sigma_{jk} \tag{A.9}$$

where $\phi_2(.,.,\rho_k)$ is the standard bivariate normal probability density function (pdf) with correlation coefficient ρ_k. For families where one of the children has not completed school yet, we have a censored schooling variable resulting in a different schooling distribution. For a child still in school we know that his or her schooling career took at least h_{ik}^c years, and we know for certain the total period of schooling will be prolonged beyond h_{ik}^c. In this situation the likelihood function equals

$$L_k^{(2)} = \int_{s_{ik}}^{\infty} f(\epsilon_{ik}, \epsilon_{jk}) d\epsilon_{ik} = \phi_1(\epsilon_{jk})(1 - \Phi_1^c(s_{ik} \mid \epsilon_{jk}))/\sigma_{jk} \tag{A.10}$$

where ϕ_1 is the univariate standard normal pdf, and where

$$s_{ik} = h_{ik}^c - \alpha' z_{ik} - \beta' z_k \tag{A.11}$$

and where Φ_1^c is a conditional univariate standard normal cumulative distribution function (cdf), defined as

$$\Phi_1^c(s_{ik} \mid \epsilon_{jk}) = \Phi_1((s_{ik} + \rho_k \epsilon_{jk})/\sigma_{ik}\sqrt{1 - \rho_k^2}) \tag{A.12}$$

and Φ_1 is the standard normal cdf. Finally, if all children still attend school, the contribution to the likelihood function reads as

$$L_k^{(3)} = \int_{s_{ik}}^{\infty} \int_{s_{jk}}^{\infty} f(\epsilon_{ik}, \epsilon_{jk}) d\epsilon_{ik} d\epsilon_{jk} = \Phi_2(-s_{ik}/\sigma_{ik}, -s_{jk}/\sigma_{jk}, \rho_k) \tag{A.13}$$

where Φ_2 is the bivariate standard normal cdf with correlation ρ_k. Together, the equations (A.9), (A.10) and (A.13) summed over the respective household types form the likelihood function.

If a family has only one child or has more than two children, the likelihood function can be derived along similar lines. Conceptually, this is not difficult, but there are major practical obstacles. One is the censoring of the dependent variable: for large families, censoring generates a multidimensional normal probabilities.[16] To simplify the analysis, we restrict the sample to families with at

[16] High-dimensional normal probabilities may be evaluated with simulation techniques; e.g., see Vijverberg (1997). However, with different households offering different dimensions, this is a daunting programming task, which we leave for future research.

least two siblings, and if a family has more than two children we randomly select two for the analysis. This greatly reduces the complexity of the programming effort and comes only at the cost of diminished precision and a small amount of randomness in the outcomes of the investigation.

B Family characteristics and educational outcomes of children

We explain children's educational outcomes with the usual set of variables such as family income and other family background characteristics while adding the IQ measure of ability, but in Section 4 we only discuss the income coefficients. This Appendix reports on the effects of other individual, family and ability controls tabulated in Table B.

Among individual-level determinants we find that younger children invest more in human capital than older ones, and that daughters stay in school somewhat longer. Having brothers or sisters has a negative effect on the educational attainment of children. All individual effects have signs and magnitudes similar to other recent work (see Haveman and Wolfe, 1995).

Among family-level variables we find, not surprisingly, that high scores on childhood IQ tests (of either mother or father) raises the number of years of schooling, and that highly educated parents stimulate their children's education. The level of education of the mother seems somewhat more closely related to the educational attainment of the child than is that of the father. This is in line with what is usually observed (Haveman and Wolfe, 1995; Ermisch and Francesconi, 2000). Statistically, however, the effect of both father's and mother's education are the same. We suspect that these findings may not be all that robust, owing to the sampling design that is inherent in the Wisconsin Longitudinal Survey: by design, the sample only includes families where one of the parents is at least a high school graduate. Since more education raises labor market attachment, mothers in our sample will probably spend relatively more time working and less time raising her offspring than the average U.S. (or Wisconsin) mother, which might explain our findings.

Note that all our estimates are about the same for both the full and sibling sample.[17] This we cannot say for our adoption sample estimates; see column 3, Table B. In terms of the effects of individual variables, family income and father's education, adopted children are mostly quite similar. The major differences concern the parental IQ measure and the years of education of the mother. The effect of parental IQ fully disappears. This is not surprising since for adopted

[17]Similarly, for all other income specifications (i.e., income measured in 1992, income components that reflect on market luck experienced in 1975 and 1992) the estimated coefficients for all other individual and family characteristics remain about the same.

children the genetic transfers within the rearing family are absent. Plug and Vijverberg (2000) use a somewhat different model, with different assumptions, and estimate that about 80 percent of all IQ transmissions run through the genes. The negligible impact of the years of education of the mother is difficult to rationalize.

Table 1: Descriptive statistics on children in the WLS sample

	all children		siblings		adopted children	
children characteristics:						
years of education	13.238	*2.597*	13.337	*2.543*	12.519	*2.742*
still in school (censored)	0.229	*0.420*	0.221	*0.415*	0.367	*0.482*
gender (daughter)	26.231	*5.171*	0.482	*0.499*	0.480	*0.500*
age	0.488	*0.499*	26.399	*5.062*	23.643	*5.396*
number of children	13626		6460		549	
family characteristics:						
number of siblings	2.245	*1.511*	2.323	*1.339*	2.019	*1.561*
gender (mother)	0.489	*0.499*	0.484	*0.499*	0.381	*0.486*
IQ parent	10.161	*1.418*	10.064	*1.406*	10.398	*1.457*
education of father in years	13.627	*2.666*	13.422	*2.541*	14.284	*2.809*
education of mother in years	12.915	*1.763*	12.810	*1.697*	13.287	*1.992*
log family income 1975	9.698	*0.491*	9.678	*0.486*	9.759	*0.443*
log family income 1992	11.000	*0.656*	10.966	*0.655*	11.111	*0.633*
log ability-free income 1975	0.000	*0.435*	0.000	*0.428*	0.000	*0.374*
log ability-free income 1992	0.000	*0.555*	0.000	*0.552*	0.000	*0.522*
number of families	5365		3230		406	

Standard deviations in italics.

Table 2: The influence of family income on the children's years of schooling

	(1)		(2)		(3)	
A: Censored regression model: All children, $N = 13626$						
log income 1975	0.376	*0.040****			0.255	*0.042****
log income 1992			0.363	*0.031****	0.299	*0.033****
B: Sibling model: Siblings, $N = 6460$						
log income 1975	0.371	*0.068****			0.242	*0.072****
log income 1992			0.374	*0.050****	0.315	*0.053****
C: Censored regression model: Adopted children, $N = 549$						
log income 1975	0.403	*0.241**			0.232	*0.263*
log income 1992			0.403	*0.189***	0.330	*0.207*

Standard errors in italics; * significant at 10% level,** significant at 5% level, et cetera. The estimated models also include individual and family variables.

Table 3: Estimating ability-free income measures in 1975 and 1992

A: Estimating family income using observed ability and human capital characteristics

WLS samples:	**all households 1975**		**all households 1992**		**sibling households 1975**		**sibling households 1992**	
log family income:								
intercept	8.524	*0.057****	8.879	*0.073****	8.466	*0.074****	8.825	*0.095****
female	-0.066	*0.012****	-0.131	*0.016****	-0.045	*0.016****	-0.142	*0.020****
IQ parent	0.028	*0.004****	0.059	*0.006****	0.025	*0.006****	0.064	*0.008****
education of father	0.034	*0.002****	0.057	*0.003****	0.041	*0.004****	0.061	*0.005****
education of mother	0.026	*0.004****	0.048	*0.005****	0.023	*0.005****	0.044	*0.007****
education of grandfather	0.009	*0.002****	0.007	*0.003****	0.008	*0.003****	0.007	*0.004**
education of grandmother	0.001	*0.002****	0.008	*0.003****	0.002	*0.003*	0.007	*0.004**
R^2	0.111		0.192		0.120		0.197	
N	5365		5365		3230		3230	

Table 3 continued: Estimating ability-free income measures in 1975 and 1992

B: Estimating ability-free income using unobserved ability and human capital characteristics

WLS samples:	**all households 1975**		**all households 1992**		**sibling households 1975**		**sibling households 1992**	
log unexplained income:								
intercept	0.000	*0.005****	0.000	*0.007****	0.000	*0.007*	0.000	*0.009*
unexplained income 1975			0.429	*0.016****			0.438	*0.021****
unexplained income 1992	0.264	*0.010****			0.264	*0.012****		
R^2	0.113		0.113		0.115		0.115	
N	5365		5365		3230		3230	

Standard errors in italics; * significant at 10% level,** significant at 5% level, *** significant at 1%.

Table 4: The influence of random income shocks on the children's years of schooling

	(1)		(2)		(3)		(4)	
A: Censored regression model: All children, $N = 13626$								
log random income 1975	0.248	*0.042******			0.640	*0.308*****		
log random income 1975×age					-0.013	*0.010*		
log random income 1992			0.301	*0.033******			0.803	*0.246******
log random income 1992×age							-0.018	*0.008*****
B: Sibling model: Siblings, $N = 6460$								
log random income 1975	0.261	*0.073******			0.764	*0.520****		
log random income 1975×age					-0.019	*0.018*		
log random income 1992			0.321	*0.053******			0.900	*0.379******
log random income 1992×age							-0.020	*0.013****

Standard errors in italics; * significant at 10% level,** significant at 5% level, et cetera.

Table B: The influence of all family characteristics on the children's years of schooling

WLS samples:	**all children**		**siblings**		**adopted children**	
years of schooling:						
intercept	5.257	*0.420******	4.744	*0.682******	8.276	*2.307******
daughter	0.158	*0.037******	0.125	*0.053******	0.204	*0.199*
age	-0.070	*0.005******	-0.075	*0.007******	-0.037	*0.023*
number of siblings	-0.107	*0.011******	-0.108	*0.022******	-0.165	*0.066*****
IQ of parent	0.128	*0.014******	0.156	*0.023******	-0.105	*0.075*
education father	0.227	*0.009******	0.239	*0.016******	0.267	*0.045******
education mother	0.230	*0.014******	0.262	*0.024******	0.004	*0.063*
log income 1975	0.376	*0.040******	0.371	*0.068******	0.403	*0.241****
family-dependent variance and correlation:						
intercept			1.193	*0.024******		
daughter			-0.187	*0.030******		
number of siblings			-0.054	*0.040****		
IQ of parent			0.027	*0.044*		
education father			0.085	*0.020******		
education mother			0.110	*0.029******		
log income 1975			-0.269	*0.060******		
Mean loglikelihood	-1.728		-3.435		-1.506	
number of observations	13626		6460		549	

Standard errors in italics; * significant at 10% level,** significant at 5% level, et cetera.

IZA Discussion Papers

No	Author(s)	Titel	Area	Date
161	V. Gimpelson D. Treisman G. Monusova	Public Employment and Redistributive Politics: Evidence from Russia's Regions	4	6/00
162	C. Dustmann M. E. Rochina-Barrachina	Selection Correction in Panel Data Models: An Application to Labour Supply and Wages	6	6/00
163	R. A. Hart Y. Ma	Why do Firms Pay an Overtime Premium?	5	6/00
164	M. A. Shields S. Wheatley Price	Racial Harassment, Job Satisfaction and Intentions to Quit: Evidence from the British Nursing Profession	5	6/00
165	P. J. Pedersen	Immigration in a High Unemployment Economy: The Recent Danish Experience	1	6/00
166	Z. MacDonald M. A. Shields	The Impact of Alcohol Consumption on Occupational Attainment in England	5	6/00
167	A. Barrett J. FitzGerald B. Nolan	Earnings Inequality, Returns to Education and Immigration into Ireland	5	6/00
168	G. S. Epstein A. L. Hillman	Social Harmony at the Boundaries of the Welfare State: Immigrants and Social Transfers	3	6/00
169	R. Winkelmann	Immigration Policies and their Impact: The Case of New Zealand and Australia	1	7/00
170	T. K. Bauer K. F. Zimmermann	Immigration Policy in Integrated National Economies	1	7/00
171	C. Dustmann F. Windmeijer	Wages and the Demand for Health – A Life Cycle Analysis	5	7/00
172	D. Card	Reforming the Financial Incentives of the Welfare System	3	7/00
173	D. S. Hamermesh	Timing, Togetherness and Time Windfalls	5	7/00
174	E. Fehr J.-R. Tyran	Does Money Illusion Matter? An Experimental Approach	7	7/00
175	M. Lofstrom	Self-Employment and Earnings among High-Skilled Immigrants in the United States	1	7/00
176	O. Hübler W. Meyer	Industrial Relations and the Wage Differentials between Skilled and Unskilled Blue-Collar Workers within Establishments: An Empirical Analysis with Data of Manufacturing Firms	5	7/00

177	B. R. Chiswick G. Repetto	Immigrant Adjustment in Israel: Literacy and Fluency in Hebrew and Earnings	1	7/00
178	R. Euwals M. Ward	The Renumeration of British Academics	5	7/00
179	E. Wasmer P. Weil	The Macroeconomics of Labor and Credit Market Imperfections	2	8/00
180	T. K. Bauer I. N. Gang	Sibling Rivalry in Educational Attainment: The German Case	5	8/00
181	E. Wasmer Y. Zenou	Space, Search and Efficiency	2	8/00
182	M. Fertig C. M. Schmidt	Discretionary Measures of Active Labor Market Policy: The German Employment Promotion Reform in Perspective	6	8/00
183	M. Fertig C. M. Schmidt	Aggregate-Level Migration Studies as a Tool for Forecasting Future Migration Streams	1	8/00
184	M. Corak B. Gustafsson T. Österberg	Intergenerational Influences on the Receipt of Unemployment Insurance in Canada and Sweden	3	8/00
185	H. Bonin K. F. Zimmermann	The Post-Unification German Labor Market	4	8/00
186	C. Dustmann	Temporary Migration and Economic Assimilation	1	8/00
187	T. K. Bauer M. Lofstrom K. F. Zimmermann	Immigration Policy, Assimilation of Immigrants and Natives' Sentiments towards Immigrants: Evidence from 12 OECD-Countries	1	8/00
188	A. Kapteyn A. S. Kalwij A. Zaidi	The Myth of Worksharing	5	8/00
189	W. Arulampalam	Is Unemployment Really Scarring? Effects of Unemployment Experiences on Wages	3	8/00
190	C. Dustmann I. Preston	Racial and Economic Factors in Attitudes to Immigration	1	8/00
191	G. C. Giannelli C. Monfardini	Joint Decisions on Household Membership and Human Capital Accumulation of Youths: The role of expected earnings and local markets	5	8/00
192	G. Brunello	Absolute Risk Aversion and the Returns to Education	5	8/00
193	A. Kunze	The Determination of Wages and the Gender Wage Gap: A Survey	5	8/00
194	A. Newell F. Pastore	Regional Unemployment and Industrial Restructuring in Poland	4	8/00

195	F. Büchel A. Mertens	Overeducation, Undereducation, and the Theory of Career Mobility	5	9/00
196	J. S. Earle K. Z. Sabirianova	Equilibrium Wage Arrears: A Theoretical and Empirical Analysis of Institutional Lock-In	4	9/00
197	G. A. Pfann	Options to Quit	1	9/00
198	M. Kreyenfeld C. K. Spiess G. G. Wagner	A Forgotten Issue: Distributional Effects of Day Care Subsidies in Germany	3	9/00
199	H. Entorf	Rational Migration Policy Should Tolerate Non-Zero Illegal Migration Flows: Lessons from Modelling the Market for Illegal Migration	1	9/00
200	T. Bauer G. S. Epstein I. N. Gang	What are Migration Networks?	1	9/00
201	T. J. Dohmen G. A. Pfann	Worker Separations in a Nonstationary Corporate Environment	1	9/00
202	P. Francois J. C. van Ours	Gender Wage Differentials in a Competitive Labor Market: The Household Interaction Effect	5	9/00
203	J. M. Abowd F. Kramarz D. N. Margolis T. Philippon	The Tail of Two Countries: Minimum Wages and Employment in France and the United States	5	9/00
204	G. S. Epstein	Labor Market Interactions Between Legal and Illegal Immigrants	1	10/00
205	A. L. Booth M. Francesconi J. Frank	Temporary Jobs: Stepping Stones or Dead Ends?	1	10/00
206	C. M. Schmidt R. Baltussen R. Sauerborn	The Evaluation of Community-Based Inter-ventions: Group-Randomization, Limits and Alternatives	6	10/00
207	C. M. Schmidt	Arbeitsmarktpolitische Maßnahmen und ihre Evaluierung: eine Bestandsaufnahme	6	10/00
208	J. Hartog R. Winkelmann	Dutch Migrants in New Zealand: Did they Fare Well?	1	10/00
209	M. Barbie M. Hagedorn A. Kaul	Dynamic Effciency and Pareto Optimality in a Stochastic OLG Model with Production and Social Security	3	10/00
210	T. J. Dohmen	Housing, Mobility and Unemployment	1	11/00

211	A. van Soest M. Das X. Gong	A Structural Labour Supply Model with Nonparametric Preferences	5	11/00
212	X. Gong A. van Soest P. Zhang	Sexual Bias and Household Consumption: A Semiparametric Analysis of Engel Curves in Rural China	5	11/00
213	X. Gong A. van Soest E. Villagomez	Mobility in the Urban Labor Market: A Panel Data Analysis for Mexico	1	11/00
214	X. Gong A. van Soest	Family Structure and Female Labour Supply in Mexico City	5	11/00
215	J. Ermisch M. Francesconi	The Effect of Parents' Employment on Children's Educational Attainment	5	11/00
216	F. Büchel	The Effects of Overeducation on Productivity in Germany — The Firms' Viewpoint	5	11/00
217	J. Hansen R. Wahlberg	Occupational Gender Composition and Wages in Sweden	5	11/00
218	C. Dustmann A. van Soest	Parametric and Semiparametric Estimation in Models with Misclassified Categorical Dependent Variables	1	11/00
219	F. Kramarz T. Philippon	The Impact of Differential Payroll Tax Subsidies on Minimum Wage Employment	5	11/00
220	W. A. Cornelius E. A. Marcelli	The Changing Profile of Mexican Migrants to the United States: New Evidence from California and Mexico	1	12/00
221	C. Grund	Wages as Risk Compensation in Germany	5	12/00
222	W.P.M. Vijverberg	Betit: A Family That Nests Probit and Logit	7	12/00
223	M. Rosholm M. Svarer	Wages, Training, and Job Turnover in a Search-Matching Model	1	12/00
224	J. Schwarze	Using Panel Data on Income Satisfaction to Estimate the Equivalence Scale Elasticity	3	12/00
225	L. Modesto J. P. Thomas	An Analysis of Labour Adjustment Costs in Unionized Economies	1	12/00

226	P. A. Puhani	On the Identification of Relative Wage Rigidity Dynamics: A Proposal for a Methodology on Cross-Section Data and Empirical Evidence for Poland in Transition	4/5	12/00
227	L. Locher	Immigration from the Eastern Block and the former Soviet Union to Israel: Who is coming when?	1	12/00
228	G. Brunello S. Comi C. Lucifora	The College Wage Gap in 10 European Countries: Evidence from Two Cohorts	5	12/00
229	R. Coimbra T. Lloyd-Braga L. Modesto	Unions, Increasing Returns and Endogenous Fluctuations	1	12/00
230	L. Modesto	Should I Stay or Should I Go? Educational Choices and Earnings: An Empirical Study for Portugal	5	12/00
231	G. Saint-Paul	The Economics of Human Cloning	5	12/00
232	E. Bardasi M. Francesconi	The Effect of Non-Standard Employment on Mental Health in Britain	5	12/00
233	C. Dustmann C. M. Schmidt	The Wage Performance of Immigrant Women: Full-Time Jobs, Part-Time Jobs, and the Role of Selection	1	12/00
234	R. Rotte M. Steininger	Sozioökonomische Determinanten extremistischer Wahlerfolge in Deutschland: Das Beispiel der Europawahlen 1994 und 1999	3	12/00
235	W. Schnedler	Who gets the Reward? An Empirical Exploration of Bonus Pay and Task Characteristics	5	12/00
236	R. Hujer M. Caliendo	Evaluation of Active Labour Market Policy: Methodological Concepts and Empirical Estimates	6	12/00
237	S. Klasen I. Woolard	Surviving Unemployment without State Support: Unemployment and Household Formation in South Africa	3	12/00
238	R. Euwals A. Börsch-Supan A. Eymann	The Saving Behaviour of Two Person Households: Evidence from Dutch Panel Data	5	12/00
239	F. Andersson K. A. Konrad	Human Capital Investment and Globalization in Extortionary States	5	01/01

240	W. Koeniger	Labor and Financial Market Interactions: The Case of Labor Income Risk and Car Insurance in the UK 1969-95	5	01/01
241	W. Koeniger	Trade, Labor Market Rigidities, and Government-Financed Technological Change	2	01/01
242	G. Faggio J. Konings	Job Creation, Job Destruction and Employment Growth in Transition Countries in the 90's	4	01/01
243	E. Brainerd	Economic Reform and Mortality in the Former Soviet Union: A Study of the Suicide Epidemic in the 1990s	4	01/01
244	S. M. Fuess, Jr. M. Millea	Pay and Productivity in a Corporatist Economy: Evidence from Austria	5	01/01
245	F. Andersson K. A. Konrad	Globalization and Human Capital Formation	5	01/01
246	E. Plug W. Vijverberg	Schooling, Family Background, and Adoption: Does Family Income Matter?	5	01/01

An updated list of IZA Discussion Papers is available on the center‘s homepage *www.iza.org*.