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LONG-RUN TRENDS IN EARNINGS AND EMPLOYMENT IN HUNGARY, 1972-1996[1]

ÁRPÁD ÁBRAHÁM and GÁBOR KÉZDI

Transition from socialist to capitalist economy led to enormous changes in earnings and employment. In our study a long-horizon descriptive analysis is presented about the major trends, including the last fifteen years of socialism. Education, gender, calendar time, age and vintage effects are separately analyzed. Aggregate (quasi-) panel analysis is used to assess the role of labor demand and labor supply, concluding that exogenous supply factors explained most of what happened before the transition, while the transition itself was dominated by large labor demand shocks. These demand shocks are in large part structural, as opposed to cyclical, and are highly correlated with vintage, gender and education. The main results are summarized in a list of stylized facts.

1. INTRODUCTION

The motivation of this paper is to provide a comprehensive study of the major trends on the labor market in the last two decades of socialism and the transition (for the period 1972–1996). There are excellent studies about wages and earnings as well as employment and unemployment. Most of them address some specific problems in addition to descriptive analysis.

[1] Comments of the participants of the Budapest Workshop on the Labor Market (1998) were very helpful. We also thank Zsombor Gergely for his help. Financial support from the OTKA grant of the Hungarian Academy of Sciences and the Phare ACE program of the European Union is acknowledged. All errors are ours. Contact: abraham@upf.es or kezdi@umich.edu.

Kertesi–Köllô (1997, 1998, 1999a, 1999b), are such papers, just to mention a few. Our study is new in three respects. We examine a wider time-horizon, we follow trends in wages and employment in a unified framework, and partly for these reasons, we follow a longitudinal approach as opposed to the mostly cross-sectional ones mentioned above. We focus on providing a general picture, with cohort effects and longitudinal analysis.

This is an empirical study, based on a number of different data sources. Our data has quasi-panel structure: time-series of consecutive cross-sections in gender-schooling-cohort cells. This allows us to decompose and analyze the wage - labor supply relationship along the major demographic dimensions. The advantages of this approach are coming from the fact that we are following groups of people: these attributes don't change over time and transitory effects are averaged out. The disadvantages are also coming from the grouping: this is not a real panel, hence variability is reduced and we have measurement error in variables because they are sample estimates. Naturally, the enormous compression of the information makes it impossible to examine many finer details. On the other hand, it enables us to concentrate on the general trends and their interactions.

The main objective is to establish stylized facts that are relevant for economic policy and can be subject of further studies. Even among the mostly descriptive results, some of our findings may be controversial and questionable, or our interpretation may be incorrect. Our goal is to initiate discussion about these facts. Comparison with results from other transition economies would be welcome.

The structure of the paper is the following: in the second section we describe the data we used, then we proceed with a descriptive analysis in the third section. The fourth section provides the decomposition of the trend in wage inequality into cohort and year effects and establishes age-earnings profiles. The econometric analysis is completed by a panel study of the changes in wages and labor supply. At the end, we summarize what we think the most important results in a list of stylized facts and conclude.

2. THE DATA

We used four different kinds of data sources to construct our "quasi-panel" database. (1) The Hungarian Central Statistical Office (CSO) conducted 4 major surveys in the 1970s and the 1980s that provide us with detailed and valid employment and earnings data (the latter collected from both the respondent and the employer). These were the Income Surveys (CSO IS-s) of 1973, 1978, 1983, and 1988, and the information in them was related to the preceding year (1972, 1977, 1982, and 1987). (2) The 1990 Census provides us with participation, unemployment and self-employment data for the year 1989. (3) From 1992 on, the CSO has been conducting a Labor Force Survey (CSO LFS) on a quarterly basis. Among other things, this survey contains ILO-standard measures of participation and unemployment, but it does not contain any information about earnings. (4) The fourth data source we use are the Wage Surveys of the National Labor Center (NLC WS) that provide us with the earnings data from the mid-1980s on, collected from the employers. For more detailed description of the data see the Data Appendix.

Our analysis focuses on a limited number of variables. Besides gender, date of birth and schooling, we use labor force participation, employment (and unemployment), wage/salary and full-time wage/salary employment, and monthly earnings, including wages and other work-related income. In what follows, we use the term wages and earnings in an interchangeable way, although we always mean monthly net earnings. The definition and measurement of these variables are explained in the Data Appendix. One important feature of the earnings data is that they are net (as opposed to gross) measures. Personal income tax was introduced in 1988 in Hungary, in a way that the 1988 earnings were "grossed up" such that they matched disposable earnings of the previous years. In a long-run comparison, therefore, one has to use the net variable. In addition, since we focus more on the supply side of the labor market, it is natural to consider earnings that people receive, as opposed to total costs of labor. The net figures are, however, estimates, which we think are biased downward (although we have no evidence proving this, see the Data Appendix, again).

The multi-source nature of our data raises questions about compatibility. Participation and employment are measured in the same way in the three different surveys, in general. The most important exception is unemployment, and therefore, labor force participation. Unemployment is measured by the ILO standards in the surveys we use from 1992 on, but it is based on (self-reported) registration in 1989. Some less important figures are interpolated from other surveys, see the Data Appendix. The compatibility of the earnings data is more of an issue. We have 2 types of sources, one (CSO IS) representing the total population, and one (NCL WS) representing those working in not too small firms (20 or more employees). The 1987 CSO IS and the 1986 NCL WS offer some possibility to judge whether it is valid to compare earnings from the two sources. As Table 5 and Figures 8-12 in the next section show, the estimates of mean and group-mean earnings seem to line up reasonably well. Table 6 and Figures 13-14 suggest that there is a significant difference in the dispersion of wages between the two surveys, but this difference may be part of an existing trend. We conclude that even if there are significant discrepancies in the two sources, our estimates are not biased much from them, possibly because of the aggregation.

The basis of our analysis is a time-series of independent cross-sections, that is a panel, or as we call, quasi-panels database. For particular groups of people, we estimated participation, employment and other "quantity" variables, and the mean and variance of earnings from the original data sets. These groups were defined by year of birth (in 5-year-wide intervals), gender and educational attainment (in 3 categories: 0-11 classes, secondary school and college or more educated). Our data set is a collection of these estimates, connected for each group through the years. We have 49-51 observations ("cells") in a particular year. Note that the characteristics that define a group don't change for an individual, therefore the quasi-panel allows us to follow groups of the same people. The advantage of this approach, we hope, is apparent in the two last sections of this paper.

Similar aggregate quasi-panels are used in the literature, although some of them connect people of the same age group instead of the same years of birth. See, for example, Deaton (1997, Chapter 2), or Katz and Murphy (1992). Note, that these data sets are technically panels, we use the quasi-term only to distinguish them from individual or household-panels that follow individuals themselves.

The large samples enable us to get quite precise estimates for the cells. We estimated the standard errors of the proportion estimates (e.g. labor force participation rate), and the mean earnings by bootstrap, except when the bootstrapping results indicated smaller sampling error than the simple random sampling estimates.[2] In the CSO Income Surveys, and the CSO Labor Force Surveys the former ones always exceeded the latter ones, while in the NLC Wage Surveys the bootstrap estimates were almost always smaller, partly because of the enormous sample sizes, and partly perhaps because of efficient stratification (see the sample design effects in the Data Appendix). In each case, we took the larger of the two estimates for our standard error. This was motivated by the fact that the NLC WS bootstrap estimates may be smaller only because of the one-step method.

Table 1

Standard Errors of the Cell Aggregates

	Mean	Median	75th Percentile	95th Percentile
Participation [a]	0.0135	0.0114	0.0176	0.0360
Employment [a]	0.0135	0.0114	0.0176	0.0360
Wage-Employment[a]	0.0194	0.0168	0.0230	0.0387
Full-time Wage-Employment[a]	0.0199	0.0167	0.0246	0.0383
Real Net Earnings[b]	0.0089	0.0033	0.0132	0.0429

[a]As the fraction of total working age population (Note: employment and participation is equivalent before 1989, because of full employment).
[b]Relative SE (SE/Point Estimate)

[2] We have chosen the bootstrapping strategy because the sample designs were rather complicated, and in some cases the exact procedure was not clear. Note that the simple bootstrap underestimates the true error if the design includes cluster or multi-level sampling, which was the case for all surveys. Unfortunately, without the variables identifying the primary sampling units a better estimation was impossible. However, the size of the samples and their efficient stratified nature makes us believe that we probably did not err too much.

There are two reasons why we need to have estimates for the standard errors. First, in the last section, we have a model where one of the estimated variables is on the right-hand-side. The fact that these variables are estimates leads to a bias in the estimator of that model, which we can estimate (and therefore correct) if we have estimates for the measurement error. Second, one has to keep the order of the standard errors in mind to judge whether the changes presented in the next section are significant in a statistical sense. Note that these are for cohort×gender×education cells, so we will have lower standard errors, by the factor of the square root of the increase in sample size.

The "quantity" estimates for the individual cells have a median standard error about 0.01-0.02, while the earnings estimates have 0.003. The upper tail in each variable has a standard error of about 0.04. The overall year-aggregates consist of 50 cells (see Table A4 in the Appendix), so the standard errors corresponding to them are of the order of 0.0005-0.001. The year×gender×schooling aggregates consist of 7-9 cells, so their standard errors are of the order of 0.001-0.007.

3. MAJOR TRENDS IN PARTICIPATION, EMPLOYMENT AND EARNINGS: DESCRIPTIVE ANALYIS

This section is an overview of the most important trends on the Hungarian labor market between 1972 and 1996. Although we aim at drawing a rather general picture, our results suffer from two kinds of shortcomings (apart from the subjectivity in our judgment about what is important). First, we focus only our basic individual characteristics: age, cohort, gender, and educational attainment. Important others like geographic, sectoral, occupational dimensions or family characteristics are not considered here. Second, we do not focus directly on the demand side of the labor market.

Both shortcomings are important: as we will show, inequalities rose dramatically within the groups defined by our simple variables, and the essence of the transition is the fundamental changes on the demand side. In spite of these problems, we think that the evidence presented in the following section captures very important facts, partly because the dimensions we are focusing at are among the most important ones, and partly because we can show indirect evidence about the left out characteristics. The ultimate goal of this chapter is to provide a list of stylized facts, which to be found in the summary. First, Table 2 gives an overview of the major demographic trends in Hungary.

Table 2

Major Demographic Changes, 1972, 1987, and 1996

				Relative Changes per Year	
	1972	1987	1996	1972-1987	1987-1996
Total Population (thousands)	10,831	10,621	10,212	–0.001	–0.004
Share of Active Age Popul.	0.53	0.52	0.50	–0.001	–0.004
Share of Education-Level Groups in the Active Age Population					
Overall					
0-11 Classes	0.79	0.66	0.60	–0.011	–0.009
Secondary School	0.15	0.23	0.28	+0.037	+0.020
College	0.06	0.11	0.12	+0.061	+0.013
Women					
0-11 Classes	0.80	0.62	0.54	–0.015	–0.013
Secondary School	0.16	0.28	0.33	+0.048	+0.020
College	0.04	0.10	0.13	+0.102	+0.027
Men					
0-11 Classes	0.79	0.70	0.66	–0.008	–0.006
Secondary School	0.14	0.19	0.23	+0.026	+0.022
College	0.07	0.11	0.12	+0.038	+0.002
30-34 Years Old	0.13	0.16	0.12	+0.015	–0.027
0-11 Classes	0.74	0.61	0.54	–0.012	–0.012
Secondary School	0.18	0.27	0.30	+0.034	+0.016
College	0.08	0.13	0.15	+0.039	+0.024

As we pointed out in the Data Appendix the figures which are presented here are not compatible with the traditional definition of active age population in two important ways: (1) we excluded all type of full-time students from our sample. (2) We excluded those men who were over 60 and those women who were over 55 (the corresponding retirement threshold levels), because holding a job after the retirement age had been completed became very rare, especially from the 1980s on.

The exclusion of these groups implies that in our data the proportion of low educated is smaller than it would be if we would use the sample of the total active age population (individuals between age 14 and 75). We will highlight all the cases when we think this non-standard definition leads to significant biases.

Table 2 shows two important tendencies: (1) the average educational level (for both genders) was increasing during the last 25 years, however in 1996 still 60 percent of the active age population held no more then apprenticeship. (Note, that this figure would be even worse if we used the standard definition of active age population.) (2) In 1972 men were slightly more educated, however by 1996 women were more educated mostly due to the fact, that the proportion of them with secondary degree is 10 percentage points higher than in the case of men. This implies that during this period the enrollment of women in both secondary and higher education was significantly higher.

Table 3 and Figure 1 to 7 show the most important trends in labor force participation. Note, that these figures are higher than figures based upon the standard active age population because mostly non-participant groups are excluded (full-time students and old-age pensioners), therefore they cannot be compared with other sources.

The tendencies are clear: in the last 15 years before the transition labor force participation increased. It was due to one direct and one indirect effect: (1) the increasing participation of low-educated women (2) and (mostly) the increase of average education, because both more educated men and women participate more. On the other hand the participation rate of all educational groups of men and highly educated women started to drop before the transition.

However the transition brought about a rather dramatic fall in overall participation. The participation rate fell for all educational groups of men and women. Low-educated women experienced the most dramatic drop. The decline has stopped for men by 1993, while for women it has stopped only around 1995.

Table 3

Major Changes in Labor Force Participation, In the Total Active-Age[a] Population and in the Pre-Retirement Ages[b,c]
1972, 1987, and 1996

	1972	1987	1996	Relative Changes per Year 1972-1987	1987-1996
Participation Rate					
Overall	0.83	0.86	0.74	+0.002	–0.016
Women	0.71	0.80	0.66	+0.009	–0.020
0-11 Classes Educ.	0.67	0.76	0.57	+0.009	–0.028
Secondary Educ.	0.87	0.87	0.74	+0.000	–0.017
College Educ.	0.93	0.88	0.82	–0.004	–0.007
Men	0.95	0.92	0.82	–0.002	–0.013
0-11 Classes Educ.	0.95	0.91	0.77	–0.003	–0.016
Secondary Educ.	0.97	0.94	0.88	–0.002	–0.008
College Educ.	0.99	0.97	0.94	–0.001	–0.003
Pre-Retirement Age[b,c] Participation Rate					
Overall[d]	0.69	0.71	0.50	+0.002	–0.032
Women	0.58	0.73	0.51	+0.018	–0.034
0-11 Classes Educ.	0.55	0.68	0.50	+0.016	–0.030
Secondary Educ.	0.80	0.90	0.66	+0.008	–0.030
College Educ.	0.94	0.90	0.86	–0.003	–0.004
Men	0.79	0.69	0.50	–0.009	–0.031
0-11 Classes Educ.	0.77	0.66	0.43	–0.010	–0.038
Secondary Educ.	0.88	0.70	0.56	–0.014	–0.022
College Educ.	0.96	0.83	0.74	–0.009	–0.012

[a]Women: age of 18-54; men: 18-59. Without students .
[b]Women: age of 50-54; men: 55-59.
[c]For the year 1996, the ratio is estimated from our panel in the following way: 0.8×(age group of 49-53)+02×(age of 54) for women, and 0.8×(age group of 54-58)+02×(age of 59) for men.
[d]0.5×women's rate + 0.5×men's rate.

Table 4 and Figures 1 to 7[3] show the evolution of unemployment and self-employment for the analyzed period. Unemployment was not existing before the transition, however evolved quickly from 1989 until it reached its peak in 1993. In Hungary female unemployment is lower than male one,

[3] All the vertical distances have a give economic interpretation on Figures 1 to 7. The distance between participation and employment rate measures the proportion of the unemployed among the active-age population. The distance between the employment rate and wage/salary employment rate measures the proportion of self-employed. And obviously, the distance between wage employment rate and full-time wage employment rate measures the part-time employment rate.

which is a rather unique feature of the Hungarian labor market. This is easy to explain if we observe that participation declined more among women that among men, therefore since women (may) have better outside opportunities they rather opted for exiting from the labor market, while men rather stayed in the labor market and searched for new employment.

Table 4
Major Changes in Unemployment and Self-Employment[a]
1972, 1987, and 1996

				Relative Changes per Year	
	1972	1987	1996	1972-1987	1987-1996
Unemployment Rate					
Overall	0.00	0.00	0.10	–	–
Women	0.00	0.00	0.09	–	–
0-11 Classes Educ.	0.00	0.00	0.12	–	–
Secondary Educ.	0.00	0.00	0.08	–	–
College Educ.	0.00	0.00	0.02	–	–
Men	0.00	0.00	0.11	–	–
0-11 Classes Educ.	0.00	0.00	0.14	–	–
Secondary Educ.	0.00	0.00	0.07	–	–
College Educ.	0.00	0.00	0.03	–	–
Self-Employment Rate (Among the Employed)					
Overall	0.15	0.15	0.26	+0.003	+0.082
Women	0.14	0.12	0.21	–0.007	+0.072
0-11 Classes Educ.	0.18	0.16	0.23	–0.008	+0.050
Secondary Educ.	0.02	0.09	0.20	+0.193	+0.131
College Educ.	0.02	0.03	0.14	+0.069	+0.341
Men	0.15	0.17	0.31	+0.012	+0.086
0-11 Classes Educ.	0.18	0.20	0.32	+0.007	+0.071
Secondary Educ.	0.05	0.15	0.32	+0.129	+0.131
College Educ.	0.05	0.11	0.25	+0.079	+0.147

The case of self-employment is rather complicated. Between 1972 and 1987 we cannot see much change. However it can be surprising that even in 1972 15 percent of total employment was self-employed. This is due to the fact that we considered the members of agricultural cooperatives as self-employed. We had several reasons for doing that: they had some freedom for their agricultural activities (they had some land for their individual use, they were growing and selling animals), they were not employees legally, and finally the compensation schemes in the cooperatives were very different from wage/salary schemes. Nevertheless, in no way we claim that they were (with the few exceptions of the members of some "special" cooperatives) sole proprietors.

We conjecture that there were important changes behind the observed stagnation in the level of self-employment before the transition. We know that there was a shift from agriculture to the other sectors to the economy during the 70's and 80's. At the same time during the 80's the opportunities for small businesses increased in a great extent. Therefore the underlying sectoral composition and the major characteristics of self-employment might change significantly between 1972 and 1987.

During the transition period the incidence of self-employment increased sharply. The increase was higher among men and among the higher educated. This tendency indicates, that the higher level of self-employment this time is reflecting the development of small private businesses. However we have to note that one of the most common way of avoiding the payment of the tax burden of salaries is to employ the same person as a sole proprietor with a business contract instead of employing her as an employee with a labor contract. This practice led to the increase of self-employment as well.

The Figures show that in the case of each group the proportion of full-time wage/salary employment was decreasing among the active age population. This decline was due to the decline in participation, to the occurrence of unemployment and to the increasing self-employment. On the other hand part-time employment did not change significantly. This is important because our earnings measure refers to the full-time wage/salary employment. However this definition covers much smaller proportion of the active age population after the transition. We have lower representation of the low educated (they participate less in the labor force and suffer from higher unemployment rates), and at the same time we have also less highly educated because of the increase in self-employment. We can expect that those with lowest productivity are the ones who are leaving the labor force (or losing their jobs), while those with highest productivity are the ones who become entrepreneurs. However both of these factors implies that ceteris paribus the observed wage/earnings inequality should decline (we lose people from the lower and upper tail of the earnings distribution).

Table 5

Major Changes in Relative Earnings (Average of the Year = 1), 1972-1987 and 1986-1996

	1972	1987	1986	1996	Relative Changes in Relative Earnings, per Year 1972-1987	1986-1996
By Education						
0-11 Classes	0.93	0.90	0.91	0.79	–0.003	–0.015
Secondary	1.04	1.02	1.00	1.05	–0.001	+0.006
College	1.54	1.43	1.43	1.63	–0.005	+0.015
By Gender						
Women	0.79	0.84	0.84	0.91	+0.004	+0.009
Men	1.15	1.13	1.12	1.08	–0.001	–0.004
By Age						
25-34	1.01	0.94	0.94	0.93	–0.004	–0.001
35-49	1.10	1.08	1.09	1.04	–0.001	–0.005
50-59	1.16	1.18	1.16	1.18	+0.001	+0.002
By Gender and Education						
Women						
0-11 Classes	0.73	0.73	0.73	0.68	–0.001	–0.008
Secondary	0.86	0.90	0.87	0.97	+0.003	+0.013
College	1.19	1.18	1.19	1.36	–0.001	+0.016
Men						
0-11 Classes	1.06	1.01	1.01	0.85	–0.003	–0.018
Secondary	1.22	1.18	1.17	1.16	–0.002	–0.001
College	1.72	1.64	1.60	1.90	–0.003	+0.021

Table 5 and Figures 8 to 12 show the evolution of real net earnings and relative net earnings. Real net earnings were declining since 1977, however this decline was accelerated sharply by the transition. The only exception is the 1992-1994 period, however the stability of real earnings was rather due to an “election budget” than economic development.

Before studying the pattern of relative earnings it is worth noting that (with respect to relative earnings) there is no significant difference by the 1986 and 1987 figures. It is important because, as pointed out in the previous section, the data for these two years are coming from two different sources (from the NLC WS for 1986 and from the CSO IS for 1987). These results therefore justify the use of the two sources in a unified framework.

Focusing on relative earnings by education, it seems that there were no important changes before the transition. The differentials were rather constant, although there was a slight tendency towards the narrowing of the educational differences. The transition brought about considerable changes. The relative earnings of low-educated employees fell sharply, while the ones with college degree gained in relative terms. Both change was sharper in the case of men than in the case of women. This indicates that the transition brought technological change, which favors skills, since at the same time the share of skilled labor among employment also increased. (See the last section for more about the role of labor demand in the transition.)

The raw measure of male-female earnings differential decreased from 21 to 9 percent. This decrease is partly due to the decreasing gap for most educational groups but also to the previously observed fact that women became more educated during the analyzed period. The role of the composition effect is highlighted by the fact that in 1996, the wage differential by educational groups is between 28 percent (for college) and 16 percent (for secondary degree), all higher than the overall differential.

On the other hand we cannot see too much movement with respect to age: the (aggregate) earning differentials of different age groups were rather stable. The only significant change was the 7 percentage points decline of the relative wage of the 25-34 year age group between 1972 and 1986.

Finally, we can follow the dispersion of earnings in Table 6 and on Figures 13 and 14. The first observation we can make that the dispersion was rather constant in time before the transition with the important exception of low-educated women, for whom it was decreasing a lot. As it is expected, it was increasing with age. Also as expected, it was increasing with education in the case of men. However in the case of women we can observe the opposite direction: the dispersion was rather decreasing with education. We do not have a clear explanation of this feature of the socialist labor market. One explanation can be that highly educated women during the socialism were mostly employed by the public sector (education, health and public administration) where wage setting was extremely rigid. However this is not explaining the fact that wages of women with 0 to 11 classes have much higher wage dispersion than men with the same educational level.

Table 6

Major Changes in the Relative Dispersion (Std. Error/Mean) of Earnings, 1972-1987 and 1986-1996

					Relative Changes in Relative Dispersion, per Year	
	1972	1987	1986	1996	1972-1987	1986-1996
Overall	0.48	0.47	0.44	0.73	–0.001	+0.071
By Education						
0-11 Classes	0.46	0.42	0.39	0.50	–0.007	+0.033
Secondary	0.43	0.42	0.41	0.55	–0.002	+0.037
College	0.55	0.57	0.54	0.90	+0.003	+0.076
By Gender						
Women	0.57	0.43	0.46	0.64	–0.016	+0.045
Men	0.43	0.47	0.44	0.77	+0.005	+0.083
By Age						
25-34	0.35	0.42	0.41	0.63	+0.014	+0.060
35-49	0.44	0.47	0.45	0.72	+0.005	+0.067
50-59	0.47	0.55	0.54	0.82	+0.011	+0.060
By Gender and Education						
Women						
0-11 Classes	0.65	0.50	0.48	0.60	–0.015	+0.029
Secondary	0.43	0.42	0.38	0.50	–0.003	+0.034
College	0.37	0.41	0.39	0.73	+0.006	+0.095
Men						
0-11 Classes	0.38	0.37	0.35	0.45	–0.001	+0.033
Secondary	0.42	0.42	0.42	0.58	+0.000	+0.042
College	0.57	0.62	0.58	0.96	+0.005	+0.074

The transition brought about a sharp increase in the dispersion of earnings for all groups. It also widened the gap between different educational and age groups (the case of women still remained puzzling). A new phenomenon is that now there is a significant difference between the dispersion of men's earnings and women's earnings.

Figures 13 and 14 approach the question of earnings dispersion from another viewpoint. How much of the dispersion of the earnings can be explained by gender, education and age ("between-group inequality") or alternatively, how big is the role of another factors not captured by our cell-defining variables ("within-group inequality")? We found that before the transition not only the dispersion of earnings but also the "composition" of this dispersion was rather constant, and within group variation was responsible about 60 percent of total variation. After the transition all measures of variation increased, however the within group variation did so the most. Here we have to note again, that by the transition mostly people from the lower and perhaps the upper tail of the earnings distribution dropped from the sample of full time wage/salary workers. This may bias downward the extent of the importance of these dimensions as determinants of overall inequality. Nevertheless, the conclusion that after the transition other characteristics than gender, education and age became increasingly important seems to be robust.

4. DECOMPOSING THE TRENDS IN EARNINGS: A PANEL ANALYSIS

So far, we did not use the panel nature of our database. In what follows, we exploit that in a simple decomposition of the earnings trends. A similar process is discussed in Deaton (1997), Chapter 2.7, also on cohort-aggregated time series of cross-sections. We estimate a regression model by education levels, with fixed effects for cohort, year and year-gender interaction and with a 2nd order polynomial in labor market experience. This allows us to decompose the changes in real net earnings into cohort- (or vintage-) and year-effects, estimate the time path of the gender earnings gap, and estimate the experience-earnings profile in a longitudinal setup.

We estimate the following model:

$$w_{cgst} = \gamma_{cs} + \theta_{st} + \delta_{gst} + \alpha_s \, exp_{cgst} + \beta_s \, exp^2_{cgst} + \varepsilon_{cgst}$$

where index c refers to the different cohorts (that is, vintage), g to gender (1 being female), s to schooling attainment (our usual 3 categories), and t to time. w_{cgst} is the average earnings in cell cgs at time t. γ_{cs} is cohort fixed-effects, θ_{st} is year fixed-effect, δ_{gst} is gender-year interaction effect. With this specification, δ_{gst} measures the trends in the inter-gender earnings differences, or the changes of the earnings (wage-) gap. exp_{cgst} is potential labor market experience, estimated in the usual way (age, which here is the age in the middle of the cohort, minus 6, minus 9, 12 or 16, depending on schooling level). In what follows, we use age and labor market experience in an interchangeable way. ε_{cgst} is the error term.

The estimation of the effects of labor market experience is subject to important identification problems. In a cross-sectional sample, it is impossible to distinguish between cohort (that is, vintage) and age effects. An estimated cross-sectional age-earnings profile shows two main effects: that of labor market experience and that attributed to the cohorts themselves. If, for example, the knowledge people acquire in school improves over time, the younger cohorts start with a better endowment of human capital, which makes their initial (and subsequent) wages higher than that of the former cohorts experienced. This phenomenon would lead to a flatter profile in cross-section than in longitudinal analysis, because the latter estimates the individual paths that are not directly contaminated by the improvement in schooling. The longitudinal approach would lead to an unbiased estimate of the true experience-earnings profiles, whatever their theoretical content is.

Unfortunately, things are a little bit more complicated. Cohort-specific differences can have two different effects: instead of simply "shifting" the profiles upward, they can change its slope, too. The longitudinal age-earnings profiles can change because of three effects. (1) Something happened to everybody's wage at that particular point of time (a shift to all wages at time t: year effects). (2) The different cohorts have different initial endowment when entering the labor market and this increases their wage throughout their career (a shift of the profile in the different cohorts: cohort fixed-effects). (3) The different cohorts experience different growth rates along their age-earnings profile (different slopes for different cohorts:

cohort growth effects). In a longitudinal context, one can estimate only two of the three effects. We have chosen to estimate the first two, therefore the fixed, cohort (and year) independent age (experience) effects.[4]

Our model is a little bit more restrictive and, in the same time, a little bit richer than what one would expect from a decomposition exercise (see, for example, Deaton, 1997). Instead of estimating fixed age-effects, we put a quadratic restriction on our age variable (measured as potential labor market experience). On the other hand, we allowed for gender and year interaction. In our sample, there is a clear trade-off between the interaction and the fixed age-effects specifications: even in this specification, we have 99 coefficients for the 501 observations. As the results show, there are non-trivial changes in the gender differences through time, so omitting the interaction would contaminate the other results. Moreover, the quadratic specification in potential experience is standard in the earnings functions, therefore this allows us to compare our results to other estimates.

We have chosen a graphical representation of the results, because of the large number of the estimated parameters. The point estimates of the specification with real wage (as opposed to log wage) on the LHS are plotted by education level in Figures 15-18. By construction, the estimated coefficients measure changes in earnings, and every change is normalized to 0 in 1972, the base year. Wages (i.e. earnings) are in 1989 terms.

The year-effects (Figure 15) show a mild decreasing trend of real earnings in the 1980s, followed by a dramatic decline during the transition, from 1987. One can also detect some of the swings of the Hungarian macroeconomic policy, such as the increase of real wages about 1994, and the effect of the 1995 correction. The time trend is basically the same in the different education groups. The gender gap (Figure 16) estimates imply a very similar trend than what the overall aggregates of the raw data show (Figure 10), except for the highest schooling levels. The 1980s and the transition experienced a steady decrease in the wage-advantage of men, for the secondary school and less educated. On the other hand, the gender gap did not change much for the college educated employees. The share of

[4] That's why the α-s and the β-s don't depend on c or t. The first to encounter this identification problem were Weiss and Lillard (1978), in an individual panel context.

college-educated women in the labor force increased faster than that of men during the whole period, while the increase of secondary-school educated women became smaller. In the same time, the participation rate of women dropped much faster among the secondary school and less educated. Therefore, the decreasing gender gap might be partly explained by changes in labor supply. Namely, less productive women probably left the labor force in a greater extent (however these supply changes were endogenous).[5]

Figure 17 and 18 present the cohort effects, the first of the two showing the fixed-effects themselves, the second one the year-to-year differences. The cohort effects are increasing, in a monotone and significant fashion. The more educated gain more by the cohort effect. Figure 18 shows more clearly the growth rates and the inter-group differences in them. The fixed cohort-effects increase more for oldest cohorts than the ones that were born right before or after World War 2. The most interesting phenomenon, however, is the dramatic increase in the gains for those born after the mid-1960s, especially in the case of the more educated. Kertesi and Köllô (1999b), and Kézdi and Köllô (2000) attribute the decrease in the slope of the cross-sectional age-earnings profiles to unobserved cohort-effects. From a slightly different angle, it is the same phenomenon we observe here.

Figure 19 shows our longitudinal estimates of the experience-earnings profiles, in relative terms. The profiles show the familiar concave pattern, with higher slope for the more educated at each point of time. They do not have a decreasing part. In fact, the profiles do not even "flatten out" towards retirement. There are two reasons why this might happen. First, the lower employment rate for older cohorts (especially during the transition) results in a selection bias when comparing wages throughout the life cycle. Second, implicit employer-employee contracts might also have a role. Note that the American literature relates the fact that profiles

[5] The large fluctuations in the college-group might result from both data error or real life changes. The latter explanation is supported by the relative smoothness of all other parameters and the fact that the transition was accompanied by rather high volatility in the real wages of public employees, most of them being women.

were found steeper than thought before to these kind of contracts, as opposed to simple selection bias (see, for example, Lumsdaine and Mithcell, 1999). In addition to the standard arguments, there is another rational for the existence of implicit contracts in our case. Throughout most of the period, the Hungarian pay-as-you-go pension system used to focus only on the earnings from the last years when calculating pensions.

Our estimates of the age-earnings profile are a lot steeper than those identified by cross-sectional differences. For comparison, we estimated a more restricted model: instead of fully interacting schooling level with all effects, we constrained all effects to be the same, except for an education-specific intercept. Figure 20 presents the profile estimated this way, together with a cross-sectional estimate for 1989 by Kertesi and Köllô (1999b). As we pointed out, the cross-sectional estimates do not measure the real life-cycle growth of earnings when vintage effects are present. Technically, the cross-sectional estimates are smaller because they cannot include the cohort effects, which, if increasing, are negatively correlated with age. In addition to this problem, estimates can be biased for other omitted variables. Even our longitudinal estimate suffers from this problem in the restricted model of Figure 20: the estimated profile is below all of the ones shown in Figure 19, although it is supposed to be an average of them. This indicates that the restriction to common effects (especially common cohort-effects) in all schooling groups creates negative correlation between the profile and the error term. If, moreover, ignoring observed heterogeneity (schooling level) has similar effect than ignoring unobserved heterogeneity, the same argument implies that even the separately estimated profiles are biased downward.

All of this does not mean that quasi-panel models are superior to the cross-sectional ones in every respect. Indeed, a lot of variation is lost in our analysis that is very important in determining earnings. Moreover, selection bias might distort the longitudinal estimates, just like the cross-sectional ones. In an aggregate quasi-panel setup, it is even more difficult to control for this bias. Finally, it is even more important to remember that our estimates are counterfactual: they are "cleaned" from year effects. Employees themselves don't experience the steep increase in earnings if year-effects are negative, as it is the case from the 1980s on.

5. CHANGES IN EARNINGS AND LABOR SUPPLY: A SIMPLE PANEL ANALYSIS

In this section, we focus on the joint changes in wages and labor supply, in our quasi-panel context. We think of this estimation as a partial equilibrium comparative static exercise. That is we look at each (price, quantity) realization, and try to recover some patterns from the joint changes. We use the term equilibrium routinely, but without implying anything about whether markets actually clear. Moreover, although at some points we argue that the changes we see are caused more by demand- or supply-side changes, we basically follow a reduced-form approach. That is, we do not specify structural relationships (in any sense of the word). That is, from this analysis, we are not able to tell what happened to labor demand or labor supply. Instead, we can compare the resulting (price, quantity) points. Our question basically is whether prices and quantities moved the same or opposite direction in certain periods, and how strong was their relation. We try to argue about the possible factors behind the changes from this information.

Let c denote cohort, i the gender×education groups (i=gs in the notation of the previous section), and t time. Also, let $\ln w_{cit}$ denote the log of net real wages, and $\ln s_{cit}$ denote the labor supply or employment of group i of cohort c at time t. We measure s_{cit} in four different ways: (1) total employment plus unemployment (labor supply: labor force participation), (2) the number of wage/salary employees plus the unemployed (labor supply: participation in the market of wage/salary employees), (3) total employment, and (4) wage/salary employment. We estimate the model with each left-hand-side variables.[6] The basic idea behind these distinctions is the possible distinction between supply and demand (1&3 vs. 2&4) and the possible dual nature of the labor market

[6] s_{cit} is number (as opposed to fraction). To avoid variation in it due to sampling error other than that in the fractions themselves (participation rate, self-employment ratio), we measure s_{cit} as the corresponding fraction at time t times the sample-median cell size.

(1&2 vs. 3&4). Obviously, when focusing on labor supply in this setup, the supply decision is reduced to labor force participation (and self-employment). We divide our sample into a pre-transition and a transition part (1972-1987 and 1987-1996, respectively). The difference between participation and employment is unemployment, which was zero before 1989, therefore the two are not distinguished in the pre-transition analysis. We estimate the following model in the two subsamples:

$$\ln s_{cit} = \alpha_{ci} + \beta_i \ln w_{cit} + \varepsilon_{cit}$$

α_{ci} are the group (cohort×gender×schooling) level fixed effects, while β_i is the percentage change in participation or employment, corresponding to a one percent change in earnings. It is tempting to interpret this coefficient as an elasticity, but our approach does not imply what kind of elasticity it would be. There are two issues involved: that of equilibrium and that of reduced form. The first issue is the validity of the comparative static interpretation, and it is related to the nature of unemployment. Therefore it can be analyzed by looking at differences in participation and employment. The second issue is about identification of changes in supply and demand. Let us focus on the latter one first.

If labor supply changes only because of exogenous, that is demographic or other non wage-related factors, then β_i identifies the long-run elasticity of labor demand for group *i*. If, on the contrary, changes in quantities are caused solely by the changes in wages, then it is the long-run elasticity of labor supply. Unfortunately, in our case, neither of these assumptions can be made.[7] There are certainly autonomous, life-cycle

[7] Freeman (1979), and Katz and Murphy (1992) analyze a similar problem in a different setup: they look at how demographic shocks affect wages of a given age-group in the U.S. Katz and Murphy (1992) argue that the cross-time variation in the size of the age-schooling groups can be treated as exogenous to what happens on the spot market for labor. This variation, therefore, identifies the slope of the aggregate labor demand curve. Even if this strategy can be justified for their case, it would not work for us. We observed large structural changes in participation within any age group and within any cohort: the transition experienced increasing non-participation and entrepreneurship. Therefore, it seems that a large part of the cross-time variation of participation in a given age group is endogenous to what happens on the labor markets.

related changes in the participation rate within a group (defined by cohort×gender×schooling), and the size of those groups certainly affects their wages. On the other hand, we have no reason to rule out the possibility that participation decisions are made endogenously.[8]

Our aim is, therefore, more modest. If the observed correlation is negative, then we cannot rule out that only exogenous supply changes caused all the movement on the labor market. In the opposite case, this hypothesis can be rejected. Indeed, one might think that negative correlation implies that exogenous labor supply factors dominated the market, while positive correlation implies that it is labor demand that played the dominant role. The more significant the co-movements are, the more reasonable is this argument.

The above reasoning makes sense in a comparative equilibrium framework with upward sloping supply curves and downward sloping demand curves. On the other hand, it is not easy to think in terms of equilibrium if there is a gap between participation and employment. It turns out, that this problem does not influence the argument much. This is the result of the nature of the transition unemployment. In addition to justifying the conclusions of the comparative statics, we can get some additional insight of what happened in Hungary by distinguishing participation and employment.

The sharp increase in unemployment during the transition indicates large shocks to labor demand. This shock probably had cyclical and structural elements: the collapse of the soviet-led trade system implies the first one, privatization, new competitive environment and technology import the second one. Structural shocks result in structural unemployment, cyclical shocks in cyclical unemployment. Therefore, one can assess the relative importance of the two by focusing on the nature of unemployment.

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[8] Indeed, by choosing cohorts as opposed to age groups, we maximize the endogenous part of the within-cohort variation of labor supply.

If unemployment is largely structural, then workers with unmatched skills get discouraged and give up looking for a job. Therefore, they become non-participants. The unemployed are in large part those that did not give up yet but otherwise are similar to the ones that became non-participants. In this case, the equilibrium quantity on the labor market is close to actual employment. A structural change in labor demand leads to lower wages and lower employment of the group with the wrong skills. Those displaced sooner appear as non-participants at a given point of time, and those displaced later appear as unemployed. This story implies strong co-movement of wages and employment, and wages and participation, too, the second being slightly weaker because of the adjustment. On the other hand, if unemployment is cyclical, smaller proportion of the unemployed give up finding a job, and they probably search longer. Therefore, while a similar co-movement is expected between employment and wages, that of participation and wages should be significantly smaller. In our notation, if β_i is positive and only slightly larger when the left-hand side variable is employment, structural shocks are likely to be responsible for things. If β_i is significantly smaller in the second case, however, cyclical components are probably very important, too.

We estimate the within- (fixed-effect) and the between estimates of β_i, in two subsamples (pre-transition and transition) with two LHS variables (standard labor supply and without self-employment). Let the A superscripts denote within-group average, and D superscripts the deviation from it: $x^A_{ci} = T^{-1}\sum_{t=1}^{T} x_{cit}$, $x^D_{ci} = x_{cit} - x^A_{ci}$. Then, the within- and the between estimators are estimated by the following equations, respectively:

$$\ln s^D_{cit} = \beta_i \ln w^D_{cit} + \varepsilon^D_{cit}$$

$$\ln s^A_{ci} = \alpha_{ci} + \beta_i \ln w^A_{ci} + \varepsilon^A_{ci}$$

The within-estimator measures the co-movement of earnings and participation/employment for a given group. The between-estimator compares the group-averages. The first, therefore, measures how a given group of people change their participation as earnings change, or how earnings change as participation varies. It is independent of cohort, gender and schooling effects, and also the size of the particular group, but not of life-cycle patterns and year effects. The second is a more complex measure: it focuses on differences independent of life-cycle variation of lifetime earnings and total lifetime participation/employment. It mixes cohort, gender and schooling effects, and is influenced by the relative size of the different groups, and year-effects, too. Note that the variation of the variables is considerably smaller in the within-model, therefore its precision is smaller.

We had to face a problem coming from the aggregate nature of our data. Both the LHS and the RHS variable are sample estimates, therefore both suffer from measurement error. The measurement error in $\ln s_{cit}$ leads "only" to inefficiency, and this cannot be cured. On the other hand, the parameter estimates are biased toward zero because of the measurement error in wages. In the between-estimator, this bias is negligible given our precision of the aggregate estimates (see the Data section). However, in a panel model this problem can be serious even if the errors are relatively small. The reason is mainly the smaller variation in the differenced variables. We estimated the measurement-error bias of our estimates, and we found that the bias is not significant: it is always below 10 per cent of the parameter estimates. Se the Appendix for the details.

Table 5

Percentage Change of Labor Supply and Employment, Corresponding to a One Per Cent Increase in Real Earnings. Total Labor Market and Wage/Salary Employment, Within-and Between-Group Estimates. 1972-1987 and 1987-1996 Subsamples *(Standard Errors in Parentheses)*[a]

	1972-1987		1987-1996			
	Participation		Participation		Employment	
	Within	Between	Within	Between	Within	Between
	Labor Force Participation and Total Employment					
Women, 0-11 Classes	0.17 *(0.14)*	–1.01 *(0.27)*	0.53 *(0.13)*	1.14 *(0.43)*	0.69 *(0.15)*	1.45 *(0.47)*
Women, Secondary School	–0.07 *(0.10)*	–1.10 *(0.27)*	0.43 *(0.21)*	1.07 *(0.41)*	0.49 *(0.23)*	1.38 *(0.45)*
Women, College	–0.03 *(0.10)*	–1.17 *(0.26)*	0.11 *(0.25)*	0.95 *(0.40)*	0.15 *(0.28)*	1.25 *(0.43)*
Men, 0-11 Classes	–0.06 *(0.11)*	–0.95 *(0.26)*	0.60 *(0.11)*	1.14 *(0.41)*	0.77 *(0.12)*	1.44 *(0.45)*
Men, Secondary School	0.01 *(0.09)*	–1.09 *(0.26)*	0.47 *(0.16)*	1.00 *(0.40)*	0.54 *(0.18)*	1.30 *(0.44)*
Men, College	–0.07 *(0.09)*	–1.10 *(0.25)*	0.55 *(0.22)*	0.93 *(0.39)*	0.56 *(0.25)*	1.21 *(0.42)*
	Participation and Employment on the Wage/Salary Market					
Women, 0-11 Classes	–0.07 *(0.15)*	–1.06 *(0.26)*	0.48 *(0.14)*	1.06 *(0.41)*	0.66 *(0.16)*	1.42 *(0.46)*
Women, Secondary School	–0.28 *(0.10)*	–1.13 *(0.25)*	0.43 *(0.22)*	1.00 *(0.40)*	0.51 *(0.25)*	1.36 *(0.44)*
Women, College	–0.07 *(0.10)*	–1.19 *(0.25)*	0.34 *(0.26)*	0.88 *(0.38)*	0.39 *(0.31)*	1.23 *(0.43)*
Men, 0-11 Classes	–0.29 *(0.12)*	–1.00 *(0.25)*	0.62 *(0.11)*	1.06 *(0.40)*	0.83 *(0.13)*	1.40 *(0.44)*
Men, Secondary School	–0.27 *(0.10)*	–1.13 *(0.25)*	0.70 *(0.17)*	0.93 *(0.39)*	0.81 *(0.20)*	1.27 *(0.43)*
Men, College	–0.21 *(0.10)*	–1.13 *(0.24)*	0.74 *(0.23)*	0.85 *(0.37)*	0.75 *(0.27)*	1.19 *(0.41)*

[a]The parameter and the SE estimates are not corrected for measurement error bias. The bias is small: for the within-estimator it is around 5-10 per cent of the estimates, for the between-estimator, it is around 1 per cent.

The results have a rather clear pattern. Pre-transition estimates are negative (or cannot be distinguished from zero), while transition estimates are positive. Within-estimates are always considerably smaller in absolute value than between-estimates. Ceteris paribus, male-female differences are negligible, except for some within-estimates for the transition, and schooling does not change the estimates a lot, either. Note that although measurement errors bias the within-estimates more than the between-estimates, their effect cannot explain the large difference. Also, while within-group variation is a lot smaller and thus yields less precise estimates, the difference between the two is significant. It has, therefore, an economic interpretation. In fact, this interpretation is probably different in the two periods, since the size of the 'elasticity' is different.

The separate treatment of the labor market of wage/salary employment did not lead to very different results. With the only exception of the pre-transition within-estimates for men, the estimates are basically the same, and the small differences don't follow any clear pattern. One interpretation of these results is that self-employment does not divide the Hungarian labor market into very different segments, or if yes, then both were subject to very similar shocks and both reacted in a very similar way. Therefore, at this aggregate level, the conclusions drawn based on analyzing the wage/salary market are by and large valid for all employees. On the other hand, the heterogeneity and the changing composition of the self-employed prevents us to detect significant discrepancies for some sub-groups.

In general, we can conclude that the transition brought about large changes in labor demand, while the changes in the socialist labor markets were either driven mostly by exogenous changes in labor supply.[9] It should be noted, however, that in this argument 'exogenous' changes in labor supply are orthogonal to the actual situation but are related to the past endogenous decisions. For example, this can happen through the determinants of schooling level, either by individual choice or central planning.

The estimates for employment are larger than those for participation, the difference being (jointly) significant in a statistical sense, not negligible but also not too large in an economic sense. Labor market

[9] Exogenous variation in labor supply is probably much larger between groups than within them. The reason is that it is partly caused by demographic changes and schooling trends, while within-variation is not. We think that the fact that between estimates are much more negative in the pre-transition period justifies our conclusion of labor supply-driven changes, as opposed to the view that pre-transition changes simply cannot be understood in a market equilibrium context.

changes in the transition were, therefore, dominated by structural demand shocks, that were highly correlated with schooling, cohort and gender.

6. SUMMARY: STYLIZED FACTS

At the end of our investigations, we summarize what we think are the most important facts we found. This list of empirical facts can be useful from different points of view. First of all, it would be important to know which of them are common regularities across all transition economies (or at least the ones similar to Hungary), and which are Hungarian specialties. Second, we think that most of our results are interesting enough in their own to be subject of further investigations. Some of them are puzzling, some of them seem to be common sense. Some of them have been already studied, and some of them have not. We think that quite a few of them (along with the ones that our aggregate analysis could not reveal) should be on the agenda of the empirical research on transition economies. And fourth, the mechanisms behind these facts can be also very interesting. Corresponding to this point, theoretical models pursuing to capture the most important features of the transition from socialism to capitalism should take into account the common facts.

(1) The educational composition of the population improved during the whole period. Female college education attainment caught up with men's by 1989. There is a slowdown in the educational improvement trend, both for women and men, the latter reaching stagnation. This is not simply the result of the decreasing size of the entering cohorts, since a similar (of course, smaller) slowdown is true for a particular age group, too.

(2) Before the transition, labor force participation was rather constant for all demographic groups (increasing for lower educated women and slightly decreasing for men). The transition resulted in a dramatic decline in the lower educated groups, especially among women.

(3) Labor force participation rate of the pre-retirement cohorts followed the overall average in the 1970s and the 1980s, with somewhat larger increase in the female rates and larger decrease in the male rates. However,

during the transition, the fall was twice as large as the average, and men experienced an even larger decline.

(4) Unemployment increased from zero to 13 per cent during the transition. The resulting unemployment rates vary a lot between education groups, but not much between men and women. It seems that low-educated women leave the labor force rather than become unemployed, even more than men do.

(5) The share of non-wage employment increased throughout the whole period, indicating the increasing importance of entrepreneurship. This trend accelerated during the transition. The increase in self-employment was much higher for the more educated groups.

(6) Part-time work was not significant in the socialist economy, and it did not become too important during the transition, either.

(7) Real wages (net earnings) increased in the 1970s, stagnated during most of the 1980s, and declined sharply during the transition.

(8) Relative wages did not change much before 1989, with the exception of the decrease for highly educated men. The transition brought about increasing differences corresponding to schooling level. The advantage of college-educated employees increased a lot, and the trend seems to continue.

(9) The gender gap in earnings decreased slightly in the 1970s, and became even narrower during the transition. This trend, however, seems to have stopped in 1996. The gender gap did not decrease for the college educated.

(10) Overall earnings inequality decreased slightly in the 1970s, did not change much in the 1980s, and increased dramatically with the transition, especially in the latest years. The level and also the increase is higher among men and among the more educated.

(11) Only a small part of the increasing inequality is due to the factors analyzed above; the most important driving forces are not captured by simple demographic variables. Within-group variation accounted for more than three fourths of total variation by 1996.

(12) There is evidence for the positive and accelerating cohort-specific benefits for the new entrants, especially for those with higher education. This includes those that were already in their early 30s during the transition. The age-earnings profiles are therefore a lot steeper in a longitudinal setup than they seem in cross-section.

(13) Trends on the labor market were probably dominated by exogenous supply effects before the transition. These exogenous effects were partly due to demographic changes, but probably partly due to trends in schooling.

(14) The transition resulted in large shocks in labor demand, which affected all groups. These shocks were mostly structural (as opposed to cyclical), and highly correlated with vintage, gender and schooling. Unemployment is structural, and non-participation is closely related to it.

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DATA APPENDIX

Data Sources

The data we use come from four different kind of sources. Table A1 shows the contribution of each type of survey. The following section (with Tables A2 through A4) describes the surveys briefly.

Table A1: Years of the Cells in the Quasi-Panel Data set and the Sources of the Data (Q: labor market "quantity" variables: participation, employment, etc. W: wages)

	CSO IS	Census 1990	CSO LFS	NLC WS
1972	Q,W			
1977	Q,W			
1982	Q,W			
1986	Q[a]			W
1987	Q,W			
1989		Q		W
1992			Q	W
1993[b]			Q	
1994			Q	W
1995			Q	W
1996			Q	W

[a]The employment data in the 1986 cells are estimates, based on linear interpolation of the ratios from the 1982 and 1987 CSO IS.

[b] There are no earnings data in the 1993 cells.

The CSO IS Data

(In Hungarian: KSH Jövedelemfelvétel)

The CSO Income Surveys were large independent cross-sectional household surveys representing the non-institutional population conducted in every five years from 1963 on. The micro data from the 1960s are lost. These surveys are excellent sources of labor market data, since they match the demographic information provided by the households with the occupation and earnings information provided by the employer for each household-member holding a job. The following table shows the most important information about the samples, the estimated effect of the sample design on standard errors, and the number of the outliers in the earnings data.

Table A2: Sample Information about the 1973, 1978, 1983, and 1988 CSO Income Surveys

Year of Survey	1973	1978	1983	1988
Year of Employment Data	1972	1977	1982	1987
Year of Earnings Data	1972	1977	1982	1987
Sample Size: All Individuals	111,037 [a]	48,361	43,474	56,439
Sample Size: Wage Data	41,009[a]	17,700	15,947	19,873
Non-Response Rate	n.a.	n.a.	0.02	0.17
N. of Labor Force Participants[b]	54,374	22,738	19,827	25,773
N. of Full-Time Wage Employed	42,291	18,170[c]	13,591	18,222
deff on Employed/Pop.[d]				
Mean deff on Employed/Pop. per Cell[d]				
deff on Overall Mean Earnings[d]	1.028	1.070	1.072	1.001
Mean deff on Cell Mean Earnings[d]	1.039	1.051	1.032	1.029
N. of Outliers in Earnings	19	21	5	76

[a]In the 1973 sample, the original sample size is 27,048 households, and the records are doubled for Budapest, and Bács, Borsod, Hajdú and Pest counties.
[b]Because of full employment in the socialist economy, participation is identical to employment until 1989.
[c]There is no full-time status information available in the 1978 data set. The above number refers to total wage employment. In the quasi-panel data set, the ratio of full-time employment to total wage employment is estimated as the average interpolated ratio between 1972 and 1982.
[d]deff is Leslie Kish's measure of the sampling design effect. It gives the ratio of the true sampling variance of the statistic (here: overall mean and mean of the means in the cells) to the simple random sample-based estimation of the sampling variance. See Kish (1965). The "true" variance here is estimated by bootstrapping, therefore deff=($VAR_{BOOTSTRAP}$ /VAR_{SRS}).

The 1990 Census Data

(In Hungarian: Népszámlálás)

The CSO conducts a census in every 10 years. We used the information of the short questionnaire, which includes everything we needed (schooling, too), except for full-time employment. Our sample was a 2 percent simple random sample of the total population, total sample size was therefore above 200,000. Non-response was 0, since it is forbidden by law.

The CSO LFS Data

(In Hungarian: KSH ELAR Munkaerôfelvétel)

The LFS is a quarterly conducted rolling household panel survey of more than 20 thousand non-institutional households. Each time, one sixth of the sample is replaced, so one household stays for 6 quarters in the sample. One main objective of the survey is to provide accurate, ILO-standard measures of participation and unemployment. It also contains rather detailed information about the employment: sector, occupation, and hours worked is provided. It is a household survey, so all information come from the respondents.

Table A3. Sample Information about the 1992, 1993, 1994, 1995, and 1996 CSO Labor Force Surveys

Year of Survey	1992	1993	1994	1995	1996
Year of Employment Data	1992	1993	1994	1995	1996
Sample Size: Total N. of Individuals	54,284	61,345	60,791	68,927	64,609
Sample Size: N. of Active-Age Individuals	36,222	30,888	30,222	33,185	32,506

The NLC WS Data

(In Hungarian: OMK Bértarifa-felvétel)

The WS are very large regular (from 1995 on yearly) cross-sectional surveys of individual earnings and occupational data, also including gender, date of birth, and schooling information. The data are collected from the employers on an administrative basis. They represent the population of firms employing more than 20 persons (more than 10 from 1995). The sampling design of the firms (and other institutions) included stratification and no clustering. Within each firm, the individuals were to be selected by the administration of the firm, based on the day (not month and year) of birth. The individual sample is therefore not literally random, but the selection criterion is probably orthogonal to any interesting variable. This way, sample design included more levels, but this did not decrease the efficiency of the estimates (see the design effects below).

Table A4: Sample Information about the 1986, 1989, 1992, 1995, and 1996 NLC Wage Survey Data

Year of Survey	1986	1989	1992	1995	1996
Year of Earnings Data	1986	1989	1992	1995	1996
Sample Size: N. of Full-Time Wage Employed	145,419	145,460	131,677	153,229	160,481
deff on Overall Mean Earnings[a]	0.999	0.999	1.000	0.999	0.999
Mean deff on Cell Mean Earnings[a]	1.000	1.000	1.000	1.000	0.999

[a]See the notes in Table A2 for the definition of deff.

The Quasi-Panel Database

The quasi-panel is a collection of groups defined by their cohort (5-year interval of date of birth), gender, and schooling level (3 categories: 0-11 classes, secondary school, college or more). These groups are followed throughout the years 1972-1996. Each cohort consists of 6 gender×education cells, except for the oldest group (because women's general retirement age has been just 5 years less than the one of men), and the youngest one (because the youngest cohort covers the 15-19 years old). The cohort-boundaries are created in such a way that they exactly cover the working-age population (men: 15-59, women: 15-54 years old) in the early surveys (note that both the surveys and the cohorts follow each other in 5 year intervals). Of course, this pattern is not shared by the 1986, 1989, 1993-6 years, so they include "truncated" cells as well. We estimated the quantity and the earnings variables in for all truncated cells that contained enough observations in the original surveys. Table A5 shows how the cohorts are represented in each year. Table 6 contains the total number of observations in the original data sets that are represented by the quasi-panel, while Table 7 provides cells-based statistics about the size of the original samples.

Table A5: Number of Data cells Per Cohort in a Given Year

Year of Birth	1972	1977	1982	1986	1987	1989	1992	1993	1994	1995	1996
1913-17	3	0	0	0	0	0	0	0	0	0	0
1918-22	6	3	0	0	0	0	0	0	0	0	0
1923-27	6	6	3	3	0	0	0	0	0	0	0
1928-32	6	6	6	6	3	3	0	0	0	0	0
1933-37	6	6	6	6	6	6	3	3	3	3	3
1938-42	6	6	6	6	6	6	6	6	6	6	6
1943-47	6	6	6	6	6	6	6	6	6	6	6
1948-52	6	6	6	6	6	6	6	6	6	6	6
1953-57	4	6	6	6	6	6	6	6	6	6	6
1958-62	0	4	6	6	6	6	6	6	6	6	6
1963-67	0	0	4	4	6	6	6	6	6	6	6
1968-72	0	0	0	0	4	4	6	6	6	6	6
1973-77	0	0	0	0	0	0	4	4	4	4	4
1978-82	0	0	0	0	0	0	0	0	2	2	2
Total	49	49	49	49	49	49	49	49	51	51	51

Table A6: Total Number of Individual Observations in the Panel

	1972	1977	1982	1986	1987	1989
Employment Data	60,936	25,700	22,837	–	29,728	101,195

Earnings Data	40,853	17,704	15,385	141,175	17,532	142,357

	1992	1993	1994	1995	1996
Employment Data	36,222	30,888	30,222	33,185	32,506
Earnings Data	129,977	–	151,016	150,671	157,766

Table A7: Number of Individual Observations per Cells in the Panel (Median, 25th Percentile, 5th Percentile, Minimum)

	"Quantity" Data				Earnings Data			
	Median	25th P.	10th. P.	Min.	Median	25th P.	10th. P.	Min.
1972	451	264	107	78	421	246	94	66
1977	267	135	61	40	246	120	59	36
1982	267	149	62	33	235	113	61	31
1986	–	–	–	–	2,087	1,248	96	32
1987	364	212	95	43	281	163	75	24
1989	1,355	738	264	129	2,300	1,297	361	106
1992	573	291	88	41	2,135	1,526	271	43
1993	506	237	114	67	–	–	–	–
1994	447	220	85	58	2,383	1,599	402	19
1995	507	239	100	61	2,486	1,606	299	59
1996	436	253	55	31	2,491	1,716	247	139

Definition of The Variables

Population. We included all individuals in the age range of 15-59 (women: 15-54) who were not full-time students. This latter requirement naturally reduces the age-range of the college-educated groups: the lower bound is in general 24. (See also the section on truncated cells.) This definition of the population has two shortcoming with respect to the labor supply decision: for young generations it is (at least to some extent) an economic decision whether they are starting to work (or searching for a job), remain out of the labor force, or studying more. On the other hand, people over the compulsory retirement age still chose to work. In fact, it was common during the pre-transition period and became very rare after the transition. Therefore this definition of the population excluding two important aspects of the labor supply reactions to the transition shock.

Labor force participation, unemployment. Labor-market participation includes employment and unemployment, and it excludes those that are in the military service or on child-care leave. (Note that the CSO definition includes these two groups.) Unemployment is measured by the ILO-standard set of questions in the CSO LFS, (from the beginning in 1992).

There was no unemployment before 1989, but the year of 1989 is problematic. As mentioned about, we used the 1990 Census (theoretical date of survey: Jan 1. 1990) for measuring the 1989 quantities. The census asks only about registered unemployment, however in that early stages of the transition we can assume that registered and ILO unemployment figure were very similar.

Wage/salary employment, full-timers. Wage/salary employment excludes agricultural co-op employment, sole proprietorship, self-employment, and helping family members. Full-time employment is an explicit question in the CSO IS (1972,77,82,87) (however the data file did not contain this variable in 1977). About 10 percent of the variable was missing in the other surveys, those we took as full-timers. In the CSO LFS (1992,3,4,5,6) we defined full-timers those that worked at least 36 hours per week on average, or work less but their job description describes this amount of work as full-time employment, or they could not tell the average (or told it changes a lot), but they worked more than 36 hours last week.[10] The 1990 Census did not contain information about full-time employment, so the corresponding rates for 1989 are interpolated using the 1987 and the 1992 figures.

Earnings. The earnings data refer to the full-time wage/salary employed. Earnings is defined as the monthly average of wages and other, work-related payments. In the CSO IS (1972,77,82,87), the earnings data are based on information from the employer, and they consist of the yearly sum of baseline wages and other work-related payments, divided by the number of months worked. See KSH (1981, 1986 and 1990) for more details.[11] Note that the workplace was the distribution unit of some welfare transfers in the socialist system, such as child-care benefits, but the earnings data don't contain these components. For the years 1972, 1977 and 1982 we used the earnings variable created by the CSO, while for 1987, in the absence of the CSO variable, we created it (with the help of the CSO staff). The NLC WS earnings data are created by Gábor Kertesi and János Köllô and follow the same logic. See Kertesi-Köllô (1997b) for more details. Besides their (gross) earnings variable, we also used their estimates for net earnings from 1989 on. (Personal income tax exists in

[10] The 1992 LFS is different from the following ones in this set of questions. However, the information needed for this definition could be recovered.

[11] The included components were base salary/wage, bonus, royalty, wage supplement and other wage and salary components. (in Hungarian: törzsbér, prémium, jutalom, jutalék, bérpótlék (w/o családi pótlék), kiegészítô fizetés, egyéb bérek).

Hungary from 1988. In 1988, the earnings were "grossed up" so that the after-tax earnings remain the same.) The estimation is based on the yearly personal income tax rates. In the absence of other information, no correction could be made for tax-deductible items or the amount of other taxable incomes. The former components would decrease, the latter ones would increase the average tax (this second is because of the progressive nature of the scheme). We think that the first effect is larger, so the net earnings are probably underestimated from 1989 on.

APPENDIX: ESTIMATING THE MEASUREMENT ERROR BIAS IN A FIXED-EFFECT MODEL[12]

Consider the model we estimate in section 5:

$$\ln s_{cit} = \alpha_{ci} + \beta_i \ln w_{cit} + \varepsilon_{cit} ,$$

and the equations estimating its within (or fixed-effect) and between-models:

$$\ln s^D_{cit} = \beta_i \ln w^D_{cit} + \varepsilon^D_{cit} ,$$
$$\ln s^A_{cit} = \alpha_{ci} + \beta_i \ln w^A_{cit} + \varepsilon^A_{cit} ,$$

where $x^D_{cit} = x_{cit} - x^A_{ci}$, the A superscripts denoting within-group average ($x^A_{ci} = T^{-1}\sum_{t=1}^{T} x_{cit}$), and therefore the D superscripts the deviation from it.

Let b^W denote the within- and the b^B between-estimator for β (in what follows, we suppress the subscripts, for notational simplicity).[13] Also, let $\ln w_t$* and $\ln s_t$* denote the population value of net real earnings and our measure of labor supply at time t, respectively. Instead of $\ln w_t$* and $\ln s_t$*, we only have estimates for them, $\ln w_t$ and $\ln s_t$, respectively. Let $\omega_t = \ln w_t - \ln w_t^*$, and $\xi_t = \ln s_t - \ln s_t^*$, and $\delta_t^2 \equiv E[\omega_t^2]$, with an average $\delta^2 \equiv T^{-1}\sum_{t=1}^{T} \delta_t^2$. Then, the definition of the fixed-effect estimator is:

$$\begin{aligned} E[b^W] &= \mathrm{Cov}[(\ln w_t - \ln w^A), (\ln s_t - \ln s^A)] / \mathrm{Var}[\ln w_t - \ln w^A] \\ &= \mathrm{Cov}[\ln w^D_t, \ln s^D_t] / \mathrm{Var}[\ln w^D_t] \\ &= \mathrm{Cov}[\ln w^D, \ln s^D] / \mathrm{Var}[\ln w^D] \end{aligned}$$

$$E[b^B] = \mathrm{Cov}[\ln w^A, \ln s^A] / \mathrm{Var}[\ln w^A]$$

where the A superscript denotes averages, the second line for $E[b^W]$ defines the difference with the D-superscript notation, and the third line assumes that these differences are drawn from distributions with common first two moments. The expected value of b is the following:

$$E[b^W] = \mathrm{Cov}[(\ln w_t - \ln w^A), (\ln s_t - \ln s^A)] / \mathrm{Var}[\ln w_t - \ln w^A]$$

[12] The estimation of measurement error in panel context was addressed first by Griliches and Hausman (1981). Here we follow their argument.
[13] The derivation below focuses on the ln$w$ specification.

$$= \mathrm{E}[(\ln w^*_t - (\ln w^A)^* + \omega_t - \omega^A),(\ln s^*_t - (\ln s^A)^* + \xi_t - \xi^A)]$$
$$/ \mathrm{Var}[\ln w^*_t - (\ln w^A)^* + \omega_t - \omega^A]$$
$$= \mathrm{Cov}[(\ln w^D)^*,(\ln s^D)^*] / \{\mathrm{Var}[(\ln w^D)^*] + \delta_t^2 + (\delta^A)^2\}$$
$$= \mathrm{Cov}[(\ln w^D)^*,(\ln s^D)^*] / \{\mathrm{Var}[(\ln w^D)^*] + 2\delta^2 \}$$
$$= \beta \times \mathrm{Var}[(\ln w^D)^*] /\{\mathrm{Var}[(\ln w^D)^*] + 2\delta^2\}$$

$$\mathrm{E}[b^B] = \mathrm{Cov}[\ln w^A,\ln s^A] / \mathrm{Var}[\ln w^A]$$
$$= \mathrm{E}[((\ln w^A)^* + \omega^A)),((\ln s^A)^* + \xi^A)]$$
$$/ \mathrm{Var}[(\ln w^A)^* + \omega^A]$$
$$= \mathrm{Cov}[(\ln w^A)^*,(\ln s^A)^*] / \{\mathrm{Var}[(\ln w^A)^*] + (\delta^A)^2\}$$
$$= \mathrm{Cov}[(\ln w^A)^*,(\ln s^A)^*] / \{\mathrm{Var}[(\ln w^A)^*] + \delta^2 \}$$
$$= \beta \times \mathrm{Var}[(\ln w^A)^*] /\{\mathrm{Var}[(\ln w^A)^*] + \delta^2\}$$

The first line for $\mathrm{E}[b^W]$ follows from the definition of the OLS estimator, the second line is the expansion of the difference operator, and the third line uses the definition of ξ and ω. The fourth line uses the assumption that the errors are uncorrelated with the measured variables, the errors are also uncorrelated between two points of time (which is true if the consecutive cross-sections are all representative samples), and the errors in w and s are also uncorrelated (which is certainly true when they come from different surveys). These imply that $\mathrm{Var}[\ln w^D] = \mathrm{Var}[(\ln w^D)^*] + \delta_t^2 + (\delta^D)^2$. The fifth line uses the fact that $(\delta^D)^2=\delta_t^2$ if $\delta_t^2=\delta^2$, and the last line uses the consistency of $\mathrm{Cov}[(\ln w^D)^*,(\ln s^D)^*]/\mathrm{Var}[(\ln w^D)^*]$ for β. Very similar arguments lead to the formula for $\mathrm{E}[b^B]$.[14] From this result, we can see that the bias in $\mathrm{E}[b^W]$ can be significant even if the relative error $(\delta/\mathrm{E}[(\ln w^D)^*])$ is small, as in our case, if $\mathrm{Var}[(\ln w^D)^*]$ is small, but $\mathrm{E}[b^B]$ is not biased severely. From now on, we focus on the within-estimator.

[14] The most fragile assumption we make here is that the measurement error is uncorrelated with the measured variable. In what follows, we use the sampling variance for δ^2, therefore we assume that the only source of error is due to random sampling. If this is true, the zero-correlation condition automatically follows (so does independence). Since all of our earnings data come from the employer, the problem of correlated errors in self-reported wage data (see e.g. Kézdi, 1998 and Kézdi, forthcoming) is not relevant. On the other hand, we use estimates for the net earnings since the data we have after 1988 (from the start of the income tax system) are before-tax figures. We have no information about the sign and size of the correlation of "true" earnings and this error component.

If we assume that the source of error is the fact that the variables we use are estimates from representative samples, then δ^2 is sampling variance of ln$w$. We have relatively good estimates for the sampling variance of w (see the first section), which we can use for the correction. Let $d = w - w^*$, so we have estimates for $E[d^2]$. Then, $\delta^2 = E[\omega^2] = E[(\ln w - \ln w^*)^2] \approx E[(w-w^*)^2/(w^*)^2] = E[(w-w^*)^2]/E[(w^*)^2] = E[d^2] / E[(w^*)^2]$, where the third equality follows from the independence of w from the measurement error. Since $E[(w^*)^2]= E[w^2] + E[d^2]$, $\delta^2 = E[d^2] / \{E[w^2] + E[d^2]\}$. Therefore, we can consistently estimate the (limiting) bias of b^W and correct our point estimates to get consistent estimates for β:

$$\begin{aligned}\beta &= E[b^W] \times \{\mathrm{Var}[(\ln w^D)^*] + 2\delta^2\} / \mathrm{Var}[(\ln w^D)^*] \\ &= E[b^W] \times \mathrm{Var}[\ln w^D] / \{\mathrm{Var}[\ln w^D] - 2\delta^2\} \\ &= E[b^W] \times \mathrm{Var}[\ln w^D] / \{\mathrm{Var}[\ln w^D] - 2E[d^2]/\{E[w^2] + E[d^2]\},\end{aligned}$$

so

$$b^W \times \mathrm{SampleVar}[\ln w^D]/\{\mathrm{SampleVar}[\ln w^D] - 2E[d^2]/\{E[w^2]+E[d^2]\}\to \beta$$

in probability.

That is, the correction factor for $E[b^W]$ is

$$\mathrm{SampleVar}[\ln w^D]/\{\mathrm{SampleVar}[\ln w^D] - 2E[d^2]/\{E[w^2]+E[d^2]\}.$$

FIGURES

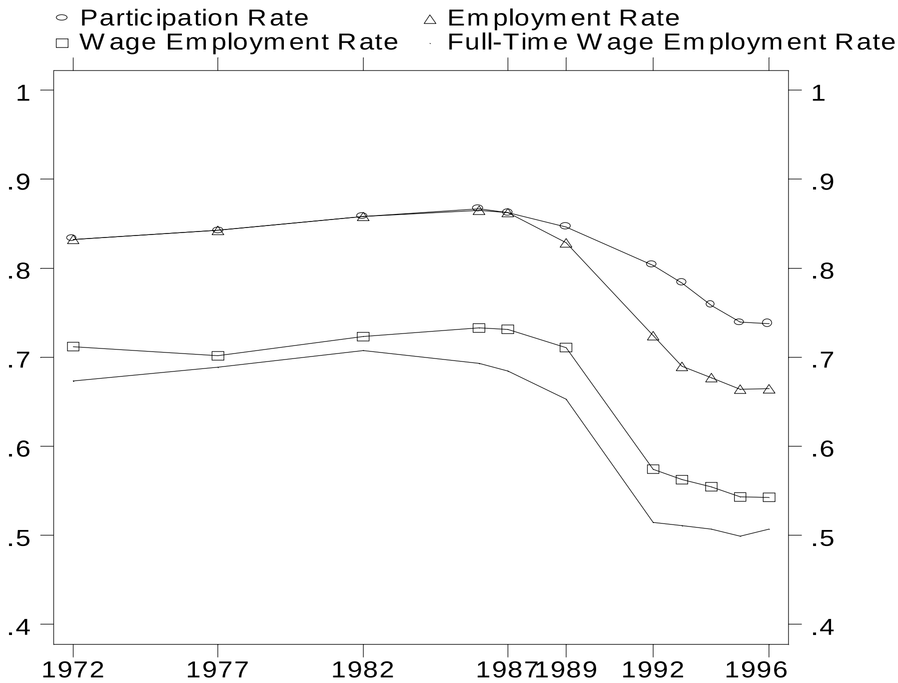

Figure 1
Labor Force Participation and Employment

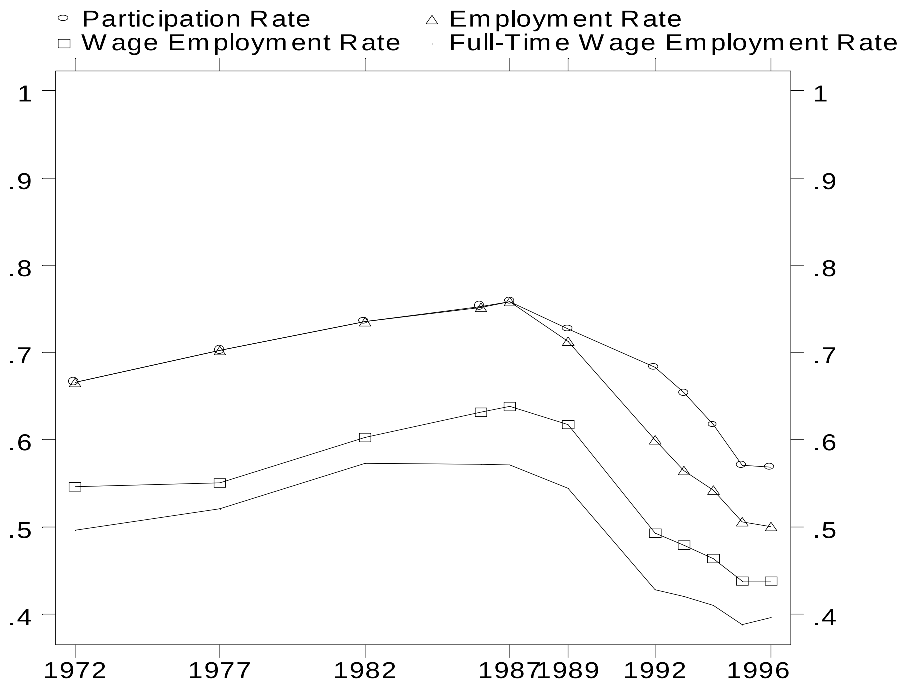

Figure 2
Labor Force Participation and Employment
Women, 0-11 Classes

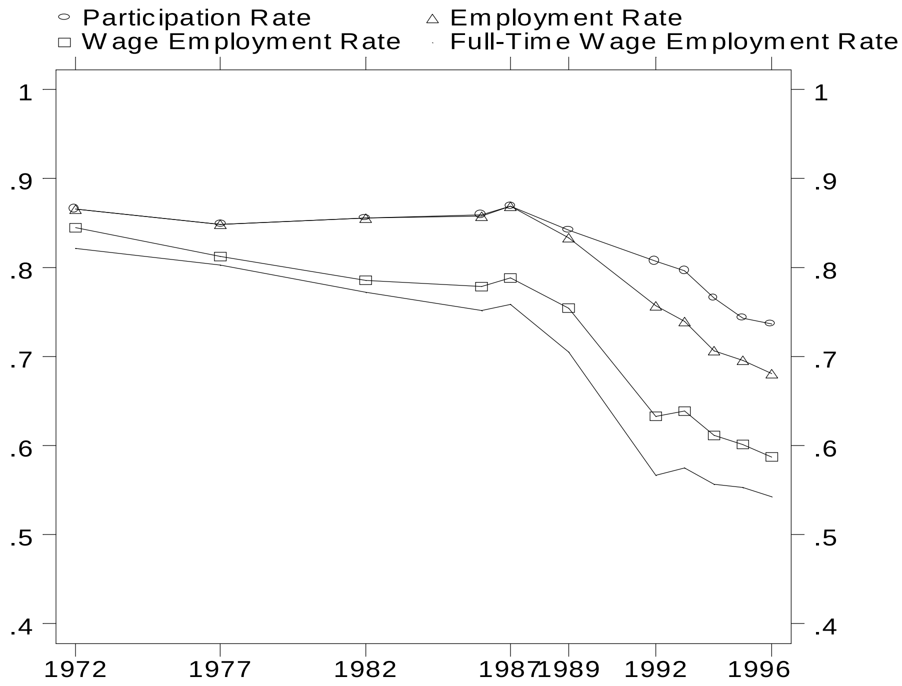

Figure 3
Labor Force Participation and Employment
Women, Secondary School

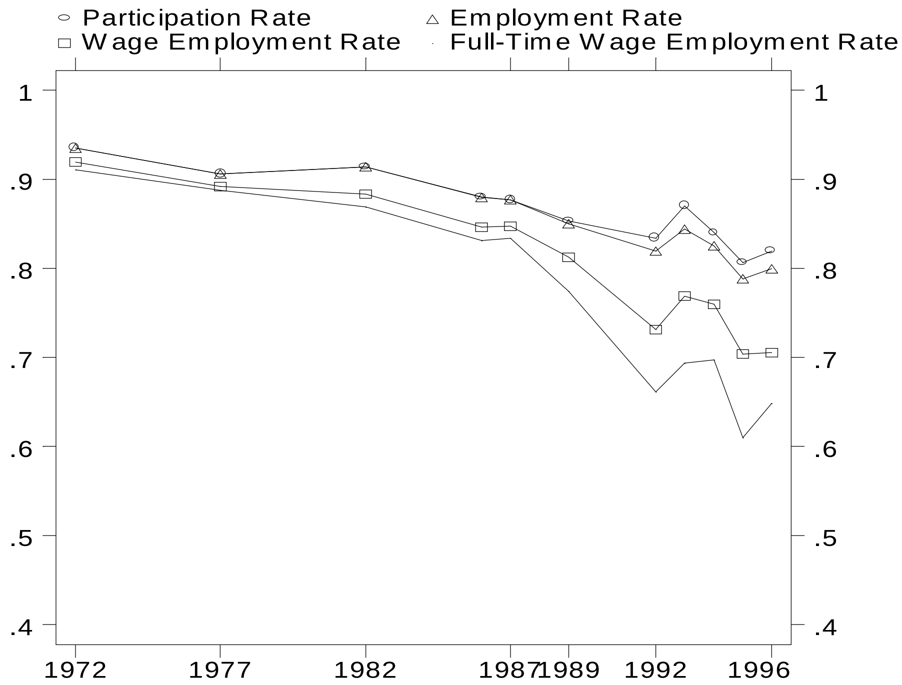

Figure 4
Labor Force Participation and Employment
Women, College

Figure 5
Labor Force Participation and Employment
Men, 0-11 Classes

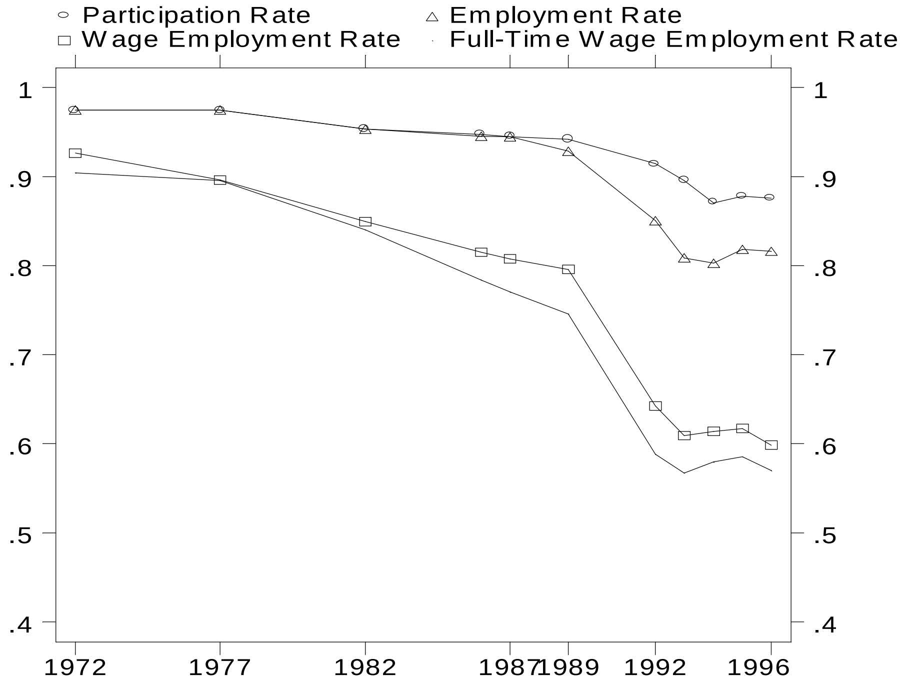

Figure 6
Labor Force Participation and Employment
Men, Secondary School

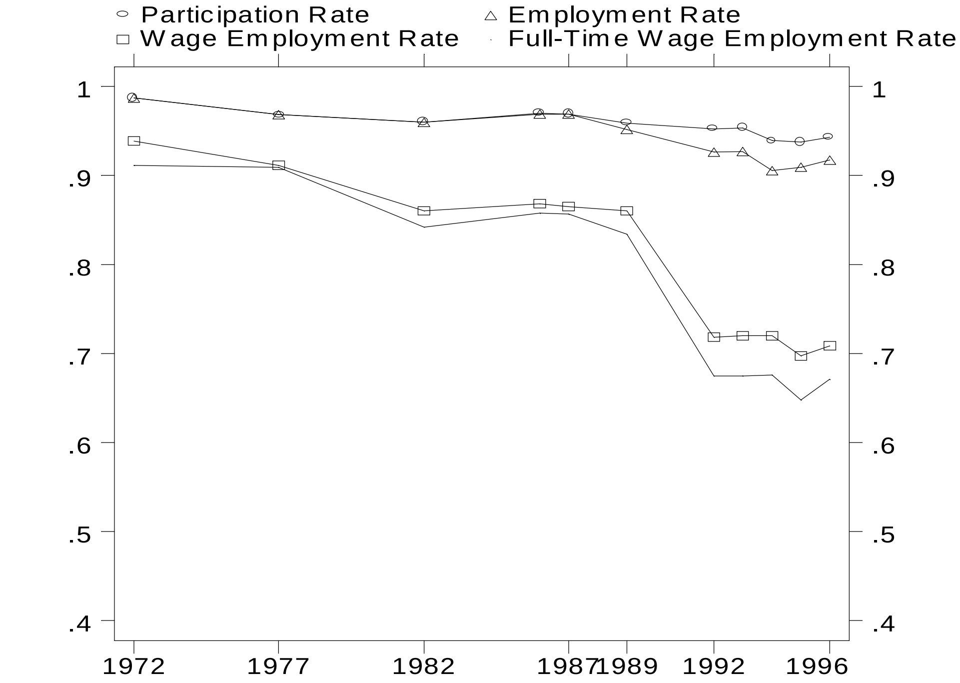

Figure 7
Labor Force Participation and Employment
Men, College

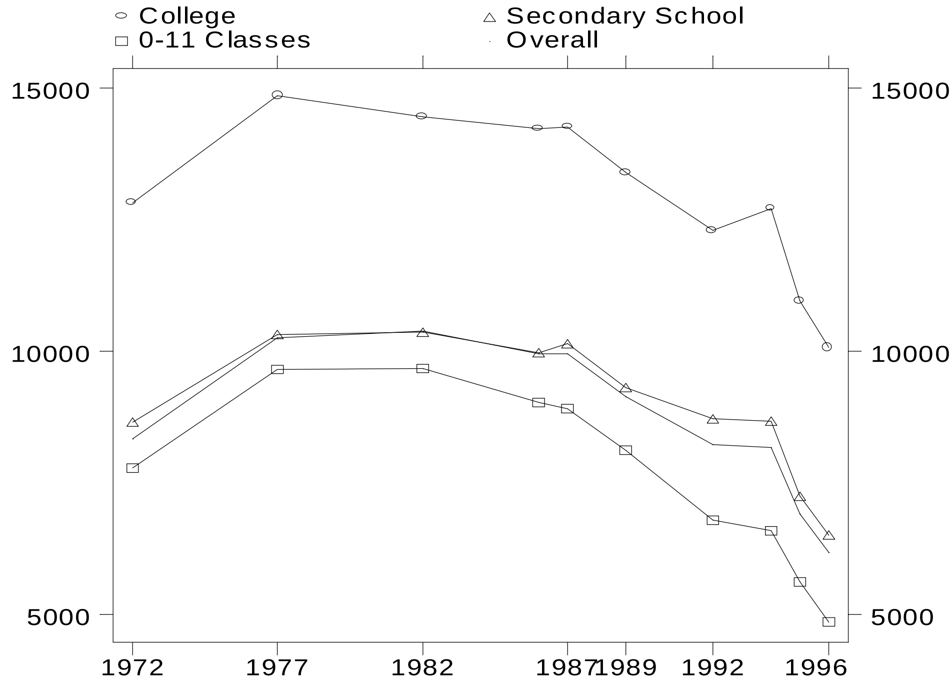

Figure 8
Real Net Earnings, Also By Education (in 1989 Hungarian Forints)

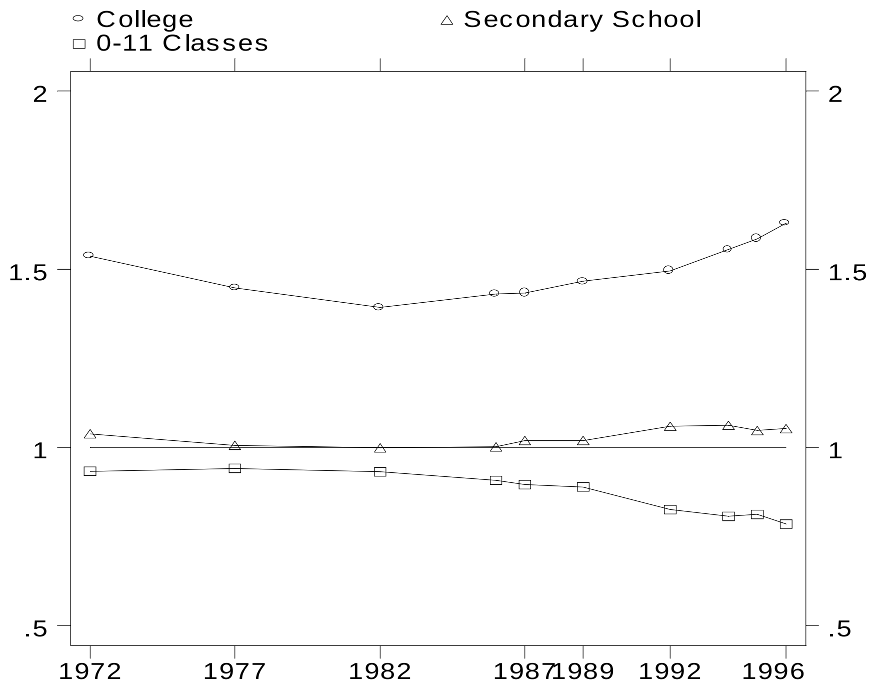

Figure 9
Relative Net Earnings, By Education

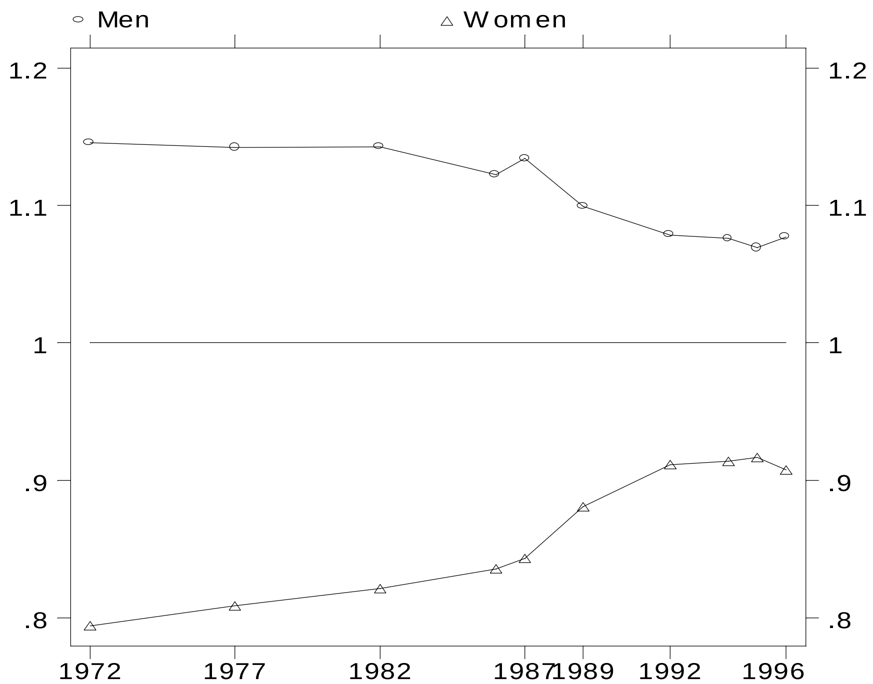

Figure 10
The "Raw" Gender Gap: Relative Net Earnings By Gender

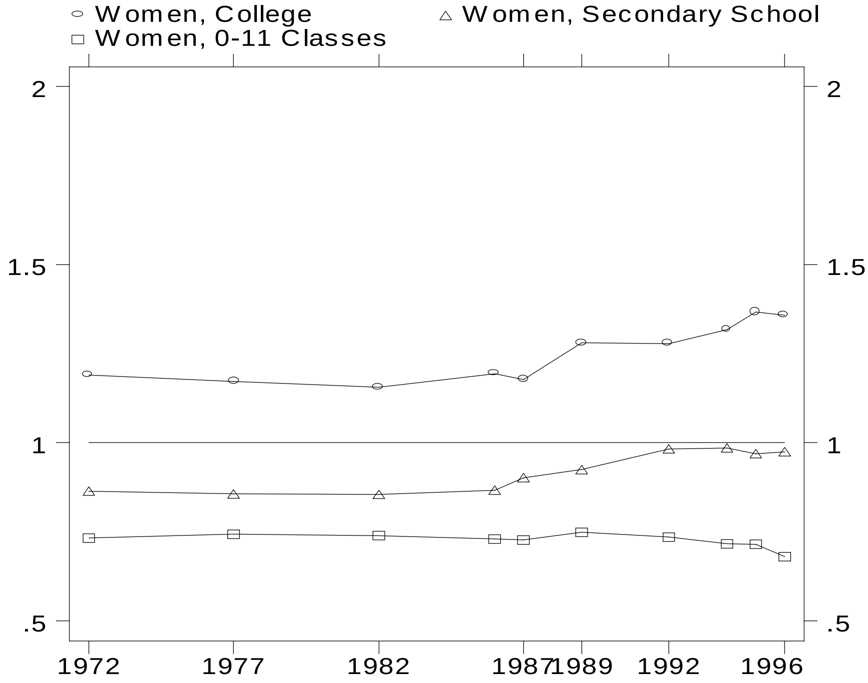

Figure 11
Relative Net Earnings of Women, By Education

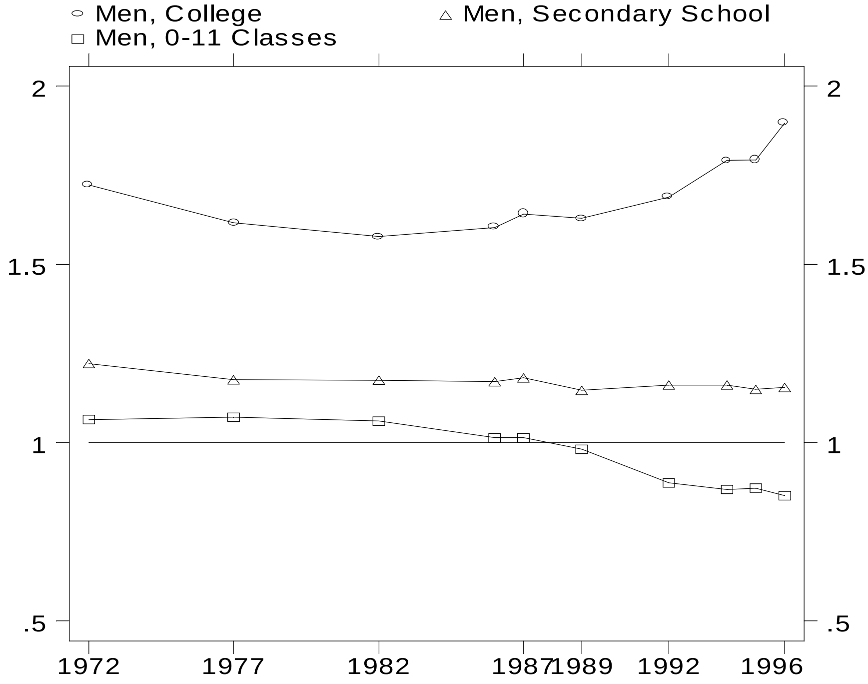

Figure 12
Relative Net Earnings of Men, By Education

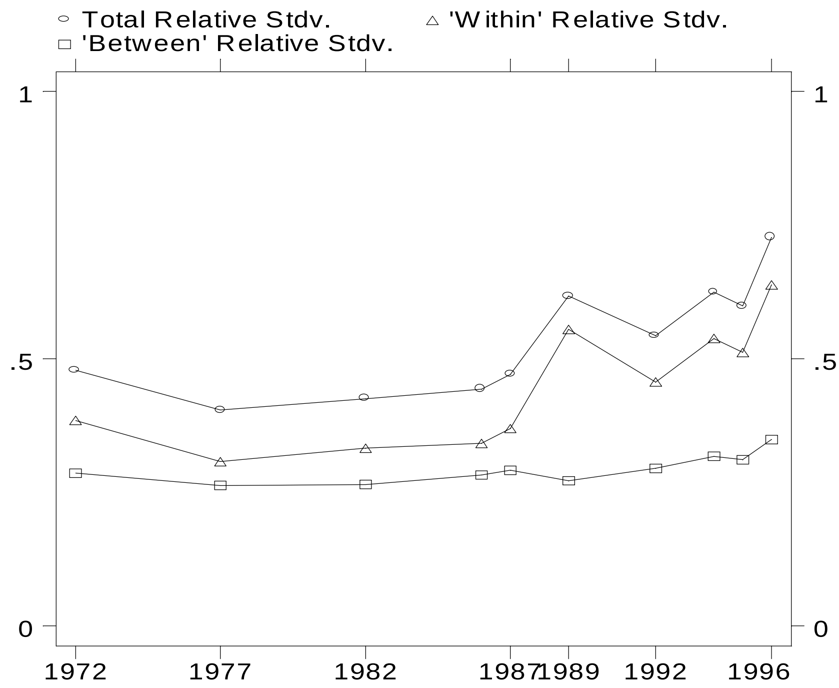

Figure 13

Relative Standard Deviation of Net Earnings, Within-Group and Between-Group Componetns

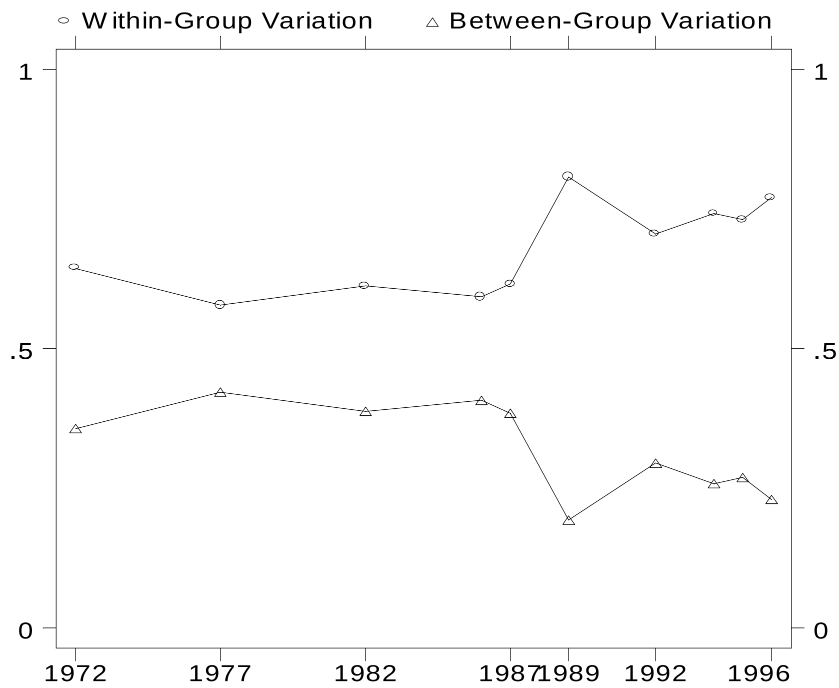

Figure 14

Proportion of Between-Group and Within-Group Variation in Net Earnings

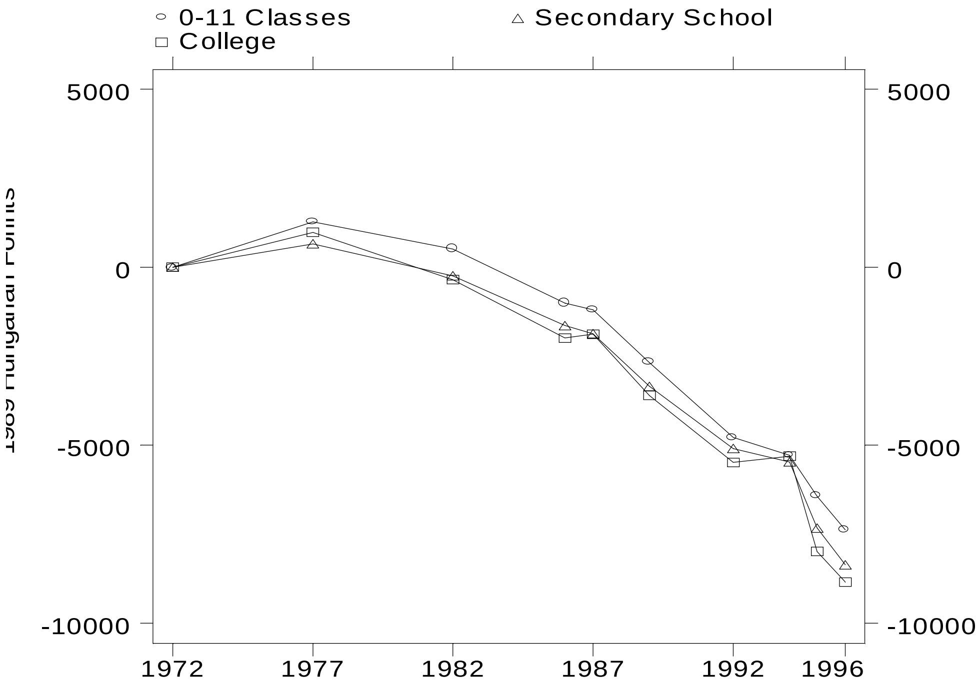

Figure 15

Year Fixed Effects on Real Net Earnings

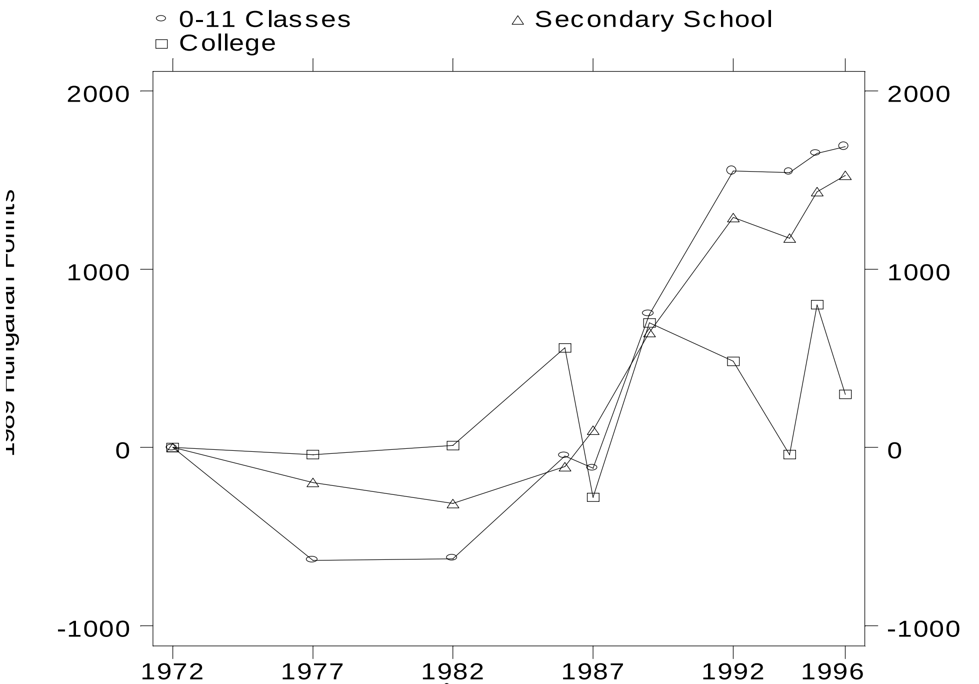

Figure 16

The "Cleaned" Gender Gap: Change in the Difference of Male and Female Salaries, 1972=0 (Year-Gender Interacted Fixed Effects on Real Net Earnings)

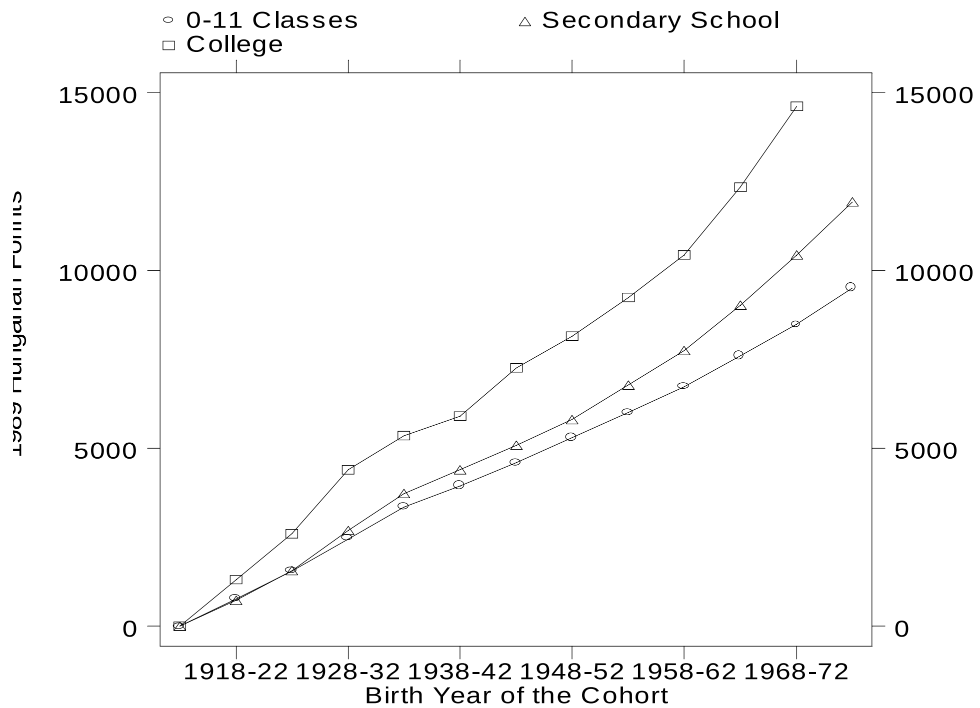

Figure 17

Cohort Fixed Effects on Real Net Earnings

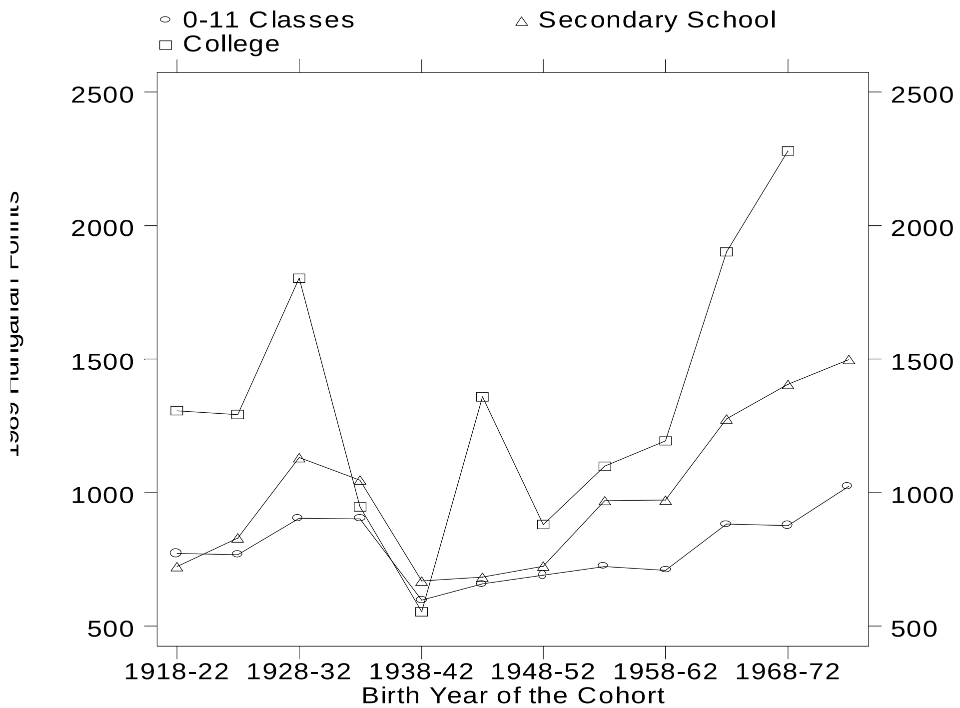

Figure 18

Differences in the Cohort Fixed Effects

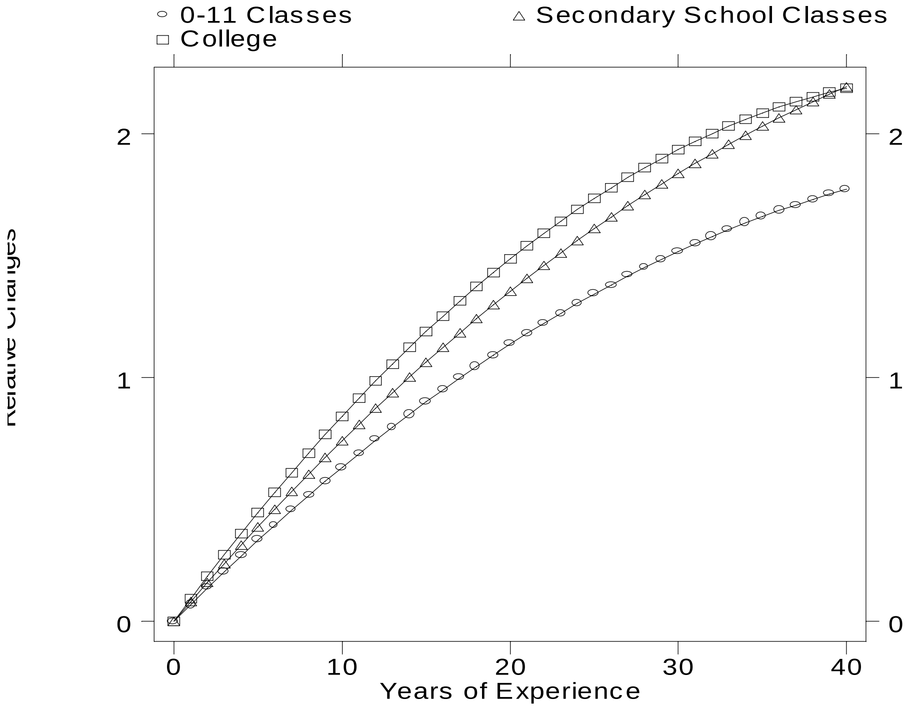

Figure 19

Age-Earnings Profiles, Relative Earnings

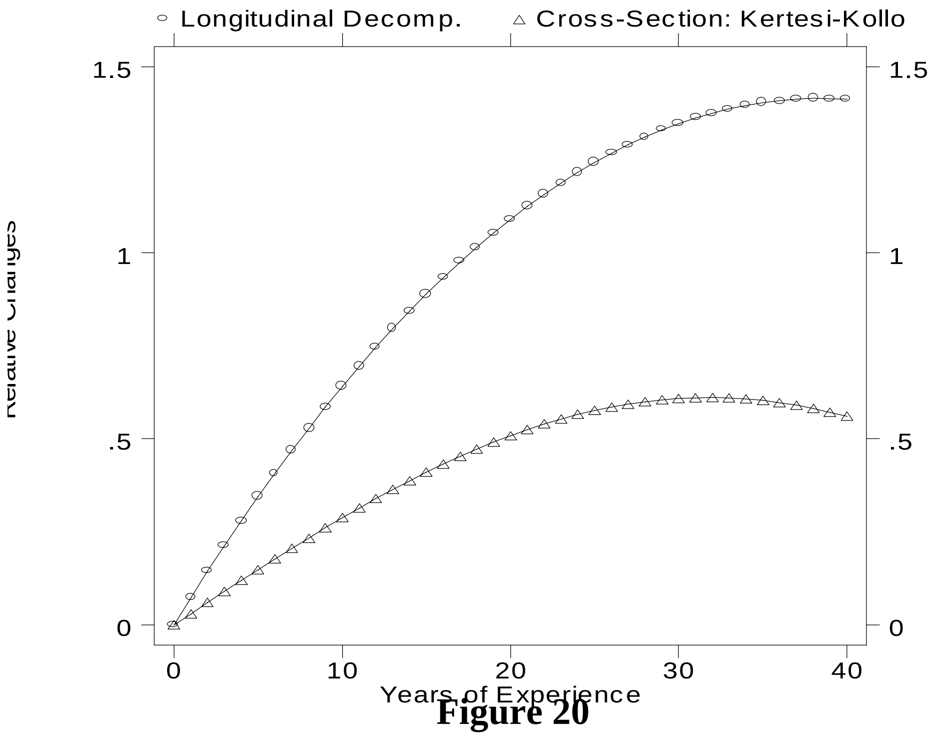

Figure 20

Age-Earnings Profiles, Relative Earnings.
Longitudinal Vs. Cross-Section Estimates (Ours vs. Kertesi & Kollo, 1999b, resp.)