The Effect of Wealth on Labor Force Participation of Older Men

Ryan Goodstein*

University of North Carolina at Chapel Hill†

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Abstract

This paper estimates the effect of wealth on the labor force participation (LFP) of older men. I use an Instrumental Variables (IV) estimation approach that corrects for measurement error in wealth and unobservable taste variation across individuals. Previous studies that do not control for these factors have found that wealth has very little effect on retirement. My IV results reveal a larger wealth effect than in most previous studies; a $20k increase in wealth reduces the probability of LFP by about 1 percentage point. The instruments are local housing price growth and unanticipated inheritances. I cannot reject the hypothesis that the effects of housing and non-housing wealth on LFP are equal, although the power of my test is low. Thus, my analysis suggests that older men are equally willing to "spend" an increase in housing and non-housing wealth on earlier retirement.

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† Department of Economics, 107 Gardner Hall CB #3305, UNC – Chapel Hill, Chapel Hill, NC 27599-3305. E-mail: ryan_goodstein@unc.edu

I. Introduction

The U.S. population will age rapidly over the next two decades as the baby boom generation reaches its elderly years. This graying of the population will adversely affect the fiscal condition of Social Security and Medicare, as the number of workers paying into these programs will decrease relative to the number of beneficiaries. Increasing labor supply at older ages is one way to improve the viability of these programs, so it is important to understand the key influences on the timing of retirement.

Many studies have examined the impact of Social Security, private pensions, and employer provided retiree health insurance on retirement. However, a number of these studies omit a potentially important factor – wealth – from their analyses of retirement behavior. For example, the influential studies by Berkovec and Stern (1991), Rust and Phelan (1997), and Stock and Wise (1990) do not incorporate wealth in their structural models of retirement[1].

Wealth at retirement age varies considerably, both across households and over time. Median real wealth of households aged 55 to 64 increased by 75% between 1989 and 2004 (Kennickell et al., 1997; Bucks et al., 2006). Many older households saw their net worth rise and fall substantially as a result of the stock market boom and bust between 1995 and 2002; more recently the booming housing market has driven up net worth for many homeowners[2]. Moreover, as employers continue to shift toward offering Defined Contribution (DC) pension plans in place of Defined Benefit (DB) pension plans, private pension benefits are increasingly

[1] These studies justify the omission of wealth for at least two reasons: (1) data on net worth is either unavailable or measured with serious error, and (2) net worth of a typical household in their sample is small, particularly if housing wealth is ignored. See Rust and Phelan (1997, p. 797) or Stock and Wise (1990, p. 1158) for a more complete discussion of why net worth is omitted from their models of retirement behavior.

[2] Cheng and French (2000) estimate that gains in the stock market increased average wealth in the U.S. by 17% between 1995 and 1999. I estimate that the housing price boom increased average wealth by 13% in 2004 among older men born between 1931 and 1941.

received as a lump-sum rather than as a Social-Security-like lifetime annuity[3]. As a result, older households will become increasingly reliant on their wealth to finance consumption in retirement. The effect of wealth on labor supply at older ages is thus an interesting and important issue.

It seems reasonable to suppose that leisure is a normal good, so an increase in wealth should increase demand for leisure. One way to increase leisure is to retire earlier. In practice, most studies have found that the wealth effect on retirement is very small and statistically insignificant (e.g. Diamond and Hausman, 1984; Blau, 1994; Samwick, 1998). However, these estimates may be biased toward zero for two reasons. First, wealth is believed to be measured with considerable error in survey data (Juster et al., 1999). Second, there is likely a positive cross-sectional correlation between wealth and retirement due to unobserved taste variation (Hurd and Reti, 2001)[4]. For example, an individual with relatively greater tastes for work or a higher degree of risk-aversion may accumulate more wealth and retire later than his peers, imparting a spurious positive correlation between wealth and LFP that will tend to offset a "true" negative effect.

In this paper I use an Instrumental Variables (IV) estimation approach to estimate the effect of wealth on retirement. I rely on two sources of variation to identify the wealth effect. The first is geographic variation in the rate of housing price changes. This variation has been particularly sharp over the past decade, as prices have boomed in some markets while remaining

[3] A DB pension plan typically pays out an annuitized amount as a function of age and job tenure at the date of retirement, and earnings in the years prior to retirement. In a DC plan pension wealth accumulates as a function of employer and employee contributions and the returns on those contributions. Few individuals elect to convert their DC pension balances into an annuity when they retire (Johnson et al. 2004)

[4] Blau (1994) and Samwick (1998) use longitudinal data, so they exploit variation in wealth both across respondents and over time. But changes in wealth over time are measured with even more error than the level of wealth (Juster et al., 1999), and there is likely persistence among respondents in the relative position in the wealth distribution, so most of the effective variation is cross-sectional.

flat in others[5]. Further, this variation is likely to be strongly associated with variation in household wealth. Over 80% of households with a head aged 65 and older own a home, and residential real estate represents the single largest asset class held by this group (Agpar and Di, 2005). I control for local economic conditions in the analysis; thus the remaining variation in the growth of local housing prices can be attributed mainly to location-specific demographic and geographic factors (e.g. population growth on a fixed supply of land), which I assume is exogenous with respect to the retirement decision.

The second source of identifying variation is unanticipated inheritances. Receiving an inheritance is common in the age range of retirement: one in five households with an individual aged 51 to 61 in 1992 received an inheritance between 1994 and 2002, with a median value of $30,000 (Brown et al., 2006). I use self-reported inheritance expectations to control for variation in anticipated inheritances. An inheritance that was anticipated by the recipient is unlikely to provide an exogenous source of variation in wealth, because retirement decisions may be affected prior to receiving the inheritance. I assume that the remaining variation in inheritance after controlling for expectations is unanticipated and therefore an exogenous shock to wealth.

This study provides two main contributions to the economic literature on retirement. First, this is the first analysis to use an IV approach to estimate the effect of wealth on retirement. The IV approach corrects for both measurement error and endogeneity of wealth. Second, I examine whether the effect of housing wealth on retirement is different than the effect of non-housing wealth. There is some debate among economists about the willingness of the elderly to use housing wealth to finance consumption in retirement (Scholz et al., 2006)[6]. Evidence from

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[5] The real growth rate of housing prices ranges from less than 20% (e.g. Dallas, TX; Pittsburgh, PA) to nearly 100% (e.g. Boston, MA; San Diego, CA) over the period 1994 to 2004 (source: author's calculations based on House Price Index data published by the Office of Federal Housing Enterprise Oversight).

[6] This question has implications for a related and controversial issue: are people saving enough for retirement? The

Venti and Wise (2004) suggests that most elderly households do not spend down housing wealth in retirement. If older men are also unwilling to consume housing wealth in the form of leisure, the effect of housing wealth on retirement should be smaller than the effect of non-housing wealth.

I estimate the model using panel data from the Health and Retirement Study (HRS) on men born between 1931 and 1941, observed over the years 1996 to 2004. My results indicate that an increase in wealth leads to earlier withdrawal from the labor force, consistent with the assumption that leisure is a normal good. The IV estimate is that a $20k increase in wealth, which would increase the wealth of a household at the median level of wealth by 19%, reduces the probability of participating in the labor force by about 1 percentage point. As expected, OLS estimates of the wealth effect are biased toward zero. These findings are robust to alternative definitions of retirement and to a variety of specification checks.

I use my estimates to simulate the impact of the extraordinary gains in housing prices since 1995 on the labor force participation (LFP) of older men. Results from a counterfactual simulation in which the housing price appreciation rate remained at its pre-1995 level imply that the LFP rate of men born between 1931 and 1941 in the US would have been 1.3 percentage points higher in 2004 had housing prices appreciated at the normal rate.

The second main finding is that the null hypothesis that housing and non-housing wealth have the same effect on LFP cannot be rejected. This result is consistent with the view of Engen et al. (2000), who argue that a substantial portion of home equity should be considered available to finance consumption in retirement. However, the IV estimates of separate housing and non-

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adequacy of saving for retirement depends crucially on whether one believes that housing wealth qualifies as "retirement savings". Venti and Wise (2004) conclude that home equity should not be counted on to finance non-housing consumption in retirement, while Engen et al. (2000) argue that at least a substantial portion of home equity should be considered retirement savings.

housing wealth effects are not very precise, so the evidence on this point is somewhat weak.

The remainder of this paper is organized as follows: Section II reviews the existing relevant literature, Section III describes the data, Section IV introduces the empirical specification used in the analysis, Section V discusses the results, and Section VI concludes.

II. Background

As noted above, studies that that treat household wealth as exogenous find that net worth has little effect on retirement. These papers typically do not focus on producing an unbiased estimate of the wealth effect; rather they simply include a control for wealth when estimating a model of retirement. For example, Blau (1994) studies quarterly transition rates in and out of full-time and part-time work among older men in the Retirement History Survey. His estimates imply that an increase in wealth from $0 to $100,000 would reduce the transition rate from full-time work to out of the labor force by 9% among older men, a counterintuitive result. Samwick (1998) examines the effect of pensions and social security incentives on the timing of retirement among older men and women in the Surveys of Consumer Finance. His results imply that financial (i.e. non-housing wealth) has essentially zero effect on entry into retirement, while an increase in housing wealth is associated with a lower rate of entry into retirement.

Recognizing that wealth is likely to be endogenous with respect to retirement, recent studies have examined the effect of exogenous shocks to wealth on the timing of retirement. Imbens et al. (2001) examine the effect of lottery winnings on labor earnings among a sample of lottery players. They report that earnings of men ages 55 to 65 decline by 16 cents for every dollar increase in the annual payout from winning a lottery. This implies that a $20k increase in wealth would reduce the probability of LFP by roughly 1.5 percentage points[7]. A number of papers have examined the effect of the 1995 – 2002 stock market boom and bust on retirement behavior. Gustman and Steinmeier (2002) estimate that the rate of full-time work among men in the HRS was 3.3 percentage points lower in 1999 than it would have been had the boom not occurred. Cheng and French (2002) reach a similar conclusion – they estimate that the LFP rate of men aged 55 to 64 was 3.2 percentage points lower in 1999 due to gains in the stock market. Coronado and Perozek (2003) find that HRS respondents who owned stocks immediately prior to the boom retired 7 months earlier on average than those who did not. However, Hurd and Reti (2001) and Coile and Levine (2006) both find that variation in stock market wealth had zero impact on aggregate retirement patterns.

My analysis builds on the work of two recent papers within this strand of the literature. Farnham and Sevak (2006) were the first to examine whether changes in housing prices have an effect on retirement behavior. The authors estimate a reduced form model of retirement in which the growth rate of local area housing prices is the key explanatory variable. Their results indicate that a nominal 10% increase in the local price of housing since 1992 is associated with a 6% increase in the annual rate of transition into self-assessed retirement over the period 1992 to 2002. The second paper, by Brown et al. (2006), was the first to examine the effect of receiving an inheritance on retirement[8]. The authors specify a model in which the probability of retiring earlier than expected (based on self-assessed retirement status) is regressed on anticipated and

[7] Lottery winnings are paid out over a 20 year period for their sample. The 20 year annuitized value of a \$20k increase in wealth is approximately \$1.5k annually. Under the assumption that mean annual earnings of men ages 55 to 65 are equal to the sample mean of \$16.1k, and that a man can choose whether or not to work but cannot adjust his hours of work, the implied LFP response from a \$20k increase in wealth is equal to (.16)*(\$1.5k)/(\$16.1k) = 1.5 percentage points.

[8] Two earlier papers examine the effect of inheritances on labor supply, although each focuses on the non-elderly population in the U.S. Holtz-Eakin et al. (1993) find that among households ages 20 to 58, inheritance recipients are substantially more likely to exit the labor force than non-recipients. Joulfaian and Wilhelm (1994) also find that inheritances lead to reductions in labor supply, although the effects are smaller and somewhat sensitive to specification.

unanticipated inheritances. The strength of this approach is that the inclusion of retirement and inheritance expectations accounts for unobservable individual heterogeneity, so that the remaining variation in inheritances is plausibly exogenous. Their results indicate that an anticipated inheritance of $100k leads to a 10% increase in the probability of retiring earlier than expected among HRS respondents over the period 1994 to 2002, and the effect for an unanticipated inheritance is over twice as large.

In summary, studies that rely on cross-sectional variation in wealth are likely to yield estimates of the wealth effect that are biased toward zero. There are few studies that use plausibly exogenous variation in wealth to estimate the wealth effect on labor supply at older ages, and those that do rely on variation in lottery winnings and stock market returns that may not be relevant for most households. Farnham and Sevak (2006) and Brown et al. (2006) investigate the effect of local area housing prices and unanticipated inheritances on the timing of retirement, but do not directly estimate the effect of wealth on labor supply at older ages. This paper is the first to use an IV approach to estimate the effect of wealth on retirement. I use local area housing prices and unanticipated inheritances to identify the wealth effect[9]. If these instruments affect the retirement decision only through their impact on wealth, this estimation approach yields a consistent estimate of the wealth effect.

III. Data

The analysis in this paper is based primarily on data from the Health and Retirement Study (HRS). These data contain an abundance of information on demographic characteristics, health, labor supply, income, and wealth for a sample of individuals in the United States born

[9] Hurst and Lusardi (2004) also used inheritances and regional house price variation as instruments for wealth in a model of entry into business ownership.

between 1931 and 1941 and their spouses. Respondents have been interviewed biannually since 1992, and data through 2004 are currently available to the public. An innovative feature of the HRS is that survey respondents are asked a number of questions on their expectations of future events, including future inheritances. Data on expected inheritances are available in years 1994 and later. As the empirical specification in this paper (described in detail in the next section) models retirement in part as a function of inheritance expectations from the previous wave, the analysis is based on HRS data from years 1996 – 2004 (and lagged values of inheritance expectations from years 1994 – 2002).

In addition to HRS data, I use the House Price Index (HPI) published by the Office of Federal Housing Enterprise Oversight to measure local housing price changes. Local areas are defined by Core Based Statistical Areas (CBSA) determined by the U.S. Office of Management and Budget. Restricted geographic identifiers are used to match local area HPI data to each respondent record in the HRS[10]. The HPI measures the growth of single family house prices based on repeated mortgage transactions for individual homes[11]. The HPI is published quarterly; I annualize the data by averaging the quarterly values for each year. I also adjust the HPI for inflation using the national Consumer Price Index series less shelter for all urban consumers. Failure to adjust for inflation would result in overstated rates of housing price growth relative to the real value of money. I compute the 10 year percentage change in HPI for each year and CBSA, and use this measure as an instrument for wealth in the analysis. The choice of a 10 year window is arbitrary, but this is the longest window over which changes in HPI can be computed

[10] Restricted HRS geographic data includes the FIPS state and county code, which can be matched to HPI data published at the CBSA level. CBSA definitions replaced the old Metropolitan Statistical Area (MSA) definitions effective in June 2003.

[11] The OFHEO uses repeated observations of housing values on single family residential properties on which at least two mortgages were originated and subsequently purchased by either Fannie Mae or Freddie Mac since January 1975. The use of repeat transactions on the same physical property helps to control for differences in the quality of of the houses within a geographic area over time. See Calhoun (1996) for more details.

for most CBSAs in the sample[12]. Housing price appreciation over the long run is likely to be a more powerful predictor of wealth than short run (year-to-year) changes since household mobility at older ages is low. As of 1995, 76% of homeowners aged 65 or older resided in their present homes for 10 years or more (Citro, 1998).

The analysis is limited to the retirement behavior of men because many women in this cohort were not career participants in the labor force. Those who are self-employed in any survey are omitted from the sample; self-employed men may be quite different than employees in their tastes for work and in the retirement incentives they face. There are 2,932 men in the HRS born between 1931 and 1941 who were not self-employed, resulting in 12,728 person-year observations for years 1996 through 2004. 11% of observations are dropped due to incomplete or missing data[13]. Of the remaining 11,296 observations, 29% reside in areas (mostly rural) in which the 10 year change in HPI is unavailable. These records are also dropped. The final analysis sample includes 8,021 person-year records.

Descriptive statistics are presented in Table 1. In this study, I focus on LFP at the survey date as the main outcome of interest. This is an objective measure of retirement that is widely used in the literature. I also examine whether the findings are robust to use of alternative measures of retirement: full-time work and self-assessed retirement status[14]. Personal characteristics of each respondent included in the analysis are age and indicators for educational attainment (no high school degree; high school degree; some college; college degree), ethnicity

[12] HPI data are not available for years prior to the mid 1980s for many CBSAs.
[13] Observations with missing or incomplete housing and stock market wealth data comprise the great majority of these drops (1,421 observations); only observations where an exact amount is reported or the series of bracketed questions used to determine the amount of housing or stock market wealth is completed are kept. An additional 3 observations are deemed outliers and are dropped (2 with total wealth in excess of 10 million; 1 record with non-housing wealth over 7.5 million). Finally, an additional 3 records are dropped due to missing health status, and 5 are dropped due to a missing local unemployment rate.
[14] I define full-time work as 35 hours or more per week and 36 weeks or more per year. If the man reports that he is either fully or partially retired, I define his retirement status as "retired". Otherwise he is "not retired".

(Hispanic; non-Hispanic), race (black; non-black), marital status (married; non-married), and bad health (equal to 1 if self assessed health status is fair or poor, 0 otherwise).

I control for local economic conditions because they may influence both employment opportunities for older men and housing prices. I include the local unemployment rate, local per capita personal income, and the share of local area jobs that are blue collar in the analysis. The latter measure reflects the likelihood that physical limitations limit work opportunities in the area for older men, and may also be correlated with prospects for future economic growth in the area[15].

Table 1 also summarizes variables that are used to control for inheritance expectations. In each wave of the HRS in years 1994 and later, respondents are asked a series of questions on their inheritance expectations. I use lagged values of inheritance expectations to ensure that the expectations were formed prior to receiving any inheritances since the past survey. The inheritance expectation questions are not answered in 15% of the responses; in these cases a missing value indicator is set to 1 and all other inheritance expectation measures are set to 0. Respondents who answer the inheritance expectation questions are first asked to rate their chances (from 0 to 10) of receiving an inheritance within the next 10 years. I convert these to a probability measure on the unit interval; 29% of respondents report a positive probability of receiving an inheritance, and the mean expected probability of receiving an inheritance is 16%. Respondents who report a positive probability are asked how large the inheritance is expected to be; this value is the variable Expected Inheritance Amount[16]. If the respondent expects an

[15] Local area unemployment rates are published by the Bureau of Labor Statistics. Local per capita income data are published by the Bureau of Economic Analysis. Share of local area jobs that are blue collar is computed from CPS data. I include men between the ages of 35 to 55 only in the computation to reduce self-selection bias due to withdrawal from the labor force. In cases where I observe 25 or less records in a CBSA I use state-level data to compute the share of manual workers.

[16] If the respondent does not provide an exact amount for his expected inheritance amount then the value is assigned based on his answers to the series of bracketed responses. In this case I set the expected inheritance amount equal to

inheritance but does not report an expected amount then Expected Inheritance Amount is set to 0 and a missing value indicator is set to 1. Conditional on expecting an inheritance, the average Expected Inheritance Amount was $68,300.

Information about the respondent's parents is also useful for predicting his inheritances. An indicator equal to 1 if the mother did not complete high school (and 0 otherwise) and a count of living parents (0, 1, or 2) as of the previous wave are included in the analysis. If the respondent was married in the previous wave then these variables are also included for the spouse's parents; otherwise these variables are set to zero.

Table 2 summarizes the distribution of wealth. Total Wealth is defined as the sum of Housing and Non-Housing Wealth. Non-Housing wealth consists of stock market wealth, vehicles, private businesses, IRA and Keough accounts, checking and savings accounts, CDs, government bonds and T-bills, bonds and bond funds, other real estate, and "other", minus non-housing debt. The mean value of Total Wealth (in 1992 dollars) is $210k, while the median value is $108k. The distribution of wealth is heavily skewed to the right. This is primarily due to the distribution of non-housing wealth; the mean value is $134k while the median is just $36k. Housing wealth is not as concentrated as non-housing wealth; the difference between the mean ($76k) and the median ($58k) is much smaller.

Table 2 also summarizes the distributions of the variables used as instruments for wealth. The mean 10 year growth rate of the real housing price over the entire sample is 15%. Appreciation rates vary substantially across local areas and over time; the 10 year growth rate is -13% at the 10^{th} percentile and 51% at the 90^{th} percentile. 3.5% of respondents received an inheritance within the past 2 years (i.e. since the previous survey); conditional on receiving an inheritance, the median value is $30k and the mean is $82k. 15% of respondents received at

the midpoint of the bracket.

least one inheritance since 1994, with a median cumulative value of $27k.

Figure 1 presents the labor force participation rate by age for the sample. Most of the "action" in retirement behavior encompasses the ages included in this survey. The LFP rate drops from nearly 90% at age 55 to 20% at ages 70 and over. Labor supply seems to drop most sharply at ages 61 through 65; these are the ages at which most men decide to retire. The sharp decline from age 54 to age 55 is unexpected; this may be due to a small sample size of men at age 54. Age patterns of the alternative retirement outcomes (Full-time work and self-assessed retirement) are quite similar.

IV. Empirical Model and Identification of the Wealth Effect

The conceptual framework for this analysis is a standard life cycle model[17]. In each period a man chooses consumption of goods and leisure to maximize expected utility over his lifetime. These choices are restricted by an intertemporal budget constraint: consumption of goods in the current period reduces savings that can be used to finance consumption in future periods, while consumption of leisure reduces earnings that can be used to finance consumption in the current and future periods. Utility is derived from consumption of goods and leisure, and preferences may depend on individual characteristics including age, health, education, and marital status.

The labor force participation choice in period t is made by comparing $V_{1t}(S_t)$ and $V_{0t}(S_t)$, where $V_{1t}(S_t)$ and $V_{0t}(S_t)$ are the value functions associated with participation and non-participation, respectively, conditional on the optimal level of consumption in each case and the optimal level of hours of work in the case of participation. S_t is the vector of state variables that characterizes the man's situation in period t. S_t includes wealth, the wage offer, retirement

[17] See Browning and Lusardi (1996) for a discussion of the life cycle model.

income in the event of non-participation in the labor force (e.g. Social Security and pension benefits), and personal characteristics (e.g. age, health, marital status). The man will choose to participate in the labor force if $V_{1t}(S_t)$ is greater than $V_{0t}(S_t)$.

A. Empirical Model

The empirical model used in this paper can be interpreted as a linear approximation to the LFP decision rule. In this specification, man *i*'s participation choice in period *t* is:

$$P_{it} = \beta_0 + \beta_1 W_{it} + \beta_2 a_{it} + \beta_3 t + \beta_4 X_{it} + \beta_5 Y_{it} + \beta_6 Z_{it-1} + \varepsilon_{it}$$

where

P_{it} = 1 if the man chooses to participate in the labor force in period t, and 0 otherwise

W_{it} is net worth

a_{it} is a vector of age fixed effects

t is a vector of year fixed effects

X_{it} is a vector of personal characteristics (education, race, ethnicity, marital status, and health status)

Y_{it} is a vector of variables that characterize economic conditions in man *i*'s local area as of the current period

Z_{it-1} is a vector of variables that characterizes expectations as of t-1 of receiving an inheritance between period *t-1* and *t*

The man's preferences for retirement are a function of his age (a_{it}) and other personal characteristics in X_{it}. The retirement decision may also depend in part on economic conditions in a given year. The vector of year fixed effects t controls for cyclical variation in national economic conditions, and Y_{it} controls for variation in local labor market conditions over time. I include lagged inheritance expectations (Z_{it-1}) in the specification to control for variation across individuals in their subjective probability distribution of receiving an inheritance.

I estimate two versions of the empirical model that differ by the specification of wealth. In the first specification housing and non-housing wealth are aggregated into a single measure of total net worth (W_{it}); this is consistent with a standard life cycle model in which there is no distinction among different types of assets. However, housing is more illiquid than other assets, and housing wealth must be converted to a liquid asset before being consumed. This may be done by selling the home and moving to a new residence, or by borrowing against the home. Venti and Wise (2004) find that it is uncommon for a homeowner to consume housing wealth by selling his home and moving to a new residence, likely due to financial and emotional transactions costs of doing so. It is more common for older households to access their housing wealth by borrowing against the home, although finance costs associated with doing so may not be trivial[18]. Thus, I estimate a second empirical specification in which housing wealth and non-housing wealth enter separately, relaxing the restriction that their effects are equal. In this case, β_1 consists of two parameters: β_1^H (the effect of housing wealth on retirement); and β_1^N (the effect of non-housing wealth on retirement

I omit the man's wage offer from the model, because wages are observed only for men who choose to work. Age, personal characteristics, and economic conditions will partially control for the wage rate, but the even after controlling for these determinants the wage rate is likely to be correlated with wealth. Thus, the parameter β_1 must be interpreted as the effect of wealth on retirement *not holding* the wage rate constant. This approach is consistent with other

[18] Most households access their housing equity either through a home refinancing or a home equity line. Nearly 60 percent of men in the HRS sample hold some form of debt against their home, and between 15 and 20 percent carry a balance on a home equity line of credit (Source: author's calculations from HRS data). Borrowing against the home via a reverse mortgage is not common, possibly due to high transactions costs of doing so (Caplin, 2000).

papers in the literature[19]. In practice, the impact of not holding wages constant on my estimate of the wealth effect may be small. Blau (1994) finds that the effect of the wage rate on labor force exits is small and not significantly different from zero when individual characteristics are controlled for in the specification, while Samwick (1998) finds that the effect of wages is sensitive to specification. If I limit my sample to individuals who worked in the previous period (wages are observable for this sub-sample), I find that the estimated wealth effect on the probability of LFP in the current period changes little when the wage rate is added to the specification.

B. Identification of the Wealth Effect

OLS estimates of the empirical model are unbiased if the error term ε_{it} is orthogonal to the explanatory variables. However, as noted earlier wealth is likely to be positively correlated with ε_{it} due to measurement error and unobservable heterogeneity across individuals. Thus estimation of the model using OLS yields an estimate of β_1 that may be biased toward zero. In this paper I use a 2 Stage Least Squares (2SLS) estimation approach in which wealth is instrumented with (1) the 10 year growth rate in the local housing market, and (2) unanticipated inheritances. The key assumption is that the instruments are correlated with wealth, but do not affect the retirement decision through any other channel.

Variation in housing price growth rates across localities is likely to be a valid instrument for wealth if growth in housing prices is due to mainly to location-specific demographic and geographic factors, i.e. population growth on a fixed supply of land. However, if growth of local area housing prices is driven by economic factors, then the identification assumption may be

[19] For example, Imbens et al. (2001) do not control for the wage in their study of the effect of lottery winnings on labor earnings. Coronado and Perozek (2003) do not control for the wage rate in their study of stock market wealth effects on the timing of retirement. Lehnert (2004) and Morris (2006) control for permanent income but not the wage rate in their studies of housing wealth effects on consumption of goods and services.

violated. I control for local area economic conditions in the empirical analysis with Y_{it}, which includes CBSA-year specific measures of the local unemployment rate, per-capita income, and percentage of the local labor force that works in manual labor[20]. However, if these controls do not capture all factors that are correlated with local area housing prices and directly affect the influence the retirement decision, the use of HPI as an instrument may be suspect.

To check the validity of local housing price growth as an instrument for wealth, I estimate reduced form models separately by home ownership status to examine the correlation between HPI and wealth, and between HPI and LFP. HPI and wealth should be more weakly correlated for non-homeowners relative to homeowners, because non-homeowners do not receive the direct financial windfall that homeowners do when housing prices increase. The estimates presented in Table 3 are consistent with this hypothesis. Each entry in columns 1 to 3 is the coefficient on HPI from a separate wealth regression. The estimate in row 1, column 1 indicates that a 0.1 increase in HPI (i.e. a 10% increase in the 10 year growth rate of local housing prices) is associated with an increase in total wealth of $12,780 among homeowners. Coefficient estimates in columns 2 and 3 decompose this effect: $5,940 of the estimated increase is in housing wealth and $6,840 is in non-housing wealth. In contrast, HPI has little effect on the wealth of non-homeowners. A 0.1 increase in HPI implies a $2,310 decrease in net worth among non-homeowners, and this estimate is not significantly different from zero[21].

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[20] I explored alternative specifications that included additional measures of CBSA-level economic activity, including educational distribution and state level GDP, and 10-year growth rates of each. The additional measures added little information, likely because measures of economic activity are highly collinear. I also estimated a model that included CBSA fixed effects. The inclusion of CBSA fixed effects is attractive because they control for long run differences in the level of economic activity across CBSAs. However, the inclusion of CBSA dummies absorbs variation across CBSAs in the average rate of 10 year housing price growth. Thus, only time series variation around the trend in housing price growth within each CBSA is left to identify the wealth effect. In practice, this variation is insufficient to identify the wealth effect. When CBSA fixed effects are added to the specification, 2SLS estimates of the wealth effect where wealth is instrumented with HPI become quite small in magnitude and statistically insignificant.

[21] All significance tests discussed in the text are at the 5% level.

These estimates suggest that, to a first approximation, the HPI is uncorrelated with the wealth of non-homeowners. Thus if the HPI is correlated with the LFP choice of non-homeowners then it is likely that unobservable factors correlated with HPI (such as local economic conditions that are not controlled for in the specification) impact the retirement decision directly and the use of HPI as an instrument is suspect. This is not the case – the results in the 4th column of Table 3 indicate that an increase in HPI has essentially zero effect on LFP among non-homeowners. Therefore I conclude that HPI is not picking up the effects of omitted local economic conditions that affect non-homeowners as well as homeowners.

The second instrument for wealth in this analysis is inheritances. An anticipated inheritance is likely to violate the identification assumption, because labor supply behavior may be affected before the inheritance is actually received. Conversely, an unanticipated inheritance is likely to be an exogenous shock to wealth. In this analysis, I include subjective inheritance expectations and parent's characteristics that may be correlated with inheritances directly in the empirical specification. By controlling for observed variables that help predict inheritances, using inheritances as an identifying instrument is equivalent to using only the part of the inheritance that is uncorrelated with the predictors[22]. This remaining variation in inheritances is assumed to be uncorrelated with the error term.

In order for the instruments to be valid, they must also be sufficiently correlated with wealth. Table 4 presents selected results from the first stage of the 2SLS estimation procedure. Each column reports coefficient estimates from separate regressions which vary by the

[22] Brown et al. (2006) present descriptive analysis which indicates that the subjective inheritance expectations help predict inheritance receipt; both the incidence of receiving an inheritance and the size of the inheritance rises with the expected probability of receipt. I estimated a reduced form model in which inheritance receipts received in the last 2 years are regressed on subjective inheritance expectations data as well as parent's (and spouse's parent's if applicable) characteristics that may affect inheritance expectations. The model also includes age and year dummies, personal characteristics, and local area economic conditions. The R-squared is .08, and nearly all of the explanatory power can be attributed to the man's subjective inheritance expectations and the characteristics of his (and his spouse's) parents. The R-squared from a model that omits these variables is less than .01.

identifying instruments for wealth. In column 1, the identifying instrument is the HPI. The F statistic on a test of the null hypothesis that the HPI can be omitted from the first stage equation is 13.36, which is above the "rule of thumb" minimum value of 10 for a sufficiently powerful instrument (Staiger and Stock, 1997). In column 2 the identifying instrument is inheritances received over the past two years. The coefficient estimate is not significantly different from zero and the F test statistic of 3.50 implies that inheritances received over the past two years is a weak instrument for total wealth. A potentially stronger instrument for wealth is the cumulative value of inheritances received, as inheritances received more than two years before the survey date are likely to be positively correlated with wealth in the current period. The results in column 3 indicate that this is the case. In this specification, total wealth is instrumented with cumulative inheritances receipts since 1994, and the F test statistic of 10.32 indicates that cumulative inheritances are a sufficiently powerful instrument for wealth in the first stage.

However, use of cumulative inheritances as an instrument may be invalid, as an instrument for total wealth may affect the LFP decision through a channel other than wealth. For example, the past inheritance may have affected past labor supply decisions, which in turn could affect the current labor supply decision. In the next section I present 2SLS estimates of the wealth effect for each alternative measure of inheritances. Specifically, I instrument for wealth using (a) HPI and inheritances received in the last 2 years, or (b) HPI and cumulative inheritances received since 1994. The first stage estimates from each of these specifications are presented in columns 4 and 5 of Table 4. In both cases the F statistic suggests these variables are sufficiently powerful instruments for wealth[23].

[23] Subjective inheritance expectations as of 1994 are also included in the specification in columns 3 and 5. These measures capture, as of 1994, each respondent's inheritance expectations over the next 10 years (i.e. through 2004). I assume that the 1994 inheritance expectations together with lagged (t-1) inheritance expectations adequately

V. Results

A. Estimates of the Wealth Effect on LFP

I first estimate the empirical model using a measure of total net worth as the key explanatory variable. I estimate the model under four alternative assumptions about identification: (1) Wealth is exogenous; (2) Wealth is endogenous and the 10 year growth rate in local housing prices is a valid identifying instrument; (3) Wealth is endogenous and unanticipated inheritances is a valid identifying instrument; and (4) Wealth is endogenous and both instruments are valid.

Estimates of the wealth effect on LFP under each alternative assumption are presented in Table 5[24]. In panel (a) the instruments for wealth are HPI and inheritances received in the past two years. The OLS estimate of the wealth effect on LFP is negative, small, and precisely estimated. A $100k increase in total wealth, which would nearly double the assets held by a man at the median level of total wealth, decreases the probability of participating in the labor force by 1.5 percentage points. The 2SLS coefficient estimates of the wealth effect on LFP are two to four times as large in absolute value as the OLS estimate, suggesting that the OLS estimate is biased toward zero. As noted earlier, this is likely due to endogeneity of wealth and measurement error in wealth. However, the magnitude of the wealth effect on LFP implied by the 2SLS estimates is somewhat sensitive to the identification assumption. In particular, the estimate in specification 2 is nearly twice as large as the estimate in specification 3, and neither is significantly different from zero[25].

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control for variation in the cumulative value of inheritances that was anticipated by the respondent, and remaining variation in cumulative inheritance receipts is unanticipated.

[24] I also estimated the model using Probit and instrumental variables Probit, and found the marginal effects to be very similar.

[25] Recall from Table 3 that the wealth level of homeowners is correlated with housing appreciation rates, while the wealth of non-homeowners is not. Accordingly, the IV estimation approach in which wealth is instrumented with HPI can be interpreted as the Local Average Treatment Effect (LATE) of wealth on LFP of homeowners (see

In panel (b) the instruments for wealth are HPI and cumulative inheritances received since 1994. The p-value of 0.758 from an over-identification test indicates that these are valid instruments for wealth. The over-identification test checks if the identifying instruments are correctly excluded from the second stage equation, i.e. that the instruments are uncorrelated with LFP through any channel other than wealth. Using these instruments, the 2SLS point estimates of the wealth effect are similar across each alternative identification assumption. The estimates of the wealth effect range from -.044 to -.056, and the estimates are significantly different from zero in two of the three cases. The most precisely estimated 2SLS coefficient on wealth (from column 4) indicates that a $100k increase in wealth reduces LFP by 4.5 percentage points.

Table 6 presents estimates of the specification in which the effects of housing wealth and non-housing wealth on LFP are allowed to differ. I find no evidence that the housing wealth effect is smaller than the non-housing wealth effect. Under the assumption that wealth is exogenous, OLS estimates imply that the housing wealth effect is *larger* than the non-housing wealth effect by a factor of three. The coefficient estimates are each significantly different from zero, and the difference between the estimated wealth effects is significantly different from zero at the 5% level (p-value = .0449). In the 2SLS estimation approach, housing and non-housing wealth are instrumented with HPI and the cumulative value of inheritances received since 1994. The 2SLS point estimates again suggest that the effect of housing wealth on retirement is larger than the effect of non-housing wealth. This is a surprising result. It is not obvious why the LFP decision might be more sensitive to housing wealth than other forms of wealth, particularly given the relative illiquidity of housing wealth as noted earlier[26]. However, the precision of the 2SLS

Imbens and Angrist (1994) for more on LATE).

[26] Others have found that housing wealth effects may be larger than non-housing wealth effects on consumption of goods and services. Bostic et al. (2005), Morris (2006), and Carrol et al. (2006) each find that the marginal propensity to consume (MPC) from housing wealth is larger than the MPC from non-housing wealth. Juster et al.

estimates is poor – neither estimate is significantly different from zero. In every case I fail to reject the null hypothesis that the wealth effects are equal. Thus in the remainder of this paper I focus on results from the model in which the key explanatory variable is total wealth – the assumption that all forms of wealth have the same effect on retirement does not appear to be too restrictive.

The full set of OLS coefficient estimates is presented Appendix Table 1. The estimates on personal characteristics, local area economic conditions, and inheritance expectations are generally of the expected sign, so I discuss them only briefly here. Age and health effects are large and significantly different from zero; the probability of participating is substantially lower at older ages and for those in bad health. Characteristics that are typical of low-wage earners (e.g. lower educational attainment, not married, black) are associated with lower LFP[27]. The estimated effects of local economic conditions are intuitive – the probability of participating in the labor force is decreasing in the unemployment rate and the share of blue-collar jobs, and increasing in per-capita personal income. An increase in the expected probability of receiving an inheritance is associated with a lower probability of participating, although the estimate is not significantly different from zero.

B. Discussion

Overall my estimates suggest that wealth has a modest effect on LFP rates of older men. A $20k increase in wealth, which amounts to a 19% increase in wealth for a man at the median level of wealth, reduces the probability of LFP by about 1 percentage point. This estimate is larger in magnitude than those from previous studies (e.g. Diamond and Hausman, 1984; Blau,

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(2005) reach the opposite conclusion. Bostic et al. (2005, p.18) speculate that housing wealth effects may be larger in some cases if consumers regard changes in house values as more permanent than changes in other financial wealth.

[27] The effects of demographic characteristics on retirement are well established in the economic literature. See Blau (1994) or Peracchi and Welch (1994) for a discussion of these effects.

1994; Samwick, 1998) in which wealth is assumed to be exogenous and the estimated effect of wealth on retirement is very small. In fact, my estimates suggest that net worth may be as important to the LFP decision as the overall generosity of Social Security[28]. Although Social Security benefits are paid out in the form of an annuity, one can summarize the value of this income flow by computing Social Security Wealth (SSW: the expected present discounted value of the annuitized benefit from retirement until death). Based on data from Mitchell et al. (1996), the mean level of SSW among men in the HRS is $84k. Moffit (1987) finds that the 40% gain in Social Security benefit generosity between 1965 and 1975 can account for no more than 20% of the drop in LFP from 43% to 33% over this period. Estimates from Krueger and Pischke (1994) are similar in magnitude. For my sample, a 40% increase in benefit generosity would increase SSW by $34k. Thus Moffit's estimate implies that a $20k increase in SSW would reduce LFP by about 1 percentage point, which is similar in magnitude to my estimate of the wealth effect. However, Stewart (1995) finds that changes to the generosity of Social Security benefits have a substantially larger effect on LFP. He estimates that the 60% increase in benefit generosity between 1955 and 1990 reduced LFP of men aged 65 to 69 by 12 percentage points. This implies that a $20k increase in SSW for men in my sample would reduce LFP among older men by about 5 percentage points[29].

Having established that the effect of wealth on the LFP choice of older men is not trivial, I now ask whether a macro-economic shock to wealth can have an important effect on LFP rates among older men. I use my estimates to assess the impact of the recent run-up (bubble?) in

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[28] Changes to the overall generosity of Social Security affect benefits equally at all ages. Changes to age specific Social Security incentives may have a relatively larger effect on retirement behavior. For example Pingle (2006) finds that increases in the Social Security Delayed Retirement Credit have played an important role in the recent increase in LFP of older men, and Mastrobuoni (2006) finds that the increased normal retirement age for Social Security benefits has also been an important cause of the rise in LFP.

[29] All measures of LFPR and benefit generosity described in this paragraph are based on Figure 1 in Stewart (1995). The LFPR is measured on men aged 65 to 69. Benefit generosity is measured as the ratio of the average Primary Insurance Amount to average taxable earnings.

housing prices in the U.S. Growth in housing prices over the past decade has been astounding. After cumulative growth of 6.5% between 1975 and 1995, real housing prices increased by over 50% on average between 1995 and 2005. I conduct a counterfactual experiment in which housing wealth is adjusted to the level it would have been if appreciation rates over the past 10 years were "normal". I define the "normal" return on housing to be 0.5 percent annually, which is slightly more than the average real appreciation rate of housing in the U.S. between 1975 and 1995[30]. Results are presented in Figure 2. The effect of the housing wealth boom is apparent in Figure 2(a): counterfactual wealth in the year 2004 is $213k, 13% lower than the actual level of wealth. Figure 2(b) presents the actual LFP rate in 2004 along with simulated LFP rates based on OLS and 2SLS coefficient estimates. The 2SLS results imply that the LFP rate would have been 1.3 percentage points (or 4%) higher in 2004 if the extraordinary gains in housing prices had not occurred.

Was this estimated 1.3 percentage point reduction in the older male LFPR due to the housing market boom a large effect? There is no definitive answer to this question, but one comparison suggests not. Copeland (2003) estimates that a 1 percentage point increase in LFP among men and women aged 55 to 64 would improve the 75 year actuarial balance of Social Security by less than 4%. The boom in housing prices over the past decade was the largest recorded in U.S. history, yet Copeland's result suggests that the resulting 1.3 percentage point drop in LFP reduced the fiscal balance of Social Security by a tiny amount compared to the magnitude of the imbalance[31].

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[30] Specifically, counterfactual wealth for person *i* in year *t* is:

$$counterfactual\ wealth_{it} = non-housing\ wealth_{it} + \left(housing\ wealth_{it}\right)\left((1+.005)^{10}/(1+hpi_{it})\right)$$

[31] The actuarial balance of Social Security is the difference between payroll tax income and the cost of the program expressed as a percentage of taxable payroll over the 75 year evaluation period. Over the 75 year period 2005 – 2080, the present value unfunded obligation of Social Security is projected to be $4.6 trillion, representing 1.9 percent of taxable payroll and 0.7 percent of GDP over the 75 year period (source: 2006 OASDI Trustees Report,

Why isn't the effect of wealth on retirement larger? One explanation is that men may choose to spend their wealth gains through increased consumption of goods and services rather than increased consumption of leisure. This may be particularly likely if institutional rigidities in the labor market limit an older man's consumption of leisure. There is some evidence of this in the literature. For example, Rust and Phelan (1987) show that age specific Social Security and Medicare incentives affect age at retirement, particularly for liquidity constrained individuals. Khitatrakun (2004) finds that age eligibility rules associated with defined benefit pension plans restrict the ability of beneficiaries to make retirement decisions freely.

An alternative way to characterize the wealth effect on retirement is the Marginal Propensity to Consume leisure ($MPC_{leisure}$) from an increase in wealth. The $MPC_{leisure}$ measures the fraction of a $1 increase in wealth that an older man consumes through reduced labor earnings. I use my estimate of the wealth effect on LFP to compute the $MPC_{leisure}$ under the following assumptions: (1) A man can choose only to participate or not participate in the labor force in each year, and he cannot adjust his hours of work; (2) if the man does participate in the labor force, he earns a salary of $36k, the mean annual salary of men aged 55 to 60 who are working in my sample[32]. My estimates imply that a $100k increase in wealth reduces labor earnings by $1,700 on average – the product of the predicted change in LFP (-.047 percentage points) and the annual wage ($36k). Thus my estimate of $MPC_{leisure}$ is .017. This estimate is similar in magnitude to Cheng and French (2000) and Imbens et al (2001)[33].

Social Security Administration). A 4% improvement in the 75 year actuarial balance would reduce the present value unfunded obligation of Social Security by $184 billion over the period 2005 – 2080.

[32] I limit the sample to ages 55 to 60 to limit possible bias from self selection into retirement. Most men do not retire before age 60.

[33] Cheng and French (2002) estimate the $MPC_{leisure}$ from a gain in wealth at 0.02. In the Imbens et al. (2002), lottery winnings are paid out annually over a 20 year period. Imbens et al (2001) find that men aged 55 to 65 consume 16 cents of every dollar received from the annual lottery payout. By computing the present discounted value of the annuitized lottery payout (assuming a 5% discount rate), their results imply that $MPC_{leisure}$ from a gain in wealth is .013.

The economic literature provides a range of estimates of the Marginal Propensity to Consume goods and services (MPC_{goods}) among households in the U.S. Here I focus on papers that estimate MPC_{goods} on a population of older workers, who are more likely to also be considering retirement. Lehnert (2004) estimates MPC_{goods} at .08 for households between the ages 52 to 62, and at .03 for households older than 62 years of age. Li and Yao (2005) estimate MPC_{goods} at .06 among households in their 50s, and Morris (2006) estimates MPC_{goods} at .13 for households 50 and older. Each of these estimates is substantially higher than my estimate of $MPC_{leisure}$, which implies that a typical older man is much more likely to adjust his consumption of goods and services than his consumption of leisure in response to an exogenous increase in wealth. Given the labor market incentives that older men face, this implies that tastes for consumption at older ages are "stronger" than tastes for leisure.

C. Robustness

As a check on the robustness of my findings, I examined a number of alternative specifications. I briefly summarize the results here.

I estimated the empirical model on other retirement outcomes: full-time work and self-assessed retirement status. 2SLS estimates of the wealth effect on the alternative measures were similar in magnitude. In the specification in which wealth is instrumented with HPI and cumulative inheritances received since 1994, a $100k increase in wealth reduces the probability of working full-time by 2.1 percentage points and self-assessed non-retirement by 2.7 percentage points. Each estimate is significantly different from zero.

I estimated the model on a sample that is limited to men who were not labor force participants in the previous period. Thus the coefficient estimate on wealth can be interpreted as the effect of wealth on the rate of transition out of the labor force. Estimates based on this approach were qualitatively similar: an increase in wealth leads to an increased probability of labor force exit. However, none of the 2SLS estimates of the wealth effect on LFP were significantly different from zero. This is due in part to a reduction in sample size from 8021 to 3084 observations.

I also examined whether the results are sensitive to the omission of men for whom data on the 10 year change in HPI are not available. The omitted respondents tended to come from more rural areas, and on average were less educated and were less likely to participate in the labor force. I re-estimated the model on an expanded sample where these men were included. I defined an "HPI_10yr missing value" indicator (equal to 1 if the HPI_10yr is available, and equal to 0 otherwise), and set missing values of HPI_10yr equal to 0. The wealth effect estimates in this case were virtually identical to those reported above.

Finally, the empirical specification used in this analysis does not allow the effect of wealth to vary with age. This may be an important restriction. The effect of an unexpected wealth shock may be larger for a relatively older man, as he has less remaining life over which to spread the increase in consumption of goods and leisure. Thus, I estimated a model in which the interaction of age and wealth (age*wealth) was added to the specification. If the wealth effect on retirement is larger for an older man, the coefficient on the interaction term should be negative. To investigate this possibility, I used a 2SLS estimation procedure in which wealth *and* age*wealth are instrumented with HPI, cumulative inheritances, HPI interacted with age, and cumulative inheritances interacted with age. The 2SLS point estimates were consistent with the hypothesis that the wealth effect is increasing in age. The results indicate that for each year of age, a $100k increase in wealth reduces the probability of LFP by an additional 0.08 percentage points. For example, a $100k increase in wealth reduces the probability of LFP by 4.2

percentage points for a 60 year old man and by 5.0 percentage points for a 70 year old man. Although the coefficient estimates on wealth and wealth interacted with age were jointly statistically significant, the coefficient estimates on each term were not significantly different from zero. Therefore I cannot reject the null hypothesis that the effect of wealth on retirement does not depend on age.

VI. Conclusion

In this paper I find that an increase in wealth leads to earlier withdrawal from the labor force among older men, consistent with the assumption that leisure is a normal good. My IV estimates indicate that a $20k increase in wealth reduces the probability of LFP by about 1 percentage point. Previous estimates from the literature implied that the wealth effect was very small and not statistically different from zero. These estimates are likely biased downward due to measurement error and to the endogeneity of wealth. My OLS estimates suggest that failure to control for these factors leads to estimates of the wealth effect that are understated by a factor of at least 2.

I find no evidence against the hypothesis that the effects of housing and non-housing wealth on LFP are equal, although the power of my test is low. Thus, my analysis suggests that older men are equally willing to "spend" housing and non-housing wealth on leisure.

I use my estimates to examine the impact that an exogenous macro level shock to wealth – the boom in housing prices in the U.S. over the past decade – had on the LFP rate of older men. I find that the growth in housing prices led to an increase in wealth of 13% on average. If this gain in wealth had not occurred, my estimates suggest the LFP rate of men between the ages of 63 and 72 would have been 1.3 percentage points (or 4%) higher in 2004. This is a relatively small effect, considering that the growth in housing prices over this time period was larger than at any time in recorded U.S. history. Thus my estimates suggest that, given the modest effect of wealth on the LFP decision of an older man, the scale of macro-economic shocks to wealth (such as the potential housing price meltdown in coming years) is not large enough to have an important effect on aggregate labor supply patterns of older men.

One reason why the effect of wealth on LFP is not larger is that older men apparently prefer to spend an increase in wealth on consumption of goods and services instead of earlier retirement. This may be a result of institutional rigidities in the labor market that affect older men, such as age-specific Social Security and pension incentives. The labor force attachment of older women and younger married women may be weaker than for older men. In future research I plan to study the labor supply behavior of these groups – the effect of a change in wealth on labor supply may be more substantial for these populations.

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Table 1: Descriptive Statistics

Variable	Mean	Std. Dev.
Labor Force Outcomes		
Labor Force Participation	0.478	0.500
Full-Time Work	0.362	0.481
Not Retired (Self-assessed)	0.511	0.500
Personal Characteristics		
Age	63.0	4.2
HS Degree	0.344	0.475
Some College	0.196	0.397
College Degree	0.210	0.407
Hispanic	0.111	0.315
Black	0.177	0.382
Married	0.770	0.421
Bad Health	0.270	0.444
Homeowner	0.814	0.389
Local Economic Conditions		
Unemployment %	5.16	2.19
Per Capita Income	24.2	4.8
Manual Labor Share	0.469	0.080
Inheritance Expectations (Lagged)		
Missing Inheritance Expectations	0.150	0.357
Expect Inheritance (0 or 1)	0.293	0.455
Inheritance Probability (0% to 100%)	0.156	0.310
Expected Inheritance Amount	68.3	195.1
Expected Inheritance Amount Missing	0.018	0.134
Mother has No HS degree	0.623	0.485
Nbr Living Parents	0.333	0.545
Married	0.751	0.432
Spouse's Mother Has No HS Degree	0.448	0.497
Spouse's Nbr Living Parents	0.419	0.638

Notes on Table 1:

8021 observations for all variables except for "Expected Inheritance Amount" (2198 observations). "Expect Inheritance" is equal to 1 if the individual reports a positive probability of receiving an inheritance in the next 10 years (i.e. Inheritance Probability > 0), and 0 otherwise. "Expected Inheritance Amount" is the dollar value of the expected inheritance condtitional on expecting an inheritance. All dollar amounts expressed in units of 1,000 1992 dollars.

Table 2: Wealth Holdings and Instruments for Wealth

Variable	Mean	Std. Dev.	Percentiles 10th	25th	50th	75th	90th
Wealth Holdings							
Total Wealth	210.3	338.4	1.3	35.0	107.6	249.6	514.8
Housing Wealth	76.0	85.8	0.0	13.4	58.1	107.6	172.1
Non-Housing Wealth	134.3	290.7	0.0	4.9	35.9	138.6	370.3
Instruments for Wealth							
HPI (10 Year Change)	0.148	0.245	-0.132	-0.023	0.104	0.288	0.505
Received Inheritance (last 2 yrs)	0.035	0.183	0	0	0	0	0
Inheritance Amount (last 2 yrs)	82.0	163.1	3.8	8.9	29.5	77.5	172.1
Received Inheritance (since 1994)	0.146	0.354	0	0	0	0	1
Inheritance Amount (since 1994)	79.8	163.0	2.9	8.3	26.8	89.2	156.2

Notes on Table 2:
8021 observations on all variables except for "Inheritance Amount (last 2 years)" and "Inheritance Amount (since 1994)", on which there are 275 and 1175 observations, respectively. "Received Inheritance (last 2 years)" and "Received Inheritance (since 1994)" is the dollar value of all inheritances received over the period specified, conditional on receiving at least 1 inheritance. All dollar amounts expressed in units of 1,000 1992 dollars.

Table 3: Effect of the 10 Year Change in Housing Price on Wealth and LFP

Ownership Type	Dependent Variable: Total Wealth	Home Wealth	Non-Home Wealth	LFP
Home Owners	1.278 ***	0.594 ***	0.684 **	-0.069 *
(N = 6527)	(0.317)	(0.070)	(0.283)	(0.040)
Non Home Owners	-0.231	-	-0.231	0.001
(N = 1494)	(0.367)	-	(0.367)	(0.080)

Notes on Table 3:
OLS estimates with standard errors clustered by individual. All specifications include fixed effects for age and year, and controls for personal characteristics, local economic condiitons, and lagged inheritance expectations. * indicates significance at the 10% level (** at the 5% level; *** at the 1% level).

Table 4: First Stage Estimates on Identifying Instruments, Dependent Variable is Total Wealth

	Specification				
Identifying Instruments	(1)	(2)	(3)	(4)	(5)
HPI	1.0153 ***			0.9718 ***	0.9909 ***
	(0.2778)			(0.2817)	(0.2794)
Inheritances (Last 2 yrs)		0.8957 *		0.8807 *	
		(0.4686)		(0.4726)	
Inheritances (since 1994)			0.7341 ***		0.7275 ***
			(0.2285)		(0.2298)
F Statistic	13.36	3.65	10.32	9.92	12.89

Notes on Table 4:

Standard errors clustered by individual, 8021 observations. Wealth includes both housing and non-housing wealth, and is measured in units of 100,000 of 1992 dollars. All specifications include fixed effects for age and year, and also control for personal characteristics, local economic conditions, and lagged inheritance expectations. Specifications (3) and (5) also include subjective inheritance expectations as of 1994. F statistic is from a joint test of the null hypothesis that the coefficient estimates on the identifying instruments are equal to zero.

Table 5: Estimates of the Wealth Effect on LFP, Alternative Specifications

	Identifying Assumption			
Instruments	(1) OLS None	(2) 2SLS House Prices	(3) 2SLS Inheritances	(4) 2SLS Both
(a) Instruments are HPI and Inheritance Receipts in Past 2 Years				
Coefficient	-0.0153 ***	-0.0586	-0.0312	-0.0389 **
Std. Err.	(0.0027)	(0.0372)	(0.0198)	(0.0185)
Over ID Test	-	-	-	0.5277
(b) Instruments are HPI and Cumulative Inheritance Receipts since 1994				
Coefficient	-0.0153 ***	-0.0558	-0.0439 ***	-0.0454 ***
Std. Err.	(0.0027)	(0.0355)	(0.0164)	(0.0152)
Over ID Test	-	-	-	0.7583

Notes on Table 5:

Standard errors clustered by individual, 8021 observations. Wealth includes both housing and non-housing wealth, and is measured in units of 100,000 of 1992 dollars. Specifications vary along 2 dimensions: the identifying assumption and the instruments used. In Column 1 wealth is assumed to be exogenous and the wealth effect is estimated using OLS. In Columns 2-4, wealth is assumed to be endogenous and the wealth effect is estimated using 2SLS, where wealth is instrumented with measures of house prices (2), inheritances (3), and both (4). In section (a), the measure for house prices is the HPI and the measure for inheritances is the value of inheritance receipts from the past 2 years. In section (b), the measure for house prices is HPI and the measure for inheritances is cumulative inheritances received since 1994. All specifications include fixed effects for age and year, and also control for personal characteristics, local economic conditions, and lagged inheritance expectations. The specification in section (b) also controls for inheritance expectations as of 1994.

* indicates significance at the 10% level (** at the 5% level; *** at the 1% level). Over ID Test is the p-value on a Sargan-Hansen test of the joint null hypothesis that the instruments are valid (i.e. uncorrelated with the error term), and that the excluded instruments are correctly excluded from the retirement equation.

Table 6: Estimates of Housing vs Non Housing Wealth Effect on LFP

	Specification	
	(1) OLS	(2) 2SLS
Housing Wealth	-0.0328 ***	-0.0863
	(0.0106)	(0.1281)
Non Housing Wealth	-0.0125 ***	-0.0292
	(0.0031)	(0.0533)
Test: housing = non-housing wealth		
	0.0449	0.8087

Notes on Table 6:

In specification 1 housing and non-housing wealth are assumed to be exogenous, and their effects on LFP are estimated using OLS. In specification 2 housing and non-housing wealth are assumed to be endogenous and their effects are estimated using 2SLS, where wealth is instrumented with HPI * Homeownership status and Cumulative Inheritance Receipts since 1994. All specifications include fixed effects for age and year, and control for personal characteristics, local economic conditions, and lagged inheritance expectations. Standard errors clustered by individual, 8021 observations.* indicates significance at the 10% level (** at the 5% level; *** at the 1% level). Wealth is measured in units of 100,000 of 1992 dollars. "Test: housing = non-housing wealth" is the p-value from an F test on the equality of coefficients on housing and non-housing wealth.

Figure 1: LFPR by Age

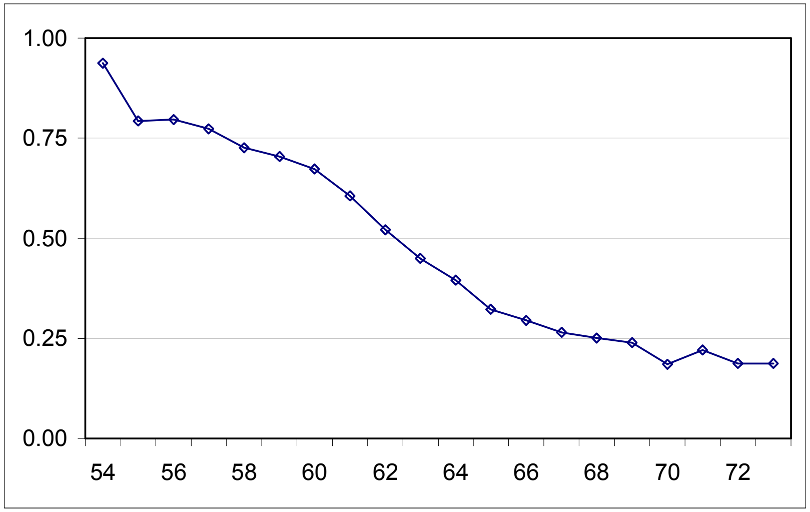
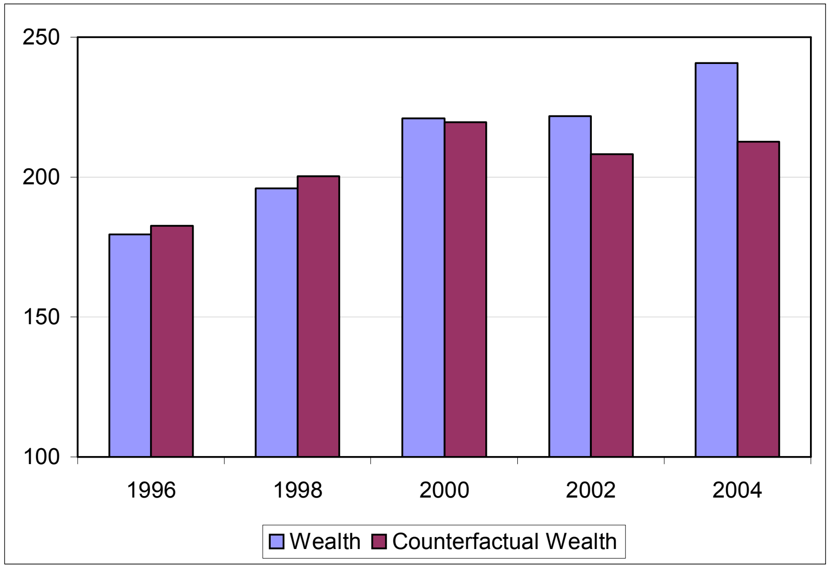
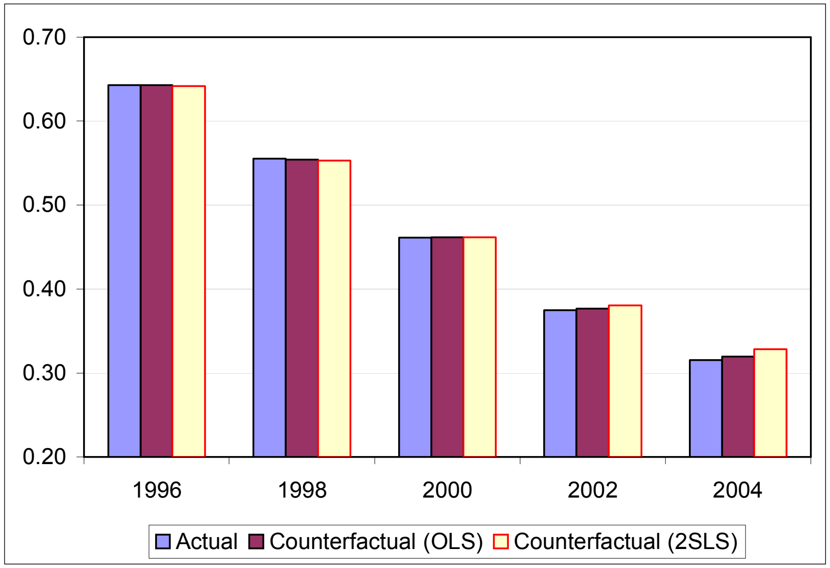


Figure 2: Counterfactual Simulation to Examine the Effect of Housing Price Increases on Labor Force Participation of Older Men by Year

(a) Actual vs Counterfactual Wealth



(b) Actual vs Counterfactual Labor Force Participation



Appendix Table 1: OLS Estimates, Dependent Variable is LFP

Dependent Variable: Labor Force Participation			
Variable	Estimate	Std. Error	
Total Wealth	-0.0153	(0.0027)	***
Year Dummies			
Year = 1998	-0.0041	(0.0117)	
Year = 2000	-0.0185	(0.0173)	
Year = 2002	0.0073	(0.0212)	
Year = 2004	0.0291	(0.0253)	
Age Dummies			
Age = 55	-0.1401	(0.0387)	***
Age = 56	-0.1046	(0.0332)	***
Age = 57	-0.1288	(0.0346)	***
Age = 58	-0.1651	(0.0337)	***
Age = 59	-0.1770	(0.0354)	***
Age = 60	-0.2077	(0.0348)	***
Age = 61	-0.2734	(0.0361)	***
Age = 62	-0.3621	(0.0365)	***
Age = 63	-0.4302	(0.0371)	***
Age = 64	-0.4695	(0.0379)	***
Age = 65	-0.5440	(0.0393)	***
Age = 66	-0.5667	(0.0411)	***
Age = 67	-0.6073	(0.0428)	***
Age = 68	-0.6123	(0.0449)	***
Age = 69	-0.6310	(0.0466)	***
Age = 70	-0.6833	(0.0482)	***
Age = 71	-0.6469	(0.0544)	***
Age = 72	-0.6902	(0.0562)	***
Age = 73	-0.6672	(0.0713)	***

Dependent Variable: Labor Force Participation			
Variable	Estimate	Std. Error	
Personal Characteristics			
HS Degree	0.0015	(0.0223)	
Some College	0.0416	(0.0258)	
College Degree	0.0913	(0.0288)	***
Black	-0.0158	(0.0232)	
Hispanic	0.1029	(0.0281)	***
Married	0.0703	(0.0239)	***
Bad Health	-0.2168	(0.0157)	***
Local Economic Conditions			
Unemployment %	-0.0048	(0.0034)	
Per Capita Income	0.5160	(0.1825)	***
Share Manual Labor	-0.0031	(0.0932)	
Inheritance Expectations			
Missing Inheritance Expectations	-0.0327	(0.0196)	*
Inheritance Probability	-0.0514	(0.0328)	
Expect Inheritance	0.0316	(0.0215)	
Expected Inh. Amt	-0.0001	(0.0061)	
Expected Inh. Amt Missing	-0.0104	(0.0398)	
Mother has No HS Degree	-0.0139	(0.0177)	
Nbr Living Parents	0.0188	(0.0148)	
Married	0.0402	(0.0246)	
Spouse's Mother has no HS Degree	-0.0361	(0.0199)	*
Spouse's Nbr Living Parents	0.0342	(0.0130)	***
Constant	0.7555	(0.0837)	***

Notes on Appendix Table 1:
OLS estimates with standard errors clustered by individual, 8021 Observations. Wealth is measured in units of 100,000 of 1992 dollars. * indicates significance at the 10% level (** at the 5% level; *** at the 1% level).