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Cohort Size Effects in the Hungarian Labor Market

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Abstract

My thesis investigates cohort size effects in the Hungarian labor market. Particularly, it estimates the selection model on wages of Jeon and Berger (1996). I find no evidence of individuals adjusting their schooling choices to demographic conditions, but extremely strong effect of family background such as parents' education. Some vague evidence is also obtained that those, who are more likely to choose lower education, have by origin lower earnings potentials. All these points to the direction that relative labor market success of the large cohorts of Ratkó kids and grandkids in Hungary are due to composition effects.

I would like to thank to my supervisor, Prof. John Earle for deepening my knowledge and interest in labor economics, TÁRKI Social Research Institute for making the dataset available for me, to Péter Hudomiet and Gábor Kátay for their invaluable comments and pieces of advice, to Attila Lindner for helping me through tedious derivations and to my parents for supporting me as much during thesis writing as ever before.

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Chapter 1

Introduction

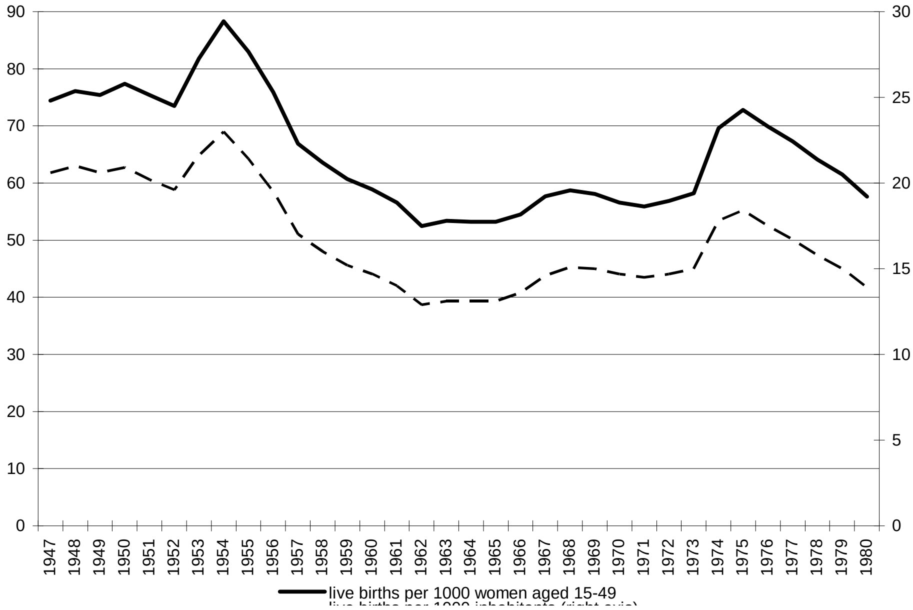
Research Question and Motivation

According to basic economic theory, besides stable demand, an increase in the supply results in lower prices and larger equilibrium quantites. In line with this, Kovács (2006) suggests, based on evidences from simple labor flow data of the Hungarian Labor Force Survey, that large cohorts in Hungary seem to have experienced better employment (and activity) opportunities since the transition. However, that database does not tell anything about their wages. One of these large cohorts is that of the so called Ratkó kids, who were born between 1953 and 1956, in the era of abortion restrictions. The other large cohort is composed of the children of the Ratkó kids (Ratkó grandkids), who were not only born to the members of a large cohort but also under some aggravation of abortion legislation (though this was not at all a total ban). These two "peaks" in Figure 1 produced almost the whole part of the increase in aggregate employment between 1995 and 2006, no matter how business or fiscal cycles moved. (Kovács 2006) May they carry their labor market success with them along the remainings of their life cycle?

My thesis focuses on a specific labor supply shock. It aims to explore labor market cohort size effects in Hungary on individual-level cross-sectional data. I will basically concentrate on wages as outcome variable, since in Central-Eastern Europe there might not have been such an analysis - presumably due to data availability problems. In the descriptive statistics part, I will particularly focus on the previously mentioned two unusually large cohorts, in case of the older ones I will explore whether the size of their birth cohort has some long run effects that can still be captured in 2005.

My model, which will be run for all cohorts and is basically the replication of Jeon's and Berger's 1996 paper on Hungarian data, accounts for, on the one hand, that larger cohorts are due to some *composition effects* in the family background. For

Figure 1.1: Live Births in Hungary, 1947-1980 *(Source: HCSO, 1992)*



instance, there is some evidence that Ratkó's abortion restrictions (increased number of live births) affected white-collar workers (see my term paper) the most and also that Romanian abortion ban affected highly educated, urban women the most (Pop-Elèches 2006). On the other hand, an adjustment problem also has to be tackled: when individuals realize that they were born into a cohort of a specific size, they individually adapt to this by choosing their schooling (*adaption effect*). Some of them may end up with less education because of investing less due to expectedly depressed wages or, being crowded out from higher education. Others may engage in more education in order to keep up with the more intense competition.

This analysis has two particular relevances. One is that the Ratkó kids, the large size group of old workers, are reaching the retirement age in two or three years. As most of them are still in the pay-as-you-go system, it would be important to know the tenure and the average wages of such a large cohort to predict fiscal effects of their retirement and their expected living conditions as pensioners. The other relevance is related to the Ratkó grandkids, the group of nearly prime-age workers again large in size. If they turn out to be a large winner cohort after the transition, they might affect subsequent (smaller and decreasing) cohorts. Are there any mechanisms present in Hungary through which we can adapt demographical changes, let them be exogenous or endogenous, such as the problem of the aging society? Do demographic cyles influence labor market variables? I will be investigating such questions.

Chapter 2

Brief Literature Review

Taking the whole labor force as if it were homogenous, even one unusual group (in size or quality) could cause a change in the only equilibrium wage and employment. However, different groups may be imperfect substitutes to each other, in which case the appearance of a group may just alter the wages of those in close substitution with them. The less a group is a substitute for the unusual cohort, the less its wage is affected. For example, older and younger workers may be imperfect substitutes if in an industry experience is of crucial role, or education may also distinguish between workers if due to a technological change, more modern (or simply higher) human capital is also needed. In most economies both have relevance, that is why typical theories partition total labor force by two dimensions, experience and education. (e.g. Berger 1983, Connelly 1986) This way four groups arise, young-unskilled, young-skilled, old-unskilled and old-skilled, which are imperfect substitutes of each other. In addition, some also claim (e.g. Stapleton-Young 1988) that these groups are not only imperfect substituites but substitutability between the old and the young falls with education.[1] In addition, another interesting aspect is that the amount of old or young workers does not only influence the age dimension of the labor force. The entry of an unusually large cohort to the labor market may be due to some exogenous distributional shift in fertility. Just as was Pop-Elèches (2006) reports for Romania, or is suspected to have happened in Hungary, that a fertility change among the highly educated resulted not just in the increase of young labor force, but also that of the highly skilled labor force.

The literature of labor market effects of cohort size was developed in the '80s and first half of the '90s, after both the baby boom and baby bust cohorts in the U.S. had entered the labor market. A strange phenomenon arose to be explained:

[1] One may argue that man and women is a third dimension of differentiating labor force. However, cohort size literature most often examines only males, exactly to be able to separate the analyzed impacts from other (e.g. discrimination or changing gender role) issues.

during the '70s both the college attainment rates and the returns to college were declining, while the situation was different in both preceding and subsequent cohorts. Card and Lemieux (2000) also claims that the depressed returns of the baby boomers have persisted eversince, while it improved for others. According to Stapleton and Young (1988) diminishing substitutability between the young and old with educations accounts for this. The authors argue that because of their lower substitutability, the higher educated are more affected by their cohort size. In larger cohorts there is larger competition, so wages may fall. Therefore, at the end of the day, people in a larger cohort may better off in case of investing less into schooling and choosing a less demanding career. The hypothesis was tested even empirically by Berger (1989). In fact, he found that large cohorts experience flatter profiles throughout their life-time than surrounding cohorts, who face lower starting wages but steeper wage path.[2]

As the indirect effect of individual adjustment by school choice is seen by certain economist a crucial labor market impact of cohort size, exploring the direct schooling effect, or human capital investment effect of cohort size became a self-sufficing research direction. Middendorf (2007) finds on European panel data that neither local unemployment rates, nor birth cohort size have an effect on schooling decisions. On the one hand, he attributes his first results to more effective capital market constraints to invest in further education in higher unemployment regions, but on the other hand, he leaves his somewhat opposing findings with U.S. results unexplained.[3] Nevertheless, he concludes that declining birth rates were not offset by the increase of education.

Connelly and Gottschalk (1995) put the same quality-quantity problem in an intergenerational framework. They build a model of two generations, where the emphasis is not just on adjustment by the schooling decision. Instead of cohort *size*, they rather focus on cohort *composition*, which allows them to take into account the plausible fact that cohort size changes may be due to the fertility change of a particular group of society, e.g. urban and educated women in Romania. Thus, composition effects may very well be substantial, as *ceteris paribus* children of more educated people tend to be more likely to acquire more schooling. In their conlusion they highlight the similar policy-oriented issue as Middendorf (2007): Just as a larger number of workers (due to a large cohort size) can be "offset" in the labor market by lower investment in human capital, therefore a smaller proportion of high educated workers, smaller cohorts can be offset by more people acquiring higher education.

[2] The same is found by Murphy et al. (1983), however, they only carried out their analysis within educational groups by differentiating labor force only by age.

[3] Some could say that in Europe people are less likely to react to expected wages, and therefore to returns to education. But the question remains: why is that? More compressed wages, therefore smaller returns to education, smaller cost of education may be a reason, or even we might think about education as more of a consumption good than an investment good.

At the beginning of the '90s, however, there arouse some voices that individual adjustment to cohort size is neglectable. Flinn (1993) builds a perfect foresight OLG model and disputes Stapleton's and Young's findings and claim that optimal adjustment in the investment in education causes minor improvement in wealth prospects. Closely related to this Klevmarken (1993) calls the attention to secon order adjustments in further research.

Bound-Turner (2007) brings a new aspect into the literature and claim that smaller investment in schooling in larger cohort is not due to individual response in the sense it was understood 10-15 years ago. They argue that in larger cohorts there is larger competition for almost the same amount of public and non-profit resources given to higher education, so the phenomenon of crowding arises. That is in their point of view, the reason for decreased college attainment rates is not due to reduced demand from the part of potential students who clearly see their lower future earnings but to the inelasticities of supply of resources.

Dahlberg and Nahum (2003) propose yet another alternative "adjustment" hypothesis and find evidence for it in Sweden. According to them, in larger cohorts, due to increased competition among the cohort members "makes people try harder and invest more in their human capital in order to maintain their relative positions" in the labor market. (Dahlberg-Nahum 2003, p. 6.) However, this may also be consistent with the arguments of Falaris and Peters (1992), who reports evidence for people in the upswing of a demographic cycle are likely to acquire more schooling and therefore stay at school for longer. On the other hand, one in the downswing of a cycle is has smaller incentives to study more. This is in contrast with the timing theory of Wachter and Wascher (cited by Falaris-Peters 1992 and Jeon-Berger 1996). According to this, different incentives are at work: in the upswing of a demographic cycle one wants to hurry with finishing schooling to enter the labor market as long before the peak cohort as possible. While in the downswing, it is more worth finishing school later and enter the labor market with as small a cohort as possible. Thus, the position in the demographic cycle entered the literature besides own cohort size because tight interactions of labor markets for neighboring cohorts. The article I will work with on Hungarian data is of the same family of models.

Chapter 3

Empirical Evidence from Hungary

3.1 Data and Method

3.1.1 Data

For the empirical part I would use the Hungarian data from the 2005 Community Statistics on Income and Living Conditions (EU-SILC 2005). This is an annual household survey managed by Eurostat, in Hungary the 2005 wave was the first time it was carried out by the Hungarian Central Statistics Office. The database was made available for me by TÁRKI Social Research Institute.

The data of this survey is comparable across countries included and numerous individual-level variables are available in a longitudinal perspective. The reference population for the survey is all private households and all individuals above 16 living in them. Sample in each country is more than 2% of the total population. (EU-SILC 2005a) The Hungarian sample cosists of 17,969 observations, out of which 9,906 are aged between 25 and 65 (4,715 men and 5191 women).

Besides demographic variables, all the waves contain variables on educational attainment (from 0 to 5 ISCED-levels), labor market status (concerning employment, unemployment, inactivity, reasn for inactivity), current or last occupation (2-digit ISCO-88 codes), current or last industry (NACE Rev1.1 codes), yearly employee and transfer incomes and average working hours a week.The definitions of labor market status, that is, being employed, unemployed or inactive are not the usual ones used by the International Labour Organization. The ILO-defined variables are only available for the exact time of the survey. For the income reference period,[1] which is the year

[1] Income reference period is the time period for which all income data, employee or transfer, is

2004 in case of Hungary, only a self-reported labor market status variable is available. That is, people reported how many months they spent in full- or part-time employment. (They also reported how many months they spent in unemployment, studyin, retired or other inactivity.) I generated the employed dummy - in lack of something better - by assigning to it 1 if the individual reported *some*, no matter how small number of months that he/she spent in either full- or part-time employment. For in this case he/she is fine to report *some* positive amount of income.

As for wages, I add up gross yearly (2004) income from wage- and self-employment activities. If current usual working hours per week was available, I assumed that these are valid for 2004 as well and divided yearly income by the months worked throughout 2004 times 4.33 times the usual weekly hours to get average hourly wage. If current usual working hours were not available, I imputed them with the months worked times 174/52, as in Hungary average full-time monthly working hours is 174 hours. Wages is 0 for those, who did not spent *any* full- or part-time months in wage- or self-employment in 2004.

However the quality of the income variable available in EU-SILC, there is a great advantage fo it: In addition to the more standard variables, each wave contains a varying module, which in 2005 aimed to explore intergenerational transmission of poverty. (EU-SILC 2005b). This module releases information on parents' age, education, occupation and activity status when individual was a teenager, number of siblings, family composition and financial problem in teenagerhood[2] of *all* individiduals aged between 25 and 65.[3]

Descriptive statistics of the variables used in this analysis are presentes in Table 1:

Cohort variables will be described in a detailed way later. Experience was generated from graduation year at the highest education level, so it is not neccesarily labor market experience, but this was the proxy which had the fewest and significantly smaller number of missing values. By virtue of this, at the same time, we can be sure that experience has been acquired with the very same education level whose choice I will model. Good health and bad health variables, and also financial difficulties and no financial difficulties are pairs of dummies, which signal positive or negative deviation from "fair" or occasionally. Child in household is a dummy taking the value 1 if there is at least one dependent child in the household of the individual. (It is not neccessarily his/her own child or children.)

available.

[2] At the age of 14, or between 12 and 16. (EU-SILC, 2005)

[3] The peculiarity of this database is that it contains these family background variables for *all* individuals and not only for those who still live in the same household with their parents. This latter is usual in census data, in which case data analysis can lead to serious selection bias (as e.g. in Pop-Elèches, 2006).

Table 1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
ln(own cohort)	9906	13.444	0.149	13.132	13.640
ln(future cohort)	9906	0.015	0.115	-0.240	0.156
ln(past cohort)	9906	-0.042	0.103	-0.156	0.240
year of birth	9906	1959.348	11.752	1939	1979
sex	9906	0.524	0.499	0	1
college	9901	0.148	0.355	0	1
high school	9901	0.569	0.495	0	1
experience	9835	25.498	12.896	0	61
intermediate skill job (ISCO-88)	9313	0.474	0.499	0	1
high skill job (ISCO-88)	9313	0.291	0.454	0	1
months spent in employment	9906	8.031	5.384	0	12
employed	9906	0.714	0.452	0	1
weekly working hours	9906	29.174	20.080	0	120
ln(hourly wage)	7070	1.111	0.565	0	5.413
western region	9906	0.317	0.465	0	1
central region	9906	0.261	0.439	0	1
good health	9822	0.454	0.498	0	1
bad health	9822	0.190	0.392	0	1
married	9906	0.722	0.448	0	1
child in household	9906	0.489	0.500	0	1
dad high school	8551	0.410	0.492	0	1
dad college	8551	0.080	0.272	0	1
mum high school	9469	0.295	0.456	0	1
mum college	9469	0.042	0.200	0	1
dad employed	8550	0.960	0.196	0	1
mum employed	9477	0.655	0.475	0	1
siblings	9606	1.912	1.884	0	20
financial difficulties	9712	0.384	0.486	0	1
no financial difficulties	9712	0.461	0.498	0	1

Of course, the dataset contains also household level variables (such as total household disposable income from social transfers), which can be linked to individual data. Although the richness of variables and the relative novelty are invaluable advantages of the dataset, the fact that it is cross-sectional and not a panel is somewhat a deficiency. This implies that I am not able to distinguish between time and age effects. And also that data is from 2004-2005, for some cohorts this analysis will reflect really long-term effects, af any.

3.1.2 Stylized Facts, Correlations

The two outlier fertility shocks are still evident from the data. In Figure 2, relative cohort size, that is, the number of persons in a 5-year cohort is plotted against the proportion of at least college graduates in 2005. The figure is interesting as for the other two education levels (high school graduates and lower educated people) there is no such fluctuation in the cohort based cross section. Although the end of the graphs are a bit misleading: the proportion of college graduates may not be decreasing, just those cohorts had not finished their studies by the time of the survey. However, these cohorts are definitely in the downswing of the boom of Ratkó grandkids.

The figure suggests that from post-world war cohorts to the approximate appearance of the Ratkó grandkids, college attendance moved opposite with cohort size. That is from larger cohorts relatively smaller proportion of people enjoyed the privilidge of receiving a college degree. Only a short intermezzo occured, among the cohortmembers of the Ratkó kids, who on the peak of artificial demographic cycle could improve their average education level.

This counter-movement was turned back for the cohorts born after the early seventies. These people, the Ratkó grandkids decided about their school choice around the transition and almost immediately faced the educational expansion and the explosive number of university places. (See the figure Appendix.)

Figure 3.1: Relationship between relative Cohort Size and Proportion of College Graduates, 1937-1981 *(Source: own calculation from EU-SILC 2005)*

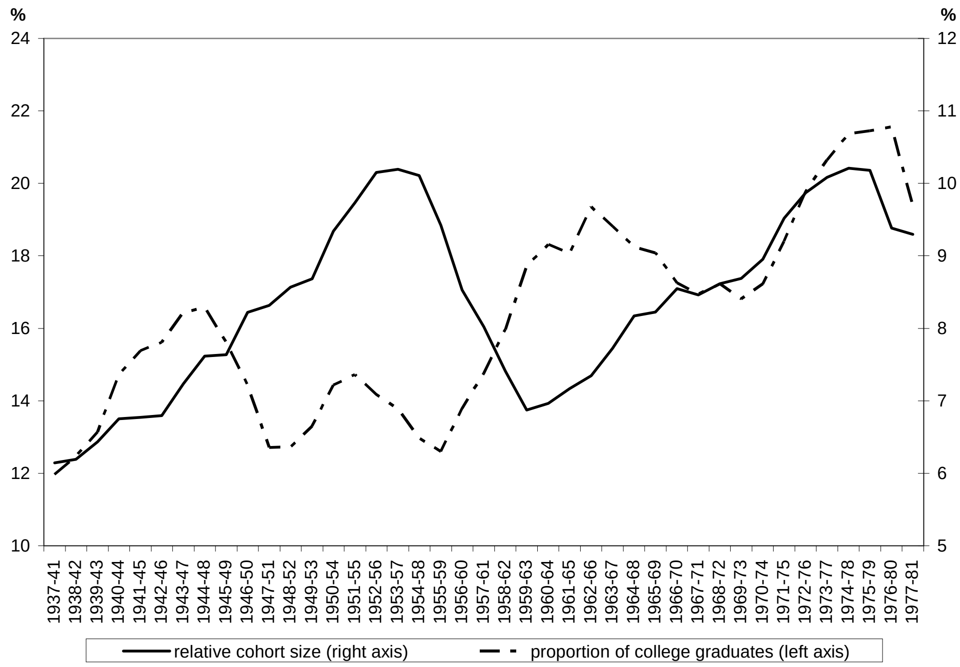


Table 2 gives us the first hint about disentangling the sources of these cohort differences. Ratkó kids are defined to have been born between 1953 and 1956, the previous cohort between 1949 and 1952, while the subsequent between 1957 and 1960. In case of the ratkó grandkids, the birth interval is more uncertain, as they are defined to be born to a Ratkó kid mother, from 1973, when abortion legislation was again aggravated for some time. So I took everybody born after 1972 as a Ratkó grantkid in the sample. (The youngest member in the sample was born in 1979.) Those born between 1966 and 1972 are considered the previos cohort.

Table 2: Mean Comparisons

	Pre-Rgk	Ratkó-gk	Pre-Rgk - Rgk diff.		
high school	0.626	0.644			
college	0.174	0.205	**		
graduation age	19.550	19.399			
employed	0.917	0.928			
intermediate skill jobb	0.488	0.497			
high skill job	0.332	0.349	*		
lnhwage	1.058	0.997	***		
mum high school	0.447	0.579	***		
mum college	0.063	0.100	***		
dad high school	0.535	0.647	***		
dad college	0.107	0.146	***		
mum employed	0.813	0.875	***		
dad employed	0.960	0.962			
siblings	1.577	1.427	***		
no finanacial difficulties	0.590	0.599			
financial diffculties	0.242	0.241			
lncoh	13.460	13.599	***		
lnfcoh	0.072	-0.063	***		
lnpcoh	-0.071	-0.042	***		
	Pre-R	Ratkó	Post-R	Pre-R - R diff.	R - Post-R diff.
high school	0.600	0.603	0.610		
college	0.123	0.140	0.151		
graduation age	19.750	20.001	19.431		
employed	0.686	0.743	0.811	***	
intermediate skill jobb	0.487	0.463	0.450		
high skill job	0.280	0.299	0.291		
lnhwage	1.195	1.189	1.178		
mum high school	0.153	0.203	0.260	***	
mum college	0.021	0.023	0.032		**
dad high school	0.301	0.355	0.379	***	
dad college	0.049	0.078	0.063	***	*
mum employed	0.529	0.621	0.668	***	**
dad employed	0.963	0.963	0.969		
siblings	2.089	1.907	1.687	**	*
no finanacial difficulties	0.380	0.441	0.475	*	
financial diffculties	0.447	0.393	0.373	**	
lncoh	13.485	13.625	13.447	***	
lnfcoh	0.120	-0.113	-0.169	***	
lnpcoh	-0.096	-0.103	0.172	***	

* significant at 10%; ** significant at 5%; *** significant at 1%

Mean comparisons show that the two outlier cohorts (in size) are strictly better in almost all aspects than the previous cohort, the difference between family background variables are highly significant. However, it turns out that Post Ratkós are even better than the Ratkós, and it is them who form the peak cohort. Anyway, this suggests (however differences ar not always significant) that in larger cohorts have more education, are more likely to work and more likely in high skill jobs, they may also graduate at an older age on average. At the same time, they seem to earn less!

To see some more first glance results, I ran simple OLS regressions to explore the "average partial effect" of cohort size and position in the demographic cycle on graduation age, education attainment, being employed, working in low or high skilled job and wages. For each dependent variable I ran two specifications, one revealing the "effect" of position (ln(future coh) and ln(past coh)) and the other the size (ln(coh)). (See exact definitions of the variables below.) To spare room, I copied the coefficients below each other in Table 3, however, the dotted line denotes that beyond and below we can see the coefficients from alternative specifications. The whole output table for graduation age and education levels can be seen in Table 4. Specifications were run on the whole sample and on the subsamples of Ratkós and surrounding cohorts and Ratkó grandkids and surrounding cohorts.

Table 3: First-Glance Estimations for Cohort Size and Position in Demographic Cycle

	graduation age	high school	college	im. skill	high skill	employed	lnhwage
1939-1979							
ln(future coh)	3.136	-0.177	0.109	0.044	0.017	-0.117	0.029
	-1.737	(0.071)*	(0.042)*	-0.046	-0.042	-0.061	-0.061
ln(past coh)	-2.065	0.036	0.024	0.01	-0.064	0.032	0.124
	-1.729	-0.09	-0.045	-0.053	-0.041	-0.066	-0.073
ln(own coh)	-7.783	0.385	-0.143	-0.006	-0.051	0.391	-0.197
	(2.289)**	(0.096)**	(0.037)**	-0.036	-0.042	(0.114)**	(0.067)**
Ratkó kids and surrounding cohorts (1949-1960)							
ln(future coh)	10.653	0.026	0.073	0.004	0.1	-0.1	0.12
	(0.264)**	-0.078	-0.051	-0.084	-0.07	-0.072	-0.103
ln(past coh)	-12.52	-0.003	0.024	0.03	-0.127	0.182	-0.123
	(0.294)**	-0.084	-0.06	-0.084	-0.07	(0.067)**	-0.101
ln(own coh)	4.206	-0.006	-0.053	-0.083	0.053	-0.081	0.029
	(0.600)**	-0.104	-0.073	-0.099	-0.081	-0.082	-0.127
Ratkó grandkids and surrounding cohorts (1966-1979)							
ln(future coh)	15.585	-0.147	0.041	-0.144	0.302	0.047	0.098
	(0.655)**	-0.121	-0.096	-0.124	(0.109)**	-0.061	-0.156
ln(past coh)	-10.887	-0.056	0.012	0.303	-0.326	-0.117	-0.024
	(0.693)**	-0.158	-0.128	-0.155	(0.133)*	-0.078	-0.168
ln(own coh)	-26.467	0.049	-0.089	0.276	-0.456	0.002	-0.438
	(0.961)**	-0.128	-0.104	-0.16	(0.142)**	-0.078	(0.205)*

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

The main point is that these results are consistent with those of Falaris and Peters (1992), and inconsistent with the timing theory: someone in the upswing of a cycle graduates later and is more likely to graduate from college. Someone in the downswing graduates earlier however this and also the estimates for his/her schooling attainment is insignificant. This result carries over for both subsamples in a more significant way. Anywhere else, however, position in the demographic cycle seem to be irrelevant, rather own cohort size has some significant effect. E.g. for schooling attainment: In a larger cohort one is less likely to attend college. However, significances are very poor, from Table 4 it is clear that education is much more influenced by family background (e.g. parents' education) than adjusting to demographic conditions. This also points towards a strong composition effect and poor individual response.

Thus, the story may be consistent: A strong influence of family background vari-

Table 4: First-Glance Estimations for Cohort Size and Position in Demographic Cycle II – The Role of Family Background

	graduation age	graduation age	high school	high school	college	college
graduation year	0.097	0.134				
	(0.019)**	(0.017)**				
high school	3.024	2.979				
	(0.164)**	(0.167)**				
tertiery educ	9.369	8.972				
	(0.269)**	(0.226)**				
ln(future coh)	3.136		-0.177		0.109	
	-1.737		(0.071)*		(0.042)*	
ln(past coh)	-2.065		0.036		0.024	
	-1.729		-0.09		-0.045	
father high school	-0.478	-0.423	0.11	0.1	0.048	0.051
	(0.131)**	(0.129)**	(0.016)**	(0.015)**	(0.012)**	(0.012)**
mother high school	-1.246	-1.215	-0.002	-0.017	0.101	0.106
	(0.201)**	(0.213)**	-0.018	-0.016	(0.016)**	(0.016)**
father college	-0.111	-0.055	-0.141	-0.144	0.346	0.346
	-0.305	-0.285	(0.023)**	(0.024)**	(0.023)**	(0.023)**
mother college	-2.25	-2.205	-0.162	-0.181	0.291	0.297
	(0.421)**	(0.432)**	(0.036)**	(0.036)**	(0.037)**	(0.037)**
siblings	0.082	0.075	-0.046	-0.044	-0.015	-0.016
	(0.028)**	(0.028)*	(0.004)**	(0.004)**	(0.002)**	(0.002)**
central	0.645	0.643	-0.013	-0.008	0.066	0.064
	(0.159)**	(0.155)**	-0.013	-0.014	(0.010)**	(0.010)**
western	0.133	0.085	0.048	0.051	-0.017	-0.018
	-0.115	-0.109	(0.012)**	(0.012)**	(0.007)*	(0.007)*
ln(own coh)		-7.783		0.385		-0.143
		(2.289)**		(0.096)**		(0.037)**
Constant	-174.955	-143.746	0.632	-4.552	0.087	2.008
	(36.714)**	(29.315)**	(0.022)**	(1.298)**	(0.008)**	(0.492)**
Observations	8191	8191	8243	8243	8243	8243
R-squared	0.43	0.46	0.07	0.08	0.2	0.2

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

ables (composition effect) may account for the higher education, and therefore these people may not adjust to cohort size e.g through schooling decision. Therefore, an above average generation - both by virtue of its size and quality - gave birth to another large, above average generation. It also rises the interesting question whether the exogenous Ratkó shock in the '50s has introduced a quasi-natural cycle into demography.

3.1.3 Identification Strategy

I apply and extend the approach of Jeon and Berger (1996), who estimate the effect of realtive cohort size on wages on the 1991 Korean wage survey. They want to explore the effect of position in the demographic cycle and cohort size on wages. A simple Mincerian OLS augmented by cohort size variables would yield a biased estimator of cohort size effects, as cohort size is already known by the time of schooling choice, so individuals may adjust their decision. However, taking cohort size into account only at the schooling decision, that is, instrumenting education by cohort size, would not solve the problem either, because cohort size is not exogenous in the wage equation: it also has a direct labor supply generating impact. Therefore, Jeon and Berger (1996) apply a self-selection framework.

On the one hand, their estimation procedure handles the previously described problem, while on the other hand their sample population is by origin a selective one as it only contains individuals employed by firms with at least ten workers. They want to estimate the following equation separately for each education level:

$$W_{ij} = X_{ij}\beta_j + u_{ij}, \tag{3.1}$$

where i denotes the individual index while j is the level of education (l for less than high school, h for high school graduate and c for at least college/university graduate). W_{ij} is wages, while X_{ij} is a vector of explanatory variables, such as experience (age) or cohort size, type of occupation, tenure at the firm and firm characteristics, such as firm size or region.[4] However, the equation has to be corrected for selection bias, as it is estimated separately for each education level, in which individuals self-select. Their selection equation is the following:

$$S_i = Y_i^*\alpha^* + u_{si}^*, \tag{3.2}$$

where S_i is the level of education, Y_i^* includes factors that may influence schooling decision: parents' education or financial difficulties in teenagerhood, and variables on the position in the demographic cycle. Family background variables were not available for Jeon and Berger; they replaced by national income and region. However, they are available for me, so the estimation can be augmented by them.

The authors define a cohort as the group of people who were born in a 5-year span, in year k and +/-2 years surrounding (in years $k\pm1$ and $k\pm2$). 5- and 3-year spans are common in literature (see references). On the one hand because with such time spans all datasets have enough observations in one cohort. On the other hand, this way the members of a single cohort (in each education category) can be concerned as perfect substitutes for each other in the labor market, so this number describes the size of labor force a given individual competes with. Jeon and Berger (1996) do not only capture cohort size but also the position of the individual in the demographic cycle: whether the given person is in the upswing or a downswing of a cycle. The variable they apply is the log proportion of the following and preceding cohorts (those born between year $k+1$ and $k+5$, and between $k-5$ and $k-1$) to the given cohort (born between $k-2$ and $k+2$). As the authors claim, "In general, upswing (or pre-peak) cohorts will have

[4] Tenure is not available for me, while firm size has too many missing observations to be included. Also, as region I will include the residence region. However, due to the fact that Hungary is divided into three quite large regions in this survey, it must be identical to the region of current or last work.

larger FCOH [following/given ratio] and smaller PCOH [preceding/given ratio]; the opposite will be true for downswing cohorts." (Jeon-Berger 1996, p. 304.) However, as the authors claim, by including the log of these proportions, the partial effect of cohort size, by holding the size of past and future cohorts constant, is also revealed (see footnote 7, p. 307 in the article).

Just as most of the literature, Jeon and Berger (1996) run their regressions only for men in order to exclude discrimination and other gender impacts like the expansion of education among women presumeably mainly due to other factors. However, in Hungary, gender differences are claimed to be small in international comparison, and educational expansion has been similar for the two sexes. Thus, in order not to decrease sample size, I will use the joint sample of men and women, by including a sex dummy in the regressions. By this, I assume that other factors taken into account do not affect the two genders differently - only a constant gap lies between them. I will only use individuals aged between 25 and 65 (born between 1939 and 1979), because family background variables are only available for them. However, this would be adequate and common in the literature anyway, as they are part of working age population who have almost surely finished their studies.

In other aspects, I will follow the approach of Jeon and Berger (1996). First, I replicate their results *among the employed* and then I will raise a critique that could be amended in my database. In their selection equation (equation (2)), the left-hand side variable is a three-value ordered variable, highest education level. Therefore, this is not a standard Heckman procedure but one in which the first stage, the selection rule is determined by an *ordered* probit. However, Heckman's generalized method works out here as well.

Let us assume the ordered probit equivalents of the assumptions of the classical Heckman model. Following Wooldridge (1999, p. 562), these are the following:[5]

1. $X_j \subset Y^*$ for all $j = c, h, l$, that is wage determinants are also determinants of employment and schooling choice in the selection equation. However, it is not important to actually *include* all X in the selection equation (if we have a good reason to think that their effect would be 0). What *is* important is that what all X_j must be randomly observable for no matter which schooling group the individual falls into.[6] Nevertheless, we must always be careful to leave enough degrees of freedom to estimate β_j from the variation of variables in the

[5] From now on, I will leave the i indices denoting different individuals.

[6] However, the estimation can be amended to be able to treat endogenous Xs (Xs that are only observable together with W in the classical Heckman example, or Xs whose certain values we may not observe in a given schooling group). See Wooldridge (1999, pp. 567-570).

In Jeon's and Berger's article (and also my replication) including occupation and firm characteristics may hurt this assumption.

final stage *linear* regression and not just the nonlinear functional form of the selection correction terms. That is we need to pose some *exclusion restrictions* that is include a smaller number of variables in the final stage than in the selection rule.

2. (u_j, u_s)s are independent of Y^* with 0 mean for all $j = c, h, l$.

3. $u_s \sim N(0, 1)$.

4. $E(u_j|u_s) = \rho_j u_s$ for all $j = c, h, l$.

Now, based on Wooldridge (1999, pp. 560-566.), it is easy to derive the correction terms. If we take the expected value

$$E(W_j|Y^*, u_s) = X_j\beta_j + E(u_j|u_s) = X_j\beta_j + \rho_j u_s \tag{3.3}$$

from because assumptions 1 and 4. And further by applying the law of iterated expectations to this equation:

$$E(W_j|Y^*, S) = X_j\beta_j + \rho_j E(u_s|Y^*, S). \tag{3.4}$$

However, as W_j is only observed if the individual is in schooling group j (otherwise W_k would be observed), it follows that:

$$\begin{aligned} E(W_j|Y^*, S &= j) = X_j\beta_j + \rho_j E(u_s|Y^*, S = j) = && (3.5)\\ &= X_j\beta_j + \rho_j E(u_s|\mu_j - Y^*\alpha^* < u_s \leq \mu_{j+1} - Y^*\alpha^*) = && (3.6)\\ &= X_j\beta_j + \rho_j \frac{\phi(\mu_j - Y^*\alpha^*) - \phi(\mu_{j+1} - Y^*\alpha^*)}{\Phi(\mu_{j+1} - Y^*\alpha^*) - \Phi(\mu_j - Y^*\alpha^*)} = X_j\beta_j + \rho_j\lambda_j && (3.7) \end{aligned}$$

where λ_js are called the inverse Mills ratios and μ_js are the parameters ("cut-off points") of the ordered probit school choice equation, which will be estimated by the first-stage of the problem.[7] So this first-stage ordered probit will yield out the λ_js, and from equation (5) it is clear that running an OLS with λ_j as an extra regressor on the selected sample will give us an unbiased estimator of β_j.

Stata has a built-in command to do this either with the two-stage estimation method or maximum likelihood, I applied the two-step procedure to be easily able to calculate marginal effects.However, this way one should adjust the standard errors by bootstrapping.[8]

[7] For three educational categories as here, $\mu_0 = -\infty$, $\mu_3 = -\infty$, while μ_1 and μ_2 will be estimated by the ordered probit.

[8] Bootstrapping did not work out for me, so my standard errors are not consistently estimated, that is are smaller than should be. However, being aware of the results, it would not add too much.

3.2 Results and Discussion

I ran four kinds of specifications of the same problem. The first is exactly that of Jeon and Berger (1996). The second includes only own cohort size instead of position in the demographic cycle.[9] The third and fourth are the modified forms of the first and the second in a way, that in the second stage, in the wage equation (in X) schooling group specific variables are plugged in instead of the usual demographic variables. That is the own and the surrounding cohorts of an individual do not consist of everyone born in the given age, but only those of them who have the same educational attainment as the individual.[10] In the labor market this is already observed, while at schooling decision only the whole cohort van be observed. So in the first stages, the usual variables are included everywhere.

I report only specification (1), which is the most comparable to the originl article. However, results for other specifications are not different in any systematic way.

Because of interaction terms and the selection correction term partial effects of demographical variables are more complicated to infer than usually. Anyway, formally they can be derived directly from equation 6:[11]

$$\begin{aligned}\frac{\partial^k E(W_j|Y^*, S=j)}{\partial X^k} &= \beta_j^k + \rho_j \alpha^{*k} \cdot (\mu_j - Y^*\alpha^*)\frac{\phi(\mu_j - Y^*\alpha^*)}{\Phi(\mu_{j+1} - Y^*\alpha^*) - \Phi(\mu_j - Y^*\alpha^*)} - \\ &\quad -\rho_j \alpha^{*k} \cdot (\mu_{j+1} - Y^*\alpha^*)\frac{\phi(\mu_{j+1} - Y^*\alpha^*)}{\Phi(\mu_{j+1} - Y^*\alpha^*) - \Phi(\mu_j - Y^*\alpha^*)} - \rho_j \alpha^{*k} \lambda_j^2,\end{aligned}$$

where β_j^k is the coefficient of the demographic variable in the second stage, in the wage equation, α^{*k} is its coefficient in the first-stage, selection equation, while ρ_j is the estimated coefficient of λ_j in the wage equation.

Actually, ρ_j is the correlation.between the error terms, so its t-test in the wage equation reveals whether there is sample selection or not. As we can see from Table 5, the selection correction term seems to be significant in the two lower schooling groups (however, with standard error correction, their significance may diasappear as well). This suggests that those who are more likely to self select into one of these schooling groups are people who are more likely to earn less (as coefficients are negative). This is more robust for the high school group across specifications but slightly significant

[9] However, the coefficient of the own cohort size can be calculated from the demographic position variables (see above), it is more direct this way.

[10] This may rise soome endogeneity issues mentioned in assumption 1 at the identification strategy. However, it does not alter the results and main conclusion.

[11] In our 3-category case, this formula only for 1, which is not an extremal category. For 0 and 2, the extremal categories the inverse Mills ratio becomes simpler, so a simpler formula has to be derivated to calculate partial effects.

Table 5

	low schooling	high school	college
_lambda	-0.0668347	-0.040524	0.0371555
	[0.043]	[0.022]*	[0.038]
sex	-0.1314794	-0.1765479	-0.2977245
	[0.031]***	[0.016]***	[0.038]***
experience	-0.0035746	0.0105329	0.0243833
	[0.014]	[0.005]**	[0.009]***
experience^2	0.0003505	-0.0001358	-0.0004813
	[0.000]	[0.000]	[0.000]
ln(future coh)	-1.480014	0.7910525	1.88485
	[1.631]	[0.421]*	[1.882]
ln(past coh)	-2.212238	0.9523657	2.554691
	[1.756]	[0.408]**	[5.255]
lnfcoh*exper	0.1677816	-0.0712068	-0.0140031
	[0.131]	[0.043]	[0.080]
lnfcoh*exper2	-0.0034772	0.001512	0.0003662
	[0.002]	[0.001]	[0.003]
lnpcoh*exper	0.1869823	-0.0567298	0.0341277
	[0.141]	[0.045]	[0.083]
lnpcoh*exper2	-0.0032414	0.0006682	-0.0016457
	[0.003]	[0.001]	[0.003]
im job skill	0.0296468	-0.0347084	-0.1674369
	[0.033]	[0.021]	[0.594]
high job skill	0.2093917	0.1774112	0.073023
	[0.080]***	[0.025]***	[0.591]
lnfcoh*im job	-0.1757391	-0.0288287	-1.53653
	[0.278]	[0.183]	[1.885]
lnfcoh*high job	-0.9669817	-0.0848443	-1.9471
	[0.648]	[0.206]	[1.804]
lnpcoh*im job	-0.2332817	0.1639767	-1.752885
	[0.311]	[0.188]	[5.266]
lnpcoh*high job	-0.4270945	-0.0303687	-2.523221
	[0.768]	[0.218]	[5.235]
central region	0.0984251	0.0991143	0.0116822
	[0.044]**	[0.020]***	[0.046]
western region	0.0810965	0.0281438	-0.0215208
	[0.034]**	[0.017]	[0.049]
Constant	0.6081852	0.9424387	1.40659
	[0.180]***	[0.047]***	[0.591]**
N	5568		

* p<0.10, ** p<0.05, *** p<0.01

for the lower educated as well in other specifications. While from the positive though never significant coefficient of lambda in the college sample we can infer that those who are more likely to self-select into the highest education category, are those who are more likely to earn more anyway.

Straightforward average partial effects for specification (1) are shown in Table 6, however without standard errors. The interpretation is the following: A one percetage point rise in the relative number of past/future cohorts compared to the given cohort changes the wages by these percentage points. These seem to be great effects, however may not be signifcant. The sign are nevertheless informative: Low educated people are worse off if surrounding cohorts increase, therefore they are the beneficiaris of large cohorts. The other two groups experience the opposite, so may prefer to live in as small a cohort as possible.

Table 6: Average Partial Effects of Position in the Demographic Cycle

	low schooling	high school	college
past cohort	-2.225	0.956	2.542
future cohort	-1.502	0.800	1.868

On the other hand, Jeon and Berger (1996) does not stop here, they also calculate a kind of predicted wage. They want to proxy the wage expectations of people at the moment of the schooling decision. So they plug in all individual characteristics that are supposed to be known at the decision (cohort sizes, region, and for me, sex), and for what is not assumed to be known (experience, occupation and tenure and firm size for them), they plug in sample means. This way they predict counterfactual and fitted (expected) wages for everybody for all three potential education levels. Putting those into one table together with actual wage Jeon and Berger (1996) argue, that schooling choices in Korea are optimal, as in all three educational groups the log of wages is the highest among counterfactual alternatives, that is the labor market.operates on the basis of comparative advantages.

In Hungary, see Table 7, the educational choice of the lower educated and college graduates are optimal. However, high school graduates earnings potential would be higher with either one of the other two educational attainment.[12] Generally, differences in earnings potential seem to be very high from this table, which underlines the issue of self-selectivity.

[12] I estimated these by a built-in command in Stata, which fits contrafactual values by taking into account the actual choice of the individual. But as a disadvantage, I just fitted these for the observed values of experience and occupation, and could not imput sample values of those variables that are assumed to be unknown at the time of schooling decision.

From the very same wages, wage differentials are calculated, and with the help of them, the authors derive a structural schooling choice equation:

$$S_i = Y_i\alpha + \delta_1(\hat{W}_c - \hat{W}_h) + \delta_2(\hat{W}_h - \hat{W}_l) + u_i, \tag{3.8}$$

where $(\hat{W}_c - \hat{W}_h)$ and $(\hat{W}_h - \hat{W}_l)$ are the calculated wage differentials, while Y_i are individual factors (region and national income for him; region and family background variables for me). They call it structural because as opposed to first stages in the selection correction procedure, where demographic variables directly enter the equation of schoolin decision, here, cohort sizes only affect educational choice indirectly, through expected wages. The authors obtain positive and significant coefficient estimates on both wage differences,[13] which may be interpreted as some pieces of evidence for adjusting schooling decision to expected wage differentials, and so to cohort sizes reflected by them.

Table 7: Actual and Counterfactual Wages by Education

actual choice	actual wage	counterfactual choice	counterfactual (fitted) wage
low school	0.8738	low school	0.8994
		high school	0.8057
		college	0.8143
high school	1.0608	low school	1.0926
		high school	1.0627
		college	1.1051
college	1.5369	low school	1.3494
		high school	1.4258
		college	1.5408

Results on Hungarian data are shown in Table 9, where the structural schooling equation may be compared with the first-stage equation. In Hungary *coefficients* of wage differentials in the structural model are also positive and largely significant, while coefficients of cohort size variables are not significant in the first-stage equation. I reported average partial effects by Stata, and it yielded absolutely no effect of cohort size variables in the first-stage and no effect of expected wage differentials in the structural model. In both equations, the effect of family background variables, especially parental education are extremely strong.[14] This supports the hyothesis I set up at the "stylized facts" part, that the effect family background so very strong in Hungary, that it nearly completely determines education, independently of cohort size or expected wages. In this analysis at least, I found no evidence for any schooling adjustment,

[13] However, they do not report marginal effects.

[14] In fact, Jeon and Berger (1996) cannot measure family background effects. So their results are also exposed to some of this risk.

so the stylized fact that large cohorts, like Ratkó kids and grandkids are relatively more successful from the labor market point of view in Hungary may fully be due to compositional effects.

3.3 A Critique and a Potential Extension

Through the analysis, one of the main advanatages of the database available for me remained unexploited. In EU-SILC one does not only observe the employed, but also unemployed and inactive people. That is, another source of selection, being employed, could be ruled out, however, methodologically the estimation would be more difficult. This selection may be of much importance as cohort crowding may be consistent with non-decreasing wages, but detereorating employment, in which case the method of Jeon and Berger is not sufficing.

One could extend Jeon and Berger's (1996) method by tackling a double and sequential selection problem. The first is self-selection into an education group as an individual behavior response to a cohort size change or to the position in the demographic cycle, just as before. Becoming employed in the labor market will be a selection based on acquired education, and again the demographic conditions, this time more from the demand side.. For self-selection into schooling groups, the selection equation is equation (2), while the outcome stage is the following:

$$emp_i = Z_i\gamma + v_i. \tag{3.9}$$

emp_i is a dummy taking the value 1, if individual i is employed during at least a part of the reference period and 0 otherwise. Z_i is a vector of factors determining employment from the supply side such as age, being married, having children, health conditions, "love of work" proxied by activity status of parents in teenagerhood, and generosity of alternative opportunities proxied by social transfer that the whole household receives. In addition, cohort size (and position in the demographic cycle) is again included in Z_i, however it influences employment rather from the demand side. The third stage of the proplem is the same wage equation (equation (1)) that we had so far on the second stage.

As in the first part of the problem even the outcome equation is a probit model, which bases on an ordered probit selection rule, the Heckman type two stage method by itself cannot be generalised to three stages.it. Rather a full maximum likelihood estimation could solve the problem, or a maximum likelihood on the part with the probit outcome and ordered probit selection, and then after obtaining the inverse Mills ratios of both stages, one could add them as selection correction terms to the final wage equation. In this case we would estimate the following by OLS: wages

$$W_{ij} = X_{ij}\beta + \mu\lambda^s_{ij}(Y_i^*\alpha^*) + \rho\lambda^e_i(Z_i\gamma, Y_i^*\alpha^*) + \varepsilon_{ij}, \tag{3.10}$$

where $\lambda^s_{ij}(Y_i^*\alpha^*)$ for $j = 0, 1, 2$ are the inverse Mills ratios belonging to the selection equation (just as above), and $\lambda^e_i(Z_i\gamma, Y_i^*\alpha^*)$ being the inverse Mills ratio from the second stage, the hazard rate to become employed [15] One should, however, show under what assumption this estimation gives unbiased $\hat{\beta}$.

At the same time, building a full maximum likelihood model would account for both selections, and with that one should not have to bother with the adjustment of standard errors.

[15] Thanks to Justin Falk, who gave mw this hint by his post on the Statalist and a helping email.

Table 9: First-Stage and Structural Schooling Equations

	First stage	Structural
sex	-0.0048905	0.6759571
	[0.032]	[0.043]***
ln(past coh)	0.33509	
	[0.223]	
ln(future coh)	0.4938388	
	[0.211]**	
high sch-low wdiff.		3.251844
		[0.202]***
col-high sch. wdiff		4.249425
		[0.200]***
dad high school	0.36335	0.3633875
	[0.042]***	[0.046]***
dad college	1.174623	1.337965
	[0.082]***	[0.089]***
mum high school	0.3741742	0.4331983
	[0.044]***	[0.048]***
mum college	1.00756	1.312335
	[0.104]***	[0.105]***
lnfcoh*mum hs	-0.0181204	-1.228631
	[0.380]	[0.398]***
lnfcoh*mum col	2.166055	1.242657
	[0.942]**	[0.955]
lnpcoh*mum hs	0.1373751	-0.6848796
	[0.403]	[0.480]
lnpcoh*mum col	0.0031494	-0.2880686
	[1.040]	[1.046]
lnfcoh*dad hs	-0.5447043	-2.239789
	[0.353]	[0.308]***
lnfcoh*dad col	-0.4808204	-2.032224
	[0.685]	[0.698]***
lnpcoh*dad hs	-0.1380952	-3.318776
	[0.372]	[0.396]***
lnpcoh*dad col	-0.4586278	-2.887457
	[0.749]	[0.844]***
financial difficulties	-0.0665661	-0.0468284
	[0.051]	[0.057]
no financial diff.	0.1000324	0.1306469
	[0.046]**	[0.051]**
siblings	-0.1576847	-0.1810381
	[0.011]***	[0.014]***
central region	0.1778199	0.6595394
	[0.041]***	[0.048]***
western region	-0.0484586	0.3385912
	[0.038]	[0.044]***
cutoffs		
cutoff1	-0.8651	
	[0.056]***	
cutoff2	1.311426	

	[0.059]***	
cut1		
Constant		1.946153
		[0.113]***
cut2		
Constant		4.639298
		[0.133]***
Pseudo-R-squared		0.321
N		5568

* p<0.10, ** p<0.05, *** p<0.01

Chapter 4

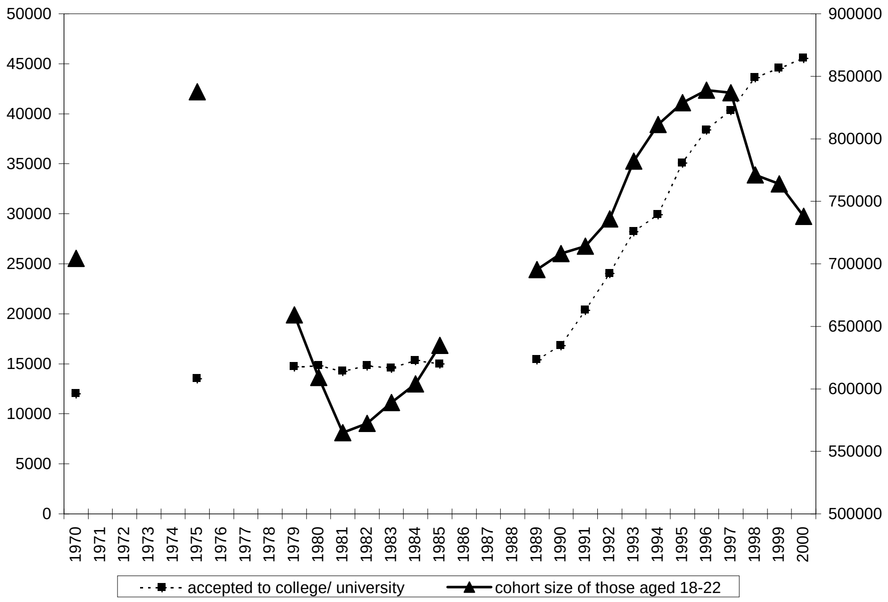
Conclusion

In my thesis I investigated cohort size effects on wages in Hungary. I replicated the paper of Jeon and Berger (1996) on Hungarian survey data. My dataset was completely different from that of the authors, and I managed to exploit one main difference, the availability of family background variables, such as parents education. I estimated a selection model of wages, in which the selection rule was an ordered tobit model of schooling decision. This model of Jeon and Berger belongs to the class of individual response literature in the field of cohort effects studies, as this modelling framework bases on indivduals' adjustment of education choices to changes in demographic movements. Out of the two basic mechanisms, however, neither was found in Hungary. People do not invest less in their human capital in larger cohorts because they expect the returns to fall, and neither do they study more to be more prepared in a more competitive labor market, or to postpone labor market entry to times of smaller entering cohorts. However, Hungary bears two unusually large cohorts as a result of abortion legislation in the communism, no adjustment efforts are suspected not even those and the surrounding crowded cohorts. In contrast, all changes in education seem to be caused by compositional effects. In Hungary the returns of schooling are high (see Table 8), education is heavily determined by parents' schooling, and education is self-selected: Those people go to high school or lower education, whose earning potentials are by origin smaller. These results call our attention to the importance of family background in such an analysis and warn to treat Jeon's and Berger's findings on individual adjustment with caution.

The other advantage of my data, that also unemployed and inactive people were represented in the dataset, remained unexploited so far, although efforts were taken to generalize this framework to a double selection problem by including employment status as an additional selection rule, which would reflect some of the slection coming from the side of labor demand. This may be crucial, especially in Europe, where wages are more rigid and crowded cohorts may not be struck by reduced wages but by falling employment. To tackle this double selection problem is an extension of this work, which assumes maximum likelihood programming.

Appendix

Figure 4.1: College/University Places and Relevant Cohort Size, 1970-2000 *(Source: HCSO 1986, Stadat)*



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