DISCUSSION PAPER SERIES

No. 4716

EARNINGS, SCHOOLING AND ECONOMIC REFORM: NEW ECONOMETRIC EVIDENCE

Nauro F Campos and Dean Jolliffe

INSTITUTIONS AND ECONOMIC PERFORMANCE and LABOUR ECONOMICS

Centre for Economic Policy Research

www.cepr.org

Available online at: **www.cepr.org/pubs/dps/DP4716.asp**

ISSN 0265-8003

EARNINGS, SCHOOLING AND ECONOMIC REFORM: NEW ECONOMETRIC EVIDENCE

Nauro F Campos, University of Newcastle, WDI-University of Michigan and CEPR
Dean Jolliffe, Economic Research Service, USDA and WDI-University of Michigan

Discussion Paper No. 4716
October 2004

Centre for Economic Policy Research
90–98 Goswell Rd, London EC1V 7RR, UK
Tel: (44 20) 7878 2900, Fax: (44 20) 7878 2999
Email: cepr@cepr.org, Website: www.cepr.org

CEPR Discussion Paper No. 4716

October 2004

ABSTRACT

Earnings, Schooling and Economic Reform: New Econometric Evidence*

How does the relationship between earnings and schooling change with the introduction of comprehensive economic reform? This Paper uses a unique dataset (covering about 3 million Hungarian wage earners, from 1986 to 1998) and a novel procedure to correct sample selection bias (based on DiNardo, Fortin and Lemieux's) to shed light on this question. We find that reform was successful in general, increasing returns to skill by 70.5%, but that there were winners and losers. The winners seem to be the college and university educated, those employed by the smaller firms and those in commerce and services. The losers are those in manufacturing and agriculture and, surprisingly, those who received their formal education after the initiation of reform.

JEL Classification: I20, J20, J24, J31, O15, O52 and P20
Keywords: economic reform, human capital and transition economics

Nauro F Campos
Department of Economics
University of Newcastle
Newcastle-upon-Tyne
NE1 7RU
United Kingdom
Tel: (44 191) 222 6861
Fax: (44 191) 222 6548
Email: n.f.campos@ncl.ac.uk

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=140612

Dean Jolliffe
Economic Research Service
USDA
1800 M Street NW
Washington DC, 20036-5831
USA
Tel: (1 202) 694 5430
Fax: (1 202) 694 5642
Email: joliffe@ers.usda.gov

For further Discussion Papers by this author see:
www.cepr.org/pubs/new-dps/dplist.asp?authorid=158751

*The authors are grateful to Tim Barmby, Elizabeth Brainerd, John DiNardo, Peter Dolton, Francisco Ferreira, Stepán Jurajda, Harmut Lehmann and seminar participants at the University of Belfast Queens, Bristol and Newcastle, BOFIT, World Bank Inequality and Pro-Poor Growth Conference and the Northeast University Development Consortium Conference for valuable comments on previous versions. We also thank János Köllo, Gábor Kezdi, Gábor Kõrösi and György Lázar (National Employment Office) for help with the data, and Dana Žlábková and Ján Plánovský for excellent research assistance. This work benefited from financial support from Phare ACE Grant 97-8085. The views and opinions expressed in this paper do not reflect the views of the Economic Research Service of the US Department of Agriculture. The usual disclaimer applies.

Submitted 12 October 2004

1. Introduction

The rise and demise of communism are, arguably, the two most important economic events of twentieth century. One of the few indisputable communist achievements was an egalitarian distribution of income, accomplished by wage compression and a concerted effort to deliver universal public education. Returns to skill and experience were determined centrally while wages were set below equilibrium. The launching of comprehensive economic reform –the transition from centrally planned to market based economy– was to have powerful yet contradictory effects on those returns. Firstly, labour market liberalization would raise returns to skill simply because communism purposefully and successfully under-rewarded it. Second, until the collapse of communism a large share of the labour force (e.g., in manufacturing) used rather outdated technologies. These skills were not expected to be highly valued in a dynamic market economy. Third, skill deterioration would have been accompanied by a devaluation of the labour market experience acquired during communism.[1] Fourth and finally, the transition from centrally planned to market economy involved a reduction in government expenditures as part of deficit-reducing fiscal strategies. Transition freed up markets but also reduced education and health expenditures. These reduced expenditures could well have translated into education of lower quality.

How did these four starkly different effects of reform play off? One does not find consensus from the large literature addressing this question.[2] Not only do point estimates of returns to skill differ (at the same point in time for the same country), but

[1] A complicating factor to keep in mind is that these effects are seldom independent. Although socialism deliberately compressed earnings, it may have been able to reward different skills in non-pecuniary terms. The anecdotal evidence points to vacations and access to consumer goods as such rewards. Further, the notion that skills and experience acquired under socialism are of little value in a market economy overlooks the fact that different skills and labour market experiences are transferable to different degrees.

[2] See Svejnar (1999) and Boeri and Terrell (2002) for reviews of the literature. See Card (1999) for a review of this literature in non-transition countries.

also there seems to be disagreement on the behaviour of these estimates over time. Consider, for the sake of illustration, the papers by Brainerd (1998) and Sheidvasser and Benitez-Silva (2000). They use different data sets covering Russian workers after the collapse of communism, between 1991 and 1999. Using rather similar estimation methods, Brainerd finds that returns to schooling double over time (from about 4 percent in 1991 to about 8 percent in 1994), while Sheidvasser and Benitez-Silva reports fairly constant estimates for 1992 to 1999, at about 4 percent. We believe that conflicting results such as these can be explained, in large part, by data features.

This paper uses data that are unique in many respects. To the best of our knowledge, this is the first paper to use objective data[3] for sufficiently large and representative samples[4] of wage earners before, during and after[5] the introduction of massive economic reform.[6] The data we use covers 2.9 million Hungarian wage earners from 1986 to 1998. The large sample sizes, encompassing the periods before and after reform, allows for a correction for potential selection bias in our estimates of returns to skill, experience, and types of education that is, also, unique in the literature.

---

[3] By "objective data" we simply mean "not retrospective data," that is, data that do not suffer potential recall bias.

[4] In light of the dramatic changes in the labour force (Campos and Coricelli, 2002), it is surprising one finds little about representativeness in the literature. Our data is representative of wage earners employed in firms with more than 20 employees (more than 10 employees after 1995) thus encompassing the nascent private sector.

[5] Despite its volume, this literature has few studies that present estimates for the period before and after the reforms. Data availability is the binding constraint. Data collected before 1989 (that is, under communism) exist in large quantities, yet most has to be re-coded. Ours is unique in yet another respect: primary data were carefully re-coded to (current) standard international classifications (see further details in section 3 below). It is also important to stress that firm level data on earnings is less prone to the under-reporting bias one commonly encounters in household surveys.

[6] In the economic reform literature, empirical evidence is scarce (for reviews of this literature see Rodrik, 1996, and Harrison and Hanson, 1999). Existing studies tend to compare effects at two points in time (before and after), focus on a single aspect of reform (for instance, trade liberalization) and assume that the reform was effectively implemented and that sufficient time has transpired to measure its impact. We try to overcome these difficult issues by studying an unambiguously broad and effective reform at multiple points in time before and after the onset of the reform.

Our main findings are: (1) that returns to a year of schooling increased by 70.5 percent from 6.1 percent in 1986 to 10.4 percent in 1998; (2) that general secondary, college and university education show the largest changes in returns from 1986 to 1998, while vocational education shows almost no change in returns; (3) that returns to education for workers in Budapest and workers in small firms throughout Hungary increase the most during this time period; (4) that the largest changes in returns to skill are for those employed in services and trade, while the smaller changes are for those in manufacturing and agriculture; and (5) that returns for those who received their education post-1989 have actually fallen since 1995.

How do these findings relate to the four different effects discussed above? Although many of these effects have been previously identified, we believe this is the first paper to present comprehensive econometric estimates for each of these effects as well as for their joint, aggregate, impact. We put forward evidence supporting both of the primary hypotheses that returns have significantly increased after reform and that the planned economy over-specialised in specific skills. Returns to skill increased rapidly overall (effect 1), but this increase was much smaller for those employed in manufacturing and in agriculture. Also in support of this latter finding, we submit that returns in larger firms and those not located in Budapest are the lowest (effect 2). Our estimated returns to experience are in line with the rest of the literature: they are low in international perspective and do not tend to increase from 1986 to 1998 (effect 3). Finally, we present evidence that returns to education acquired after communism have fallen since 1995 (effect 4).

The paper is organised as follows. The next section briefly reviews the literature. Section 3 describes our data and methodology. Section 4 presents our econometric results. Section 5 concludes.

2. Previous studies

A review of the relevant empirical literature shows that there is little consensus regarding rates of return to education at similar points in time and even less regarding the trajectory of these estimates before, during and after reform. We believe that issues of data quality and sample representativeness are important factors in explaining the lack of consensus. Supporting our belief that a lack of sufficiently representative sample data is a determinant of the conflicting empirical results, Munich, Svejnar and Terrell argue that “Depending on the number of individuals that one's sample contains from different age categories, one's estimates may reflect the concave or flatter parts of the wage-experience profile. This may account for some of the differences among the estimates obtained ...” (2002, p. 25). Filer and Hanousek also lend support to this view by noting that “The bottom line is that it is even more important than usual in dealing with data from transition countries to pay careful attention to the details of how the data were collected and the exact wording of questions and sample design (2002, pp.237-8).”

While our review of the literature is brief and unavoidably selective, we have included all papers, to our knowledge, that report returns to skill in transition countries for years before and after 1989. We start with the available estimates for rates of return to years of schooling. Flanagan (1998) studies the Czech labor market and reports that the returns to a year of schooling increased from 4.3 percent in 1988 to 5.7 percent in 1996.[7] Chase (1998) finds that the returns to schooling for men double between 1984 and 1993 in the Czech Republic (from 2.4 percent in 1984 to 5.8 percent in 1993).[8]

---

[7] Returns to skill increased from 3.7 percent in 1988 to 4.5 percent in 1996 (males), while for females this change was from 5.1 percent in 1988 to 7 percent in 1996. It should be noted that these results are based on data from two different surveys.

[8] For Czech women, Chase finds that the returns to one year of schooling increased from 4.2 percent in 1984 to 7 percent in 1993. The author also reports estimates for the Slovak Republic. For males, the change was from 2.8 percent in 1984 to 4.9 percent in 1993, while for females it was from 4.4 percent in 1984 to 5.4 percent in 1993.

Vecernik (1995) estimate that returns to education increased from 4 percent in 1988 to 5.7 percent in 1992 for males in the Czech Republic.[9] Munich, Svejnar and Terrell (2002) use retrospective data for the Czech Republic to report that returns to education for males increased from 2.7% in 1989 to 5.8% by 1996. [10]

Rutkowski (1996) also finds that returns increased in Poland between 1987 and 1992, though by a smaller extent (40 percent increase). His estimate for 1987 is a 5 percent per year of schooling return, increasing to a rate of about 7 percent in 1993. Jolliffe finds that the rate of return to an additional year of secondary school in Bulgaria in 1995 was 4.9 percent for males and 8.1 percent for females (2002).

Brainerd (1998) finds that returns to education in the Russian Federation increased from 3.1 percent in 1991 to 6.7 percent in 1994 for males and, for females, from 5.4 percent in 1991 to 9.6 percent in 1994. Sheidvasser and Benitez-Silva (2000) find lower and fairly constant estimates for the period 1992 to 1999, at about 4 percent. It is also important to note that Sheidvasser and Benitez-Silva is one of the few papers in the transition literature that controls for selection bias.[11]

Krueger and Pischke (1995) find that returns to schooling decline from 7.7 percent

---

[9] Vecernik uses microcensus data for about 90,000 respondents. Although the survey instrument is the same, he calls attention to the existence of fundamental differences before and after the fall of communism, such as "whereas earnings in the 1989 Microcensus were confirmed by employers, they are only reported by respondents in the 1992 Microcensus" (1995, p. 358).

[10] Notice that while Chase (1998) and Flanagan (1998) present evidence that returns to labour market experience acquired under communism declines in the Czech transition, Munich et al. (2002) report that there is no evidence for such decline.

[11] We are aware of two papers that report instrumental-variable (IV) estimates: Filer et al. (1999) who use occupation codes to instrument schooling and Sheidvasser and Benitez-Silva (2000) who use two dummy variables indicating institutional changes to the education system. Interestingly, although the IV point estimates of returns are higher than reported OLS estimates, they show a declining trend in the Czech Republic between 1995 and 1997 and in Russia between 1992 and 1999. We choose not to replicate these IV approaches because we consider the identifying assumptions of both models untenable. In particular, we do not believe occupation can be modeled as a determinant of schooling but not wages, nor do we think that there is sufficient variation in two binary variables to identify schooling.

in 1988 to 6.2 percent in 1991 in the former Republic of East Germany.[12] Bird, Schwarze and Wagner (1994) also find evidence of this decline in East Germany between 1988 and 1991, from 4.4 percent to 4.1 percent.

The empirical transition literature also reports returns to different types of education. This is of particular interest in this context because "from the perspective of market economies, overinvestments in vocational-apprenticeship training and underinvestment in university education appear to be the major distortions in human capital formation under central planning" (Flanagan, 1998, p. 300). Therefore, if skills are "excessively specialized" one should observe a distinctive behaviour for returns to vocational-apprenticeship secondary education.

The available evidence supports this distinctiveness. For instance, Orazem and Vodopivec (1995) shows that for Slovenian males the premium (over incomplete primary) for elementary school increases from 4.4 percent in 1987 to 10.7 percent in 1991, for high school it increases from 31.9 percent to 40.6 percent, for university it increases from 71.5 percent to 94.3 percent, while for vocational it only increases from 16.3 percent to 20.1 percent.[13] Using (non-recoded) WES data, Kertesi and Kollo (1999) reports that the returns to vocational education also grew much slower than for other types of education in Hungary. Keane and Prasad (2002) and Norkooiv et al. (1998) present qualitatively similar evidence for Poland between 1986 and 1996 and for Estonia between 1989 and 1995, respectively,[14] as these two papers report that returns to vocational education increase much slower then for other types of education. In contrast, Flanagan (1998) shows that the premium to vocational education more than doubles in the Czech Republic between 1988

[12] Though, it should be noted that their results are based on data from different survey instruments over the two years.
[13] These changes are similar for Slovenian females, except that the premium to vocational education shows even a smaller increase, from 16.2 to 18.3 percent.
[14] Keane and Prasad use one of the largest samples in the literature, slightly less than 200,000 observations in total. Norkoiv et al. use retrospective data for Estonia.

and 1996, while that for university increases only by a quarter.

In summary, there seems to be disagreement over rates of return to skill as well as over the trajectory of these estimates before, during and after reform. It is our view that these conflicting results can be largely explained by differences in data features. Therefore, close attention to data quality and a thorough approach towards representativeness are of fundamental importance in generating credible estimates.

3. Data and the empirical specification

The data used in our analysis come from the Wage and Earnings Survey (WES) of the National Labor Center in Hungary, and it contains information on wages, education, type of employment, and other demographic data. We use data from the five years of 1986, 1989, 1992, 1995, and 1998 to cover the communist and transition periods. Considerable effort went into assuring that variables are coded consistently over time. This was carried out with assistance from the National Labor Center and the Hungarian Central Statistical Office and involved substantial recoding of the data on industrial sector, legal form (ownership) and occupation.[15]

3.1 Sample design

For all five years the samples are stratified on the characteristic of whether the wage earner is designated as a manual or non-manual laborer. The sampling units are wage earners and these are selected following a systematic, random selection procedure. The details of the procedures varied across the years in ways that affect the sample weights, but across all years the design is random and the estimates are representative of the

[15] For details on how consistent definitions of industry, ownership, and occupation codes were obtained for the thirteen-year time frame of our analysis, see Campos and Žlábková (2001).

sample frame. The sample frame for the years 1986, 1989, and 1992 includes all wage earners in private and public firms with more than 20 employees. The frame changed to include all wage earners in firms with ten or more employees for the 1995 and 1998 samples.[16]

In addition to this change in the sample frame, the procedure for sample selection changed on three occasions. In 1986 and 1989 wage earners were selected following a systematic, random design with a fixed interval of selection. From the list of all manual wage earners in each firm, one observation was randomly drawn and then every seventh employee thereafter was drawn. The same random selection procedure was used for all non-manual wage earners, except that the interval of selection was set to every fifth employee.[17]

For the years 1992, 1995, and 1998, the selection procedure changed from a fixed-interval selection procedure to a systematic selection procedure based on date of birth. Again the frame was stratified on manual and non-manual laborers, and then single-stage random draws were made for each labor type.[18] In this case, rather than a random starting point and fixed interval of selection, the design is based on the fact that date of birth is randomly and approximately uniformly distributed across days of the month.

For the years 1995 and 1998, there is one further element to the sample design that is associated with the changing frame. In these two years, the frame includes

---

[16] One problem with WES data is the changing sampling frame, but we argue that a strength of the WES data is that there is enough information and a sufficiently large sample so that we are able to adjust our estimates appropriately (which is far from common across the literature).
[17] Kish (1965) notes that systematic sampling is "perhaps the most widely known selection procedure" (p. 113) and suggests that the simplicity of this design reduces the potential for introducing error in the selection procedure.
[18] In the case of manual laborers, all workers born on the 5th or 15th day of each month were selected; and in the case of non-manual laborers, all were selected who were born on the 5th, 15th, or 25th day of each month. This sampling method gives a representative sample of 7-8% of the workers in such firms (6.4% of manual workers and 9.7% of non-manual workers).

smaller firms with total number of employees between 10 and 20. There is an important difference in the sample design associated with this supplementation of the sample frame in that selection occurred in two stages. In the first stage, 20 percent of these small firms (10 to 20 employees) were selected and then in the second stage all wage earners in these firms were selected into the sample.

With the exception of the supplementation to the sample frame in 1995 and 1998, the sample design is a stratified, single-stage, systematic random draw that results in estimates which are representative of the sample frame population.[19] The relevant issue is that the comparison across time suffers the slight problem that the frame changed for the last two years, 1995 and 1998. For the years 1986, 1989, and 1992, estimates are representative of all wage laborers who work in firms that have at least 20 employees. For the years 1995 and 1998, the estimates are representative of all wage laborers who work in firms that have at least 10 employees. If it were the case that firms with 10 to 20 employees reward education in a way that is systematically different from firms with 20 or more employees, then it is possible that observed changes in returns to education over time result in part from changing the sample frame in 1995.

One way to correct for this change in the sample frame is to exclude from our analysis those firms with 10 to 20 employees in years 1995 and 1998. This would result in estimates for all years that are representative of a population of laborers employed in firms with 20 or more employees. Unfortunately, our data does not identify these firms and we can only restrict our analysis by excluding firms with less than 50 employees from all years. The analysis loses some information on small firms, but the gain is that all estimates are based on a similar population.

---

[19] For more details on the properties of a stratified sample see Chapter 3 of Kish (1965) or Chapter 5 of Cochran (1977). For more details on single-stage, systematic random sampling, see Chapter 4 of Kish or Chapter 8 of Cochran.

Our preferred results are based on this restricted sample since they are representative of the same population across all years, but this choice comes at the cost of dropping small firms. In terms of precision of the estimates, the cost is not significant as the sample sizes remain large. The restricted sample sizes range from a low of 48,261 in 1989 to a high of 383,720 wage earners in 1989. In order to check the robustness of our results to this constraint on firm size, we also examine our results for the full samples across all years. When we consider the full sample of all firms, our analysis is based on a pool of more than 2.9 million wage earners.

3.2 The empirical specification

Wage equations are estimated using a standard Mincer equation, taking the form:

$$\ln(w_i) = \beta_1 S_i + \beta_2 E_i + \beta_3 E_i^2 + \boldsymbol{\beta_4 X_i} + \varepsilon_i \qquad (1)$$

where the i subscript denotes the individual, w is wages, S is years of schooling,[20] E is potential experience, and **X** contains a set of variables to control for institutional and demographic characteristics as well as spatial price differences. Each of these variables is described in more detail below.

The monthly value of wages used in our analysis is the sum of the official base wage received and other payments that the employee receives monthly (rewards given in the reference month, provisions, overtime work, shift work, other special payments, e.g. in mining). In addition, the value of wages includes a pro-rated estimate of irregular payments (1/12 of irregular payments in the previous year).

As a means of assessing the quality of the WES wage data used in this paper, Table 1 compares the monthly value of wages as measured by the WES and data from

---

[20] In some specifications S will be a vector of dummy variables representing graduation from different school types, such as primary, secondary, or university.

the International Labour Organization (ILO) and the Hungarian Central Statistical Office. Standard errors are only available for the WES data, so it is not possible to estimate p-values for the test of whether the differences in means are significant. From examining the ratio of the difference to the standard error for the WES data, though, it appears that the differences in mean values are likely to be statistically significant for all years. Due to the large sample sizes however, even qualitatively small differences in the mean values will be statistically significantly different. It is perhaps more useful to note that for all years except 1989, the difference between the two estimates for mean wages is less than (or equal to) one percent of the mean wage. This suggests that the WES wage data is very similar to other estimates of wages for Hungary, and we assert, of reasonably high quality.

[Insert Table 1 about here]

Two measures of schooling are examined in this paper. The first measure is a vector of six dummy variables that denote the highest type of completed schooling. The school types include primary, three types of secondary (vocational, technical, and gymnasium or general), college, and university.[21] In 1998, 22 percent of wage earners had only primary schooling or less, while 19 percent had college or university education. Of the remaining 59 percent who completed some form of secondary schooling, slightly less than half these wage earners attended vocational school (28 percent of the total). The second measure of schooling is an estimate of years of school attainment, which is created by converting the data on highest school type completed into years of schooling. The average value of this variable increased from 9.7 in 1986 to 11.3 in 1998.[22] Potential experience is constructed as the wage earner's age less six

[21] The omitted category is those individuals with less than primary schooling.
[22] The difference between these two means is statistically significant with a p-value of less than 0.001.

years and less years of schooling.

The variables designated by **X** include a set of eight dummy variables to control for potential differences across industries.[23] The set of controls also includes dummy variables for the size of the firm. For the sample restricted to only those firms with 50 or more employees, the specification includes a dummy variable for those firms with more than 300 employees. For the full sample, an additional dummy variable is included in the regression that marks those firms with less than 50 employees. To control for the large differences in wages across gender, **X** includes a dummy variable for males.

In addition to the controls for industry, size of firm, and gender; **X** contains a dummy for each of the 19 counties of Hungary and for the capital, Budapest. These spatial-specific, binary variables control for any variation that is specific to Budapest or some particular county. In particular, these dummy variables control for spatial variation in prices, which is likely to be significant with wages and prices in Budapest higher than other regions.

The county dummy variables will also control for region specific differences in labor markets, which are also potentially important given that unemployment rates are relatively lower in Budapest, the counties along the Budapest–Vienna highway, and the counties along the Hungarian–Austrian border. Similarly, the county dummies will control for the potential measurement issue that a year of schooling may result in different levels of human capital accumulation over different regions if there are differences in schooling quality across regions.

The controls for firm size and industry, as well the county fixed effects, greatly reduce the potential for omitted variable bias in our estimation of equation (1). Having

[23] The eight classifications are: industry, construction, transportation and telecommunications, trade, water, services, health and social services, and public services. The excluded classification is agriculture.

data that has been collected using the same survey instrument over the years of 1986 to 1998 also significantly improves the credibility of measured changes. Frequently, comparisons over time come from different data sources and one is left with the question of whether the change over time is a consequence of actual changes in the population, or is simply the result of changing the survey instrument. These are important advantages to using the WES data.

The disadvantage of using the WES data is that the choice of variables is small and our ability therefore to empirically address violations of the OLS parametric assumptions is limited. One well-known violation of the classical assumptions occurs if education is correlated with the regression residual. One candidate cause of correlation is that there is some omitted variable, such as innate ability, that is both correlated with education and with wages. Failing to include ability in the regression results in correlation between education and the residuals and biased point estimates. One way to correct this type of bias is to use instrumental variables to purge education of that component which is correlated with the residuals. This approach though, requires that there is some variable that is both correlated with schooling but also reasonably excluded from the wage equation. We argue that there is no such variable in the WES data that allows us to credibly instrument education, and we caution that one should keep this potential bias in mind when interpreting the results. A mitigating factor is that our analysis is focused how returns have changed over the communist and transition years. If the magnitude of these potential biases has not changed over time, then their impact on drawing inferences about change over time is limited.

Another potential cause of correlation between education and the regression residual can occur if people select in and out of the sample based on some characteristic that is correlated with the return to schooling.[24] For example, if persons with high returns are more likely to be wage earners and those with expected low returns opt out of the sample, then this induces correlation and results in sample-selection bias. This source of bias is typically corrected by explicitly modelling the selection decision, but this requires that there exists at least one variable that explains the decision to work in the wage sector but does not explain the wage level.[25] Again, we believe that no such variable exists in the WES data, but we argue that the unique features of the labor market in 1986 will help us to control for sample-selection bias.

A fundamental characteristic of the centrally-planned economies was that workers had very limited ability to select in or out of the wage market. In principal, all persons of working age were required to work, official unemployment was close to zero, and the opportunity to choose to work in a non-wage employment was highly limited. An implication of this lack of freedom to select out of the wage market means that the pre-transition, 1986 WES estimates of the wage equation (1) will not suffer from sample-selection bias.[26]

Access to the 1986, pre-transition data is a unique feature of our analysis that allows us to control for selection bias in the later, post-1989 years. Our principal assumption is that the decision to participate in the wage market is correlated with age,

---

[24] For East Germany from 1990 to 1994, Hunt (2002) finds that the 10 point improvement in the gender wage gap is a result of the reduction of participation of low wage women in the labour market.

[25] Just to make this point clear: this data set does not contain observations for those individuals not in the wage market. The standard Heckman correction can therefore not be made because it is not possible to estimate a participation equation.

[26] Munich, Svejnar and Terrell (2002) note that "In addition to regulating wages, the central planners regulated employment and admissions to higher education. With minor exceptions, all able-bodied adults were obliged to work. Jobs were provided for everyone and employment security was assured" (2002, p. 6). Further, Horvath et al. (1999) argue that the registered unemployment rate in Hungary increased rapidly with the launching of reforms reaching 1.4% in 1990.

sex and schooling demographics.[27] The change that is empirically observed in these characteristics in the post-1989 WES data comes from either people selecting in and out of the wage market or true population changes. If we assume that any change of these characteristics in the sample after 1989 is due to people choosing to leave and enter the wage market, then we can control for selection bias by re-weighting the WES data to have the same demographic composition as the 1986 data.

This methodology is similar to that proposed by DiNardo, Fortin and Lemieux (1996), who propose a semi-parametric estimation strategy to answer questions such as: what would the distribution of wages now be, if worker's attributes had remained as before. They note that the methodological contribution of their paper is to show that the estimation of such counterfactual densities can be "greatly simplified by the judicious choice of a reweighting function" (1996, p. 1009). In this paper, we generate a baseline density by treating our 1986 sample as one in which sample-selection bias is negligible, and re-weight the other four years according to the demographic distribution of the 1986 sample. More specifically, we partition each of the WES samples into six age categories (under 30, 30-34, 35-39, 40-44, 45-49, 50 and over), the seven school types described above and sex; for a total of 84 age-sex-education categories. We then define the proportion of the population that belongs to each of these categories in year t as:

$$\nu_k^t \equiv \sum_i^{j_k} \omega_{i,k}^t / \sum_i^{j_k} \sum_k^K \omega_{i,k}^t \qquad (2)$$

where the k subscripts runs from 1 to K and represents the 84 age-sex-education categories, i subscripts the individual observation and runs from 1 to j_k for each of the k categories, and $\omega_{i,k}$ represents the weight or expansion factor for individual i in category k.

---

[27] Svejnar (1999) and Boeri and Terrell (2002) observe that early retirement schemes, youth unemployment and the reduction of female labour force participation rates are stylised facts of the transition.

To re-weight the data such that the demographic composition in later years matches the composition from 1986, we construct new weights for each year:

$$\varpi_{i,k}^{t} = (\nu_{k}^{86} / \nu_{k}^{t})\omega_{i,k}^{t} \quad \forall k \in K \tag{3}$$

The ν^{86} term in the numerator adjusts the weights to reflect the demographic composition in 1986, and the ν^{t} in the denominator normalises the adjustment to ensure that the sum of unadjusted weights equals the sum of adjusted weights.

For example if low-educated, young males comprise a larger proportion of the sample in 1986 than in 1995, we adjust upwards the 1995 weights to ensure that they represent the same proportions across both years. One difficulty with this approach is that it doesn't allow for any true population changes in the age, sex and education composition of the sample.[28] While this affects the interpretation of the results, it is in some ways a desirable characteristic. Changes in returns to education can result from changes in the composition of the labor market and from how the labor market rewards education, conditional on the characteristics of the labor market. By re-weighting the data to the 1986 demographic composition, we purge from our analysis changes in labor supply and focus on market changes in the demand for wage labor. This focus allows us to examine whether firms are responding to liberalisation by providing greater returns to investment in human capital.

4. Results

To examine how returns to schooling have changed from 1986 to 1998, county fixed-effects estimates of equation (1) are provided in Tables 2-5. Panel A of Table 2 provides weighted, fixed-effects estimates for all firms with more than 50 employees, while

[28] The Hungarian Central Statistical Office reports that the Hungarian population declined by about 2 percent between 1990 and 1998 and has also aged slightly during this period.

Panel B and the other tables all report fixed-effects estimates for the same sample except the estimates are weighted following the formula above and represent our selection-corrected estimates.

The standard errors listed in all regression tables are corrected for heteroskedasticity of unknown form through use of the 'sandwich variance estimator'. This commonly used estimator was first introduced by Huber (1967) and White (1980) and has the advantage of providing consistent estimates of the variance-covariance matrix when errors are heteroskedastic or if the residuals exhibit some form of dependence.[29] The disadvantage, as noted by Kauermann and Carroll (forthcoming), is that the sandwich variance estimator is inefficient and often times results in estimated standard errors that are too conservative (large). Given the large sample sizes, the cost of the 'sandwich' or robust variance estimates are not qualitatively important, and the benefit of consistency is desirable.

4.1 Returns to years of schooling

The results from Panel A of Table 2 show that the uncorrected returns to a year of schooling increased by 92 percent from a return of 6.1 percent in 1986 to 11.7 percent in 1998.[30] Panel B shows that the selection-corrected return to schooling for wage earners from firms with 50 or more employees increased by 70 percent from 6.1 percent in 1986 to 10.4 percent in 1998. This result strongly supports the hypothesis that central planners undervalued education and the market has quickly corrected this.

A comparison of the panels shows that the sample-selection bias is positive and

---

[29] An important advantage of the WES design is that the sample was selected in a single stage, and there is therefore no need to correct estimates of the sampling variance for any sort of design-induced dependence. Scott and Holt (1982) show that this type of correction for dependence can be quite large for multi-stage sample designs.
[30] This increase in returns is statistically significant.

quite large in the 1990s. The direction of the bias is consistent with the hypothesis that persons who expect to receive higher returns in the wage market, choose to enter; and those who expect lower returns, opt out. It is also noteworthy that the decision of workers to select in and out of the sample appears to happen quickly. By 1992 the magnitude of the bias is over 10 percent and stays at approximately this level throughout the 1990s.

Recall that throughout this paper, we restrict our analysis to only those firms with 50 or more employees. While this restriction corrects for the change made to the sample frame in 1995, it comes with the disadvantages that a large portion of the sample has been dropped and that our analysis excludes wage earners working in smaller firms. Examination of the full sample reveals that the restriction on the sample does not affect the qualitative nature of the results. For example the selection-corrected estimates from the sample of all firms, increase from 5.7 percent in 1986 to 10.6 percent in 1998.[31]

[Insert Table 2 about here]

Table 3 compares the change over time of returns to school years by firm type and location. Panels A and B aim to provide some insight into the responsiveness of firms to market change. Panel A compares returns by firm size because it is typically the case that smaller firms are more likely to be new entrants to the market and are often viewed as more responsive to changing market conditions. The estimates in Panel A appear to support this view. In 1986, returns to schooling for workers in small firms were lower than in mid-size and large firms. During the 1990s though, schooling returns grew much faster in small firms and by 1998 returns were much higher in small firms than in larger firms.[32]

[31] Note also that there is no increasing trend on our estimates of returns to potential experience.
[32] One caveat to interpreting the results from the small firms, as discussed earlier in the paper, is that the definition of small firms changed from 20 to 50 employees to 10 to 50 employees in 1995.

Panel B estimates returns separately for firms located in and outside of Budapest. The presumption in this case is that market changes have been more vigorous in the capital city, and Budapest firms might face more competitive forces to change. In 1986, returns to schooling were slightly lower in Budapest than elsewhere in the country. By the early 1990s, this ranking was reversed and by 1998 returns to schooling in Budapest were 24 percent higher than elsewhere in the country. Recall that the presence of industry dummy in the estimated equation helps control for changes in the sectoral composition.[33]

[Insert Table 3 about here]

The purpose of Panel C is to provide evidence on whether there have been any important qualitative changes in schooling post-1989. We know that in 1986 and 1989 all of the wage earners acquired their schooling prior to transition. By 1998, we know that the youngest wage earners in our sample acquired the large majority of their schooling during the post-1989 years. By contrasting the returns for the youngest wage earners in the WES sample with all others, we can observe whether the market considers schooling attained in the post-communist years more or less highly.

One hypothesis might be that schools post-1989 have responded to the changing market needs, and provided more marketable skills. A competing theory is that since the era of transition has seen large declines in education budgets for many countries, including Hungary, the resulting quality of education has declined. There is much evidence supporting this latter hypothesis.

Immediately post-1989, total public expenditures on education as a percent of gross domestic product increased in Hungary from 5.7 percent in 1989 to 6.6 percent in

[33] It is important to mention that information on private firm ownership is only available after the year 1992. As most of the literature, we find little difference in the returns to skill in private vis-à-vis public enterprises form 1992, 1995 and 1998. These results are available from the authors' upon request.

1992. During the next five years though, expenditure declined by 35 percent reaching a low of 4.4 percent in 1997 (Berryman, 2000). As one example, the "Bokros package" of 1995 reduced allocations for salaries of staff at higher education facilities by 20 percent (World Bank, 1998).[34] As a result of the declining expenditures on education post-1989, studies note that many teachers have had to take on second jobs (Fretwell and Wheeler, 2001) and that academic performance has been declining (World Bank, 1997).

The empirical evidence in Panel C is consistent with the hypothesis that the market perceives a deterioration in the quality of education.[35] In 1986, the returns to schooling for those 20 years of age and younger were about 61 percent less than the returns for persons over 20 years of age. In the early years of transition, this gap narrowed and by 1992 the difference stood at 17 percent. The difference in returns, though, increased over the next six years and by 1998, wage earners who were schooled in the post-1989 years had returns that were 163 percent less than the returns for those who received more of their schooling prior to 1989. A comparison of returns to schooling in 1986 and 1998 shows that there was essentially no change for people 20 years of age and younger, while for those over the age of 20, returns were 72 percent higher in 1998. This alarmingly pattern suggests a decline in school quality that could have negative repercussions on future economic growth and for the earnings potential of the generation that attained its education during the post-transition years of the 1990s.[36]

Under communism, a substantial share of the labour force was employed in large state-owned industrial enterprises. Thanks to government subsidies, those firms were able to survive with minimal technological modernization. Consequently, at the

---

[34] The Bokros package refers to a series of fiscal austerity measures imposed by then-Finance Minister Bokros in response to a severe fiscal crisis.

[35] There is an extensive literature on school quality in non-transition countries. See, among others, Behrman and Birdsall (1983), Betts (1995), Card and Krueger (1992), and Glewwe (1999).

[36] See Fan, Overland and Spagat (1999) for a theoretical discussion of this possibility.

outset of transition a considerable share of workers had skills that were relevant for production technologies that were by and large obsolete. One thus expects that returns to the skill of those in manufacturing would be lower than returns to skill of workers in other sectors (especially services). Table 4 shows our estimates of the returns to skill from 1986 to 1998 in eight sectors, based on the consistent sample of wage earners in firms with 50 or more employees.

[Insert Table 4 about here]

A noteworthy result from Table 4 is the difference between the changes in returns to education from 1986 to 1998 in trade and services, on the one hand, and the same changes in agriculture and industry, on the other. The largest increases in returns to skill are in services and trade, by 97 and 92 percent, respectively. The smallest changes in returns are for industry and agriculture, by 39 and 25 percent, respectively. These results are consistent with the notion that the planned economy under-valued labour used in the production of non-physical goods and services. In light of our previous result on the declining quality of education after 1989, it is important to call attention to returns in the health and social services (which includes education services). By 1998, out of a total of eight sectors, only agriculture shows a lower level of returns to skill than health and social services. This provides additional indirect evidence on the deterioration of the quality of education after 1989.

4.2 Returns by type of school

By assigning years of schooling to persons who have attended different types of schooling, an implicit assumption is being imposed that a year of vocational schooling, for example, is the same as a year of general secondary schooling. This assumption made in section 4.1 simplifies interpretation of the results and helps to clearly demonstrate that returns to schooling have been increasing, but it may mask important information in terms of what types of schooling are being more heavily rewarded in the labor market.

There are several potential explanations for the varying returns to types of schools during transition. One explanation is that different types of school produce different skill sets and these skills may be more or less well suited to the needs of the new market economy. Another related explanation is that the government traditionally steered students into certain types of schools and this planned aspect of the economy no longer provided the correct mix of skills.

Both of these explanations are based on the idea that the changing market environment produced changes in the market value of certain skills. These hypotheses ignore the fact that under the planned economy, returns to skills were set by planners and not determined by the market. Prior to transition, wage setting was used to favor certain industries and certain types of labor. To this end, labor that had been trained in technical and vocational schools and was involved in the production of certain goods tended to be more highly valued, while labor that had been more academically trained and less likely working in the physical production of goods was less highly valued. Presumably the market economy rewards the value added by labor and is indifferent as to whether the added value is in terms of some physical commodity or, for example, in terms of some service.

Table 5 lists the returns to each of the six types of schooling (ranging from primary to university) over five points in time from 1986 to 1998. The listed estimates are those based on the consistent sample of wage earners in firms with 50 or more employees.[37] One result to note is that the returns to vocational schooling were virtually

[37] Results for the full samples are available from the authors upon request and are similar in that

unchanged between 1986 and 1998. This result is striking given the dramatic increase in the return to a year of schooling over this time period.

[Insert Table 5 about here]

Another result shown in Table 5 is that the largest percentage change in returns is for those wage earners who completed secondary general education. Their returns to schooling increased by 67 percent between 1986 and 1998. The next largest change in returns to schooling is a 62 percent increase for those who completed university, followed by a 57 percent increase in returns for those completing college. The result that the returns to general secondary schooling increased the most during transition is consistent with the belief that the planned economy under-valued (relative to the market economy) labour used in the production of non-physical goods and services. One indication that students are responding to the changing structure of returns by school type is that the share of students in general education increased from 24 percent in 1990 to 28 percent by 1997 (Fretwell and Wheeler, 2001).

The estimates in Table 5 also reveal that the returns to school type follow a similar pattern except for returns to technical schooling which declines significantly during the early years of transition, while returns to the other school types increase. This finding provides empirical evidence supporting the theoretical argument of Nelson and Phelps (1966) and Schultz (1975) that general education may enhance an individual's ability to adapt to a changing market environment. In contrast, the value of training in specific technical skills is more dependent on market fluctuations. When skills training is well targeted to the specific demands of the market, then returns are high; when market conditions change, there will be a lag before the curricula can adjust to provide

---

the percentage change to secondary general education is the greatest of the period from 1986 to 1998. Similarly, when using the full sample, the rate of change for each school type follows the same patterns.

the correct mix of skills. This result further adds to the discussion of the relative benefits of general education versus training in specific skills.

5. Conclusions

In this paper, we studied the effects of the introduction of massive economic reform – the transition from centrally planned to market economy– on the labour market.

We tried to improve upon the existing reform literature by focusing on a highly effective reform, defined broadly and covering the periods before, during and after its implementation.

One important result in this paper is that returns to schooling are large throughout the Hungarian transition, at around 10 percent and above since 1995. The returns to a year of schooling increased by 70 percent from 6.1 percent in 1986 to 10.4 percent in 1998. While these returns are larger than those available for other transition economies and for Western Europe, we believe they are credible estimates for several reasons.

While many East European countries have education levels that are on par with Western Europe, average wages in East Europe continue to be much lower than in Western Europe. Since estimated returns to schooling are measured in terms of a percentage change in wages, if West and East European markets were to provide similar returns to schooling in terms of wage levels, then this means higher returns from estimating Mincer equations such as equation (1). Our prior assumption is that if markets are truly liberalized, then rates of returns in transition countries will be higher than those found in West Europe until there is some convergence in wage levels.[38] We

[38] We note that this is consistent with Psacharopolous (1994), who summarizes an extensive literature by showing that returns to education typically range from about 6-7 percent for high-income countries to about 11-12 percent for low-income countries.

find ourselves puzzled by the extensive literature showing similar returns across transitional countries and Western Europe.

We believe that the difference found in our estimates may be in part based on some unique characteristics of the data we examine. Our data was collected using the same survey instrument over the years of 1986 to 1998, covering pre- as well as transition years. Studies that are based on multiple survey instruments for the temporal analysis face the difficult question of whether the observed change results from changes in the examined population or changes in the survey instrument. Further, our data was painstakingly re-coded to current standard international classifications to minimize errors in comparisons over time.

The unique characteristics of the WES data used in this paper allowed us to address the important issue of school quality. Because the WES sample sizes are so large (totalling about 2.9 million wage earners from 1986 to 1998), we are able to estimate wage equations for a small sub-sample of very young workers in each of the five years. This analysis reveals the alarming result that the returns to education for the generation that received much of its schooling post-1989 declined significantly during the mid and late 1990s indicating a recent deterioration of school quality.

Use of the WES data though, also imposes difficult estimation issues similar to those faced with using labor force survey data for estimating returns to schooling. In particular, with essentially all labor force surveys, the researcher has no information on the population that chose not to participate in the wage market and therefore can not control for sample-selection bias. Our approach to correct for this potential bias has been to exploit the somewhat unique fact that we have representative data from 1986, well before the transition began and when workers had very limited choice as to whether they would participate in the wage sector. Our estimation strategy in essence is to use the demographic composition (based on 84 age-sex-schooling classifications) of the 1986 data as a basis to identify which types of people disproportionately select in or out of the sample in later years.

We argue that the 70 percent increase in returns to a year of schooling between 1986 and 1998 is evidence that the planned economy under-valued education and liberalization has allowed the markets to correct this. As further evidence that market forces have led to increasing returns, we separately estimate returns by firm type and location. We believe that firms located in Budapest face greater market pressures and we find that indeed returns to education increased more in Budapest than elsewhere. We also believe that smaller firms are more likely to adapt more quickly to market pressures and also disproportionately represent the newer, private firms. This belief is consistent with our results that the returns to schooling increased the fastest for the smallest firms.

Finally, by examining returns by type of schooling rather than an aggregated measure of years of schooling, we shed further light on whether the type of schooling received in pre-transition Hungary proved to be appropriate for the liberalized, post-1989 market. The common assumption is that socialist economies under-valued and under-supplied general education. Our analysis supports this view. We note that after 1989, an increasing proportion of students have been choosing to attend general school over vocational and technical schooling. In spite of this increasing supply of students in general schooling, the estimated returns to university, college, and secondary general education all increased by more than 55 percent between 1986 and 1998. This is in contrast to the returns to secondary technical training, which increased by 43 percent and secondary vocational, which increased by a mere six percent over this time period.

The empirical evidence in this paper supports the belief that the liberalized economy has responded to market forces and is providing large returns for human capital investments. The evidence also suggests that wage earners are responding to the changes in the market and making better investment choices. All of this bodes well for future growth. The alarming caveat to this conclusion, though, is that the significant declines in public expenditures on education and smaller returns to skill in the health and social services sector appear to have resulted in a decline in the quality of this investment and the markets have quickly recognized this.

References

Berryman, Sue, *Hidden Challenges to Education Systems in Transition Economies*, Washington, D.C.: The World Bank, 2000.

Bird, Edward, Johannes Schwarze and Gert Wagner, "Wage Effects of the Move Toward Free Markets in East Germany," *Industrial and Labor Relations Review* 47 (3): 390-400, 1994.

Behrman, Jere and Birdsall, Nancy, "The Quality of Schooling: Quantity Alone is Misleading." *American Economic Review*, 73(5): 928-946, 1983.

Betts, Julian, "Does School Quality Matter? Evidence from the National Longitudinal Survey of Youth." *Review of Economics and Statistics*, 77(2): 231-250, 1995.

Boeri, Tito and Katherine Terrell, "Institutional Determinants of Labor Reallocation in Transition," *Journal of Economic Perspectives*, 16 (1): 51-76, 2002.

Brainerd, Elizabeth, "Winners and Losers in Russia's Economic Transition," *American Economic Review*, 88 (5): 1094-1116, 1998.

Card, David, "The Causal Effect of Education on Earnings," in O. Ashenfelter and D. Card (eds) *Handbook of Labor Economics* Volume 3. Amsterdam: Elsevier, 1999.

Card, David and Krueger, Alan. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy*, 100(1): 1-40, 1992.

Campos, Nauro and Žlábková, Dana, "The Wrong Mix: A First Look at Occupational Mobility during the Hungarian Transition," Prague: CERGE-EI DP 59, March 2001.

Campos, Nauro and Coricelli, Fabrizio, "Growth in Transition: What We Know, What We Don't, and What We Should," *Journal of Economic Literature*, XL (3): 793-836, 2002.

Chase, Robert. "Markets for Communist Human Capital: Returns to Education and Experience in the Czech Republic and Slovakia." *Industrial and Labor Relations Review*, 51(3): 401-23, 1998.

Cochran, William G. *Sampling Techniques.* New York: Wiley Series in Probability and Statistics, 1977.

DiNardo, John, Fortin, Nicole and Lemieux, Thomas. "Labor Market Institutions and the Distribution of Wages, 1973-1992: A Semiparametric Approach." *Econometrica,* September 1996, 64(5): 1001-1044.

Fan, Simon, Overland, Jody and Michael Spagat, "Human Capital, Growth and Inequality in Russia." *Journal of Comparative Economics*, 27 (4): 618-643, 1999.

Filer, Randall, Jurajda, Štepán and Ján Plánovský. “Returns to the Market: Valuing Human Capital in the Czech and Slovak Republics,” CERGE-EI WP 125, 1999.

Filer, Randall and Hanousek, Jan, “Research Data from Transition Economies.” *Journal of Economic Perspectives*, Winter 2002, 16(1): 225-240.
Flanagan, Robert, “Were Communists Good Human Capitalists? The Case of the Czech Republic,” *Labour Economics*, 5 (2): 295-312, 1998.

Fretwell, David and Wheeler, Anthony, “Hungary: Secondary Education and Training,” Washington, D.C.: The World Bank, 2001.

Glewwe, Paul. *The Economics of School Quality Investments in Developing Countries: An Empirical Study of Ghana*. New York: St. Martin's Press, 1999.

Harrison, Ann and Gordon Hanson, “Who Gains from Trade Reform? Some Remaining Puzzles,” *Journal of Development Economics*, 59 (1): 125-154, 1999.

Horvath, Reka, Abraham, Arpad, Horvath, Tibor and Tams Kopeczi-Bocz, “Background Study on Employment and Labour Market in Hungary,” Barcelona: European Training Foundation, 1999.

Huber, Peter. “The Behavior of Maximum Likelihood Estimates Under Non-standard Conditions.” *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability,* vol. 1, 221-233, 1967.

Hunt, Jennifer. “The Transition in East Germany: When is a Ten Point Fall in the Gender Wage Gap Bad News?” *Journal of Labor Economics,* 20 (1): 148-169, 2002.

International Labour Organization. *LABORSTA: Labour Statistics Database*, Geneva, 2001.

Jolliffe, Dean, “The Gender Wage Gap in Bulgaria: A Semiparametric Estimation of Discrimination." *Journal of Comparative Economics*, 30(2): 276-295, 2002.

Kauermann, Göran and Carroll, Raymond. “A Note on the Efficiency of Sandwich Covariance Matrix Estimation.” *Journal of the American Statistical Association*, forthcoming.

Keane, Michael and Eswar Prasad, “Changes in the Structure of Earnings during the Polish Transition,” IMF Working Paper No. 02/135, August, 2002.

Kertesi, Gabor, and Janos Köllo, “Economic Transformation and the Return to Human Capital: The Case of Hungary, 1986-1996,” Budapest Working Papers on the Labour Market, BWP 1999/6, 1999.

Kish, Leslie, *Survey Sampling*, New York: John Wiley & Sons, 1965.

Krueger, Alan and Pischke, Jorn-Steffen. "A Comparative Analysis of East and West German Labor Markets: Before and After Unification," in *Differences and Changes in Wage Structure*, Freeman, R. and Katz, L. (eds.), Chicago: University of Chicago Press, 1995.

Munich, D., Svejnar, J. and K. Terrell, "Returns to Human Capital under the Communist Wage Grid and During the Transition to a Market Economy," Ann Arbor: William Davidson Working Paper Number 272a, January 2002.

Nelson, Richard and Phelps, Edmund. "Investment in Humans, Technological Diffusion, and Economic Growth." *American Economic Review*, 56(2): 69-75, 1966.

Orazem, Peter and Milan Vodopivec, "Winner and Losers in Transition: Returns to Education, Experience and Gender in Slovenia," *World Bank Economic Review* 9:2 201-230, 1995.

Rodrik, Dani, "Understanding Economic Policy Reform," *Journal of Economic Literature* 34 (1): 19-41, 1996.

Rutkowski, Jan, "High Skills Pay Off: The Changing Wage Structure during Economic Transition in Poland," *Economics of Transition* 4 (1): 89-112, 1996.

Schultz, Theodore W. "The Value of the Ability to Deal with Disequilibria." *Journal of Economic Literature* 13(3): 827-46, 1975.

Scott, Andrew and Holt, Tim. "The Effect of Two-Stage Sampling on Ordinary Least Squares Methods." *Journal of American Statistical Association*, 77 (380): 848-854, 1982.

Sheidvasser, Sofia and Hugo Benitez-Silva, "The Educated Russian's Curse: Returns to Education in the Russian Federation," Yale University, mimeo, 2000.

Svejnar, Jan, "Labor Markets in the Transitional Central and East European Economies." In Orley Ashenfelter and David Card, Eds., *Handbook of Labor Economics*, Vol. 3, pp. 2809-2857. Amsterdam, London and New York: Elsevier, North-Holland, 1999.

Vecernik, J., "Changing Earnings Distribution in the Czech Republic: Survey Evidence from 1988-1994," *Economics of Transition* 3 (3): 355-371, 1995.

White, Halbert L. "A Heteroscedasticity-consistent covariance matrix estimator and direct test for heteroscedasticity." *Econometrica*, 48 (4): 817-838, 1980.

World Bank, "The Effect of Education Decentralization Reforms on Resource Allocation, Quality, and Equity in Hungarian Schools," in *Abstracts of Current Studies: Labor Markets and Education,* Washington, D.C.: The World Bank, 1997.

World Bank, *Republic of Hungary Higher Education Reform Project: A Staff Appraisal Report*, Report No. 16536-HU, Washington, D.C.: The World Bank, 1998.

Table 1: Comparison of Wage Estimates from WES and ILO Data

	1986	1989	1992	1995	1998
Mean Wage, ILO[a]	6,260	10,461	22,294	39,854	68,718
Mean Wage, WES[b]	6,312	9,761	22,466	40,190	69,415
Standard Error of Mean Wages, WES	4	8	41	77	162
Difference in Wages, ILO-WES	52	-700	172	336	697
Difference/Standard Error	13.9	-86.0	4.2	4.4	4.3
Difference as a percent of ILO Mean	0.8%	-6.7%	0.8%	0.8%	1.0%

[a] LABORSTA data, International Labour Organization (2001) and the Hungarian Central Statistics Office.

[b] Wage and Employment Survey (WES), National Labor Center, Hungary.

Notes: Wages are monthly figures in Forint (HUF). ILO data on mean wages for 1986 and 1989 are based on ISIC (revision 2) codes 2-9, and the WES estimates are restricted to these same industry codes.

Table 2: Returns to Years of Schooling, 1986-1998
Spatial and Industry Fixed-effects Estimation of Equation (1)

	1986	1989	1992	1995	1998
Panel A: Uncorrected Estimates					
Years of Schooling	0.061	0.078	0.096	0.112	0.117
	(0.0004)	(0.0004)	(0.0007)	(0.0007)	(0.0007)
Dummy: Male=1	0.280	0.279	0.154	0.141	0.136
	(0.0022)	(0.0022)	(0.0042)	(0.0036)	(0.0038)
Potential Experience	0.028	0.026	0.029	0.025	0.025
	(0.0003)	(0.0003)	(0.0007)	(0.0006)	(0.0007)
Experience Squared / 100	-0.041	-0.037	-0.036	-0.028	-0.030
	(0.0007)	(0.0007)	(0.0015)	(0.0013)	(0.0015)
Firm Size: 300+ Employees	-0.009	0.001 [a]	-0.014	-0.138	-0.162
	(0.0051)	(0.0019)	(0.0054)	(0.0034)	(0.0034)
Observations:	149,274	383,720	48,261	371,882	334,207
R-squared:	.45	.41	.43	.43	.44
Panel B: Selection-corrected					
Years of Schooling	0.061	0.073	0.082	0.098	0.104
	(0.0004)	(0.0004)	(0.0009)	(0.0009)	(0.0012)
Dummy: Male=1	0.280	0.295	0.216	0.169	0.163
	(0.0022)	(0.0024)	(0.0070)	(0.0043)	(0.0055)
Potential Experience	0.028	0.021	0.024	0.020	0.018
	(0.0003)	(0.0004)	(0.0011)	(0.0008)	(0.0010)
Experience Squared / 100	-0.041	-0.028	-0.031	-0.018	-0.015
	(0.0007)	(0.0008)	(0.0024)	(0.0018)	(0.0024)
Firm Size: 300+ Employees	-0.009	-0.002 [a]	-0.024	-0.139	-0.171
	(0.0051)	(0.0022)	(0.0093)	(0.0042)	(0.0051)
Observations:	149,274	383,720	48,261	371,882	334,207
R-squared:	.45	.40	.39	.38	.38

[a] The only statistically insignificant point estimates—1989, Firm Size: 300+ Employees.

Notes: Dependent variable is the log of monthly wages. Sample consists of all firms with 50 or more employees. Standard errors, in parentheses, are robust to heteroscedasticity of unknown form. The eight industry dummy variables are jointly significant and are excluded from the table. County, fixed effects are also jointly significant. All listed point estimates are significant with a p-value of less than 0.01 except for the firm-size dummy. This dummy is significant with a p-value of less than 0.05 for all except in 1986 and 1989.

Table 3: Comparing Returns by Firm Size, Location and Age
Spatial and Industry Fixed-effects Estimation of Equation (1)

	1986	1989	1992	1995	1998
Panel A: Returns by Firm Size					
Small Firms	0.056	0.060	0.080	0.093	0.115
	(0.0002)	(0.0003)	(0.0006)	(0.0008)	(0.0010)
Medium-sized Firms	0.061	0.071	0.081	0.099	0.107
	(0.0004)	(0.0005)	(0.0010)	(0.0012)	(0.0015)
Large Firms	0.059	0.088	0.089	0.098	0.101
	(0.0015)	(0.0006)	(0.0023)	(0.0013)	(0.0017)
R^2 / Observations:					
Small Firms	.41 / 682,133	.29 / 549,562	.41 / 88,568	.40 / 158,046	.45 / 152,953
Medium Firms	.45 / 139,692	.38 / 307,405	.38 / 41,203	.40 / 184,085	.41 / 192,480
Large Firms	.54 / 9,582	.56 / 76,315	.44 / 7,058	.35 / 187,797	.32 / 141,727
Panel B: Returns in Budapest					
Budapest	0.059	0.070	0.094	0.103	0.123
	(0.0006)	(0.0011)	(0.0025)	(0.0023)	(0.0030)
Not Budapest	0.062	0.074	0.079	0.097	0.099
	(0.0004)	(0.0004)	(0.0010)	(0.0009)	(0.0012)
R^2 / Observations:					
Budapest	.51 / 40,952	.31 / 55,290	.35 / 8,099	.33 / 31,682	.36 / 33,887
Not Budapest	.40 / 108,322	.37 / 328,430	.36 / 40,162	.37 / 340,200	.37 / 300,320
Panel C: Returns by Age					
20 Years and Younger	0.038	0.051	0.070	0.075	0.040
	(0.0040)	(0.0062)	(0.0112)	(0.0114)	(0.0152)
Older than 20 Years	0.061	0.073	0.082	0.099	0.105
	(0.0004)	(0.0004)	(0.0010)	(0.0009)	(0.0012)
R^2 / Observations:					
20 Years Old	.22 / 5,964	.24 / 13,635	.17 / 1,331	.17 / 6,140	.24 / 2,805
Older	.43 / 143,310	.39 / 370,085	.38 / 46,930	.38 / 365,742	.38 / 331,402

Notes: Dependent variable is the log of monthly wages. Sample consists of all firms with 50 or more employees. Standard errors, in parentheses, are robust to heteroscedasticity of unknown form, and listed point estimates are significant with all p-values less than 0.01. The remaining results from estimation of equation (1) are suppressed for the sake of brevity.

Table 4: Returns to Years of Schooling by Industry, 1986-1998
Spatial and Industry Fixed-effects Estimation of Equation (1)[a]

	1986	1989	1992	1995	1998	% Change 1986-1998
Industry	0.070	0.070	0.073	0.095	0.097	39%
	(0.0011)	(0.0015)	(0.0025)	(0.0024)	(0.0040)	
Construction	0.058	0.066	0.068	0.082	0.096	66%
	(0.0013)	(0.0023)	(0.0044)	(0.0045)	(0.0075)	
Agriculture	0.052	0.041	0.053	0.067	0.065	25%
	(0.0007)	(0.0014)	(0.0029)	(0.0029)	(0.0040)	
Transport/Communications	0.080	0.080	0.098	0.115	0.125	56%
	(0.0045)	(0.0057)	(0.0082)	(0.0068)	(0.0088)	
Trade	0.071	0.078	0.086	0.106	0.136	92%
	(0.0014)	(0.0025)	(0.0035)	(0.0038)	(0.0042)	
Services	0.064	0.075	0.088	0.105	0.126	97%
	(0.0015)	(0.0020)	(0.0033)	(0.0029)	(0.0030)	
Health and Social Services	0.058	0.080	0.073	0.097	0.083	44%
	(0.0006)	(0.0003)	(0.0025)	(0.0014)	(0.0012)	
Public Services	0.078	0.108	0.102	0.115	0.113	44%
	(0.0006)	(0.0005)	(0.0011)	(0.0007)	(0.0011)	

[a] *Estimated returns are from separate regressions for each industry.*

Notes: Dependent variable is the log of monthly wages. Sample consists of all firms with 50 or more employees. Standard errors, in parentheses, are robust to heteroscedasticity of unknown form. The remaining results from estimation of equation (1) are suppressed for the sake of brevity. All point estimates for the experience and gender variables are statistically significant. All listed parameters are statistically significant with a p-value less than 0.001.

Table 5: Wage Premiums by School Type, 1986-1998
Spatial and Industry Fixed-effects Estimation of Equation (1)

	1986	**1989**	**1992**	**1995**	**1998**	**% Change 1986-1998**
School Types						
Primary	0.085	-0.025	0.068	0.074	0.101	19%
	(0.0049)	(0.0058)	(0.0114)	(0.0127)	(0.0192)	
Secondary, Vocational	0.209	0.087	0.226	0.209	0.222	6%
	(0.0053)	(0.0063)	(0.0156)	(0.0132)	(0.0203)	
Secondary, Technical	0.381	0.388	0.207	0.519	0.546	43%
	(0.0054)	(0.0063)	(0.0126)	(0.0136)	(0.0201)	
Secondary, General	0.303	0.259	0.464	0.475	0.507	67%
	(0.0059)	(0.0064)	(0.0122)	(0.0141)	(0.0202)	
College	0.554	0.531	0.765	0.835	0.869	57%
	(0.0068)	(0.0063)	(0.0128)	(0.0136)	(0.0199)	
University	0.720	0.741	0.981	1.055	1.166	62%
	(0.0057)	(0.0067)	(0.0145)	(0.0153)	(0.0218)	
Observations:	149,274	383,720	48,261	371,882	334,207	
R-squared:	0.46	0.42	0.43	0.40	0.40	

Notes: Dependent variable is the log of monthly wages. Sample consists of all firms with 50 or more employees. Standard errors, in parentheses, are robust to heteroscedasticity of unknown form. The remaining results from estimation of equation (1) are suppressed for the sake of brevity. All point estimates for the experience and gender variables are statistically significant. The firm-size and industry dummies are jointly significant as well as the county, fixed effects. All listed parameters are statistically significant with a p-value less than 0.001. The point estimate for primary schooling in 1992 has the smallest t-statistic with a value of 10.6.