Portfolio Allocation over the Life Cycle: Evidence from Swedish Household Data*

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Abstract

This paper provides empirical evidence on life-cycle patterns in the asset allocation of Swedish households. Data on household portfolio allocation are collected from the HINK surveys for the period 1982-1992, and portfolio shares of different asset categories are regressed on age, period, and cohort dummies as well as socio-economic and demographic variables. There are evident differences in the age profiles for the demand of different assets. The fraction of "risky" financial assets follows a hump-shaped age profile, as does the share of total real assets. While the probability of ownership of "safe" financial assets increases over life, the weight in the portfolio has a U-shaped age pattern. This is also true for the fraction of total financial assets. Furthermore, there are differences in the asset allocation of different birth cohorts; the portfolio weight of real assets is relatively higher for the "baby-boom" generation, while younger generations are more prone to invest in "risky" financial assets.

Keywords: life cycle; asset demand; portfolio choice; cohort; differential mortality; Sweden

JEL classification: C2, D91, E21, G11, J10

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1 Introduction

A well-known prediction from the life-cycle hypothesis of saving is that the size of an individual's wealth is related to his or her age. In its most stylised form (e.g. Modigliani, 1986) the model predicts that the accumulation of wealth will display the familiar hump shape (or rather a pinnacle shape) over the individual's life cycle, where the wealth is built up during the working years and consumed during retirement. However, neither in the stripped-down version of the life-cycle model, nor in recent extensions including, for example, precautionary and buffer-stock motives for saving, is much said about the allocation of wealth across different assets. Most discussions of age-dependent asset demand can be found in the related literature on household portfolio choice, but mainly in theoretical work. In empirical investigations the issue is usually limited to an inclusion of age as a control variable in regressor sets.

Determining possible life-cycle patterns for households' portfolio choice is important in the light of the ongoing population ageing in the industrialised countries as well as the ”baby-boom” generation's potential influence on the macroeconomy, something which recently has attracted attention. Studies of the stability of social-security systems have mainly focused on the level of retirement saving of the "baby boomers". Recent reforms of social-security systems e.g. in Sweden allow a bigger role for individual and public pension funding, with more individual responsibility for the investment decisions, but there has been surprisingly little research on how the saving is, and might be, allocated across assets. Furthermore, life-cycle related shifts in the portfolio weights of real and financial assets might influence the determination of other macroeconomic variables such as relative prices on assets, or the wealth distribution.

Even though there seems to be an agreement among financial advisors of the advantage for individuals of different ages to hold portfolios with different composition – typically of less risky nature the older one gets – there has been no agreement about the rationale for this in the theoretical portfolio-choice literature (see Canner et al., 1997). In a static "mean-variance" model (Markowitz, 1952; Tobin, 1958) life-cycle related portfolio choice can only be an issue if demographics are allowed to affect underlying parameters such as the risk aversion, since this measure will govern the relative amount of wealth allocated to riskless vs. risky assets. In dynamic models the scope for life-cycle considerations is wider since the investment opportunity set can be allowed to change, and this invalidates the myopic behaviour of the investor in the static "mean-variance" world (Merton, 1971).

The fact that investment opportunity sets do seem to vary over time has stimulated interest in "long-horizon" portfolio-choice models[1] where a central feature is that the opportunity to rebalance the portfolio in later time periods will affect the portfolio choice today. Since risky assets provide a hedge against adverse shifts in investment opportunities, a "long-horizon" investor will hold larger proportions in risky assets, where the hedging component will depend on the investment horizon (Brennan et al., 1997). Related to this is the literature on background risk, i.e. risk introduced by stochastic state variables.[2] For example, Bodie et al. (1992) and Viceira (1999) show that the introduction of labour income into the portfolio-choice model, and thus the introduction of human as well as financial capital, will cause investors to rebalance the portfolio in order to mitigate the exposure to risk implicit in the human capital. What this means in terms of re-allocation of wealth between riskless and risky assets over time ultimately will depend on the nature of the stochastic properties of the assets and the state variables. However, the general notion from these models is that the amount of financial wealth invested in risky assets should fall over life, or perhaps follow a hump-shaped profile as in the model by Viceira (1999).

Some authors have focused attention on the fact that investing in real assets is a more complex decision in which an investor also acts as a consumer. Since increases in housing consumption in most cases only can be achieved by a simultaneous housing investment, this constraint will distort the portfolio choice and the investor will overinvest in housing (e.g. Brueckner, 1997). This can induce a dramatic life-cycle pattern in the portfolio shares of risky financial assets, as shown by Flavin and Yamashita (1998), since young households with large holdings of real estate relative to their net worth are highly leveraged in housing. They are therefore forced to hold a high-risk portfolio and will respond by using their net worth to reduce risk, either by paying down their mortgage or by buying bonds instead of stocks. Stock ownership will be more attractive to older households with a greater accumulated wealth and therefore a reduced ratio of housing to net worth.

A number of articles have focused on and explored the stylised finding across countries that most households hold only a subset of all available assets (e.g. Haliassos and Bertaut, 1995). Different features have been suggested as responsible for such underdiversification, including transaction costs (King and Leape, 1987), tax rules (e.g. Poterba, 1999), and short-sale constraints (King and Leape, 1998). It is easy to imagine some or all of these features to

[1] E.g. Samuelson (1994) and Campbell and Viceira (1999, 2000). See also Gollier and Zeckhauser (1997).
[2] This literature is growing rapidly, see e.g. Heaton and Lucas (2000), Cocco et al. (1998), Koo (1999), Campbell et al. (1999), and Gollier and Pratt (1996).

be correlated with life-cycle status. For example, King and Leape (1987) let the acquisition of information over time – crucial for ownership of 'information-intensive' assets like corporate equity – be proxied by age. A related matter is borrowing constraints, which bring an additional age-related dimension to the choice of allocating wealth between risky and riskless assets (e.g. Constantinides et al., 1999).

Despite the theoretical ambivalence, empirical investigations of life-cycle patterns of household portfolio choice remain scarce[3] – unlike the related matter of life-cycle wealth where much empirical work has been done.[4] The lack of research has mainly to do with the substantial data requirements needed to study this topic. Few countries collect detailed data on household asset allocation and in the countries that do the quality varies. The purpose of this paper is to study the life-cycle patterns in households' portfolio choice using a Swedish database suitable for this task – the HINK (Household Income Distribution) surveys for the years 1982-1992. The surveys are based on Swedish income tax registers and contain detailed information on income and assets, as well as a number of socio-economic and demographic conditions. Since data on roughly 10 000 households each year are available on a yearly basis – with a rotating panel component – the database is well suited for analyses of life-cycle portfolio choice.

The specific focus of this paper will be on identifying and investigating life-cycle patterns in the demand for different assets. Specific attention will be paid to make statements regarding the age effects robust to the pitfalls of cross-section analyses. Previous analyses of the asset demand (at least financial asset demand) of Swedish households have mainly utilised cross-section data. As is well known, age patterns in cross-sections are not necessarily the same as life-cycle patterns since it is impossible to separate age effects from birth-cohort effects, and Poterba and Samwick (1997) have shown that there might be quite large cohort effects in households' demand for assets.

However, using panel data raises the methodological question of how to separate the effects from age, cohort, and period. Since these variables are perfectly linearly related this can not be done without identifying assumptions. The paper includes a discussion of suggested solutions in previous studies, as well as the method used here. The main conclusion from this discussion is that recently proposed methods are not particularly useful for the present appli-

[3] Notable exceptions are the papers by Poterba and Samwick (1997) and Poterba (1998). See also the papers on household portfolios in the forthcoming anthology by Guiso et al. (2000).
[4] However, there is a large literature, initiated by Mankiw and Weil (1989), focused on real assets and the age-specific demand for owner-occupied real estate.

cation, and that they will not be suitable for handling the macroeconomic development as well as the institutional changes that occurred over the particular period.

The rather eclectic starting point for the empirical investigation is to acknowledge a number of potential determinants of a life-cycle related portfolio choice and to include related variables in the empirical specification. The general method will be to contrast results from regressions including only age, period, cohort, and family controls with a "full" specification including a number of demographic and economic variables. By comparing the estimated age effects from the two specifications it is possible not only to identify life-cycle patterns, but also to get some notion of which variables are responsible for the generation of these patterns. The investigation is limited to four aggregate asset categories: "risky" financial assets, "safe" financial assets, total real assets, and total financial assets. These categories have been in focus in previous studies, and there are, as mentioned above, theoretical reasons to expect the demand for these categories to have different age patterns.

The main results can be summarised as follows: there are clear life-cycle effects in the asset demand of the households. Not surprisingly, the most robust pattern is the one for real assets, which follows the hump-shaped pattern typically found for the demand for owner-occupied houses. The probability of owning financial assets follows more of a trend-like pattern that increases over life. However, the share of gross wealth invested in financial assets has a U-shaped age pattern. This result is almost identical for "safe" financial assets, which is an indication of the marginal role "risky" financial assets play in the portfolio of the majority of Swedish households. Accordingly, the results for risky assets are not as clear-cut as for the other asset categories. However, the overall impression is that the ownership of risky assets follows a hump-shaped profile over life.

Beside the estimated age effects, the results also indicate important cohort and period effects. Regarding real assets, the estimated cohort effects are very robust and follow a hump-shaped pattern that peaks for the "baby-boom" generations born in the 1940s. Furthermore, there are indications that the demand for risky assets is relatively higher for younger cohorts. Hence, the results here confirm the findings in Poterba and Samwick (1997) that asset demand in fact display noticeable cohort effects that are important to control for in empirical work. In addition to the age and cohort effects, the period effects are generally important in all regressions. However, the interpretation differs depending on what asset category, and what decision one is studying. For example, in some cases the period effects can be interpreted in terms of steadily increasing participation in the asset market, while in other cases the effects catch the variation of the asset prices over the period.

The results also provide some clues to the mechanisms behind the life-cycle effects. Family status, as well as the housing situation, is in general important for the portfolio choice, and there are at least some support for previous findings of a more risk averse behaviour from households headed by single females. The results also support theoretical arguments that human capital and ability to vary the labour supply will affect the demand for risky assets. Human capital, here proxied by socio-economic group membership, is important, but since socio-economic group membership is correlated with wealth, the effect is difficult to separate from a pure wealth effect. Interestingly, some effects are skewed in a way that is consistent with the stylised feature that wealthier households have lower mortality rates. Other studies have found it important to control for such differential mortality effects in descriptions of life-cycle behaviour (e.g. Jianakoplos et al., 1989).

The remaining sections of the paper start with a description of the data material in section 2. Section 3 presents some stylised features of the asset demand of Swedish households, where *prima facie* life-cycle patterns of portfolio choice are derived by cohort plots of the demand for different assets. This is followed in section 4 by the empirical analysis. Since the focus of the paper is on identifying life-cycle patterns, the section starts with a discussion of the problem of separating age effects from period and cohort effects. This overview is followed by results from the empirical analysis. The results are summarised in the fifth and final section.

2 Data description

The data used in this paper are the household income distribution surveys (HINK) for the years 1982 to 1992. The usefulness of the surveys for studies of asset demand is limited after 1992 since only wealth holdings larger than roughly $100.000 need to be declared. The HINK surveys, which are administered by Statistics Sweden (SCB), contain data for approximately 10 000 households every year. The data are collected for two panels consisting of 5 000 households, where one panel is renewed every year. Hence, in general, every household is included in the survey two years in a row. The exception is HINK 1992, where the same households as in HINK 1989 were included in order to facilitate evaluations of the tax reform 1990/91. The participating households in each panel are selected by stratified sampling of the non-institutionalised part of the Swedish population, where the primary strata are farmers, other self-employed, pensioners, and others.[5] Further description of the data is given in the appendix, which also contains an overview of the different asset categories.

Because of the major problems with the quality of data on apartment buildings, farm property and assets associated with unincorporated business, this paper will follow the conventional practice when using HINK data, and exclude households with declared wealth in these asset categories. Admittedly, the exclusion of these households is not innocuous; because of the stratified sampling the sample-restriction reduces the number of households by roughly 25% each year. Furthermore, holdings of financial assets like stocks and bonds are concentrated to wealthy households, and there is a relatively high number of self-employed and households owning commercial real estate in the upper wealth brackets. This could make the analysis of "risky" assets more difficult. In addition to the restriction described above, households for whom it is obvious that there has been a change of the household head have also been excluded from the analysis.

This paper will focus on four, aggregated asset categories. Total real assets and total financial assets are analysed separately since the life-cycle demand for these assets should differ. Also, since there is interest in investigating life-cycle patterns in the holdings of asset with different risk, the financial assets have been divided into "risky" and "safe" assets. Basically, the risky-asset measure is the sum of listed shares, other shares, and bonds, while the safe assets are bank deposits and cash holdings. The exact definitions are provided in the appendix.[6] Note that the definition of the risky-asset variable in 1991 and 1992 includes an aggregate, non-itemizable asset variable – including both financial and real assets like bonds, children's wealth, personal inventories etc. – from households that used a simplified tax form. As shown in the empirical section, the inclusion of this measure in the risky-asset variable results in large period effects for these two years.

In addition to data on asset holdings and other economic variables, the HINK surveys contain a number of socio-economic and demographic variables and the "full" regression specification includes a number of variables that are meant to capture effects that might induce a life-cycle dependent portfolio choice. See the appendix for further details and Tables A.1 and A.2 for the definitions. A weakness of the HINK surveys is that they lack data on

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[5] Up until 1985 further stratification according to income and family status was made within these groups. Furthermore, in some years the surveys have been supplemented with a special sample of households in the top of the wealth distribution. However, these additional samples have not been available for analysis in this paper.

[6] Of course, the definitions are open for discussion. As described by Pålsson (1996), bonds during this period consisted of two types – savings certificates that were basically risk free and lottery bond loans that were associated with some risk. Since it is not possible to separate them in the data, I have treated the aggregate bond

self-appreciated or implicit measures of household risk tolerance. Also, there are no readily available proxies for household background risk and the surveys lack information on the number of years of schooling – the most common proxy variable for human capital. However, there is a measure of socio-economic group membership available that does contain some information on education.

3 Life-cycle patterns in Swedish households' demand for assets – cohort plots

This section presents graphically some life-cycle features of the asset holdings of Swedish households for the period 1982 to 1992. As is well known, inference on life-cycle patterns from age effects calculated from a single cross section of data might be incorrect for a number of reasons.[7] A typical example is the effect of productivity growth, which will raise the expected lifetime income of consecutive generations or birth cohorts. Therefore, a finding of, say, relatively larger values of owner-occupied homes for the average middle-aged household compared to the average elderly household in a cross section does not necessarily imply a life-cycle pattern of reduced housing. It could just as well be the result of middle-aged households adjusting their housing stock to their relatively higher expected lifetime income. In order to make more reliable statements about life-cycle patterns one needs to follow cohorts over time and investigate the demand for housing as the cohort ages.

The analysis below investigates the asset demands of the Swedish population divided into birth cohorts for the years 1982-1992. The cohorts are aggregated into 5-year groups where the first cohort consists of households whose heads were born between 1898 and 1902 (i.e. who were 80-84 years of age in 1982), the second consists of those born between 1903-1907 (i.e. who were 75-79 in 1982) etc. Age and birth year for a cohort is defined as the mid age and birth year in the relevant age and cohort interval. To get a reasonably large number of observations for each cohort the analysis is limited to cohorts with an age of 20 to 85 (i.e. limited to households whose heads are 18-22 and 82-87 respectively), leaving a total of 15 cohorts. In each year the mean value of the variable under consideration is calculated for each cohort, the values are plotted against the middle age of the households in the cohort, and the values are connected for each cohort. This results in 15 "cohort lines".

Figure 3.1 below shows plots of the average ownership rates of real and financial assets respectively, as well as the average size of these holdings. Intuitively, one would expect the

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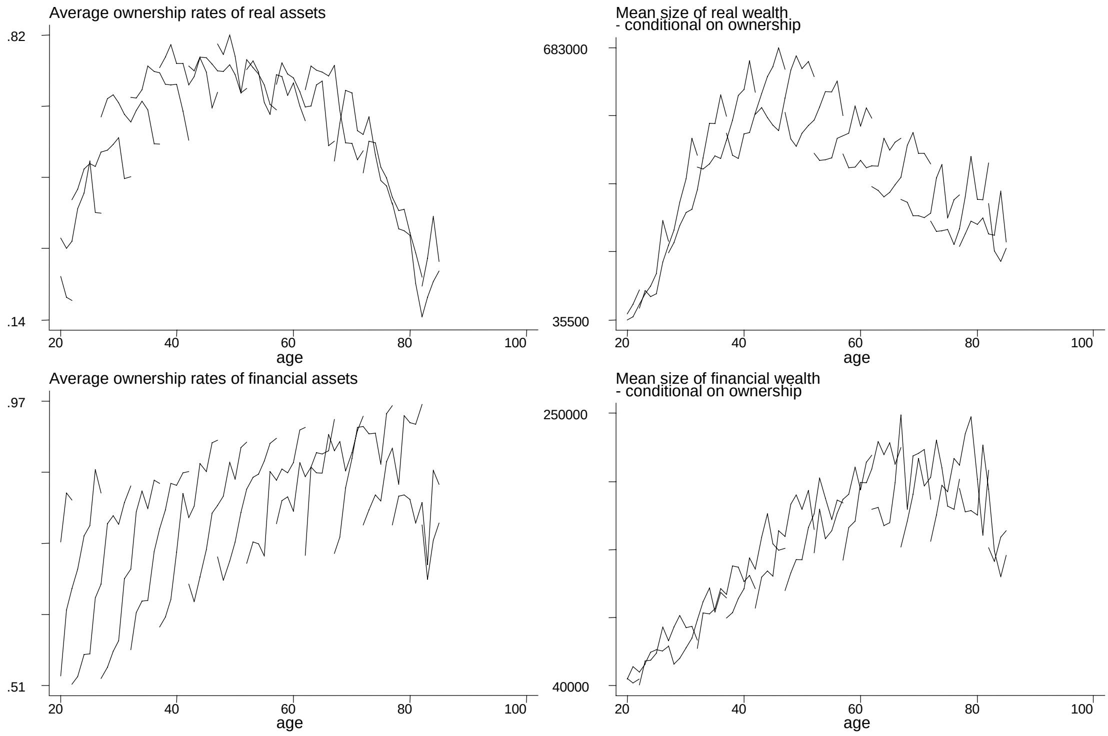
measure as a risky asset (contrary to Pålsson). A reason for this is that, in the sample, 70 % of bondholders hold stocks as well, indicating that the ownership decision for the two assets is related.

[7] See Shorrocks (1975) and Jianakoplos et al. (1989) for further discussions on this topic.

demand for real and financial assets to differ over life. For young households most of the still relatively small stock of savings is probably held in financial assets for specific purchase purposes, including saving for down payments on owner-occupied houses. The demand for financial assets can also be expected to be relatively greater for middle-aged households with grown-up children, their housing investments behind them, and the peak income years ahead. There is also the implicit prediction from the life-cycle saving hypothesis that households should prefer to liquidate real assets for consumption purposes during retirement.

Figure 3.1 Ownership rates of real and financial assets, and the size of the holdings.



Notes: see appendix for variable definitions. The mean number of observations used to calculate the mean for each cohort in the graphs in the first column is 472. For the mean size of real wealth in the second panel in the first row the mean number of observations is 296, and for the mean size of financial wealth in the second panel in the second row 370. In the conditional asset demands in the right column, outliers – defined as the values in the 1st and 99th percentiles – have been excluded.

In Figure 3.1, the graphs in the left column display the average ownership rates, while the right column shows the conditional cohort mean values of these holdings, i.e. the size of the real and financial asset holdings conditional on ownership of an asset in the category. There are clear differences in the age patterns between the two asset categories. For real assets both the ownership rate and the conditional asset demand are distinctly hump shaped with a peak around 50-55 for ownership and somewhat earlier for the conditional asset demands. In contrast, the age pattern for ownership of financial assets has a very different shape where the investments increase monotonically over the ages. It is only for the oldest households that one can detect a reduction. This pattern is the same for the conditional demand for financial assets, but the decumulation – or the levelling out of the accumulation – of the demand of the elderly appears more clearly. However, note that particular features of the age pattern for the oldest households are associated with a high degree of uncertainty since they are based on few observations.[8]

From the graphs it is apparent that also the cohort and period effects differ between the two asset groups. For example, below the age of 60, the mean ownership rate of financial assets increases for each consecutive cohort, i.e. for a given age there are apparent vertical distances between the generations. Such cohort effects are also present for the conditional real-asset demand. Focusing on the shape of individual cohort lines over time there are common patterns that shift the level of the age distribution. From the first panel in the second row in Figure 3.1 it is clear that the ownership rate of financial assets increases over time for basically all cohorts. This feature can probably be attributed to the improved data reporting over the period and not to behavioural changes; values on households' bank deposits for the latter part of the period are figures reported directly to the tax authorities by the banks. Therefore, the trended increase in bank deposits is probably the result of the households underreporting their deposits in the beginning of the period.

Figure 3.2 Portfolio shares of real and financial wealth conditional on ownership.

Notes: see appendix for variable definitions. The mean number of observations used to calculate the cohort means are 303 for real wealth, and 379 for financial wealth.

Even though the graphs above give an impression of possible cohort and period effects it is not possible to identify separate effects of age, cohort, and period in a strict sense. Generally, in cohort graphs, the age effect is interpreted as the common shape of the different cohort lines with respect to age, horizontal distances between the different cohort lines measure cohort effects, while fluctuations over time that are similar to all of the cohort lines are period

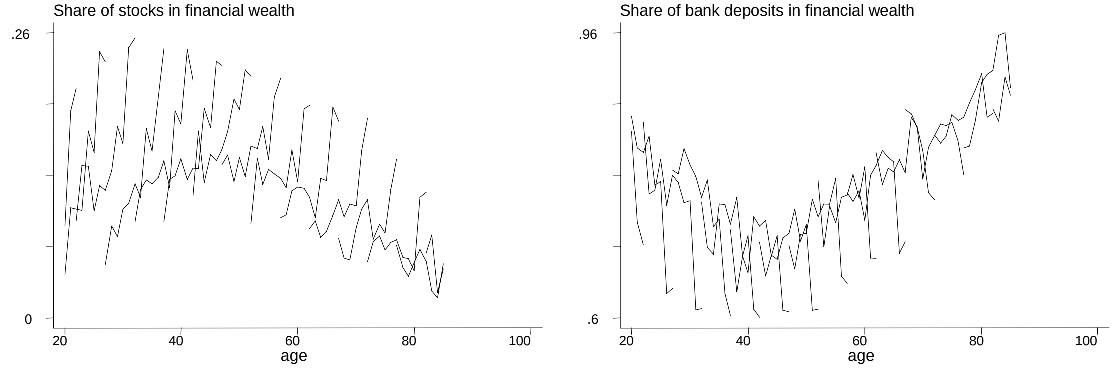
[8] The average number of observations for the two oldest cohorts is generally between 70 and 100 for the plots of

effects. However, since age, birth year, and time are perfectly linearly correlated the effects are not separable from each other. So, strictly speaking, when the cohort lines are not flat we can at most say that there are age and/or period effects, and when there are vertical differences between the cohort lines that there are cohort/period effects. The identification problem of age, period, and cohort is discussed in more detail in section 4.1.

The same cohort analysis as above but for portfolio shares of real and financial wealth in gross wealth is displayed in Figure 3.2 above. The weight of real wealth in the portfolio increases up to the age of 40-45 after which it decreases, although there is some indication of an upturn for the eldest. The pattern for the share of financial wealth in the right panel is basically the mirror image of that of real wealth (even though the two shares need not sum to one here) with a U-shaped life-cycle pattern that reaches a trough around the age of 40. This is an indication of the dominating role real assets play in households' portfolios. Although the demand for financial wealth has an altogether different age pattern than that for real wealth (c.f. Figure 3.1) the pattern for the *weight* of financial wealth basically becomes the inverse of the age pattern for the demand for real assets.

Figure 3.3 below plots the share of stocks and bank deposits in total financial wealth. As mentioned in the introduction, there has been some discussion in the literature of how the risk characteristics of a household's portfolio should change over life. Obviously, such a discussion would be redundant if no apparent shifts in the portfolios can be observed. Therefore, the results in Figure 3.3 are particularly interesting since there is a clear difference in the age pattern between these two asset types. For bank savings the age pattern is distinctly U-shaped with a trough around the age of 45. The pattern for stocks, on the other hand, is somewhat hump-shaped with a decreasing weight for older age groups.

Figure 3.3 Share of stocks and bank deposits in financial wealth.



Notes: see appendix for variable definitions. The mean number of observations used to calculate the cohort means are 379.

financial assets.

4 Life-cycle patterns in households' demand for assets - empirical analysis

This section starts with a discussion of the identification problem in regression models including separate effects of age, period, and cohort. Some suggested solutions in earlier literature are discussed, as well as the approach used here. Finally, regression results are displayed and commented.

4.1 Identification of age, period, and cohort effects

Following the mean behaviour of cohorts over time, as in the graphs in section 3, is one way of getting some insights into life-cycle patterns. A more elaborate way to separate age, cohort, and period effects is by means of regression analysis. However, as suggested in section 3, the identification of the separate effects is essentially a question of imposing restrictions on how all three effects are related. Since the variables satisfy an exact linear relationship, i.e. *calendar year – age = birth year*, a linear regression on the variables is impossible without some form of identifying assumptions that will separate the effects. The perfect collinearity will also limit identification of any polynomial and/or interaction terms of the variables included in the regression (see further Fienberg and Mason, 1985).

If the regression is specified in terms of dummy-variable sets instead of the "trend" variables age, birth year, and calendar year, the identification problem still remains of course, even though the reason is not as easily seen through. Suppose we specify a regression with time dummies (one for each of T years), cohort dummies (one for each of C cohorts), and age dummies (one for each of A years of age). In order to avoid the trivial problem that each set sums to one, one dummy in each category is dropped:

$$y = \alpha_0 + \sum^{T-1} \delta_t D_t + \sum^{C-1} \gamma_c D_c + \sum^{A-1} \beta_a D_a + \varepsilon \tag{4.1}$$

The identification problem, corresponding to the perfect linear relationship between age, cohort, and period, is the following: for a household with time-dummy variable $s = 1$, the j^{th} age dummy is equal to one if and only if the $(s\text{-}j)^{\text{th}}$ cohort dummy is equal to one (Heckman and Robb, 1985). Hence, in a regression, there will be a linear dependence among the columns of the regressor matrix, and the equation can not be estimated.

Following Deaton and Paxson (1994), this point can be made more concrete by rewriting equation (4.1) in terms of the dummy-variable matrices:

$$y = \iota\alpha + TD\delta + CD\gamma + AD\beta + \varepsilon\,, \tag{4.2}$$

where ι is a vector of ones, and *TD*, *CD*, and *AD* are the year, cohort, and age-dummy matrices with δ, γ, and β the corresponding parameter vectors. Let λ_n be the (transpose of the) vector (1, 2, 3, …, *n*), and define such a vector for year, cohort, and age with *n* equal to the number of columns in each of the three dummy matrices. Then there will be the exact linear relationship:

$$CD\lambda_{C-1} = TD\lambda_{T-1} - AD\lambda_{A-1} + n_{(C-1)}\iota\,, \tag{4.3}$$

where the constant term equals the total number of cohorts in the first observation period, *C*, minus one. Because of this linear relationship the parameters in equation 4.2 are not identified.[9]

This study will follow the procedure of identifying age, period, and cohort effects by imposing the restriction that the effects of age and cohort is the same, or at least not systematically different, for five-year intervals. To be specific, instead of dummy variables for each age and birth year, there are dummy variable for ages 20-24, 25-29 etc. and for birth years 1898-1902, 1903-1907 etc. This assumption solves the identification problem since (4.3) no longer will hold, but identification is achieved at the cost of some inability to track households over time correctly (Fienberg and Mason, 1985). Note that with this modelling scheme there is no need to put restrictions on the time effects. An identifying assumption often used in the empirical literature that deals with life-cycle consumption and wealth accumulation is an approach pioneered by Deaton and Paxson (1994) in which the year effects are restricted to sum to zero and be orthogonal to a linear time trend. The time effects are then interpreted as stationary macroeconomic shocks that affect all households in the same way, and linear trends in the data are attributed to age and cohort effects.[10] The choice of letting age and, particularly, cohort effects pick up trends in the data are motivated by the prediction from the life-cycle/permanent income framework that productivity-growth will make lifetime earnings higher for each consecutive cohort.

However, to consider the general macroeconomic development stationary during this particular period is hardly an innocuous assumption since we know, for example, that both the real estate and stock markets boomed during the period. So, in order to clear the data from any trends due to this feature, the year effects should be allowed to pick up the evolution of the

[9] For a more detailed analysis of this point, see Kapteyn et al (1999).

[10] Due to the perfect collinearity between the age, period and cohort variables, any linear trend in the data can either be attributed to period effects or to a combination of age and cohort effects. See example in Deaton and Paxson (1994).

asset-prices.[11] Also, by leaving the time effects unrestricted they are free to capture changes in variable definitions and/or systematic improvements of the data quality over time (both of which are known to be present in the data here). Thus, for this particular study it seems wise to include the year effects unrestricted.

Cohort analyses, and the study of the identification problem of age, period, and cohort effects, have a long tradition in applied work in the social sciences (e.g. Mason and Fienberg, 1985). Solutions to the identification problem have basically been of two types: either restrict the relative effects of the age, period, and cohort parameters in some way, or use predictions from underlying theory to impose structure on some of the effects. Examples of the first approach is to simply ignore one of the effects, to put restrictions on time dummies as in Deaton and Paxson (1994) and/or to aggregate age and cohort dummies over some interval (used here), or to combine aggregate dummy variables for one effect with some polynomial in the "trend" variable for another effect.

In contrast, the second identification approach is based on the notion that the identification problem to some extent is an artificial problem in that it arises from an inability to model the underlying effects that age, period, and cohort are meant to approximate – variables that do not necessarily share the identification problem. Typically, age effects are supposed to catch changes in preferences or state variables over a household's life cycle, cohort effects particular features of these preferences or state variables caused by the household's formation during a certain period in history, and the period effects are supposed to reflect the general macroeconomic environment over time. So, one way of identifying the effects is to use implications from underlying theory and model one or more of the age, period, and cohort effects. In the empirical literature on wealth accumulation over the life cycle the structure is normally imposed on the cohort effects and ascribed to productivity growth. Examples of different approaches that use this assumption can be found in King and Dicks-Mireaux (1982), Jappelli (1999), and Kapteyn et al. (1999).

Articles that advocate the modelling of cohort effects have proposed that this is a better way of determining the causes behind cohort differences, and that simple cohort-dummy variables can not distinguish between changes in general economic conditions and changes in preference parameters. While using theoretical predictions to bring more of structural interpretability to the cohort effects is an appealing idea, finding variables to proxy the cohort effects is a very difficult task; if the proxy variables are based on macroeconomic variables

[11] The evolution over time of the individual cohort traces of both mean holdings of real wealth in Figure 3.1 upper-right panel, and stocks in Figure 3.4 left panel, resemble time plots of the price indices of the two assets.

these will definitely be plagued by business-cycle noise, or other changes in general economic circumstances. Also, stipulating *a priori* what the cohort effect is due to might not provide the empirical flexibility necessary to pick up erratic cohort features – something cohort dummies actually might do.

Naturally, the choice of restrictions on age, period, and cohort effects must be made with reference to the particular application (as well as data material) at hand, and to one's *a priori* beliefs about the effects. But while productivity growth is a natural candidate for cohort effects in related studies of life-cycle wealth/saving, it is not clear that it would be in the present asset-demand application. Certainly, one can imagine arguments why higher lifetime earnings will affect the probability to invest in different assets. However, as motivated by the brief theoretical exposition in the introduction, cohort effects in households' portfolio choices would probably be better captured by features that in systematic ways have faced members of different cohorts with different investment opportunity sets, or that have shaped their attitudes regarding the stochastic properties of certain assets – things that might depend on the particular circumstances of an asset's market introduction, on the household's experience of a particular market episode, or on institutional properties such as tax incentives or specific policy measures during certain eras.[12] Furthermore, there might be cohort effects of the Easterlin-type in that relative cohort size will influence the labour market outcome of the cohort, which in turn will affect the behaviour of cohort members (e.g. Easterlin et al., 1993).

4.2 Outline of the empirical section

Estimation of portfolio-choice models raises a number of methodologically important issues that have been dealt with in different ways in the literature. The paper by Miniaci et al. (1999) offers a thorough discussion of these issues. Briefly put, since the portfolio choice involves both decisions of which assets to include in the portfolio and how much to invest given this choice, the investigation of the second choice in isolation can lead to biased results because of data censoring. Potentially, this is an important problem given the fact that the great majority of households actually invest in very few asset categories. Because of this, some papers limit the analysis to the asset-ownership decision, while others incorporate both decisions by using estimation methods for censoring and sample-selection. The econometric analysis in this paper will be kept fairly straightforward, focusing on standard Tobit regressions of the portfolio shares of the aggregated asset categories on the pooled data set. In order to control for house-

[12] Two examples: in Sweden, investments in new owner-occupied dwellings were subsidized in different forms from the early 1970s to the early 1990s, and credit markets were deregulated in the mid 1980s.

hold heterogeneity fixed-effect regressions (without censoring) on the portfolio shares have also been made. More refined econometric analyses using, for example, semi-, or non-parametric estimators that can deal with household-fixed effects within a censoring framework (described in Miniaci et al., 1999) are left for future research.

4.3 Tobit regressions

As usual in studies of household demand, there is a question of how to deal with households with "zeros", in this case households who choose not to invest in a particular asset. We would expect that some of these households are at a corner solution, and that they actually would like to sell the asset short, i.e. to hold a negative share of the asset. Since we are dealing with portfolio shares here, there will also be households with "ones", i.e. with all of their wealth in one asset. The argument is analogous for these households and some of them probably would like to share larger than one. The standard method to deal with this issue is to imagine an underlying latent "desired" portfolio-share, which do not have restrictions on the values it can take. Since the observed variable is restricted to lie between zero and one, we are in fact observing a truncated variable, and the standard Tobit-estimator can be used to deal with the censoring of the sample. Failure to deal with this censoring will lead to inconsistent estimates of the population parameters.

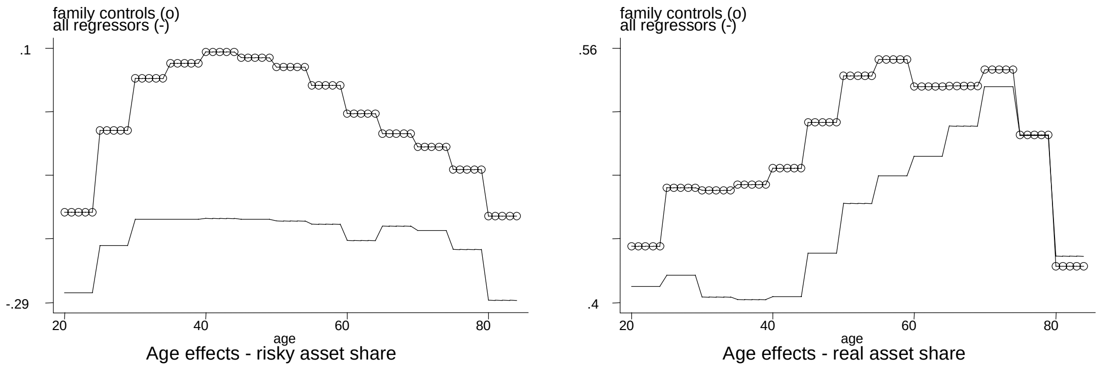
The standard Tobit framework assumes that the decision to invest is exactly the same as the decision how much to invest, something that might not necessarily be the case. In order to investigate this assumption, logit regressions of the ownership decision in isolation have also been made, and the results are displayed in appendix B. Furthermore, an investigation of the two decisions in unison, allowing the parameters in the discrete and continuous decisions to differ, has been made using the standard Heckman 2-step estimator. Results from these regressions are commented on below.

Figure 4.1 below plots the estimated age effects from Tobit-regressions of the fraction of risky and real assets on age, cohort, and time dummies as well as a number of socio-economic and demographic variables.[13] Since the results for the safe and financial asset shares are just one minus the results for the risky and real asset shares respectively, these results are not displayed below. In addition to the estimated age effects in the "full" model, Figure 4.1 also

[13] To avoid that the dummy sets sum to one, one age, cohort, and year dummy have been excluded in the regressions. All results should therefore be interpreted relative to this "reference household", which is a household whose head was 20-24 years old in 1982, i.e. who was born between 1958 and 1962.

shows, as a point of reference, the estimated age effects from regressions including just age, period, cohort, and family controls.

Figure 4.1 Age effects from Tobit regressions – risky and real asset shares



Notes: Patterns are scaled with the constant. Reference group is the group aged 20-24. The effects are the estimated parameters from the Tobit-regression, i.e. these are age effects for the underlying latent portfolio shares.

As seen from the left graph the age effects for the share of risky assets, like the cohort plot for stocks in section 3, is hump shaped with an increasing share of risky assets up to the age of 40, and a declining share after the age of 60. The estimated age coefficients are generally well determined with only a few individually insignificant. Hence, there is support for theoretical models that predict a demand for risky assets over life that first rises and then falls. The result in Pålsson (1996), that age is positively correlated with risk-averse behaviour, is partially confirmed here since there is evidence of a declining share of risky assets in the portfolios of elderly Swedish households. It is also apparent that addition of economic and demographic variables goes some way towards explaining this life-cycle pattern – the hump becomes much less pronounced when these variables are included.

The right-hand graph in Figure 4.1 above shows the age effects from the regression on the share of real assets in gross wealth. The familiar hump shape, often found for the demand of owner-occupied houses, appears clearly and the individually significant effects from the middle aged confirm the relatively higher demand of these age groups. Interestingly, the estimated age effect in the regression with a “full” regressor set is more skewed, with a peak much later in life. In all probability, this is an effect of the positive correlation between wealth and longevity/mortality, which makes it more likely that we observe relatively wealthier elderly households. This effect should be accentuated for real assets since people living in nursing homes or old people’s homes are excluded from the sample population in the HINK surveys. So, conditional on variables such as education, income etc. that can explain much of the general hump shape, we are left to explain the effect of the differential mortality.

Note that this mortality effect to some extent is controlled for by the inclusion of socio-economic group dummies. The negative correlation between class and mortality in Sweden has been confirmed in a number of papers in the field of medical sociology (e.g. Vågerö and Lundberg, 1995). According to these studies, blue-collar workers and junior white-collar workers have a lower survival probability, and are more likely to experience bad health, than executives and white-collar workers in intermediate positions. Also, these differentials remain after retirement. Hence, by including the socio-economic dummies the mortality effect is controlled for in the working population. But since retirees are classified as a single socio-economic group in the HINK surveys, the dummies do not pick up the differential mortality in the retired population. Since the regression results seem to be influenced by these wealth-related effects it would seem natural to include net wealth as a regressor, as is frequently done in the empirical asset-demand literature. However, this will most certainly give biased results since wealth is endogenous in the determination of the asset demand, and it will probably distort any life-cycle results since wealth in itself has a very distinct life-cycle pattern.[14] Nevertheless, to highlight the impact of wealth on the estimated life-cycle effects, regressions with net worth included have been made, and it turns out that it is mainly for the ownership decisions that the results change. These results are commented on in the last section.

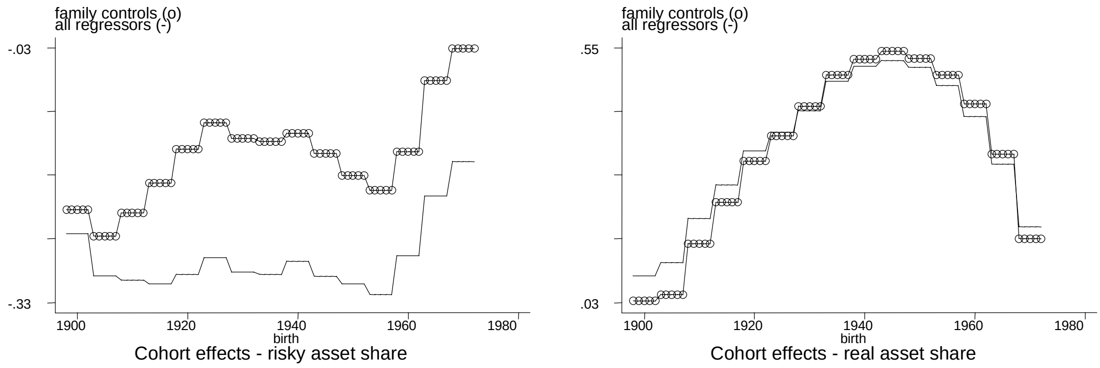
Beside the age effects, some interesting cohort and period effects appear in these regressions. Figure 4.2 displays the estimated cohort effects for risky and real assets. For the share of risky assets in the left panel, there is an increasing and significant effect of cohorts born after 1948, which could be a result of the financial deregulations and money-market expansion in the mid 1980s. Ownership of stocks increased dramatically among households from 1975, mostly due to an increased popularity of investments in mutual funds during this period. Since the demand for financial assets is relatively higher in young adulthood (c.f. Figure 3.2 and Figure 4.1 left panel), it is not surprising to find higher shares of risky assets for the cohorts that entered into young adulthood at the time of the financial market boom of the 1980s.

The cohort effect for the share of real assets in the right panel in Figure 4.2 is remarkably stable across regressions, and basically all cohort coefficients are individually significant. The cohort pattern is hump-shaped and peaks for the baby-boom cohorts of the 1940s. This could be explained by historical features of the housing market; at the time the generations born in the 1940s entered the family-building age in the 1970s, the relative advantage of home ownership compared to other housing tenures was large. High inflation, rising marginal tax rates that

[14] Modelling the determination of wealth accumulation and the portfolio choice jointly, as in Hochguertel et al. (1997), has been left for future research.

increased the value of tax deductibilities of mortgage-interest payments, and expectations of high future house prices, made debt-financed investments in real assets attractive. The tax reform of 1983-85 reduced the value of the tax relief on interest payments, and the user cost of owner occupancy was increased further by the tax reform of 1991. This could explain the decline in relative size of the real-asset share for post baby-boom generations.

Figure 4.2 Cohort effects from Tobit regression – risky and real asset shares



Notes: estimation results for the cohort-dummy variables. Patterns are scaled with the constant. Reference group is the group born 1958-1962. Note that these are cohort effects for the underlying latent portfolio shares.

As for the estimated period effects (not displayed), there are particularly large effects for the last two years of the period for the risky (and safe) asset shares. Incentive effects of the tax reform in 1991 might explain this since it, at least in theory, made financial-asset investments more interesting for households. However, there is another more trivial explanation – also due to the tax reform. As described in the data section, the quality of the wealth variables in the HINK-data was reduced after the reform, and the large period effects in 1991 and 1992 could simply be the result of the inclusion of the "other-asset" variable among the risky assets. For the real-asset share, the estimated period effects mostly reflect the pattern in the real house price index, with the upturn in 1985-86 clearly visible as a kink in the pattern of the estimated effects.

Effects from other variables included in the regressor set, the inclusion of which is commented on in the data appendix, is displayed in Table 4.1 below. Age, cohort, year, and income effects are left out in order to keep the table within reasonable bounds. From Table 4.1 it is apparent that education can play a large role for the demand for risky assets since the share of risky assets increases with education level. Note, however, that these dummy variables do not really control for years of schooling, but for socio-economic group membership. So the effect can not be solely attributed to higher education, but could also be an effect of the high correlation between socio-economic group and wealth. In comparison, the effect of socio-economic group on the demand for real assets is reversed, with every group, except one, investing a smaller share of their wealth in real assets. This effect is probably intimately related to the housing-consumption constraint discussed in section 1. Because of the constraint, households typically overinvest in owner-occupied houses, and for households with little other wealth this asset will dominate the portfolio.

Table 4.1 Regression results, Tobit regressions

	Risky asset share		Real asset share	
	Coefficient	Std error	Coefficient	Std error
Married	-0.045[a]	0.008	0.086[a]	0.008
Single fem	-0.050[a]	0.007	-0.174[a]	0.007
Kids	0.004	0.003	0.056[a]	0.003
Mortg/Inc	-0.00002	0.00004		
Marg tax	0.003[a]	0.0002	-0.0002	0.0002
Vocational	0.030[a]	0.008	0.032[a]	0.007
Junior	0.100[a]	0.008	-0.037[a]	0.008
Middle	0.133[a]	0.007	-0.022[a]	0.007
Upper	0.157[a]	0.008	-0.067[a]	0.008
Unspec	0.067[a]	0.009	-0.057[a]	0.008
Business	0.179[a]	0.013	0.106[a]	0.012
Retired	0.037	0.019	-0.004	0.018
Student	0.152[a]	0.021	-0.238[a]	0.024
Lfp	-0.023[a]	0.006	0.047[a]	0.006
Metro	0.027[a]	0.004	-0.088[a]	0.004
Co-op	-0.007	0.007		
Renter	-0.046[a]	0.005		
Constant	-0.275[a]	0.025	0.410[a]	0.022
Test age	5.50[a]		3.7[a]	
Test cohort	5.76[a]		31.24[a]	
Test year	141.04[a]		66.6[a]	
Test inc.	45.35[a]		47.69[a]	
# obs	55597		60407	
Se	0.432		0.440	
Ps. R^2	0.09		0.187	
# left	20326		12923	
# right	1785		4810	

Notes: explanations of the abbreviations and description of the variables in appendix A. Superscript "a" means that the coefficient is significantly different from zero at the 1%-level and "b" at the 5%-level. The tests of the age, cohort, and year dummies, as well as the income variables, are Wald tests of the joint significance of the coefficients. "# obs" is the number of observations, "Se" is the standard error of the regression, and "Ps. R^2" is a pseudo-R^2 measure calculated as one minus the ratio of the log likelihood from the regression and the log likelihood from a constant-only model. "# left" is the number of left-censored and "# right" the number of right-censored households. Note that the coefficients are the marginal effects for the underlying latent portfolio share. All estimations performed in STATA 6.0 (StataCorp, 1999).

Another result that confirms previous findings in the literature concerning investment in risky assets is the negative effect of single female households. This result is in line with Pålsson (1996) and Jianakoplos and Bernasek (1998) who find that single women exhibit a relatively higher degree of risk aversion in financial decision making than single men do. Note also that the number of full-time working adults reduces the share of wealth allocated to risky assets. Because the potential for increasing the labour supply is limited for full-time workers, this result confirms the theoretical prediction that reduced possibility to vary the labour supply will make investments in risky assets less attractive (c.f. Bodie et al., 1992). Regarding the demand for real assets, the family situation is important, as could be suspected. The second column in Table 4.1 shows that single female households invest less in real estate than single male households do, while couples invest more. The demand also increases with the number of children and the number of adults working full time. Furthermore, households living in metropolitan areas have relatively less of their wealth in real assets, but more in risky assets. Finally, estimates regarding the income variable show that all income deciles, except the first, have a positive effect on both the demand for risky and real assets.

As mentioned above, the discrete decision to invest in an asset and the continuous decision of how much to invest need not be driven by the same mechanisms. To check the sensitivity of this assumption, appendix B shows results from logit regressions of the decision to own assets in the four different asset categories under study. The regressor set is the same as in the Tobit regressions. Interestingly, as shown in Figure B.1, all logit regressions display the same shift of the age effect as in the Tobit regressions, where the peak of the pattern shifts up into higher ages when all demographic and economic variables are included in the regressions. As hypothesised above, this could be explained by a situation where variables included in the "full" regressor set, while being able to explain most of the life-cycle pattern, neglect the selection effect from relatively wealthier elderly households. Admittedly, this differential-mortality interpretation depends on the initial hypotheses (with some support in the stylised findings in the cohort graphs). For example, ownership of safe and financial assets over life also displays a skewed pattern, but shifts to financial and safe assets in retirement are actually what we expect.

From Table B.1 it is clear that the results regarding the ownership decision is similar across asset categories and that many of the results from the Tobit regressions also can be found here, e.g. the role of human capital for ownership of risky assets. However, Figure B.1 shows that the age effects for ownership of safe and financial assets are somewhat different than the age effects implied by the Tobit regressions. The probability of owning financial assets has a hump-shaped, or possibly increasing, age pattern while probability of ownership of safe asset increases over life. Since the life-cycle effects for the unconditional portfolio shares apparently are dominated by real and risky assets (c.f. the Tobit results in Figure 4.1), it will also be interesting to investigate the patterns of the conditional asset shares in a sample-selection framework. This has been done using the Heckman 2-step estimator (results available on request).[15] The assumption here is that the ownership decision dominates the outcome decision, leaving a sequential process; first the household decides whether to invest, and conditional on this decision it decides how much if its gross wealth to invest. With this set-up, the results from the first step will be very similar to the logit regressions displayed in appendix B.

As it turns out, the estimated effects in the second step, i.e. in the conditional asset demand, are qualitatively very similar to the effects found in the Tobit regressions. The estimated age effects show basically the same patterns as in the Tobits. An exception is the conditional share of risky assets for which there is basically no discernible age pattern with the "full" regressor set, and no joint significance of the age coefficients. The age dummies in the risky-asset *selection* equation are still jointly significant however.

4.4 Fixed-effect regressions

Heterogeneity across households is of course an important issue as it might bias the results. For example, the "skewed" age effects found above might be the result of the inability to control for household-specific effects. If the interpretation of this feature holds, i.e. if it depends on differential mortality, a regression that controls for household-fixed effects should be able to catch this provided this difference in mortality can be considered a fixed feature of the household.[16] Since the HINK surveys contain a rotating panel component it is possible to control for household heterogeneity to some extent. However, the parametric Tobit model does not have an analogous fixed-effect model, and the random-effect methods are highly dependent on distribution assumptions and are computationally burdensome. Instead, this section contains results from fixed-effect regressions on the portfolio shares.[17] Even though the censoring is ignored it might not be that important for the estimated life-cycle effects; experiments with OLS regressions on the pooled data set give results that are very similar to the censored and sample-selection estimations.

Beside the differential-mortality assumption, there are further motives for using a fixed-effect framework as opposed to random-effects. Regression specifications that arise from

[15] As is well known, results from the Heckman 2-step estimator are sensitive to the identification of the selection and outcome equations. Here, the same set of explanatory variables was used to model both mechanisms, implying that the identification of the parameters in the outcome equation were achieved only by the non-linearity of the probit transformation of the selection equation. These results should therefore be interpreted with caution.

[16] This is commented on further in the last section.

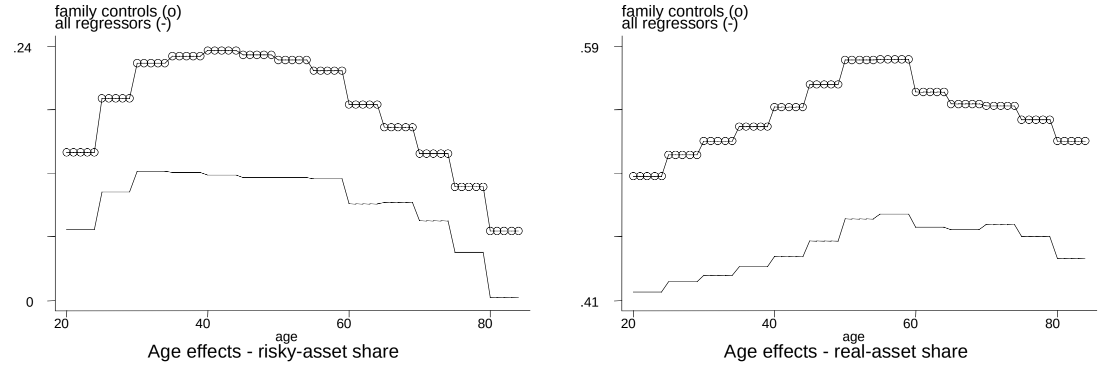
[17] Note that the "within" estimator is robust to some forms of sample-selection bias, e.g. if the probability of a household investing in an asset is constant over time. See Verbeek and Nijman (1996).

theoretical life-cycle models normally include an effect of household heterogeneity that can be given a specific interpretation based on the underlying theory – e.g. as marginal utility of wealth in intertemporal consumption models. Such effects will probably be correlated with the regressors, which invalidates a random-effects framework. Even though there is no particular underlying theoretical model to tie the regression specification to, it is a reasonable assumption in this application that the household-specific effects are correlated with the other explanatory variables.

The normal procedure is to treat the individual effects as fixed and use some form of within-transformation to estimate the model. However, since the focus here is on exploring life-cycle patterns, it would be of interest to get estimates of the effects of time-invariant variables like cohort membership. Of course, if the estimation method involves transforming all variables by taking deviations from household-specific means, these estimates are not readily available. An alternative method, employed by Kapteyn et al. (1999), will therefore be used here. The approach, based on arguments in Mundlak (1978), amounts to modelling the individual effects by specifying an auxiliary regression in which the expectation of the individual effects conditional on the regressors is assumed to be a linear function of household-specific means of all time-varying regressors. To accommodate a life-cycle specification, it will also be a function of the cohort membership of the household. Appendix C contains further details.

Figure 4.3 below graphs the estimated age effects of regressions on the unconditional shares of risky and real assets. Just like the Tobit results, the age profile for the risky asset share is hump shaped, where the share of risky asset in the portfolio increases up to the age of 35 and declines after the age of 60. The effects for the elderly are not well determined in general however. The hump-shaped age effect for the demand for real assets is also the same as in previous regressions. But as seen from the second panel in Figure 4.3 there is less of a skewed pattern here compared to the Tobit and logit regressions, and the peak of the age effect conditional on all regressors is kept in the middle-aged groups. Hence, once we control for individual heterogeneity this selection problem is reduced. Provided this is a mortality effect it should not come as a surprise – the necessity of using panel data to correct for mortality bias is stressed by Jianakoplos et al. (1989) who find that cross-sectional profiles can be seriously biased by the presence of productivity growth as well as mortality effects.

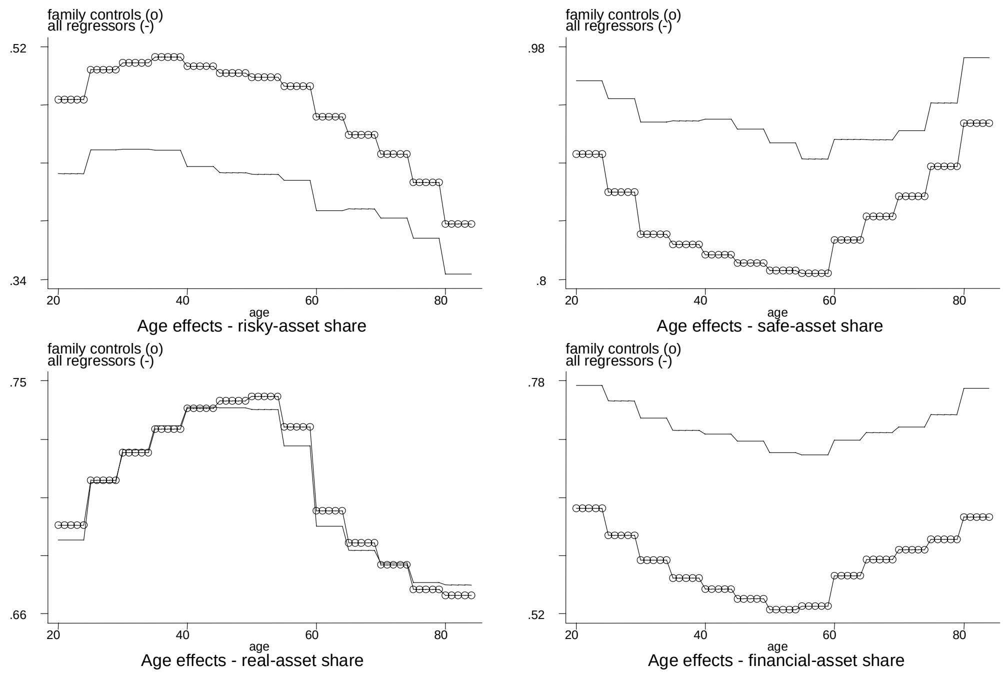
Figure 4.3 Age effects –panel regression on the unconditional shares of risky and real assets



Notes: estimation results for the age-dummy variables. Patterns are scaled with the constant. The reference group is the group aged 20-24. General estimation results available on request.

As a comparison to the effect for the unconditional portfolio shares in Figure 4.3, the estimated age effects from panel regressions of the conditional shares, i.e. conditional on ownership, are displayed in Figure 4.4 below. For risky assets there is very little difference compared to the results in Figure 4.3 while the conditional portfolio share of real assets displays more of a decumulation at older ages than the unconditional portfolio share. Judging by other experiments, conditioning on ownership actually makes the peak of the age profile for the weight of real assets stay in the late 50s. So, the "skewness" of the age effect seems main ly to be a feature of the discrete decision of ownership.

Figure 4.4 Age effects – panel regression on the conditional shares of risky and real assets



Notes: see Figure 4.3

Finally, note that for the conditional asset shares there is no forced link between the results for the risky/safe and real/financial assets since the two asset shares no longer must sum to one. However, the overall impression from the regressions is that the effects are similar to the effects for the unconditional portfolio shares.

5 Summary and discussion

The focus of this paper has been on identifying life-cycle patterns in Swedish households' demand for different asset categories. The data on household portfolio allocation were collected from Swedish income tax registers – the HINK surveys – for the period 1982-1992. Statements regarding life-cycle effects are by necessity conditional on the identification scheme of age, period, and cohort effects. The starting point here has been to allow the specification of these effects to be flexible enough to handle the macroeconomic development, as well as the institutional changes, over the studied period. Results from regressions indicate that there are clear life-cycle effects of the households' demand for the asset categories under study, which are risky financial assets, safe financial assets, total financial assets, and total real assets.

Tobit regressions show that the share of risky and real assets have hump-shaped age profiles, while the allocation of safe and financial assets follow a U-shaped pattern over life. However, logit regressions indicate that the discrete decision of ownership of safe and financial assets differ somewhat from these patterns; the probability of owning financial assets is hump-shaped, or perhaps increasing, and the probability of owning safe assets increases over life. Besides the age effects, the results also indicate that there are clear cohort effects. As hypothesised above, these effects might be the result of general economic and institutional features during certain periods. The estimated life-cycle effects can, to some extent, be explained by demographic and economic variables like income, socio-economic group membership, family status, labour-force participation, and tenure choice.

Note that, for simplicity, the standard parametric version of e.g. the Tobit estimator has been used here. The main problem with this type of estimator is the sensitivity to violations of assumptions regarding normality and homoscedasticity of the error terms, both of which will make the estimator inconsistent. Non-normality can be handled, for example, by employing estimators that are more robust to distributional issues. Such semi-parametric estimators may also be modified to account for household heterogeneity within a censoring framework. The analysis showed that controlling for household-fixed effects does matter; some results from the Tobit and logit regressions indicated differential-mortality effects, i.e. that the results were influenced by the higher survival rate of wealthy households. These effects were not present when household-fixed effects were controlled for.

In order to check the sensitivity of the results, a number of different regressions have been made, including random-effect logits and Tobits. In sum, the results were very close to the results displayed above. However, including net wealth among the regressors changes the estimated life-cycle effects somewhat, mainly for the ownership decision; in all logit regressions there are no clear age profiles and virtually no individually significant age effects when net wealth is included (although the age dummies still are jointly significant). This suggests that the age profile of wealth accumulation is picking up the estimated life-cycle patterns of the asset ownership. The age profiles for the portfolio shares, on the other hand, do not change much when wealth is included, but the age effects are not always jointly significant.

The inclusion of net wealth, although making the results susceptible to endogeneity bias, is common in empirical portfolio-choice applications, and might seem natural since there appears to be wealth-related mortality effects. There is, however, an important caveat regarding the potential for *observed* net wealth to capture differential mortality effects. The point made by Attanasio and Hoyner (1995) is that the negative correlation between observed wealth and mortality is not a structural relationship. The two variables are most likely determined simultaneously by underlying factors that presumably are connected to the *lifetime* wealth, or some age-invariant wealth measure. Therefore, at a given point in time for a birth cohort, the relative position of individuals in the current wealth distribution is not the same as their relative position in the distribution of lifetime wealth. This is both because the life-cycle pattern of wealth holdings can be different for individuals with different life expectancies, and because differential mortality will change the wealth distribution of the population.

Besides using estimators more robust to distributional and censoring assumptions, future studies would also benefit if measures that account for household background risk can be constructed. Previous studies (e.g. Hochguertel, 1997) have confirmed the theoretical predictions that uncertainty regarding future income is an important determinant of the allocation of financial assets. Unfortunately, the HINK surveys do not contain much useful data for this purpose. Other studies (e.g. Alessie et al., 2000) have also emphasised that there is a high degree of persistence in the portfolio composition in that lagged asset ownership can explain a very large part of current ownership. Although it might be difficult with the limited panel dimension of the HINK surveys, in general, it would be interesting to use a dynamic framework so that such state dependence can be separated from individual heterogeneity.

In any case, the present analysis provides empirical evidence on life-cycle behaviour that will be useful for future modelling of saving behaviour and portfolio choice. One important result is that different assets display different life-cycle patterns. The standard life-cycle model of saving does not distinguish between assets, and the hump-shaped age profile typically predicted with this model does not apply to the accumulation of all asset categories; the shares of safe assets and total financial assets in fact display the opposite profiles. A second finding, that confirms the results in Poterba and Samwick (1997), is that one should not generalise life-cycle asset demand and portfolio behaviour of households in a certain age bracket without taking account of the households' birth-cohort membership. The paper presents evidence that there are quite significant cohort effects for the demand of different assets. Therefore, it is important to distinguish between different cohorts as they, for example, approach retirement, since the economic behaviour of older cohorts may not translate to younger cohorts.

Appendix

A Data appendix

The information on asset holdings in the HINK database is mainly gathered through Swedish government registers on income tax returns. Hence, the data quality on different assets varies with different tax rules, and with the accuracy of the declared values. The quality of data for listed shares, for example, is likely to be good since the tax authorities get information on households' holdings directly from the banks and brokers, while assets such as personal inventories (cars, boats, art etc.) are likely to suffer both from underreporting and unreliable valuation since only a few of such items are subject to taxation. This is a problem since the value of these items represent a significant part of household wealth (e.g. Bager-Sjögren and Klevmarken, 1998). The data on bank deposits probably suffer from underreporting in the beginning of the period. However, the data quality for the last part of the period is very good since Swedish banks now report deposits and interests directly to the tax authorities.

The most problematic are assets and inventories associated with unincorporated business and farms, since these assets are notoriously difficult to evaluate. As for real estate, the main problem is market valuation and the data on co-operative apartments. The tax-assessed value of different kinds of real estate is transformed to market values via regional purchase-price indices. This will give an accurate value on average but might result in large over-, or under-estimates of individual buildings. No market values are calculated for real estate and inventories in unincorporated business. Also, the tax-assessed values on individual co-operative apartments are imputed values from the tax-assessed value of the whole apartment building. Since the mortgages on the whole apartment complex often exceed this tax-assessed value, the value of the individual apartment is set to zero. Furthermore, there is no purchaseprice index that could be used to transform declared values to market values. This is one of the major weaknesses of the HINK surveys since values and ownership rates on co-operative apartments thus will be under-estimated. Finally, the HINK material includes very little information on pension wealth, mainly due to the fact that the main part of retirement savings in Sweden is collective rather than individual.

In the regressor set, some variables merit further comments. The variable "Mortg/inc" is a continuous variable defined as the value of total mortgage debt divided by the labour income of the household. The argument, due to Fratantoni (1998), is that an important type of risk associated with homeownership is committed-expenditure risk, i.e. the risk assumed by committing to make mortgage payments over a long horizon out of an uncertain stream of labour income. This should induce a precautionary motive for assuming less risk in the financial part of the portfolio. Since the actual mortgage payments are not available in the data, I use total mortgage debt as a proxy.

The marginal tax rate, included to pick up tax-clientele effects, is calculated according to the method outlined e.g. in Agell and Edin (1990). In order to avoid the endogeneity between the portfolio choice and the actual capital income, an imputed capital income measure is used, where it is assumed that all households get a stylised annual rate of return on their net wealth. The interest rates roughly reflect the average rate of return on savings accounts in each year. Also included in the regressor set is labour income, where the income variable is linearly splined in deciles (where the delimitation of the deciles is based on the income distribution for all years pooled together). Hence, the effect of income is modelled as a piecewise linear function where effects from households in different income brackets are allowed to differ.

In order to control for the ability to vary the labour supply, since this possibility should increase the willingness to assume more risk in the portfolio choice, a measure of labour force participation is also included in the regressions. The variable combines information on hours worked with the number of employed adults in the household. To control for geographical location, typically found important in housing studies, the regressor set also includes a dummy for residence in a metropolitan area, defined as the three largest cities in Sweden. Finally, controls for different housing tenures are also included in the analysis of safe and risky assets (but excluded in the analysis of real and financial assets because of the endogeneity) in order to check whether the financial portfolio choice of owner-occupiers differs from households residing in co-operative and rental apartments. Note that these two dummy variables will be correlated with the mortgage debt-to-income ratio.

Table A.1. Composition of household wealth.

Stocks	listed shares, other shares, mutual funds, convertibles, options etc.
Bonds	savings certificates (sparobligationer) and lottery bond loans (premieobligationer)
Children's wealth	Wealth of children living at home – mainly bank deposits, bonds and shares
Bank deposits	
Cash	Contains assets not included among the above – cash holdings in excess of normal housekeeping money, bills receivable, claims etc.
Other assets	Non-itemizable asset category in the HINK surveys 1991 and 1992 – aggregate of assorted assets (bonds, children's wealth, durables etc.) from households using a simplified tax form.
"Risky" financial assets	stocks + bonds + 0.5 * children's wealth + other assets
"Safe" financial assets	bank deposits + cash + 0.5 * children's wealth
Total financial wealth	
Owner-occupied homes	Market value of one- and two-family dwellings.
Secondary dwellings	Market value of secondary homes, summerhouses etc.
Co-operative apartments	Tax-assessed value of co-operative apartments.
Personal inventories	Durables and capital goods – mainly cars and boats.
Total real wealth	
Gross wealth	

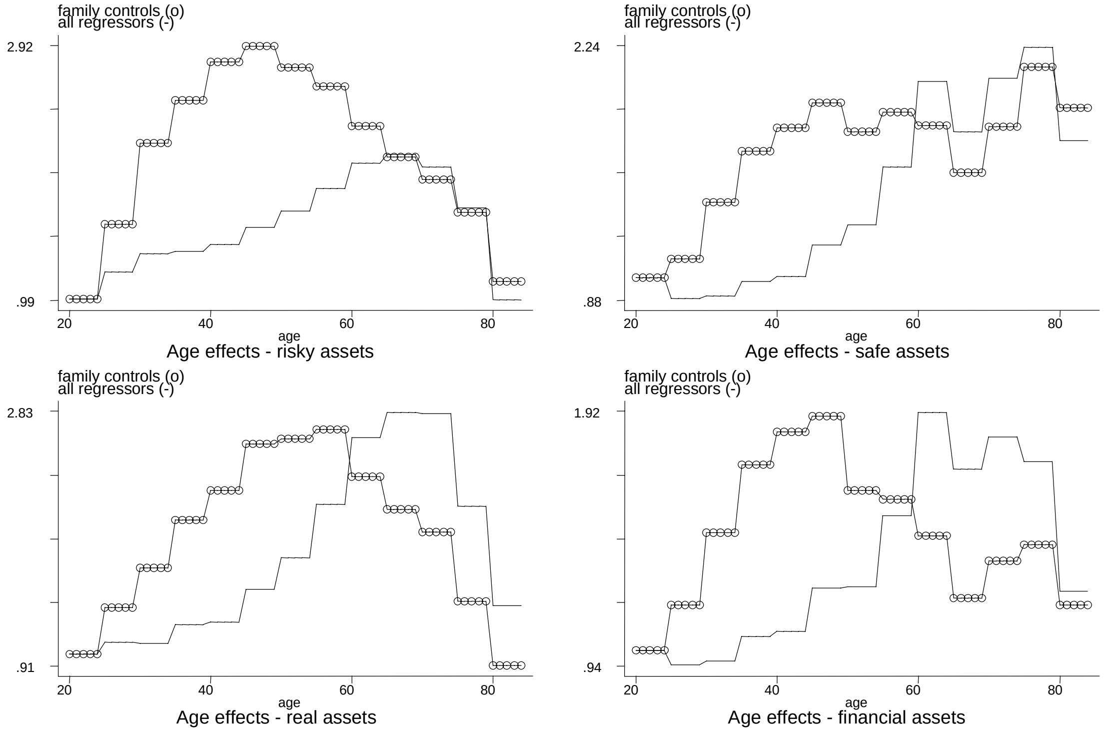
Note: The analysis excludes households with declared wealth in farms, unincorporated businesses, and apartment buildings. All variables are in real 1992 prices.

Table A.2 Description of regressors

Abbreviation	Description
Married	Dummy variable equal to one for married or cohabiting couples.
Single fem	Dummy variable equal to one if household is headed by single female.
Reference:	*Household headed by single male.*
Kids	Number of children in the household –18 years of age.
Mortg/inc	Total mortgage debt divided by non-capital income. Scaled by 1000000
Marg tax	Marginal tax rate of household.
	Adjustments are made for joint taxation. Calculations made with imputed capital income.
	Since the tax reform in 1991, capital income is taxed to a flat rate of 30%, which means that all households are given a marginal tax rate of 30% in 1991 and 1992.
Income	Declared non-capital income. Scaled by 1000000.
	In regressions included linearly splined in deciles.
Dummy variables for socio-economic group membership.	
reference:	*Worker in manufacturing or non-household service sector with no vocational training.*
	Normally less than 2 years of education beyond compulsory school.
Vocational	Worker in manufacturing or non-household service sector with vocational training.
	Normally 2 years or more of education beyond compulsory school.
Junior	Junior employee.
	Normally less than 3 years of education beyond compulsory school.
Middle	Employee in intermediate position.
	Normally between 3 and 6 years of education beyond compulsory school.
Upper	Executive or employee in high position.
	Normally more than 6 years of education beyond compulsory school.
Unspec	In employment, but not classified according to the above.
Business	Farmer, self-employed.
Retired	Old-age pensioner.
Student	Recipient of student aid.
Lfp	Labour force participation. Discrete variable.
	2 adults working full time = 2, thereafter a declining scale according to intensity of participation.
Metro	Dummy variable equal to one if household resides in Stockholm, Gothenburg, or Malmö.
Co-op	Dummy variable equal to one if household lives in co-operative apartment.
Renter	Dummy variable equal to one if household lives in rental apartment.

B Results from logit regressions

Figure B.1 Age effects from logit regressions



Notes: results expressed in odds ratios for the age dummy variables. Reference group is the group aged 20-24.

Table B.1 Regression results, logit regressions

	Risky assets		Safe assets		Real assets		Financial assets	
	Coeff	Std error	Coeff	Std error	Coeff	Std error	Coeff	Std error
Married	1.255[a]	0.042	1.819[a]	0.070	1.992[a]	0.071	2.294[a]	0.093
Single fem	1.100[a]	0.032	1.468[a]	0.045	0.629[a]	0.019	1.432[a]	0.046
Kids	0.923[a]	0.011	0.808[a]	0.012	1.081[a]	0.016	0.853[a]	0.013
Mortg/Inc	1.000	0.0001	1.006	0.004				
Marg tax	1.018[a]	0.001	1.021[a]	0.001	1.025[a]	0.001	1.025[a]	0.001
Vocational	1.193[a]	0.037	1.188[a]	0.041	1.489[a]	0.053	1.282[a]	0.046
Junior	1.817[a]	0.065	1.859[a]	0.078	1.316[a]	0.053	2.065[a]	0.091
Middle	2.378[a]	0.076	2.378[a]	0.095	1.896[a]	0.072	2.890[a]	0.125
Upper	3.098[a]	0.127	3.578[a]	0.211	1.961[a]	0.103	4.586[a]	0.302
Unspec	1.170[a]	0.040	0.980	0.036	0.864[a]	0.032	0.951	0.036
Business	3.156[a]	0.178	3.368[a]	0.232	3.707[a]	0.246	4.096[a]	0.286
Retired	1.433[a]	0.135	1.346	0.205	1.371[a]	0.157	1.458[b]	0.234
Student	2.475[a]	0.211	2.172[a]	0.177	0.909	0.092	2.198[a]	0.190
Lfp	1.050	0.029	1.224[a]	0.042	1.503[a]	0.050	1.345[a]	0.048
Metro	1.000	0.019	0.865[a]	0.019	0.470[a]	0.010	0.719[a]	0.016
Co-op	0.816[a]	0.026	0.613[a]	0.026				
Renter	0.506[a]	0.011	0.324[a]	0.009				
Test age	67.14[a]		42.56[a]		84.22[a]		32.18[a]	
Test cohort	103.89[a]		74.74[a]		45.70[a]		87.34[a]	
Test year	2032.61[a]		849.79[a]		61.63[a]		1162.09[a]	
Test inc.	487.11[a]		113.31[a]		430.79[a]		184.90[a]	
Ps. R^2	0.189		0.212		0.282		0.211	
# obs	69114		69114		69114		69114	

*Notes: explanations of abbreviations in the appendix. All coefficients expressed in odds ratios. Standard errors estimated using the robust Huber/White/sandwich estimator. Superscript "*a*" means that the coefficient is sig-*

nificantly different from zero at the 1%-level, "b" at the 5%-level. The tests of the age, cohort, and year dummies, as well as the income variables, are Wald tests of the joint significance of the coefficients. "Ps. R2" is a pseudo-R2 measure calculated as one minus the ratio of the log likelihood from the regression and the log likelihood from a constant-only model. "# obs" is the number of observations in the sample.

C Description of the "Mundlak variant" of the fixed-effect estimator

The estimated regression equation is the following:

$$y_{th} = \mu_0 + X'_{th}\beta + \sum_{\tau=1983}^{1992} \delta_\tau D_{\tau t} + \sum_{i=\text{"25–29"}}^{\text{"80–84"}} \gamma_i D_{ith} + u_h + \varepsilon_{th}\text{, where} \quad (4.4.a)$$

$$u_h = \overline{W}_h \alpha + \sum_{c=\text{"1898–1902"}}^{\text{"1968–1972"}} \lambda_c D_{ch} + \theta_h\text{, and} \quad (4.4.b)$$

$$\overline{W}_h = 1/T \sum_t W_{th} \quad (4.4.c)$$

In (4.4.a), t and h are time and household indices respectively, X_{th} is a vector of household demographic and socio-economic characteristics, $D_{\tau t}$ is a time dummy, D_{ith} is an age dummy, u_h is a household-specific (random) effect, and ε_{th} is a random iid error term. The household-specific effect in (4.4.b) consists of the time-average of W_{th}, which is a matrix of all time-varying regressors in X_{th}[18], the cohort dummies, D_c, and θ_h which is a random variable uncorrelated with the explanatory variables. Provided that all variables in X are included in W, Hsiao (1986) shows that substitution of (4.4.b) into (4.4.a) and estimation of the resulting regression specification by GLS will make β equal to an estimate from the standard 'within-estimator', while α will be the difference between β estimated with the 'between-' and 'within-estimators'.

[18] It is not obvious what variables to define as time varying. For example, socio-economic group might be considered a non-varying attribute, but some households do change status over time. The regressions follow the conservative approach of including all variables (except age, time, and cohort dummies) in the time-varying set.

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