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Do Households Benefit from Financial Deregulation and Innovation? The Case of the Mortgage Market

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Abstract:

The U.S. mortgage market has experienced phenomenal change over the last 35 years. Most observers believe that the deregulation of the banking industry and financial markets generally has played an important part in this transformation. One issue that has received particular attention is the role that the housing Government Sponsored Enterprises (GSEs), Fannie Mae and Freddie Mac, have played in the development of a secondary market in mortgages. This paper develops and implements a technique for assessing the impact of changes in the mortgage market on individuals and households.

Our analysis is based on an implication of the permanent income hypothesis: that the higher a household's future income, the more it desires to spend and consume, *ceteris paribus*. If we have perfect credit markets, then desired consumption matches actual consumption and current spending on housing should forecast future income. Since credit market imperfections mute this effect, we can view the strength of the relationship between housing spending and future income as a measure of the "imperfectness" of mortgage markets. Thus, a natural way to determine whether mortgage market developments have actually helped households by decreasing market imperfections is to see whether this link has strengthened over time.

We implement this framework using panel data going back to 1969. We find that over the past several decades, housing markets have become less imperfect in the sense that households are now more able to buy homes whose values are consistent with their long-term income prospects. However, we find no evidence that the GSEs' activities have contributed to this phenomenon. This is true whether we look at all homebuyers, or at subsamples of the population whom we might expect to benefit particularly from GSE activity, such as low-income households and first-time homebuyers.

JEL Classifications: D14, G21, R21

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1. Introduction

The U.S. mortgage market has experienced phenomenal change over the last 35 years. Gone are the days when most households got a cookie-cutter, 30-year, fixed-rate, level-payment mortgage from a savings and loan that took deposits at 3 percent and lent out at 6 percent. And gone are the days when the typical lender held that mortgage on its books until the maturity of the loan. Today, consumers choose from an extensive menu of mortgages offering flexibility along almost every dimension. Furthermore, most lenders hold the mortgage for a very short time; typically, they sell the mortgage on the secondary market and, more often than not, financial intermediaries then pool that mortgage with many other mortgages and sell the cash flow in a complex security called a collateralized mortgage obligation.

As noted below, many researchers have argued that this transformation has enhanced efficiency by integrating the mortgage market with the broader financial markets. But there has been little research investigating how or even whether the transformation of mortgage markets has benefited the average homeowner. This question is obviously pertinent to both government agencies and consumer advocacy groups that have significant concerns about housing finance at the family level. However, it may also be of interest to a much wider group of researchers and policymakers who are concerned with monetary and fiscal policy. For example, some macroeconomists argue that the well-documented decrease in business-cycle volatility can be partially attributed to the increasing ease with which households can obtain mortgage financing as well as access their existing home equity. Stock and Watson (2003) state, "One explanation for the decreased volatility in residential, but not nonresidential, construction is the increased ability of individuals to obtain nonthrift mortgage financing…"

In this paper, we use a novel technique to assess the impact of changes in the mortgage market on individual households. Our analysis starts with an implication of the permanent income hypothesis: that the higher a household's expected future income, the more it desires to spend and consume, *ceteris paribus*. If perfect credit markets exist, desired consumption matches actual consumption and current spending should forecast future income. Since credit market imperfections mute this effect, we view the strength of the relationship between house spending and future income as a measure of the "imperfectness" of mortgage markets.

We apply this methodology using household-level data from the Panel Study of Income Dynamics (PSID) and consider the forecasting ability of a newly purchased home on future income. We find that households buying bigger houses have higher future incomes, all else being equal. Furthermore, we find that the forecasting relationship of housing purchases and future income has changed over time. The least-squares estimate of the sensitivity of future income to current housing expenditures more than doubled over the length of our sample, from 1969 to 1999. Further, the change was not smooth over time—the application of econometric techniques for locating unknown structural breaks suggests that the relationship changed discretely in the mid 1980s. Following the logic of the previous paragraph, we view the increased sensitivity of future income to house spending as evidence that mortgage markets have become less imperfect over time.

With this finding in hand, we set out to accomplish two goals. First, as success has many fathers, we attempt to establish paternity of the improved system of housing finance. Was it deregulation? Was it the creation of a secondary market? Or was it the activities of the Government Sponsored Enterprises (GSEs), Fannie Mae and Freddie Mac, whose mission is to help families realize the "American dream" of owning a home? Assessment of paternity presents particular difficulties for us, as many of the changes in mortgage markets occurred simultaneously, during the period from 1977 to 1983. To skirt these problems, we focus on the period after 1983 when mortgage market deregulation had mostly run its course and secondary markets were in place, but during which activities by the GSEs fluctuated considerably. We find that the activities of the GSEs had little or no impact on the imperfectness of mortgage markets.

Our second task is to assess the distributional consequences of the new mortgage market. Advocates for the GSEs, in particular, argue that the transformation of the mortgage market has improved opportunities for the less-well-off and for first-time homebuyers, claiming that the more efficient allocation of risk has enabled lenders to lend to more-marginal borrowers. We find minimal evidence of this. In general, the situation for more-marginal borrowers appears to have changed no more than for less-marginal borrowers, and for first-time homebuyers in particular, it appears to have changed much less.

In Section 2, we provide a brief review of the transformation of the mortgage market from 1970 to 1999, the period that we study. Section 3 presents a simple model that illustrates the possible effects of changes in borrowing constraints on the relationship between spending on housing and future income. Section 4 then describes our empirical specification and the data. In Section 5 we present our results, including an assessment of the sensitivity of our findings to alternative specifications of the model. Section 6 investigates the role of the Government Sponsored Enterprises and is followed by a brief conclusion.

2. The Mortgage Market 1970–1999

This section briefly reviews developments in the U.S. mortgage market from 1970 to 1999. We look first at the deregulation and securitization of the mortgage market, next at the development of new mortgage designs, and then at other developments, including the emergence of the subprime market and the introduction of anti-discriminatory legislation. We conclude the section by reviewing earlier research on innovations in mortgage markets.

2.1 Deregulation and securitization

From the Depression through the late 1970s, deposits in savings accounts provided almost all financing for home loans. Depression-era regulations, updated at various points, channeled low-cost deposits to the thrift industry (Mason 2004). Regulations took many forms and included usury ceilings, interstate banking prohibitions, limits on branching, and perhaps most infamously, Regulation Q, which capped deposit rates and forbade banks from paying interest on checking deposits (England 1992, Gilbert 1986). While these regulations provided some stability, they also, predictably, led to major inefficiencies. Most significantly, by making bank deposits the principal source of funds for mortgages, regulators forced lenders to finance long-term assets with short-term liabilities, a situation referred to in the industry as the mismatch problem (Modigliani and Lessard 1975).

Despite its flaws, the system basically worked until the mid 1960s. Low inflation and stable interest rates meant that the usury ceilings, interest-rate caps, and the mismatch problem did not generate major difficulties. However, starting in the mid 1960s, inflation and interest rates rose, driving up the cost of funds for the savings and loan industry. This forced Congress to act, beginning a process that would culminate, 20 years later, in the transformation of the U.S. mortgage market from a largely deposit-financed system to a largely capital-markets-financed system.

In 1968, Congress moved aggressively to develop a secondary market for mortgages, that is, a market in which banks could sell mortgages they had originated to other investors. It took an old government agency charged with creating a secondary market, the Federal National Mortgage Association (FNMA, founded in 1938 and now known as Fannie Mae) and divided it into two separate entities. The first was a government agency called GNMA (Government National Mortgage Association, later known as Ginnie Mae), which bought mortgages guaranteed by the Federal Housing Authority and Veterans Administration, and the second was a shareholder-owned but government-sponsored enterprise still called FNMA, which bought other mortgages. The Federal Home Loan Bank Board (FHLBB) created the Federal Home Loan Mortgage Corporation (later known as Freddie Mac) in 1970, with a mandate to buy loans from members of the Federal Home Loan Bank system. This period also saw the emergence of the mortgage-backed security, a bond whose cash flows are backed by homeowners' mortgage payments. Ginnie Mae issued its first security of this kind in 1970, and Fannie Mae and Freddie Mac followed shortly thereafter (Bartlett 1989). Within a couple of decades, this innovation would transform the industry.

Despite these moves by Congress, the problems continued to mount. Boston Fed President Frank Morris described efforts to stabilize mortgage finance over the 1966–1975 period as a "decade of failure" (Morris 1975). Secondary markets were slow to develop; deposits into banks and savings and loans remained the chief source of funds for home mortgage lending. Secondary markets seem an obvious solution to the mismatch problem now, but they did not appear that way to contemporary observers, even to brilliant economists. Franco Modigliani assumed that the solution to the mismatch problem lay in mortgage design, and not one participant at a conference he organized in 1975 to explore the issue made any mention of secondary markets in the accompanying conference volume (Modigliani and Lessard 1975). The first privately issued mortgage-backed security appeared in 1977 and was generally considered a failure (Ranieri 1994).

Continued instability and high interest rates in the late 1970s initiated the final phase of the reinvention of housing finance in America. In 1977, Merrill Lynch invented the Cash Management Account, in effect allowing non-banks to circumvent Regulation Q (Cocheo 2003). This innovation, combined with many others, severely reduced the availability of funds for the thrift industry, which was still bound by Regulation Q. Even when regulators finally allowed them to pay competitive interest rates, thrifts confronted state usury laws. These laws often meant that the thrifts could not lend profitably, and so they simply stopped lending altogether (Shaman 1979).

The impending collapse of the thrift industry spurred Congress and regulators to action, and over the next six years, legal and regulatory changes transformed mortgage lending (Bartlett 1989). The Depository Institutions Deregulation and Monetary Control Act of 1980 ordered the phase-out of Regulation Q over the next six years and overrode or pre-empted state usury ceilings. In 1982, Congress passed the Garn-St. Germain Depository Institutions Act, which extended the 1980 act, pre-empting state laws that constrained the types of mortgage products originators could offer. In 1984, the Secondary Mortgage Market Enhancement Act solved many of the technical problems facing mortgage-backed securities. Finally, the Tax Reform Act of 1986 created an investment vehicle called a REMIC (Real Estate Mortgage Investment Conduit) that allowed securitizers to divide the flows of principal and interest from mortgage-backed securities into different classes ("tranche securities") tailored to the needs of different investors.

Two key events dramatically accelerated the development of a secondary market. The first occurred in October 1981, when the FHLBB, the main regulator of thrifts, introduced a change in accounting rules that had the effect of allowing lenders to sell mortgages on the secondary market without booking a large accounting loss (Mason 2004). This change created a liquid secondary market for mortgages virtually overnight (Lewis 1989). Secondary market sales of mortgages increased more than four-fold, from $12 billion in 1981 to $52 billion in 1982 (Bartlett 1989).

The second influential event was the realization that issuers could skirt many of the problems that had bedeviled early mortgage-backed securities, by enlisting one of the Government Sponsored Enterprises. The GSEs' federal charters meant, for example, that their securities were exempt from state investor protection laws (Ranieri 1994). In addition, investors believed, perhaps erroneously, that Fannie Mae and Freddie Mac securities were backed by the full faith and credit of the federal government. Freddie Mac, initially, and later Fannie Mae, worked closely with Wall Street firms and became the largest issuers of mortgage-backed securities.

Figures 1 and 2 display the evolution of GSE securitization activity from 1970 to 2004. The figures show two different measures of GSE securitization activity: real mortgage debt securitized by Fannie and Freddie in dollars and the ratio of GSE-securitized mortgages relative to all outstanding home mortgages. Figure 1 displays measures of the stocks of these variables, while Figure 2 displays the corresponding flows. Both the levels and the ratios have grown substantially over the time of our sample. The percentage of the stock of home mortgages securitized by the GSEs increased dramatically from approximately 0 in 1975 to over 40 percent by 2002.

2.2 Innovation in mortgage design

The menu of available mortgage choices in 1999 vastly exceeded the options that were available in the 1970s. In the 1970s, because of a combination of regulation and inertia, the mortgages available to borrowers consisted almost exclusively of fixed-rate, level-payment instruments. Among other things, consumer groups, as they do to this day, viewed features like variable interest rates as dangerous, and they worked assiduously to prevent adoption (Guttentag 1984). Even when they allowed variable-rate mortgages, regulators established restrictions that severely limited their usefulness. For example, the FHLBB allowed variable-rate mortgages, but the rate could not change by more than 50 basis points every six months, nor could it rise by more than 2.5 percentage points over the life of the loan (Macauley 1980).

However, the 30-year, fixed-rate mortgage was particularly unsuitable for the high-inflation, high-volatility environment of the 1970s. The combination of high nominal interest rates and fixed payments over time meant that households faced very high real payments early in the life of the loan. Some borrowers would have been better served by mortgage designs that allowed relatively lower nominal payments early in the life of the loan, so that the real burden would be distributed more evenly over time. Regulators eventually relented in their opposition to alternative mortgage designs and allowed lenders to offer innovations, including the Graduated-Payment Mortgage (GPM, first offered in 1977) and the forerunner of today's Option ARM (first offered in 1980), both of which allow borrowers to make a monthly payment that falls short of the interest due on the mortgage (Phalan 1977, Harrigan 1981).

Initially, new mortgage designs merely inoculated borrowers against high inflation. With high inflation, a graduated-payment mortgage offered a flow of real payments comparable to that of a traditional mortgage with low inflation. However, although the appearance of high inflation enlarged the mortgage menu, its disappearance in the mid 1980s did not shrink the menu. Regulators made no effort to prohibit the alternative mortgages developed in the late 1970s and early 1980s. The use of these new products in a low-inflationary environment allowed lenders to offer borrowers much less rigorous repayment schedules than had prevailed even in the 1950s and 1960s. In other words, the net effect of regulators' responses to high inflation was to liberalize mortgage markets considerably when compared with the traditional system of the 1950s and 1960s.

2.3 Other changes

Three other changes in mortgage markets in this period are worth noting. First, concerns emerged in the early 1970s of race and gender-based discrimination in mortgage markets, leading to the passage of the Equal Credit Opportunity Act (ECOA). The original ECOA, passed in 1974, prohibited discrimination on the basis of sex and marital status. In 1976, Congress substantially expanded the law, adding age, race, color, religion, and national origin to the list of factors on which lenders could not discriminate (Elliehausen and Durkin 1989). Subsequent research on the impact of ECOA has yielded mixed conclusions with respect to its effects (see, for example, Ladd 1982 and Munnell et al. 1996).

Second, in the mid 1990s, lenders adopted automated underwriting procedures, which reduced the cost of originating new mortgages (Straka 2000). This change was driven, at least partly, by two things. The successful experience of credit-card issuers with numerical credit scores allowed lenders to substitute these scores for loan officer judgment in the analysis of the creditworthiness of the borrower. Also, the anonymity of automated underwriting procedures allowed lenders to refute claims of racial discrimination more easily.

Third, the subprime market, the part of the mortgage business dedicated to borrowers with less-than-perfect credit histories, emerged in the mid 1990s. Until the mid 1990s, a borrower was either prime and got a loan at the going rate, or was subprime and did not get a loan at all (Munnell et al. 1996). In the mid 1990s, a new generation of lenders began to offer loans to subprime borrowers, but they demanded much higher interest rates as compensation for the added risk. Subprime originations grew from $65 billion in 1995 to $332 billion in 2003 (Chomsisengphet and Pennington-Cross 2006).

2.4. Prior research on innovations in the mortgage market

What was the impact of all these institutional changes in the mortgage market? Empirical researchers have approached this question in two ways. The first examines the extent to which mortgage markets and capital markets have become integrated over time. The second focuses on the role that new activities of the GSEs and various institutional changes have played in these developments. We now discuss them in turn.

One way to investigate the extent to which mortgage markets and capital markets are integrated is to look at the time-series relationship between interest rates on mortgages and Treasury yields. The idea is that if capital can flow freely in and out of the mortgage market, then Treasury yields and mortgage-market yields should move together over time. A variety of studies using this general approach have found that, in fact, the correlation between Treasury yields and mortgage yields was greater in the 1980s than in the 1970s. (See, for example, Devaney and Pickerill 1990, Hendershott and Van Order 1989, Goebal and Ma 1993, and Devaney, Pickerill, and Krause 1992).[1]

An alternative approach is to examine mortgage markets across regions. The idea here is that in a well-functioning mortgage market, regional conditions should reflect credit availability in the national capital market rather than in a particular region. Of course, other characteristics that might affect mortgage rates and that vary systematically across regions must also be taken into account. Rudolph and Griffin (1997) found that the coefficient of variation of mortgage rates across Metropolitan Statistical Areas decreased from 1963 to 1993, a finding that is consistent with more integration over time.[2] In the same spirit, in a well-functioning mortgage market, the terms of a family's mortgage should be independent of the particular institution that originates the mortgage. Loutskina and Strahan (2006) show that, at least in certain segments of the mortgage market, this has in fact become the case.[3]

In short, then, both strains of the empirical literature point in the same direction—over time, the mortgage market has become more integrated with national capital markets, and from this perspective, the mortgage market has become a better-functioning market.

While the improvement in the operation of housing finance markets has been well-documented, the contribution of the GSEs' securitization activities has received little attention.[4] Interestingly, several papers run counter to the widely held view that securitization played a major role. As we mentioned above, some securitization occurred in the 1970s**,** but Rudolph, Zumpano, and Karson (1982) found that regional variation in contract rates did not decrease over this period and concluded that "the secondary market has not significantly reduced differences in local markets." In this context, a paper by Goebel and Ma (1993) is of more

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[1] Statistical models based upon the Arbitrage Pricing Theory from finance suggest that integration of the mortgage market with traditional capital markets increased during the 1980s. See Bubnys, Khaksari, and Tarimcilar (1993).

[2] Rudolph, Zumpano, and Karson (1982) examine how a variety of attributes of mortgage contracts (contract rate, loan initiation fees, maturity, and loan-to-value ratio) varied by Standard Metropolitan Statistical Area between 1968 and 1978. They find that while the variability of fees and charges and loan maturity declined over this period, the contract rate and loan-to-value ratio did not change. This period predates both the big increase in GSE securitization activity and other important changes in housing finance institutions.

[3] They highlight the role of securitization, arguing that credit supply evolves independently of conditions at particular banks in the highly securitized "conforming" loan market (the part of the market for which the GSEs are permitted to buy up mortgages).

[4] In contrast, there has been a great deal of attention focused on the impact of the GSEs on the level of mortgage rates. See, for example, Ambrose and Thibodeau (2004) and Passmore, Sparks, and Ingpen (2002).

relevance, because their sample period encompasses the 1980s, which, as noted above, witnessed the huge expansion of Fannie's and Freddie's securitization activities. Goebel and Ma's vector autoregression analysis of the relationship between Treasury and mortgage-market yields suggests that "the two markets were already integrated before the full development of the secondary mortgage markets between 1984 and 1987."

An important aspect of all the studies discussed above is the centrality of the relationship between Treasury rates and mortgage rates. While important and interesting, this tack removes the focus from where we believe it really belongs, which is the impact of securitization on *households.* What we care about ultimately is not the correlation between Treasury rates and mortgage rates *per se*. Rather, the key question is whether securitization (or any other development in the housing finance market) enhanced the likelihood that households could borrow enough to buy a home that maximized their utility, given their lifetime income.[5] We next develop a model that allows us to investigate the impact of mortgage-market innovations at the household level.

3. Model

This section develops a model of how changes in the market for housing finance affect individual households. A key result is that relaxing constraints on mortgage lending increases the cross-sectional sensitivity of future income to housing expenditures.[6] We first describe a simple example that illustrates the basic intuition behind this claim.

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[5] Linneman and Wachter (1989) is the only study to our knowledge that attempts to assess the impacts of mortgage-market developments at the household level. They find a diminished impact of borrowing constraints on tenure choice over time, and they attribute their findings to the development of adjustable rate mortgages (ARMs) and the increased use of seller financing and other "non-traditional" financing schemes. Campbell and Cocco (2002) examine the effects of different mortgage designs in a dynamic life-cycle model with borrowing constraints.

[6] See Artle and Varaiya (1978) for the first theoretical analysis of the implications of borrowing constraints on homeownership, Brueckner (1986) for a 2-period version of the same model in discrete time, and Engelhardt (1996) for an empirical implementation. For a discussion of the user cost of housing in a model without borrowing constraints, see Himmelberg, Mayer, and Sinai (2005).

Suppose we have two types of families who differ in future income, low (Y_L^f) or high (Y_H^f), and who face a choice between two types of house, small (H_S) or big (H_B).[7] Assume further that households have different levels of wealth and that they can borrow to finance their home purchase. Otherwise, the families are identical; in particular, they have the same level of *current* income. For purposes of this example, we assume that, if allowed to borrow an unlimited amount, the low-income-growth families would choose small houses, while the high-income-growth families would choose big houses. However, in the presence of credit constraints, this separation of types might not emerge. For example, lenders typically require that monthly housing expenditures fall below a certain percentage of current income. Thus, current income fixes some maximal amount the household can borrow. This need not limit the size of the house the family can buy if the family has access to sufficient other assets such as its own wealth or the wealth of its close relatives. If not, even a high-income-growth household ends up buying a small house.

In this example, credit constraints reduce the difference in average observed income-growth rates between small- and big-house buyers**.** To see why, recall that under our assumptions, in the absence of credit-market constraints, every high-income-growth family purchases a big house and every low-income-growth family purchases a small house. In the presence of constraints, some high-income-growth families instead purchase small houses, and thereby drive up the average income growth associated with small-house buyers.[8] On the other hand, the income growth of large-house buyers stays the same. Hence, the borrowing constraints attenuate the observed relationship between income growth and the size of current home purchases.

This argument is laid out more formally in Figure 3. Suppose initially that 2/3 of the high-income-growth families buy a small house (all of the low-income-growth families also buy

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[7] This discussion implicitly assumes that housing is entirely a consumption decision. In fact, housing is also purchased for investment purposes. However, under the conditions we assume, the basic results are independent of whether the motive is consumption or investment.

[8] Holding current income and wealth constant, households with higher expected future income (y_1^H) are more likely to be constrained than those with lower expected future income (y_1^L). This is because high-income-growth households would like to borrow more than low-income-growth households in order to smooth consumption and consume more today, but their borrowing is limited by current income.

a small house), so that the average future income of the small-house buyers is $Y(H_S) = \frac{2}{5}Y_H^f + \frac{3}{5}Y_L^f$, whereas only high-income-growth families buy a big house, meaning that $Y(H_B) = Y_H^f$. Now, suppose we relax the constraint so that only 1/3 of the high-income-growth families buy a small house. In this case, the average future income of small-house buyers falls to $Y(H_S) = \frac{1}{4}Y_H^f + \frac{3}{4}Y_L^f$, whereas the future income of big-house buyers stays the same. Figure 3 illustrates how the movement of high-income-growth families from small houses to big houses as the credit market constraint is relaxed raises the sensitivity of average future income to house size—that is, line AC is flatter than line BC**.** In a regression context, this means that the less constrained the borrowing environment, the greater the responsiveness of future income to the size of a home purchase, *ceteris paribus.* As shown below, this is precisely what we find in the data. We present a formal, algebraic version of this argument in Appendix B.[9]

4. Empirical Setup

The previous section provided some intuition along with a simple model to explain why imperfections in housing finance such as borrowing constraints could weaken the observed relationship between housing purchases and future income and, conversely, how relaxing credit constraints could strengthen this relationship. Thus, by looking at how the relationship between current home purchases and future income has changed over time, we can make inferences about whether the market for housing finance has become less imperfect. This section develops an econometric model for implementing this idea. We then discuss the data used to estimate the model. Finally, we provide a simple graphical summary of the evolution of the relationship between house values (normalized by current income) and income growth in our data.

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[9] Our simple model assumes that households with a given level of income growth face the same borrowing constraints; they differ only by wealth. However, the analysis extends to any variable that affects a family's ability to buy a house, not just wealth. In particular, one could show that when families have the same wealth but differ in the borrowing limits they face, then if individuals' borrowing limits are all relaxed, the relationship between current house value and expected future income strengthens. This case is particularly important given the evidence in Munnell et al. (1996).

4.1 Econometric specification

At least since Muth (1960), researchers studying housing demand have understood that, at least in principle, permanent income should play a role in the demand for housing. A typical empirical model assumes that when household *i* purchases a home in year *t,* the real value of the house at the time of purchase, H_{it}, can be written

$$H_{it} = \beta_0 + \beta_1 RS_{it} + \beta_2 X_{it} + u_{it}, \qquad (4.1)$$

where RS_{it} is some measure of lifetime resources including permanent income, X_{it} is a vector of socio-demographic variables, the β's are parameters to be estimated, and u_{it} is a random error term.[10] Because future income is not known with certainty, the household forms the conditional expectation of lifetime resources using information available to it at time *t*. By definition, expected lifetime resources for family *i* in year *t*, are:

$$\mathrm{E}_t[RS_{it}] = w_{it} + y_{it} + \sum_{s=1} (1/R)^s \, \mathrm{E}_t[y_{i,t+s}],$$

where y_{it} is real income at the time of purchase, $y_{i,t+s}$ is real income *s* periods forward, and w_{it} is current real wealth. Given this definition, we re-write the housing demand equation as

$$H_{it} = \beta_0 + \beta_1 y_{it} + \beta_2 \mathrm{E}_t[y_{i,t+f}] + \beta_3 w_{it} + \delta X_{it} + \varepsilon_{it}\,. \qquad (4.2)$$

Rather than including the expected sum of the entire stream of future earnings, for simplicity we include only the expectation of income at one point in time in the future. In practice, we use values of future income two and four years out; that is, we estimate our empirical equation both for *f* equal to 2 and 4. In addition, we also use the 5-year average of realized future income. Because we do not observe the household's forecast of future earnings, but rather the realized values, forecast error is introduced by substituting the realized values for the forecasted values. Were future income to remain on the right-hand side of the equation, this would introduce an errors-in-variables problem and lead to inconsistent estimates. To avoid this problem, we invert the demand equation and move realized future income to the left-hand

[10] See, for example, Polinsky and Ellwood (1979).

side and housing demand to the right-hand side. At the same time, we assume a log-linear specification:[11]

$$\ln(y_{i,t+f}) = \gamma_0 + \gamma_1 \ln(H_{it}) + \gamma_2 \ln(y_{it}) + \gamma_3 \ln(w_{it}) + \gamma_4 X_{it} + \xi_{i,t+f}, \qquad (4.3)$$

where $\xi_{i,t+f}$ is household i's forecast error of future income at time t. Adding this forecast error to the residual, least-squares estimation of this equation allows for a consistent estimate of the parameter of main interest, the elasticity of future income with respect to housing, γ_1. If we were to confront equation (4.3) with data, we would view a rejection of the hypothesis that $\gamma_1 = 0$ as evidence in favor of the joint hypothesis of forward-looking behavior and the ability of households to make reasonably accurate income forecasts.

We can also interpret equation (4.3) simply as a forecasting model. That is, the equation represents a fairly standard, cross-sectional analysis of the determinants of future levels of income, in which we include the usual demographic variables such as age, education, race, sex, and past values of income.[12] The only real difference from typical forecasting models is that the model here is augmented with housing purchase expenditures as a right-hand-side variable. According to the permanent income model, consumption and expenditure should reflect people's information about future income. By this logic, if people have better information about their future income than is contained in income's own history, then consumption and expenditure variables should help to forecast income. Housing expenditures are a natural choice in this context, because for most households the purchase of a home is the largest purchase that it will make over its lifetime.

As noted below, an important practical problem in estimating equation (4.3) is that we have only fragmentary data on w_{it}, household wealth. The omission of wealth could bias our estimate of the parameter of main interest, γ_1. To assess the direction of this bias, we begin by noting that the expected sign of the coefficient on wealth in equation (4.3) is negative. To see

[11] This specification often appears in the literature. See Mayo (1981) for a review of the early housing demand literature and a discussion of functional form. We also used a linear specification, and there were no substantive differences in our results.

[12] In other specifications we also inserted lagged values of income along with current income as explanatory variables. This did not affect the estimated coefficient on the value of the house, but reduced the estimated coefficient on current income. Because including lagged income values reduced our sample size and because our main concern is the estimated coefficient on house value, we include only current income.

why, consider two individuals who have identical current incomes and buy identical homes. Now, suppose that one has higher wealth than the other. Given that the high-wealth person bought exactly the same home and that the permanent income elasticity of demand for housing is positive, it follows that his expected future income must be lower than that of the low-wealth person. In short, wealth has a negative sign in the forecasting equation. At the same time, we expect a positive correlation between wealth and the size of the house purchase. Putting all of this together, standard omitted variable bias considerations suggest that leaving wealth out of the equation will tend to bias downward the coefficient on the house purchase, γ_1. That is, our estimate of γ_1 underestimates the ability of current house purchases to predict future income.[13]

We use household-level data to estimate the following regression model, which is simply equation (4.3) excluding wealth and including a set of time effects D_t,[14]

$$\ln Y_{it+f} = \alpha_0 + \alpha_1 \ln Y_{it} + \alpha_2 \ln H_{it} + \beta X_{it} + D_t + \xi_{i,t+f}, \qquad (4.4)$$

where the α's and β's are parameters, and $\xi_{i,t+f}$ is a random error. Our primary focus is on the coefficient on housing expenditures. If households are forward-looking and can predict their future incomes with some accuracy when making housing choices, we expect α_2 to be positive.[15] Furthermore, a higher sensitivity of housing expenditures with respect to future income implies a greater value for α_2. As discussed in the previous section, this is critical for our purposes, because it suggests that the impact of changes in the housing finance system on households can be assessed by the estimated elasticity of future income with respect to housing expenditures. This observation allows us to examine from the *household's* standpoint the widespread belief that the housing market has become less imperfect over time.

Specifically, in the context of our model, a less imperfect housing finance system suggests an increased observed elasticity of future income with respect to housing

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[13] The same argument suggests that γ_1 is biased downward by the omission of any variable that measures a family's access to funds that would allow it to overcome borrowing constraints. An example is parental wealth, which could measure the ability of parents to contribute to the children's home purchase.

[14] We include time effects to control for aggregate influences such as business-cycle effects.

[15] As suggested earlier, this argument hinges on whether or not households can predict their future income streams with some accuracy. For example, if households are forward-looking in their housing decisions but are very bad at predicting their future income, we will not find a statistically significant relationship between the value of a newly purchased house and future income. In effect, then, we are testing the joint hypotheses that households are forward-looking and that they can predict their future incomes with some accuracy.

expenditures. Hence, if housing market innovations over time have benefited households, we expect the coefficient on housing expenditures in equation (4.4) to increase over time, *ceteris paribus*. Algebraically, this translates into the value of α_2 growing in magnitude over time. To test this notion, we begin by augmenting equation (4.4) with an interaction term between a linear time-trend and house value:

$$\ln Y_{it+f} = \alpha_0 + \alpha_1 \ln Y_{it} + \alpha_2 \ln H_{it} + \alpha_3 {*} t \ln H_{it} + \beta X_{it} + D_t + e_{it}. \qquad (4.5)$$

If the relationship between housing expenditures and future income is in fact becoming stronger over time, then α_3 should be positive.[16]

The housing demand formulation, equation (4.2), reminds us that H_{it} might be correlated with the error term in our estimation equation (4.5). No compelling instruments are available in our data, but we do not view this as a serious problem for two reasons. First, one likely effect of the correlation is to bias α_2 downward, making it more difficult to find evidence of forward-looking behavior. The argument is similar to the one we made above with respect to the consequences of omitting wealth from the equation. Specifically, imagine that the source of the correlation is unobserved taste differences for housing.[17] Now, we know that in equation (4.5), any taste variable that increases the demand for housing would have a negative coefficient if it were included in the equation. This is so because if X and Y have the same current income and buy the same house, but Y has a stronger taste for housing, then Y's future income must be lower, *ceteris paribus*. Further, H_{it} is likely to be positively correlated with the missing taste variable. Putting this together, standard omitted variable bias arguments suggest that α_2 will be biased downward, putting an extra burden on finding the forward-looking behavior hypothesized by our model.[18]

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[16] For purposes of clarity, we interact house value with a time trend rather than interacting each time effect with house value. A linear time trend is a simple way to summarize whether the magnitude of the house value coefficient changes over time. Interacting with a full set of time effects would produce results that would be difficult to interpret.

[17] Alternatively, such a correlation could be generated by regional differences. Perhaps, for example, families in high-growth regions tend to prefer larger houses. Therefore, in some variations on our basic model not reported here, we included state and regional effects but found that they had no substantive effect on our results.

[18] For this reason, one must be cautious in assigning a structural interpretation to α_2 as a housing demand parameter.

The second reason is that much of our interest is in the *change* in the coefficient on H_{it} over time. Even if a correlation between H_{it} and the error generates a bias in α_2, we see no reason to believe that there is a systematic bias in the change in α_2 over time.

4.2 Data

Our primary data come from the Panel Study of Income Dynamics (PSID) for 1968 to 2001. The PSID is an annual panel data survey, which contains, among other things, detailed information on family income and demographic variables. In addition, it includes information on home purchases and their value. Some of the issues involved in using the PSID to estimate our model, including the reliability of the data on homeownership and housing finance, are discussed in Appendix A.

Our basic sample includes all households that purchased a home in the period spanning 1969–1999.[19] This includes both first-time homeowners making the transition from renting to owning a home and existing homeowners who are moving into a new house. The PSID's measure of the purchase price is the value of the home as reported by the household during the first year of occupancy.[20]

For some of our specifications, we distinguish between first-time house purchasers and purchasers who were already homeowners. If the household reported in the previous interview that it was renting, and reports in the current interview that it owns a home, then we characterize it as a home-purchaser. Furthermore, if the household has never reported owning a home in previous interviews, then we label it as a first-time homebuyer. There is a subtle issue

[19] We excluded 1968, the first year of the PSID, because of problems with the question pertaining to whether the household had moved within the past year.

[20] The PSID has two variables that allow us to identify house purchases. The first is a question regarding the tenure choice of the household (that is, renting versus owning a home), and the second identifies households that have changed residence between the current and previous interviews. We used information from the house value question to double-check the accuracy of the tenure choice question. The value of the house is one of the few PSID questions that is imputed and thus does not have any missing observations for homeowners. Since this question is missing for renters, we were able to double-check that households that reported a tenure switch from renting to owning did in fact purchase a house in the time between interviews. In addition, the PSID has a variable that provides information regarding the accuracy of the house value survey question. It is a dummy variable that reveals whether or not the value of the house was edited/imputed and also provides information regarding the extent to which it was edited. When we estimated our models including only those values that were not edited in any way, we found no substantive changes in our results.

regarding households that enter the PSID after the initial 1968 wave. We do not have a complete tenure choice history for these households, and therefore we cannot say for certain whether they are first-time homebuyers. For these cases, we adopt the following rule of thumb: If the head of the household is less than 30 years old, then the house purchase is considered to be part of the first-time homebuyer sample. If the head is 30 or older, then the household is assumed to have previously bought a home at some point in the past, and it is placed in the repeat-homebuyer sample. Our results are not sensitive to this cutoff age: We tried decreasing the cutoff to 25, and while it reduced our first-time homebuyer sample by approximately 200 observations, it did not change any of the results.

Altogether, we identified 15,087 house purchases over this period. In several cases, the reported value of the purchased house was extremely low, even after converting from nominal to real values. In the majority of these cases, the household reported not having taken a mortgage, leading us to believe that these may be inheritances or transactions between family members that are not particularly relevant to our study. Therefore, we eliminate all observations for which the real value of the house is less than $5,000. This reduces our sample of house purchases to 14,808. After deleting observations with missing values for income (current and future), demographic variables, and observations that were part of the Survey of Economic Opportunity (SEO) portion of the PSID,[21] we end up with a baseline sample of 6,782 for our 2-year future income measure, 5,409 for our 4-year future income measure, and 4,392 for our 5-year-average income measure. The percentage of households purchasing a home as a share of the total number of households in our baseline PSID sample fluctuates between 5.6 percent (1983) and 9.6 percent (1978) for the 30-year span of our data, and it displays no noticeable patterns.

Our income measure is total income, including both labor and capital income and transfers, received by both the husband and wife and any other individuals in the household. In order to correct for changes in the price level over time, we deflate the income and house value

[21] The SEO sample is not a nationally representative sample as it over-samples poor and immigrant families; studies based on the PSID typically exclude it.

variables by the Personal Consumption Expenditure (PCE) Chain-Type Index, with 2000 as the base year.

As noted above, our left-hand-side variable is income *f* years in the future. It is hard to know just how far into the future households look and with what degree of accuracy, so the exact value of *f* is bound to be arbitrary. We look at income both two and four years ahead. We choose these two values for *f* mainly because of data considerations. As a consequence of the fact that the PSID switched from an annual to a biennial survey in 1997, it turns out that values for *f* of 2 and 4 maximize our sample size.[22] We also use the 5-year average of future income realizations.[23]

As far as demographic variables,[24] the *X*-vector of equation (4.1), we include: a cubic in age, education (a series of dichotomous variables), race, whether the head of the household is female, and family size.[25] The means and standard deviations of the variables for the different samples are reported in Tables 1 and 2.

4.3 A Preliminary Look at the Data

Our model posits that to the extent that the market for housing finance has become less imperfect over time, the relationship between current house value and income growth should strengthen. To see whether such a relationship is present in the data, we began by computing for each household that purchased a home the ratio of the value of the home to family income. We then computed the mean ratio for each octile of the distribution as well as the mean value of

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[22] As a consequence of the 1997 change, we do not have data for the years 1998 and 2000. Therefore, we must discard house purchase data for the observations for which future income, y_{it+f}, lies in 1998, 2000, or after 2001. For example, if we were looking at income three years into the future, we would need to eliminate house purchases in 1995, 1997, 1999, and 2001; if we were looking five years ahead, we would be forced to throw away 1993, 1995, 1997, 1999, and 2001. Thus, using two and four years, respectively, allows us to maximize the sample size. For the case of f = 2, we need to discard only 1996 and 2001, while for f = 4, we need to discard only 1994, 1996, 1999, and 2001.

[23] At least as far back as Friedman (1957, pp. 23–25), empirical investigators have been forced to deal with the ambiguous nature of the time horizon that is appropriate in the permanent income model. Friedman discarded both mean lifetime income and the short-term mean of the anticipated probability distribution of income as proxies for permanent income, instead advocating a measure based on an intermediate time horizon.

[24] All variables pertain to the head of the household, except the education variables, which correspond to whichever spouse has the highest level of education.

[25] To conserve space, we omit the coefficients on age-squared and age-cubed, both of which are statistically significant at the 1-percent significance level for all of the specifications. The coefficient on age-squared is negative, which is consistent with the hump-shaped income profile commonly estimated in the literature.

income growth (over a 2-year period) for the families in the respective octiles. Figure 4 contains plots of house value relative to current income (in logs) against income growth for both the first half of our sample (pre-1985) and the second half of our sample (post-1985, inclusive).

Two features of the graph are of interest. First, the relationship between house value relative to current income and income growth during both the beginning and end of our sample period is positive. This is consistent with the notion of forward-looking behavior of households. Second, over time income growth has become more sensitive to normalized house value for households with relatively low levels of housing, but has stayed the same for households with relatively high levels of housing. This makes sense in terms of our theory—the families who bought large homes at the beginning of our sample period presumably were less constrained than those who bought small homes. Therefore, the loosening of constraints that took place over time likely affected their behavior less. However, while it is encouraging that this figure is consistent with our theoretical framework, we must be careful not to place too much stock in it because it does not take into account effects from other variables that could be contributing to such a pattern in the data. A multivariable analysis is required, to which we now turn.

5. Results

This section presents the results of our basic model, including a specification that allows for the possibility of structural breaks in the relationship between the value of a current house purchase and future income. We then examine how the parameter estimates differ across various subsets of the population and whether the substantive findings are robust to alternative specifications.

5.1 Basic results

The results for estimating equation (4.5) for 2- and 4-year horizons are shown in column (1) of Tables 3 and 4, respectively, while results for the average of future income over five years are shown in Table 5.

The parameter of main interest is the estimated coefficient on the log of house value. For $f = 2$ (column (1), Table 3), the coefficient is 0.118, while for $f = 4$ (column (1,), Table 4), it is 0.096. In both cases, the coefficients are significantly different from zero at conventional levels. The implication is that every 10-percent increase in the value of a new home is associated with a roughly 1.2-percent increase in income two years from now, and a 1-percent increase in income four years into the future. Furthermore, the corresponding estimate in column (1) of Table 5, which uses a 5-year average of future income as the dependent variable, is positive and significant, with a value of 0.083. Thus, as hypothesized, and consistent with forward-looking behavior, the value of a newly purchased house has power in predicting future income, even conditional on current income.

The coefficient on the interaction of the linear time-trend with house value tells us whether the relationship between house value and future income has become stronger over time. For all of the specifications except the first-time homebuyers, the estimated coefficients are significant, with values ranging between 0.004 and 0.006. An estimate of 0.004 implies that in 1969, a 10-percent increase in the value of the house corresponded to a 1.2-percent increase in income two years later, while in 1999, a 10-percent increase in the value of the house was associated with a 2.5 percent increase in income two years into the future. Thus, the point estimate implies that by the end of our sample period, the relationship between the value of the house and future income more than doubled. Within our conceptual framework, this suggests that for the baseline sample, constraints loosened over time, consistent with the notion that developments in housing finance made it easier for households to purchase homes in line with their future income prospects. While this does not constitute a formal test of our model, it is comforting that the result is the same as that suggested by both casual observation and previous econometric work that followed a very different approach.

We now discuss briefly the other coefficients in our basic model. Most of the demographic variables are statistically significant and possess signs that are consistent with our prior research. The coefficient estimate of 0.39 on current income for the 2-year and 4-year regressions replicates the usual result that income has an autoregressive component. Future income is increasing in age, *ceteris paribus*, consistent with typical analyses of age-income profiles. The coefficient estimates on the education variables imply that future income (two and four years ahead) is approximately 17 percent higher for high school graduates with some college experience than for high school drop-outs, and more than 25 percent higher for college graduates. The race dichotomous variables are not statistically significant for the 2-year regression. However, in the 4-year and 5-year-average regressions, households that are neither Caucasian nor Black (*other race*) have future income realizations that are approximately 11 percent lower than that of white households. Female-headed households have future incomes that are between 27 and 37 percent lower than those of male-headed households, depending on our definition of future income. Size of household is also statistically significant and positive, although the coefficient's magnitude is small. It suggests that for each one-person increase in the size of a household, its income in the future is approximately 1.5 percent higher. The coefficient estimates for the time effects are omitted from the tables to conserve space; however, they are included in all of the regression models. For the most part, they are statistically significant.

5.2 Breakpoint analysis

Our approach so far has been to use the interaction of time and house value to determine whether the observed elasticity of future income with respect to current housing expenditures has increased over time. An alternative approach is to let the data determine whether there was a discrete structural change in the relationship between future income and housing expenditures, and if so, to see whether the timing of the change can be related to changes in the operation of the mortgage market. In terms of our basic model, equation (4.4), the question is whether at some point in time there was a discrete change in α_2, the coefficient on house value.

To implement this idea, we use Bai's (1999) likelihood-ratio-type test for multiple structural changes in regression models. The test determines both the number of structural breaks and the location of each break in the data. The particularly novel aspect of the test is that both the null and alternative hypotheses allow for the possibility of breakpoints.[26] This implies that one can use the methodology to test for multiple breakpoints against the null hypothesis of a single (or multiple) breakpoint(s). For example, one can test for three versus two breakpoints.

[26] The limiting distribution of the test statistics for tests of only a single structural break are derived assuming the absence of breaks. Thus, when the null hypothesis is rejected for such a test, only a single breakpoint is estimated.

The intuition behind the test is fairly simple. Essentially, it consists of calculating the sum of squared residuals (SSR) for each possible partition of the data corresponding to the number of breakpoints under the null hypothesis, n_0, and taking the smallest value. Then, one does the same for each possible partition of the data corresponding to the number of breaks under the alternative hypothesis, n_1, and compares the minimum SSR under the null to the minimum SSR under the alternative. If the SSRs are not "significantly" different from each other, then the null hypothesis is not rejected and it is assumed that the data contain n_0 breaks. If they are different, then the null hypothesis changes to n_1 breaks, the alternative hypothesis to $n_2 = n_1 + 1$, and the procedure repeats itself until the null is not rejected. For a more detailed explanation of the methodology, the reader is directed to Bai's paper.

The findings from the breakpoint analysis for the baseline sample are reported in the first column of Table 6. We find a single breakpoint in 1985 for the models that predict income two years and four years into the future, and one in 1984 for the 5-year-average specification. The fact that the locations for the breakpoints are so close across all of the models shows that the algorithm for finding the breakpoints is robust to at least minor changes in specification.

The mid 1980s breakpoint suggests that changes in consumer behavior were caused by structural changes in financial markets as opposed to, say, the anti-discrimination laws (which were passed 10 years earlier) or the development of the sub-prime market (which occurred 10 years later). The 1985 timing might at first glance seem surprising, given that Congress passed the two main acts that deregulated the savings and loan industry in 1980 and 1982. However, the acts explicitly provided for a gradual phase-out of regulations. For example, Regulation Q was not fully eliminated until 1986. In the same way, there were substantial lags in the development of the secondary market. Although nascent secondary markets emerged in the 1970s, they did not really mature until 1986, when the Tax Reform Act of 1986 established the legal framework for mortgage markets that exists today. There was also a lag between the emergence of new mortgage designs (which occurred in the late 1970s and early 1980s) and a serious augmentation of the mortgage choice set. When the new designs originally appeared, they merely maintained reasonable mortgage choices in a high-inflationary environment. Meaningful expansion of the menu of mortgage types required both alternative prototypes and lower nominal interest rates and inflation, and this combination did not occur until 1986, when single-digit mortgage rates appeared for the first time since 1978. In short, given the realities of the evolution of the market for housing finance, a breakpoint in the mid 1980s seems perfectly reasonable.

A potential problem with this interpretation is that the broader macroeconomic environment was changing at around the same time. Specifically, the mid 1980s also saw the end of the high-inflation, high-interest-rate environment as well as the end of the great disinflation of the early 1980s.[27] The problem arises because our theory suggests that a fall in nominal rates, holding real rates constant, would have the same effect as a relaxation of borrowing constraints. To see why, recall that with traditional mortgages, a high nominal rate drives up the real value of initial monthly payments without raising the real interest cost. High nominal rates increase the monthly payment for a given home, and if the lender's allowable maximum fraction of monthly income that can be spent on housing remains constant, then this effectively makes the borrowing constraint more stringent. However, we doubt that the reduction in nominal interest rates during the mid 1980s is driving our results. If that were the case, we would expect to see two structural breaks, because relatively low nominal rates prevailed both before 1979 and after 1985. The notion that nominal interest rates are driving the process is also implausible given our previous results that found a positive time trend in the relationship between house value and future income (see Table 3). Given that nominal rates were relatively low at the beginning of our sample period and again at the end, such a time trend would not have emerged if these rates were the driving mechanism.[28]

We turn now from the timing of the structural breaks to an assessment of their quantitative impact. To do this, we augment equation (4.4) with an interaction term between house value and a dichotomous variable that takes a value of 1 for the year of the break and the years thereafter. For example, for the baseline sample, in which we find a breakpoint in 1985, the regression model is:

[27] For a detailed discussion and documentation of this disinflation and its effects on the macroeconomy, see Goodfriend and King (2005).

[28] From 1969 to 1977, the average nominal interest rate on a 10-year Treasury bill was 7.09 percent; from 1978 to 1985 it was 11.30 percent; and from 1986 to 2001 it was 6.94 percent.

$$lnY_{it+f} = \alpha_0 + \alpha_1 lnY_{it} + \alpha_2\ ln\ H_{it} + \alpha_3'(d8501 * ln\ H_{it}) + \beta X_{it} + D_t + e_{it}, \qquad (5.1)$$

where *d8501* takes the value of 1 for years between 1985 and 2001, and 0 for years between 1969 and 1984. In the spirit of the breakpoint analysis, equation (5.1) does not include interaction terms between time and house value.[29]

The parameter estimates for equation (5.1) are shown in column (5) of Tables 3, 4, and 5. The results reinforce the findings from the breakpoint analysis. In all cases, the estimates are positive and statistically significant at the 1-percent significance level. Focusing on the 2-year model (Table 3), the estimated coefficient corresponding to the interaction term, $(d8501 * ln\ H_{it})$, for the baseline sample is 0.071. This means that prior to 1985 the estimated elasticity of future income (two years ahead) with respect to house value was 0.146, while after 1985 it increased to 0.217. This is a substantial increase in the forecasting relationship between house purchases and future income, and it confirms the notion that mortgage markets became much less imperfect sometime in the mid 1980s.

5.3 Differences across sub-groups

Much of the public policy discussion of housing finance has focused on the ability of disadvantaged households to buy homes. In this context, we are particularly interested in the impact of the development of mortgage markets on poor families, female-headed households, and blacks. As well, there have been concerns about first-time homebuyers. In this section, we estimate the model separately for each of these sub-groups.

Poverty, race and gender. To investigate the evolution of the housing finance situation facing the poor, we estimate our model using only data from the SEO poverty sample in the PSID, which we refer to as the "poverty sample" hereafter. Summary statistics for the poverty sample are reported in columns (4)–(6) of Table 1.

The results when we use only observations from the poverty sample to estimate equation (4.5) are reported in column (2) of Tables 3, 4, and 5, and are very similar to their counterparts estimated using the baseline sample in column (1). The estimated coefficient on

[29] If α_2 does change within each subsample, then we should have found more than one breakpoint in our above analysis.

house value in column (1) of Table 3 is 0.132, while the corresponding estimate for the 4-year specification in Table 4 is now actually higher, with a value of 0.146, indicating that, for the poverty sample, the relationship between the value of the house purchase and income four years into the future is slightly stronger. However, for the 5-year-average specification, the estimate of 0.117 is a bit smaller. The respective coefficients on the interaction term, $time*loghval_{it}$, are similar in magnitude to their counterparts in the baseline specifications, although for the 4-year regression, the estimate is not statistically significant. Thus, poorer families exhibit similar forward-looking behavior in their housing decisions, and this behavior has also become stronger over time, although the effect seems to be sensitive to our definition of future income.

Breakpoint analysis results for the poverty sample are reported in column (2) of Table 6. A single structural break in 1981 is found for the 2-year model; one break is found in 1978 for the 4year model (significant at the 10-percent level); and one break is found in 1980 (significant at the 5-percent level) for the 5-year-average model.

The quantitative magnitudes of the various breaks (based on estimating equation (5.1)) are displayed in column (6) of Tables 3, 4, and 5. The shifts are also similar in magnitude compared with those for the baseline sample, with values of 0.10, 0.12, and 0.09, for the 2-year, 4-year, and 5-year-average models, respectively. In the case of the 4-year specification, the magnitude of the coefficient estimate associated with house value approximately doubled after the breakpoint, increasing from approximately 0.10 before the break to 0.22 after the break. These results suggest that the decrease in market imperfectness for poorer households was similar in magnitude to that in our baseline sample.

This begs the question of why the breakpoint for the poverty subsample occurred earlier than for the sample as a whole. Given that the key financial market innovations mostly took effect later, we searched for alternative stories. Because poor families are disproportionately headed by women and blacks (see Table 1), a natural starting point for such an investigation would be to estimate the model separately by gender and race.

For female-headed households, we find strong evidence of a breakpoint in 1979 for the 5-year-average specification, minor evidence of a breakpoint in 1981 for the 2-year specification, and no evidence of a breakpoint for the 4-year specification. We do not find evidence of a breakpoint for black households in any specification.[30]

A possible explanation for the finding for female-headed households relates to legislation passed in the mid 1970s. In Section 2 we noted that concerns about possible discrimination based on race and gender led to the passage of the Equal Credit Opportunity Act (ECOA) and its amendment in the mid 1970s. Whether or not legislative measures were effective in helping minorities and women was (and still is) a controversial topic. That said, expanded opportunities for women could explain our findings of a breakpoint in the late 1970s and early 1980s, given that some have argued that ECOA was implemented with a lag (see, for example, Munnell et al. 1996). And because households in the poverty sample were twice as likely to be headed by women as in the baseline sample, this could explain our poverty sample results as well. The failure to find a breakpoint in the black subsample could reflect the stubbornness of racial discrimination in lending markets. As we mentioned above, some researchers argue that racial discrimination remained pervasive in mortgage markets as late as 1990.

First-time versus repeat homebuyers. Another group of people who may be relatively disadvantaged when it comes to housing finance are first-time homebuyers. We expect families who currently own or previously owned a home to have advantages over first-time homebuyers when it comes to financing the purchase of a new house and obtaining a mortgage. For example, existing homeowners have an established credit history and often have a cushion of available equity. Therefore, this group would stand to benefit least from a less imperfect mortgage market, since they are less likely to be borrowing-constrained in the first place. The other side of the same coin is that if mortgage-market improvements really are efficacious, they should have a relatively large impact on first-time homebuyers. To investigate these issues, we divide our sample into first-time homebuyers and repeat homebuyers, and estimate our models separately for each sample. Summary statistics for these samples are shown in Table 2.

The third column in Tables 3, 4, and 5 shows the parameter estimates from the basic equation (4.5) for first-time homebuyers. The estimates are very similar to those for the baseline

[30] These results are available upon request.

and poverty samples. The coefficient on house value ranges from 0.136 for the 2-year regression to 0.077 for the 5-year average regression, which is consistent with the results from the other samples. However, the results provide no evidence that forward-looking behavior has changed over time for first-time homebuyers. The coefficient associated with the linear time-trend interaction term is not significant for any of the specifications, implying that the observed relationship between house expenditures and future income has not changed over time.

The corresponding breakpoint results are consistent with those above. Column (3) of Table 6 tells us that there is no evidence of a structural change in the coefficient estimate of house value in the first-time homebuyer sample, and this result is the same for all variants of the model. This suggests that the decreased imperfectness of the mortgage market did not have a significant effect on first-time homebuyers.

Our failure to find any break in the first-time homebuyer sample is somewhat puzzling, because one would certainly expect this group to face borrowing constraints. One possible explanation is that before subprime lending and credit-scoring, lenders focused almost entirely on credit histories. In such a regime, the relative lack of credit histories for first-time homebuyers may have presented an obstacle that even substantial innovation in mortgage markets could not overcome. Because our sample ends in 2001, we cannot say much about the effects of the creation of the subprime market and the expanded use of credit scoring.

The results for repeat homebuyers are displayed in column (4) of Tables 3, 4, 5, and 6. They are also very similar to the results for the baseline sample. The only differences are that the point estimates corresponding to the linear time-trend interaction term are somewhat higher (in all specifications) than in the baseline sample, and the structural break occurs one or two years earlier.

High-housing versus low-housing families. As a final check on our model, we divide our sample according to the ratio of house value to current income at the time of purchase. As already noted, according to our simple theoretical model, the sensitivity of future income to housing expenditures should increase more over time for families who originally purchased relatively small homes, because they are the families who are most likely to have been constrained On this basis, we would expect the increase in the sensitivity of future income to housing expenditures to be much more pronounced for households purchasing lower-valued homes relative to their income at the time of purchase.

There is bound to be some arbitrariness in determining what is a "high" or "low" value of housing relative to current income. We simply set the dividing line where the log of the ratio was equal to 1. For the "low" sample, we find strong evidence of a single breakpoint in 1984 for the 4-year and 5-year-average specifications of the model (significant at the 5-percent level), and a breakpoint in 1985 for the 2-year specification (although not quite significant at the 10-percent level). For the "high" sample of households, we find no evidence of a breakpoint in the relationship between future income and housing purchases.[31] These findings are consistent with the implications of the simple theoretical model presented in Section 3 and with Figure 4, the plot of the distribution of house expenditures versus the distribution of future income from Section 4.3.

5.5 The role of wealth

We have shown that, at least for the population as a whole, the estimated coefficient on house value has increased over time. We interpret this finding as a consequence of the attenuation of market imperfections. According to this view, early in the sample period, because of these market imperfections, future income prospects played only a small role in current housing decisions. Presumably, current wealth and availability of family transfers were instead the critical variables. As a consequence, the value of the current house purchase was a poor predictor of future income. However, later in the sample, as constraints were relaxed, the correlation between current wealth and current housing expenditures decreased, as did the correlation between family transfers and current housing expenditures. At the same time, the relationship between current house purchases and future income increased.

Is this story valid? The best way to answer this question would be to include in the empirical model detailed information on household wealth and family transfers for the entire

[31] We also estimated equation (4.5) for only the families with high ratios of housing to current income; we found that the estimated coefficient of the interaction term between time and house value was not significantly different from zero for any of the model specifications, further reinforcing the findings from the breakpoint analysis. These results are available upon request from the authors.

sample period. Although this is not possible, the PSID does provide supplementary information regarding household wealth for four years: 1984, 1989, 1994, and 1999. Therefore, we re-estimated the income forecasting equation with wealth as an explanatory variable for these years.[32] For this sample, which has only 1,233 observations, the coefficient on house value is 0.22 with wealth included, while it is 0.21 without wealth. Hence, omitting wealth from the equation does weaken the predictive power of house value, as argued above; however, the difference is small and not statistically significant. While it is comforting that the coefficient moved in the direction we predicted, we must regard this question as being open until more complete data become available.

5.6 The role of labor supply

Our model posits that people decide on the purchase of their home given their beliefs regarding future earnings, and we find such a relationship in the data. However, an alternative interpretation is that causality runs in the other direction—households decide to purchase a house that is beyond their means, and then work harder in the future to earn enough income to make the mortgage payments.

There are basically two feasible channels through which a household could increase its income over a 2-to-4-year time horizon. The first and more likely channel is by increasing labor supply, either by working more hours or by obtaining a second job. To see whether the correlation between the value of a current house purchase and future income is driven by increases in labor supply, we estimated a regression of total annual hours worked on the value of the house purchase, current hours worked, income, and our usual set of demographic variables, two years (as well as four years) after a house purchase. If the increased labor supply scenario were operative, we would expect to see a statistically significant, positive coefficient estimate associated with house value. However, the estimated coefficient is not statistically different from zero in any of our specifications.

[32] It would be more informative if we had wealth information for earlier years in our sample, because we believe that is when the correlation between wealth and housing was the highest, and hence the bias from omitting it most severe.

A second channel through which a household might affect its income in the short run is occupational change. The PSID contains information about the occupation of both the head of the household and the spouse.[33] We use this information to construct a dichotomous variable that takes a value of 1 if either the head of the household or the spouse switched occupations during the two (four) years after a house purchase, and 0 otherwise. We then estimated a probit model for occupational switches, in which we included the value of the house purchase, current labor income, a set of occupation indicator variables, and a set of demographic variables. If households were buying houses that they could not afford and then switching jobs in an effort to increase income, we would expect that higher expenditures on housing would increase the probability of switching occupations. However, we find that the value of the house purchase has no statistically discernible effect on the probability of switching careers within two years of a purchase. Exactly the same result is obtained when we look at a 4-year time horizon.[34]

5.7 Measuring income

An important consideration when measuring income is whether capital income should be included as well as labor income. We believe the answer is yes, as forward-looking households likely consider all sources of income when contemplating a house purchase. However, because capital and business income are probably more difficult to forecast than labor income, it is useful to confirm that our results are not being driven by this definition of income. Therefore, we re-estimated all of the forecasting equations using only household labor income.[35] We found no substantive differences from the previous results.[36]

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[33] The coding of occupation in the PSID presents some technical issues. In the early years (up to 1980), occupation was coded at the 1-digit or 2-digit Census level, while in the later years it was coded at the 3-digit level. To construct an occupational code that is consistent for the entire span of the data, we used the 1968–1980 Retrospective Occupation-Industry Files, a PSID supplement that provides 3-digit occupation codes for household heads and spouses pre-1980. We then constructed our own 1-digit code for the entire 32-year sample (12 different classifications).

[34] These results are available upon request to the authors.

[35] Another issue is whether income should be measured in real or nominal terms. We have used real income, because it is more consistent with the basic permanent income hypothesis. However, to make sure that our results are not sensitive to this distinction or our choice of deflator, we re-estimated the model using nominal magnitudes for house value and income, and we found that the results were essentially unchanged.

[36] The coefficient on current income increased and the cubic term in age was no longer significant, but the estimated coefficient on house value was not noticeably changed.

6. Investigating the Contribution of the GSEs

We now use our econometric model to assess the impact of the securitization activities and portfolio decisions of the housing GSEs, Fannie Mae and Freddie Mac. To begin, we define $GSElev_t$ to be the proportion of the stock of all home mortgages that were securitized by Fannie and Freddie in year *t*, and $GSEacq_t$ to be the corresponding flow variable. Figures 1 and 2, respectively, show the evolution of each of these ratios over time. With our measures of GSE activity in hand, the question is how to use them to estimate the extent to which the loosening of borrowing constraints documented above can be attributed to the securitization activity of the GSEs. A straightforward approach is to interact one or another measure of their activity in period *t*, GSE^*_t, with house value in our empirical regression model:

$$lnY_{it+f} = \alpha_0 + \alpha_1 lnY_{it} + \alpha_2 \ ln \ H_{it} + \alpha_3'' GSE^*_t \ lnH_{it} + \beta X_{it} + D_t + e_{it}. \qquad (6.1)$$

A positive value of α_3'' implies that more extensive GSE activity is associated with H_{it}'s being a better predictor of future income, which in turn implies an improved ability for households to purchase homes that are in line with their future income prospects. In short, a positive value of α_3'' is consistent with the notion that GSE activity has helped to make the housing finance market less imperfect.

An important question is the time period over which the model should be estimated. As emphasized above, the 1970s and early 1980s were a period of tremendous flux in U.S. financial markets. The technological and regulatory environments changed dramatically. We have no way to parameterize these changes, and hence, no way to discern the independent effect of the GSEs' securitization activities. In mechanical terms, if we were to estimate equation (6.1) with observations going back into the 1970s, the coefficient on the *GSE* variables would be biased upward because it would be picking up not only the impact of securitization, but also the impact of the omitted changes. In order to deal with this problem, we begin the sample in 1983. By then, most of the deregulatory changes had been enacted, so we can feel fairly confident that the coefficients on the *GSE* variables are in fact reflecting the impact of their securitization activities, and not other changes in the financial environment. In any case, prior to 1983, GSE securitization activity was almost negligible. Before 1982, outstanding mortgages securitized by the GSEs were never more than 2 percent of all home mortgages outstanding. In 1982 this number increased to 5 percent, while after 1990 the proportion of all home mortgages outstanding securitized by the GSEs never fell below 25 percent and was often well above 30 percent (see Figure 1).

Tables 7, 8, and 9 show the estimated parameters of equation (6.1) for each measure of future income (two years forward, four years forward, and the 5-year future average, respectively). The format of these tables is identical to that of the previous tables that show our basic results. Each column exhibits results using data from the indicated sample (baseline, poverty, first-time homebuyers, repeat homebuyers). Columns (1)–(4) include $GSElev_t$ in the regression, while columns (5)–(8) include $GSEacq_t$. The results are quite clear for the baseline sample (column 1): Whether we measure GSE securitization activity in terms of stocks or flows, and whether we are trying to estimate future income two years ahead, four years ahead, or taking the five-year average, one cannot reject the hypothesis that the coefficient on the interaction term between the log of house value and GSE securitization activity is zero. Thus, we find no evidence that the securitization activities of Fannie and Freddie played any role in making the housing finance market less imperfect.

The GSEs and marginal borrowers. One of the missions of the GSEs is to facilitate the financing of housing for low and middle-income families. Indeed, in 1992 Congress passed legislation requiring the Department of Housing and Urban Development to establish quantitative goals for Fannie and Freddie with respect to their purchases of mortgages from families in these income groups (Ambrose and Thibodeau 2004). At least since that time, the extent to which the GSEs actually help low-income families has been a subject of fierce political debate, with the GSEs claiming that they have helped marginal borrowers the most and their critics arguing the opposite. Taking the GSEs' claims at face value, perhaps even if they have not improved the system of housing finance for the population as a whole, they have improved things for marginal borrowers. In terms of our model, if the GSEs' claims were correct, we would expect to see significant and positive GSE interaction terms in the models estimated with the poverty and first-time homebuyer samples.

Columns (2)–(3) and (6)–(7) display the findings when we estimate equation (6.1) using the poverty and first-time homebuyer samples. The point estimates are statistically insignificant for both samples in all of the specifications. Hence, we cannot reject the hypothesis that GSE securitization activity played no role in reducing the capital market imperfections facing low-income families and first-time homebuyers.

The retained portfolio. In addition to securitizing mortgages, the GSEs issue debt to purchase mortgages and mortgage-based securities. This so-called "retained portfolio" has increased enormously over time. From 1990 to 2001, it increased from 4.8 percent to 16.0 percent of total home mortgages outstanding. The retained portfolio has emerged as an object of fierce political debate. Critics of the GSEs, including former Federal Reserve Board Chairman Alan Greenspan, have argued that the hedging activities associated with the retained portfolio are a dangerous source of systematic risk to the U.S. financial system.[37] In this view, the retained portfolio does nothing to help homeowners; it is merely a way for the GSEs to turn themselves into very profitable but risky hedge funds. In contrast, the GSEs argue that their demand for mortgage-backed securities is an important source of liquidity in the housing market and therefore leads to substantial benefits to homeowners.

Our model provides a natural framework for assessing the impact of the retained portfolio on the financial environment for homeowners. To begin, we construct both stock and flow measures of the retained portfolio, normalized by the total stock and flow of home mortgages outstanding, respectively. The measures include GSE holdings of GSE-issued mortgage-backed securities as well as other mortgage debt held by both Fannie and Freddie.[38] Following the tack described above for estimating the effect of GSE securitization activity, we interact both of these variables with house value and include the interaction terms in our regression models. We then estimate the models using the post-1983 portion of our sample. The results, not shown here for the sake of brevity,[39] provide no evidence that the retained portfolio has made the housing finance market less imperfect for households. More precisely, whether

[37] See Kopecki (2006). For further discussion of the systemic risk issue, see Jaffee (2003).

[38] The series used for constructing these measures were obtained from the FAME database maintained by the Board of Governors of the Federal Reserve. The corresponding mnemonics are DTMMPHM_N.M, DTMFHFFRFC_N.Q, DTMNPHN_N.M, and DTMFHFFAFN_N.Q.

[39] These results are available upon request.

we measure the GSE retained portfolio in terms of stocks or flows, and whether our left–hand-side variable is future income two years ahead, four years ahead, or a 5-year average, one cannot reject the hypothesis that the coefficient on the interaction term between the log of house value and the GSE retained portfolio is zero.

7. Conclusions

Taken together, our results suggest the following conclusions: First, the housing finance market has become substantially less imperfect over time. Second, for the population as a whole, there appears to have been a discrete improvement in the housing finance markets in the early to mid 1980s. We conjecture that this was due to a combination of innovative mortgage products, deregulation, and the development of a secondary market in mortgages. Third, one cannot reject the hypothesis that the GSEs and their activities in the secondary market have failed to improve the housing finance environment facing low-income and first-time homebuyers.

More broadly, we have argued that a life-cycle approach to thinking about questions regarding housing finance is both theoretically attractive and empirically tractable. This approach takes advantage of the life-cycle prediction that current behavior can predict future income in the presence of well-functioning credit markets. It might be fruitfully applied in other contexts. Possible issues include measuring the "affordability" of housing, assessing the extent of mortgage market discrimination, and other topics as well.

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Appendix A

Some PSID issues

The PSID tracks members of its first-wave (1968) families, including all those leaving to establish separate family units. Children born to a member of a first-wave family are classified as sample members and are in many cases tracked as separate family units when they set up their own households. Ex-spouses and other adult sample members who move out of PSID family units are also tracked to their new family units. Thus, new PSID families originate from two sources: Children from original PSID families who grow up and establish separate households, and marriage partners who divorce and go their separate ways. This dynamic structure allows two ways to define and identify a household across time. One possibility, which is consistent with the PSID's definition, is to identify a household simply by its head (usually the husband). Such a definition would not factor in a change of spouse. For example, if the husband and wife divorced, and the husband later remarried, the new family created by the new marriage would be identified as the original household. The second possibility, which we adopt, is to identify a household as a unique husband/wife pair. With respect to the above example, if the husband and wife of the original household divorced, then the household would cease to exist in the sample, and a new household would form with the new marriage. We choose to define a household this way because we feel that it is more consistent with our analysis of housing choice and income behavior.

Another important issue is the reliability of the PSID data on homeownership and housing finance. We performed a comparison of homeownership and housing finance data in the PSID with information from other sources, in order to be sure that we are working with a nationally representative sample. Specifically, we compared our annual homeownership rates from the PSID to those produced by the federal government's Office of Federal Housing Enterprise Oversight (OFHEO). During the last three decades, there is never more than a 2-percentage-point difference between OFHEO's annual rates and those from our PSID data. We also compared PSID mortgage data to corresponding data from Chicago Title's Annual Survey of Recent Home Buyers. The PSID has mortgage information for most of our sample period, including the original value of the mortgage and the annual mortgage payment. Average loan-to-value ratios and payment-to-income ratios for first-time homebuyers displayed very similar patterns over time in each data set.

Appendix B

A simple model of housing choice

This appendix presents a more formal version of the model discussed in Section 3. We show that in the context of a simple, two-period model, the relaxation of a debt-to-income constraint (DTI) increases the sensitivity of future income to housing expenditures. Our derivation is comprised of two pieces. First, we show that in a world with two possible types of houses (big and small), household wealth determines choice of dwelling, *ceteris paribus*. If household wealth exceeds some threshold level w^*, that household buys a big house; if wealth falls short of the threshold, it buys a small house. Further, we show that w^* is weakly decreasing in δ, the maximum allowable ratio of debt to current income. Second, we consider a model with households that differ only in the level of their future income, either low (y_1^L) or high (y_1^H), and show that an increase in the maximum allowable debt-to-income ratio, by changing w^* and thus the mix of buyers of houses of different types, increases the sensitivity of future income growth to current house purchase.

B.1 Derivation of the wealth threshold

We consider a deterministic, two-period world in which a representative household faces the following problem. Given current income, y_0, future income, y_1, and current wealth, the household must choose between one of two houses ($H^B > H^S$) and consumption today and in the future. Borrowing to finance a house purchase is represented by θ and the amount of the mortgage is constrained to be less than or equal to a constant fraction of current income, δ, the maximum DTI ratio. We also assume a constant interest rate, r, log-utility in which consumption of non-durables and consumption of housing are additively separable, and for simplicity, no discounting over time on the part of households. The household's problem is to maximize

$$\log(c_0^\alpha H^{1-\alpha}) + \log(c_1^\alpha H^{1-\alpha})$$

subject to:

$$c_0 = y_0 + w - H + \theta, c_1 = y_1 - (1+r)\theta,$$

$$\delta y_0 - \theta \geq 0.$$

We separate the decision into two parts. First, the household chooses an optimal consumption profile conditional on a particular house. Letting $V(H;\delta, w, y_0, y_1, r)$ measure the indirect utility conditional on house choice H, the household then chooses the house that maximizes V.

We establish two facts about this problem. First, we show that holding all else equal, there exists a critical or threshold value, w^*, such that if w falls short of w^*, the household chooses to buy a small house, and if w exceeds w^*, the household chooses to buy a large house. Second, the critical value, w^*, is weakly decreasing in δ, the maximum allowable debt-to-income ratio. Taken together, these facts imply that in households that differ only by wealth, an increase in δ raises the fraction of that population that opts for the large house.

To begin, let:

$$\Delta V(\delta, w) = V(\delta, H^B, w) - V(\delta, H^S, w).,$$

where for notational simplicity we suppress the additional arguments of V. Our first goal is to show that when $w \geq w^*$, $\Delta V(\delta, w) > 0$ and when $w \leq w^*$, $\Delta V(\delta, w) < 0$. We proceed by setting up a Lagrangian for the conditional problem:

$$L = \log(c_0^\alpha H^{1-\alpha}) + \log(c_1^\alpha H^{1-\alpha}) +$$
$$\lambda_0 \cdot (y_0 + w - H + \theta - c_0) + \lambda_1 \cdot (y_1 - (1+r)\theta - c_1) + \lambda_{DTI}(\delta y_0 - \theta)$$

plus the usual complementary-slackness conditions. By the envelope theorem,

$$\partial V / \partial w = \lambda_0 = 1/c_0 > 0.$$

If the household is unconstrained, then λ_{DTI} is zero, and the associated initial consumption is

$$c_{0,\lambda_{DTI}=0} = (y_0 + y_1/(1+r) + w - H)/2. \tag{1}$$

If it is constrained then λ_{DTI} is positive, and initial consumption is

$$c_{0,\lambda_{DTI}>0} = (1+\delta) y_0 + w - H. \tag{2}$$

In order to determine the sign of the term $\frac{\partial \Delta V}{\partial w}$, we need to know the relationship between consumption if the family buys a small house, c_0^S, and consumption if the family buys a big house, c_0^B. To think about this, it is useful to consider the various possibilities with respect to the borrowing constraints that face the family. If the household is constrained whether it buys the big house or the small house, then equation (1) shows that $c_0^S > c_0^B$. If the household is unconstrained whether it buys the big house or the small house, then equation (2) shows that $c_0^S > c_0^B$. In either case, purchase of a house comes at the expense of current consumption. Another regime is for the borrower to be constrained in buying the small house and unconstrained in buying the big house, but this is impossible. This leaves only the case in which the household is unconstrained in buying the small house, but constrained in buying the big house—the case which we next explore.

We know that for a given house, unconstrained initial consumption is always larger than constrained initial consumption. That is:

$$c_{0,\lambda_{DTI}=0}^B > c_{0,\lambda_{DTI}>0}^B$$

and

$$c^S_{0,\lambda_{DTI}=0} > c^S_{0,\lambda_{DTI}>0}.$$

Furthermore, by equation (1),

$$c^S_{0,\lambda_{DTI}>0} > c^B_{0,\lambda_{DTI}>0},$$

implying that

$$c^S_{0,\lambda_{DTI}=0} > c^S_{0,\lambda_{DTI}>0} > c^B_{0,\lambda_{DTI}>0}.$$

Thus, $\frac{\partial \Delta V}{\partial w} = \frac{1}{c^B_0} - \frac{1}{c^S_0} > 0$. That is, as wealth increases, so does the gain in utility associated with buying a big house.

Now, $\Delta V(\delta, w^*) = 0$ implicitly defines a function $w^*(\delta)$. Our goal is to show that $w^{*'}(\delta) < 0$. Differentiating both sides of $\Delta V(\delta, w^*) = 0$ with respect to δ yields

$$\frac{\partial \Delta V}{\partial w} w^{*'}(\delta) + \frac{\partial \Delta V}{\partial \delta} = 0, \tag{3}$$

implying that

$$w^{*'}(\delta) = -\frac{\partial \Delta V}{\partial \delta} / \frac{\partial \Delta V}{\partial w}.$$

We have already shown that the denominator is positive. To finish the proof, we simply need to sign the numerator. Using the envelope theorem again,

$$\frac{\partial \Delta V}{\partial \delta} = [\lambda^B_{DTI} - \lambda^S_{DTI}]y_0.$$

If neither constraint is binding, then $w^{*'}(\delta) = 0$ because both of the λ's are zero. If the constraint binds only for the big house, then $w^{*'}(\delta)$ exceeds zero because $\lambda^B{}_{DTI} > \lambda^S{}_{DTI}$. If the constraint binds for both houses, then we use the first-order condition of the optimization problem, which says that

$$\lambda_{DTI} = [\lambda_0 - \lambda_1](1+r) = [\frac{1}{c_0} - \frac{1+r}{c_1}].$$

Thus

$$\frac{\partial \Delta V}{\partial \delta} = [\frac{1}{c^B_0} - \frac{1+r}{c^B_1} - \frac{1}{c^S_0} + \frac{1+r}{c^S_1}]y_0 > 0,$$

where the last inequality follows from the fact that when the DTI constraint binds, the budget constraint implies that $c^B_1 = c^S_1$ and $c^B_0 < c^S_0$. Hence, we have shown that w^* is weakly decreasing in δ, the maximum allowable debt-to-income ratio. Algebraically, $w^{*'}(\delta) \le 0$.

B.2 Effect of relaxing the DTI constraint on different types of households

We now assume two types of households that differ only in their future income, y_1^i, where *i* is either "high" (H) or "low" (L). We assume that there is a continuum of households with measure 1 of each type of household, indexed by their wealth w. We assume that the wealth distribution is bounded above by $\overline{w}$ and below by $\underline{w}$, and that it has a cumulative distribution function that is strictly increasing. For each type of household, we can compute a wealth threshold function, $w_i^*(\delta)$. For simplicity, we make the following three assumptions on the wealth distribution:

$$\overline{w} < w_L^*(\infty), \tag{4}$$

$$\underline{w} > w_H^*(\infty), \tag{5}$$

$$\overline{w} > w_H^*(0) > \underline{w}. \tag{6}$$

Conditions (4) and (5) imply complete separation of types when there are no borrowing constraints. When unconstrained, the richest low type of household buys a small house and the poorest high type of household buys a big house. However, Condition (6) guarantees that the constraints matter: If we eliminate borrowing altogether, the richest high types still buy a big house, but the poorest opt for a small house.

In Section 3, we made two claims, which we prove in turn. First, we argued that "the borrowing constraints attenuate the observed relationship between income growth and the size of current home purchases." To see why, start with the situation with no constraints. In that case, all high types buy big houses and all low types buy small houses. Thus,

$$Y(H_B,\infty) = y_1^H \text{and} Y(H_S,\infty) = y_1^L,$$

where $Y(H_j,\delta)$ is the average future income of buyers of house, $j = B, S$, when the debt-to-income constraint equals δ. Now we introduce a finite debt-to-income constraint. By continuity and condition (6), there exists δ sufficiently small, call it $\hat{\delta}$ such that at least some high types buy a small house, meaning that the pool of small-house buyers is now a mix of high and low types, rather than a monoculture of high types, so

$$Y(H_S,\hat{\delta}) > Y(H_S,\infty).$$

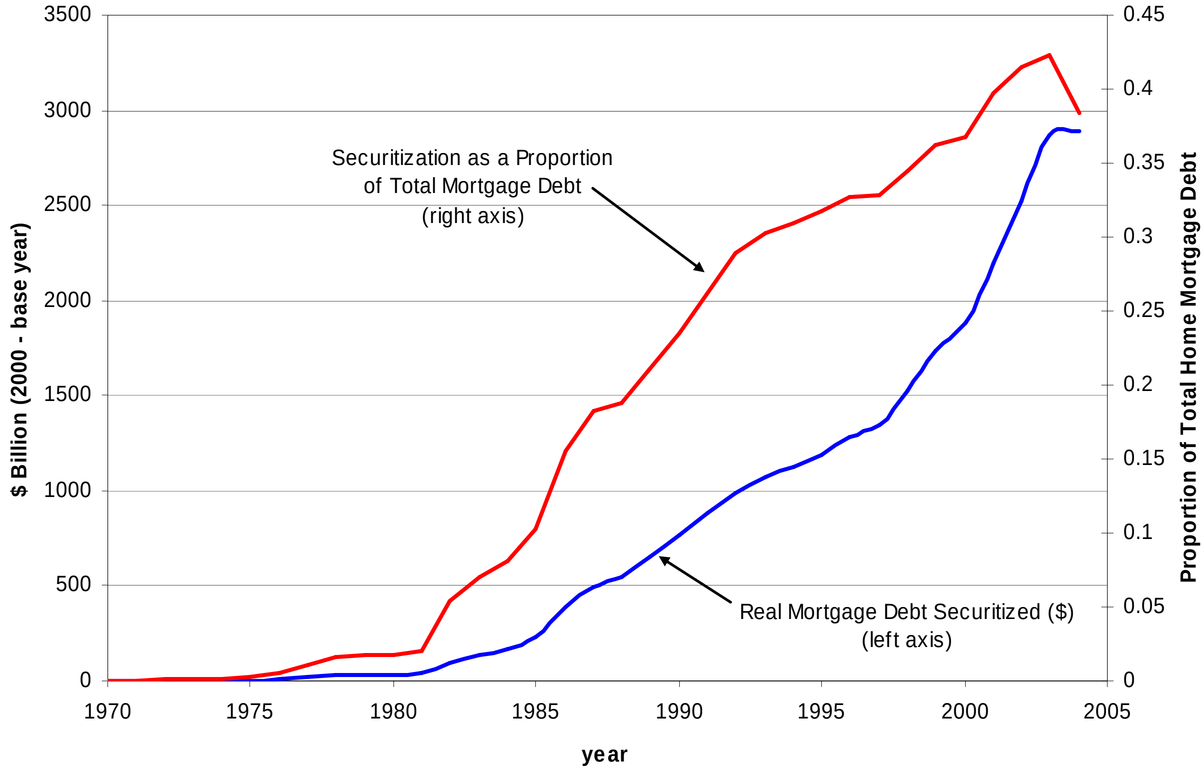
But, the income of the big-house buyers does not change, as condition (4) assures us that only high types ever buy the big house, so

$$Y(H_B,\hat{\delta}) = Y(H_B,\infty),$$

implying that the income gap between big-house buyers and small-house buyers shrinks when we introduce a binding constraint.

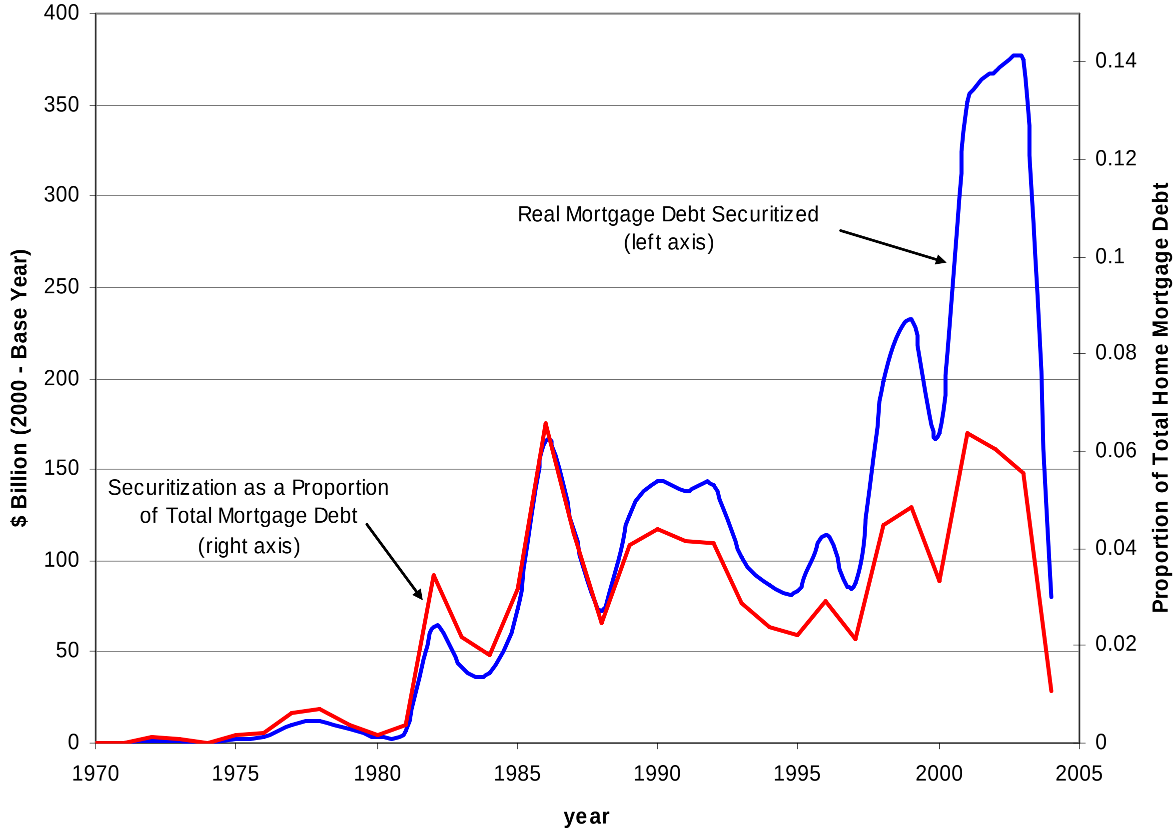
Our second claim was that relaxing an existing debt-to-income constraint increased the gap between average income growth of big-house buyers and small-house buyers. To see why, suppose we have an economy with debt-to-income constraint level $\hat{\delta}$, as above. If we raise δ, the number of high types that buy the small house falls, since $w_H *'(\delta) < 0$, and the number of low types stays the same, lowering the average income of small-house buyers. The cross-sectional relationship between house size and income growth increases. Because the average future income of big-house buyers again remains the same, it follows that the gap shrinks when we lower δ.

Figure 1: GSE (Fannie Mae and Freddie Mac) Securitization (levels)



Source: Board of Governors (FAME database) and Flow of Funds.

Figure 2: GSE (Fannie Mae and Freddie Mac) Securitization (flows)



Source: Board of Governors (FAME database) and Flow of Funds.

Figure 3: Borrowing Constraints and the Relationship between Income Growth and Housing Expenditure

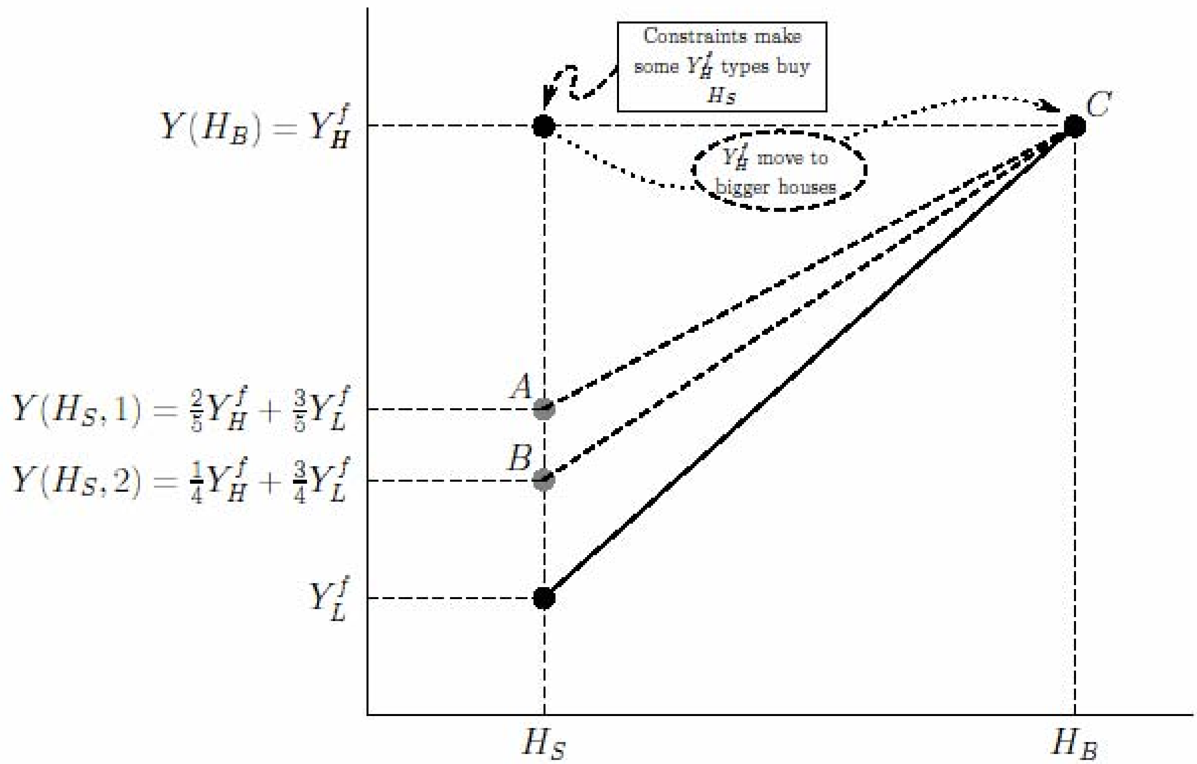
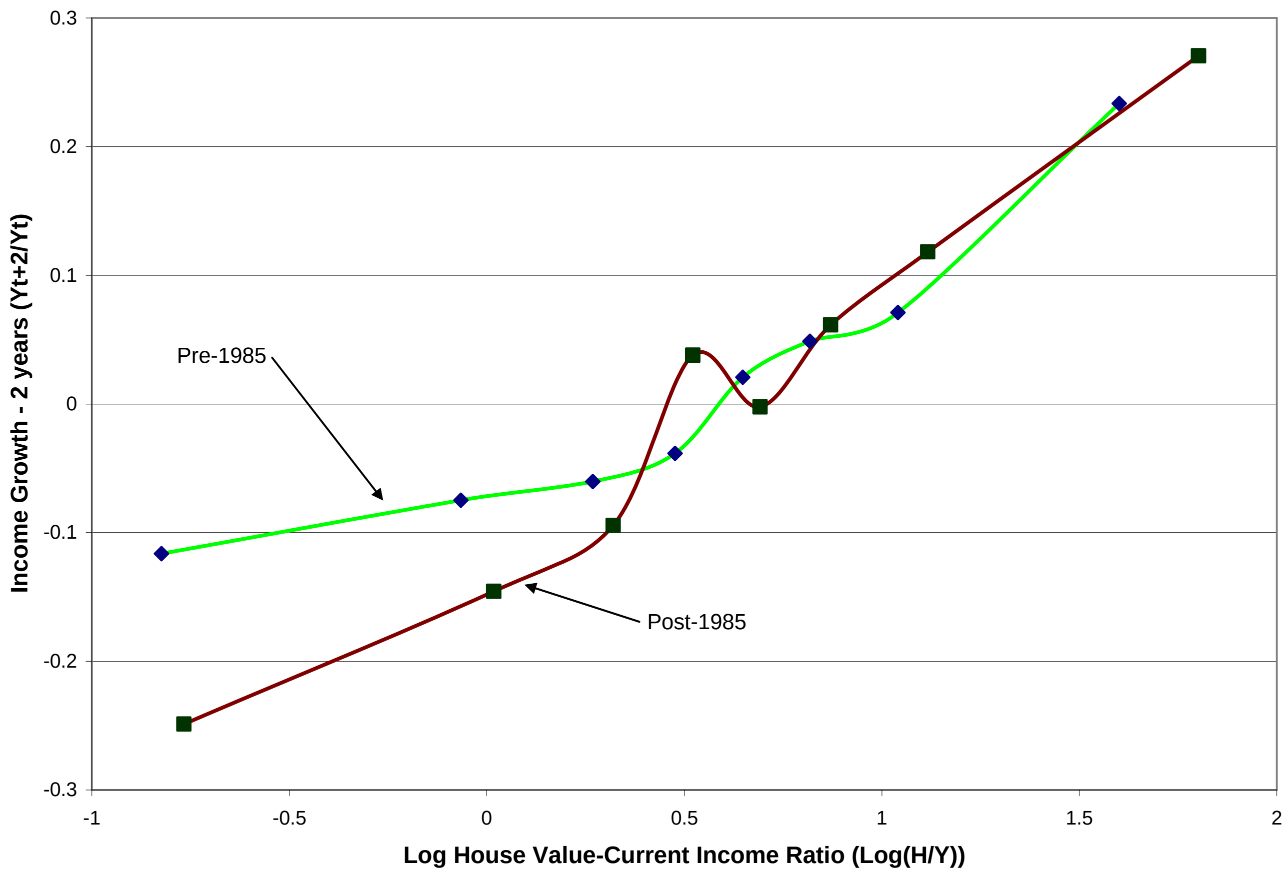


Figure 4: Octile Plot — House Value vs. Income Growth



Source: Panel Study of Income Dynamics, 1969 to 2001.

Table 1: Summary Statistics for the Baseline and Poverty Samples

	Baseline Sample			SEO Poverty Sample		
Real Quantities (2000 Dollars)	**Freq**	**Mean**	**Stdev.**	**Freq**	**Mean**	**Stdev.**
total family income	9,444	57,372	58,687	4,872	40,845	32,170
labor income	9,444	45,942	41,028	4,872	33,410	28,122
house value	9,603	112,603	109,385	4,786	73,640	76,570
principal outstanding	6,851	84,476	72,608	3,218	60,831	48,985
size of household	9,673	3.0	1.4	4,872	3.5	1.8
age of head	9,672	39	22	4,872	37	12
*Percentages**						
male	9,673	86.7	34.0	4,871	76.4	42.4
white	9,503	91.3	28.2	4,710	36.8	48.2
black	9,503	4.9	21.6	4,710	61.7	48.6
other race	9,503	3.8	19.1	4,710	1.5	12.2
educ < high school	9,665	7.4	26.1	4,862	13.5	34.2
educ = high school	9,665	31.7	46.5	4,862	39.9	49.0
high school < educ < college	9,665	34.3	47.5	4,862	34.4	47.5
educ = college	9,665	18.1	38.5	4,862	8.5	27.9
educ > college	9,665	8.5	27.9	4,862	3.7	18.8

Source: Panel Study of Income Dynamics, 1969 to 1999.
*Means and standard deviations of dichotomous variables expressed as percentages.

Table 2: Summary Statistics for First-Time and Repeat Homebuyer Samples

	First-Time Homebuyers			Repeat Homebuyer		
Real Quantities (2000 Dollars)	**Freq**	**Mean**	**Stdev.**	**Freq**	**Mean**	**Stdev.**
total family income	4,333	47,172	33,800	5,111	66,018	72,340
Labor income	4,333	39,604	30,464	5,111	51,316	47,548
House value	4,419	89,189	73,562	5,184	132,562	129,181
principal outstanding	3,194	71,671	53,332	3,657	95,659	84,411
size of household	4,457	2.9	1.4	5,216	3.1	1.4
Age	4,457	34	13	5,215	44	27
*Percentages**						
Male	4,457	84.1	36.6	5,216	88.9	31.5
White	4,373	88.4	32.0	5,130	93.8	24.2
Black	4,373	6.7	25.1	5,130	3.3	17.9
Other race	4,373	4.8	21.5	5,130	2.9	16.8
educ < high school	4,453	7.5	26.3	5,212	7.3	25.9
educ = high school	4,453	34.6	47.6	5,212	29.2	45.5
high school < educ < college	4,453	34.1	47.4	5,212	34.4	47.5
educ = college	4,453	17.0	37.5	5,212	19.2	39.3
educ > college	4,453	6.8	25.3	5,212	10.0	30.0

Source: Panel Study of Income Dynamics, 1969 to 1999.
*Means and standard deviations of dichotomous variables expressed as percentages.

Table 3 – Parameter Estimates for All Samples: Income 2 Years in Future

	INTERACTION WITH TIME-TREND				BREAK POINTS		
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Base	(6) Poverty	(7) Repeat
log(total family income)	0.391 (7.03)	0.411 (6.56)	0.441 (6.86)	0.362 (4.66)	0.391 (7.02)	0.411 (6.56)	0.361 (4.65)
log(house value)	0.118 (3.73)	0.132 (3.65)	0.136 (4.26)	0.114 (2.14)	0.146 (6.31)	0.133 (4.36)	0.147 (4.06)
age	0.053 (3.62)	0.045 (1.68)	0.038 (1.93)	0.060 (3.08)	0.053 (3.69)	0.045 (1.68)	0.061 (3.16)
high school	0.098 (1.83)	0.048 (1.06)	-0.006 (0.12)	0.154 (1.70)	0.097 (1.85)	0.046 (1.02)	0.158 (1.78)
some college	0.185 (3.44)	0.192 (3.98)	0.064 (1.41)	0.261 (2.93)	0.184 (3.48)	0.191 (3.96)	0.264 (3.00)
college	0.290 (4.96)	0.224 (2.92)	0.213 (4.10)	0.333 (3.43)	0.289 (5.01)	0.223 (2.90)	0.335 (3.50)
> college	0.377 (6.12)	0.376 (5.99)	0.287 (5.21)	0.426 (4.27)	0.376 (6.19)	0.372 (5.96)	0.427 (4.35)
black	-0.007 (0.24)	-0.091 (3.18)	-0.036 (0.93)	0.056 (1.29)	-0.008 (0.26)	-0.091 (3.16)	0.054 (1.27)
other race	-0.016 (0.3)	0.048 (0.60)	-0.198 (1.16)	-0.050 (0.76)	-0.015 (0.28)	0.054 (0.67)	-0.045 (0.68)
female	-0.285 (6.85)	-0.340 (6.72)	-0.235 (4.36)	-0.290 (4.73)	-0.285 (6.85)	-0.341 (6.74)	-0.288 (4.73)
Size	0.018 (2.51)	0.022 (2.90)	0.012 (1.35)	0.020 (1.90)	0.017 (2.47)	0.023 (2.95)	0.020 (1.88)
time*loghval	0.004 (2.71)	0.005 (2.51)	0.001 (0.69)	0.006 (2.47)			
d7801*loghval						0.100 (3.38)	
d8401*loghval							0.121 (3.10)
d8501*loghval					0.071 (2.76)		
observations	6,915	3,436	3,056	3,859	6,915	3,436	3,859

* Columns (1)–(4) provide estimates of equation (4.5) in the text for the 2-year income-forecasting horizons for various subsamples. Columns (5)–(7) provide estimates of equation (5.1) in the text for the 2-year income-forecasting horizon. All regressions are estimated using data from 1969 to 2001 and include a constant, a set of year effects, and a cubic in age. "Baseline" refers to the entire sample; "Poverty" refers to the SEO sample; "First" refers to the sample that includes only first-time homebuyers; and "Repeat" refers to the sample that includes only repeat homebuyers.
**All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics.

Table 4 – Parameter Estimates for All Samples: Income 4 Years in Future

	INTERACTION WITH TIME-TREND				BREAKPOINTS		
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Base	(6) Poverty	(7) Repeat
log(total family income)	0.389 (6.10)	0.290 (5.70)	0.507 (16.46)	0.327 (3.90)	0.389 (6.09)	0.289 (5.69)	0.326 (3.89)
log(house value)	0.096 (3.30)	0.146 (3.66)	0.102 (2.97)	0.106 (2.46)	0.143 (5.98)	0.109 (3.27)	0.150 (4.29)
age	0.042 (2.51)	0.051 (2.09)	0.013 (0.51)	0.096 (3.82)	0.043 (2.55)	0.052 (2.16)	0.096 (3.82)
high school	0.089 (1.77)	0.089 (1.97)	0.066 (1.46)	0.108 (1.29)	0.086 (1.72)	0.091 (2.04)	0.108 (1.29)
some college	0.171 (3.38)	0.197 (3.90)	0.115 (2.35)	0.202 (2.64)	0.168 (3.34)	0.200 (3.97)	0.200 (2.61)
college	0.267 (4.81)	0.308 (4.14)	0.213 (3.72)	0.299 (3.61)	0.264 (4.80)	0.311 (4.19)	0.294 (3.58)
> college	0.361 (6.34)	0.416 (6.37)	0.288 (4.91)	0.404 (4.95)	0.358 (6.33)	0.419 (6.48)	0.402 (4.95)
black	0.005 (0.14)	-0.111 (3.63)	0.017 (0.42)	0.009 (0.16)	0.005 (0.13)	-0.112 (3.63)	0.003 (0.05)
other race	-0.114 (2.06)	0.002 (0.01)	-0.191 (2.66)	-0.068 (1.08)	-0.118 (2.14)	-0.002 (0.02)	-0.070 (1.04)
female	-0.368 (6.21)	-0.382 (7.95)	-0.298 (3.95)	-0.431 (5.25)	-0.368 (6.22)	-0.382 (7.95)	-0.434 (5.29)
size	0.016 (1.71)	0.020 (1.68)	0.017 (1.32)	0.011 (0.77)	0.015 (1.65)	0.020 (1.72)	0.010 (0.73)
time*loghval	0.006 (3.68)	0.003 (1.23)	0.002 (0.90)	0.007 (3.11)			
d7801*loghval						0.115 (2.95)	
d8301*loghval							0.126 (3.05)
d8501*loghval					0.092 (3.35)		
observations	5,409	2,688	2,384	3,025	5,409	2,688	3,025

* Columns (1)–(4) provide estimates of equation (4.5) in the text for the 4-year income-forecasting horizons for various subsamples. Columns (5)–(7) provide estimates of equation (5.1) in the text for the 4-year income-forecasting horizon. All regressions are estimated using data from 1969 to 2001 and include a constant, a set of year effects, and a cubic in age. "Baseline" refers to the entire sample; "Poverty" refers to the SEO sample; "First" refers to the sample that includes only first-time homebuyers; and "Repeat" refers to the sample that includes only repeat homebuyers.
**All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics.

Table 5 – Parameter Estimates for All Samples: Average of Income 5 Years in Future

	INTERACTION WITH TIME-TREND				BREAK POINTS		
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Base	(6) Poverty	(7) Repeat
log(total family income)	0.438 (5.99)	0.326 (4.72)	0.585 (26.38)	0.374 (4.15)	0.437 (5.96)	0.327 (4.73)	0.373 (4.13)
log(house value)	0.083 (3.52)	0.117 (3.18)	0.077 (2.99)	0.093 (2.82)	0.124 (5.59)	0.150 (5.42)	0.139 (4.71)
age	0.047 (3.93)	0.006 (0.34)	0.029 (1.55)	0.081 (4.86)	0.048 (3.97)	0.007 (0.41)	0.080 (4.85)
high school	0.076 (2.53)	0.045 (1.38)	0.053 (1.49)	0.080 (1.96)	0.075 (2.52)	0.043 (1.33)	0.079 (1.97)
some college	0.151 (4.41)	0.175 (4.48)	0.086 (2.37)	0.181 (4.19)	0.149 (4.42)	0.172 (4.43)	0.178 (4.20)
college	0.247 (6.02)	0.287 (5.76)	0.188 (4.70)	0.270 (5.10)	0.245 (6.04)	0.286 (5.74)	0.268 (5.12)
> college	0.319 (6.30)	0.400 (7.54)	0.245 (5.60)	0.345 (5.51)	0.318 (6.33)	0.397 (7.51)	0.343 (5.53)
black	-0.026 (0.97)	-0.078 (3.69)	0.007 (0.21)	-0.027 (0.71)	-0.027 (0.98)	-0.079 (3.73)	-0.032 (0.85)
other race	-0.110 (2.26)	0.158 (2.49)	-0.151 (1.98)	-0.108 (2.50)	-0.113 (2.35)	0.160 (2.54)	-0.107 (2.49)
female	-0.312 (6.53)	-0.385 (7.73)	-0.223 (5.83)	-0.379 (5.57)	-0.312 (6.53)	-0.386 (7.74)	-0.380 (5.57)
size	0.015 (3.34)	0.023 (3.77)	0.006 (0.92)	0.017 (2.78)	0.014 (3.26)	0.023 (3.72)	0.016 (2.70)
time*loghval	0.005 (3.60)	0.006 (2.71)	0.002 (1.24)	0.006 (3.04)			
d8001*loghval						0.091 (3.68)	
d8301*loghval							0.077 (3.14)
d8401*loghval					0.069 (3.92)		
observations	4,392	2,211	1,831	2,561	4,392	2,211	2,561

* Columns (1)–(4) provide estimates of equation (4.5) in the text for the 5-year average income-forecasting horizons for various subsamples. Columns (5)–(7) provide estimates of equation (5.1) in the text for the 5-year-average income-forecasting horizon. All regressions are estimated using data from 1969 to 2001 and include a constant, a set of year effects, and a cubic in age. "Baseline" refers to the entire sample; "Poverty" refers to the SEO sample; "First" refers to the sample that includes only first-time homebuyers; and "Repeat" refers to the sample that includes only repeat homebuyers.

**All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics

Table 6 - Breakpoints in the Relationship Between House Value and Future Income

	BASELINE	POVERTY	FIRST-TIME HOMEBUYERS	REPEAT HOMEBUYERS
Income 2 Years Ahead	1985***	1981*	None	1983***
observations	6,915	3,436	3,056	3,859
Income 4 Years Ahead	1985***	1978*	None	1984**
observations	5,409	2,688	2,384	3,025
Avg. Income 5 Years Ahead	1984***	1980**	None	1983***
observations	4,392	2,211	1,831	2,561

*** — Significant at the 1 percent significance level
** — Significant at the 5 percent significance level
* — Significant at the 10 percent significance level

* The entry in each cell indicates the year in which there is a breakpoint in the value of α_2, the coefficient on log (home value) in the basic equation (4.4).

Table 7 – Parameter Estimates for All Samples: Income 2 Years in Future

	INTERACTION WITH GSE SECURITIZATION ACTIVITY IN LEVELS				INTERACTION WITH GSE SECURITIZATION ACTIVITY IN FLOWS			
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Baseline	(6) Poverty	(7) First	(8) Repeat
log(total family income)	0.327	0.390	0.379	0.289	0.327	0.390	0.382	0.288
	(5.16)	(5.06)	(4.90)	(3.29)	(5.17)	(5.06)	(4.94)	(3.28)
log(house value)	0.201	0.279	0.078	0.250	0.170	0.263	0.117	0.196
	(3.08)	(3.74)	(0.84)	(2.59)	(3.04)	(4.37)	(1.02)	(2.92)
age	0.053	0.062	0.048	0.060	0.052	0.063	0.046	0.058
	(2.47)	(1.63)	(1.52)	(2.23)	(2.46)	(1.64)	(1.47)	(2.19)
high school	-0.013	0.005	-0.187	0.168	-0.030	0.011	-0.213	0.151
	(0.12)	(0.05)	(2.01)	(0.87)	(0.28)	(0.11)	(2.26)	(0.81)
Some college	0.109	0.195	-0.069	0.283	0.094	0.199	-0.097	0.269
	(0.98)	(1.80)	(0.74)	(1.49)	(0.88)	(1.86)	(1.06)	(1.46)
college	0.230	0.173	0.109	0.375	0.214	0.178	0.081	0.358
	(2.04)	(1.17)	(1.15)	(1.94)	(1.98)	(1.21)	(0.89)	(1.93)
> college	0.331	0.406	0.199	0.489	0.314	0.416	0.165	0.472
	(2.86)	(3.06)	(1.90)	(2.51)	(2.84)	(3.19)	(1.68)	(2.53)
black	-0.013	-0.106	-0.043	0.057	-0.014	-0.106	-0.039	0.052
	(0.28)	(2.33)	(0.68)	(0.97)	(0.30)	(2.34)	(0.62)	(0.87)
other race	-0.144	-0.050	-0.235	-0.044	-0.143	-0.055	-0.227	-0.042
	(1.36)	(0.31)	(1.24)	(0.61)	(1.35)	(0.34)	(1.20)	(0.59)
female	-0.255	-0.317	-0.211	-0.274	-0.255	-0.316	-0.211	-0.272
	(5.02)	(4.41)	(2.99)	(3.67)	(5.01)	(4.42)	(2.97)	(3.67)
size	0.018	0.025	0.016	0.019	0.018	0.025	0.015	0.018
	(1.90)	(1.80)	(1.11)	(1.43)	(1.89)	(1.79)	(1.02)	(1.40)
GSElev*loghval	0.163	-0.152	0.383	0.135				
	(0.80)	(0.52)	(1.32)	(0.44)				
GSEacq*loghval					1.985	-0.486	1.564	2.474
					(1.19)	(0.34)	(0.39)	(1.49)
observations	3,937	1,880	1,573	2,364	3,937	1,880	1,573	2,364

* Columns (1)–(4) provide estimates of equation (6.1) in the text using GSE activity in levels, **GSElev**, for the 2-year income-forecasting horizon for various subsamples. Columns (5)–(8) provide estimates of equation (6.1) in the text using GSE activity in flows, **GSEacq**, for the 2-year income-forecasting horizon. All regressions are estimated using data from 1983 to 2001 and include a constant, a set of year effects, and a cubic in age. For definitions of column headings, see note to Table 5.
** All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics.

Table 8 – Parameter Estimates for All Samples: Income 4 Years in Future

	INTERACTION WITH GSE SECURITIZATION ACTIVITY IN LEVELS				INTERACTION WITH GSE SECURITIZATION ACTIVITY IN FLOWS			
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Baseline	(6) Poverty	(7) First	(8) Repeat
log(total family income)	0.312	0.241	0.478	0.236	0.312	0.241	0.481	0.236
	(4.04)	(4.37)	(10.97)	(2.67)	(4.04)	(4.37)	(11.26)	(2.66)
log(house value)	0.190	0.238	0.058	0.288	0.245	0.140	0.137	0.356
	(3.74)	(4.02)	(0.74)	(4.23)	(3.72)	(2.39)	(1.00)	(5.45)
age	0.085	0.092	0.070	0.133	0.084	0.092	0.069	0.132
	(3.48)	(2.75)	(2.17)	(3.48)	(3.41)	(2.72)	(2.13)	(3.48)
high school	0.173	0.221	-0.010	0.311	0.158	0.231	-0.026	0.313
	(1.05)	(1.18)	(0.11)	(1.15)	(0.97)	(1.25)	(0.27)	(1.17)
Some college	0.245	0.361	0.037	0.363	0.229	0.369	0.018	0.363
	(1.45)	(1.97)	(0.32)	(1.36)	(1.38)	(2.03)	(0.15)	(1.39)
college	0.367	0.586	0.174	0.482	0.351	0.594	0.157	0.484
	(2.20)	(3.08)	(1.58)	(1.82)	(2.13)	(3.14)	(1.42)	(1.86)
> college	0.451	0.654	0.176	0.595	0.435	0.665	0.153	0.597
	(2.68)	(3.30)	(1.26)	(2.28)	(2.61)	(3.41)	(1.13)	(2.33)
black	0.002	-0.128	0.068	-0.020	0.003	-0.129	0.073	-0.022
	(0.03)	(2.66)	(0.88)	(0.27)	(0.04)	(2.70)	(0.92)	(0.30)
other race	-0.150	-0.427	-0.237	-0.104	-0.154	-0.453	-0.239	-0.104
	(2.25)	(1.43)	(2.88)	(1.26)	(2.31)	(1.53)	(2.99)	(1.27)
female	-0.440	-0.398	-0.414	-0.439	-0.441	-0.398	-0.415	-0.440
	(5.01)	(5.38)	(3.27)	(3.86)	(5.01)	(5.43)	(3.24)	(3.88)
size	0.013	0.007	-0.005	0.018	0.012	0.006	-0.006	0.019
	(0.79)	(0.25)	(0.18)	(0.82)	(0.77)	(0.23)	(0.21)	(0.84)
GSElev*loghval	0.261	-0.183	0.329	0.121				
	(1.44)	(0.73)	(1.14)	(0.50)				
GSEacq*loghval					0.051	1.738	-0.259	-1.242
					(0.03)	(1.10)	(0.06)	(0.66)
observations	2,653	1,285	1,020	1,633	2,653	1,285	1,020	1,633

* Columns (1)–(4) provide estimates of equation (6.1) in the text using GSE activity in levels, **GSElev**, for the 4-year income-forecasting horizons for various subsamples. Columns (5)–(8) provide estimates of equation (6.1) in the text using GSE activity in flows, **GSEacq**, for the 4-year income-forecasting horizon. All regressions are estimated using data from 1983 to 2001 and include a constant, a set of year effects, and a cubic in age. For definitions of column headings, see note to Table 5.
** All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics.

Table 9 – Parameter Estimates for All Samples: Average Income 5 Years in Future

	INTERACTION WITH GSE SECURITIZATION ACTIVITY IN LEVELS				INTERACTION WITH GSE SECURITIZATION ACTIVITY IN FLOWS			
	(1) Baseline	(2) Poverty	(3) First	(4) Repeat	(5) Base	(6) Poverty	(7) First	(8) Repeat
log(total family income)	0.350 (3.30)	0.263 (3.19)	0.601 (15.70)	0.274 (2.49)	0.349 (3.29)	0.262 (3.20)	0.601 (15.79)	0.274 (2.49)
log(house value)	0.188 (4.22)	0.260 (4.65)	0.079 (1.69)	0.236 (4.29)	0.157 (3.37)	0.223 (4.28)	0.048 (0.77)	0.201 (3.72)
age	0.071 (3.88)	0.042 (1.46)	0.042 (1.34)	0.106 (4.31)	0.071 (3.91)	0.042 (1.45)	0.040 (1.31)	0.107 (4.34)
high school	-0.013 (0.19)	0.080 (0.70)	-0.073 (0.81)	0.029 (0.27)	-0.024 (0.33)	0.081 (0.71)	-0.081 (0.89)	0.021 (0.20)
some college	0.088 (1.13)	0.248 (2.11)	-0.011 (0.12)	0.142 (1.30)	0.079 (1.04)	0.248 (2.11)	-0.017 (0.19)	0.135 (1.25)
college	0.184 (2.19)	0.386 (3.07)	0.091 (0.94)	0.236 (2.01)	0.175 (2.11)	0.387 (3.08)	0.084 (0.88)	0.229 (1.97)
> college	0.310 (3.16)	0.530 (3.87)	0.175 (1.72)	0.375 (2.88)	0.300 (3.12)	0.529 (3.88)	0.168 (1.67)	0.366 (2.87)
black	-0.012 (0.24)	-0.049 (1.43)	0.104 (1.84)	-0.062 (1.00)	-0.012 (0.23)	-0.049 (1.43)	0.104 (1.82)	-0.059 (0.93)
other race	-0.090 (1.10)	0.105 (0.83)	-0.101 (0.86)	-0.090 (1.70)	-0.092 (1.09)	0.095 (0.73)	-0.103 (0.85)	-0.090* (1.73)
female	-0.328 (4.90)	-0.407 (5.61)	-0.207 (3.44)	-0.372 (4.44)	-0.329 (4.91)	-0.407 (5.61)	-0.207 (3.44)	-0.372 (4.44)
size	-0.001 (0.13)	0.023 (1.91)	0.007 (0.48)	-0.014 (1.32)	-0.001 (0.17)	0.022 (1.88)	0.007 (0.49)	-0.015 (1.36)
GSElev*loghval	0.144 (0.85)	0.000 (0.00)	0.155 (0.65)	0.066 (0.31)				
GSEacq*loghval					1.540 (1.67)	0.973 (0.85)	1.603 (0.89)	1.262 (1.20)
observations	1,825	925	644	1,181	1,825	925	644	1,181

* Columns (1)–(4) provide estimates of equation (6.1) in the text using GSE activity in levels, **GSElev**, for the 5-year-average income-forecasting horizons for various subsamples. Columns (5)–(8) provide estimates of equation (6.1) in the text using GSE activity in flows, **GSEacq**, for the 5-year-average income-forecasting horizon. All regressions are estimated using data from 1983 to 2001 and include a constant, a set of year effects, and a cubic in age. For definitions of column headings, see note to Table 5.

** All regressions are estimated using robust standard errors. Figures in parentheses are t-statistics.