Cohort Effects on Earnings Profiles: Evidence from Sweden*

Susanne Dahlberg and Ruth-Aïda Nahum†

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Abstract

The cohort crowding literature suggests that the size of one's generation, or cohort, has repercussions on the level and shape of one's earnings profile. We estimate cohort size effects on earnings profiles and further assess whether these profiles are affected by the individuals' position in the Baby Boom. Using a rich individual based panel data set, we follow the Swedish Baby Boomers of the 1940's and the following Baby Bust of the 1950's from 1968 to 1999. Our results indicate that there are significant cohort effects on the earnings profile which are fairly consistent across gender but not across education levels. Large cohorts have a higher overall earnings level than small cohorts. Cohorts born in an upswing of a boom have a higher earnings level than cohorts born in a downswing. The effects on return to experience vary across education and experience levels.

JEL-Classification: J21; J31

Keywords: Earnings profiles, Baby Boom, Cohort size, Panel data

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†Department of Economics, Uppsala University, Box 513, SE-751 20 Uppsala; Institute for Future Studies, Box 591, 101 31 Stockholm Sweden, fax: +46 (0) 18 471 14 78, e-mail: susanne.dahlberg@nek.uu.se; ruth-aida.nahum@nek.uu.se

1 Introduction

Mincer-type equations typically explain wages as a function of individuals' experience, education and a number of control variables. However, the cohort crowding literature suggests that the size of one's generation, or cohort, has repercussions on the level and shape of one's earnings profile. If that is the case, it is important to incorporate demographic variables into the framework of a Mincer-type equation. Most of the literature in this area has focused on the post-second world war American context, where cohort effects have been found to have a significant impact on individuals' earnings profiles. With cohort effects we refer to both cohort size and position in the demographic cycle, i.e. whether one is born in an upswing or a downswing of a boom. The Swedish context differs in many respects from the American. The Swedish Baby Boom (see Fig. 1) of the 1940's started almost a decade earlier than the American one. It was short-lived covering only 10 years and was not as large in magnitude. It is also important to note that the Swedish labour market has been more regulated than the American one, possibly making it less likely for cohort effects to be significant. Little has been done on an aggregate level to assess cohort effects on individuals' earnings profiles in Sweden. Given the large differences between the countries, we find it of interest to see if the Swedish case differs from the American one with respect to cohort effects on earnings profiles.

The aim of this study is to estimate the effects of cohort size and position in the demographic cycle on returns to experience as well as on the overall earnings level. The study is conducted on cohorts born during the Swedish Baby Boom of the 1940's and the following Baby Bust of the 1950's. We further assess whether the effects on return to experience vary during the individuals' working life.

Compared to much of what has been used in the literature in this area, we have an exceptionally rich individual based data set, LINDA. The data is representative for the Swedish population, and it covers three percent of the total population. We can follow the individuals over time, which enables a longitudinal analysis and does not confine us to an analysis of a cross section or a panel of cross sections. LINDA spans a period of three decades, from 1968-1999. Since this data is representative for every year, those who pass away or emigrate disappear from LINDA, while newborns and immigrants are added to the data. Most individuals in our study are followed over a substantial part of their working life.

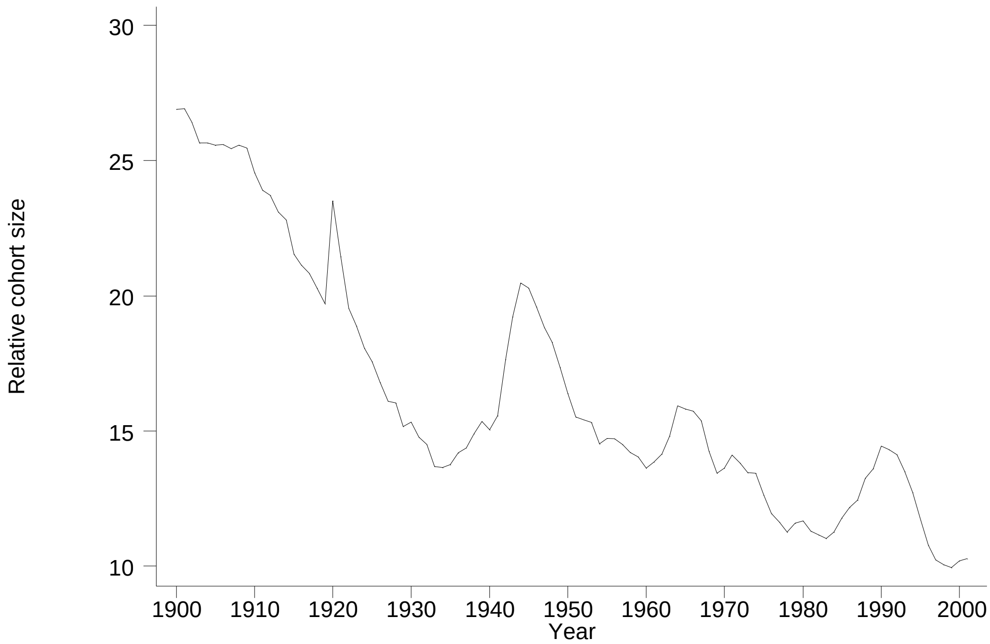

Figure 1: Number of births per thousand inhabitants in the 20th century Sweden. The number of births increased drastically in the early 1940's as compared to the low fertility period of the 1930's. After a sharp peak 1944 the number of births declined until the end of the 1950's. Considering the relatively low fertility later on, it is obvious that the Baby Boom generation of the 1940's must have had a great impact on the economy.

Our results show that, for the Swedish Baby Boom of the 1940's and the following Bust of the 1950's, cohort size has had a significant impact on earnings profiles. Our main finding is that large cohorts have a higher overall earnings level than small cohorts. Results regarding the return to experience differ between education and experience levels. In the beginning of working life, high educated in large cohorts have flatter profiles while low educated have steeper profiles than small cohorts. Later in working life, after 12 years of experience, large cohorts have flatter profiles, but the profiles turn steeper after 25 years of experience. Cohorts born in an upswing of a demographic cycle have a higher earnings level than cohorts born in a downswing. Here again, the effect on the steepness differs between education and experience levels.

The next section presents an overview of the literature as well as a theoretical background to cohort effects on earnings profiles. Section 3 describes the data, followed by a section on the empirical framework. The results are presented in section 5, and the last section summarises and concludes.

2 Cohort effects in the literature

2.1 Cohort size and earnings profiles

The main argument in the cohort crowding literature, e.g. Welch (1979) and Easterlin (1987)[1], is based on the assumption of imperfect substitution between young and old workers. Since young and old workers do not compete for the same jobs, a large young cohort entering the labour market faces a tougher competition due to its large numbers, whereas older workers should not be as affected. The tougher competition faced by large cohorts has implications both on their level of earnings and return to experience. Easterlin (1987) suggests that the cohort effect persists throughout one's career. According to Easterlin (1987), it is the wages of young workers relative to that of old workers that decline when a large cohort enters the labour market. This implies that the cross-sectional earnings profile, i.e. the earnings profile across ages a given year, would get steeper. It is important to note that this is not the effect we are estimating in this study, and it is neither the

[1] Easterlin presented his hypotheses as far back as in the 1960s. See for example: "The American Baby Boom in Historical Perspective" from 1962, and "Population, Labor Force, and Long Swings in Economic Growth." from 1968.

case in Welch's article. We follow individuals over time and compare the average lifetime earnings level and the return to experience of large cohorts with that of small cohorts. In other words, we estimate the cohort effects on the longitudinal earnings profiles. This will enable us to see how cohorts are affected during their career. Easterlin does suggest that the cohort effect persists throughout one's career. However, a measure on the relative earnings between young and old is only feasible when estimating the effect on one age group, and not when estimating the effect for a cohort over time. It is not obvious which group to relate the earnings to as the cohort gets older.

Based on US data, Welch (1979) finds evidence of a negative effect on earnings of belonging to a large cohort. He estimates one main effect over life, and one effect for the early part of the individuals' working life using an early career spline. Both effects are negative with a stronger early career effect, pointing at a steeper earnings profile for large cohorts. Welch looks at education levels separately, assuming that individuals compete mainly with others in the same level of education. That is, substitution occurs mainly within education levels. He finds evidence of stronger cohort effects for higher levels of education and suggests that the degree of substitution varies across education levels - the lower the education level, the higher the substitutability between experience groups.

Berger (1989) interacts cohort size with both experience and its square to allow the cohort effects to vary with experience. He includes the size of surrounding cohorts, also interacted with experience, to control for position in the demographic cycle. In contrast to Welch, he finds a positive effect on the earnings level and a negative effect on the return to experience, resulting in flatter earnings profiles for large cohorts. Berger uses a human capital approach to interpret his results and his arguments are based on the theories by Stapeltone and Young (1988) and Nothaft (1985). Their theories state that members of a large cohort would be inclined to invest less in education. The starting-point is that the earnings of large cohorts are depressed by their large numbers, and that high educated are more affected by their own cohort size due to their lower substitutability. This implies a lower return to education for large cohorts. Berger uses these models to explain post-schooling investments in human capital, where people choose between different career paths, some comprising more learning than others. Large cohorts, who are more inclined to choose careers with less investments, should then have higher initial earnings and flatter profiles than small cohorts. Cohorts surrounded by large cohorts choose more investments, and hence get lower initial earnings and steeper profiles, than cohorts surrounded by smaller cohorts. Berger's findings support these theories and he concludes that large cohorts invest less in post-schooling human capital, while cohorts surrounded by large cohorts invest more.

An alternative hypothesis is that the increased competition among members of large cohorts makes people try harder and invest more in their human capital in order to maintain their relative positions. In this respect, members of large cohorts will invest more in education. According to Ohlsson (1986), this is what has happened in Sweden. He finds strong positive correlations between cohort size and enrolment rates for cohorts born 1904-1954. However, he does not control for any other factor that could affect the enrolment rates, e.g. business cycles and the expansion of the educational sector.

Most empirical evidence in this area is based on the US experience. One exception on Swedish data is Klevmarken (1993), who, based on a rich panel data set, did not find any significant cohort effects. Considering the differences in the Swedish context compared to the American, his results are credible. He also controlled for demand effects by including industrial investments and found them to be of much greater importance than cohort effects. However, finding a good measure for labour demand, which one can be confident is not a proxy for something else is not easy. One could for example argue that there is a problem of capital-skill complementarities when using industrial investments as a measure of labour demand. Furthermore, including the demand perspective in our study would make us loose comparison grounds with much of the literature in this area, which has focused on the supply side effects. Another possible reason for Klevmarken's insignificant results could be that he estimated his regressions on an aggregate level for the entire labour force using only three waves of data. If cohort size effects hit experience and education groups differently, the effect on the aggregate level could very well be cancelled out.

Two other Swedish studies have looked at specific occupational groups. Tasiran & Gustafsson (1992) look at salesmen and shop assistants, and find negative cohort size effects and steeper earnings profiles for large cohorts. Jonsson & Klevmarken (1978) look at engineers and find flatter profiles for large cohorts.

2.2 Position in the demographic cycle and earnings profiles

Another aspect of cohort effects is the position in the demographic cycle. It is possible that those born in the leading edge of a boom face a different situation on the labour market, than those born in the fall, the lagging edge. There are no clear theories of what to expect in this case. However, as in the case of cohort size, substitutability between age groups is an important factor when trying to clarify the effects of the position in the demographic cycle.

In an Easterlin-type framework, it is possible to simply assume that young and old workers are not substitutes, since this kind of model concerns the effects of a large young generation on the relative wages between young and old workers. When looking at the effects of the position in the demographic cycle, it is not that simple. Two cohorts with an age difference of only one year are of course close substitutes, but what about an age difference of three years, or five years, or ten?

Consider a labour market where all workers are perfect substitutes. The effect of a large cohort entering the market would be spread out equally over cohorts, and the effect on the individual level would be small. In a labour market where substitution takes place only within cohorts, the effect of a large cohort would be confined to that specific cohort, and the effect would be large. If there is some degree of substitution between adjacent cohorts, which is probably the most realistic case, the own cohort as well as some of the surrounding cohorts are affected.

What is the effect then of belonging to e.g. the lagging edge, that is entering the labour market after large cohorts, while one's own cohort also is rather large? When entering the labour market individuals are more affected by older cohorts than younger, since the younger cohorts have not entered yet. The older cohorts have managed to gain a few years of experience and thus have an advantage over the entering cohorts. In the first type of labour market there should not be any difference between the leading and lagging edge, since all workers are perfect substitutes. In the second type of labour market, when substitution takes place only within cohorts, the lagging edge cohorts might be better off since the older peak cohorts have paved the way on the labour market possibly giving a rise in labour demand. This is in line with what Easterlin expects for cohorts born after large cohorts. In the case with some degree of substitution between adjacent cohorts the effect is harder to foresee. It depends on how substitutable one's cohort is with the surrounding cohorts. If one's cohort is substitutable with the surrounding cohorts including the peak cohorts one's earnings should be adversely affected of belonging to the lagging edge, since then one has to compete not only with the own cohort but also the large peak cohorts. If one's cohort is only substitutable with those who are just a few years older, the effect could go in either direction. On the one hand, the effect could be negative since one is still following large cohorts who are substitutes. On the other hand, the effect could be positive if the older large cohorts who are not substitutes have, so to say, paved the way.

This discussion has focused on what happens in the beginning of one's working life. What happens in the later phases of working life depends on whether older cohorts maintain their relative advantage of having more experience. Moreover, the substitutability with adjacent cohorts could vary with experience.

Macunovich (1999) conducts an empirical study on an updated version of the data used by Welch (1979) and Berger (1989). She controls for position in the boom with the first and second differences of her cohort size variables, and finds that those born in the lagging edge fare worse than those born in the leading edge.

Berger (1989) controls for the size of the surrounding cohorts. He finds that being born before or after a large cohort has a depressing effect on the earnings level, but a positive effect on the return to experience. The effect of being born after a large cohort is larger than the effect of being born before a large cohort, implying that Berger's results are in line with Machunovich's: those born in the fall of a demographic cycle are worse off compared to those born in a rise.

What could be expected for different education levels? Even if we assume that high educated are less substitutable across experience groups than low educated, we cannot anticipate the effect of the individuals' position in the boom. If low educated are easily substitutable between age groups, while high educated are close substitutes with those who are e.g. up to five years older and younger, then the high educated should be more affected by their position in the boom. If low educated are close substitutes with those who are five years younger and older, while high educated are only substitutable within the own cohort, then the low educated should be more affected. Deeper knowledge on the degree of substitutability within education groups would help entangle the effects of the position in the demographic cycle.

2.3 Related literature

There is a related literature on cohort size effects on unemployment. Looking at youth labour markets, Korenman & Neumark (2000) show that large youth cohorts lead to increases in the unemployment rate of youth. Bloom, Freeman & Korenman (1987) show that large cohorts tend to have a negative effect on employment and earnings. However, looking at regional labour markets, Shimer (2001) finds results contradicting the cohort crowding literature. An increase in the youth share of the working age population reduces the youth unemployment rate as well as the prime age unemployment rate. He explains these results to be due to a higher tightness on the youth labour market. In a similar study on Swedish data, Nordström Skans (2002) shows the same tendency for the youth, but not for the prime aged.

3 Data

The analysis is based on data from LINDA, a register-based longitudinal data base. It contains a representative sample, of approximately three percent, of the Swedish population. The information is taken from registers such as income registers (Inkomst- och förmögenhetsstatistiken), Population and Housing Census (Folk- och Bostadsräkningen) as well as Higher education register (Högskoleregistret), (Edin & Fredriksson 2000). The Income register is based on filed tax reports, which makes information on income contingent on the tax legislation of that year. The 1991 tax reform poses some problems of comparability before and after the reform. However, we do not see any clear break in the trend.

Our data set covers information on earnings for individuals born between 1941 and 1960, and spans the years 1968 to 1999. This results in an unbalanced panel. The oldest cohort, those born 1941, are first observed at the age of 27, whereas we start observing the younger cohorts, those born from 1949 and onwards, at the age of 19. Individuals are followed as long as the data allows, but only up to the age of 50. Our data contains 38 759 men and 33 017 women. The total number of observations for men are 744 279, and for women 457 039.

Earnings are measured in terms of annual labour earnings, in 1999 price level. Unemployment insurance as well as social benefits are not included in this measure of earnings whereas sick pay is included. Since we do not have any information on the number of hours worked, and since we would have preferred full time equivalent earnings, we exclude observations below a minimum earnings level of what can be considered as full time wages. This minimum level corresponds to full time earnings for the lowest quartile of catering assistants employed in the local government, and amounts to 13 200 SEK/month in 2001. Since information on the distribution of wages is not available before 2000, we assume that the lowest quartile's share of the mean wage of local government employees was the same before 2000 as in 2001. For men, this share is 69%, and for women 76%. By doing this, we exclude the majority of part time workers, since part time workers are more concentrated among low income workers. Looking at manufacturing workers in the 1984 survey of Household Markets and Nonmarket Activities, (see Klevmarken and Olovsson 1984), 44% in the bottom quartile worked less than 30 hours, as compared to 4% in the remaining part of the earnings distribution. Our results are not sensitive to marginal changes in the minimum level of earnings.

Information on education is available from 1991 onwards, which implies that the individuals born 1941-1960 are between 31 and 50 years of age when their level of education is observed. Since we do not have information on the individuals' education when they enter the labour market we have to assume that the observed education level corresponds to that when entering the labour market. Education is divided into four levels. The first level refers to a maximum of two years of high school education, (8-11 years). The second refers to three years of high school education (12 years), the third level to up to two years of college or university education (13-14 years), and the fourth level to three or more years of college or university education (15+). These education levels correspond to those used by Welch (1979) and Berger (1989).

Our measure of experience is

$$EXPERIENCE = AGE \; - \; YEARS\ IN\ EDUCATION \; - \; 7$$

As our education variable is observed at a late stage in life, experience becomes negative when individuals are observed prior to the minimum age they must have reached in order to have fullfilled their education. The negative observations have been set to zero. However in many of these cases, the individuals have low earnings levels and fall out of our sample due to our earnings limit.

Aggregate demographic data, migration and unemployment data is taken from Statistics Sweden (SCB). Data relating to GDP per capita is retrieved from the National Institute of Economic Research (Konjunturinstitutet).

4 The Empirical Setting

4.1 Distinguishing between effects

Turning back to the issue of pinpointing cohort effects on earnings profiles, it is clear that age, time and cohort effects need to be discerned. If wages of an individual are influenced by events associated with the cohort's birth year (C), the particular year in which wages are observed (Y) and the age in the observation year (A), as well as by other variables. Since Y = C+A, an identification problem arises, making it impossible to include birth year, age and observation year in the same regression. As Heckman and Robb(1985) argue, the underlying problem is basically a lack of information. Age, cohort and time dummies are only proxies for the underlying information. Age is a proxy for physiological aspects related to aging which affect ones situation on the labour market, as well as a proxy for factors related to the experience accumulated over the years. Period effects are a proxy for factors, which are time variant and constant for all individuals a given year, for example macroeconomic variables such as GDP growth rate and unemployment. Cohort affiliation reflects characteristics, which are specific to a given cohort, such as cohort size and the position in the Baby Boom. In our study we deal with the identification problem by using underlying information for at least one of the effects.

Even though the identification problem can be overcome by using underlying information, the problem of distinguishing between the effects still persists. Period effects can for example be interlinked with cohort effects in a way that is difficult to discern. An example is macroeconomic variables which, a given year, can affect cohorts differently, depending on for example where in their career phase they have reached, or which sector they belong to. There is a clear trend in terms of sector affiliation of cohorts in Sweden as stated in Statistics Sweden's press release (nr 2002:233), making cohorts more or less vulnerable to macroeconomic shocks depending on which sector they belong to.[2] It is however impossible to clearly disentangle all the

[2] According to the press release, those born in the 1940's are to a large extent working

effects since it would necessitates detailed knowledge of how specific cohorts are affected by macro economic shocks.

An illustrative way of showing the difficulty in separating time, age and cohort effects, suggested by Burbidge et al (1997), is to examine three-dimensional graphs to get a picture of the relative importance of these effects. Figures A and B show 3-D graphs from our data for men and women respectively.

4.2 Econometric specifications

Our economic specifications are based on Mincer's wage equation where the earnings profile is conventionally assumed to be hump-shaped. Since we are interested in estimating cohort effects separately for different experience levels, we do not use the traditional quadratic specification. Instead, we use a spline transformation where the earnings profiles are assumed to be linear within specific experience levels, resulting in different slopes for each level. When constructing the spline, we divide the sample into three groups referring to experience levels 0-11, 12-24 and 25+ years respectively. These spline boundaries are chosen in order to have three periods of comparable size. The estimation results are not sensitive to marginal changes in spline boundaries.

4.2.1 Our cohort size measure

We measure cohort size as the number of births per thousand inhabitants for each birth cohort. Measuring cohort size as the actual size of the cohorts when observed on the labour market would induce endogeneity problems due to migration. Earnings levels are likely to affect both immigration and emigration. In this respect, birth rates can be considered exogenous. Furthermore, birth rates are cohort specific and fixed over time, making it easier to identify cohort effects as compared to using a time varying cohort size measure, like actual cohort size. Part of the time variation is due to endogeneity.

in public authority, those born in the 1950's work within municipality and county council, whereas those born in the 1960's are to a larger extent employed within the private sector. Considering that these are the sectors expanding during the labour market entry of the respective cohort, this is yet an example of the difficulty in distinguishing between period and cohort effects.

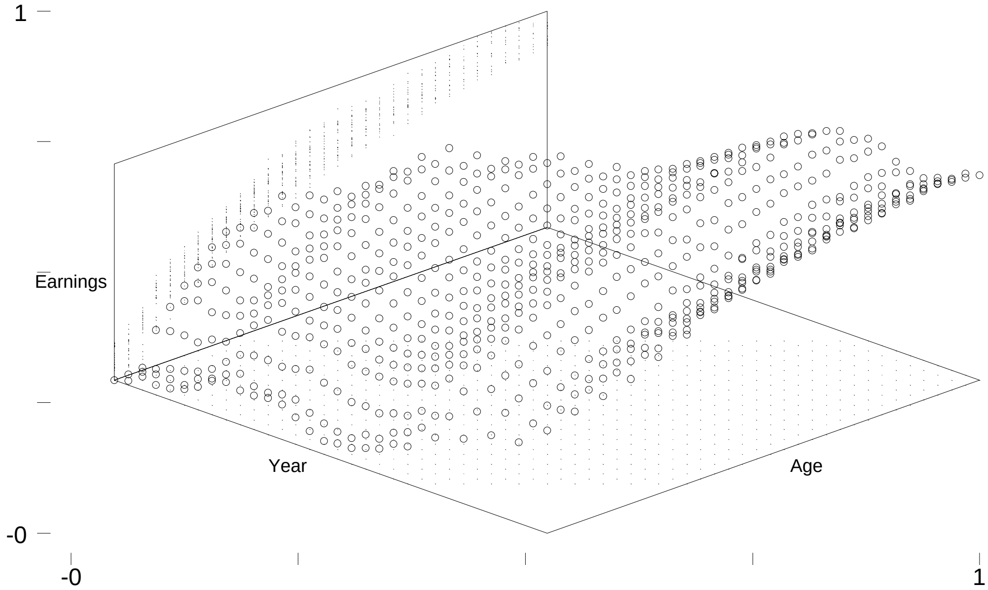

Figure 2: Age-year-income profile for men. The Age axis starts from the left where age = 19, the Year axis also starts from the left where year = 1968. Along the year-axis we can follow the time effects for each age, for example how the income of nineteen year olds has changed over time. Along the age-axis we can see the average income for different age groups a given year, i.e. the cross-sectional age income profile. To see the cohort effects we follow the diagonal. For example, to follow the 1941 birth cohort we start in the left corner, where year = 1968 and age = 19, and move towards the right corner where year = 1999 and age = 50. (Note that we follow the 1941 cohort only until 1991.)

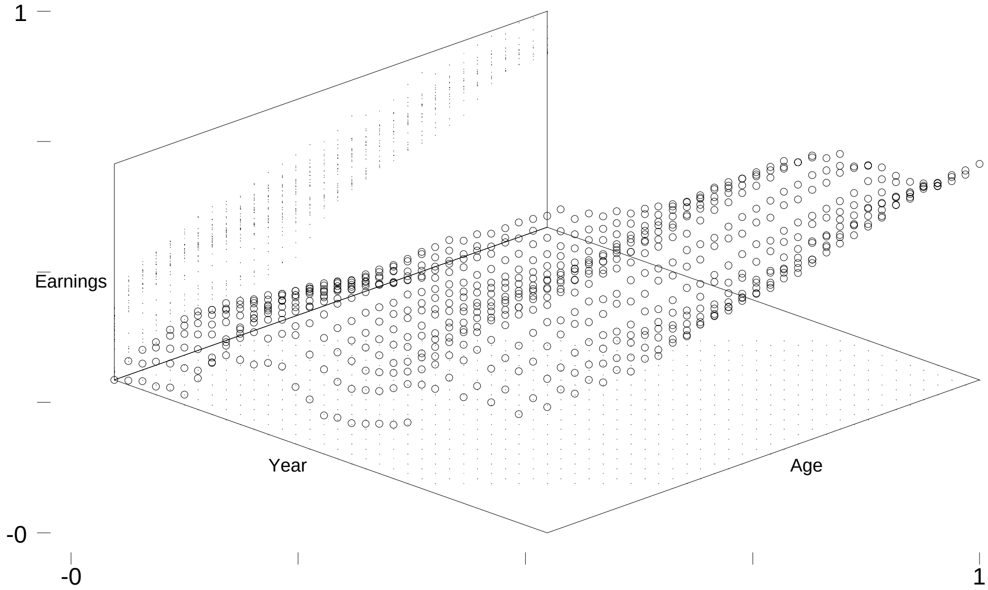

Figure 3: Age-year-earnings profile for women. See Figure 2 for explanation of the axes.

The reason for using a cohort size measure relative to the population and not to the labour force, as Berger (1989) and Welch (1979) do, is also to avoid potential endogeneity. The size of the labour force depends on the participation rate, which can be affected by earnings levels, economic fluctuations and hidden unemployment. Therefore, a cohort size measure relative to the labour force would be affected both by the dependent variable and the time effects. Both birth rates and normalisation by population is commonly used in more recent studies, e.g. Korenman & Neumark (2000) and Shimer (2001).

The position in the demographic cycle is measured in terms of the difference of the cohort size variable. That is, for a cohort born in the upswing of a demographic cycle the variable is positive, and for a cohort born in the downswing it is negative.

4.2.2 Period effects

To control for time effects we use both time dummies and macroeconomic variables. Our macroeconomic variables, GDP per capita, unemployment and net migration correspond to time controls commonly used in the literature. GDP per capita controls for the business cycle and net migration controls for the net inflow of working age migrants. In the Swedish context, unemployment is not correlated with GDP per capita for most of the period studied. The level of unemployment was relatively constant at a low level until the 1990s.

4.2.3 The regression equations

Our aim is to estimate cohort effects on both the earnings level and the returns to experience, i.e. both the level and the slope of the earnings profile. We do this in two different regression equations, one for the level effects and the other for the effects on the slope. Both equations are estimated on the four levels of education separately, 8-11 years, 12 years, 13-14 years and 15 and more years of education. The superscripts 1 and 2 refer to equation (1) and (2). The first equation is

$$\ln y_{it} = c^1 + \sum_{s=1} \alpha_s^1 E_{sit} + \beta_1^1 C_i + \beta_2^1 \Delta C_i + \rho_k^1 T_t + \varepsilon_{it}^1 \qquad (1)$$

where c^1 is a constant, E stands for experience, C for cohort size, ΔC for change in cohort size, T for time controls, $s = 1, ., 3$ denote experience splines, and ε_{it}^1 denotes the residuals. Equation (1) is estimated with pooled OLS, with both year dummies and macro variables as period controls. We use the OLS with year dummies as a benchmark regarding the time controls in order to assess whether the macro variables capture the time effects adequately. It would be of interest to test whether there are individual specific characteristics that we have not taken into account. However, since the cohort size variables are fixed over time it is not possible to estimate equation (1) with fixed effects. This implies that our estimates might be biased due to the omission of relevant individual specific variables.

In the equation (2) we estimate the effects on the return to experience, by interacting cohort size and change in cohort size with the splined experience.

$$\ln y_{it} = c^2 + \sum_{s=1}^{3} \alpha_s^2 E_{sit} + \sum_{s=1}^{3} \gamma_s E_{sit} C_{it} + \sum_{s=1}^{3} \delta_s E_{sit} \Delta C_{it} + \rho_k^2 T_t + \varepsilon_{it}^2 \quad (2)$$

The return to experience in equation (2) is given by

$$\mathrm{Re}(E)_{si} = \alpha_s^2 + \gamma_s C_i + \delta_s \Delta C_i \quad (3)$$

Equation (2) is estimated both with fixed effects and pooled OLS using macro variables. OLS estimation does not consider the panel structure of the data, which fixed or random effects estimation do. If the random effects specification is correct, both fixed effects and OLS are still consistent but not efficient. However, if the fixed effects specification is correct, that is, if there are individual specific effects that are correlated with the explanatory variables, both OLS and random effects are biased. Fixed effects estimation is however always consistent. Is there any reason to expect fixed effects? There are several factors, such as ability, region of birth and which sector individuals work in which could be referred to as fixed effects. Ability could be correlated with schooling and therefore experience. Both region of birth and sector could be correlated with cohort size and the period effects, as mentioned earlier.

If OLS and fixed effects estimations give different results, we conclude that the fixed effects estimation is the correct one. If there are significant fixed effects, it is possible that they include cohort effects. However, those cohort effects are time-constant, and the aim of equation (2) is to identify the time-varying cohort effects. Therefore, it is important to control for the time-constant cohort effects, which we do in the fixed effects estimation. One could argue that we instead should estimate a regression equation with both cohort size and change in cohort size together with the interaction terms. However, it is not possible to estimate that specification with fixed effects since the cohort size variables are fixed over time. Therefore we cannot take into account the fixed effects, which we claim are of interest. In other words, even if we were to estimate this specification with OLS, we cannot test if the estimates are unbiased.

Due to our specifications we are not able to estimate the initial cohort effects on earnings, that is, whether the earnings during the first years on the labour market are higher or lower for a large cohort compared with a small cohort. In equation (1), we estimate the average effect on the overall lifetime earnings, corresponding to a parallel shift in the profile for different cohorts. In equation (2), the OLS estimations give all cohorts a joint intercept, which implies that we identify the cohort effects only on the slope and not on the level. The fixed effects estimations do not give all cohorts the same intercept. Instead, every individual will have its own intercept, which will include not only the cohort effects but also the effects specific to each individual. Even if it would be possible to use specifications that enables us to estimate the initial effects, there are some limitations in the data. The oldest cohort in our sample is first observed at the age of 27 and probably entered the labour market much earlier, at least those who are low educated. By using the estimates from both equations, we will try to find out what the qualitative initial effect is.

4.2.4 The unbalanced data

We have an unbalanced data set in several respects. As mentioned earlier some individuals are followed from an earlier age, whereas others are followed to an older age. This gives an unbalance to the extent that some cohorts are not present in some of the years we observe.

This imbalance appears in the equations we have specified. In the first spline, which refers to the first 11 years on the labour market, cohorts born in the 1940s would be underrepresented especially in the lower education levels.The reason is that when we start observing individuals in 1968, cohorts born between 1941-1949 are observed at ages ranging between 27 for the oldest cohort and 19 for the youngest cohort. Cohorts born after 1949 are observed from the age of 19. Further, the low-educated have attained more experience and are thus even more underrepresented in the first experience phase. The second experience group is the most balanced in terms of birth cohorts. The third experience group is unbalanced since those born in the 1950s, as opposed to those born in the 1940's, do not reach 50 years of age. Their last observation ranges between the ages 39-49, meaning that they are underrepresented in the third experience group, especially in the higher education levels.

If some cohorts were, due to the imbalance in our sample, not observed during a period in their working life when they for example have a high earnings growth, the estimated slope of the earnings profile for this experience range would be underestimated.

5 Results

The estimation results are presented in table 1-8. Equation (1) estimated with OLS with dummy variables is presented in table 1 and 2, for men and women respectively for different levels of education. Table 3 and 4 present the OLS estimates with macro variables. The OLS estimations of equation (2) are presented in table 4 and 5, whereas the fixed effects estimates are presented in tables 6 and 7.

This result section is organised as follows. The effects of cohort size and position in the cycle on the earnings profiles are discussed in separate sections. Each section starts by looking at the cohort effect on the earnings level, based on estimates of equation (1) presented in tables 1-4. Then follows a presentation of the effects on the slope of the earnings profile based on the estimations of equation (2) presented in tables 6-8. Finally, some concluding implications on the overall shape of the earnings profile are drawn.

But first, some words on the period effects followed by a discussion on the adequacy of OLS vs FE estimations. The results of the two estimations are relatively consistent for both genders.[3]

[3] We have estimated model 2 with OLS with period dummies as well. The results for men are very much the same as in the OLS model with macro variables. For women, the results exhibit some discrepancies. It could be that our macro variables do not capture the time effects in a proper way for women. Since the literature in this area focuses exclusively on men, we have based our choice of macro variables on men. Which macro variables could be relevant for women? Variables we have thought of are women's share

5.1 Period effects

When comparing the macro variables and dummy variables estimates of equation (1), in tables 1-4, we conclude that our main results are robust to the choice of time controls. Having time dummies or macroeconomic variables as time controls does not alter the results. Even though R-square is higher in the dummy variable specification we can be confident that the macro variables pick up time variations satisfactorily. (Graphs on estimated Y vs. actual Y are presented in Appendix B.)

GDP per capita has the expected positive effect on earnings. Unemployment's positive effect on earnings could be due to the special Swedish context. The level of unemployment was relatively constant at a low level until it increased drastically in the early 90s and settled at a higher level, during which time we experienced an overall increase in the real wage level. At first glance, it may be surprising with a positive effect of migration on earnings for men. However, migration is likely to increase at a time when the demand for labour, and consequently also the wage level, is high.[4] The only time migration has a negative impact on earnings is for low educated women. This may be an indication that low educated women are close substitutes to new immigrants on the labour market.

Before examining cohort effects let us start by considering other implications of these estimates. As can be seen in the `Experience 0-11` coefficient for men, there is evidence that early career wage growth is more rapid for those with more schooling, in line with what is common in this kind of analysis. The overall shape of the earnings profile is in line with a hump shaped earnings profile for men. For women the same tendency of higher slope for higher education levels does not appear. The overall shape of their earnings profile is not consistent with the common hump shaped profile. Only in the dummy variable specification for the highest and the lowest education groups does a hump shaped profile appear. Whereas the second and third education levels have a stagnating slope in the second experience phase, followed by a steep slope in the third experience phase. The stagnating slope in the middle could depend on an interruption in working life due to child care.

of the labour force, and the size of the public sector. However, these variables are more or less time trends, which induce multicollinearity problems in our framework.

[4]It would naturally have been optimal to instrument this variable in order to avoid the risk of reverse causation. However, we choose not to do this since there is a lack of relevant instrument variables and since we are only interested in controlling for migration.

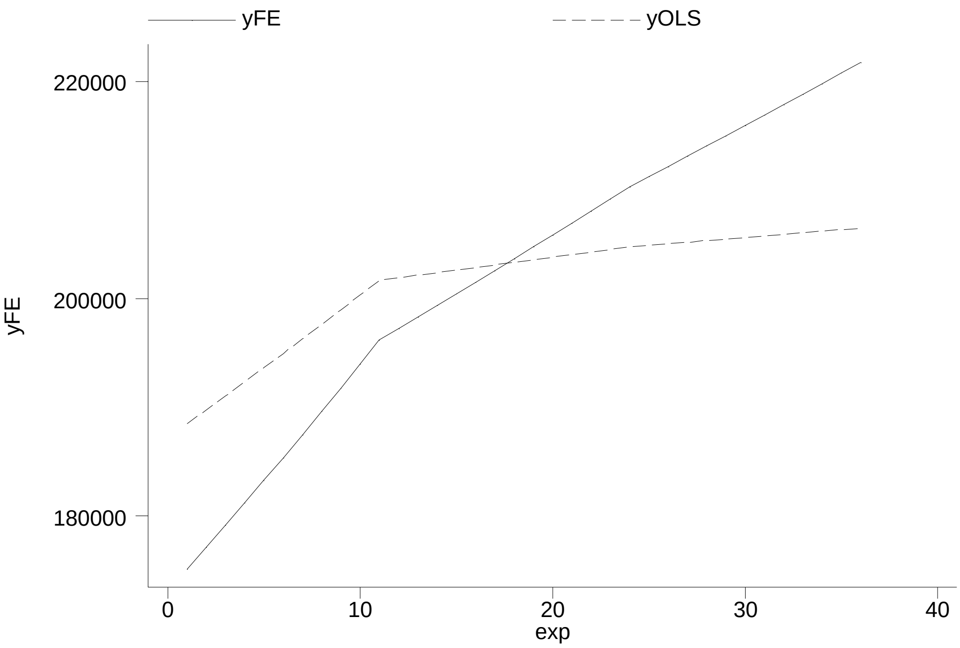

Figure 4: Simulated profiles for men with 8-11 years of education. The simulations are based on equation (2) and the estimations with OLS with macro variables and fixed effects respectively. Cohort size and the period controls are assumed to be constant on their median values.

5.2 OLS vs fixed effects

The hypothesis that all individual effects are equal can be rejected for both men and women. The p-values of this F-test are very low, $p = 0.0000$. However, hypothesis tests tend to become significant when the sample is large, as is our case. To get a better picture of the differences, or similarities, between our OLS and fixed effects estimates, we have simulated earnings profiles based on the two estimations, presented in Figure 3. We have assumed the time controls to be constant on their median value.

In these simulated profiles we can see that OLS predicts a much flatter profile than the fixed effects estimates.[5] These simulations confirm the result of the hypothesis test, implying that the OLS estimates are biased in equation

[5] The slope of the OLS profiles are flatter in the other education levels as well.

(2), and that there are individual specific characteristics that need to be considered.

Regarding the macro variables in equation (2), the signs of the coefficients are the same in the fixed effects estimation as in the OLS, except for unemployment. One possible explanation could be that the effects of unemployment varies over individuals. Since OLS does not take into account individual specific effects it gives biased results, and in this case even the wrong sign.

5.3 Cohort size effects

5.3.1 Effects on the overall earnings level

In estimating equation (1) we identify cohort effects on the overall earnings level, i.e. the average effect throughout working life.

Cohort size has a significant positive effect on the level of earnings irrespective of education level and sex, as shown in Table 1 & 2. This implies that belonging to a large cohort has a significant positive effect on the individuals' average life-time earnings.

For both men and women, the highest education group is the most positively affected of belonging to large cohorts, although there is no clear tendency that the effect increases with level of education.

5.3.2 Effects on the return to experience

The analysis of cohort effects on return to experience is based on the results from the fixed effects estimation. For men and women in their first experience phase, i.e. the first 11 years on the labour market, large cohorts with up to 12 years of education have higher returns to experience than small cohorts. When looking at the results for high educated in this experience phase, those in large cohorts have flatter earnings profiles than small cohorts. For men, the effect increases with education level. For women, the effect is strongest for those with the lowest level of education followed by the highest education level.

For both men and women in their second experience phase, large cohorts get lower returns to experience than small cohorts. Men in all education levels are affected equally strong, while women in the first and second education level are affected the most.

The results for those in their third experience phase turn out to be positive except for women with 13-14 years of education. The effect increases with education level.

5.3.3 The overall effects of cohort size

Combining the information in equation (1) and equation (2) we can conclude that for the two lowest education groups, large cohorts have both steeper earnings profiles in the first and third experience phase, and have an overall higher earnings than their small cohort counterparts. It is however not clear if they start out at a lower initial earnings and catch up later in their working life, or if they start out at a higher initial earnings and are advantaged throughout their entire working life. An interesting comparison is the study of Tasiran et al (1992) who studied shop assistants and salesman in Sweden over the period 1948 to 1989. This occupational group can be compared to our lower education groups. They found a clear negative effect of cohort size on the level of earnings for the youngest ages and this effect was similar in magnitude among genders. The effect disappeared for older workers translating into a steeper earnings profile for this occupational group. We find a positive overall effect for all age groups and a steeper earnings profile for the first experience phase. Our results do not rule out the possibility of a lower initial earnings for these education groups.

Large cohorts belonging to the two highest education groups also have an overall higher level of earnings but they have flatter earnings profiles in the two first experience phases, followed by a steeper earnings profile in the third experience phase. We can almost certainly claim that large cohorts in these two education groups enter the labour market with higher initial earnings than smaller cohorts, that is unless the steeper earnings profile in the third experience phase accounts for the higher overall earnings. But this seems unreasonable since it would require a huge catch-up in the third experience phase. For men, the fact that the highest education group has the highest positive cohort size effect in the overall earnings level, as well as the strongest negative effect on the slope in the first experience phase further strengthens our claim that large cohorts in these two education groups enter the labour market with higher initial earnings than smaller cohorts. The flatter profiles for larger cohorts belonging to higher education groups is in line with the conclusion drawn from Jonsson et al (1978) study on how engineers are affected by cohort size in Sweden.

The positive effect on the overall earnings level of belonging to a large cohort is not, per se, a falsification of Easterlin's cohort crowding hypothesis. As mentioned earlier, Easterlin anticipates lower earnings for large young cohorts relative to that of older cohorts. Our conclusions are based on the overall level effects on earnings over individuals' working life. Our results differ from Welch (1979) who finds negative overall effects of cohort size on the level of earnings of belonging to large cohorts, increasing with level of education. He also finds stronger entry effects than persistent effects for all but the first education level, translating into steeper profiles if one belongs to large cohorts. Our results do claim initially steeper earnings profile for large cohorts belonging to the two lowest education groups. But we find evidence of an overall positive effect of cohort size which is inconsistent with Welch's results. Furthermore we can not confirm Welch's (1979) results that higher education groups are more negatively affected of belonging to large cohorts. In contrast to Welch, we claim that when it comes to the overall earnings level, regardless of sex, individuals belonging to the highest education group are the most positively affected of belonging to large cohorts.

For those in the two highest education levels, our results are in line with Berger's (1989) findings. When controlling for the size of surrounding cohorts he found a positive cohort size effect on the level of earnings and a negative effect on the return to experience. Berger looked at the first 15 experience years that corresponds to our first experience phase 0-11 years plus the first years of the second phase. In other words, our results regarding the third phase are not comparable to Berger's results. He finds his results support the theories by Stapleton and Young (1988) and Nothaft (1985), that large cohorts invest less in post-schooling human capital and thereby get higher initial earnings and flatter profiles. We find flatter profiles for high educated in their first two phases and higher initial earnings, which could be an indication of less post-schooling human capital investments. However, we also find that large cohorts have higher lifetime earnings, which is more associated with higher human capital investments, than is the case for small cohorts. For low educated, our results suggest more post-schooling human capital investments for those in large cohorts.

5.4 Effects of position in the demographic cycle

5.4.1 Effects on the overall earnings level

The position in the demographic cycle, captured by change in cohort size, shows that being born in the leading edge of a boom has a positive effect on the level of one's earnings irrespective of education level and sex, as can be seen in table 1 and 2. For both men and women the magnitude of the effect is strongest for the highest education group, i.e. high educated are more strongly affected of belonging to the lagging edge than low educated.

5.4.2 Effects on the return to experience

Based on the fixed effects estimations of equation (2), presented in tables 7 and 8, the effects of change in cohort size during the first experience phase imply that those born in the upswing of a demographic cycle have higher returns to experience than those born in a downswing. This supports the view that in the beginning of one's working life, one is sensitive to the competition of older cohorts who have managed to gain a few more years of experience. This holds irrespective of education level. However, for women with least education the effect is not significant. For men, the strongest effects are found in the second education level, while women in the two highest education levels are most affected.

Our results indicate that during the second experience phase it is better for men to follow a large older cohort than to precede a large younger cohort, i.e. those born in the leading edge have flatter profiles compared to those born in the lagging edge. This result is also true for women in the two highest education levels. This disadvantage of leading edge cohorts could imply that, during this phase, individuals compete to a larger extent with younger cohorts than older cohorts. The effect for men in the lowest education level is insignificant, indicating that the lowest educated have a higher substitutability across experience groups and are thus substitutable not only with the immediately surrounding cohorts. For women in the two lowest education groups, the effect is different. Those born in the leading edge have steeper profiles than those born in the lagging edge, as was the case in the first experience phase.

During the third phase there is a clear difference between education levels. The high educated are advantaged of belonging to the leading edge, that is having relatively few older cohorts in front of them, indicating that they compete more with older cohorts than younger cohorts. The lower educated groups are disadvantaged of belonging to the leading edge.

5.4.3 The overall effects of position in the demographic cycle

Combining the results of equation (2) with that of equation (1), that lifetime earnings are on average higher for those born in the leading edge than for those born in the lagging edge, it is hard to determine what the effect of a change in cohort size is on the initial level of earnings. The high educated groups start with steeper profiles for leading edge cohorts, followed by flatter in the second phase and steeper again in the third phase. This implies that they can start either on a higher or lower level. In the same way it is hard to identify the initial effect in the low educated groups.

Berger (1989) controls for the size of surrounding cohorts, which is basically the same as what we do. According to his results men born in the leading edge have higher earnings than those born in the lagging edge. Furthermore, the leading edge cohorts have steeper profiles than lagging edge cohorts. Since his results apply for men with up to 15 years of experience, his results are mainly comparable to our results for those in the first experience phase. It would have been interesting to know what his results would have been if individuals with more experience had been included. We can however conclude that our results are in line with Berger's. It should be noted that Berger focuses on the size of the surrounding cohorts and not whether one belongs to the leading or lagging edge.

Macunovich (1999) controls for change in cohort size with both the first and second difference in cohort size. She finds that those born in the leading edge are better off than those born in the lagging edge, which is in line with our results from equation (1).

6 Conclusions

The aim of this study is to identify cohort effects on individual earnings profiles in the Swedish context.

Our findings regarding effects on earnings levels are straightforward. Regardless of gender and education level, belonging to a large cohort and belonging to a cohort born in the upswing of a demographic cycle have a significant positive effect on individuals' overall earnings level. However, this is not a falsification of the hypotheses by Easterlin. As we explained earlier, his theories concern the cross-sectional earnings profile, while we look at the longitudinal profiles. The fact that we find that large cohorts have on average higher real earnings, does not rule out the possibility that they started their careers with lower earnings relative to that of prime aged workers. Our results support the results by Berger, that when controlling for the effect on the slope the effect on the level is positive, even though the level effect in Berger's study only apply for those with up to 15 years of experience.

Regarding the effect of cohort size on the slope of the earnings profile, the effects differ between lower and higher education levels during the initial 11 years of labour market experience. Low educated have a steeper earnings profile if they belong to large cohorts, as opposed to high educated who have a flatter earnings profile. The effect of cohort size on return to experience turns negative during the second experience phase, irrespective of gender or education level, and is positive during the third experience phase.

The position of one's cohort in the demographic cycle also has effects on the slope of ones earnings profile. Irrespective of gender and education level, those born in an upswing of a demographic cycle have higher returns to experience during their first 11 years on the labour market.

For the second and third phases, the effect of one's position in the demographic cycle varies across education levels. High educated men belonging to the leading edge are disadvantaged during the second phase, while they are better off in the third phase compared to their counterparts born in the lagging edge. Men in the remaining three education levels belonging to the leading edge are disadvantaged both during the second and the third phase. The same result holds for women during the third phase, whereas during the second experience phase, women in the two lowest education levels are better off if they belong to the leading edge.

Despite the fact that women's earnings profiles differ from the traditional expected hump-shaped profiles, which we find for men in the equation (1) estimation, we have found little differences in cohort effects across gender. This is surprising given that men and women face different labour markets since women are to a much larger extent represented in the public sector. The difference between genders in the overall hump shape could be due to interruption in the career due to childbearing years.

Our main findings are to a large extent in line with that of Berger (1989). However, they are not consistent with his human capital interpretations, since we cannot find convincing evidence in our results that support the theories by Stapleton and Young that large cohorts invest less in human capital. The question is if the cohort effects on earnings profiles are mainly channelled through human capital investments, or if there are other ways that cohort size and position in the demographic cycle affect labour market outcomes. For example, it is possible that the positive effects on the earnings level we find are due to positive cohort size effects on aggregate demand, which have positive effects on labour demand. We do not know much about these kinds of mechanisms, probably because it would be very difficult to isolate the effects. The only study on this we have come across is Macunovich (1999), who tries to separate the cohort effects on aggregate demand and labour supply with two different cohort size variables, in a large growth model. How the two measures separate between demand and supply is not clear. However, there is no doubt that demographic fluctuations must have some impact on both aggregate and labour demand.

More research is also needed regarding the demographic effects on human capital attainment, both on the choice of formal education and the choice of post-schooling human capital investments. Instead of making human capital interpretations based on studies on earnings profiles, it should be more informative to study the demographic effects directly on human capital investments or educational choice.

The results in this paper can only speak for the 1940s and the 1950s Baby Boom and Bust generation's experience on the labour market. A natural question is whether our results can give an indication of how the coming Baby Boom generations of the 1960's and 1990's are to be affected by their cohort size and position in the Boom. It would be naïve to assume that the effects would be necessarily similar since the structure of the labour market changes over generations. However, given that men and women have similar cohort effects despite the different situation they face on the labour market, this could be an indication that changes in the labour market have little effect on cohort effects and that our results could apply even to other baby boom generations.

It is interesting to note that we in accordance with American literature find stable and significant cohort effects on earnings profiles, considering the differences between the American and the Swedish context. Both the fact that the labour market has been more regulated in Sweden and that the Baby Boom was much smaller in magnitude and short-lived would lead us to expect a limited or different role to be played by cohort effects on individual earnings profiles in Sweden as compared to the American case.

Klevmarken (1993), one of the few Swedish studies that has not limited itself to study a specific occupation group, also did not find evidence of cohort effects on earnings profiles. However, his results are not directly comparable with ours since he controlled for labour demand effects and estimated his regression equations on the entire labour force, while we divided the sample into education/experience categories. There are differences in cohort effects between these categories, which could be one of the reasons why we find significant effects. Another reason is most likely the quality of the data. Our data is much larger both in the cross-section and time-series dimension.

Table 1: OLS equation (1), with time dummies for MEN
Dependent variable: Log annual earnings

Variable	**8-11 years**	**12 years**	**13-14 years**	**15+ years**
Experience 0-11	0.0094**	0.0249**	0.0267**	0.0305**
	(0.0003)	(0.0006)	(0.0005)	(0.0006)
Experience 12-24	0.0037**	0.0156**	0.0111**	0.0057**
	(0.0001)	(0.0003)	(0.0004)	(0.0004)
Experience 25+	-0.0041**	0.0039**	-0.0022†	-0.0178**
	(0.0002)	(0.0006)	(0.0012)	(0.0016)
Cohort Size (C)	0.0089**	0.0014*	0.0013	0.0136**
	(0.0002)	(0.0006)	(0.0008)	(0.0008)
Change in C	0.0114**	0.0169**	0.0145**	0.0247**
	(0.0004)	(0.0010)	(0.0013)	(0.0014)
Intercept	11.9045**	11.9658**	12.0216**	11.9398**
	(0.0041)	(0.0098)	(0.0125)	(0.0136)
N	426431	119060	86600	112188
R^2	0.142	0.1976	0.2464	0.1933

Significance levels : † : 10% ∗ : 5% ∗∗ : 1% Standard errors in parentheses

Table 2: OLS equation (1), with time dummies for WOMEN
Dependent variable: Log annual earnings

Variable	**8-11 years**	**12 years**	**13-14 years**	**15+ years**
Experience 0-11	0.0084**	0.0140**	0.0086**	0.0123**
	(0.0002)	(0.0007)	(0.0004)	(0.0005)
Experience 12-24	0.0031**	0.0042**	0.0060**	0.0032**
	(0.0001)	(0.0005)	(0.0003)	(0.0004)
Experience 25+	0.0010**	0.0047**	0.0067**	0.0022†
	(0.0002)	(0.0009)	(0.0008)	(0.0012)
Cohort Size (C)	0.0023**	0.0051**	0.0020**	0.0092**
	(0.0003)	(0.0010)	(0.0006)	(0.0007)
Change in C	0.0052**	0.0091**	0.0042**	0.0226**
	(0.0005)	(0.0015)	(0.0010)	(0.0012)
Intercept	11.8747**	11.8438**	11.9583**	11.9164**
	(0.0043)	(0.0143)	(0.0093)	(0.0112)
N	236869	42595	87289	90286
R^2	0.1711	0.2	0.1947	0.1622

Significance levels : † : 10% $*$: 5% $**$: 1% Standard errors in parentheses

Table 3: OLS equation (1), with macro variables for MEN
Dependent variable: Log annual earnings

Variable	**8-11 years**	**12 years**	**13-14 years**	**15+ years**
Experience 0-11	0.0070**	0.0221**	0.0220**	0.0227**
	(0.0003)	(0.0005)	(0.0005)	(0.0006)
Experience 12-24	0.0028**	0.0140**	0.0121**	0.0071**
	(0.0001)	(0.0003)	(0.0004)	(0.0004)
Experience 25+	-0.0019**	0.0097**	0.0025*	-0.0102**
	(0.0002)	(0.0006)	(0.0012)	(0.0016)
Cohort Size (C)	0.0098**	0.0021**	0.0030**	0.0137**
	(0.0002)	(0.0006)	(0.0008)	(0.0008)
Change in C	0.0122**	0.0178**	0.0169**	0.0313**
	(0.0004)	(0.0010)	(0.0013)	(0.0014)
Unemployment	0.0064**	-0.0012*	0.0058**	0.0172**
	(0.0002)	(0.0005)	(0.0006)	(0.0006)
Net migration	0.0075**	0.0062**	0.0060**	0.0057**
	(0.0002)	(0.0004)	(0.0005)	(0.0006)
GDP/capita	0.0024**	0.0006**	0.0018**	0.0035**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Intercept	11.4789**	11.8639**	11.6897**	11.3522**
	(0.0082)	(0.0213)	(0.0286)	(0.0289)
N	426431	119060	86600	112188
R^2	0.1121	0.1834	0.2253	0.1714

Significance levels : † : 10% $*$: 5% $**$: 1% Standard errors in parentheses

Table 4: OLS equation (1), with macro variables for WOMEN
Dependent variable: Log annual earnings

Variable	**8-11 years**	**12 years**	**13-14 years**	**15+ years**
Experience 0-11	0.0047**	0.0103**	0.0033**	0.0059**
	(0.0002)	(0.0007)	(0.0004)	(0.0005)
Experience 12-24	0.0030**	0.0051**	0.0076**	0.0043**
	(0.0001)	(0.0005)	(0.0003)	(0.0004)
Experience 25+	0.0032**	0.0094**	0.0117**	0.0101**
	(0.0002)	(0.0009)	(0.0008)	(0.0013)
Cohort Size (C)	0.0006*	0.0034**	0.0014*	0.0089**
	(0.0003)	(0.0009)	(0.0006)	(0.0007)
Change in C	0.0035**	0.0067**	0.0033**	0.0245**
	(0.0005)	(0.0016)	(0.0010)	(0.0012)
Unemployment	0.0062**	0.0061**	0.0043**	0.0101**
	(0.0002)	(0.0007)	(0.0004)	(0.0005)
Net migration	-0.0014**	0.0002	-0.0007†	0.0018**
	(0.0002)	(0.0006)	(0.0004)	(0.0005)
GDP/capita	0.0017**	0.0020**	0.0016**	0.0027**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Intercept	11.5948**	11.5253**	11.6896**	11.4812**
	(0.0093)	(0.0334)	(0.0216)	(0.0250)
N	236869	42595	87289	90286
R^2	0.127	0.1724	0.1525	0.1293

Significance levels : † : 10% ∗ : 5% ∗∗ : 1% Standard errors in parentheses

Table 5: OLS equation (2), with macro variables for MEN
Dependent variable: Log annual earnings

Variable	8-11 years	12 years	13-14 years	15+ years
Experience 0-11	-0.0430**	-0.0051**	0.0065**	-0.0043*
	(0.0007)	(0.0016)	(0.0020)	(0.0021)
Experience 12-24	0.0531**	0.0585**	0.0575**	0.0630**
	(0.0007)	(0.0019)	(0.0023)	(0.0027)
Experience 25+	-0.0122**	-0.0385**	-0.0524**	-0.0638**
	(0.0015)	(0.0064)	(0.0136)	(0.0198)
Interaction between cohort size and experience				
$C * E$ 0-11	0.0029**	0.0015**	0.0010**	0.0017**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
$C * E$ 12-24	-0.0031**	-0.0026**	-0.0026**	-0.0031**
	(0.0000)	(0.0001)	(0.0001)	(0.0002)
$C * E$ 25+	0.0008**	0.0029**	0.0036**	0.0038**
	(0.0001)	(0.0004)	(0.0008)	(0.0011)
Interaction between change in cohort size and experience				
$\Delta C * E$ 0-11	0.0019**	0.0031**	0.0040**	0.0062**
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
$\Delta C * E$ 12-24	0.0000	-0.0014**	-0.0035**	-0.0064**
	(0.0001)	(0.0002)	(0.0003)	(0.0003)
$\Delta C * E$ 25+	-0.0030**	-0.0031**	-0.0018	0.0024
	(0.0002)	(0.0006)	(0.0012)	(0.0016)
Unemployment	0.0017**	-0.0066**	-0.0003	0.0090**
	(0.0002)	(0.0006)	(0.0007)	(0.0007)
Net migration	0.0079**	0.0073**	0.0083**	0.0096**
	(0.0002)	(0.0005)	(0.0006)	(0.0007)
GDP/capita	0.0031**	0.0012**	0.0018**	0.0030**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Intercept	11.5616**	11.8317**	11.7541**	11.6730**
	(0.0055)	(0.0139)	(0.0172)	(0.0183)
N	426431	119060	86600	112188
R^2	0.1251	0.1892	0.2319	0.1759

Significance levels : † : 10% ∗ : 5% ∗∗ : 1% Standard errors in parentheses

Table 6: OLS equation (2), with macro variables for WOMEN
Dependent variable: Log annual earnings

Variable	8-11 years	12 years	13-14 years	15+ years
experience 0-11	-0.0284**	-0.0261**	-0.0190**	-0.0135**
	(0.0008)	(0.0025)	(0.0016)	(0.0019)
experience 12-24	0.0392**	0.0523**	0.0494**	0.0393**
	(0.0008)	(0.0025)	(0.0017)	(0.0022)
experience 25+	-0.0030†	-0.0281**	0.0076	0.0146
	(0.0017)	(0.0087)	(0.0091)	(0.0152)
Interaction between cohort size and experience				
$C * E$ 0-11	0.0019**	0.0021**	0.0013**	0.0013**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
$C * E$ 12-24	-0.0023**	-0.0029**	-0.0025**	-0.0020**
	(0.0000)	(0.0002)	(0.0001)	(0.0001)
$C * E$ 25+	0.0004**	0.0023**	0.0007	0.0003
	(0.0001)	(0.0005)	(0.0005)	(0.0008)
Interaction between change in cohort size and experience				
$\Delta C * E$ 0-11	0.0007**	0.0011**	0.0025**	0.0049**
	(0.0001)	(0.0003)	(0.0002)	(0.0002)
$\Delta C * E$ 12-24	0.0007**	0.0004	-0.0026**	-0.0042**
	(0.0001)	(0.0004)	(0.0002)	(0.0003)
$\Delta C * E$ 25+	-0.0036**	-0.0049**	-0.0019*	0.0006
	(0.0002)	(0.0009)	(0.0008)	(0.0013)
Unemployment	0.0039**	0.0024**	0.0011*	0.0058**
	(0.0002)	(0.0008)	(0.0005)	(0.0006)
Net migration	0.0004†	0.0031**	0.0026**	0.0049**
	(0.0002)	(0.0006)	(0.0004)	(0.0005)
GDP/capita	0.0024**	0.0026**	0.0019**	0.0024**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Intercept	11.5178**	11.4942**	11.6784**	11.6735**
	(0.0063)	(0.0207)	(0.0131)	(0.0155)
N	236869	42595	87289	90286
R^2	0.1379	0.1806	0.1636	0.1335

Significance levels : † : 10% $*$: 5% $**$: 1% Standard errors in parentheses

Table 7: FE equation (2), for MEN
Dependent variable: Log annual earnings

Variable	**8-11 years**	**12 years**	**13-14 years**	**15+ years**
experience 0-11	-0.0111**	0.0063*	0.0656**	0.0891**
	(0.0016)	(0.0028)	(0.0027)	(0.0029)
experience 12-24	0.0492**	0.0662**	0.0706**	0.0610**
	(0.0007)	(0.0016)	(0.0020)	(0.0021)
experience 25+	-0.0157**	-0.0371**	-0.0431**	-0.1226**
	(0.0012)	(0.0044)	(0.0089)	(0.0121)
Interaction between cohort size and experience				
$C * E$ 0-11	0.0013**	0.0012**	-0.0020**	-0.0034**
	(0.0001)	(0.0002)	(0.0001)	(0.0002)
$C * E$ 12-24	-0.0026**	-0.0028**	-0.0027**	-0.0025**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
$C * E$ 25+	0.0012**	0.0030**	0.0036**	0.0073**
	(0.0001)	(0.0002)	(0.0005)	(0.0007)
Interaction between change in cohort size and experience				
$\Delta C * E$ 0-11	0.0047**	0.0108**	0.0087**	0.0031**
	(0.0004)	(0.0005)	(0.0005)	(0.0004)
$\Delta C * E$ 12-24	0.0000	-0.0021**	-0.0045**	-0.0063**
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
$\Delta C * E$ 25+	-0.0034**	-0.0033**	-0.0014†	0.0026**
	(0.0001)	(0.0004)	(0.0008)	(0.0010)
Unemployment	-0.0038**	-0.0123**	-0.0136**	0.0004
	(0.0004)	(0.0008)	(0.0010)	(0.0010)
Net migration	0.0067**	0.0075**	0.0065**	0.0068**
	(0.0001)	(0.0003)	(0.0004)	(0.0004)
GDP/capita	0.0024**	0.0003	-0.0005†	0.0021**
	(0.0001)	(0.0002)	(0.0003)	(0.0003)
Intercept	11.6264**	11.9118**	12.0820**	11.7642**
	(0.0126)	(0.0290)	(0.0388)	(0.0408)
Nbr of obs	426431	119060	86600	112188
Nbr of groups	22569	5855	4414	5921
R^2	0.2563	0.3605	0.4644	0.4381

Significance levels : † : 10% ∗ : 5% ∗∗ : 1% Standard errors in parentheses

Table 8: FE equation (2), for WOMEN
Dependent variable: Log annual earnings

Variable	8-11 years	12 years	13-14 years	15+ years
experience 0-11	-0.0542**	-0.0214**	0.0242**	0.0512**
	(0.0018)	(0.0039)	(0.0023)	(0.0025)
experience 12-24	0.0628**	0.0704**	0.0610**	0.0409**
	(0.0009)	(0.0026)	(0.0018)	(0.0020)
experience 25+	0.0154**	0.0078	0.0255**	-0.0091
	(0.0015)	(0.0067)	(0.0071)	(0.0103)
Interaction between cohort size and experience				
$C * E$ 0-11	0.0041**	0.0021**	-0.0011**	-0.0027**
	(0.0001)	(0.0002)	(0.0001)	(0.0001)
$C * E$ 12-24	-0.0029**	-0.0033**	-0.0025**	-0.0015**
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
$C * E$ 25+	0.0004**	0.0011**	0.0003	0.0019**
	(0.0001)	(0.0004)	(0.0004)	(0.0006)
Interaction between change in cohort size and experience				
$\Delta C * E$ 0-11	0.0005	0.0021*	0.0050**	0.0050**
	(0.0004)	(0.0009)	(0.0004)	(0.0004)
$\Delta C * E$ 12-24	0.0013**	0.0007*	-0.0031**	-0.0045**
	(0.0001)	(0.0003)	(0.0002)	(0.0002)
$\Delta C * E$ 25+	-0.0043**	-0.0057**	-0.0022**	0.0014†
	(0.0001)	(0.0007)	(0.0006)	(0.0008)
Unemployment	-0.0109**	-0.0082**	-0.0100**	-0.0014
	(0.0004)	(0.0013)	(0.0009)	(0.0009)
Net migration	0.0013**	0.0038**	0.0018**	0.0030**
	(0.0002)	(0.0005)	(0.0003)	(0.0004)
GDP/capita	-0.0007**	0.0003	-0.0001	0.0016**
	(0.0001)	(0.0003)	(0.0002)	(0.0002)
Intercept	11.9222**	11.8345**	11.9995**	11.8214**
	(0.0158)	(0.0481)	(0.0342)	(0.0371)
Nbr of obs	236869	42595	87289	90286
Nbr of groups	19007	3046	5747	5217
R^2	0.2681	0.3406	0.3025	0.3133

Significance levels : † : 10% $*$: 5% $**$: 1% Standard errors in parentheses

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Table 9: Summary statistics

Variable	**Mean**	**(Std. Dev.)**	**Min.**	**Max.**	**N**
Individual specific information for men					
Y	234247.6	(104902.2)	127602.9	11343137.0	744279
lnY	12.308	(0.307)	11.757	16.244	744279
experience	17.518	(7.85)	0	38.000	744281
Individual specific information for women					
Y	184477.2	(57323.267)	121659.3	7299537.5	457039
lnY	12.094	(0.235)	11.709	15.803	457039
experience	17.586	(8.337)	0	36.000	457043
Time specific information					
Cohort Size (C)	16.511	(2.248)	13.633	20.461	
Change in C	-0.070	(0.823)	-0.969	2.080	
GDP/capita	180.604	(27.772)	130.134	235.091	
Unemployment	3.420	(2.305)	0.269	8.247	
Net migration	2.205	(2.083)	-1.741	6.738	

A Descriptive statistics

B Estimated vs. actual profiles

Estimated profiles for low educated. The fit is equally good for the other education levels. The dotted lines denote actual earnings.

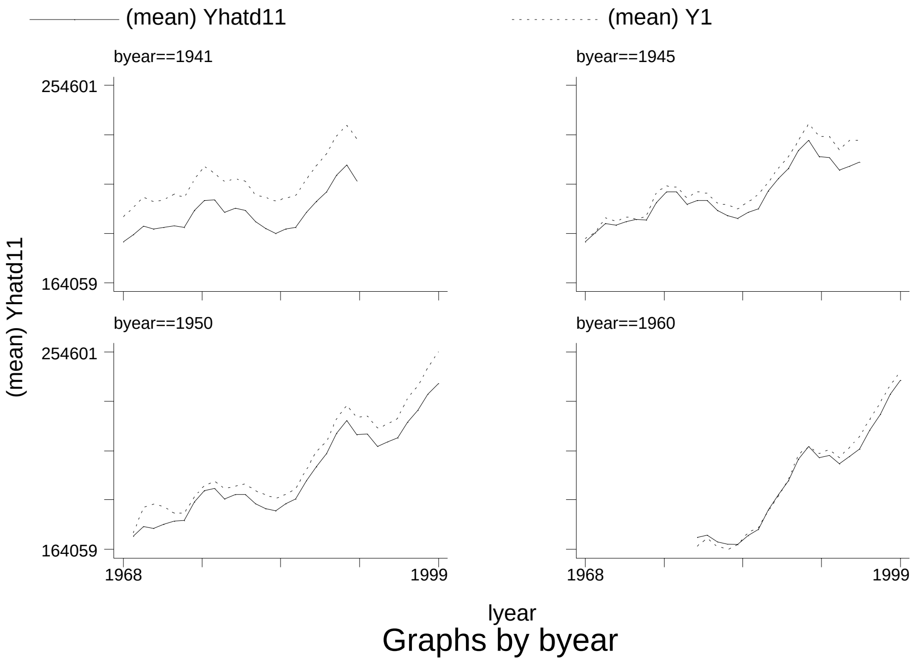

Figure 5: Equation (1) with OLS with period dummies. MEN. The dotted lines denote actual earnings.

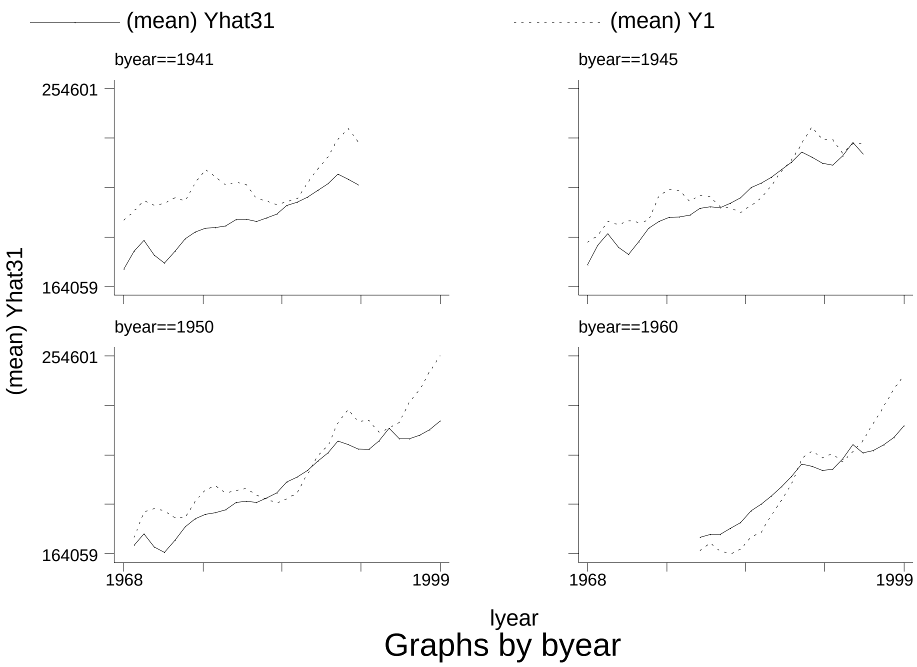

Figure 6: Equation (1) with OLS with macro variables. MEN. The dotted lines denote actual earnings.

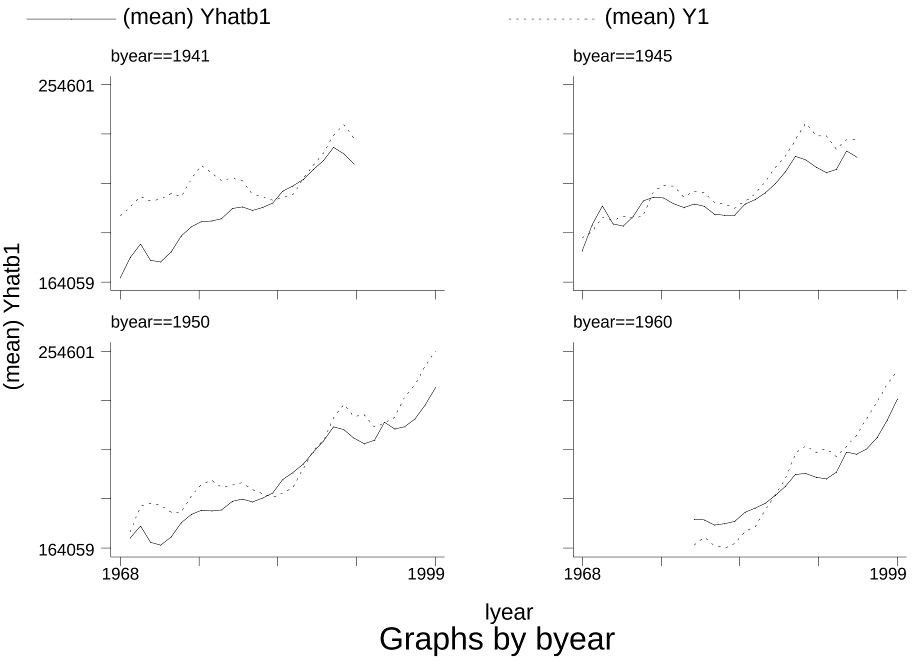

Figure 7: Equation (2) with OLS with macro variables. MEN. The dotted lines denote actual earnings.

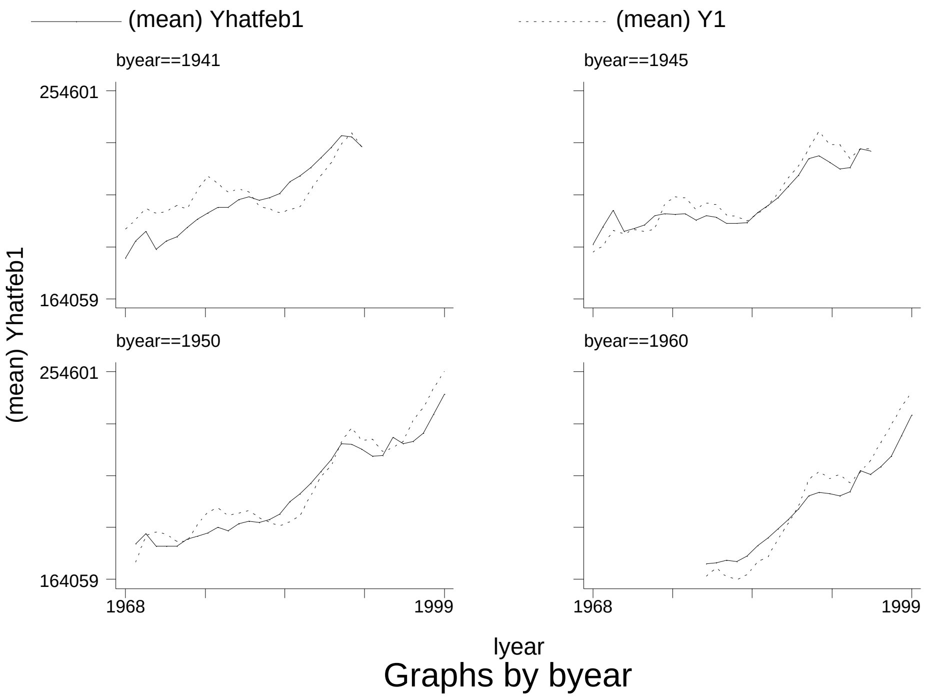

Figure 8: Equation (2) with fixed effects. MEN. The dotted lines denote actual earnings.

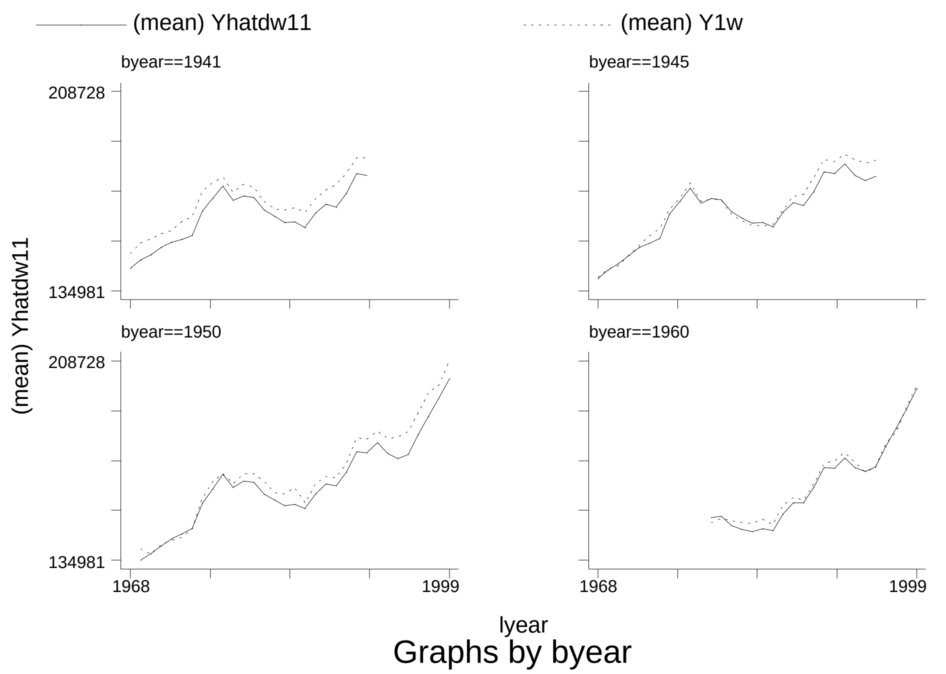

Figure 9: Equation (1) with OLS with period dummies. WOMEN. The dotted lines denote actual earnings.

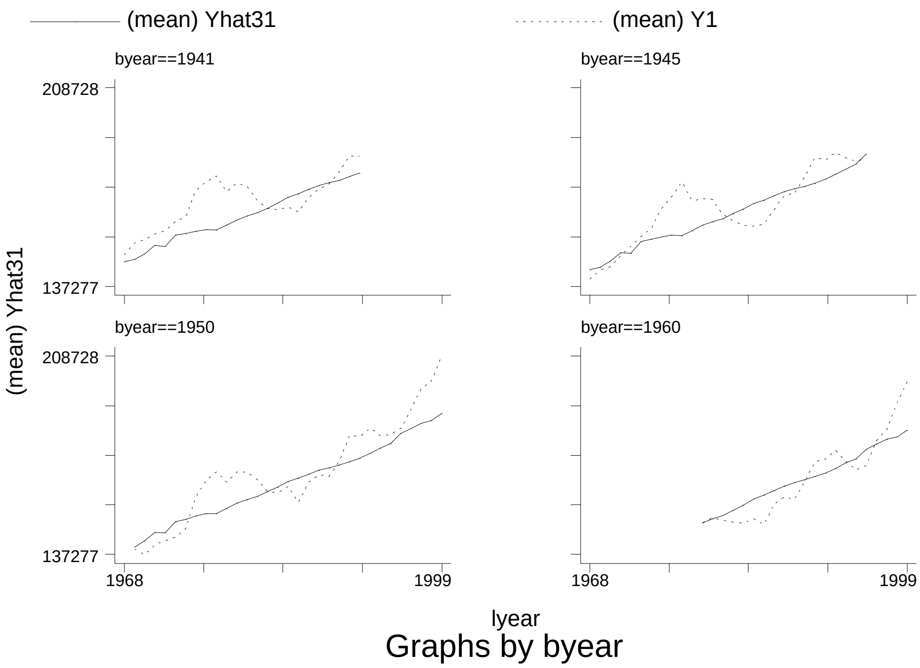

Figure 10: Equation (1) with OLS with macro variables. WOMEN. The dotted lines denote actual earnings.

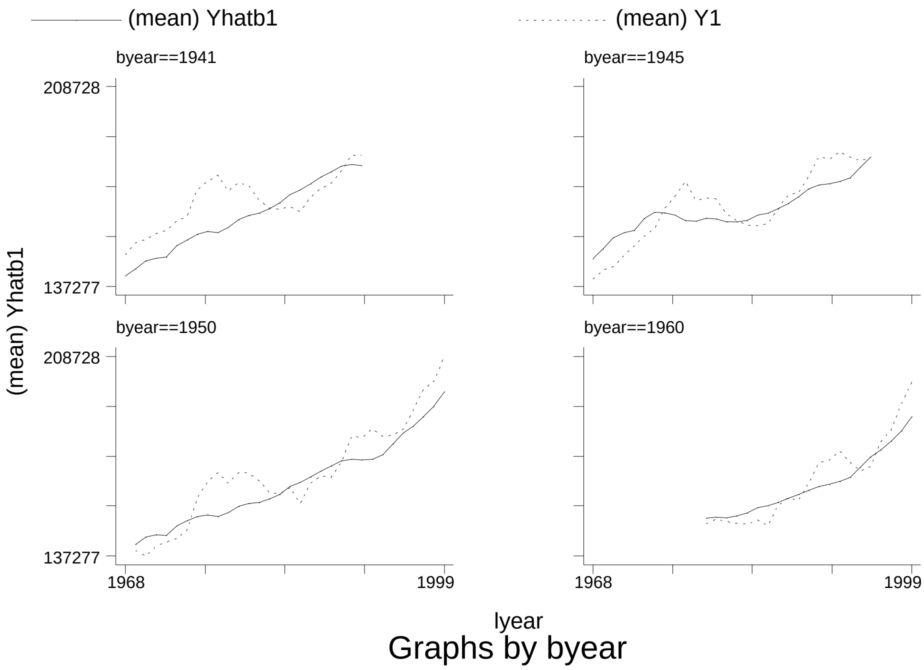

Figure 11: Equation (2) with OLS with macro variables. WOMEN. The dotted lines denote actual earnings.

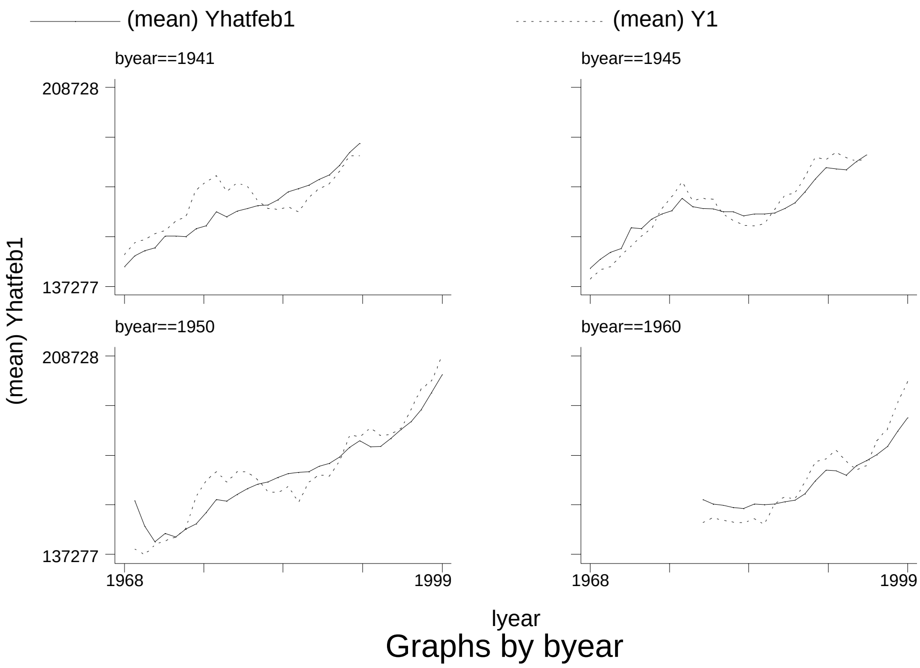

Figure 12: Equation (2) with fixed effects. WOMEN. The dotted lines denote actual earnings.