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Demography and housing demand—What can we learn from residential construction data?*

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Abstract

There are obvious reasons why residential construction should depend on the population's age structure. We estimate this relation on Swedish time series data and OECD panel data. Large groups of young adults are associated with higher rates of residential construction. But there is also a significant negative effect from those above 75. Age effects on residential investment are robust and forecast well out-of-sample in contrast to the corresponding house price results. This may explain why the debate around house prices and demography has been rather inconclusive. Rapidly aging populations in the industrialized world makes the future look bleak for the construction industry of these countries.

Sammanfattning

Det finns uppenbara skäl att förvänta sig att bostadsinvesteringar bör bero på befolkningens åldersstruktur. Vi skattar denna relation i en svensk tidsserie samt i en länderpanel omfattande OECD-länder. Stora grupper unga vuxna samvarierar med högre bostadsinvesteringar som andel av BNP. Men det finns också en signifikant negativ effekt från åldersgruppen över 75. Ålderseffekterna på bostadsinvesteringarna är robusta och ger goda prognoser för data bortom urvalet som använts för skattningen. Detta kontrasterar mot motsvarande resultat för husprisekvationer. Dessa resultat kan förklara varför debatten kring huspriser och demografi inte har lett till någon konsensus om effekterna. Snabbt åldrande befolkningar i den industrialiserade världen innebär dystra framtidsutsikter för byggnadsindustrin i dessa länder.

1. Age Distribution and Housing Demand

The debate following the publication of the much-cited paper "The Baby Boom, the Baby Bust, and the Housing Market" by Mankiw and Weil (1989) demonstrated that estimating the effect of demographic change on housing demand entails some stumbling blocks. A number of criticisms were raised against the Mankiw and Weil argument. Some papers suggest that the demographic effect vanishes when a properly specified model is tested (e.g. Engelhardt and Poterba, 1991), but there is no real consensus around which model is properly specified. Swan (1995) argues that the demographic demand variable constructed by Mankiw and Weil, in effect double counts population rather than measuring demand and ignores real income growth, but still finds substantial demographic effects after correction for that.

One difficulty is that using cross-section data to assess the housing demand of different age groups confound differences between cohorts with differences between age groups (Green and Hendershott, 1996). There are, however, several other reasons why house prices may move out of step with demographic change. First, the asset price of houses may move in the opposite direction of rental rates, since the latter depends on user costs while the former also depend on anticipated capital gains, a point made by Hamilton (1991), and the demand for housing, of course, reacts on the rental rate rather than the asset price. Second, institutional and social changes may cause a move towards home-ownership that could drive house prices upwards even if rental rates and housing demand are decreasing. Third, the standard Poterba (1984) model of stock-flow equilibrium in the housing market also implies that the price responses to persistent shocks may be confounded by initial overshooting due to the time lag inherent in adjustment of the housing stock. An initial price increase in response to a positive demand shock may be masked in data by the following decreases as prices adjust downwards as the housing stock adapts.

The quantity response is, however, unambiguous in direction. One crucial point is that in the long run housing supply should be very elastic. Hence persistent changes in demand—e.g. demographically induced changes—will mainly be reflected in quantity rather than price adjustments. Holland (1991) makes this point and shows that US net residential investment actually is cointegrated with the Mankiw-Weil demand variable, while that is not the case for real house prices. Pettersson (1990) in a Swedish study on historical data back to the 19th century also notes a connection between demography and building cycles.

Our approach is inspired by the cited papers but differs by letting the influence of population age shares be directly estimated without imposing a given form on how demand varies with demography. The gain of this is that age effects are estimated without any potentially inappropriate a priori restrictions. This approach also makes it easy to forecast using demographic projections. We do indeed find that in Swedish post-war data residential investment has a stable and theoretically expected long-run statistical relation to the age distribution. Young people at the age of household formation show a positive correlation to residential investment and old people have a strong negative effect. But we fail to find a similar stable relation between the age distribution and house prices although a strong correlation is present. Our estimates of age effects on residential investment on the other hand remain quite stable over different housing policy regimes. Similar results hold in an OECD cross-country panel thus corroborating our Swedish results.

In the next section we first establish that a strong and stable correlation between residential investment and age structure is present in both a Swedish time series 1950-1996 and an OECD panel 1964-1995. In Section 3 we discuss the theoretical background further and demonstrate that house price regressions on age structure fail to show a corresponding stability. Finally in Section 4 we sum up our observations and conclusions.

2. Residential investment and age structure

Our point of departure is an exploration of the correlation pattern between age structure and residential investment. We use Swedish residential investment 1950-1996 as share of GDP as dependent variable and regress this on the population age shares. Details regarding data can be found in the appendix. Since the sum of age shares equals one we drop the intercept rather than an arbitrary age share. It is intuitively simpler to interpret estimates using all age shares. We need to keep in mind though that the linear combination of age shares also estimates the intercept.

The level of detail in the specification of the age distribution entails an important trade-off. Age group shares are rather highly correlated and it will in general be difficult or impossible to achieve reasonably precise estimates of age group coefficients if all one-year groups or even all five-year age groups are used as regressors. In the literature this is dealt with either by aggregating the information of the distribution into one or two variables (using dependency ratios or

mean age or age group ratios) or by imposing a polynomial restriction on the coefficients (like in Fair and Dominguez, 1991 or Higgins, 1998). Mankiw and Weil (1989) also construct their housing demand variable by imposing a housing demand structure that has been estimated in a microeconomic cross-section. These restrictions, however, discard some information on relative movements in the distribution that are important. Curiously, Mankiw and Weil actually report two different age profiles estimated from microeconomic data. From the differences in these profiles it seems that the elasticity of demand differs over age groups, thus a priori invalidating the restriction imposed. We have found that aggregation to five or six age groups with approximately homogeneous economic behavior is a working compromise in most situations.

Here we subdivide the population into six age groups. *Children* 0-14 years old obviously do not take economic decisions themselves, but are rather the result of economic decisions. Therefore, when we later (in the panel estimations) have to drop an age group we drop the children. *Young adults* 15-29 years old is the group where household formation takes place. *Mature adults* 30-49 years old are predominantly found in family households with children. *The middle aged* 50-64 years old are at their peak income and net savers. *Young retirees* 65-74 years old are still active consumers but no longer working. *Old retirees* 75 and above are more exposed to ill health restrictions and have considerably higher mortality rates.

2.1. Swedish residential investment 1950-1996

Demographic projections with relatively low uncertainty can be made at least 20 or more years into the future. Although this paper is not about forecasting per se, it is of interest to start by exploring how well a pure age model of residential investment is forecasting out-of-sample. This is so because in regressing variables with high persistence on each other there is always a risk of spurious results. Out-of-sample forecasts provide a good check against this. In Table 1 we therefore present the results from exploring a pure age model with and without lags of the dependent variable and comparing these with estimates on non-residential construction in order to rule out that we are picking up some general business conditions effect.

We find that working age population has a positive effect on residential investment while increasing population shares in the dependent age groups have a depressing effect on residential construction. In the pure age model the youngest

adult age groups have a more positive effect on residential construction than the older working age groups.

The estimates for non-residential construction, although equally dependent on age structure, show a quite different age pattern. Here, there is a clear peak for the 50-64 age groups. Thus we can exclude the possibility that we happen to catch a general investment effect in the age pattern.

In all cases the age specification captures a very substantial part of the variation in construction activity, see Figure 1, where we include the predictions from a pure age model and compare them to the latest data from Statistics Sweden for 1993-2003. The predicted upturn around 2010 reflects an increase in the share of young adults in the population as the baby boom cohorts born around 1990 reach the age of household formation.

While it is uncontroversial to claim that a larger inflow of young people into the age groups of household formation will shift up housing demand and consequently are expected to stimulate residential investment, the claim that increasing old age groups will depress housing demand is more controversial, since it could be a cohort effect due to increasing life time incomes over cohorts. It is, therefore, important to evaluate the evidence on this point more closely.

Pitkin and Myers (1994) investigates this and finds that there are indeed strong cohort effects, but there still remain a depressing effect from the oldest when that is controlled for. Theoretically optimal life cycle consumption plans will under finite life assumptions attain a pronounced hump shape over age. Empirical evidence indicates that hump shaped demand over age is present for many types of consumption and that age-specific mortality rates indeed are important explanations, although there are probably also other mechanisms at work, see e.g. the study by Banks et al. (1998).

Increasing death rates when the share of old people increases also means more vacancies will be created in the existing stock, everything else equal. The demand effect from an increasing share of retirees with declining housing consumption is thus reinforced by a supply effect as well.

As shown in Figure 1 there is one period around 1990 when the fit of the pure age model is less good. This can be chalked up to a structural break in the time series. There was a major Swedish tax reform in 1991 that made interest deductible against capital income—at a uniform tax rate of 30 percent—instead of as previously against income with marginal tax rates ranging between 30 and up to 80 percent. For most people this reform meant a substantial increase in capital costs. At the same time a gradual withdrawal of interest subsidies for housing

and an introduction of property taxes on own homes further increased user costs and caused a collapse in the residential construction market as house values on average plummeted with around 25 percent. Still, in spite of this confounding break at the end of the time series the forecast model performs very well. The out-of-sample forecast from 1997 and onwards catches the general trend in the out-of-sample years extremely well for such a long forecast horizon. That is prima facie evidence against the possibility of a spurious regression. If the relation is spurious such forecast performance is extremely unlikely.

Introducing lags in the specification picks up persistent effects from the variables—and we would expect housing demand effects to be persistent. With a lag coefficient of around 0.8 the long-term effect is approximately $1/(1-0.8) = 5$ times larger, so the age coefficients should decrease by a factor 5. This is approximately true for the young adults but not for the other age groups. This may be indicating that indirect and less persistent effects are attributed to the age coefficients when we control for low frequency variation by the lagged dependent variable.

The autoregressive coefficient is also fairly high causing renewed concern that R^2s and t-values are unreliable. The rule of thumb that R^2 greater than the Durbin-Watson statistic indicates spurious regression means that this possibility still cannot be ignored. Although the pure age model seems to pick up the trends in construction fairly well and also exhibit the expected age profile, some statistical problems still are present which merit further investigation in the following sections.

2.2. Framework for interpretation

To continue the investigation it is helpful to have a look at the theoretical framework underlying the Mankiw-Weil debate to interpret the estimations. The standard stock-flow partial equilibrium model developed in Poterba (1984) describes the adjustment of house prices and the housing stock to long-run equilibrium. Although more elaborate general equilibrium models are available today (see Leung 2004 for an overview), the debate based on Mankiw and Weil (1989) which our paper centers on were mainly within the Poterba framework. While richer models, of course, contribute to a more complete picture of the housing market and its interaction with the macroeconomy our ambition is limited to the more modest empirical demonstration that quantity adjustment is well captured by demographics while prices are not.

Under the usual regularity conditions we can solve for the short-run equilibrium

rental rate $q^* = q(H)$ which is decreasing in the housing stock H and derive the dynamic path of the standard unit house price Q. Let the user costs of the standard unit be ν, which includes depreciation, repair and maintenance costs, and real capital costs (net of taxes). Real house price changes are then given by

$$\dot{Q} = -q(H) + \nu Q. \tag{2.1}$$

Following Poterba (1984) construction investment, $\psi(Q, r)$ is assumed to be an increasing function of real house prices but we add to this that residential investment could also be a decreasing function of real capital costs, r. Adding a depreciation term the dynamics of the housing stock are then described by

$$\dot{H} = \psi(Q, r) - \delta H. \tag{2.2}$$

Setting the rates of change to zero we get what amounts to long-run demand and supply schedules. The intersection determines the steady-state values of the real house price and the housing stock in long-run equilibrium.

Within this standard model framework age structure may affect several relations. Here only two important points are noted:

- First, the level of the rental rate for a given housing stock should increase with the number of people in high demand age groups, i.e. essentially the working population. This shifts the long-run demand schedule upwards. House prices momentarily increase but along the adjustment path *negative* rates of change will be observed following the shock. Since an age shock is persistent and takes place in many small steps, it will be hard to distinguish the impact effect from the adjustment effects in actual data.

- Second, the quantity of housing demanded increases with income growth. This effect is higher for the younger cohorts with anticipated higher life cycle income than for the older. This implies a continuous upward drift of the demand schedule with income growth that is moderated according to the age structure. This drift makes separate empirical identification of income and age effects difficult.

Age structure related shifts in housing demand imply that the dynamic process is aiming at a shifting steady-state target. In particular we would expect an upward drift due to income growth. As remarked in the introduction real income growth cannot be ignored and recent papers, like Davis and Heathcote (2005) using

a production function approach, verify that this is empirically important at the macro level. To get rid of this systematic shift we focus on the ratio of residential investment to GDP. Admittedly this is a rough way of compensating for the drift but it has the advantage of placing the measurement error we make in using GDP for personal income on the left hand side of the regression equation thus avoiding the bias we would otherwise have to deal with. The income elasticity of demand is commonly assumed to be unitary while the corresponding elasticity of supply is controversial but commonly found to be rather high, see e.g. Harter-Dreiman (2004). The empirical results below do not indicate that this approximation leads to any systematic bias although standard errors may be higher than they would have been in a more sophisticated specification.

In Figure 2 we see the Swedish age distribution from 1950 to 2000. If the supply schedule is stationary we would expect that the entry of the 1940s baby boom into middle age brackets will shift the demand schedule for housing relative to income downwards since they are now at the peak of their housing career. As the 1940s cohorts start to retire in the next decade we would expect housing demand to subside further and then to rise again as the latest baby boom reach household formation ages.

The income normalized gross volume of housing construction I in short-run equilibrium is linearized using equation (2.2).

$$I = \alpha_0 + \alpha Q + \beta r, \quad \alpha > 0, \beta < 0. \tag{2.3}$$

Substituting for Q by using equation (2.1) we can express residential investment as a fraction of GDP, I, as

$$I = \alpha_0 + \alpha \frac{q + \dot{Q}}{\nu} + \beta r \tag{2.4}$$

Our basic assumption that housing demand varies systematically over age leads to a dependence of the rental rate q on the age distribution of the population. We do not derive the form of this influence but approximate it by a sum of weighted age group shares where the weights are to be estimated. We use shares since we also want to avoid the upward shift in aggregate housing demand caused by population growth. The normalisation of the dependent variable by dividing with GDP accounts both for increases in GDP per capita and population growth. Using a_k for the age group's share of total population we arrive at a regression

specification that extends the pure age model estimated above

$$I = \alpha_0 + \frac{\alpha}{\nu}\sum_{k=1}^{n}\omega_k a_k + \beta r + \frac{\alpha}{\nu}\Delta Q. \tag{2.5}$$

User cost per index unit ν is part of the coefficients for age structure and the rate of change in house prices. Unless α/υ is at least stationary over time this may cause problems. However, modelling user costs explicitly leads into practical measurement difficulties. It is difficult to find reliable measures for the relevant marginal tax rate as well as the expected real interest rates. Even more important in this long time series context we have in the Swedish case shifts in housing policies. These shifts affect user costs in ways that are very hard to summarize in any simple form. The robustness of the pure age model gives an indication that it is defensible to simplify by assuming the coefficients to be stationary, and this is vindicated by the results below.

Though we cannot take into account all possible shifts in user costs, the theory indicates that we should at least control for capital costs, but real interest is already included in equation 2.5. In Table 2 we report results using the specification above. House price changes and real interest are lagged one step since planning and adjustment lags in construction are quite long.

This specification turns out to improve the precision of the age estimates considerably. Recursive coefficient estimates with successively shorter time series are stable back to the late 1970s even if there is some minor turbulence in the extreme years in the beginning of the 1990s, when tax reforms and strongly increasing loan-to-value ratios rattled Swedish mortgage markets. Thus, we feel confident of the empirical validity of our assumption about stationary coefficients. Since house price changes and real interest rates could be suspected of endogeneity and simultaneity we also run instrumented regressions.[1]

The age coefficients are quite stable over different specifications, with the exception of young retirees which becomes statistically insignificant as a tax reform dummy (=1 from 1991 and onwards) is introduced along with a dummy for capital market deregulation (=1 from 1985 and onwards). A strong positive effect from age groups in the process of household formation and a strong negative effect from age groups over 75 conform to the results in Table 1. The essential difference to the pure age model is the negative effect from the middle-aged. This may have to

[1] The instrument set includes the third and fourth lag of the house price index and the second and third lag of inflation and real interest rates.

do with their portfolio shift from real to financial assets, i.e. with asset demand rather than housing demand.[2] Inter alia Andersson (2001) for Sweden and Skinner (1989) for the US present microeconomic evidence that such a portfolio shift really takes place around 50 years of age. House price changes have the expected positive effect and the negative effect from real interest is also expected in the investment equation. However, the interest effect vanishes when we instrument the regressions.

The tax regime change in 1991 and the capital market deregulation in the 1980s should have affected both interest and inflation responses in the economy. Both these structural breaks may explain why the Durbin-Watson test for first-order serial correlation is in the middle of the inconclusive range where we can neither accept nor reject the hypothesis of no serial correlation in the residuals. Indeed as we control for the structural breaks the null of no first-order serial correlation is accepted at the five percent level. The tax reform significantly raised user costs so it is slightly surprising that we get a significant positive effect of the change in tax regime on residential investment. Recall, however, that increases in user costs would directly impact on the rate of house price change, which is already included. The coefficient of ΔQ increases considerably when the tax reform dummy is included thus indicating a collinearity effect. Excluding the price change variable (not reported) indeed changes the sign of the tax reform dummy to the expected negative value. The negative dummy coefficient for capital market deregulation decreases residential investment levels compared to previous regulated markets. Since the purpose of the abolished regulation was to stimulate housing construction by different forms of subsidies this is what we would expect.

Summing up the time series evidence so far we have two tentative conclusions.

- There are strong and important age structure effects that explain most of the variation in residential investment in Sweden since 1950.

- The negative effect from the 75+ group on residential investment as compared with its effect on non-residential construction is consistent with a considerable negative effect on housing demand by declining individual demand in this group. However, as mentioned mortality rates are accelerating

[2] This conjecture is based on the observation that the negative effect is much less when we do not restrict the coefficient on the first and second lag of prices to be equal in magnitude. Then the positive first lag effect is greater. The theoretical restriction to a differenced term thus tends to underestimate the total price effect on investment and some of the negative relative price effects of the portfolio shift are caught by the age group instead.

at this age creating more vacancies in the existing stock and this is probably a complementary important supply channel for the negative effect.

There are no longer any indications that the regression might be spurious. The residuals are stationary and in fact passes a test for normal distribution. Because cointegration implies that the LS estimates are superconsistent, converging faster than normal to the true values, we found it worthwhile to estimate an error correction model—a methodology that has otherwise mostly been used in house price models, see e.g. Malpezzi (1999) for a general apporach and Heiborn (1998) who applies it to Mankiw-Weil type age demand variables in the Swedish context. The ECM estimates are reported in the last column of Table 2. The coefficient on the linear combination of age shares (using the values in column 4) minus the residential investment lag is close to 0.9 indicating a fairly fast convergence to the long-run equilibrium. The restriction of the coefficient for the linear combination of age shares to be the same with opposite sign as the coefficient for the residential investment lag is clearly accepted by the data. We take this as final evidence that spurious regression is no issue here. In the appendix a more detailed account of the ECM model can be found.

2.3. Panel data OECD 1964-1995

The time series estimates for Sweden could be unique for some institutional or historical reason. Therefore we decided to check a cross-country panel sample to see if the results survive in a wider context. For panel regressions to work well the cross-section sample should be reasonably homogeneous in order to minimize problems with model heterogeneity. This consideration led us to focus on OECD data with similar income levels and construction technology, although there are still large disparities in terms of housing policies and mortgage markets that could make heterogeneity an issue. Imperfections in capital markets are important determinants of both house prices and construction (Chen 2001, Ortalo-Magne and Rady 2004) and housing credit markets do differ quite a lot across countries. The results below must therefore be interpreted as average patterns with potentially substantial variation across countries.

When we use panel data we can estimate a fixed effect model which allows us to take account of unobservable country-specific effects as well as time-specific effects. This gives us a better control for omitted variables and period and cohort effects. The assumption is that the error term can be decomposed as

$$u_{it} = \mu_i + \kappa_t + \varepsilon_{it} \tag{2.6}$$

where the country-specific effect μ_i is taken to be constant as is the time-specific effect κ_t and ε_{it} a white noise disturbance. An assumption that these terms are all random results in a more efficient estimator since no degrees of freedom are lost estimating fixed parameters. However, this random effects model is also more sensitive to mis-specification bias of different kinds. Our specification is based on several radical simplifications and may well suffer from specification problems so we prefer the more robust fixed effects estimator.

In Table 3 we report a number of regressions with housing investment as the dependent variable and both fixed time and country effects. In the panel estimations we have to drop one age group (children) since we estimate time- and country-specific intercepts (also note that the Durbin-Watson statistic is not reported since it is invalid in the panel context). In column 1 we now see a somewhat different age pattern with the 30-49 group having the largest positive effect on residential investment. It is well known that many countries in Southern Europe as well as Japan exhibit a pattern of household formation that is considerably delayed as compared to the Swedish pattern where almost everybody have left home at the age of 30 (for one explanation relying on availability of housing credits, see Chiuri and Jappelli 2003 who are using an extensive international micro data material).

In column 2 we put in the relative price of investment as a proxy for house prices and capital costs as a means to get some control for differences in credit availability. This is significant with the correct sign but the age pattern reappears although with an insignificant coefficient for the 50-64 age group. The relative price of investment is likely to be simultaneously determined with residential investment shares so in column 3 we also report TSLS estimates.[3] This reestablishes the 15-29 group as the prime positive influence on residential investment but the middle age group is still insignificant separating the OECD pattern from the previously found Swedish time series pattern. Coefficients shrink and are less precisely estimated as we instrument for relative investment prices. However, as subsidiary evidence that the correlation between age structure and residential investment is a general phenomenon not due to peculiarities of the Swedish data, these results do corroborate our previous results. The evidence also indicate that the precise age pattern is likely to be quite sensitive to national peculiarities in the availability of housing finance.

[3]Instruments are age variables and lagged GDP deflators and the second and third lag of the relative price itself.

3. Age Effects on House Prices?

In eq. 2.5 we have also implicitly defined a house price regression

$$Q = \sum_{k=1}^{n} \omega_k a_k + \frac{1}{\nu} \Delta Q + u \tag{3.1}$$

where we would expect the age variables to show the same pattern up to a constant multiplier as in the residential investment equation. Note that our formulation also makes the dynamic system recursive and allows us to ignore residential investment as an independent factor in the determination of house prices.

3.1. Swedish house prices 1952-1999

Demographic data and CPI indexes were obtained from Statistics Sweden and house price series 1952-1999 (linked from Statistics Sweden sources) was kindly put at our disposal by Lennart Berg who also provided the interest series based on rates of interest offered by mortgage underwriters. Further details on data are available in the appendix.

As is clear from Figure 3 there are two quite distinct regimes in house price dynamics in Sweden, one calm period up to 1978-1979 and another much more volatile thereafter, which correspond to the beginning of the deregulation of the previously highly regulated housing and credit market. In order to get any sensible results at all in the price regressions we therefore had to make the age coefficients regime dependent by multiplying one set of age variables with a dummy for the period 1979-1999 and then including both sets of variables in the regression. The first two columns in Table 4 report a LS estimate for the early period. The second column specify the addition that has to be made to the first column in order to get the coefficient for the later period. As witnessed by the estimated model in Figure 3 this gives a fairly good fit.

Starting with the least squares estimate in the first column, the coefficients in the period 1953-78 are actually the opposite of the expected pattern. The age groups that promote residential investment actually lowers prices and vice versa except for the positive coefficient on the young adults. This could be because we are estimating a reverse causality where increased residential investment causes prices to go down. It is, however, also possible that this reflects the rather heavy subsidies to families with children and pensioners during this period, which should have made these groups less price sensitive. However, adding the second column

for the later period we get very strong positive effects from the young and the elderly but an equally strong negative effect from mature adults. This is much harder to make any sense of, since subsidies have been successively removed or diminished during this period. There is also a clear indication of autocorrelation in the residuals so further investigation is needed.

The TSLS estimate[4] in columns 3 and 4 (with price change on the LHS and including interest on the RHS as a control) yields marginally more reasonable results. There is no significant autocorrelation and the estimates in the regulation period could be rationalized if the middle aged are no longer saving in real estate and thus influence prices negatively. However, as we add the dummy interacted coefficients the strong negative mature adult effect is still not credible even though the rest are at least in the approximately right direction.

Testing for a unit root in house prices this cannot be rejected. Thus we may have a spurious regression problem. In order to see if more sensible results can be achieved by using the change in price on the LHS instead, i.e. assuming that price levels and age shares are cointegrated the equation

$$\Delta Q = -\sum_{k=1}^{n} \gamma_k a_k + \gamma_3 Q + u \tag{3.2}$$

was estimated by GMM and reported in the last two columns. Since we would now expect a reversed pattern only the negative effect of young adults is really expected.

It is conceivable, however, that the group of age variables would perform well for forecasting even if insufficient variation, reverse causality and multicollinearity prevent reasonable individual coefficient estimates. After all, the fit of the pure age model in Figure 3 is not that bad, even if we identify the age coefficients in the deregulated period by only some twenty observations. However, projecting a forecast of the pure age model for prices results in a 400 percent real price index increase by 2010, a forecast which clearly cannot be taken seriously. Thus, we conclude that age variables perform much more poorly in Swedish house price equations than in residential investment equations. To judge from our results this is probably due to...

- ...residential investment adapting to the demographic changes in a similar

[4] Both the instrumented estimates in Table 4 have an instrument list that includes—besides age variables—the second and third lag of real interest rates, real house prices and inflation.

way both under the current market regime and under the previous more regulated regime.

- ...that the impact effect and adjustment effect of residential investment goes in the same direction, which is not the case for the price effect where the impact effect overshoots the equilibrium level.

- ...that residential investment covers the whole housing market and not only owner-occupied housing where prices do not necessarily co-vary with the relevant demand price, that is the rental price.

More sophisticated models can, of course, be developed in order to estimate price effects from demography—and above references to such work have been given—but the simple and easy-to-forecast approach just using age structure and demographic projections— that works for residential investment—will not work for house prices.

4. Conclusions

The Mankiw-Weil debate on demography and housing demand could give the impression that demographic effects on housing demand are unimportant or spurious. As we have tried to show in this paper, such a conclusion can be mistaken. A preliminary conclusion is that theoretical considerations and empirical data,— although not strictly conclusive— support a clear effect of demographic change on residential construction even if the price effect for several reasons may be less clear-cut. Given limitations in the data available, our empirical results show that the quantity effects work much the same over different financial regimes as well as housing policy changes.

A noteworthy finding —in view of the rapid population aging that the OECD countries will undergo in the next century—is the negative effect on residential construction of increases in the old age population. This is a robust feature in our Swedish results as well as in the OECD panel results. It is a feature we should expect when a population of individuals with finite lives gets older.

It would, therefore, be unwise to ignore that population ageing might affect housing demand negatively. Rather, more research into clarifying the mechanisms of these effects seems called for. An important reason is that the correlation between age structure and residential construction can prove to be a valuable forecasting tool. A good forecasting performance of a very simple regression of

residential investment on age structure in out-of-sample Swedish data for several years ahead does not contradict this possibility.

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A. Data appendix

We have used several sources for our data, and the Swedish time series are not publicly available, but we will provide the relevant data on request for anyone interested in replicating our results.

Our Swedish residential investment variable was kindly provided by Byggentreprenörerna, an organization for Swedish construction firms. Data from this source are available in a consistent series 1950-1996 while National Accounts data have to be linked due to changes in definition. The residential investment (expressed in constant 1991 SEK) was divided by total GDP (also in constant 1991 SEK) to get the rate used in the regressions.

The house price measure is a real estate index constructed on the basis of real estate taxation values. Taxation values are based on local market transactions weighted by the distribution of local real estate over a number of standard categories. It therefore also reflects the upward trend in housing standards. In its current form this index is available from 1975 and onwards. Lennart Berg at Uppsala University has interpolated the series backwards to 1952 by trends in local market transaction prices (so called 'köpeskillingskoefficienter'). We also owe him for the compilation of mortgage interest rates that we have used as controls. Demographic data are from Statistics Sweden, estimates and projections downloaded in 1997.

In the panel estimation we use disaggregated investment data from OECD (1997) Economic Outlook. Our basic OECD sample contains 18 of the OECD countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany (BRD), Ireland, Italy, Japan, New Zealand, Norway, Spain, Sweden, Switzerland, the United Kingdom, and the United States. Missing data for several countries in the beginning of the 1960s made it simpler to start from 1964. We have a full set of annual observations from 1964-1995, except for West Germany, for which the economic series end in 1994. We then have at most 32 complete observations per country (31 for West Germany). Since we lose one observation in calculating growth rates our estimation sample adds up to a total of 557 observations. Annual demographic data are from United Nations (1994) starting in 1950. Again West Germany is an exception: we obtained age structure data for this country from Statistisches Bundesamt.

The investment rates were in this case taken from current price ratios. The comparability over countries of price deflators is dubious, so we considered the current price ratios to be safer. We had difficulties in finding comparable interest

and house price data for the whole sample and finally substituted the relative price of investment to get some proxy for these variables. These data were obtained from OECD (1997) National Accounts.

B. ECM equation

Standard tests cannot reject the null hypothesis that the residential investment share series is integrated of order 1. In the sample period this will be the case for several of the age share series, too. There is thus hypothetically a risk that the high t-values and R^2s are spurious regression results. Also a fractionally integrated series—"long memory" or persistent series—may give rise to spurious regression in the sense that t-values and R^2 converges to spurious values (Tsay and Chung 2000).

However, if the age shares form a linear combination with the residential investment series and that linear combination is stationary, our results should still be consistent. In fact they are superconsistent with substantially faster convergence to the true parameter values than an ordinary OLS estimate. In order to check whether spurious regression is a problem we estimate an error correction model based on the specification we used above. Estimating a co-integrating vector by e.g. the Johansen procedure cannot be implemented in this case. Johansen's method is based on a VAR-system where it is assumed that the co-integrating variables are endogenously determined. With so many variables with long memory a VAR system with many lags runs into serious identification problems on annual data, so this is not a feasible option here.

The basic ECM builds on the generic model

$$y_t = \beta_0 + \beta_1 x_t + \beta_2 x_{t-1} + \beta_3 y_{t-1} + \varepsilon_t.$$

Under the assumption that $\beta_1 + \beta_2 + \beta_3 = 1$ an ECM representation can be derived by assuming a stationary long-run relation $y - x$

$$\Delta y_t = \beta_0 + \beta_1 \Delta x_t + (\beta_3 - 1)(y_{t-1} - x_{t-1}) + \varepsilon_t.$$

With only around 45 usable observations—depending on the number of lags used as instruments—and six age shares plus another five explanatory variables we necessarily have to restrict this generic model. Since age shares are highly serially correlated identification problems arise if we include both current and lagged values of the age shares. We use current values to achieve as little collinearity

as possible with the other lagged explanatory variables.This does not invalidate the basic idea. Instead of the generic model above we have

$$y_t = \beta_0 + \beta_1 x_t + \beta_3 y_{t-1} + \beta_4 z_t + u_t$$

where z are other independent variables. Assuming now that $\beta_1 + \beta_3 = 1$ it is easily seen that we can replace $\beta_1 = 1 - \beta_3$ and subtract y_{t-1} from both sides to get the equivalent form

$$\Delta y_t = \beta_0 + (1 - \beta_3)(x_t - y_{t-1}) + \beta_4 z_t + u_t$$

The basic level equation we have used previously is (2.5) and we only need include a lag of residential investment in this to get an ECM representation of this form.

$$\Delta I = \beta_0 + (1 - \beta_3)\left(\sum_{k=1}^{n} \beta_k a_k - I_{-1}\right) + \beta r_{-1} + \gamma \Delta Q_{-1} + \boldsymbol{\delta}'\mathbf{X} + u \qquad \text{(B.1)}$$

Table 1: Time series regressions on Swedish data 1950-1996. Least squares estimates. Standard errors are corrected for heteroskedasticity and autocorrelation up to 3 lags. Absolute t-values in parentheses, bold face marks estimates significantly different from zero on the 5 percent level. Q is the p-value for the Ljung-Box Q-test for serial correlation in the residuals with 11 lags.

Dep. var.	Residential construction		Non-residential construction.	
	pure age model	*with lag*	*pure age model*	*with lag*
Lag of dep. var.	—	**0.825**	—	**0.615**
		(8.09)		(4.73)
Age 0-14	**-0.536**	**-0.359**	**-0.262**	**-0.118**
	(2.62)	(3.76)	(2.68)	(2.03)
Age 15-29	**0.746**	0.119	0.166	0.087
	(2.76)	(0.86)	(1.60)	(1.44)
Age 30-49	**0.329**	**0.234**	0.012	0.039
	(3.65)	(4.55)	(0.23)	(1.38)
Age 50-64	0.156	0.146	**0.719**	**0.274**
	(0.43)	(1.13)	(4.91)	(2.61)
Age 65-74	0.025	0.109	-0.228	**-0.199**
	(0.07)	(0.73)	(1.81)	(3.07)
Age 75+	**-1.553**	**-0.649**	**-0.332**	-0.132
	(4.73)	(3.04)	(2.80)	(1.60)
Adj R^2	0.83	0.94	0.91	0.95
D-W	0.45	—	0.72	—
Q (p-values)	0.00	0.42	0.00	0.87

Table 2: Time series regressions on Swedish data. Standard errors are corrected for heteroskedasticity and autocorrelation up to 3 lags. Absolute t-values in parentheses. Q is the p-value for the Ljung-Box Q-test for serial correlation in the residuals up to 10 lags. Sargan is the p-value for a test of whether the overidentifying restrictions implied by the instrument regressions hold. Restriction is the p-value for the hypothesis that the coefficient for the linear combination of age variables is the same as the lagged residential investment coefficient with opposite sign, i.e. the coefficient for the adjustment term in the ECM equation.

Dep. var.	Residential investment			ΔResid. inv.
	LS	*TSLS*		
Constant				**0.052**
				(10.2)
Age 0-14	**-0.345**	**-0.222**	**-0.318**	
	(4.79)	(3.70)	(4.14)	
Age 15-29	**1.311**	**1.184**	**1.221**	(adjustment
	(16.9)	(7.71)	(12.3)	term)
Age 30-49	**0.466**	**0.400**	**0.498**	**0.883**
	(13.9)	(9.32)	(11.8)	(10.2)
Age 50-64	**-0.589**	**-0.612**	**-0.757**	
	(5.63)	(3.97)	(6.21)	
Age 65-74	**-0.754**	-0.402	**-0.071**	
	(6.27)	(1.56)	(0.30)	
Age 75+	**-1.373**	**-1.545**	**-1.857**	
	(15.7)	(10.6)	(8.83)	
ΔQ_{-1}	**0.642**	**0.694**	**1.625**	**0.569**
	(4.15)	(3.51)	(6.11)	(3.51)
Real interest$_{-1}$	**-0.146**	-0.050	-0.003	-0.058
	(6.31)	(0.86)	(0.07)	(1.68)
Tax reform 1991-			**0.011**	0.001
			(3.91)	(0.75)
Deregulation 1985-			**-0.009**	0.002
			(5.24)	(0.87)
Adj R^2	0.96	0.97	0.96	0.51
D-W	1.59	211.53	2.13	—
Q (p-values)	0.12	0.05	0.09	0.20
Sargan	—	0.36	0.38	0.31
Restriction ECM				0.74

Table 3: OECD 18 country annual sample 1964-1995. Sample of 539 obs. Standard errors are corrected for heteroskedasticity and autocorrelation up to 3 lags. Absolute t-values in parentheses.Wald tests for joint significance of age variables.

Residential invest.	Both effects		
		LS	*TSLS*
Age 15 to 29	**0.153**	**0.153**	**0.139**
	(2.95)	(3.01)	(2.36)
Age 30 to 49	**0.298**	**0.208**	0.105
	(3.10)	(2.01)	(0.90)
Age 50 to 64	**0.198**	0.097	0.006
	(2.19)	(1.00)	(0.05)
Age 65 to 74	0.028	0.013	0.004
	(0.23)	(0.11)	(0.03)
Age 75+	**-0.808**	**-0.789**	**-0.728**
	(4.48)	(4.47)	(4.14)
Rel price of inv		-0.038	-0.054
		(3.82)	(4.47)
Adj R^2	0.15	0.21	0.17
$\chi^2(5)$ age var:	44.7	40.5	34.6
sign level	0.00	0.00	0.00
Sargan	—	—	0.14

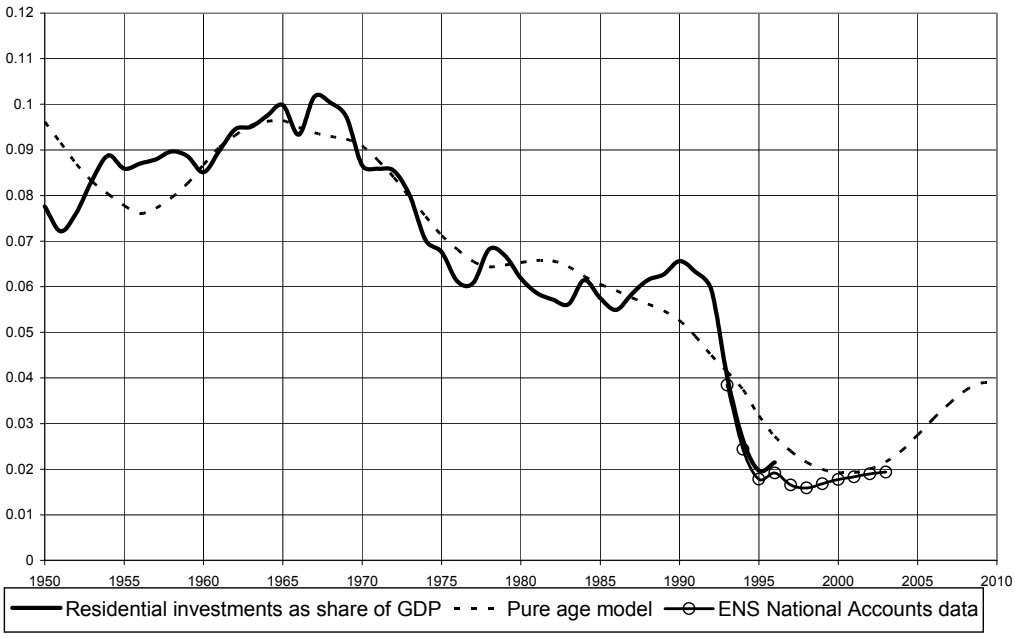

Figure 1: The share of residential investment in Swedish GDP 1950-1996. The dashed line shows predictions from the pure age model in Table 1. Residential investment is from Byggentreprenörerna and GDP from Statistics Sweden. The share 1993-2003 according to ENS National Accounts is shown for comparison and to get an out-of-sample validation.

Table 4: Time series regressions on Swedish data. Standard errors are corrected for heteroskedasticity and autocorrelation up to 3 lags. Absolute t-values in parentheses. Q is the Ljung-Box Q-test for no serial correlation up to 10 lags. Sargan tests the overidentifying restrictions.

Dep. var.	House price index Q				ΔQ	
	LS		*TSLS*			
	1953-78	*1979-99*	1953-78	*1979-99*	1953-78	*1979-99*
Age 0-14	**0.223**	*+0.214*	-0.195	**+0.582**	**-0.221**	**-0.259**
	(3.98)	(1.08)	(1.03)	(2.86)	(4.59)	(3.24)
Age 15-29	**0.141**	**+4.223**	**0.350**	**+3.505**	-0.037	**-2.670**
	(4.35)	(7.01)	(2.82)	(7.33)	(1.06)	(5.14)
Age 30-49	**-0.096**	**-7.300**	0.178	**-5.743**	**0.117**	**+2.896**
	(3.67)	(9.67)	(1.68)	(12.0)	(4.57)	(3.37)
Age 50-64	**-0.216**	**+0.343**	**-0.608**	**+0.604**	0.070	*+0.008*
	(6.13)	(3.17)	(2.91)	(2.78)	(1.28)	(0.09)
Age 65-74	-0.658	**+4.547**	1.754	*+0.239*	0.539	*-0.246*
	(1.44)	(4.05)	(1.14)	(0.15)	(1.63)	(0.34)
Age 75+	**1.827**	**+7.672**	-1.473	**+8.578**	**-1.257**	**-2.194**
	(3.35)	(7.19)	(0.78)	(4.57)	(3.03)	(2.03)
Q					**0.613**	
					(5.98)	
ΔQ			**0.411**			
			(2.27)			
Real interest			**-0.131**			
			(3.11)			
Adj R^2	0.88		0.87		0.65	
D-W	1.75		2.04		1.87	
Q (p-values)	0.00		0.65		0.01	
Sargan	—		0.15		0.46	

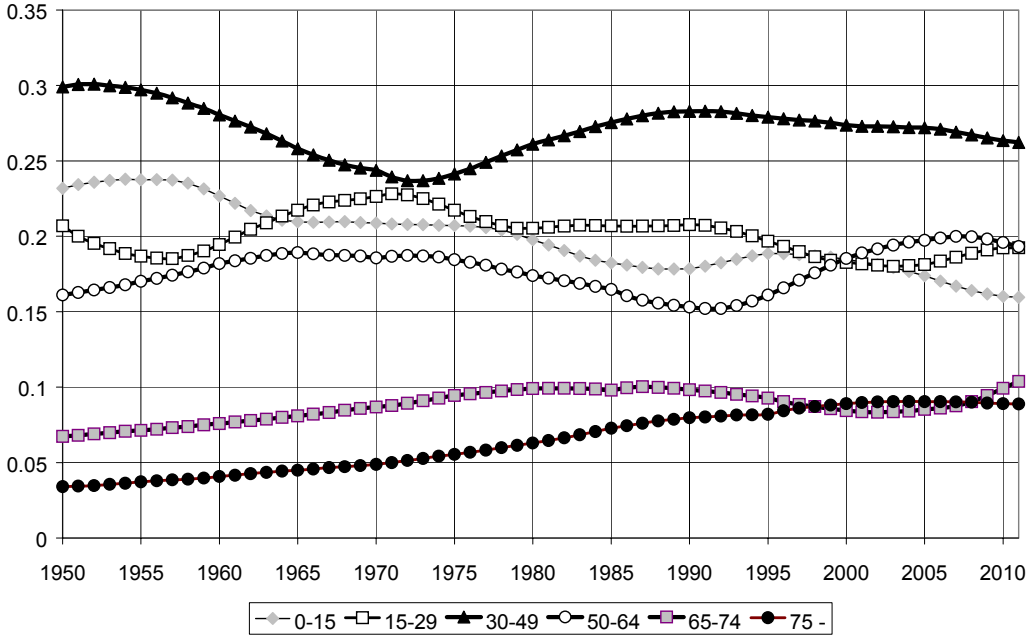

Figure 2: Evolution of population shares of age groups in Sweden. The last 10 years are projections. Source Statistics Sweden.

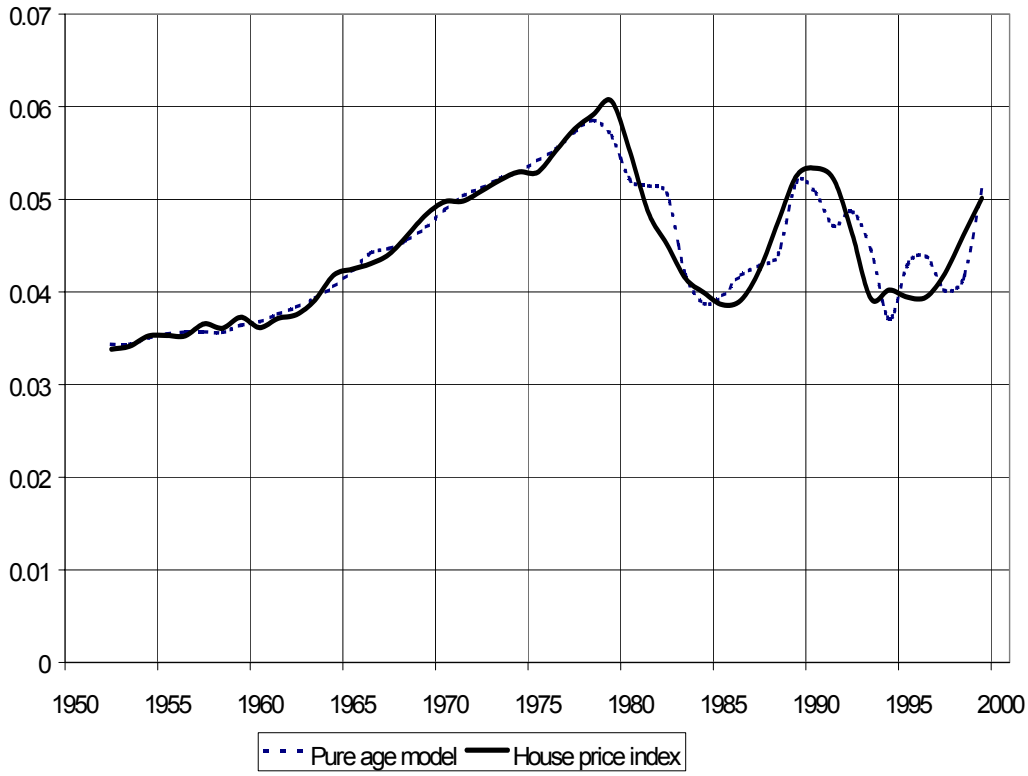

Figure 3: House price index in Sweden 1952-1999 compared to the pure age model of house prices.

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