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The Life-Cycle Hypothesis Revisited: Evidence on Housing Consumption after Retirement

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The Life-Cycle Hypothesis Revisited: Evidence on Housing Consumption after Retirement*

by Miriam Beblo and Sven Schreiber†

this version: November 2010

Abstract

According to the life-cycle theory of consumption and saving, foreseeable retirement events should not reduce consumption. Whereas some consumption expenditures may fall when goods are self-produced (given higher leisure after retirement), this argument applies especially to housing consumption which can hardly be substituted by home production. We test this hypothesis using micro data for Germany (SOEP) and find that income reductions when entering retirement have a negative effect on housing expenditures for tenants. For some econometric specifications, this effect is significantly stronger than the one of income changes at other times. While this result suggests that the strict consumption-smoothing hypothesis is violated for the subgroup of non-home owners (60% in Germany), the effect is quantitatively small, which explains the ambiguity of previous findings.

Keywords: consumption smoothing, retirement-consumption puzzle, SOEP

JEL codes: C33 (Models with Panel Data), D91 (Intertemporal Consumer Choice; Life Cycle Models and Saving), E21 (Consumption; Saving; Wealth),

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1 Introduction

Do people save too little? Put differently, do they undersave compared to the benchmark prediction of the standard life-cycle model? (Modigliani and Brumberg, 1954; Friedman, 1957) Undersaving would mean that people are unable to smooth their consumption paths according to the permanent-income hypothesis of consumption and saving,[1] and "the best evidence of undersaving is probably the observation that, upon retirement, individuals, on average, reduce consumption substantially." (Akerlof, 2002, p. 424) That is the so-called retirement-consumption puzzle. Since the consumption function is a central building block of most (macro-) economic models, it is a fundamentally important issue whether the standard life-cycle model provides a good approximation to reality or not. By conducting a new test of the consumption-smoothing hypothesis we contribute to the literature on whether people save enough for retirement, and therefore whether the life-cycle theory of consumption is valid in general. The novelty of our approach revolves around our focus on housing consumption. First, these expenditures cannot be substituted by the increased leisure after retirement (see below). Second, we exploit the fact that the majority of the German population do not own their homes, which means (a) that their housing expenditures are directly observable as rents paid, and (b) that they are potentially more prone to the undersaving problem due to the absence of housing wealth.

There is no consensus in the economic literature on the existence of a retirement-consumption puzzle, and the debate is still ongoing. On the one hand, there is the position that "retired people are commonly believed to tailor their consumption to a concept of income rather than to the value of their assets." (Akerlof, 2007, p. 18) Banks, Blundell, and Tanner (1998) conclude: "We argue that the only way to reconcile fully the fall in consumption with the life-cycle hypothesis is with the systematic arrival of unexpected adverse information." This finding would at least reject the life-cycle-cum-rational-expectations strong form of the model, since "sys-

[1] Of course consumption smoothing may still imply rising or falling consumption paths given differentials between personal time discount rates and net (after-tax) interest rates, but discrete and sudden jumps are ruled out.

tematic" and "unexpected" together are incompatible with rational expectations. Bernheim, Skinner, and Weinberg (2001) also reject life-cycle models in favor of rule-of-thumb or mental-accounting savings behavior. Benartzi and Thaler (2007) cite broad evidence that the standard model fails.[2]

On the other hand there is an important opposing strand of the literature which argues that extended models of optimizing and forward-looking behavior are compatible with the empirical observations.

First and perhaps most fundamentally, Aguila, Attanasio, and Meghir (forthcoming) do not find any expenditure drop after retirement at all for broadly defined consumption goods, using micro data for the US.

Secondly, although they do not directly analyze consumption after retirement, recently some researchers have addressed the undersaving issue by putting particular emphasis on the complex institutional environment facing the agents. Gourinchas and Parker (2002) are able to fit a model of optimal life-cycle consumption expenditures to the US data "quite well" taking into account realistic labor market features. Scholz, Seshadri, and Khitatrakun (2006) also claim that the household-specific predictions from an optimizing model with a realistic account of the environment are close to observed wealth values; however, still 20% of households hold less wealth than would be prescribed by the optimal decision model.

Third, the following additional objections to the validity of the puzzle have been put forward in the literature, see also Hurst (2008) for a survey, and Hurd and Rohwedder (2008) for associating changes in consumption with these arguments:

- Measured consumption expenditure of many goods may decrease at retirement because of increased home and self-production, and also because work-related expenses become unnecessary. Baxter and Jermann (1999) in general find that allowing for home production explains the apparent excess sensitivity of consumption to income that would otherwise invalidate the permanent-income hypothesis. Aguiar and Hurst (2005) find "dramatically" rising time use on home production which substitutes for example the drop in expendi-

[2]The retirement-consumption puzzle is just one manifestation of the general (alleged) excess sensitivity of consumption to current income. For evidence on this phenomenon see for example Campbell and Mankiw (1990); Attanasio and Browning (1995); Reis (2006).

tures on food, such that food consumption stays roughly unchanged for retirees. With German SOEP panel data Schwerdt (2005) finds a positive correlation between consumption reductions at retirement and (proxies for) home production, but he argues that not all of the fall of consumption can be attributed to that effect, because there is a general rise in home production. Recently Lührmann (2010) refined those findings for Germany by combining both consumer expenditures and time use data pre and post retirement. She reveals a significant drop in expenses at retirement which coincides with an increase in time spent on home production.

- The distinction between anticipated and unanticipated changes is important. Blau (2008) finds that with anticipated retirement there is no consumption drop in data from the US Health and Retirement Study (HRS),[3] although the consumption drop is too large for the unanticipated retirees. Haider and Stephens (2007) show that a portion (but less than half) of the observed drop of consumption at retirement can be attributed to retirement happening unexpected. Smith (2006) has a similar result showing that food spending only decreases if (early) retirement happens involuntarily.

Our approach of testing the life-cycle model of consumption addresses these issues in the following ways.

First, in order to circumvent the problem of measuring home production (whether it rises and if so, by enough to substitute the consumption drop), we focus on a specific aspect of consumption, namely that of housing. Housing cannot be substituted by home production, indeed it will usually be a complement to the increased leisure time budget in the utility functions of individuals. Therefore according to the neoclassical model, demand (and thus expenditures) for housing could actually increase slightly after retirement (of course holding other things such as household size equal). In contrast, a drop in housing consumption would be inconsistent with the life-cycle model.

An empirical problem related to the measurement of housing consumption oc-

[3] Interestingly, this would mean that no (net) expenditures are substituted by home production, in contrast to the previously described explanation of the same retirement-consumption puzzle.

curs if people own their dwellings and therefore no payments can be observed.[4] In this regard the German institutional (and possibly cultural) environment is well suited for our analysis because Germany is a country where less than half of all households own their homes. This has the advantage that housing expenditures are directly observable for many households as rents paid. For this reason we analyze only non-home owners. Obviously this implies that our results will not be (necessarily) representative for all individuals. Indeed, it is plausible that home owners suffer much less from the under-saving problem precisely because of their owned house or apartment, which represents cumulated savings (e.g. see Lusardi and Mitchell, 2007). However, since the life-cycle model is a hypothesis relating to *all* economic agents, focusing on a suitable sub-group is still an informative approach.

Secondly, with respect to the issue of expected vs. unexpected retirement, in our empirical analysis we control for early retirement events (which were partly used as labor-market policy measures in the 1990s in Germany). We also account for disability that may induce premature retirement. The remaining retirement events should typically be well predictable for the individuals.[5]

The general idea of the test is to specify a panel-econometric model to explain the reduction of housing expenditures. We use several different dependent variables (some of which are binary, and one is continuous) and thus different model variants to operationalize the concept of housing consumption reductions . Apart from other control variables which will be explained in detail below, the models include two income growth variables, one for those observations when people

[4] Also, high transaction costs of selling one's house or apartment will induce home owners to simply stay in their old home and not adjust housing expenditures even if they actually desired to do so. But note that in Germany another version of this argument also applies to tenants, because the rent contracts of tenured occupants are protected against quick rises of rent payments (see the institutional information in the appendix). As a consequence, giving up a long-inhabited apartment in Germany could imply a substantial rise of rent payment obligations for the new apartment (holding other apartments characteristics fixed). We account for this institutional fact in the econometric models by including the cumulated tenure in the old apartment as a control variable.

[5] It may be argued that while the timing of retirement may be anticipated, the amount of the income drop (i.e. the effective replacement rate) would at least be partly unexpected by the individuals. However, in the German defined-benefit public pension system the replacement rate is relatively transparent. Also, while the amount of the drop may still be unexpected, it would not be consistent with the life-cycle model to posit that rational individuals *systematically* overestimate their post-retirement income levels.

have just entered retirement, and another income growth variable for the remaining observations. Under the life-cycle hypothesis we expect the latter variable to contribute to explaining (and be positively correlated with) housing consumption changes, because a certain fraction of general income changes will come through unexpected and permanent shocks which would shift permanent income.

However, income growth of people entering retirement should not significantly contribute to the explanation of a reduction of housing expenditures in the panel sample. Otherwise, we would tend to think that the retirement-consumption puzzle is in principle relevant and that the life-cycle model does not hold in general. Of course, there may also be unforeseeable permanent shocks at retirement, most notably unexpected revaluations of an individual's capital asset portfolio. However, the vast majority of Germans have held assets with deterministic payoffs instead of risky stock portfolios, apart from their implicit entitlements in the pay-as-you-go pension system. Therefore these shocks (which are not observable in our dataset) should be negligible.

Our main result is that we indeed find that (negative) income growth at (foreseeable) retirement helps to explain a reduction of housing consumption. The point estimates of that effect even turn out to be larger (in absolute value) than the coefficients for income growth of non-retirers. Therefore our test rejects the life-cycle model of consumption as a generally valid theory of economic behavior. However, the effect is not large even for our subgroup of non-homeowners which may explain the ambiguous conclusions in the existing literature.

This paper is structured as follows: In the next section we review the theoretical background for consumption (particularly housing consumption) behavior by distinguishing effects within neoclassical versus behavioral economics models. Our empirical approach of testing the consumption-smoothing hypothesis is outlined in section 3. Section 4 presents and discusses the estimation results. Section 5 concludes.

2 Theory

There are a number of relevant theories for the question of how current consumption is determined, and to what extent it depends on wealth or current income.

2.1 Neoclassical effects

Before discussing explanations and hypotheses based on behavioral economics, let us first revisit neoclassical models and how they could be compatible with certain observed patterns of consumption, especially housing consumption around the time of retirement. By "neoclassical" we basically mean models with agents who are optimizers constrained by their environment (including their budget), who have time-consistent preferences, and whose expectations are not systematically biased. Even under these assumptions it could be the endogenously predicted behavior to move to a cheaper home right after retirement and not before, for the following reasons:

1. The search for a new home could be so costly in terms of forgone leisure that it is optimal to postpone it until after retirement, when leisure is not scarce anymore.

2. Workers are geographically bound to some extent by the location of their workplace. They are only free to move away when they retire. Transaction costs of moving may make it optimal to combine this locational move with the move to a cheaper home and not move twice. In this case the observed move should then indeed lead to a sufficiently different geographical location.

3. In the same vein, retirees are free to move to places relatively far from economic centers (cities, plants, offices, etc.), where housing may in general be cheaper. Thus a move right after retirement to a cheaper home would not prove by itself that the reason was the reduced current income. As with the previous reason this effect could only apply to retirees who move relatively far away from their old homes.

With respect to the costs-of-search explanation number 1 it should be borne in mind that our sample is deliberately restricted to men beyond the age of 55 whose children are typically not very time-demanding of their parents anymore. Note also that the typical amount of hours worked per year is quite a bit lower in Germany than for example in the US. Therefore it seems somewhat implausible that forgone leisure should inhibit people from searching for a cheaper home. Finally, people may hire agents; in Germany those are usually only paid in case of a successful match.

Addressing explanation number 2 we note the fact that only home tenants (as opposed to home owners) are included in the sample, see below. But for home tenants moving does not imply selling an illiquid asset and investing a large amount of funds in a new home. Thus the involved transaction costs of moving would not seem to be prohibitive and could therefore not plausibly explain the deferral of moving to a new home.[6]

Note that all of these effects relate to the retirement event as such, not to an associated income change. Therefore we will include those events in our empirical model as a (binary) control variable to capture those effects. In order to get a general picture about the reasons for moving we also specify a model to explain the move to a new home as such, without conditioning on reduced rent payments.

2.2 Behavioral economic hypotheses

1. Norms / mental accounts: according to this view, the current income (or a certain portion of it) of an agent is an entitlement to spend. Also, instead of saving his income an agent spends a certain proportion of current income on housing because that is customary and thus "norm"-al in the literal sense.[7] Norms are hard to identify empirically; while the ratio of rent to current income is observable and thus could give a hint of mental accounts playing a part, a high rent-income ratio is of course also a "rational" reason to move.

[6] Apart from this theoretical argument a more pragmatic issue is that more detailed geographical information in the SOEP is subject to certain usage restrictions. Future research could possibly incorporate such an analysis.

[7] See Shefrin and Thaler (1988) for a model with mental accounts.

2. Procrastination: it could also be the case that agents are perfectly aware that they should rationally be moving to a cheaper home, but they suffer from procrastination effects. (O'Donoghue and Rabin, 1999) This explanation in isolation would imply that agents used to have a good reason to live in their expensive home. The most likely case is the space requirement of children. An observable and testable implication of this may be that people without children or having separated or with other reasons to have lived in a spacious home would not be affected so much by procrastination.

3. Finally, another simple explanation is myopia, i.e. the assumption that agents simply do not consider their future needs. In its extreme form that would imply that no income changes ever lead to an adjustment of current consumption until wealth is depleted. In general, myopia of course induces undersaving and tends to prevent wealth accumulation.

Note that the assumption of hyperbolic discounting (present bias) alone is not sufficient to generate an excess sensitivity of consumption to current income, as pointed out for example in Akerlof (2007, fn 39) by invoking the analogy to Barro's well-known model with bequests (Barro, 1974): The future selves of an individual in a model with hyperbolic discounting effectively take the role of the heirs in the dynastic model. This insight changes a little in a "golden-eggs" model a la Laibson (1997), where a rational agent with hyperbolic discounting preferences (and who knows about his time inconsistent preferences) will invest in illiquid assets to avoid temptation in the future. Effectively such an agent makes his future selves liquidity-constrained. However, this implies an asymmetric sensitivity of consumption to current income, even if the change of income is anticipated: If that income rises a relatively large portion of it will be consumed, because the future income could not be (easily) pre-committed. But if that income is expected to fall as in the case of retirement, a rational agent with hyperbolic discounting would have invested in assets with a corresponding duration (time to maturity).[8] Thus he

[8] In developed countries such as Germany those types of assets clearly exist and are quite widespread; for example so-called "capital life insurance" contracts which are essentially savings plans that pay an annuity or a lump-sum payment after retirement.

would be able to smooth his cash flow and hence his consumption expenditures.

3 Empirical approach

Our general approach to test the life-cycle hypothesis can be described as follows. Our underlying assumption is that housing consumption and leisure are not substitutes in the utility functions of agents (but might be complements). Hence, we perform one-sided tests of the following hypotheses:

1. H0: no or positive effect of income changes at retirement on housing consumption (compatible with the life-cycle hypothesis)
   H1: negative effect

2. H0: weaker effect at retirement (compatible with the life-cycle hypothesis)
   H1: stronger effect at retirement

The details of implementing these tests are described in this section.

3.1 SOEP

To investigate the housing consumption behavior of people entering retirement we draw on panel data of the German Socio-Economic Panel (SOEP). The SOEP is a yearly micro-data panel which has been conducted in annual interviews of individuals and households since 1984 in West Germany and since 1990 in East Germany.[9] It is well suited for our analysis as it contains detailed information on both the retirement and the housing issues. From wave to wave respondents report whether they have changed their employment status because of retirement and whether they have moved to another apartment, including rental costs before and after. Respondents also provide information about their household size, income and other living circumstances. Moreover, this information is available over a long period of time which enables us to gather a decent number of respondents who actually enter retirement within the observation period.

[9] For a detailed description of the data set see Wagner, Frick, and Schupp (2007).

Despite the many advantages of longitudinal data, panel attrition may be a particular problem when studying moving behavior. According to the official documentation, panel attrition in the SOEP, related to households that were lost after they moved to unknown new addresses, is roughly 0.5% on average each year (Kroh, 2009). If it does have any effect on our results at all, this attrition is expected to bias our findings in the direction of the life-cycle hypothesis.

3.2 Sample selection

Due to some inconsistency in the wording of the SOEP questionnaires before 1993, the retirement event cannot be deduced correctly, so we start with the panel wave 1994 (i.e. t-1 starts at 1993). The latest available wave at the time of writing is from 2008. Given a massive rent catch-up in East Germany from constrained levels in the years after unification, we leave out East German observations before 1997. Self-employed workers and those with unemployment status in t and t-1 are also excluded from the sample.

As we want to compare the housing behavior of recently retired workers with other individuals (or households), we do not restrict the sample to those going into retirement. Nevertheless, in order to obtain a relatively homogeneous sample we include men between the ages of 55 and 75, centered around the standard nominal retirement age of 65.

An important feature of our analysis is that we focus on home tenants, thus excluding home owners. The main reason for this exclusion is that the current cost of housing is unobservable for non-tenants. But it is clear that home owners tend to stay in their apartments or houses after retirement. In general their behavior is probably consistent with the life-cycle hypothesis to a larger extent than the behavior of tenants, because the asset of a home itself constitutes a considerable savings item for retirement. Therefore we acknowledge that in our setup we would find non-consumption-smoothing results more easily than in a representative sample of the whole population. If we find violations of the life-cycle hypothesis, this finding would then not apply to the roughly 40% of the households in the SOEP who are home owners.

The number of observations along the time dimension is of course different for each cross-sectional unit. Due to the fact that some variables are constructed as first time differences or lags we lose one observation in the time dimension for each unit.

3.3 Important variables and estimation methods

We consider several variants of how to operationalize the reduction of housing consumption expenditure. The dependent variable can be either of the following:

1. y^m ("move"): For comparison purposes, we also analyze the move events in our sample per se, i.e. irrespective of whether the new home is cheaper than the old one or not. $y^m_{it} = 1$ if individual i "did not live in the same home last year", $y^m_{it} = 0$ otherwise. The mean of this variable in the sample (of course with repeated observations per individual) is 5.1%.

2. y^{ch} ("move_cheaper"): Whether or not a move to a cheaper home took place. This is constructed as $y^m_{it} 1(\Delta r_{it} < 0)$, where $1()$ is the indicator (Heaviside) function and Δr_{it} is the growth rate (log-difference) between this year's and last year's rent paid. This variable has a mean value of 1.9%.

3. y^{co} ("move_cost"): Whether respondents answered "for cost reasons" as the main reason for moving ($y^{co}_{it} = 1$). However, this information is only available in the SOEP from 1997 onwards. The mean of this variable is 1.0%.

4. Δr ("rentdiff"): Finally, as a continuous variable we also analyze the growth rate of rent paid, as used before in the construction of y^{ch}. Note that this concept does not presuppose a move to a new home, but might also capture renegotiation of rents. The simple unconditional distribution of this variable is displayed in figure 1.

In all cases the mainly interesting coefficient(s) are those that refer to income growth of retirers (people entering retirement), i.e. the interaction of the income growth variable with the retirement event dummy. If the income drop at retirement has a positive effect on moving to a cheaper home (or a negative effect on the cost of the new home), we would interpret the evidence as being incompatible with the

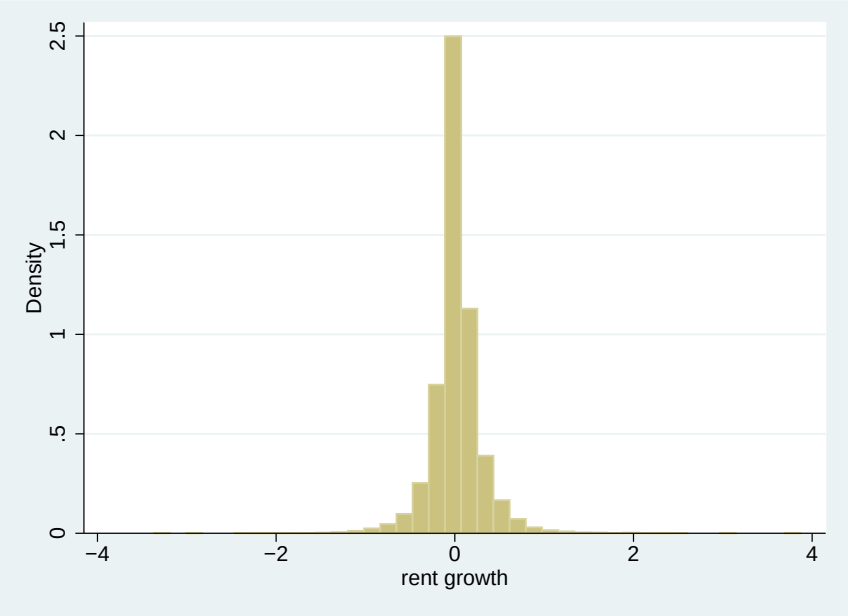

Figure 1: Distribution of rent growth. (Measured as the time difference of the logarithm, 0.1 ≈ 10% growth, hence values below -1 are possible.)

life-cycle hypothesis. This is especially true if the effect were stronger than that of the income changes of non-retirers, since changes at retirement should be more foreseeable on average.[10]

The two income growth distributions are shown in figure 2. If we separate East and West German retirers we get median household income growth rates of -9.9% in the East and -14.2% for West Germany. The main reason for the relatively low income reduction in East Germany are the lower wages coupled with generous pension entitlements that were granted after unification.

Considering the income growth of retirers' households it is also interesting that a sizable part of the observations displays rising income. As we consider household income, this increase may stem from life insurance contracts that become due or a rising income of the spouse. Hence, it could be the case that there is a different puzzle reflected in those observations, namely the possibility that many people in Europe even save too much for old age, given the relatively high level of state-provided old-age pensions and health care benefits, compared to countries like the US. However, that aspect is beyond the scope of this paper.

[10]The income change of non-retirers is also constructed as an interaction term, namely that of the income growth variable and the inverse of the retirement event dummy (1 minus...).

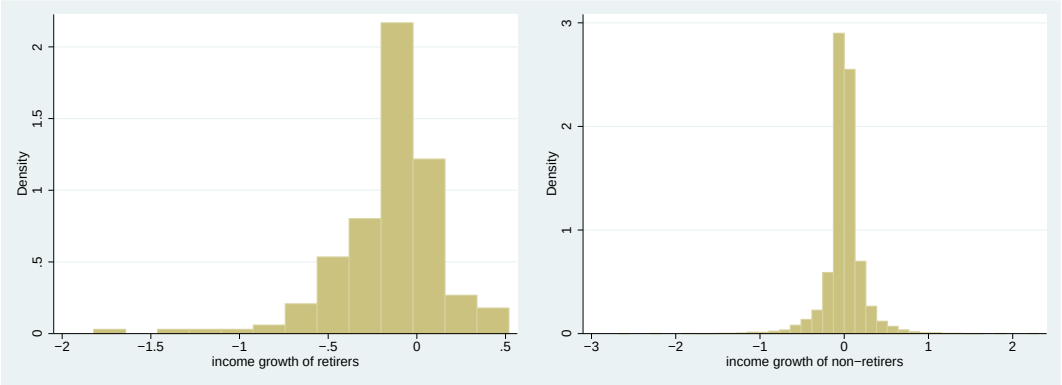

Figure 2: Income growth of retirers (left) and the rest (right). (Measured as the time difference of the logarithm, $0.1 \approx 10\%$ growth, hence values below -1 are possible.)

The fraction of observations with a retirement event is 1.8%, where the construction of the retirement event dummy is actually not trivial: In the SOEP questionnaire, respondents who have entered retirement recently (since last year's survey) can be identified with a combination of questions (i) on the termination of the last job within the past or the current year and (ii) on the reason for leaving that job. This information is available and, to the best of our knowledge, reliable since the survey year 1993. We define the variable "entering retirement" as taking the value 1 if the respondent reports the termination of his job since last year's questionnaire and old-age pension or early retirement as the reason for this event. Entering an early-retirement scheme is also considered in an additional variable "early retirement" which, correspondingly, takes the value 1 if the respondent reports the termination of his job since last year's questionnaire and early retirement as the reason for this event. We interpret "early retirement" as an interaction variable which covers the additional effect of entering retirement rather unforeseeably and therefore unanticipatedly. In the SOEP questionnaire, the early retirement information is available until the year 1998 only. Hence the variable is always zero afterwards. However, if a respondent reports a (regular) retirement event in two subsequent years, we interpret the first event as an early retirement and set the respective variable to 1.

3.4 Model specification

For the binary dependent variables as defined before we use the following probability model in our panel context:

$$\mathsf{Pr}(y_{it}^s = 1|\mathbf{x}) = \Lambda(\alpha_i^s + \mathbf{x}_{it}'\beta^s), \quad i = 1...N, \quad t = 1...T_i, \tag{1}$$

where $s \in \{m, ch, co\}$ indexes the different dependent variables and α are the unobserved individual-specific effects. Note that $\mathbf{x}$ contains some time-invariant variables as well as time dummies. The time dimension for this unbalanced panel varies between all theoretically possible values (one to fourteen).

As $\Lambda()$ is the logistic CDF, we choose a (panel) logit model instead of a probit model. For the probit class, a fixed-effects specification that does not suffer from the incidental parameters problem is not available. In contrast, for the panel logit model it is possible to use a conditional likelihood which does not depend anymore on the unit effects and which can be used to estimate the remaining parameters consistently. A corresponding Hausman test variant can be used to assess whether the specification with uncorrelated (random) effects also yields consistent estimates. As in the standard linear panel model, the random effects specification is more efficient if its assumptions are met. But indeed it turns out that the Hausman test rejects some of the specifications with random effects in favor of (conditional) fixed effects.

Note that units where all outcomes are the same (all zero or all one over time) do not contribute information to the conditional likelihood for the fixed-effects logit. Hence below we report sample sizes without those units for the fixed-effects specifications; this is a feature of the conditional fixed-effects model and should not be mistaken for an arbitrary sample selection.

For the continuous variable Δr it turned out that a dynamic panel specification is appropriate:

$$\Delta r_{it} = \alpha_i^{AB} + \rho_1 \Delta r_{i,t-1} + \rho_2 \Delta r_{i,t-2} + \mathbf{x}_{it}'\beta^{AB} + \epsilon_{it}^{AB} \tag{2}$$

To estimate this model we use the well-known Arellano-Bond GMM estimator which wipes out the individual-specific effects by first differencing.

For comparison we also estimate a static linear panel model:

$$\Delta r_{it} = \alpha_i^S + \mathbf{x}_{it}'\beta^S + \epsilon_{it}^S \tag{3}$$

Whether a specification with random or fixed effects is more appropriate here can be determined again by the standard Hausman tests.

In all model variants our choice of control variables to account for the background noise of residential mobility is based on and extends the estimation results of Tatsiramos (2006). Tatsiramos (2006) studied residential mobility of people over the age of 50 in several European countries using the ECHP. As the German contribution to the ECHP data set is an adjusted sample of the SOEP, we draw on those variables that proved statistically significant in explaining moves in Germany in the specification by Tatsiramos (2006). These include whether the respondent lost his spouse, experienced a health shock (disability, not the continuing status, 3.5%), lives in a couple (77%) and with children (4.3%). In our specification we did not include living in an apartment and whether housing costs are a burden nor household wealth because we did not find appropriate panel information in the SOEP. In the estimation by Tatsiramos (2006), entering retirement proved an additional important determinant of moving behavior; as discussed in section 2.1 we also include such a dummy – called "retiring" – in our models to capture effects that would be compatible with the life-cycle model, and to make sure they are not erroneously attributed to the income changes at retirement.

Of course we also include other control variables in our models: Binary variables are time dummies (fixed effects along the time dimension), indicators for East (mean of 30%) and North West Germany (referring to the states of Sleswick-Holsatia, Hamburg, Bremen, Lower Saxony, and North Rhine-Westphalia; mean 38%), loss of spouse (0.53%), new job (1.4%), regular employment status of spouse (9.5%), German nationality. Note that some of the variables are not time-varying and therefore do not appear in fixed-effect specifications below.

Another variable is the income growth at entering early retirement to account for unexpected retirement events. In contrast to regular old-age retirement, in many cases early retirement – which in Germany had been used also as a type of labor market policy and thus could happen to older workers who were laid off – can be considered as unanticipated. Since early retirement events are a subset of all retirement events, controlling for early retirement is thus desirable to focus on anticipated old-age retirement.[11] See also Blau (2008) for the importance of this distinction. Furthermore, log rent per square meter, the rent-income ratio, (log) household income, the change of household size as an integer-valued variable, age, and the tenure in the current home (divided by 10 as a scaling device) are also controlled for.

We always allow a "realization" lag between the income drop and the potential effect on housing consumption. That is, the income growth variables for retirers and for the rest are always included contemporaneously and with a lag (of one year).

To gain estimation efficiency we remove insignificant terms in a general-to-specific fashion, based on a cutoff significance level of 30%. However, at least one of the income growth terms is always retained given that that estimate is the central issue of this paper.

4 Estimation results

In table 1 the results of our panel logit estimations with respect to the binary dependent variables of moving to a cheaper home or due to cost reasons are reported. For the first variant, with move_cheaper as the dependent variable, we use the fixed-effect specification because the Hausman test rejects the random effects assumption, whereas for the second variant, with move_cost, the assumption is not rejected. Both variants include time dummies which are however not reported in the table. The income growth variables are only relevant when they are lagged, the

[11] The information about early retirement is only available in the SOEP up to 1998. However, the incidence of early retirement had been steadily decreasing in the 1990s, as it was recognized that (mis-) using it for labor market policy purposes implied a heavy burden on the pension system. Therefore the effects of having to ignore it (setting it to zero) after 1998 should be small.

Table 1: Estimation results for reducing housing consumption by moving

	move_cheaper (FE)	move_cost (RE)
household inc. growth at retirement	–	–
lag of above	-2.58 2.04	-2.73^{***} 1.09
household inc. gr. of non-retirers	–	–
lag of above	-1.77^{***} 0.59	-0.86^{*} 0.56
retiring	1.57^{**} 0.62	1.25^{**} 0.65
age	–	0.98 0.86
age squared/100	-0.82^{**} 0.38	-0.01 0.01
change of household size	-0.92^{***} 0.27	–
log household income t-1	3.47^{***} 0.93	0.78^{**} 0.40
rent-income ratio t-1	8.75^{***} 2.07	4.65^{***} 1.43
tenure in home t-1 (years/10)	2.38^{***} 0.44	-0.25^{*} 0.14
log rent per square meter t-1	–	–
N, $\sum T_i$	119, 863	1520, 7104
log likelihood	-150.93	-269.64
equality test (income gr.)	0.16, p=0.69	2.16, p=0.1063
Hausman test random effects	68.33, p=0.00	22.81, p=0.1190

Notes: Panel logit estimates, RE – random effects, FE – (conditional) fixed effects. Standard errors below estimates, two-sided significance levels denoted by *** (1%), ** (5%), * (10%). For the income growth variables, one-sided tests are applied. Definitions of variables (see also subsection 3.3): "move_cheaper" – moved into cheaper home, "move_cost" – moved because of (self-proclaimed) cost reasons. Time dummies also included.

contemporaneous terms were insignificant and were thus removed. Note that the initially included variables for early retirement also turned out to be insignificant.

For both dependent variables, income growth always lowers the probability of reducing housing consumption by moving (so an income drop raises that probability). The point estimate of that effect is between 50 percent (variant 1) and more than 300 percent (variant 2) higher for retirers, contrary to what the life-cycle hypothesis would suggest. That is, in our sample people on average do adjust their housing expenses downward once they retire (instead of showing anticipative behavior). However, the significance of that result is ambiguous; in the case of moving to a cheaper home, the coefficient for retirers is not significant, while in the case of moving due to cost reasons, the significance level is even 1%. It is thus not surprising that the effect for retirers is only close to being significantly different from the one for the rest of the observations in the latter variant.[12]

After having established the significant impact of retirement events on moving due to cost reasons, we turn to the quantitative interpretation of the results, since the coefficients in logit (or probit) models are not quantitatively meaningful by themselves. Figure 3 shows the estimated additional probabilities on moving for cost reasons of simulated income drops by 30 percent with or without a retirement event, compared to a benchmark base probability. While the additional effect of an income reduction at retirement is quite large compared to the low general (annual) probability of moving, and particularly compared to the additional effect of an income reduction at other times, the implied additional probability of 0.92 percentage points is quantitatively small.[13]

The results for the dependent variable "move" capturing any move are shown in table 2. But even though the point estimate of the income growth effect of retirers is almost twice as large as for non-retirers, a formal test is again unable to reject the equal magnitude of the effects. It is interesting that the retirement dummy does not significantly explain the moves. Therefore it appears that there is no general

[12]Note that this Wald-type test is inherently two-sided, whereas our underlying hypothesis pair was one-sided; thus the p-value may be overstated. For the other results, with respect to hypothesis 1, it did not matter whether we considered one-sided tests or simply the standard two-sided tests.

[13]Note that marginal effects can only be calculated for a random effects model and are, therefore, not presented for move_cheaper as the dependent variable.

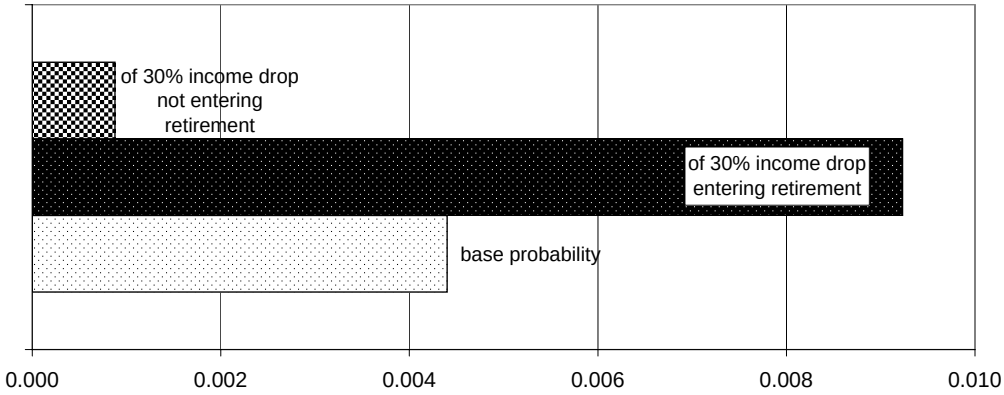

Figure 3: Marginal effect on moving due to cost reasons (move_cost).
(Calculations based on estimates in table 1, for median values of explanatory variables and individual effects equal to zero. The confidence intervals for the additional effects of the income change are (from top to bottom): [-0.0073,0.0014], [-0.0774, 0.0159].)

tendency for German tenants to relocate when the household head retires. We only observe a significant influence when the retirement event is combined with information on the income growth.

With respect to the overall background factors explaining the general residential mobility in the sample, our estimates only partly concur with the findings of Tatsiramos (2006). Our findings do not confirm being disabled or living with children as being statistically significantly related to moving. Concerning the variables log household income, rent-income ratio, and log rent per square meter, note that they must be interpreted together. In isolation the positive coefficient of income levels may seem implausible, but it must be seen in conjunction with the coefficient of the rent-income ratio where income appears in the denominator; similarly with the rent related variables.

Let us now turn to the analysis of actual rent paid as a cardinal measure of housing consumption expenditures. Our main goal here is to estimate the effect of income growth on rent growth, again separated between income growth of retirers and non-retirers. Since rent growth is likely to be correlated over time, we prefer a dynamic specification. In the left column of table 3 we therefore employ the Arellano-Bond GMM method where we allow for two lags of the endogenous variable. The diagnostic tests confirm the validity of the model's assumptions;

Table 2: Estimation results for any move (cross-check)

	move (FE)
lagged household inc. gr. at retirement	-1.87^{**}
	0.94
lagged household inc. gr. of non-retirers	-1.01^{***}
	0.33
retiring	0.63
	0.45
age	1.07^{*}
	0.62
age squared	-0.01^{*}
	0.00
loss of spouse	1.12^{*}
	0.66
new job	0.91
	0.72
living in couple t-1	-0.70^{*}
	0.41
change of household size	-0.27^{*}
	0.16
rent-income ratio t-1	2.14
	1.42
log household income t-1	1.07^{**}
	0.48
tenure in home (years/10)	2.66^{***}
	0.27
N, $\sum T_i$	269, 1840
equality test (income gr.)	0.84, p=0.36
Hausman test random effects	138.48, p=0.00

Notes: Panel logit estimates, RE – random effects, FE – (conditional) fixed effects. Standard errors below estimates, two-sided significance levels denoted by *** (1%), ** (5%), * (10%). Variable definition (see also subsection 3.3): "move" – moved into new home. Time dummies also included.

the residual second-order correlation is not significant and the instruments appear valid (Sargan test).

Again, the income growth variable for retirers appears highly significant, with a positive sign which means that income drops reduce rents paid. Furthermore, the corresponding income growth coefficient for non-retirers is close to zero, and here the equality of the two coefficients is even rejected at the (two-sided) 5% level. We obtain a broadly similar picture as with the logit specifications before: Income growth significantly affects housing consumption, and the effect is larger for retirers than for non-retirers. Nonetheless, the magnitude of the effect is small; this dynamic model reveals a long-run elasticity of 0.12 from income to rent for retirers, compared to an elasticity of 0.024 for non-retirers.

As a sensitivity analysis we also present a static model with robust standard errors, in the right-hand column of table 3. The coefficients also have plausible signs. Income changes affect rent changes positively, whereas relatively high rent levels as measured by a high rent-income ratio and a high rent per square meter lead to slower rent growth. And the longer the household has inhabited its dwelling the lower is the rent change.

In general, remember that the estimates only refer to non-home owners and as far as they contradict the life-cycle model they are likely to be larger than for home owners.

5 Conclusions

Based on the behavior of 55 to 75-year olds in the German SOEP we found that (foreseeable) retirement events have a partly significant negative effect on moving to cheaper homes, and thus on housing expenditures. Our point estimates of the income growth effects on moving (to cheaper homes or for cost reasons) and on rent growth proved always higher for retirers. According to the life-cycle model we would have expected the effects of foreseeable income changes at retirement to be zero or at least smaller than the impact of other income changes. This suggests that the consumption-smoothing hypothesis of the life-cycle model of consumption and

Table 3: Estimation results for rent growth

(selected regressors)	dynamic (2S-Arellano-Bond)	static FE w/ HAC s.e.
1st lag rent growth	-0.28^{***} 0.059	n.a.
2nd lag rent growth	-0.11^{***} 0.032	n.a.
household income growth at retirement	0.17^{***} 0.065	0.093^{*} 0.0561
household income growth of non-retirers	0.034^{*} 0.018	0.036^{**} 0.0182
change of household size	0.025 0.017	-0.058^{***} 0.0187
retiring	0.034 0.027	–
log household income t-1	–	-0.252^{***} 0.0382
rent-income ratio t-1	–	-1.42^{***} 0.187
log rent per square meter t-1	-0.44^{***} 0.101	-0.598^{***} 0.0378
tenure in home (years/10)	–	-0.046^{***} 0.0097
N, $\sum T_i$	1095, 4670	1838, 9601
log likelihood, pseudo R2	n.a.	1183.889, 0.444 (adjusted: 0.311)
F-test unit effects	n.a.	P(F(1837, 7739) > 2.11931) = 2.16286e-107
no residual AR	AR(2): p=0.847	AR(1): DW=1.93
Sargan over-id	$\chi^2(41) = 45.25$, p=0.299	n.a.
equality test	4.03, p=0.045	0.94, p=0.33
Hausman test vs. random effects	n.a.	3260, p=0

Notes: Linear panel estimates. Left column: Two-step Arellano-Bond GMM estimator with lagged endogenous variables. Right column: Static fixed-effects model with autocorrelation-robust variance estimator. Standard errors below estimates, two-sided significance levels denoted by *** (1%), ** (5%), * (10%).

saving may be violated for the large subgroup of non-home owners in Germany. In principle this evidence confirms the existence of a retirement-consumption puzzle. As the leading explanations of this puzzle are given by behavioral economic theories, aggregate models would have to allow for heterogeneous agents not only in the sense of different endowments and shocks, but also in the sense of different behavioral rules in order to capture these aspects of reality.

However, our results do not constitute strong evidence against the life-cycle model in quantitative terms. First, our sample was deliberately restricted to non-homeowners, and we expect the life-cycle model to be more accurate for home owners due to their systematically higher cumulated savings that financed their home in the first place. Secondly, the estimated effects for income drops at retirement were sometimes only weakly significant. And finally, the elasticity with respect to housing expenditure growth appears to be quite small. To summarize, our paper may explain why previous empirical results have been rather ambiguous regarding a rejection of the life cycle hypothesis: the relatively small effect may often be hidden by the noise in the data.

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A Institutional information Germany

It may be useful to summarize some characteristics related to the German housing market.

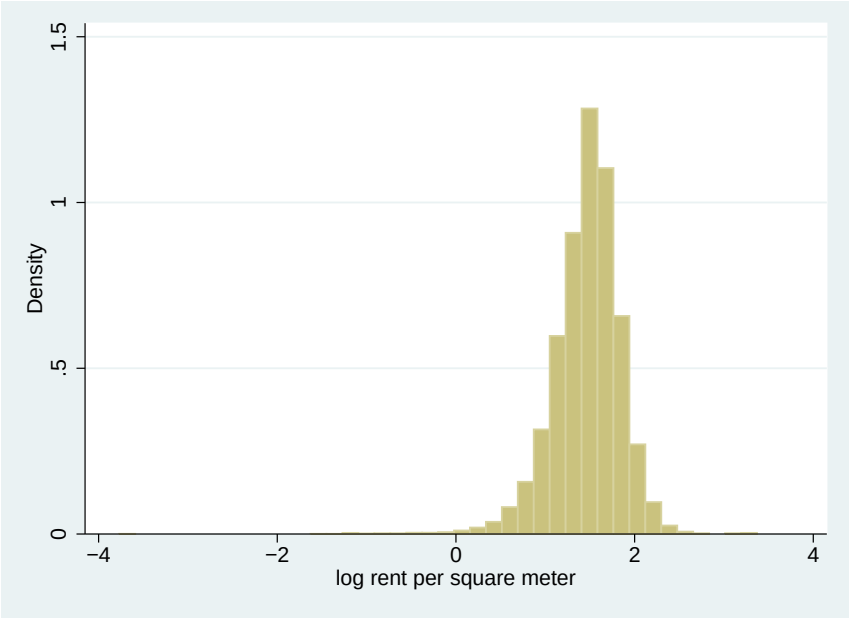
There is a means-tested subsidy called "housing money": *Wohngeld*. For registered unemployed it was subsumed under general unemployment benefits and social assistance (ALG II) starting in 2005 with the Hartz reform. Those who receive *Wohngeld* are already in an "appropriate" apartment, so they should not have any reason to move to a cheaper home. In any case, the subsidy is part of the measured household income. Unemployed people are not part of our sample.

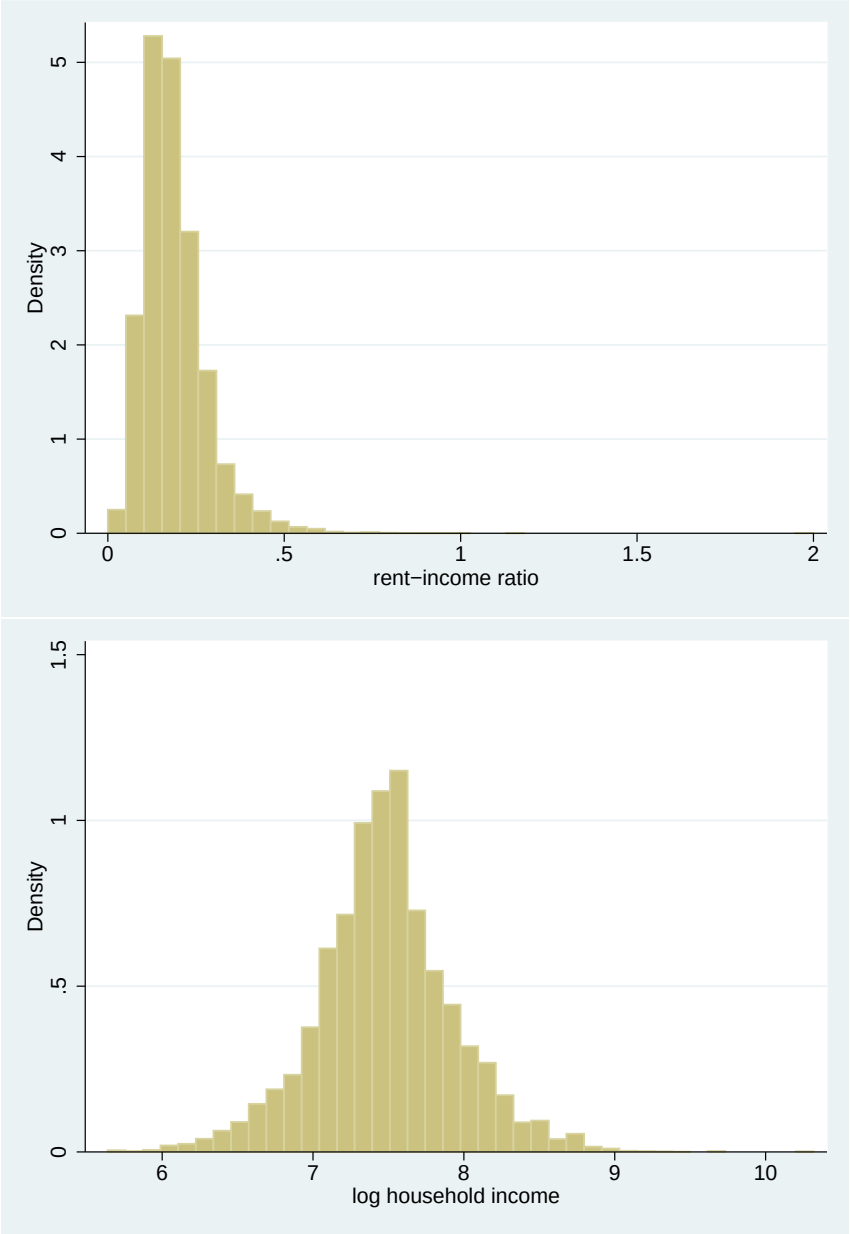
The time that must elapse after the tenant announces his desire to end a rent contract until the contract legally ends had been subject to another reform: since June 2005 it is generally only 3 months for tenants, whereas until August 2001 it was up to 12 months dependent on the past contract duration. Between September 2001 and May 2005 it depended on whether it was an old contract (old rules) or new contract (new rules). In contrast, for landlords it has always depended on the contract duration and mirrors the old rules for tenants (up to 12 months).

Regulation of rent increases applying to apartments which are not "price constrained" (without *Mietpreisbindung* –note that there are also apartments where construction was state-subsidized and rents are therefore price constrained):

- Within 3 years the rent in an existing contract can only grow by 20% (not counting recurrent costs like staircase cleaning or elevator maintenance etc. (*Betriebskosten*), or modernization expenses.
- The increased rent may not exceed the "local standard comparison rent" (LSCR, *ortsübliche Vergleichsmiete*) which is determined based on official surveys.
- Raising the rent requires mandated approval by the tenant; if the tenant does not grant this, the landlord must sue the tenant to legally get the mandated approval, and prove in court that the rent increase meets legal requirements (e.g. conforms to LSCR).
- A contract with a new tenant may not specify a rent exceeding the LSCR by 50%, but within this limit the landlady is in principle free to choose which amount she demands.

B Descriptive information on control variables

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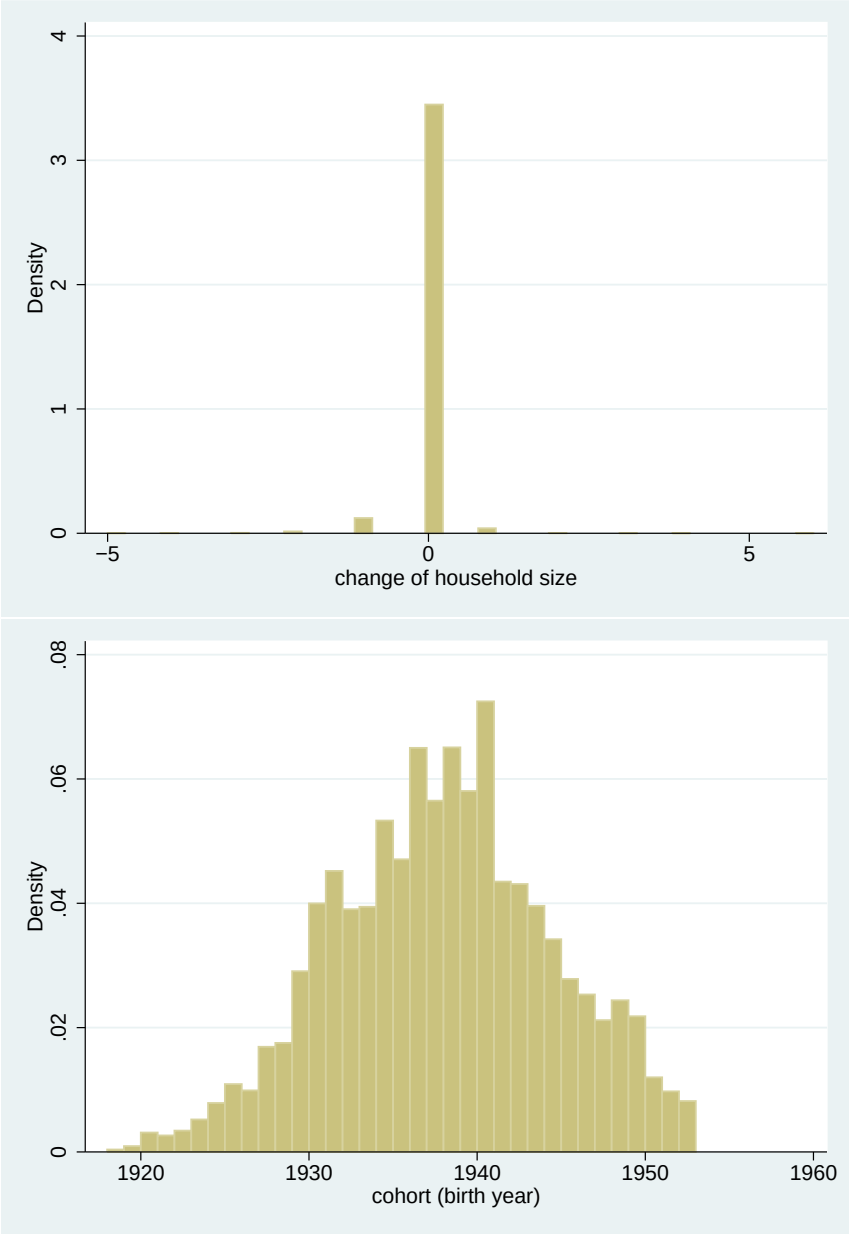
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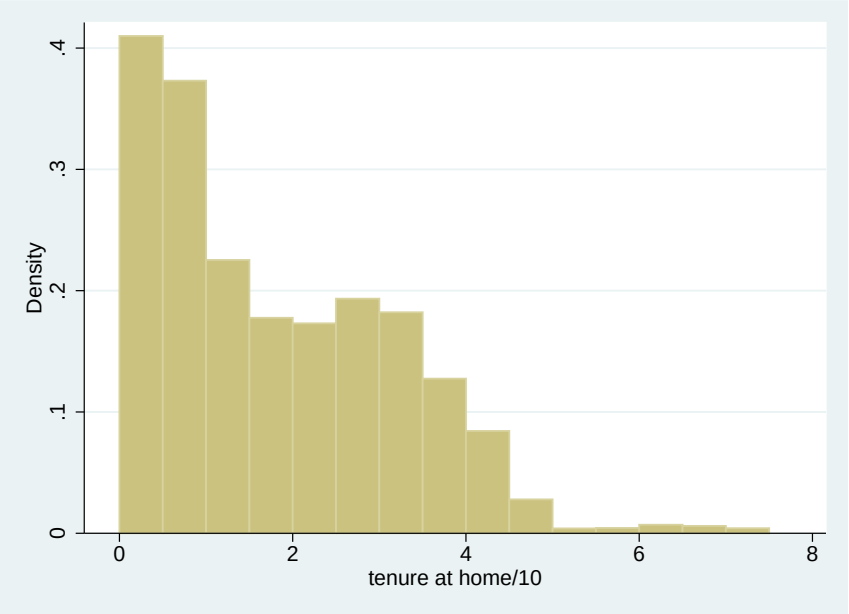
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