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Children of the Post-Communist Transition: Age at the Time of the Parents' Job Loss and Dropping Out of Secondary School

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Children of the Post-Communist Transition: Age at the Time of the Parents' Job Loss and Dropping Out of Secondary School*

Gabor Kertesi and Gabor Kezdi

Abstract

Using data on children whose parents lost their jobs during the post-communist transition of Hungary, we address the causal effect of unexpected long-term unemployment of parents on their children's educational achievement. We estimate the effect of the children's age at the time of their parents' job loss on their probability of dropping out of secondary school (an event that follows the parents' job loss by many years). The treatment is an additional year reared in a family with regularly employed parents, which can be interpreted as additional human capital investment. We provide bounding estimates to the causal effect. The estimated bounds are tight, they show a substantial effect, and the effect is significantly stronger for preschool age children than for older ones.

KEYWORDS: childhood poverty, skill formation, dropping out of school

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1. Introduction

Whether parental employment and childhood poverty have significant causal effects on educational outcomes is still unclear (Ludwig and Mayer, 2006). The results of Mayer (1997) and Shea (2000) suggest at most modest effects, while Chevalier, Harmon, O'Sullivan and Walker (2005) and Oreopoulos, Page and Stevens (2005) find strong effects. An important reason for inconclusive and conflicting results is the fact that it is hard to find exogenous variation in family conditions. Unobserved parental characteristics that are related to labor market outcomes are likely to directly affect their children's education outcomes as well. If one also wants to know whether the effect varies with the age of the children, variation in family conditions should also be unexpected – another condition rarely met in usual circumstances.

This paper tries to circumvent these problems. It estimates the effect of parental employment on children's education, using data on children whose parents lost their jobs during the post-communist transition of Hungary. Our question is whether children are more likely to drop out of secondary school if they were younger when parents lost their jobs. The post-communist transition provides a unique opportunity to analyze the effects of unexpected job losses because in communist economies, virtually all working-age people were employed in stable jobs. This paper looks at Hungary, the country that experienced the fastest and largest job destruction (Svejnar, 2002).

In the analysis, 'treatment' is an additional year reared in a family in which at least one parent had a stable job. We use pooled cross-sectional data with information about the last job loss for those who were not employed when interviewed. We focus on two-parent families whose children were between three and fifteen years of age at the time of their parents' job loss. We do not compare outcomes to a single 'control group' of children whose parents kept their job; instead, we compare outcomes of children who were at different ages at the time of their parents' job loss. An important consequence of focusing on jobless parents is a strong sample selection: parents in these families were jobless for a long time. But selection is on a right-hand side variable: for children not in the sample, the age at the time of the parents' job loss would be greater than their age at observation. Therefore, the sample selection does not bias our estimates if the effect is homogenous. If, on the other hand, the effects are heterogeneous, our estimates show the average effect for the poorest and least productive segment of the society.

If treatment is exogenous, the estimated effect is a reduced form of a series of causal links, from parents' employment to investment in their children's skills, from investments at some earlier age to skills at some later age, and from skills at that later age to the probability of dropping out of secondary school. Our data has no information about earnings, income or wealth, a fact that restricts our focus to the reduced form effect of employment. Exogeneity may not be fully assured in our data, but we identify and estimate upper and lower bounds for the true causal effect. The estimated bounds are tight, and the estimates are strong and robust. The results therefore suggest that all three causal effects are strong.

We also find substantially stronger effects for younger (preschool-age) children. This result has important implications regarding the age-differentials in skill acquisition. James Heckman and co-authors (Heckman, 2006; Carneiro and Heckman, 2003; Cunha, Heckman, Lochner and Masterov, 2006; Cunha and Heckman, 2007) argue that investments into human capital in early childhood provide higher returns than investments in later ages. Whereas their theoretical arguments are compelling, the empirical evidence is somewhat fragmented. Under the conditions we derive later, our analysis provides a comprehensive estimate for the hypothesized relationship for the age range of three to fifteen. Our results support the hypothesis of Heckman and co-authors.

Many studies looked at the effect of childhood poverty on child outcomes using detailed longitudinal datasets from the U.S. Haveman, Wolfe and Spaulding (1991) estimated a strong negative relationship between childhood poverty and high-school completion. Contrary to our results, they also found that the effect of early childhood poverty was weaker than the effect of later poverty. Duncan and Brooks-Gunn (1997), as well as Duncan, Yeung, Brooks-Gunn and Smith (1998) also estimated strong association between family poverty and educational outcomes. They found the association to be stronger for poverty during early childhood, in line with our results. They also found that the association was especially strong among poor families. Using Canadian data, Oreopoulos, Page and Stevens (2005) looked at the effect of parents’ job displacement on their children’s labor market outcomes. They found a strong negative effect on earnings, identified from the bottom of the earnings distribution of the parents. They did not look at whether the effect varied with respect to children’s age. All of the above results are reduced form in the same sense as ours: they have no direct measures of investments into human capital, and they infer children’s’ skills from subsequent outcomes. Our paper complements these studies by making use of a different source of identification. The identification strategy parallels the studies on the effects of economic hardship on child outcomes caused by the Great Depression (Elder, 1974) or the collapse of farming in the Midwest in the 1980’s (Conger and Elder, 1994), but our analysis is more formal.

The remainder of the paper is structured the following way. The next section shows how our estimates fit into the life-cycle model of skill formation developed by Heckman and co-authors. The third section describes the data, and the fourth section introduces the measurement strategy and discusses identification issues. The fifth section presents the results, and the last part concludes.

2. Theoretical background

Our estimates identify a link between unexpected long-term unemployment of parents of preschool or elementary school age children on the one hand, and the probability of the children dropping out of secondary school on the other hand. A model with age of the child at the time of the parents' job loss as a right-hand-side variable estimates the effect of an additional year of parental employment on the dropout probability. As we shall derive in this section, that effect is a reduced form of a series of causal links.

In the economics literature, skill acquisition is usually modeled as the result of investments into human capital. As will be shown below, under certain conditions, evidence of stronger reduced-form effect on the dropout probability at younger ages indicates higher returns to human capital investment into children when they are younger. In order to identify those conditions, we invoke the theoretical model of dynamic skill formation developed by Heckman and co-authors (Cunha, Heckman, Lochner and Masterov, 2006; Cunha and Heckman, 2007). We lay out a multi-period version of their model with a single skill dimension, and the measured effect will be expressed within this framework. We start at the individual level and arrive at average estimates at the end of the section.

Skill is one-dimensional and is the basis of human capital, which gets rewarded in the labor market in adult life. In this one-dimensional case, skills and human capital can be treated as equivalent without loss of generality. Time is discrete. Individual *i* at age *t* possesses a measure of skills S_{it}, which is the result of the following production process:

$$(1) \qquad S_{it} = f_t\left(S_{it-1}, I_{it}\right).$$

Skills at time *t* are produced using skills brought from the previous period (S_{it-1}) and investments made during the current period (I_{it}). Everything that happens to the child that affects his or her skills is included in the investment term, I_{it}. In the standard human capital literature these are conscious investments in order to achieve higher earnings in adult life. But no such interpretation is needed for our purposes: I_{it} can include unintended consequences of the parents' behavior or other effects of the family environment if those affect the skill formation of the child. The production function may be different at different ages, but its form is common across individuals.

By recursion, one can arrive at a form in which skills are the result of the initial skill endowment and the series of investments:

$$S_{it} = f_t\left(S_{it-1}, I_{it}\right) = f_t\left(f_{t-1}\left(S_{it-2}, I_{it-1}\right), I_{it}\right), ..., = F_t\left(S_{i0}, I_{i1}, ..., I_{it}\right). \tag{2}$$

Heckman and co-authors postulate that investments at younger age produce higher return than investments at later age. This hypothesis can be formulated as

$$\frac{\partial S_{it}}{\partial I_a} = \frac{\partial F_t\left(S_{i0}, I_{i1} ..., I_{it}\right)}{\partial I_a} > \frac{\partial S_{it}}{\partial I_b} = \frac{\partial F_t\left(S_{i0}, I_{i1} ..., I_{it}\right)}{\partial I_b} \quad \text{if } \; 0 < a < b \leq t . \tag{3}$$

According to their argument, the inequality can be the result of two mechanisms. First, some younger ages may be more "sensitive" than later ages in the sense that the production process is more responsive to investment. Second, even if all ages are equally sensitive, earlier investments may have higher returns if earlier skills do not only have a positive effect on later skill formation but they are also direct complements to later investment. If credit markets are imperfect or incomplete, the result of (3) is that investment in earlier childhood produces higher returns than investments in adolescent or adult years. This consequence is summarized by Figure 1, reproduced from Heckman (2006). One purpose of our measurement is to contrast that conjecture with empirical evidence.

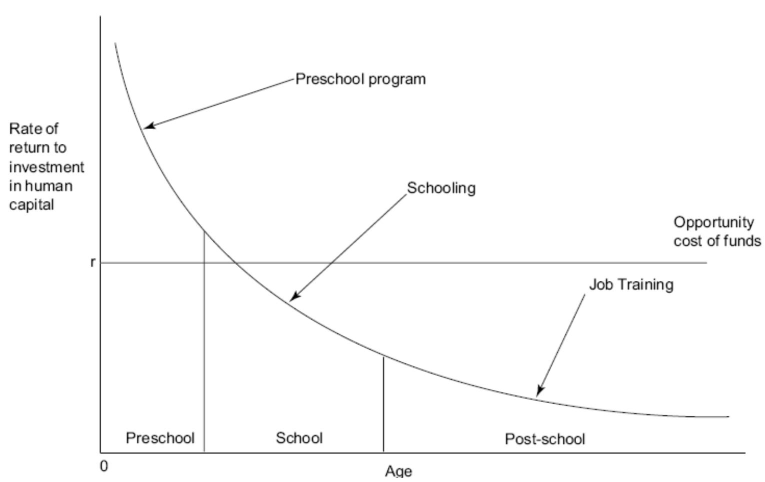

Figure 1. Hypothesized returns to optimal human capital investments by the age of investment. Reprinted from Heckman (2006).

In this paper we estimate the effect of one year delay in the parents' job loss on the dropout probability of their children years later. The first question is the overall magnitude of the effect. The second question is whether the effect varies with the age of the child at the time of the parents' job loss.

Let p_{it} denote the probability that individual i drops out of school at age t. Young people drop out of school if their skill level S_{it} is less than what would be required for (or optimal for) continuing with the studies. We model dropping out as a decision that takes individual-specific alternatives into account, and allow the skill threshold to vary from individual to individual by an independent additive term ε. The only feature of this model that is needed for our empirical analysis is that lower skills lead to a higher dropout probability. This negative relationship is allowed to vary with the age (t) but not across individuals of the same age:

$$(4) \qquad p_{it} = p_{it}\left(S_{it}, q_t\right) = \Pr\left(S_{it} < q_t + \varepsilon_{it}\right) \text{ so that } \frac{\partial p_{it}}{\partial S_{it}} < 0 .$$

Let E_{ia} denote the parents' employment status when the child is a years old: E_{ia} = 1 if at least one parent works at age t, and E_{ia} = 0 otherwise. We shall measure the dropout probability as a *reduced form* function of the age at the time of the parents' job loss (A) in the following way:

$$(5) \qquad p_{it}\left(A = a\right) = p_{it}\left[S_{it}\left(E_{i0} = 1, .., E_{ia-1} = 1, E_{ia} = 0, ..., E_{it} = 0\right) < q_t + \varepsilon_{it}\right]$$

The thought experiment at the individual level links dropout probability to the age at the time of the parents' job loss. In discrete time, it identifies the difference between two dropout probabilities, in which the age at job loss is one year apart:

(6)

$$\begin{aligned} \alpha_{ia} &\equiv p_{it}\left(A = a+1\right) - p_{it}\left(A = a\right) \\ &= p_{it}\left[S_{it}\left(.., E_{ia} = 1, E_{ia+1} = 0, ...\right) < q_t + \varepsilon_{it}\right] - p_{it}\left[S_{it}\left(.., E_{ia-1} = 1, E_{ia} = 0, ...\right) < q_t + \varepsilon_{it}\right] \end{aligned}$$

Assume that investment is the function only of current employment and not the entire history. If job loss is unexpected, as will be in our data, future job loss should have no effect on current investments. As a result, investments prior to age a are the same whether job loss occurred at age a or a+1.[1] In this case, we

[1] In reality, past employment history may have cumulative effects on current investments. That would result in investments after age a+1 to be different for job loss at age a versus a+1. With the assumption that only current employment matters for investment, we rule out that possibility, and assume that future investments are also the same. This obviously strict assumption is in fact not

can define investment at time s if parents are employed, parents not employed, and their difference the following way:

$$(7)\qquad \begin{aligned} &I_{ia}^{+} \equiv I_{ia} \mid E_{ia} = 1, \quad I_{ia}^{-} \equiv I_{ia} \mid E_{ia} = 0, \\ &\Delta I_{ia} \equiv I_{ia}^{+} - I_{ia}^{-} \end{aligned} .$$

The dropout probability differential in the thought experiment can be approximated by the product of the investment differential at age a, the effect of investment at age a on skills at t, and the effect of the skill level at t on the dropout probability at t:

$$(8)\qquad \begin{aligned} \alpha_{ia} &= p_{it}\left(A = a+1\right) - p_{it}\left(A = a\right) \\ &= p_{it}\left[S_{it}\left(I_{i0}^{+}..,I_{ia-1}^{+},I_{ia}^{-},I_{ia+1}^{-},...I_{it}^{-}\right) < q_t + \varepsilon_{it} \right] \\ &\quad - p_{it}\left[S_{it}\left(I_{i0}^{+}..,I_{ia-1}^{+}I_{ia}^{+},I_{ia+1}^{-},...I_{it}^{-}\right) < q_t + \varepsilon_{it} \right] \\ &\approx \frac{\partial p_{it}}{\partial S_{it}}\left[S_{it}\left(I_{i0}^{+}..,I_{ia-1}^{+},I_{ia}^{-},I_{ia+1}^{-},...I_{it}^{-}\right) - S_{it}\left(I_{i0}^{+}..,I_{ia-1}^{+}I_{ia}^{+},I_{ia+1}^{-},...I_{it}^{-}\right)\right] \\ &\approx \frac{\partial p_{it}}{\partial S_{it}}\frac{\partial S_{it}}{\partial I_{ia}}\Delta I_{ia} \end{aligned}$$

The parameter in the thought experiment identifies the effect of investment at age a on skills at age t $\left(\partial S_{it}/\partial I_{ia}\right)$, measured in terms of dropout probability $\left(\partial p_{it}/\partial S_{it}\right)$, a negative number, and multiplied by ΔI_{ia}, the investment effect of job loss at age a.

A regression on a sample where age at the time of the parents' job loss is exogenously assigned estimates the average partial effect, i.e. α_{ia} averaged over all individuals i. This average reduced-form effect is the product of the average causal effects provided that individual heterogeneity is uncorrelated:

$$(9)\qquad \begin{aligned} \alpha_a &\equiv \mathrm{E}\left[p_{it}\left(A = a+1\right) - p_{it}\left(A = a\right)\right] \\ &\approx \mathrm{E}\left[\frac{\partial p_{it}}{\partial S_{it}}\frac{\partial S_{it}}{\partial I_{ia}}\Delta I_{ia}\right] = \mathrm{E}\left[\frac{\partial p_{it}}{\partial S_{it}}\right]\mathrm{E}\left[\frac{\partial S_{it}}{\partial I_{ia}}\right]\mathrm{E}\left[\Delta I_{ia}\right] \equiv \frac{\partial p_t}{\partial S_t}\frac{\partial S_t}{\partial I_a}\Delta I_a \end{aligned}$$

necessary, but it simplifies the derivations. Weak dependence of investments on past employment is sufficient, as long as measurement of skills is far enough from the year of the parents' job loss.

Uncorrelated heterogeneity is obviously a strong assumption, one that requires that those who experience the job loss should be similar to those who do not in terms of all three components: the effect of employment on investment, the effect of investment on later skills, and the effect of skills on dropping out. Since we do not expect that to hold in general, we interpret our estimates to be valid only for the population it represents. In other words, one should keep in mind that our estimates are *identified from the relevant segment of the population.* In our case these are children whose parents were at high risk at losing their job during the post-communist transition and who found no job afterwards.

The first question is the magnitude of the reduced-form effect, averaged over all ages, $\alpha = Avg_a\left(\alpha_a\right)$. We expect α to be negative. The effect of skills on the probability of dropping out should be negative; the effect of investments on later skills should be positive; and the effect of employment on investment should also be positive. The magnitude of the reduced-form effect is expressed in terms of dropout probability, which can be interpreted by comparing it to the national average or the dropout rate when $A=a_{max}$. A large magnitude implies that each of the three effects is substantial.

The second question is whether returns to investment in age *a* are greater than returns in age *b*, $\frac{\partial S_t}{\partial I_a} > \frac{\partial S_t}{\partial I_b}$, if $0 < a < b \le t$. The difference in the two estimates that correspond to two different ages of job loss (*a* versus *b*) is the following:

$$(10) \qquad \alpha_a - \alpha_b \approx \frac{\partial p_t}{\partial S_t}\left(\frac{\partial S_t}{\partial I_a}\Delta I_a - \frac{\partial S_t}{\partial I_b}\Delta I_b\right) = \frac{\partial p_t}{\partial S_t}\left(\frac{\partial S_t}{\partial I_a} - \frac{\partial S_t}{\partial I_b}\right)\Delta I \qquad \text{if} \quad \Delta I_a = \Delta I_b \equiv \Delta I\,.$$

In order to interpret larger reduced-form effects at age *a* than age *b* as evidence for larger effects of investments at age *a*, one has to assume that the effect of job loss on employment on investments is the same at age *a* as at age *b*.[2] If the effect of job loss on investment is greater in age *a*, the reduced form effect may be larger even if the effects of investment are the same. Note that this problem is

[2] A counterexample is when *a* is preschool age, *b* is elementary school age, not all children go to preschool, but all children go to elementary school. Then an effect of long-term parental unemployment of a preschool age child may be that the child is not enrolled in preschool, while no such enrollment effect is present for an elementary school age child. If preschool history is not observed, as is the case in our data, differences in the reduced form effect may be the result of differences in investment, not differences in returns. We try to control for this affect by using a proxy, the availability of preschool in the village when the child was preschool-age, but that proxy is probably not perfect.

present in any reduced-form estimate of the timing of family poverty or parental unemployment on children's outcome. While this distinction may not be important for some policy conclusions, it certainly limits the degree to which it provides evidence for age-differences in returns to human capital investment, the question posed by Heckman and co-authors.

3. Data

The data come from pooled cross-sections of the Hungarian Labor Force Survey (HLFS) between 1997 and 2005. HLFS is a large monthly survey of more than 20,000 individuals per month. The survey contains standard questions about demography and employment, and from 1997, it has asked the year of the last regular full-time job of people who were not employed by the time of the interview. Therefore the restriction of the survey for years starting with 1997. The sample for this analysis consists of young people of age 15 to 20 who lived with both of their parents at the time of the interview.

The vast majority of parents were born sometime between 1945 and 1970. Figures 2A to 2C show the dramatic employment decline of this cohort in Hungary. Male employment fell from 98 per cent in 1987 to 80 per cent by 1994. At the same time, female employment fell from 81 to 68 per cent. The decline was concentrated on the 1987-1994 period and was close to linear.[3] Figures 2B and 2C show that the least educated (0-8 years of education) experienced a 25-30 percentage points decline. The figures demonstrate that virtually all men were employed in 1987 regardless of educational attainment, and employment rate differences were small for women as well.

[3] Hungary was special in that layoffs from state-owned firms started a few years before the eventual collapse of the communist system. See Köllő (2000) for more information.

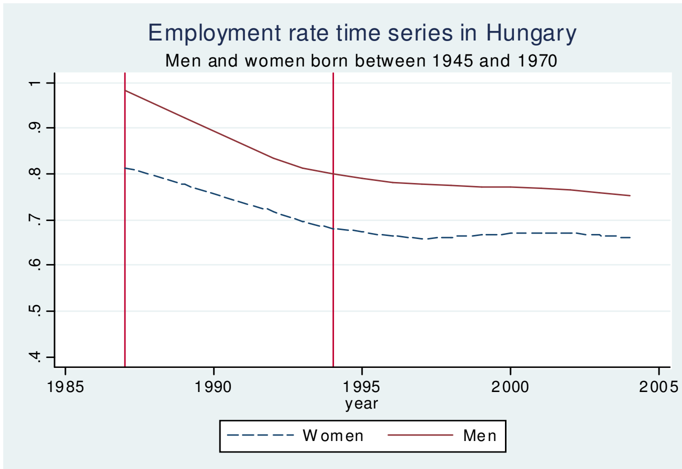

Figure 2A. Employment rate in Hungary, 1987 to 1994. Cohorts born between 1945 and 1970.

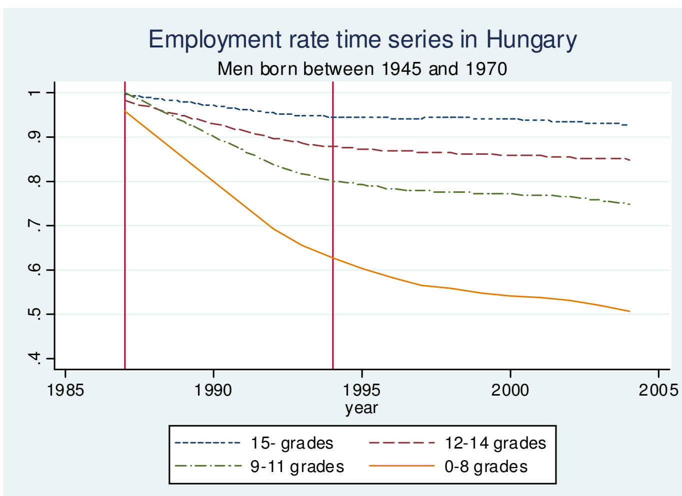

Figure 2B. By educational attainment. Men.

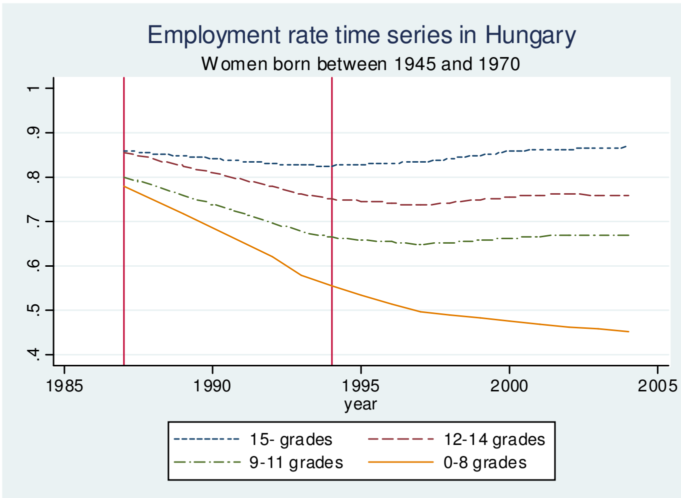

Figure 2C. By educational attainment. Women.

We look at 15-20 year-old children who lived with both of their parents at the time of the interview, and whose parents lost their jobs between 1987-1994 and remained jobless ever since. Restriction to living with both parents is necessary in order to gain information about the parents' employment status and the time of their job loss. Recall that we use data from interviews between 1997 and 2005. In families where parents lost their job in 1992 but were interviewed in 2005, the children (15-20 years old at the interview) were 2-7 years old at the time of their parents' job loss. In families who were observed in 1997, the children were 10-15 years old in 1992. Timing and sample size limit our analysis to children who were at least 3 years old at their parents' job loss.

The outcome measure is dropping out of the school system after eighth grade.[4] Dropouts include those who did not start any vocational or secondary school and those who did start one but did not finish it. This dropout measure is clearly age-specific, a fact we keep in mind throughout the analysis. Dropout status is closely related to adult labor market outcomes. People with eight grades of education are very likely to end up in poverty in post-communist Hungary. Recall that Figures 2B and 2C indicate substantial disadvantage in employment

[4] The Hungarian school system is similar to other former communist countries. Elementary is typically eight grades long, with a modal graduation age of fourteen. After having finished eighth grade, children can chose between three options. They could leave school (legally if over sixteen but practically even before sixteen, as the compulsory age was not strictly enforced for a long time); they could go to a vocational training school (two to four years); or they could go to a "proper" secondary school (four or five years) with a so-called maturity exam at the end. Starting with 1990, some of the best performing pupils enrolled into 6 and 8-year secondary schools after 6th and 4th grade, respectively. The 1990's has seen a major expansion of secondary school enrollment, a trend that will affect our identification strategy.

probability for those without secondary or vocational education. Kertesi and Köllő (2002), Kézdi (2005) and others showed that their earnings have also dropped substantially relative to the more educated. Köllő (2005) showed that the least educated Hungarians are even less employable than their counterparts in the U.S. or Western Europe, and the dominant reason is their lower level of skills. People who drop out of the school system without completed vocational or secondary degree are therefore very likely to stay marginalized in the future.

Table 1. Sample selection and dropout rates

	Observations	Dropout rate
All 15-20 years old who live with at least one parent	47,571	0.072
All 15-20 years old who live with both parents	39,034	0.067
Of them, no parent is employed	4,167	0.215
No parent is employed, job loss 1988-1994	1,081	0.265
Of those, complete observations (final sample)	991	0.264

Steps of the selection of our sample are shown in Table 1. The overall sample size is more than 47,000. Of the 47,000, 39,000 lived with both parents, 4,000 of whom lived in families where no parent had a job. Of the 4,000, parents of about 1100 children lost their jobs between 1988 and 1994. The 991 children with complete observations compose the sample used for estimation. In the estimation sample, the unconditional dropout rate is at 26 per cent, compared to the 7 per cent in the overall sample.

Selection into the estimation sample is very strong, and it is obviously nonrandom. People who lost their jobs at around 1990 and found no employment afterwards are surely from the bottom of the productivity distribution. Missing from our sample are families with parents who were employed at the time of the interview (or lost their job after 1994). The right-hand side variable of interest is the age of the child at the time of the parents' job loss. This variable would be larger than the age of the child at observation for families with employed parents. It would be somewhat less for unemployed parents with recent job loss. Sample selection is therefore on the right-hand side variable of interest. As a result, it does not affect consistency of the estimated effect if the effect is homogenous. If effects are heterogeneous, our estimates show the effect for the poorest and least productive segment of the society.

4. Measurement model and identification

We estimate models for the dropout probability on the sample of 15-20 years old young Hungarians who live with both of their parents and whose parents have no job. The sample is constrained to parents who lost their jobs between 1988 and 1994. The variable of interest is the age at the time of the parents' job loss, defined as the age of the child in the last year when at least on parent had a job. Whether the father's or the mother's job loss matters more will be investigated at the end of the analysis. Dropout probability is age-specific, and therefore we always control for the child's age at the interview (when dropout status is measured).

The measurement model is based on the assumption that the event of dropping out of the school system is a negative function of (unmeasured) skills. More specifically, as introduced in (4), one drops out of school at age t if skills are below a certain level, determined by an age-specific constant and random variation around the constant. We specify a linear model for unmeasured skills S, and assume normally distributed random variation:

$$(11) \qquad p_{it} = \Pr\left(S_{it} < q_t + \varepsilon_{it}\right) \ , \text{ where } \varepsilon_{it} \sim iidN\left(0,1\right) \text{ and } S_{it} = \pi_t ' w_{it}$$

Each individual is observed once, at some age t ($15 \leq t \leq 20$). The dropout probability of individual i is a function of her/his age (t) both because the expected skills threshold, q_t, is a function of t, and because the skill production function may vary with t. We model the second relationship by adding a time-specific constant to the time-invariant effect. The dropout probability then follows the probit model[5]

$$p_{it} = \Phi\left(S_{it} - q_t\right) = \Phi\left(\alpha a_i + \beta ' x_i + \gamma_t - q_t\right).$$

Since γ_t and q_t are not identified separately, the estimation model is reformulated as

$$(12) \qquad p_{it} = \Phi\left(\alpha a_i + \beta ' x_i + \delta ' d_i\right)$$

where d_i is a vector of dummies of the age (t) of the individual (i) at the interview, a_i is age of the individual in the last year when at least one of her/his parents had a job, and the x_i is a vector of additional covariates. The parameter of interest is α. Under the assumption of exogenous variation of a (the age at the time of the parents' job loss), α identifies the reduced form effect of the effect of one year's parental employment on human capital investment, the effect of that investment on skills at age t, and the effect of skills at age t on the dropout probability at age

[5] Logit and linear probability models give essentially the same results.

t. If employment affects investments in a positive way, and skills affect the dropout rate in a negative way, α should be negative.

Besides the overall effect of one year employment's worth of investments in skills, we are interested how that effect may vary with a_i itself. In a more general form, the linear specification αa_i is replaced with a more general function of $\alpha(a_i)$, such as a linear spline or a set of dummy variables. Observable family and individual characteristics will be controlled for in all models. Vector x_i in (12) always contains gender, the parents' age and education, and the number of children in the family who are older than the respondent, the number of children who are younger. For checking robustness, we also use a larger set of covariates with region, city size, and measures of preschool availability in the village/town when the child was at preschool age. The results are very similar to those obtained with a smaller set of covariates.

The most important question is whether exogeneity of a_i is a valid assumption. Our sample covers young people of different ages (15 to 20 old), who were interviewed in different years (1997 to 2005), and whose parents lost their jobs in different years (1987 to 1994). All estimates are conditioned on the age of the child at the time of the interview (d_i), which leaves two sources of variation of age at the time of the parents' job loss: birth year and/or year of job loss. Conditional on age at interview and year of interview, we look at people who were born in the same year. They can differ in terms of their age at the time of the parents' job loss only because that event happened in different calendar years. On the other hand, when we condition on age at interview and the year of the parents' job loss, we look at people whose parents lost their jobs in the same year. They can differ in terms of their age at the time of the parents' job loss only because they were born different years.[6] The question is whether the two alternative sources of variation are exogenous to unobserved skills of the children or anything else that may affect the dropout probability.

The year of the parents' job loss may be positively correlated with the parents' skills if the least productive workers were displaced first. This may introduce endogeneity if there is a direct link from parents' skills to their children's skills. Therefore we expect the magnitude of the estimates identified from year of job loss to be biased upwards (to look stronger than reality). On the other hand, estimates identified from year of birth may lead to a downward bias in their magnitude (they may look weaker than reality). In post-1990 Hungary,

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[6] $\text{Age}_{\text{interview}} = \text{Year}_{\text{interview}} - \text{Year}_{\text{birth}}$ and $\text{Age}_{\text{job loss}} = \text{Year}_{\text{job loss}} - \text{Year}_{\text{birth}}$. Therefore, $\text{Age}_{\text{interview}} = \text{Year}_{\text{interview}} - \text{Year}_{\text{job loss}} + \text{Age}_{\text{job loss}}$. In a regression with $\text{Age}_{\text{job loss}}$ and $\text{Age}_{\text{interview}}$ already on the right-hand side, one can control for either $\text{Year}_{\text{job loss}}$ or $\text{Year}_{\text{interview}}$ but never both. If $\text{Year}_{\text{job loss}}$ is controlled for, variation in $\text{Age}_{\text{job loss}}$ is the result of variation in $\text{Year}_{\text{interview}}$ (due to variation in birth year). If $\text{Year}_{\text{interview}}$ is controlled for, variation in $\text{Age}_{\text{job loss}}$ is the result of variation in $\text{Year}_{\text{job loss}}$.

younger cohorts faced, ceteris paribus, lower dropout probabilities, because of demographics and the rigidity of the secondary school system. Younger cohorts happen to be smaller, and secondary school capacities did not adjust to the smaller number of students. But younger cohorts were younger at the (here fixed) year of the job loss, and therefore the general trends would make their dropout probability look smaller than in a controlled experiment. Figure 3 supports this argument by showing a negative association of birth year and the dropout rate for two groups of children who are not in the sample (both groups have employed parents, educated in one group, uneducated in the other). The two sources of identification therefore form an upper bound and a lower bound for the true effect.[7]

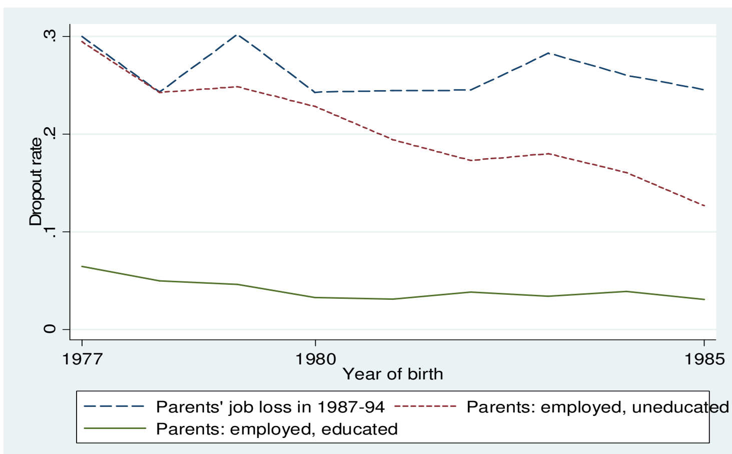

Figure 3. Dropout rate by birth cohort

[77] The estimates are identified from pooled cross-sections of families. Most families have only one eligible child in the sample, and therefore the effect is identified mostly from between-family variation. On the other hand, 120 families have more then one eligible child in the sample. As a robustness check, we estimated within-family models to see whether the effects are similar to the pooled ones. In within-family models the effect is estimated from the dropout difference of younger children in a family relative to older siblings. Such estimates control for all otherwise unobservable parental characteristics and are solely based on cross-cohort identification. On the other hand, their magnitude may be biased upward (look stronger than reality) because most of the older siblings are first-borns, who are likely to receive more resources in poor families. The results show that the within estimates are indeed significantly *larger* than the pooled estimates, whether the estimated model is linear fixed-effects, linear first differences, or correlated random effects probit. At the same time, pooled estimates do not change if siblings are dropped. These results provide additional support to our interpretation that the pooled estimator identified from birth year is a lower bound to the true effect.

5. Results

The estimates are gained from probit models as specified in (12). The parameter of interest is of the age of the child when parents lost their job. Recall that the effect of the age at the job loss can be interpreted as the effect of one more year spent in a family where at least one parent had a stable job. Under the maintained assumptions, α is a reduced form of a series of causal links, from parents' employment to investment into their children's skill, from investments to skills at a later age, and from skills to the probability of dropping out of secondary school.

Estimates from three versions of the model are shown. All versions control for the covariates listed in the previous section. The sample consists of adolescents of age 15 to 20 who live with both of their parents, and whose parents lost their job between 1988 and 1994, and remained non-employed since. In model (1) neither the year of the interview nor the year of the parents' job loss is controlled for. Model (2) controls for the year of interview, while model (3) controls for the year of job loss. Model (2) identifies the effect from variation in calendar year of the parents' job loss, and therefore it may produce estimates biased upwards in magnitude (too strong negative effects). Model (3) identifies the effect from variation in year of birth, and therefore it may produce estimates biased downwards in magnitude (too weak negative effects). Model (1) mixes the two sources of identification and is therefore expected to produce results in between. In order for easier interpretation of predicted values, all right-hand side variables but the age at the job loss (a) are normalized to have zero mean. Age at the time of the parents' job loss is between 3 and 15, and it is normalized so that it takes the value zero at age 3. Standard errors allow for arbitrary correlation of unobservables within the family, in order to take care of the few siblings in the sample.

Table 2. The effect of the child's age at her/his parents' job loss on her/his dropout probability. Probit estimates.

Dependent variable: dropout	**(1)**	**(2)**	**(3)**
Age at the time of the parents' job loss			
Probit coefficients	-0.080	-0.095	-0.058
	[0.020]**	[0.026]**	[0.026]**
Average partial effects	-0.023	-0.027	-0.017
Control variables			
Calendar year of the interview		Yes	
Calendar year of the parents' job loss			Yes
Other controls	Yes	Yes	Yes
Observations	991	991	991
Pseudo R-squared	0.21	0.21	0.21

Sample. Hungarian Labor Force Survey pooled cross-sections of 1997 to 2005 (without July and Aug surveys). Young people of age 15-20 who live with both of their parents, parents lost their job in 1988-1994 and remained non-employed since.
The parents' job loss is the last year when at least one parent had a stable job.
"Other control" variables: gender, age and education of parents, number of children in household (separately for children below 14 and above 14).
Standard errors are clustered at the family level.
* significant at 5%; ** significant at 1%

Table 2 contains the main results (detailed results are in the Appendix). Besides estimated probit coefficients and their standard errors, the table shows each corresponding average partial effect estimate (i.e. the sample average of individual-specific partial or "marginal" effects). The estimates from the three models follow a pattern as expected. The estimates in model (2) are likely to be stronger than the true effect, while those in model (3) are likely to be weaker. The two put a bound on the true effect. The average partial effect of the age of the child at the parents' job loss is about –2 per cent (in absolute terms, lower bound is –1.7, upper bound –2.7 per cent). Children who were one year younger when their parents lost their job experienced a dropout probability that is about 2 per cent higher. The effect is not only statistically significant but it is also substantial. Recall that overall dropout rate is 26 per cent in the selected sample (7 per cent nationally). For a child with average other characteristics, the implied dropout probability is roughly 40 per cent if the child was 3 years old at the job loss, while it is 15 per cent if he/she was 15.

The second question is whether the effect is larger at younger ages. Figure 4A shows predicted dropout probabilities, with age at the time of the parents' job loss entered as year of age dummies. The figure shows estimates for all three models. Estimates from model (1) are between estimates from models (2) and (3), as expected. The three models show the same qualitative relationship. Predictions shown on figure 4A are noisy, but the steep decline between age 4 and 7 is remarkable. In order to show more powerful estimates of the differences, we estimated probit models with a linear spline with a break at age 7.

Table 3. The effect of the child's age at her/his parents' job loss on her/his dropout probability. Probit estimates including linear spline break at age 7.

Dependent variable: dropout	**(1)**	**(2)**	**(3)**
Average partial effects			
Age at the time of the parents' job loss	-0.027	-0.031	-0.022
	[0.006]**	[0.008]**	[0.008]**
Additional effect if age was 2 to 7	-0.026	-0.025	-0.026
	[0.012]*	[0.012]*	[0.012]*
Control variables			
Calendar year of the interview		Yes	
Calendar year of the parents' job loss			Yes
Other controls	Yes	Yes	Yes
Observations	991	991	991
Pseudo R-squared	0.21	0.21	0.21

Notes: see below Table 2.

Table 3 shows the estimates for the age spline parameters in the three specifications. The variables are specified so that the additional variable measures the difference between the overall effect and the effect before (including) age 7. The table shows the average partial effects of the two variables of interest. The estimates of the differential effect for preschool age children are the same in all three models, while the older age effects follow the same pattern as before. As a result, the relative difference is actually slightly larger for model (3) than model (2). The important result from Table 3 is that, in all specifications, the effect before age 7 is about double of the effect after age 7.

Figure 4B shows the predicted probabilities from the linear spline probit models. Figure 4C shows the slope (first derivatives) of the curves on Figure 4B. For easier interpretation, it shows the negative (the absolute value) of the slope. Under the assumptions laid out in section two, this is our empirical counterpart to the age-returns profile popularized by Heckman (2006) – reproduced here in Figure 1. Figure 4C shows our estimates of the age-specific reduced form returns to parental employment on dropout rates. Each model produces downward sloping lines made of two segments, with a break at age 7, and for each model the first segment is significantly steeper. The negative slope of each line segment is a property of the probit specification, and it may (or may not) be the artifact of measuring the effect in terms of a probability. On the other hand, the difference in the slopes is not a result of such an artifact. It shows that the reduced form effect of spending one more year in a family where at least one parent is employed is stronger if that year is between age 3 and 7 than if it is between age 7 and 15.

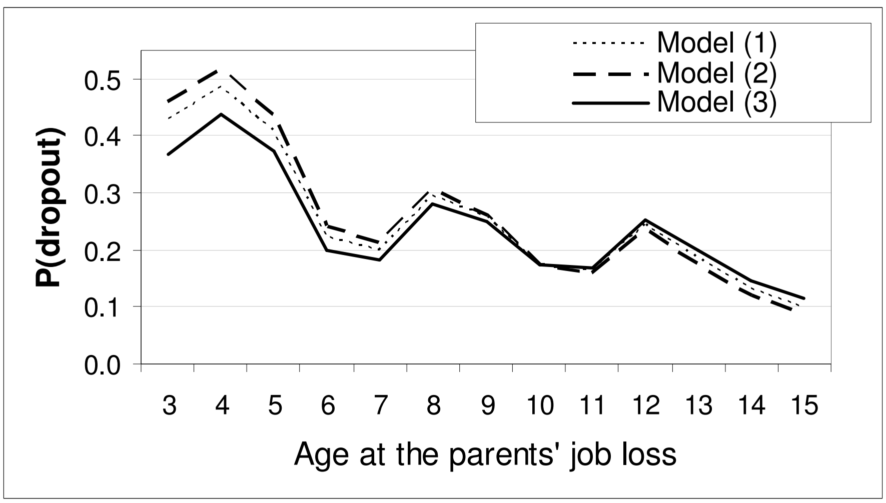

Figure 4A. Predicted probability profiles, from probit with age dummies.

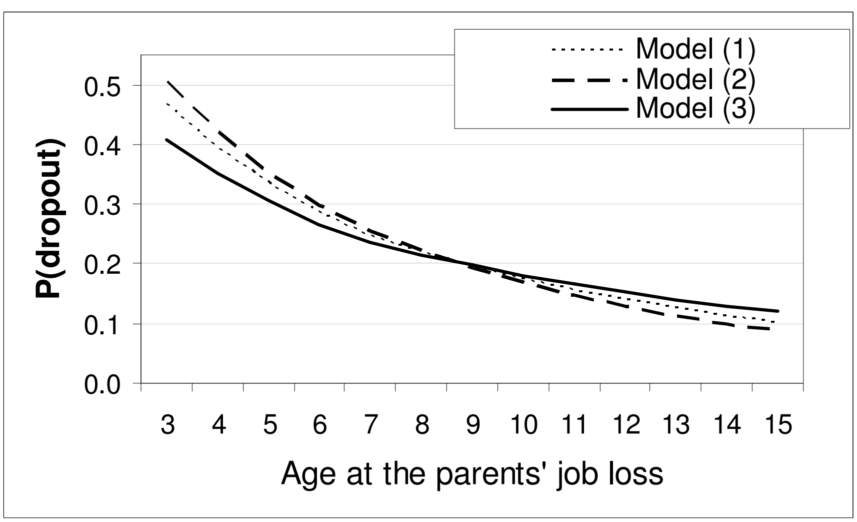

Figure 4B. Predicted probability profiles, from probit with linear spline with age.

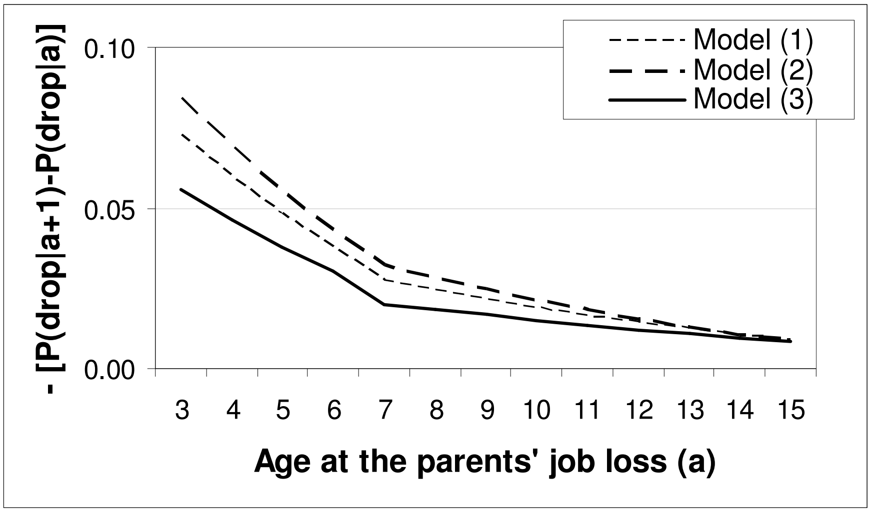

Figure 4C. Absolute value of the slope of the predicted probability profiles, from probit with linear spline with age. Estimates of the age-specific reduced form returns to parental employment on dropout rates.

Table 4 shows results (average partial effects) with separate age at the mothers' and the fathers' job loss. In each specification the two are very close. These results suggest that it is having or not having one wage earner in the family that matters, not the identity of that wage earner. They are therefore consistent with the role of childhood poverty in the causal mechanism from parental employment to skill formation.

Table 4. The effect of the child's age at her/his parents' job loss on her/his dropout probability. Separated for age at the mother's and father's job loss. Probit estimates.

Dependent variable: dropout	(1)	(2)	(3)
Probit coefficients			
Age at the mother's job loss	-0.014	-0.015	-0.014
	[0.004]**	[0.005]**	[0.004]**
Age at the father's job loss	-0.011	-0.013	-0.010
	[0.005]*	[0.006]*	[0.005]*
Control variables			
Calendar year of the interview		Yes	
Calendar year of the parents' job loss			Yes
Other controls	Yes	Yes	Yes
Observations	991	991	991
Pseudo R-squared	0.21	0.21	0.21

Notes: see below Table 2.

All the results in Table 2 and Table 4 are robust to changes in sample selection rules (age of 15 to 20, job loss year of 1988 to 1994, job loss age between 3 and 15 years, interview year of 1997 to 2005). The relative magnitudes between preschool and elementary school age (Table 3) are also robust to such changes but they are sometime significant only at the 10 per cent level.

6. Conclusions

We have shown that the parents' long-term unemployment has a strong, negative causal effect on their children's skill formation. The results are estimated from a sample of children whose parents lost their jobs during the post-communist transition of Hungary and remained jobless. All effects are identified for families with two parents who are likely to be at the bottom of the skill distribution.

We have also shown that the effect is twice as large for children of age three to seven than for older children. This result is consistent with larger returns to human capital investments at earlier ages, an argument put forward by Heckman and co-authors (ibid). Under the assumptions derived above, our results in Figure 4C provide empirical estimates to the hypothesized relationship by Heckman (2006) (Figure 1 in this paper). The estimated curve is close to the hypothesized one, in particular showing larger returns in age three to seven than at later ages.

The results imply that long-lasting negative employment shocks have severe intergenerational consequences. Those consequences are especially severe for younger children. Therefore, it would make sense for subsidized employment programs to focus more on parents with young children. At the same time, however, employment programs are unlikely to provide solid long-term employment. Or, if they can, they are likely to be prohibitively expensive. Communist economies practiced large-scale subsidized employment for low-skilled workers for decades, with ominous economic and political consequences. If providing employment for low skilled parents is prohibitively costly, policy should focus on alleviating the harmful effects in other ways. Early childhood interventions and focused preschool programs may provide promising alternatives. In order for such programs to work, we need to understand the mechanisms behind the strong effect of parental employment on the skill formation of their children.

Appendix: Detailed estimates

Table A1. The effect of the child's age at her/his parents' job loss on her/his dropout probability. Probit coefficients.

	Probit index linear in age			Probit index linear spline in age (break at age 7)		
	(1)	(2)	(3)	(1)	(2)	(3)
Age at the time of the parents' job loss	-0.08	-0.095	-0.058	-0.095	-0.109	-0.074
	[0.020]**	[0.026]**	[0.026]*	[0.022]**	[0.028]**	[0.027]**
Age at the time of the parents' job loss if 3-7 old				-0.089	-0.087	-0.087
				[0.044]*	[0.044]*	[0.044]*
Age at interview	0.224	0.238	0.201	0.216	0.229	0.194
	[0.038]**	[0.041]**	[0.042]**	[0.038]**	[0.041]**	[0.042]**
Female	-0.082	-0.077	-0.075	-0.084	-0.08	-0.078
	[0.098]	[0.098]	[0.098]	[0.098]	[0.098]	[0.098]
Father's education (years)	-0.181	-0.179	-0.179	-0.184	-0.183	-0.183
	[0.033]**	[0.033]**	[0.033]**	[0.033]**	[0.033]**	[0.033]**
Mother's education (years)	-0.257	-0.252	-0.25	-0.257	-0.253	-0.25
	[0.055]**	[0.054]**	[0.054]**	[0.055]**	[0.054]**	[0.054]**
Father's age	-0.013	-0.013	-0.014	-0.013	-0.014	-0.014
	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]
Mother's age	-0.012	-0.012	-0.012	-0.012	-0.012	-0.012
	[0.012]	[0.012]	[0.012]	[0.013]	[0.013]	[0.013]
Number of children 0-14 old	0.189	0.188	0.188	0.193	0.191	0.192
	[0.052]**	[0.052]**	[0.052]**	[0.052]**	[0.052]**	[0.052]**
Number of children above 15	0.143	0.142	0.142	0.14	0.14	0.14
	[0.061]*	[0.062]*	[0.062]*	[0.062]*	[0.062]*	[0.062]*
Year of interview		-0.028			-0.025	
		[0.031]			[0.031]	
Year of parents' job loss			-0.044			-0.042
			[0.034]			[0.034]
Constant	-0.238	-0.126	-0.394	-0.084	0.014	-0.236
	[0.152]	[0.198]	[0.191]*	[0.174]	[0.215]	[0.210]
Observations	991	991	991	991	991	991

Robust standard errors in brackets
* significant at 5%; ** significant at 1%

Table A2. The separate effect of the child's age at her/his mother's and father's job loss on her/his dropout probability. Probit coefficients.

	(1)	(2)	(3)
Age at mother's job loss	-0.044 [0.014]**	-0.047 [0.015]**	-0.045 [0.014]**
Age at father's job loss	-0.039 [0.017]*	-0.044 [0.020]*	-0.036 [0.018]*
Age at interview	0.208 [0.036]**	0.212 [0.038]**	0.208 [0.036]**
Female	-0.095 [0.098]	-0.095 [0.098]	-0.092 [0.098]
Father's education (years)	-0.178 [0.033]**	-0.177 [0.033]**	-0.175 [0.033]**
Mother's education (years)	-0.245 [0.054]**	-0.243 [0.054]**	-0.255 [0.055]**
Father's age	-0.014 [0.010]	-0.014 [0.010]	-0.013 [0.010]
Mother's age	-0.011 [0.013]	-0.011 [0.013]	-0.011 [0.013]
Number of children 0-14 old	0.178 [0.052]**	0.177 [0.052]**	0.176 [0.052]**
Number of children above 15	0.127 [0.063]*	0.127 [0.063]*	0.124 [0.063]*
Year of interview		-0.012 [0.028]	
Year of mother's job loss			0.000 [0.000]
Year of father's job loss			-0.001 [0.000]**
Constant	-0.364 [0.130]**	-0.328 [0.154]*	0.887 [0.306]**
Observations	991	991	991

Robust standard errors in brackets
* significant at 5%; ** significant at 1%

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