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LIFE-CYCLE VARIATIONS IN THE ASSOCIATION BETWEEN CURRENT AND LIFETIME INCOME: COUNTRY, COHORT AND GENDER COMPARISONS

by

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Life-Cycle Variations in the Association between Current and Lifetime Income: Country, Cohort and Gender Comparisons*

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Abstract

This study applies Haider and Solon's (2005) generalized errors-in-variables model to Swedish income tax data in order to produce estimates of the association between current and lifetime income. Our estimates of this association demonstrate strong life-cycle patterns. This implies that the widespread use of current income as a proxy for lifetime income (following the standard errors-in-variables model) leads to inconsistent parameter estimates (a.k.a. life-cycle bias). Estimates for comparable cohorts of Swedish and American men demonstrate surprising similarities. There are, however, significant gender and cohort differences in this association which, in turn, lead to statistically significant and quantitatively meaningful differences in life-cycle biases. The results from this study can aid the applied researcher in analyzing and correcting for life-cycle bias.

Keywords: errors-in-variables model, life-cycle bias, lifetime income.

JEL codes: C10, C40, C50, D31.

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1 Introduction

Numerous empirical studies within a wide variety of branches in economics have used current income as a proxy for lifetime income following the textbook errors-in-variables model. This common practice is due to the simple fact that researchers seldom have access to data on lifetime income. Unfortunately, this empirical simplification does not come without a price.

Modigliani & Brumberg (1954) and Friedman (1957) are two examples of early, influential studies that have discussed the attenuation bias due to measurement error which arises in these types of models. These two studies are also known for the considerable energy they spend emphasizing the importance of life-cycle considerations in empirical work. Subsequent research by Jenkins (1987), Haider & Solon (2005) and Grawe (forthcoming) have illustrated an important link between these two strands of research; between errors-in-variables bias and life-cycle considerations. They show that if the association between current and lifetime income changes over the life-cycle, then any standard errors-in-variables model which uses current income as a proxy for lifetime income will be misspecified. In turn, this misspecification leads to inconsistent estimates of the model coefficients above and beyond the bias due to classical measurement error. Given the widespread use of the textbook errors-in-variables model using current income as a proxy for lifetime income, the potential scope of this problem is vast.[1]

Haider & Solon's (2005) paper (henceforth H&S) can be viewed as an important step towards constructing a useful guide for applied researchers on how best to analyze and correct for this problem. They present us with estimates of life-cycle variations in the association between current and lifetime earnings for men in the United States

[1]Haider and Solon (2005) found 14 articles in the refereed issues of the *American Economic Review* published in 2003 in which current income was used as a proxy for individual or family long-term income and as many articles in that year's May *Proceedings* issue.

born between 1931 and 1933. Their estimates of this association can be mapped directly into life-cycle biases. They demonstrate how the size (and direction) of this bias varies significantly with age. Furthermore, they show how their estimates can be used to help us design our empirical studies so as to mitigate this bias, or, when that is not possible, how we can use their estimates to correct for this bias.

The most important questions we need to answer now concern the generality of their results. Can H&S's estimates be used to correct for life-cycle bias in studies using different data sets or are they specific to their particular earnings data set? Can their estimates be used to correct for life-cycle bias in studies concerning earnings (or other measures of income) in other countries, of other cohorts of men, or of women?[2]

The purpose of this paper is to look for answers to these important questions. To do this, we apply H&S's generalized errors-in-variables model to Swedish income tax data in order to produce estimates of the association between current and lifetime income for three cohorts of men and women.

We follow H&S's research outline quite closely in order to make our main results as comparable to theirs as possible. At the same time, this study makes several significant contributions. First, we use two high-quality data sets, the Longitudinal Individual Data for Sweden (LINDA) and the Swedish Level of Living Survey (SLLS), both of which have advantages over the data used by H&S. The main advantage is that our income data are uncensored. This allows us to use a first-best estimation method.[3] Second, the size of the LINDA data set allows us to produce very precise estimates. Third, we make a country comparison between the US and Sweden. Fourth, we compare the estimated associations between three different birth cohorts. Fifth, we compare the estimated associations for both men and women to see if there are any

[2]H&S caution their readers not to assume that their estimates hold exactly for different data sets, countries and cohorts.

[3]The fact that H&S's data are heavily censored forces them to use a more complicated, second-best estimation methodology. See Section 3 for more details.

significant gender differences.

The outline of our paper is as follows. In Section 2, we introduce H&S's generalized errors-in-variables model and demonstrate the potential for life-cycle bias analytically. We also motivate why we should expect life-cycle bias to appear in applied work.

In Section 3, we give a brief description of the SLLS data and the complementary income data which has been collected from the Swedish tax registers for individuals in the SLLS sample. Our first set of results using the SLLS data are reported in Section 4. They show that there are statistically significant and quantitatively meaningful gender and cohort differences in the association between current and lifetime income, which, in turn, translate into significant differences in life-cycle biases. We do not, however, find any meaningful difference when comparing H&S's estimates for American men with those of the relevant cohort of Swedish men.

The LINDA data set is presented in Section 5. A new set of estimates of life-cycle variations in the association between current and lifetime income are presented in Section 6. We find significant gender differences in the LINDA data, which reconfirms our earlier results using the SLLS data. Our findings concerning significant cohort differences also remain unchanged.

In Section 7, we demonstrate how to use our estimates, in conjunction with those of H&S, to analyze and correct for life-cycle bias. Section 8 concludes.

2 Haider and Solon's Generalized Errors-in-Variables Model

In this Section, we present three simple errors-in-variables models: one in which the independent variable is measured with error, one in which the dependent variable is measured with error, and a third in which both the dependent and independent

variables are measured with error.[4] We do this in order to demonstrate the life-cycle bias which can arise when using current income as a proxy for lifetime income and to make clear exactly what it is we intend to estimate. Two important lines of research in economics are used to illustrate these models, intergenerational income mobility and the permanent income hypothesis. But, the principles illustrated here are relevant to numerous other topics in economics.

2.1 Intergenerational Income Mobility

Imagine that we want a measure of income mobility between fathers and sons.[5] To do this we estimate the following model

$$y_i = \beta x_i + \varepsilon_i,$$

where y_i is the log of son i's lifetime income, x_i is the log of his father's lifetime income, and ε_i is a random disturbance which is uncorrelated with x_i. The coefficient β measures the association between the incomes of the father and son. It is our measure of intergenerational income mobility.

Assume now that we lack data on the son's true lifetime income, since we only observe the son at the start of his career. Instead, we use current income y_{it} as an imperfect proxy for the son's lifetime income. In a standard errors-in-variables model, current income would be modeled as the sum of lifetime income plus a random disturbance. In H&S's generalized errors-in-variables model, the association between current and lifetime income is allowed to vary over the life cycle. They model current income as

$$y_{it} = \lambda_t y_i + u_{it},$$

[4] All models are expressed in deviation form.

[5] See Solon (1999) for a review of the literature on intergenerational income mobility.

where λ_t is the association between current and lifetime income at age t and u_{it} is a random disturbance which is uncorrelated with y_i.

If we now use OLS to estimate our model

$$y_{it} = \beta x_i + \varepsilon_i,$$

the probability limit of the slope coefficient $\widehat{\beta}$ is

$$\text{plim } \widehat{\beta} = \frac{Cov(x_i,\, y_{it})}{Var\,(x_i)} = \lambda_t \beta. \tag{1}$$

In the standard errors-in-variables model, where $\lambda_t = 1$, the OLS estimate of β is unbiased. In H&S's generalized model, it is biased by a factor of λ_t.

Given this result, we must ask ourselves wether or not we believe that this generalization of the standard errors-in-variables model is a realistic and important one. Does λ_t, in fact, vary systematically over the life-cycle? Is there evidence in support of this generalization? We feel that there is.

Using SLLS data similar to ours, Björklund (1993) found a strong life-cycle pattern in the correlation between current and lifetime income. He reports correlations that are quite low (sometimes negative) up until the age of 25. But, after the age of 35, these correlations are consistently high (around 0.8).[6] H&S find that "in contrast to the textbook assumption that λ_t equals 1 throughout the life cycle, $\widehat{\lambda}_t$ begins at 0.24 at age 19, increases steadily until it rises to about 1 at age 32, and then declines some in the late forties".

[6] Björklund (1993) is a significant precursor to H&S's work and of our work too. He presents the same type of information that we do in this paper, but in a different manner, since his purpose was somewhat different. His paper illustrated that income inequality measured using current income was greater than that using lifetime income and that the variance in income followed a U-shaped pattern over the life-cycle. Björklund's (1993) correlations, however, do not map directly into magnitudes of errors-in-variables biases needed to correct the estimates obtained in the type of empirical work typically carried out by economists.

Furthermore, numerous studies within the literature on intergenerational income mobility have found that intergenerational income elasticities between fathers and sons increase in a systematic and significant manner as the sons' earnings are observed at later points in his career (Reville, 1995; Solon, 1999; Chadwick and Solon, 2002; Abul Naga, 2002). This would not be the case if λ_t was a constant. If λ_t was a constant, the probability limit of the OLS estimator for β would not vary systematically over the life-cycle as it appears to do. Standard models of human capital investment and accumulation also predict that λ_t should vary systematically over the life-cycle (see e.g. Ben-Porath, 1967; Heckman, 1976).

The large literature estimating age-earnings profiles has led most economist to expect a hump-shaped profile for life-cycle earnings.[7] But, this literature has also led us to expect different life-cycle profiles for different types of workers. For example, it is often found that high income workers have steeper life-cycle earnings profiles than low income workers. Allowing for heterogeneity in earnings and income profiles over the life-cycle is one of the central themes in the new literature on earnings dynamics and inequality (Baker, 1997; Haider, 2001; Baker & Solon, 2003; Gustavsson, 2004).

It is this heterogeneity in life-cycle income profiles which generates life-cycle bias. If all individuals had parallel life-cycle income profiles, then we could control for changes in earnings over the life-cycle using a simple polynomial in age. Controlling for age does not, however, remove life-cycle bias since it arises from heterogeneity across life-cycle income profiles.

This idea can be illustrated with a simple numerical example. Imagine two individuals living in a two-period model. For simplicity, assume that they do not discount the value of future income. In period one, agent A chooses to go to college and re-

[7]See Rosen and Taubman (1982) for estimates of age-earnings profiles of US males using a matched sample of Social Security and Current Population Survey records and Klevmarken (1982) for a comparison of age-earnings profiles in the US and Sweden.

ceives an income of 1. In period two, agent A works and receives an income of 3. Agent B decides to work in both periods and receives an income of 2 in each period.

Each agent has a total lifetime income of 4 (with an annuity value of 2). There is no difference between their lifetime incomes. As such, lifetime income should not be able to explain any observed differences between these two agents. On the other hand, a measure of the current income difference when they are young would say that agent A is worse off than agent B (a negative bias), while a measure of their current income when they are old would say that Agent A is better off (a positive bias). Controlling for the average growth rate in income would not change this result, since life-cycle bias is produced by variations around the central tendency.

2.2 Friedman's Permanent Income Hypothesis

Imagine now that we want to test Friedman's (1957) permanent income hypothesis

$$c_i = \beta y_i + \varepsilon_i,$$

where c_i is permanent consumption of individual i, y_i is a measure of that person's permanent income and ε_i is a random disturbance which is uncorrelated with y_i.[8] Assume that we have an appropriate measure of consumption, but lack data on permanent income and are, therefore, forced to use current income as a proxy. We proceed by estimating the following consumption function

$$c_i = \beta y_{it} + \varepsilon_i,$$

where y_{it} is current income.

Once again, we adopt H&S's generalized errors-in-variables model, so that current

[8] We have altered Friedman's (1957) notation in order to make it consistent with H&S's.

income is modeled as

$$y_{it} = \lambda_t y_i + u_{it}.$$

The OLS estimate of β in this model is biased by a factor of θ_t

$$\text{plim } \widehat{\beta} = \frac{Cov(y_{it},\, c_i)}{Var(y_{it})} = \frac{Cov(y_{it},\, y_i)}{Var(y_{it})}\beta = \theta_t \beta, \tag{2}$$

where

$$\theta_t \equiv \frac{Cov(y_{it},\, y_i)}{Var(y_{it})} = \frac{\lambda_t var(y_i)}{\lambda_t^2 var(y_i) + var(u_{it})}. \tag{3}$$

The OLS estimate of β in the standard model, with $\lambda_t = 1$, is, of course, also biased. It suffers from an attenuation bias. In the generalized model, the size and direction of this bias depends on λ_t. In fact, if $\lambda_t < 1$ and if $Var(u_{it})/Var(y_i)$ is small enough, then the bias becomes an amplification rather than an attenuation (Haider and Solon, 2005).[9]

2.3 Extending the Examples

It is not uncommon in the literature on intergenerational income mobility that both the father's and the son's lifetime incomes are proxied by their current incomes. In this case, the generalized errors-in-variables model becomes

$$y_{it} = \beta x_{is} + \varepsilon_i,$$

[9] Using current income together with a geometrically weighted average of past incomes, as is commonly done in the consumption function and permanent income literature (see e.g. Chapter 4 of Deaton (1992) for an overview), may mitigate some of the attenuation bias due to the standard errors-in variables problem, but it does not correct for life-cycle bias.

where y_{it} and x_{is} are modeled as

$$y_{it} = \lambda_t y_i + u_{it}$$

and

$$x_{is} = \lambda_s x_i + w_{is},$$

respectively, and where u_{it} and w_{is} are random disturbances which are uncorrelated with y_i and x_i, and $s \neq t$. Once again, the probability limit of $\widehat{\beta}$ is biased

$$\text{plim } \widehat{\beta} = \lambda_t \theta_s \beta. \tag{4}$$

The associated bias is equal to the product of λ_t and θ_s, where θ is defined in Equation 3.[10] Of course, the same bias will appear in our example concerning the permanent income hypothesis if we use both current consumption and current income as proxies for their permanent values.

2.4 What Have We Learned (...So Far)?

What have we learned from these examples? First, allowing for plausible, life-cycle variations in the association between current and permanent income significantly alters results concerning the properties of OLS estimators of error-in-variables models. Measurement error in the dependent variable is no longer innocuous. Measurement error in the dependent variable can lead to either an attenuation or an amplification bias. Second, the estimation biases which arise from using current income as a proxy for lifetime income can be summarized by two parameters: λ_t, which is the slope

[10] Using a multi-year average of current income does not eliminate life-cycle bias nor does it completely eliminate attenuation bias (Mazumder, 2001, 2005; Haider and Solon, 2005).

coefficient of current income on permanent income

$$y_{it} = \lambda_t y_i + \varepsilon_{it}, \tag{5}$$

and θ_t, which is the slope coefficient of permanent income on current income

$$y_i = \theta_t y_{it} + \varepsilon_{it}. \tag{6}$$

Consistent estimates of λ_t and θ_t can be obtained using OLS.

3 The Swedish Level of Living Survey

The Swedish Level of Living Survey (SLLS) is one of the longest running longitudinal social science surveys in the world.[11] It was first conducted in 1968. Thereafter, it has been replicated in 1974, 1981, 1991 and 2000. The basis for the survey was a random sample of 1/1000 of the Swedish population between 15 and 75 years of age. The same respondents have been interviewed again in later waves and 1,750 respondents have, in fact, participated in all five waves. The data from the interviews have been complemented with information from various registers. In particular, we use information on total net income (*sammanräknade nettoinkomster*) before taxes from these registers. This income measure is available from 1951 to 2000 with the exception for 1959, which is missing due to changes in the administrative routines followed by Statistics Sweden. Thus, in contrast to most studies, we have nearly career-long income histories for the individuals in our sample. The SLLS data used in the first part of this study are similar to those used in Björklund (1993).

Our measure of pre-tax, total net income is the sum of an individual's labor

[11] See Eriksson and Åberg (1987) or Fritzell and Lundberg (1994) for details.

earnings (and labor related earnings, such as taxable sick benefits, unemployment benefits, and parental leave payments), income from one's own business, pensions, capital income and realizations of capital gains. Deficits in any source of income are deducted. In the year 1991, however, only capital deficits are deducted since we do not have information regarding other potential income deficits. Starting in 1992, we do not have information on capital deficits either. We do not believe this to be a large problem, however, since income from capital accounts for less than 2.6 percent of total income for the individuals in the SLLS sample (Björklund, 1993).[12]

The income data connected to the SLLS comes from tax registers. We have income data for everyone who has paid income tax (and/or wealth, inheritance, or property tax) and also for everyone who has filed a declaration without actually having paid any taxes. There are two potential problems in using income data from the tax register. First, people with income below the tax exempt level are not required to file a declaration (unless they are required to pay wealth, inheritance or property taxes). But, the tax exemption level is quite low and people have strong incentives to declare some income, because labor income opens the door to many social insurance programs. Furthermore, starting in 1982, Statistics Sweden also uses information on individuals' income as reported by their employers. This eliminates any left-censoring that might occur when using income measures based solely on tax declarations.[13] The second problem associated with our register data is that a number of social benefits (most notably sickness and unemployment benefits) became taxable between 1973

[12]For 1991 we have both a measure of total income net of capital deficits and a measure of gross total income. The correlation between these two measures in our sample is 0.98.

Due to this switch from net to gross income, we do obtain a few observations of unusually high measures of income between 1991-2000. To deal with this anomaly, we top-code these values down to 1.5 million Swedish Kronor. We feel that this is a reasonable approach given that we don't have a single observation of net income above this threshold (either in nominal or in real terms) in our sample before 1991. Top-coding does not affect our estimates of λ_t and θ_t.

[13]The minimum positive income observed in our data set is 1000 Swedish Kronor for the years 1950-58 and 1969. For all other years, it lies between 1 and 132 Swedish Kronor.

and 1974 and, hence, appear in our measure of total income. For 1975, Björklund (1993) was able to deduct these benefits from his measure of total income and reports that they accounted for roughly 5 percent of total income in that year. But, the importance of these sources of income may have changed over time.

Our measure of economic status differs from that used by H&S. They use a measure of labor earnings taken from Social Security records for a sample of men from the University of Michigan's Health and Retirement Survey. In Section 6.3, we use data from LINDA to run a sensitivity analysis to see if our estimates of life-cycle bias change if we move from a measure of economic status based on total income to one based solely on labor earnings.[14]

The SLLS data set has several clear advantages over that used by H&S. First, there is no attrition from the original sample due to the need to ask permission from individuals to access to their tax records. In Sweden, these records are in the public domain.[15] Second, H&S's earnings data pertain only to jobs covered by Social Security.[16] Third, we have income data up until 2000, which allows us to follow two different birth cohorts for 41 and 40 years, respectively. Fourth, we have data on both men and women, which allows us to search for potential gender differences in the association between current and lifetime income. Fifth, and most importantly, our data are not censored.

The Social Security data that H&S use are heavily right-censored, since income

[14] We have such detailed information in our LINDA data set that we can define earnings and/or income as we see fit. The only drawback with the LINDA data set is that it does not start until 1968. Thus, we can not compare it directly to H&S's estimates from 1951-1991. That is why we have chosen to first estimate our measurement equations using SLLS data. No pure earnings measure comparable to H&S's measure of earnings is available in the SLLS data set.

[15] Roughly 75 percent of the participants in the Health and Retirement Survey agreed to allow access to their Social Security earnings records. The fact that 25 percent of the participants did not agree to releasing their records, however, does not seem to be as important as one might think. Haider and Solon (2000) show that the remaining 75 percent are surprisingly representative of the complete sample.

[16] Coverage was between 66 and 79 percent between 1951-56, but has exceeded 80 percent in each year since then. Coverage exceeds 85 percent for most of the sample. (Haider and Solon, 2004)

is only recorded up to the maximum amount subject to Social Security taxation.[17] Censorship forces H&S to use a more complicated, three-step method to estimate λ_t and θ_t. They first use a limited-dependent-variable model to estimate the joint distribution of uncensored annual earnings and then create a sample of 4000 uncensored earnings histories by drawing from this distribution. Equations 5 and 6 are then estimated using these simulated earnings histories. The fact that our data are not censored allows us to estimate Equations 5 and 6 directly using OLS.

We apply a number of sample restrictions to our data in order to make our results as comparable to H&S's results as is possible. We include only those individuals for whom we have at least 10 positive observations on annual income. In our first experiment, we examine estimates of men from the same birth cohort and we follow them over the same time period. We remove those individuals who immigrated to Sweden after the age of 16 and we require that individuals did not fall out of our sample due to death or migration from Sweden.

Our measure of lifetime income is the annuity value of the discounted sum of real annual income. Current earnings are deflated using the consumer price index from Statistics Sweden. Following H&S, real earnings are discounted using an interest rate of 2 percent.

4 Results Using SLLS Data

We begin by calculating the 41×41 autocorrelation matrix of log annual income for the years 1951 to 1991 for Swedish men born between 1929 and 1933. This means that we follow these men from the (average) age of 20 until the (average) age of 60. There are 215 men in this sample. We observe at least 14 years of positive annual

[17] Their data are also subject to a less dramatic left-censoring, since one does not have to declare small amounts of income.

income for each man in the sample.[18] On average, we observe 38 years of positive annual income per man. The median number of positive observations is 40.[19]

This particular time period and cohort of men was purposefully chosen so as to be comparable with the cohort of American males studied in H&S.[20] Since our income data are uncensored, we are able to calculate these correlations directly from the data, while H&S use the estimated autocovariances from a Tobit analysis. Standard errors for our correlations have been calculated using a bootstrapping procedure.[21]

Table 1 shows the autocorrelations for 1975 to 1984, a time when the men in our sample are between the ages of 42 and 55. Our correlations are shown in bold face type, while the correlations from H&S are included (below our own) in normal face type. Table 2 shows the average autocorrelations of orders 1 through 6 calculated from our Table 1. We see that earnings of American men are slightly more persistent than incomes of Swedish men at the first two orders of autocorrelations, but are identical at the fifth and sixth orders of autocorrelations.

The autocorrelations of log annual earnings for these same men, when they are younger, are reported in Table 3. Here, we see quite low autocorrelations for both Swedish and American men. American men, however, appear to settle into their lifetime earning patterns earlier than Swedish men. The first order autocorrelation

[18] Throughout this paper, we require at least 10 positive observations on income for each individual if they are to be part of our sample. But once this restriction is put into place, we sometimes find that the minimum number of observations is, in fact, larger than 10.

[19] Income data from 1959 are not available (see Section 3). This means that 40 years is the maximum number of positive observations we can have for any individual.

[20] H&S have earnings data for men in the US born between 1931 and 1933 for the years 1951-91. In 1951, these men are on average 19 years old. They have a total of 821 individuals in their sample. We have only 124 male individuals born in these three years who meet all of the necessary criterion. We have, therefore, added men born in 1929 and 1930 to our sample. This gives us a total of 215 men.

[21] Our bootstrapping procedure entails resampling from the available observations (with replacement) 1000 times. The correlation from each resampling was calculated and saved. This produces a distribution of resampled correlations corresponding to each sample correlation. We then equate the standard deviation from the distribution of resampled correlations to the standard error of the actual, sample correlation.

of their annual earnings is already 0.80 at the (average) age of 23. Keep in mind, however, that we are using a broader income measure than that used by H&S.

Figure 1 shows our estimates of λ_t along with a 95 percent confidence interval. The estimates start out well below 1 when the men are young. It grows quickly; crossing 1 at age 34. After age 33, our estimates of λ_t are not statistically significantly different from 1, except at ages 40 and 45 (which exceed 1)[22] and ages 54 through 57 (which are less than 1).[23] Thus, for Swedish men born between 1929 and 1933 there does not appear to be a significant life-cycle bias from using current income as a proxy for lifetime income, as long as current income is measured after the age of 33. This conclusion will be strengthened by our sensitivity analysis in Section 4.3.

Figure 2 shows our estimates of θ_t. These estimates stabilize after the age of 30 and are equal to about 0.5. Since $\widehat{\lambda}_t$ is approximately equal to 1 after age 30, $\widehat{\theta}_{30+}$ can be interpreted as a measure of attenuation bias due to measurement error.

On average, our estimates of λ_t are somewhat larger (and closer to 1) than those reported in H&S (see Figure 3), especially when the men are young. Their estimates exhibit a slightly stronger life-cycle bias than our own. Our estimates of θ_t, on the other hand, are somewhat lower (see Figure 4). Given that we are using a broader measure of economic status and that the distributions of income and earnings are much more compressed in Sweden than they are in the US, we feel that the similarities between our estimates and those of H&S are more striking than the differences.

4.1 Gender Differences

In this section, we produce new estimates of λ_t and θ_t using a sample of 225 Swedish women born between 1929 and 1933. We observe at least 11 years of positive annual

[22] The highest $\widehat{\lambda}_t$ is reached at age 40 and is equal to 1.15. At age 45, the estimate of $\widehat{\lambda}_t$ is equal to 1.12.

[23] The estimate of $\widehat{\lambda}_t$ at age 54, 55, 56 and 57 are 0.86, 0.87, 0.77 and 0.80, respectively.

income for each woman in the sample. On average, we have 30 positive annual observations per woman. The median number of positive observations is 32.

Table 4 shows us that prime-age female income is less persistent than prime-age male income. Our new estimates of λ_t and θ_t are shown in Figures 5 and 6, respectively. We see that there are significant life-cycle biases at nearly all ages. Only at ages 30 to 33, 35, 44, 45 and 48 is $\widehat{\lambda}_t$ not statistically significantly different from 1. Our estimates of λ_t cross 1 (on their way up) at age 32 and again (on their way down) at age 45. They peak at age 38 with a $\widehat{\lambda}_t$ equal to 1.63, which is quite a bit larger than the peak for men. Life-cycle bias is a more serious problem for Swedish women then for Swedish men, since women display more variety in their life-cycle income profiles (i.e. most men work full-time, while women are split into three categories: housewives, part-time workers and full-time workers).

The large number of zero income years for some women may affect our estimates. We address this possibility in the sensitivity analysis carried out in Section 4.3. With this qualification in mind, we conclude that there are statistically significant and quantitatively important differences in the association between current and lifetime income between men and women. If researchers use a women's current income as a proxy for her lifetime income, their estimates will almost certainly be fraught with large life-cycle biases.

4.2 Cohort Differences

Thus far, we have found both surprising similarities across countries and quite large gender differences. Now, we would like to see if there are any significant cohort differences in the association between current and lifetime income. To do this, we produce new estimates of λ_t and θ_t for a younger cohort of Swedish men born between 1939 and 1943. Following this, we repeat the experiment for a younger cohort of

women also born between 1939 and 1943.

This new sample of men consists of 281 individuals who meet all of our selection criteria. We have at least 20 years of positive annual income for each of them. On average, we have 38 positive observations per young man. The median number of positive observations is 40.

Income of the younger cohort of men is less persistent than that of the older cohort of men (see Table 5). Estimates of λ_t and θ_t for this new cohort of Swedish men are shown in figures 7 and 8. Statistically, it is hard to distinguish the estimates of λ_t for this younger cohort from the older cohort of Swedish men. The few statistically significant differences between the estimates of λ_t for the old and young cohorts of Swedish men do, however, lead to important differences for the applied economist. First, at age 20 and 21, the associations between current and lifetime income are 0.02 and -0.05, respectively.[24] This, we believe is due to the higher investment in schooling among the younger cohort.[25] Those who will eventually become high income individuals have lower than average incomes when young.

For the younger cohort, $\widehat{\lambda}_t$ peaks at age 50. The value of $\widehat{\lambda}_t$ at age 50 is equal to 1.32 with a standard error of 0.09. For the older cohort, $\widehat{\lambda}_{50}$ is equal to 0.97 with a standard error of 0.06.[26] In general, our estimates of λ_t are more in line with what one would expect to see in a standard model of human capital accumulation and income growth; we find a negative bias when these men are younger and a positive bias when they are older.

Differences between the two cohorts of women are even larger. Income of the younger cohort of women is less persistent than that of the older cohort (see Table

[24] The t-ratio for the null of equality between $\widehat{\lambda}_{21}$ of the two cohorts is 3.51.

[25] Swedish men in the older cohort have on average 8.01 years of schooling. Swedish men in the younger cohort have on average 11.31 years of schooling. Years of schooling are taken from SLLS 1991.

[26] The t-ratio for the null of equality between $\widehat{\lambda}_{50}$ of the two cohorts is 3.24.

5). Estimates of λ_t and θ_t for the younger cohort can be seen in Figures 9 and 10.

These estimates were produced with a sample of 276 Swedish women born between 1939 and 1943. We have at least 12 positive observations on annual income for each woman. The mean number of positive observations is 34 and the median is 36.

There are a number of striking differences that can be seen by simply comparing Figure 9 with Figure 5. The hump shaped life-cycle trajectory of $\widehat{\lambda}_t$ for the older cohort of women has been replaced by a profile that is more similar to that of the two cohorts of men. There are significant life-cycle biases at particular ages. But overall, current income appears to track lifetime income from age 27 and onwards.[27] This implies that the life-cycle income profiles for this sample of women are more homogeneous than those of the older cohort.

4.3 Sensitivity Analysis

Our measure of lifetime income is the annuity value of the discounted sum of real annual income. This was calculated using an interest rate of 2 percent. We have re-estimated λ_t and θ_t for both cohorts of men and women using an interest rate of 4 percent. We find no significant changes in our estimates.

When estimating the association between current and lifetime income, we restricted our sample to those individuals for which we had at least 10 positive observations of annual income. This selection criterion was not very restrictive for men. But, we have more zero incomes among our sample of women. This is only natural given that our sample includes homemakers as well as career women.

To get a feel for how our estimates of λ_t for women are affected by the presence of zero incomes in the data, we estimated a set of alternative λ_t's. These are displayed in Figures 11 and 12. We first estimated λ_t for our sample of women with no regards

[27] This result, however, is partially due to the large standard errors associated with these estimates.

as to how many zero incomes each woman had. Then, we re-estimated λ_t, adding our baseline restriction requiring 10 or more positive observations, then 20 or more, and finally 30 or more positive observations.

For the older cohort, we find that there is a stronger life-cycle bias for the sample of women with 20 or more years of positive incomes than for our baseline sample and a lower life-cycle bias when using the whole sample.[28] We also find a stronger life-cycle bias for those women in the younger cohort who have 20 or more years of positive incomes.[29] In both cases, these differences are seldom statistically significant.

We repeated this exercise for both cohorts of men. The results of which are displayed in Figures 13 and 14. This sensitivity experiment strengthens our previous conclusion for the older cohort of men that life-cycle bias does not appear to affect them once they have reached their early thirties.[30] For the younger cohort, our measures of life-cycle bias is much larger for men in the restricted sample (requiring many years of positive incomes) than for the sample as a whole.[31]

5 Longitudinal Individual Data for Sweden

We continue our analysis using data from the longitudinal database LINDA (Longitudinal Individual Data for Sweden). LINDA is a fully representative sample containing information on 3.35 percent of the Swedish population.[32] Information on income and earnings is drawn from the same registers as those used to construct the SLLS income data, which makes our income variables comparable across data sets. LINDA also

[28] The number of observations used in each successive estimation drops from 229, to 225, to 205, to 134.

[29] The number of observations used in each successive estimation drops from 277, to 276, to 271, to 235.

[30] The number of observations drops from 215, to 215, to 212, to 207, to 115.

[31] The number of observations drops from 281, to 281, to 281, to 270, to178.

[32] See Edin and Fredriksson (2000) for more information on LINDA.

provides us with enough detailed information concerning the different components of income that we are able to construct a measure of earnings similar to the one used by H&S.

The size and detail of the LINDA database has made it an extremely valuable tool for social scientists interested in studying questions concerning the development and distribution of income in Sweden. It allows us to follow nearly 11,000 (!) men and women born between 1948 and 1950 for 35 years. The only drawback (for our purposes) is that we will "only" be able to follow our individuals from 1968 to 2002. H&S were able to follow their men for 41 years and we were able to follow our previous two cohorts for 41 and 40 years, respectively. Thus, our claim of having "nearly career-long income data" is less true here.

Our measure of total net income before taxes constructed using the LINDA database is identical to the one from the SLLS data. It is the sum of all sources of declared income net of any deficits. To construct a measure of labor earnings, we begin with income from employment and/or salary from self employment. To this, we add income from maritime employment. From this, we deduct a number of social benefits which are taxable in Sweden and give Swedish pension points, but that would not have been reported to US Social Security authorities as earnings that give pension points in the US. The US and Swedish systems are quite different in this regard and our purpose is to mimic H&S's measure of earnings as closely as possible. To this end, we subtract unemployment benefits, parental benefits and a variety of other payments from the Swedish social welfare authorities from total earnings in order to produce our earnings measure.[33]

Since the income variables in LINDA are constructed from the Swedish income tax registers, there are several potential problems one should consider. As noted

[33] See Table 7 for a year-by-year description of the variables we use to construct our measures of income and earnings.

in Section 3, a number of social benefits (most notably sickness and unemployment benefits) became taxable between 1973 and 1974. But since the cohort of men and women we study were (on average) only 24 years old in 1973, they were not collecting significant amounts of income from these two sources between 1968 and 1973. So, we do not consider this to be a major problem for our measure of net income. We have subtract these benefits from our earnings variable.

Another potential difficulty is the Swedish tax reform of 1990-91. This tax reform entailed both a decrease in marginal tax rates and a broadening of the tax base. Edin and Fredriksson (2000) argue that this makes income variables from the tax registers before and after the tax reform incomparable. Fortunately, this claim is not entirely correct. First of all, the broadening of the tax base was accomplished mainly by implementing a more unified system of taxing capital and company profits, an expansion of the VAT coverage and an elimination of loopholes and preferential treatment of certain types of earned income (Agell et al., 1996). Although, this may have caused a number of individuals to shift portfolios, most people continued to declare the same types and amounts of income as before with the addition of only a few minor posts such as lunch coupons, car usage and other fringe benefits. Furthermore, we see no structural break in our series for average income between 1990 and 1991, even if we take into account the general downturn in the economy.[34]

6 Results Using Data from LINDA

We begin by calculating the 35×35 autocorrelation matrix of log annual income for the years 1968 to 2002 for Swedish men born between 1948 and 1950. We follow these

[34] More generally, we would not recommend researchers to use the aggregate income and earnings variables reported in LINDA. They are not as consistent (over time) as the variables that we have constructed ourselves using the underlying individual variables.

men from the (average) age of 19 until the (average) age of 53. There are 5492 men in our LINDA sample. This sample is more than six times larger than the sample of American men used by H&S and, once again, the data are uncensored.

We have at least 11 positive observations on income for each man in the sample. On average, we have 33 positive observations. The median number of positive observations is 34.

Average autocorrelations of log annual income of prime-age men are presented in Table 6. Our estimates of λ_t for this cohort of men are shown in Figure 15. They show a clear pattern of strong life-cycle bias at nearly all ages. Our estimates start off well below zero when these men are young. They cross 1 at age 34 and peak at age 48 with a value of 1.45 (0.021). The pattern of life-cycle bias shown in Figure 15 is consistent with a standard model of human capital formation in which high income workers have steeper income profiles than low income workers.[35] Our estimates of θ_t (shown in Figure 16) are also indicative of a serious life-cycle bias as well as an attenuation bias from classical measurement error.

6.1 Gender Differences

Our earlier estimates of λ_t and θ_t using the two SLLS data sets demonstrated significant gender differences. Now we would like to see if this also holds true for our LINDA data set. To find out, we first calculate the 35×35 autocorrelation matrix of log annual income for the years 1968 to 2002 for Swedish women born between 1948 and 1950. We follow these women from the (average) age of 19 until the (average) age of 53. There are 5163 women in our LINDA sample. We have at least 10 positive observations on income for each woman. On average, we have 32 positive observations

[35] See Figure 1 in H&S for a theoretical example that would produce a pattern for λ_t identical to the one we find in the LINDA data.

and the median is 33.

Average autocorrelations of log annual income of prime-age women are presented in Table 6. These autocorrelations are slightly higher for Swedish women than they are for Swedish men. More importantly, we find that there are significant gender differences in our estimates of the association between current and lifetime income, which, in turn, translate into significant gender differences in life-cycle bias (compare Figures 15 and 17).

6.2 Cohort Differences

Comparing Tables 5 and 6, we see that the average autocorrelations of log annual income of prime-age Swedish men are largest for the oldest cohort and smallest at longer lags for the youngest cohort. Thus, each new cohort has experienced more variation in income (over time) than its predecessor.

Estimates of λ_t for the three different cohorts of Swedish men are reproduced in Figure 19. In order to make these estimates more comparable we have re-estimated the two SLLS life-cycle trajectories using only the first 35 available observations. Figure 19 shows us that each successively younger cohort suffers from a larger life-cycle bias than its predecessor when using current income as a proxy for lifetime income.

Comparing the oldest and youngest cohort of prime-age women, we see that the first three autocorrelations of logged annual income have decreased, while the last three have increased (compare Tables 5 and 6). Estimates of λ_t for the three different cohorts of Swedish women are reproduced in Figure 20. Once again, we have re-estimated the SLLS life-cycle trajectories of $\widehat{\lambda}_t$ using only 35 years of data in order to make these trajectories more comparable. Figure 20, demonstrates a strong shift to the left, which implies a larger, positive life-cycle bias at earlier ages for the youngest

cohort in comparison to the oldest cohort. This leftward shift is due to large changes in the underlying life-cycle income profiles of women.

6.3 Sensitivity Analysis

Our measure of lifetime income is the annuity value of the discounted sum of real annual income. This was calculated using an interest rate of 2 percent. We have re-estimated λ_t and θ_t for both men and women using an interest rate of 4 percent. We find no significant changes in our estimates.

A measure of earnings is not available in the SLLS data set. We have, therefore, chosen to focus our efforts on income. But, we do have a measure of earnings in the LINDA data set similar to the one used by H&S. Figure 21 shows us that the association between current and lifetime earnings for Swedish men leads to somewhat larger (average) life-cycle biases at earlier ages than does income. The opposite is true for women (see Figure 22). The life-cycle bias associated with using current earnings as a proxy for women's lifetime earnings is lower than the bias we found using income.

To get a feel for how our estimates of λ_t for women are affected by the presence of zero incomes in the data, we estimated a set of alternative λ_t's. These are displayed in Figures 23. As before, we first estimate λ_t for our sample of women with no regards as to how many zero incomes each woman had. Then, we re-estimate λ_t, adding our baseline restriction requiring 10 or more positive observations, then 20 or more, and finally 30 or more positive observations. Once again, we find a stronger life-cycle bias for the restricted sample than for the broader sample. For men, we find little impact of zero incomes (see Figure 24).

7 Applying our Results

Before concluding, we would like to provide several concrete examples of how our estimates of λ_t and θ_t can be used to analyze and correct for life-cycle bias?[36] Assume, once again, that we want to estimate a measure of income mobility between fathers and sons and that we only have one observation on the current income of sons when they are 30 years old and one observation on current income of fathers when they are 55 years old. Equation 4 tells us that in order to obtain an unbiased estimate of intergenerational mobility, β, we need to divide our estimate of β by $\widehat{\lambda}_{30} \times \widehat{\theta}_{55}$. Our estimate of λ_{30} for the youngest LINDA cohort is equal to 0.78 (0.016) and our estimate of θ_{55} for the oldest, SLLS cohort of men is equal to 0.63 (0.039). Thus, we should divide our estimate of β by 0.49 (0.032) to correct for life-cycle and measurement error biases.

H&S's estimates of λ_t are approximately equal to 1 for men in their early thirties to mid forties. Thus, using current income of a man in this age bracket as a proxy for his lifetime income appears relatively unproblematic. Our estimates of λ_t using SLLS data reaffirms this result. Our estimates of λ_t for the youngest cohort of Swedish men using LINDA data are much more precise. They tell us that $\widehat{\lambda}_t$ is only equal to 1 at age 34. For women, however, using income from when they are in their thirties is about the worse choice you can make. For the oldest SLLS cohort, we would like to use income after the age of 45 and, for the youngest LINDA cohort, we would like to use income after the age of 40.

The fact that we find significant cohort differences in life-cycle bias makes our study highly relevant to the new literature concerning trends in intergenerational mobility (see e.g. Mayer and Lopoo, 2005; Lee and Solon, 2005). For example,

[36] Our estimates of θ_t also entail a correction for the attenuation bias which arises from classical measurement error.

Mayer and Lopoo (2005) follow sons born between 1949 and 1965. They regress the income of sons at age 30 onto the income that his parents had when he was between the ages of 19 and 25. Our estimate of λ_{30} for men born between 1948-50 is equal to 0.78 (0.016). Since we have no estimates of λ_{30} for men born after this period, let us assume that λ_{30} is the same for all sons born between 1949-65. Our estimates of θ_t for the "fathers" in our sample (i.e. of older men), however, have not remained constant. They have fallen steadily from cohort to cohort. For men born between 1929-33, $\widehat{\theta}_{52}$ is equal to 0.62 (0.035).[37] For men born between 1939-43, $\widehat{\theta}_{52}$ is equal to 0.42 (0.026) and for men born between 1948-50, $\widehat{\theta}_{52}$ is equal to 0.33 (0.005). Thus, the falling trend in elasticities reported by Mayer and Lopoo (2005) may simply be due to their failure to correct for life-cycle and attenuation bias.

8 Conclusion

Our goal has been to produce estimates of the association between current and lifetime income in order to say something about the existence, size and direction of life-cycle bias. This was done by applying Haider and Solon's (2005) generalized errors-in-variables model to Swedish income tax data. The unusual quality of our data has allowed us to obtain estimates by running simple OLS regressions. We view this as a unique strength of our study. Our data has also enabled us to estimate this association for three different birth cohorts of men and women.

We found distinct life-cycle patterns in the association between current and lifetime income. This implies that the widespread use of current income as a proxy for lifetime income (following the standard errors-in-variables model) leads to inconsistent parameter estimates. Estimates for comparable cohorts of Swedish and American

[37] The choice of $\widehat{\theta}_{52}$ implies that these "fathers" had their "sons" at age 30. But, the idea presented here is not sensitive to this choice.

men demonstrated surprising similarities. But we also found significant gender and cohort differences in this association which, in turn, lead to statistically significant and quantitatively meaningful differences in life-cycle biases. The results from this study, used in conjunction with those of Haider and Solon (2005), can aid the applied researcher in analyzing and correcting for life-cycle bias.

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Table 1: Autocorrelations in Log Annual Income of Men, 1975-1984.

Year	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984
1975	**1**[a]	**0.87**	**0.77**	**0.75**	**0.49**	**0.74**	**0.65**	**0.50**	**0.65**	**0.71**
		(0.04)[b]	(0.06)	(0.05)	(0.18)	(0.06)	(0.09)	(0.14)	(0.06)	(0.06)
	1[c]	0.92	0.82	0.79	0.77	0.74	0.69	0.65	0.62	0.59
1976		**1**	**0.87**	**0.84**	**0.83**	**0.84**	**0.79**	**0.76**	**0.76**	**0.74**
			(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)
		1	0.88	0.83	0.79	0.77	0.73	0.69	0.66	0.64
1977			**1**	**0.78**	**0.72**	**0.77**	**0.74**	**0.65**	**0.67**	**0.73**
				(0.09)	(0.08)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)
			1	0.91	0.84	0.77	0.77	0.70	0.68	0.69
1978				**1**	**0.90**	**0.84**	**0.79**	**0.76**	**0.71**	**0.69**
					(0.03)	(0.04)	(0.04)	(0.05)	(0.05)	(0.06)
				1	0.88	0.80	0.78	0.72	0.70	0.69
1979					**1**	**0.85**	**0.79**	**0.81**	**0.75**	**0.70**
						(0.05)	(0.04)	(0.08)	(0.06)	(0.06)
					1	0.87	0.84	0.78	0.73	0.71
1980						**1**	**0.88**	**0.79**	**0.79**	**0.78**
							(0.04)	(0.07)	(0.04)	(0.05)
						1	0.89	0.83	0.76	0.73
1981							**1**	**0.82**	**0.84**	**0.84**
								(0.05)	(0.04)	(0.04)
							1	0.87	0.82	0.78
1982								**1**	**0.83**	**0.78**
									(0.06)	(0.06)
								1	0.89	0.82
1983									**1**	**0.79**
										(0.06)
									1	0.87

Source: SLLS data.

a) Our autocorrelations are in bold typeface.

b) Our standard errors are calculated by means of a bootstrapping procedure.

c) Haider and Solon's (2005) autocorrelations are in normal typeface.

Table 2: Average Autocorrelations, 1975-1984.

Order of Autocorrelation	Table 1	Haider and Solon (2005)
1	0.84	0.89
2	0.79	0.82
3	0.80	0.78
4	0.73	0.75
5	0.72	0.72
6	0.69	0.69

Source: SLLS data.

Table 3: Autocorrelations in Log Annual Income of Men, 1951-1960.

Year	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960
1951	**1**[a]	**0.38**	**0.29**	**0.36**	**0.44**	**0.30**	**0.22**	**0.20**	**n.a.**	**0.10**
		(0.08)[b]	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)		(0.08)
	1[c]	0.64	0.30	0.22	0.22	0.31	0.35	0.30	0.31	0.30
1952		**1**	**0.43**	**0.26**	**0.28**	**0.27**	**0.24**	**0.17**	**n.a.**	**0.19**
			(0.07)	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)		(0.07)
		1	0.66	0.35	0.30	0.34	0.35	0.33	0.32	0.28
1953			**1**	**0.56**	**0.36**	**0.42**	**0.29**	**0.26**	**n.a.**	**0.36**
				(0.06)	(0.08)	(0.06)	(0.08)	(0.07)		(0.06)
			1	0.68	0.47	0.40	0.29	0.28	0.27	0.22
1954				**1**	**0.63**	**0.46**	**0.41**	**0.32**	**n.a.**	**0.27**
					(0.06)	(0.06)	(0.07)	(0.09)		(0.08)
				1	0.70	0.54	0.43	0.40	0.40	0.33
1955					**1**	**0.64**	**0.52**	**0.45**	**n.a.**	**0.39**
						(0.06)	(0.09)	(0.11)		(0.11)
					1	0.80	0.56	0.48	0.48	0.43
1956						**1**	**0.70**	**0.60**	**n.a.**	**0.46**
							(0.06)	(0.08)		(0.09)
						1	0.71	0.59	0.59	0.53
1957							**1**	**0.74**	**n.a.**	**0.52**
								(0.07)		(0.07)
							1	0.85	0.78	0.71
1958								**1**	**n.a.**	**0.52**
										(0.09)
								1	0.86	0.76
1959									**1**	**n.a.**
									1	0.89

Source: SLLS data.

a) Our autocorrelations are in bold typeface.

b) Our standard errors are calculated by means of a bootstrapping procedure.

c) Haider and Solon's (2005) autocorrelations are in normal typeface.

Table 4: Average Autocorrelations, 1975-1984.

Order of Autocorrelation	Men Aged 42-55 Born 1929-33	Women Aged 42-55 Born 1929-33
1	0.84	0.86
2	0.79	0.76
3	0.80	0.71
4	0.73	0.62
5	0.72	0.58
6	0.69	0.52

Source: SLLS data.

Table 5: Average Autocorrelations of Different Cohorts.

Order of	Men Aged 42-55		Women Aged 42-55	
Autocorrelation	Born 1929-33	Born 1939-43	Born 1929-33	Born 1939-43
1	0.84	0.79	0.86	0.81
2	0.79	0.70	0.76	0.69
3	0.80	0.66	0.71	0.67
4	0.73	0.64	0.62	0.63
5	0.72	0.64	0.58	0.55
6	0.69	0.63	0.52	0.55

Source: SLLS data.

Table 6: Average Autocorrelations, 1992-2001.

Order of Autocorrelation	Men Aged 42–53 Born 1948-50	Women Aged 42-53 Born 1948-50
1	0.80	0.82
2	0.72	0.75
3	0.67	0.70
4	0.63	0.66
5	0.60	0.63
6	0.57	0.60

Source: LINDA data.

Table 7: LINDA Variables Used to Construct Income and Earnings.

Income	
Year(s)	Variable Names
1968-77	SNIN + SJOIN
1978-79	SINK7 - AVUSKF
1980-86	SINSJO - AVUSKF
1987-90	SINK + INSJO - AVUSKF
1991	SINKF + INKAP + INSJO - UNRVA - UNRVP
1992	SINKSJO - USKNRV
1993	CSFVISJ - NUNDT
1994-97	CSFVISJ - NUNDER
1998-2002	CFVIKI - NUNDER
Earnings	
Year(s)	Variable Names
1968-70	INTJ + SJOIN
1971-73	AINTJ + BINTJ + SJOIN
1974-75	AINTJ + BINTJ + SJOIN - SPA - SPAF - DAGPE - UTBG
1976	AINTJ + BINTJ + SJOIN - SPA - SPAF - DAGPE
1977	AINTJ + BINTJ + SJOIN - SPA - SPAF - DAGPE - UTBG
1978	INATJ + INSJO - SJUKPAN - SJUKPAF - KAS...
	...- DAGPARB - OVRSOCF
1979	INATJ + INBTJ + INSJO - SJUKPAN - SJUKPAF - KAS...
	...- DAGPARB - OVRSOCF
1980-82	INATJ + INBTJ + INSJO - LIVRSKP - VARDB - SJUKPAN...
	...- SJUKPAF - KAS - DAGPARB - OVRSOCF
1983-86	INTJ + INSJO - LIVRSKP - VARDB - SJUKPAN - SJUKPAF...
	...- KAS - DAGPARB - OVRSOCF
1987-91	INTJ + INSJO - SOCERS - SJUKPA
1992	INTJ + INSJO - SOCERS - SJUKA - ASKAD
1993-97	TTJ + TSJO - TSOCERS - TSJUK
1998-2002	TTJ - TSOCERS - TSJUK

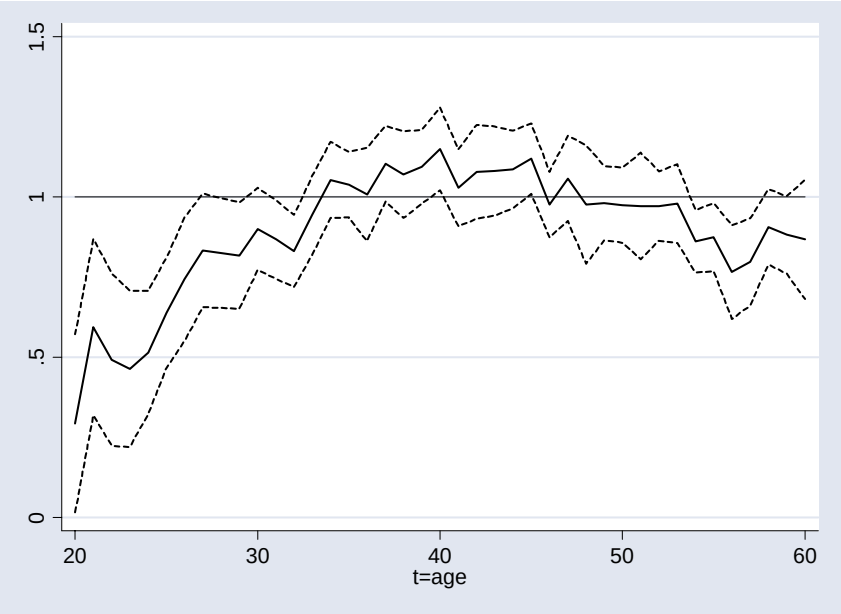

Figure 1: Estimates of λ_t for Swedish Men Born 1929-33.

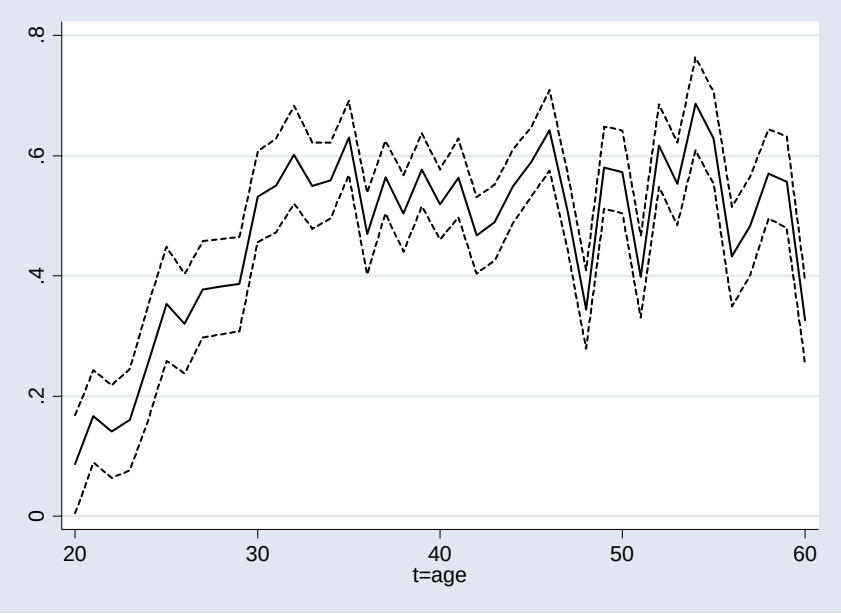

Figure 2: Estimates of θ_t for Swedish Men Born 1929-33.

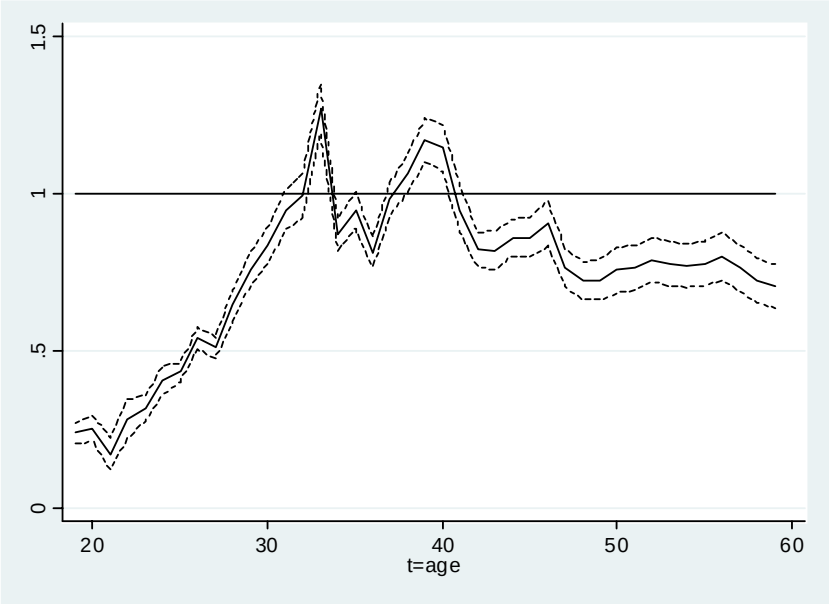

Figure 3: Haider and Solon's (2005) Estimates of λ_t for American Men Born 1931-33.

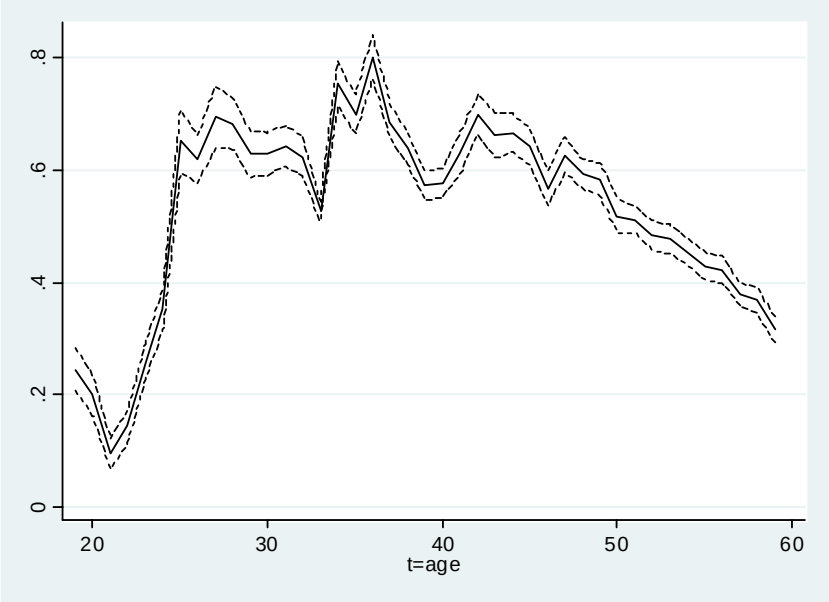

Figure 4: Haider and Solon's (2005) Estimates of θ_t for American Men Born 1931-33.

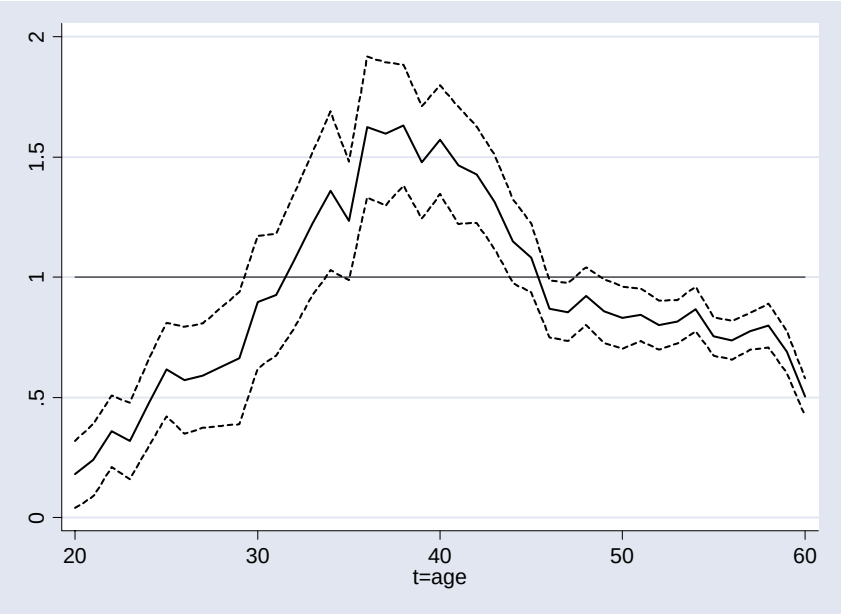

Figure 5: Estimates of λ_t for Swedish Women Born 1929-33.

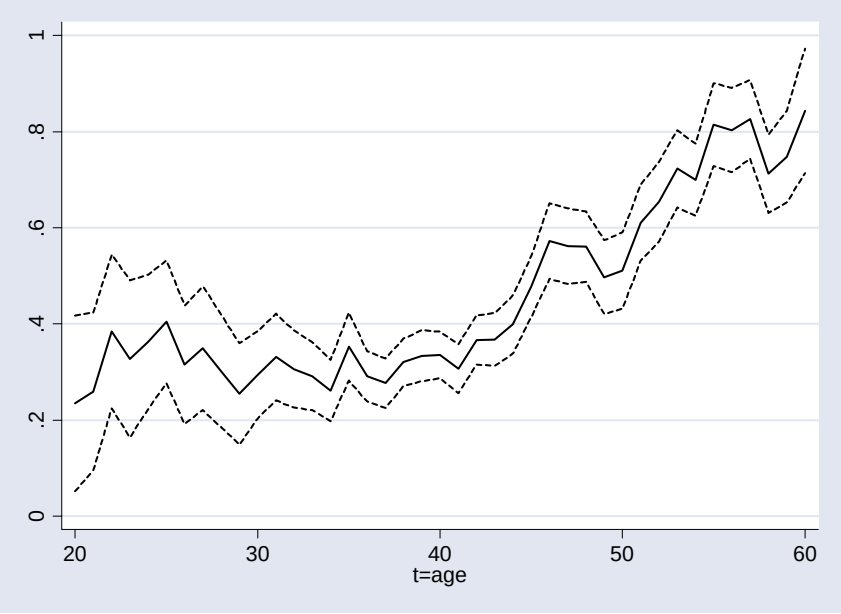

Figure 6: Estimates of θ_t for Swedish Women Born 1929-33.

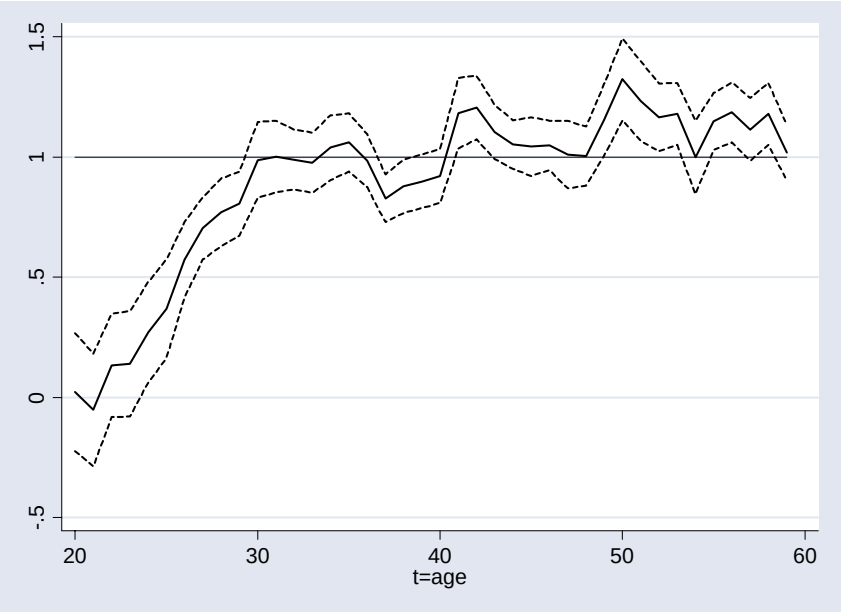

Figure 7: Estimates of λ_t for Swedish Men Born 1939-43.

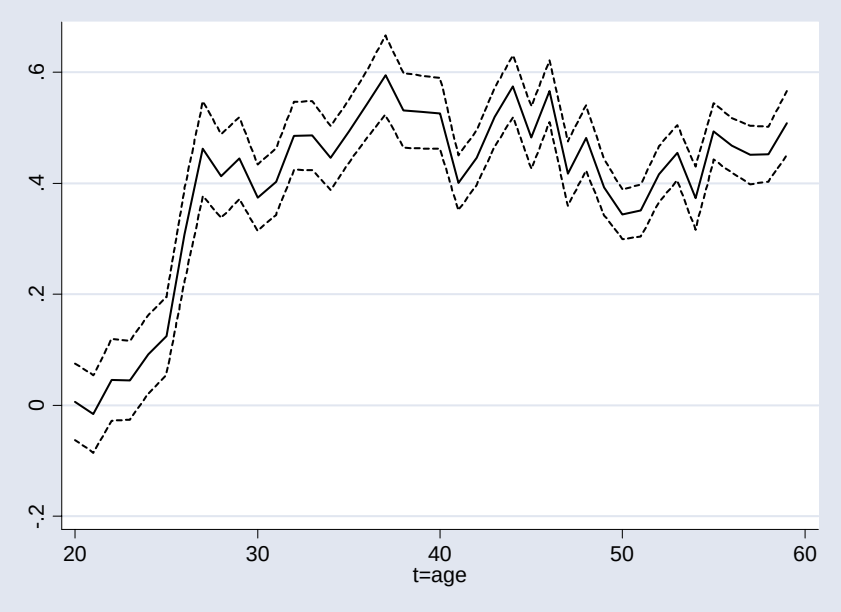

Figure 8: Estimates of θ_t for Swedish Men Born 1939-43.

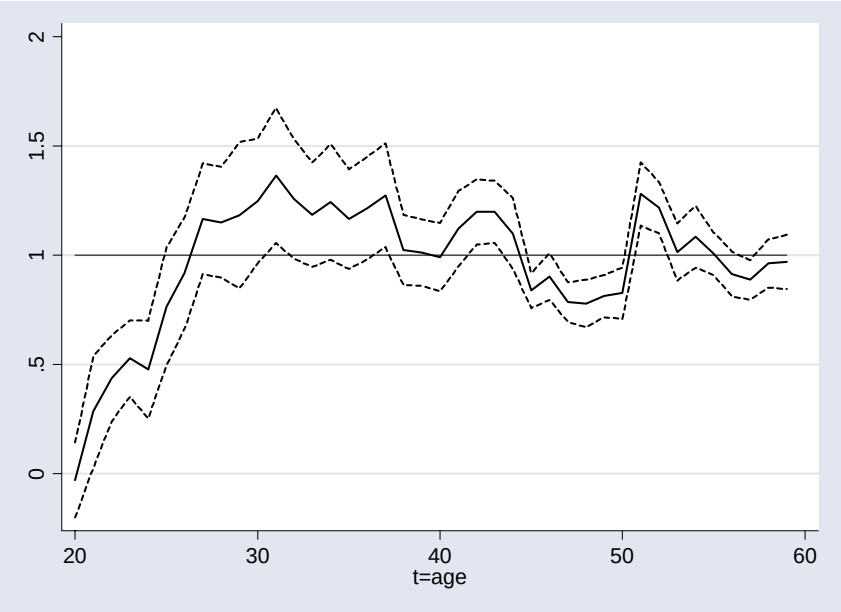

Figure 9: Estimates of λ_t for Swedish Women Born 1939-43.

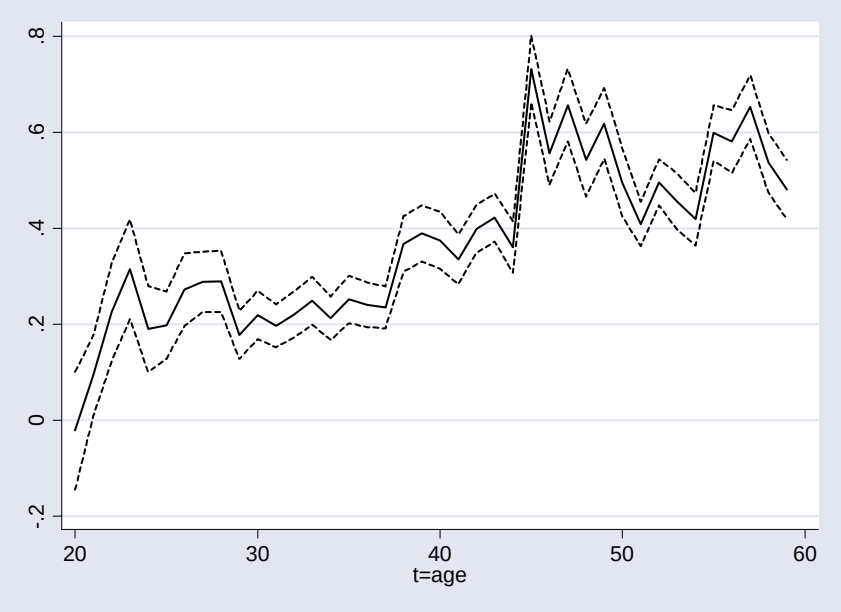

Figure 10: Estimates of θ_t for Swedish Women Born 1939-43.

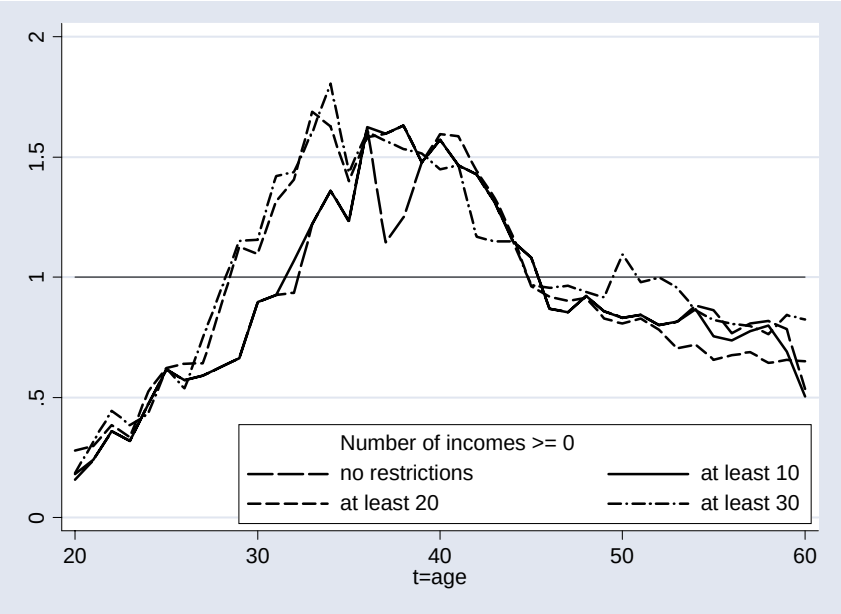

Figure 11: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Women Born 1929-33.

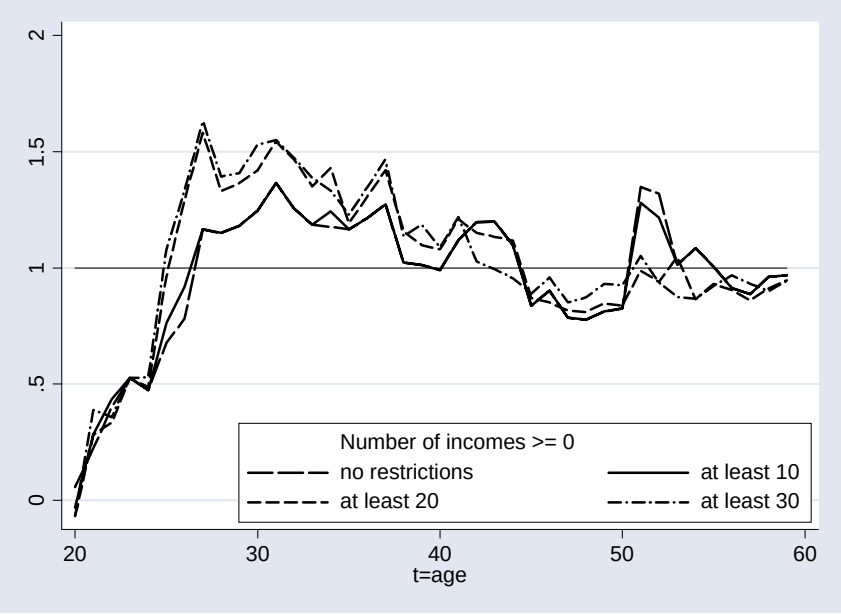

Figure 12: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Women Born 1939-43.

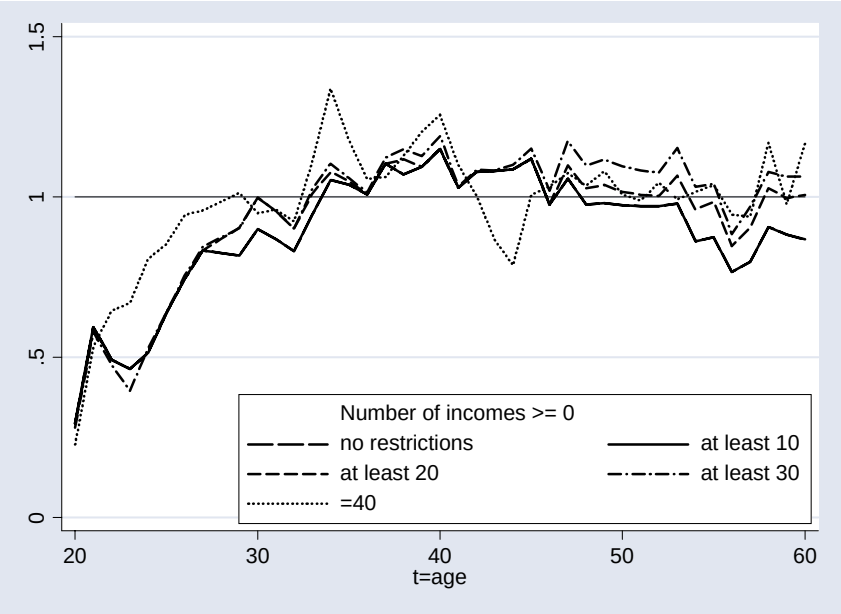

Figure 13: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Men Born 1929-33.

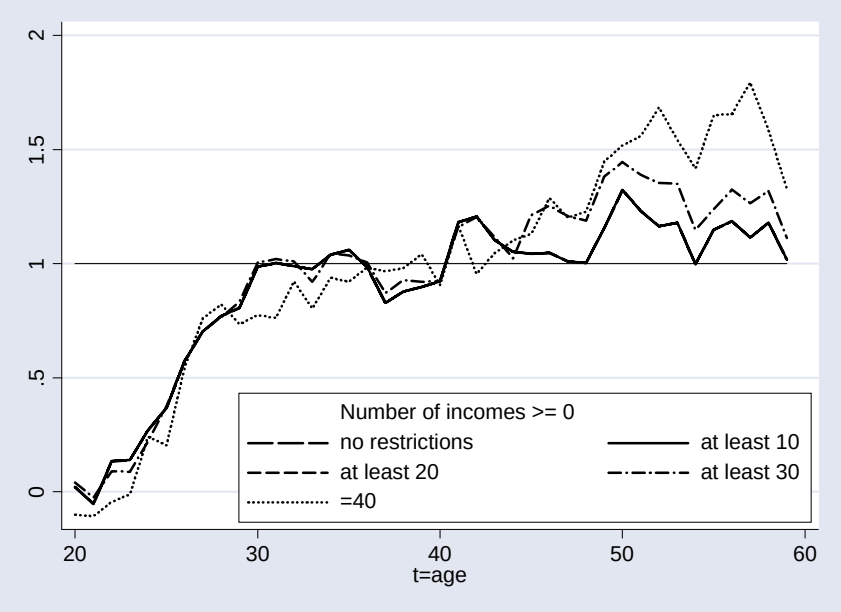

Figure 14: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Men Born 1939-43.

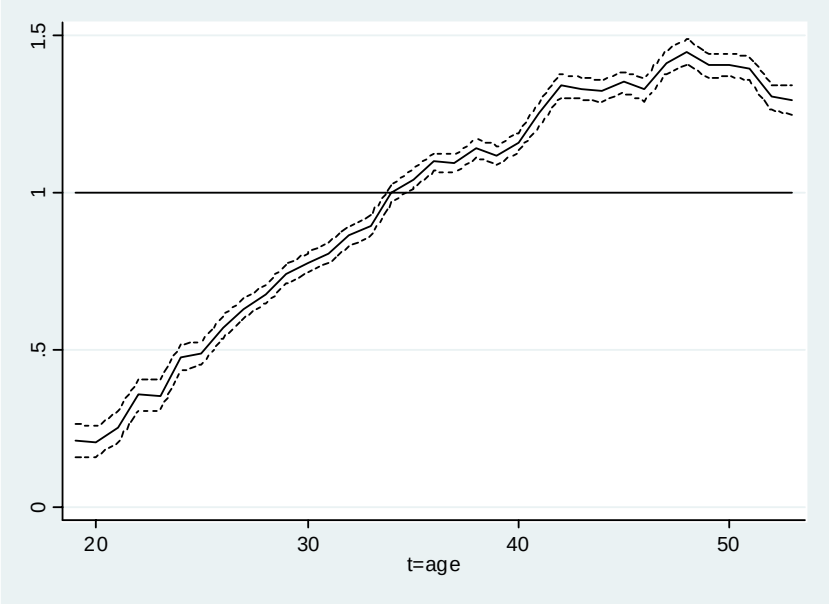

Figure 15: Estimates of λ_t for Swedish Men Born 1948-50.

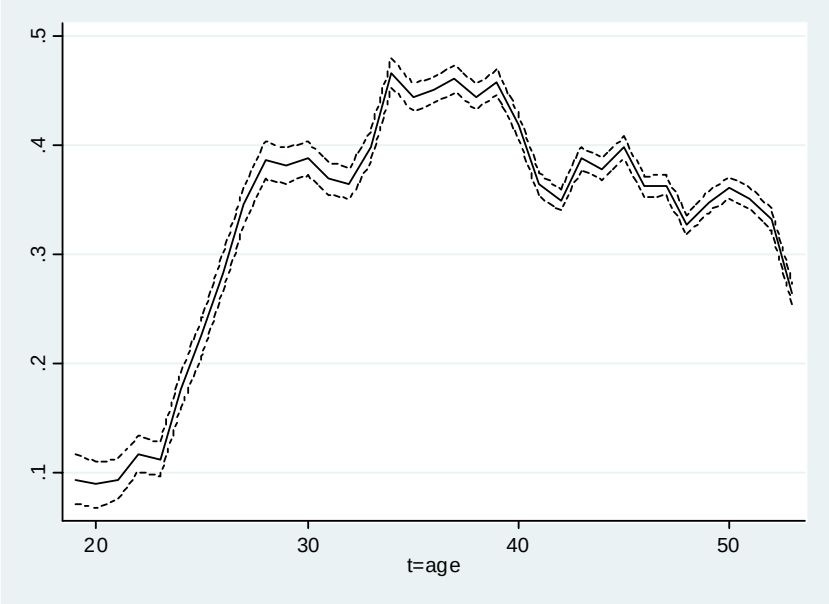

Figure 16: Estimates of θ_t for Swedish Men Born 1948-50.

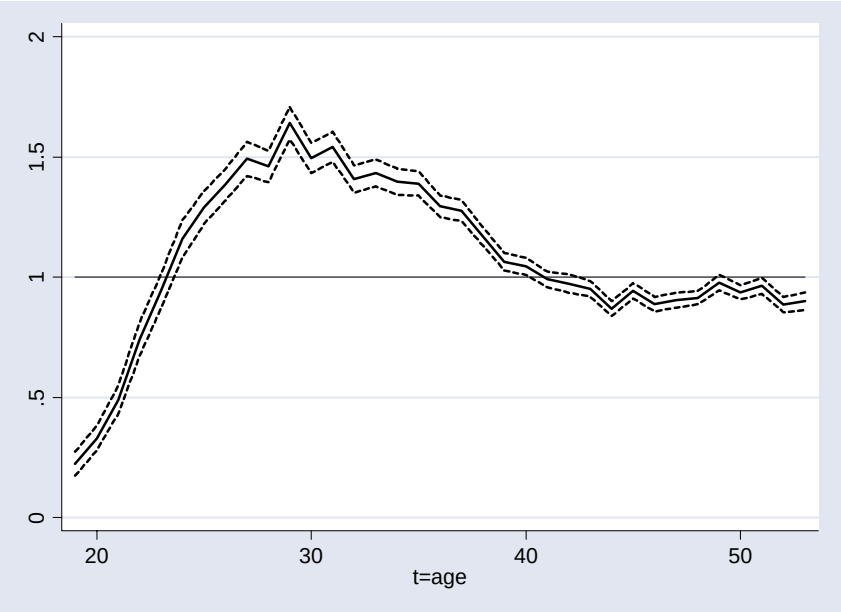

Figure 17: Estimates of λ_t for Swedish Women Born 1948-50.

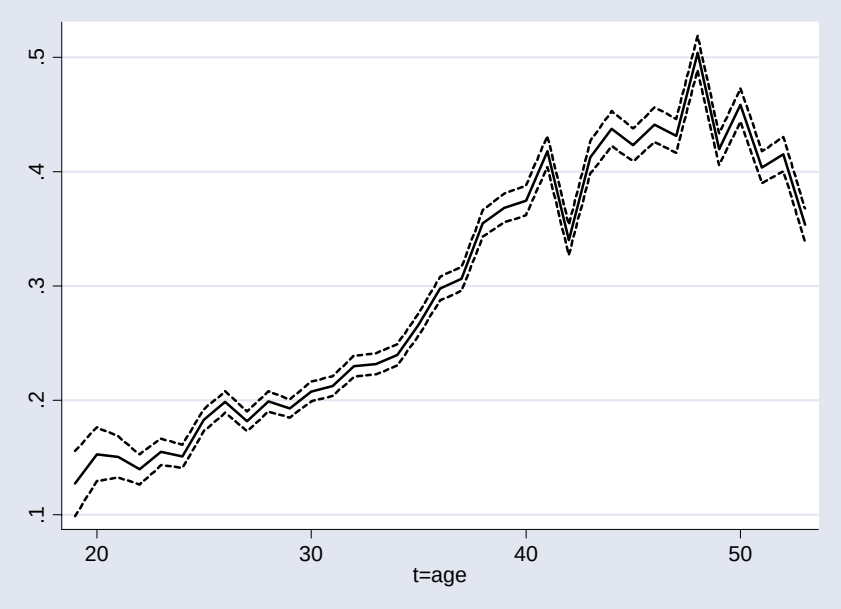

Figure 18: Estimates of θ_t for Swedish Women Born 1948-50.

Figure 19: Estimates of λ_t for 3 Cohorts of Swedish Men.

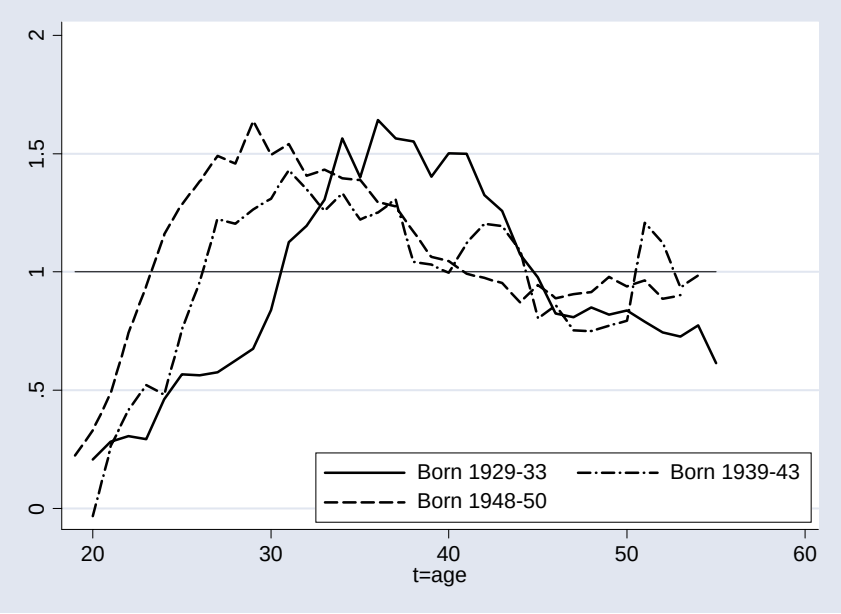

Figure 20: Estimates of λ_t for 3 Cohorts of Swedish Women.

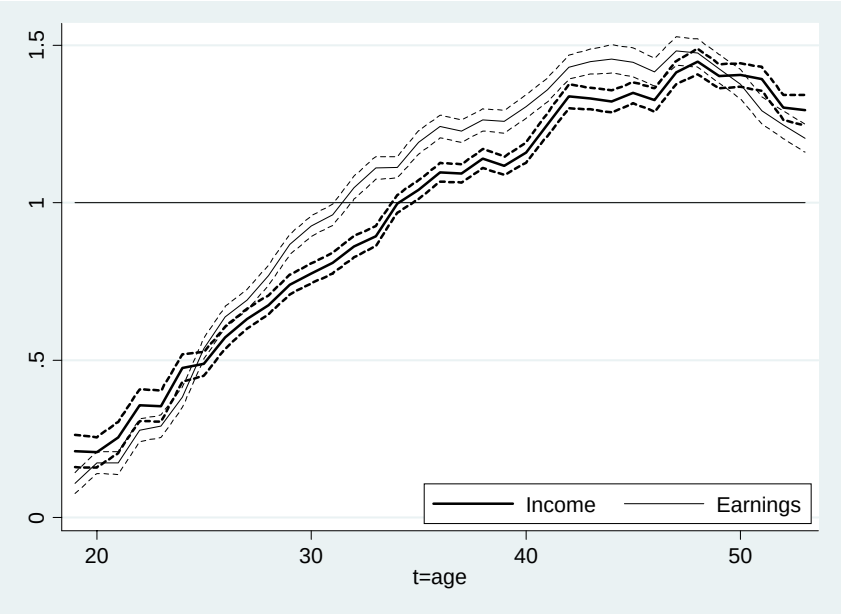

Figure 21: Estimates of λ_t for Earnings and Income of Swedish Men Born 1948-50.

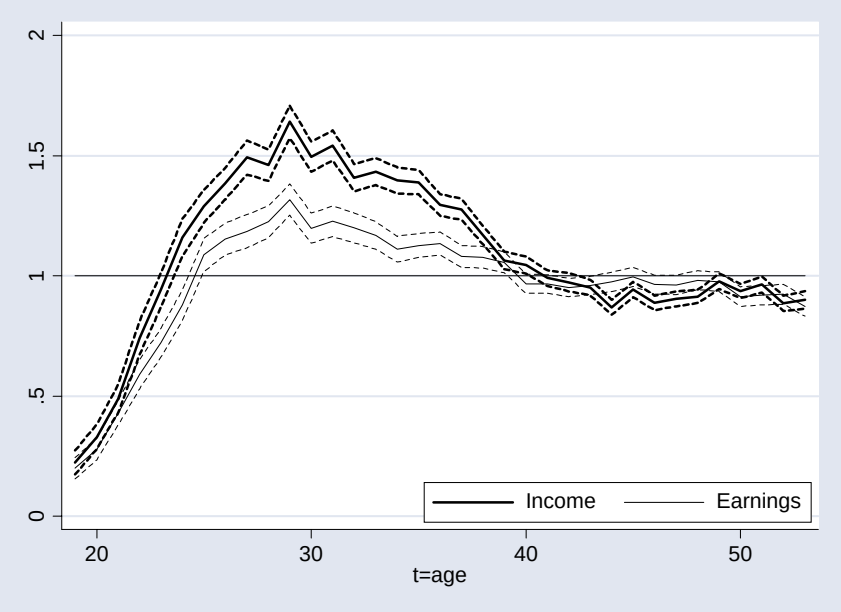

Figure 22: Estimates of λ_t for Earnings and Income of Swedish Women Born 1948-50.

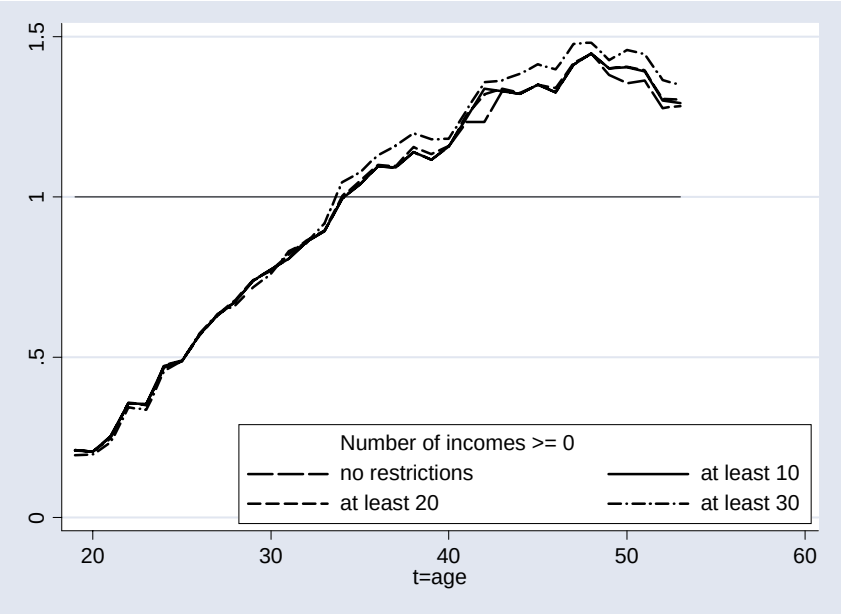

Figure 23: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Men Born 1948-50.

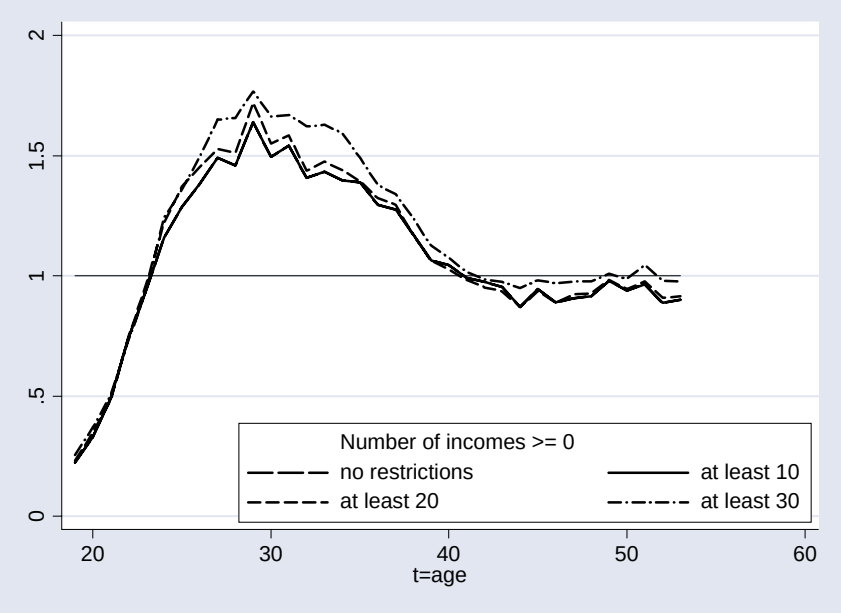

Figure 24: The Impact of Zero Incomes on $\widehat{\lambda}_t$ for Swedish Women Born 1948-50.