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Abstract

The nascent literature on wealth and health has not yet produced a complete or coherent picture of causal dynamics. We exploit geographic and time variation in housing market conditions to shed light on the impacts of wealth and local conditions on health. Our approach is motivated by hypotheses emerging from the biological literature, which would place physiological stress in causal pathways linking socioeconomic outcomes to health. Our key identifying assumption is that current asset prices reflect prospects for future appreciation; two houses that are believed by both buyers and sellers to have similar prospects will be priced similarly at baseline. We restrict all our comparisons to within metropolitan areas, and use third-party valuation estimates provided by private real estate consultancy firms along with data from the U.S. Health and Retirement Survey on the period from the 1990s up to 2006. We find that respondents living in communities where home values appreciated more rapidly were less pessimistic about their own future economic well-being, had fewer functional limitations, performed better on physical challenges under controlled laboratory conditions, and had better profiles of cardiovascular risk. We also find that home value appreciation showed no statistical relationship with a host of placebos like fixed personal characteristics or health outcomes from before the follow-up period of our analysis, which shores up our identifying assumption. We make a first attempt to distinguish wealth effects driven by value appreciation itself from the effects of changes in local conditions that drive the value appreciation in the first place. We find evidence that most–though not all–of our main results quantify wealth impacts.

1 Introduction

A vast literature has documented robust empirical relationships between various health and socioeconomic outcomes; the empirical regularity of these "socioeconomic gradients" is uncontroversial.[1] The causal dynamics *underlying* these gradients, on the other hand, have been more difficult to identify–in large part because health outcomes and socioeconomic outcomes are assigned in highly patterned ways. Propensity to higher levels of wealth and better health are likely to correlate with any number of characteristics that remain unobserved in most data, threatening to confound causal interpretation of observed relationships (Goldman, 2001, Strauss and Thomas, 2007). A more detailed understanding of these causal dynamics, however, has important implications for public policy.

This study contributes to the literature on wealth and health. We focus on a specific type of asset– owner occupied housing. Market dynamics make changes in asset prices a compelling source of external variation in accumulated wealth; we investigate the causal impact of this wealth accumulation on specific health outcomes in homeowners. We focus on outcomes that are physiologically related to chronic stress exposure. Our approach is motivated by an intriguing hypothesis emerging from the biological literature, that some properties of the physiological stress response may have been better adapted to our evolutionary history than to conditions of modern civilization (McEwen, 1998, Sapolsky, Krey, and McEwen, 1986); in modern conditions, this usually protective biological system can even have deleterious effects. Notwithstanding this motivating framework, we do *not* purport that our empirical approach pins down any specific biological mechanism.

Less has been written on the health effects of wealth outcomes than those of other types of socioeconomic outcomes. Furthermore, the nascent literature has not yet produced a com-

[1] For examples from this vast literature, see for example Adler and Stewart (2010), Finch (2003), Condliffe and Link (2008) and Fortson (2008).

plete or coherent picture of causal dynamics. On the one hand, there is evidence that some forms of wealth accumulation can cause improvements on some measures of long-run health status; on the other, a growing list of null results points to the critical role of nuances in measurement and study design. In an approach similar in spirit to ours, Smith (2007) examined changes in stock wealth during the 1990s and found little evidence of causal impact on health outcomes. A handful of striking results have even illustrated circumstances under which suddenly coming into possession of certain types of assets can have negative effects on health. Even though still in its infancy, this literature[2] raises questions about intricacies inherent to the multidimensionality of wealth and health. "Wealth" might include assets of varying degrees of liquidity– cash, a home, or even a purely illiquid and non-transferable "asset" like the good will of friends or family that can be parlayed into a stream of economically valuable services if circumstances require. "Health" might include conditions that are more or less acute, and also more or less somatic– psychological affect, anthropometric outcomes, or mortality (the most chronic outcome of all). Context likely matters; individual reactions to changes in wealth are likely to be affected by expectations about the future, in which case expectations would modify the health effects of changes in wealth. These nuanced interrelationships complicate single-dimensional counterfactual questions like, "Would more wealth bring better health?" and thereby leave fundamental public policy questions wide open. What specific health effects would some specific wealth augmenting program have? Does the *type* of wealth matter?

Our approach focuses on the impact of wealth accumulation embodied in owner-occupied housing, which is among the less liquid assets in many Americans' portfolios. To facilitate our analysis, we draw on several complementary data sources, including primarily the Health and Retirement Study (HRS). The HRS provides extensive information about economic conditions and health of a population representative cohort of Americans in late middle age

[2]We discuss the existing literature in more detail in section 2 below.

and older; this information includes assessments of physiological function obtained under controlled conditions. For our main analyses, we supplement this information with two time series of ZIP code level valuation estimates provided by independent, private sector real estate consultancy firms. The HRS also provides self-reported estimates of the value of every house owned by survey respondents; we use this information in our extended analyses. In section 3, we discuss in greater detail the relative costs and benefits of each source for valuation estimates.

Empirical assessment of the health impact of any wealth outcome must face the challenges arising from the fact that propensity to higher or lower levels of wealth is likely to correlate with unobserved characteristics. We address these challenges by taking advantage of geographic differences in time trends in housing market prices. For the vast majority of our sample, the real value of houses increased over the followup period. The housing bubble exploded about a year later.[3] For these homeowners, price increases would translate directly to wealth accumulation. Our key identifying assumption is that house prices reflect prospects for appreciation in the future; a group of houses in the same metropolitan area, priced similarly at baseline, should have similar prospects for future appreciation. In such a group, the houses that appreciate more rapidly in value are those that were "lucky" enough to experience a positive realization that buyers and sellers would have believed *ex ante* to be just as likely for the others. Based on this identifying assumption, we match communities on the basis of housing prices at baseline, and relate differences in subsequent appreciation to differences in health outcomes. All of our analyses are restricted to within metropolitan areas.

Our focus on housing wealth, and on individuals in late middle age and beyond, is motivated

[3]Although national trends indicate that average growth in home prices had stagnated by 2006, the full brunt of the subprime mortgage crisis did not take hold until early 2007, and the first *decline* in average home prices was not observed until later that year (Maitland and Blitzer, 2009).

by important empirical questions around the potential health impacts of this specific form of wealth on this specific subpopulation. On the one hand, housing wealth may be less divisible than other forms of wealth, and the services it produces may be less fungible than the income produced by other forms of wealth. In that case, as long as homeowners simply consume the services produced by their houses, even medium-run accumulations or decumulations driven by price fluctuations in the market around them might be inconsequential to their welfare.[4] On the other hand, the literature contains evidence that housing wealth accumulated by late middle age occupies a special place in the wealth portfolios of homeowners. It may represent a uniquely important buffer against negative economic shocks later in life (Venti and Wise, 2000, 2001, Walker, 2004). Furthermore, there is evidence that housing price changes substantially affect consumption prospects (Case, Quigley, and Shiller, 2005). Therefore, even for homeowners who never intend to liquidate the value embodied in their houses, rates of appreciation can still have important implications for financial stress.

Our findings are also relevant to the enormous literature on relationships between local neighborhood conditions and health.[5] That nascent literature establishes an empirical relationship between neighborhood characteristics and health outcomes, but much remains unknown about the causal dynamics. In addition to having a direct wealth effect, price appreciation may *reflect* changes in local characteristics that make a community a more desirable place to live. It is possible that these outcomes *themselves* may have health consequences. The key point is that prices represent a *summary measure* of these outcomes. Our identifying assumption is that by matching on baseline prices, we match communities

[4] We thank Seth Sanders for inspiring this simple illustrative example: Suppose an individual keeps a vegetable garden; all the vegetables produced by the garden are all the vegetables she eats. Suppose a new discovery about the health effects of vegetables drives up market demand for vegetables, but it remains the case that all the vegetables she grows are all the vegetables she eats. In that case, one may not necessarily expect the demand shock in the market around her, and the consequent change in the price of vegetables, to have any impact on her welfare.

[5] For a review of this literature, see Fortson and Sanbonmatsu (2010), Cummins, Curtis, Diez-Roux, and Macintyre (2007), Berkman and Kawachi (2003) and references therein.

that have similar prospects for the realization of these outcomes over the medium run. If any of these outcomes are better in one community than the others, it is for reasons that were unforeseen and uninfluenced by buyers or sellers *ex ante*. Much of the evidence on health impacts of local conditions has been drawn from natural (Lleras-Muney, 2010) and randomized (Fortson and Sanbonmatsu, 2010) experiments in which individuals *move* from one neighborhood to another. By contrast, medium run price movements can shed light on the impacts of environmental change resulting from developments *within* a neighborhood over time.

Our first set of results shores up our identifying assumption. We demonstrate that after controlling for initial housing values, baseline characteristics do not predict subsequent appreciation. The fact that appreciation is uncorrelated with a broad array of baseline *observed* characteristics lends credence to our assumption that it is also unrelated to the even larger and potentially more troubling array of relevant *unobserved* characteristics.

Having thus shored up our identifying assumption, we proceed to the main analyses. We find that within a metropolitan area, those homeowners who resided in communities where housing values appreciated most rapidly ended up in substantially and significantly better health, on several measures. They were more sanguine about their own economic prospects, and had fewer functional disabilities and better cardiovascular outcomes. These patterns were observed not only among those homeowners who remained in their original homes throughout the follow up period, but also among those who moved during the follow up period; we are able to follow movers for our analysis.

The results in our main analyses would represent a reduced-form aggregation of the wealth effects of price changes along with the effects of whatever local improvements drove the observed price changes. We do not presume to be able definitively to disentangle these two broad sources of impact; however, we do make a first attempt to explore the distinction, albeit with appropriate caution. Our results provide evidence that most– though not all– of our main analyses identify wealth effects.

Taken together, our results indicate that wealth accumulation driven by the housing market has important welfare implications. This wealth accumulation is perceived by homeowners, insofar as it affects their outlook on their own economic prospects. Furthermore, it has substantively and statistically significant impacts on cardiovascular health, and even the aging process more generally. Programs to increase demand for local housing assets, therefore, may be expected to generate improvements in terms of stress-related health.

The paper is organized as follows: In the next section, we draw on literatures from biology, public health, and the social sciences to outline the conceptual framework motivating our analysis. In section 3, we describe our data sources and empirical approach; we also provide some empirical evidence to shore up our key identifying assumption. In section 4, we present our main results, which we extend in section 5. We conclude with a discussion of remaining unanswered questions. One unanswered question that we emphasize–especially in light of the prolonged collapse in housing values over the more recent past–is whether shorter run changes have analogous impacts to those we observe here.

2 Conceptual framework and previous literature

2.1 Socioeconomic outcomes, physiological stress, and health

A class of hypotheses that has received increasing attention in the past decade focuses on stress-related biological processes that may link socioeconomic outcomes to health (McEwen, 1998, Seeman, Epel, Gruenewald, Karlamangla, and McEwen, 2010, Dowd, Simanek, and Aiello, 2009). In this framework, stress becomes physiologically damaging when normally adaptive stress responses are deployed too frequently, or at a chronically high level. Research on the health damaging effects of stress has focused heavily on the hypothalamic-pituitary-adrenergic axis (HPA), a system that regulates deployment of glucocorticoids– a class of corticosteroid hormones that includes cortisol– in response to stimuli perceived as stressful (McEwen, 1998).

Glucocorticoids help mobilize the characteristic "fight or flight" physiological response in response to acute challenge. For example, glucose is quickly released into the blood stream for a short-term energy boost, and heart rate and blood pressure increase to deliver fuel to muscle throughout the body. Digestion, growth, bone and tissue repair, reproduction and other physiological processes not needed to address the acute challenge are suppressed. The inflammatory immune response ramps up in anticipation of any potential injury or infection. Glucocorticoids also play a central role in the negative feedback loops that shut off this response once the threat has passed. These feedback loops give the system its characteristic plasticity that would be familiar to anyone who has ever been acutely startled.

This system is well adapted to addressing an acute challenge (for example, the threat posed by a predator). It may not be as well suited to modern environments in which exposure to stressors is chronic or anticipatory (McEwen, 1998). After sustained deployment of glucocorticoids over sufficient time, the system gradually loses its normal plasticity. This can lead to levels of circulating glucocorticoids that might appear inconsistent with the degree to which an individual is "stressed" in the colloquial sense of that word. Chronic activation can also lead to a blunted or under-active HPA response (Miller, Chen, and Zhou, 2007).

There is evidence in laboratory experiments and observational studies that chronically high or low levels of glucocorticoids contribute to pathogenic processes across multiple physiological systems[6] ; some of the powerful side effects of prolonged exposure to glucocorticoids will be

[6]For fuller discussion of the evidence on each of the effects we mention here, see for example: Lupien,

familiar to anyone to whom similar corticosteroids (like prednisone) have been prescribed by a doctor.

Changes in the function of the heart and blood vessels that improve physical performance over short periods can induce chronic hypertension if prolonged. Constant mobilization of energy stores can lead to insulin resistance and an increased risk of type 2 diabetes. Other metabolic adjustments that improve capacity to fight or to flee can, after a time, disrupt the body's ability to distribute fat around the body, leading to abdominal obesity. Each of these stress mediators brings increased risk of cardiovascular disease; they are also implicated in cognitive impairments, depression, osteoporosis, impairment of the body's ability to manage infection, and more rapid aging at the cellular level. The cumulative wear and tear of prolonged exposure to glucorticoids, therefore, may not necessarily result in any one specific illness; it may lead to any number of disease endpoints, or even just a more rapid progression toward general age-related frailty.

Although controlled animal and human studies provide compelling evidence for health impacts of exposure to chronic stress, it remains an open empirical question whether these mechanisms are an important explanatory factor for health patterns in real world populations. The stress response may be less adapted for psychosocial stressors for which our acute stress response is less adaptive– especially if they involve the anticipation of challenges, or are characterized by lack of control and predictability (Sapolsky, 2005). Many socioeconomic outcomes could plausibly be characterized as psychosocial stressors in that sense. Net wealth (and housing wealth in particular) may be especially important for stress conditions as individuals and families approach the stage in their life cycle when they will rely on their wealth to support their consumption (Modigliani and Brumberg, 1954, Venti

McEwen, Gunnar, and Heim (2009), Wolkowitz, Epel, Reus, and Mellon (2010), Bauer, Jeckel, and Luz (2009), Chrousos and Kino (2007), Lupien, de Leon, de Santi, Convit, Tarshish, Nair, Thakur, McEwen, Hauger, and Meany (1998), Miller, Chen, and Zhou (2007), Rosmond (2005), Cacioppo, Kiecolt-Glaser, Malarkey, Laskowski, Rozlog, Poehlmann, Burleson, and Glaser (2002) and references therein.

and Wise, 2001, Benitez-Silva and Dwyer, 2002). In that case, low levels of housing wealth may represent increased vulnerability to negative shocks like an accident or an unexpected increase in recurring expenditure. This sense of vulnerability may in turn lead the body to over-deploy its stress response, resulting in the ill health effects observed in the laboratory studies.

Neighborhood conditions constitute another class of socioeconomic outcomes that may constitute physiological stressors. More desirable neighborhoods may be places with higher levels of security, or places where social networks are more developed. Neighborhood improvements on these dimensions may reduce the stress burden on residents associated with insecurity or social isolation, and be reflected in higher prices.

The results in our main analyses would represent a reduced-form aggregation of the wealth effects along with the effects of whatever local improvements drove the observed price changes. Both sources of effects– regardless of the biological mechanisms driving their impact– are of important policy and scientific interest. For example, neighborhood revitalization programs would be expected to impact health through both channels. In our extensions from the main analyses, we make a first pass at disentangling these two sources of impact.

2.2 Other biological mechanisms

Notwithstanding the stress-based conceptual framework motivating our analysis, we do *not* purport that our approach pins down any specific biological mechanism. The health outcomes we analyze may be affected by housing wealth and local improvements through biological channels other than stress.

Changes in any form of wealth may impact some of the health outcomes we analyze by changing demand for various consumption goods. For example, increases in wealth may reduce the need for precautionary savings out of current income.[7] This would increase an individual's level of total consumption, and therefore increase demand for normal goods and reduce demand for inferior goods. If richer foods or tobacco products were normal goods, then increases in wealth might thereby increase consumption of these goods with negative consequences for health. On the other hand, if health promoting goods like medical care and exercise were normal goods, then increases in wealth might have positive consequences for health. The net effect of wealth on health through the consumption demand channel would ultimately depend on the balance of these types of effects.

Neighbhorhood outcomes may impact health outcomes through channels other than stress as well. Increased availability or environmental improvements that make a place more amenable for exercise may make it a more desirable place to live, thereby driving up housing prices. If residents avail themselves of these resources, then their health might improve as a result of the salubrious effects of better food or exercise. More generally, *any* outcomes that home buyers are willing to pay for will be reflected in price changes. If these outcomes are salubrious, then higher prices will reflect better health conditions; if they are deleterious, the opposite will be true. The net relationship between price outcomes and health outcomes through the willingness-to-pay channel would ultimately depend on the balance of these types of effects.

2.3 Previous literature

The literature relating health and wealth is considerably smaller than that relating health and other socioeconomic outcomes like income or education. Nonetheless, consistent associations have been reported. Menchik (1993) found an inverse association between wealth and mortality over 15 years of follow-up in the National Longitudinal Survey of Older Men,

[7] Engelhardt and Kumar (2011), for example, present evidence that exogenous increases in pension wealth crowd out other forms of household saving; it is plausible that increases in housing wealth might have similar impacts.

which persisted in the presence of controls for permanent income, baseline health, and other sociodemographic characteristics. Attanasio and Hoynes (2000) analyzed data from the Survey of Income and Program Participation, and reported that mortality was about three times higher in the lowest wealth quartile, compared with the highest wealth quartile. In the Health and Retirement Survey, Hurd and Kapteyn (2003) found that increases in wealth increased the odds of remaining in the top self-reported health category in the following survey wave. More recently, Hajat, Kaufman, Rose, Siddiqi, and Thomas (2011) have found significant associations between wealth and mortality and self-reported health over twenty years of the followup period of the Panel Study of Income Dynamics.

Less is known about the causal dynamics underlying these associations. Two important papers use time-series methods to explore these dynamics. Adams (2003) used the Asset and Health Dynamics of the Oldest Old (AHEAD) panel (respondents aged 70 and over) to test whether wealth "Granger-causes" health. They found no evidence that changes in wealth affect the subsequent likelihood of mortality or the sudden onset of health conditions, although they did report evidence that changes in wealth affect the risk of more gradual onset health conditions including mental conditions. Michaud and van Soest (2008) employed an Arellano-Bond estimator using six waves of the HRS and found no time-series evidence of causal wealth effects on various health outcomes.

Turning to another class of approaches, two papers have explicitly examined the health consequences of potentially exogenous changes in wealth. Meer, Miller, and Rosen (2003) used inheritance as an instrument for changes in wealth in the PSID, and found a substantively and statistically insignificant relationship between changes in wealth and changes in self-reported health. Using an approach closely related to ours, Smith (2004, 2007) examined changes in stock wealth in the HRS and PSID during the 1990s and found only one significant association with health–an increase in stock market wealth being associated with an increased likelihood of the onset of arthritis. One key difference between this approach and ours is that detailed stock portfolio composition is unobserved in the data, making it impossible to distinguish empirically between wealth changes that are driven by changes in specific asset prices and reallocations by individuals of their asset portfolios. This distinction is important, since the former source of change in asset wealth is driven by market factors which are likely external to any individual characteristics, whereas the latter is almost certainly driven not only by changes in respondents' circumstances but also by characteristics of the respondents *themselves* like their ability or willingness to trade off short run liquidity for long run wealth. To our knowledge, housing wealth is unique in this regard– respondents' homes are the only asset for which price effects can be empirically isolated.

A few papers have even presented striking results that highlight circumstances in which the acquisition of specific assets can be deleterious to health. Bruckner, Brown, and Margerison-Zilko (2011) look at the impacts of cash transfers to Cherokee Indians from a casino on reservation lands. When they reach adulthood, young Cherokee become eligible for large lump-sum transfers which represent accumulated payments held in trust through their adolescence. Ethnographic and statistical evidence indicates that these large transfers are often parlayed into assets or durable goods that have deleterious health effects. For example, young men are disproportionately likely to purchase new cars in the month when they become eligible for the transfer, and to die in motor vehicle accidents in the weeks immediately following. Evans and Moore (2009) find similar patterns following receipt of lumps of income in other contexts.

The array of patterns reported in this literature highlights the intricacies inherent to the analysis of multidemensional concepts like "wealth" and "health." The set of outcomes that are captured under each of these headings are enormous and complex in their own right. For each of these sets of outcomes, the causal dynamics that determine their evolution over a life course and their distribution across a population can take a generation or more to play out. The challenges to identifying how these two sets of outcomes *interact* are therefore substantial. These challenges are further exacerbated by the fact that the effects of each type of outcome on the other are almost certainly conditioned on other individual and community characteristics, including for example expectations about the durability of accumulated wealth and norms about how it should be shared with others.

The literature summarized here sheds invaluable light on the causal dynamics linking these outcomes, but many questions remain unanswered. Approaches that have relied on the time-sequence in which outcomes are observed raise questions about the role of dynamic factors like expectations, and also about the time gaps between when outcomes can be observed by survey respondents and when they can be observed in the data. Approaches that rely on instruments to identify the impact of one outcome on another raise questions about the exogeneity and excludability of the instruments.

We focus on wealth augmentation driven by *price* changes, because asset prices represent a compelling external source of variation; markets create incentive for many independent actors to build expectations about the value of an asset into its current price, and more generally to allow long run changes in the price to result only from factors that would be generally unforeseeable.

3 Data and Empirical Approach

3.1 Overview

Our empirical strategy exploits arguably quasirandom variation in housing prices over the medium run–that is, from the early-to-mid 1990s[8] until 2006. For the vast majority of our sample, the real value of houses increased over the followup period.

For homeowners, these price changes would translate directly to wealth accumulation. Our key identifying assumption is that house prices reflect prospects for appreciation; a group of houses in the same metropolitan area, priced similarly at baseline, should have similar prospects for future appreciation. In such a group, the houses that appreciate more rapidly in value are those that were "lucky" enough to experience a positive realization that buyers and sellers believed *ex ante* to be just as likely for the others. Therefore, while price *levels* are almost certainly correlated with a large number of observed and unobserved characteristics of homeowners, we make the more plausible identifying assumption that price *changes* are external to any individual potential buyer or potential seller; in that sense, we assume price appreciation to be "quasirandom." Based on our identifying assumption, we use flexible controls to match communities on the basis of housing prices at baseline, and relate differences in subsequent appreciation to differences in health outcomes.

3.2 Data sources

We draw on three complementary sources of data for our analyses. All individual-level information comes from the Health and Retirement Study. We complement that information

[8]As we discuss below, the baseline year varies across respondents, primarily based on the year of birth of the respondent.

with estimates of housing values from two independent private sector sources. One set of valuation estimates is generated by DataQuick, a California-based real estate consultancy firm, and the other was originated by Case and Shiller.

3.2.1 The Health and Retirement Study

The Health and Retirement Study began in 1992 as a longitudinal study of 7500 randomly selected Americans born between 1931 and 1941, along with their 5200 spouses. These respondents form the "core" HRS sample (Survey Research Center, University of Michigan, 1992). Every respondent was targeted for reinterview every two years. Three additional cohorts were added as the study matured[9]:

- In 1996, the study expanded to absorb the closely related AHEAD study, which included a population representative sample of individuals born before 1920;
- In 1998, the study expanded to include 1600 individuals randomly drawn from the "Children of the Depression Era (CoDA)" birth cohorts (1920-1931), along with their 800 spouses;
- also in 1998, the study expanded to include 1600 randomly drawn individuals from the "War Babies (WB)" birth cohorts (born 1941-1947), along with their 1100 spouses;
- finally, in the year 2004, the study added 2000 randomly drawn "Early Baby Boomers (EBB)" (born 1947-1953) along whith there 1300 spouses.

As each new cohort was added, it was also absorbed into the biennial reinterviews. Therefore, by the 2004 wave of the study, the total sample targeted for biennial followup had grown to

[9] The cohort names are those used in the original HRS documentation.

include about 13,000 individuals born over the period 1920-1953, along with their current and former spouses. The 2006 wave marks the end of the followup period for our analyses.

In its earliest implementations, HRS included detailed modules on debt, income, and wealth–including a set of questions on housing. Respondents were asked the type of housing they occupied (for example, whether it was a condominium, a single family detached house, a ranch/farm, or a mobile home), whether they owned their housing, and to estimate the market value of their home.

The survey also collected demographic information including age, sex and educational attainment for respondents as well as their coresident and noncoresident children. Health information included self-reported information on diagnosed conditions, the degree of difficulty of several activities of daily living, and general health status.

In 2006, HRS incorporated a major innovation; a randomly selected half of respondents were targeted for inclusion for a new biomeasures module. This module includes health information that had not previously been collected, including anthropometrics as well as information on biomarkers that may be relevant to undiagnosed or subclinical conditions like chronic high blood sugar or hypertension. This module increases the scope of information available; it also allows us to supplement self-reported health information with related measures reported by someone other than the respondents themselves.[10]

Over the followup period, the HRS was also innovating steadily between waves to improve and expand the information available on respondents' psychosocial outlook and on their expectations about the future.

[10]The self-reported information is sometimes described in the literature as "subjective," while that reported by someone other than the respondent is sometimes described as "objective." In this paper, we avoid such loaded terminology, and instead refer to these different types of information as either "self-reported" or "externally measured."

We also make use of the study's geocoded information on respondents' residential location at the ZIP code level, which is available under restriction.

3.2.2 Private sector estimates of housing values

DataQuick is a private sector real estate consulting firm based in California. Across the United States, county offices record housing characteristics and sales prices for every home sold in their jurisdiction. In most states, this information is available to the public; firms like DataQuick collect it, and use proprietary analytical approaches to estimate the market value of residential properties.[11] DataQuick intends its estimates for sale to private sector, for-profit clients including lenders and real estate investors. We purchased a dataset of median valuation estimates for single family detached houses over the period 1988-2007, in each of 2400 ZIP codes in which HRS respondents were residing at the time of their baseline interview. We further supplemented the valuation estimates from DataQuick with analogous information on 2000 ZIP codes from the Standard and Poor's/Case-Shiller Home Price Index.[12] We verified that the two series are very highly correlated.[13] In the interest of parsimony, we focus in this paper on analyses using the DataQuick data.

We incorporate the price data as a complement to respondents' own self-reported estimates of the value of their houses. The external valuation estimates have two distinct advantages over the self-reports. First, for obvious reasons self-reports can only be asked of homeowners about their current homes. Analyses using the self-reports would therefore necessarily exclude all

[11]To learn more about this aspect of DataQuick's business, see `http://www.dataquick.com/products/valuation/`.

[12]For more discussion on this index, see Shiller (2007), Case, Quigley, and Shiller (2005), and Case and Shiller (1987, 1989).

[13]DataQuick's estimates are annual, and denominated in dollars, whereas Case-Shiller reports a quarterly ZIP-specific index value, with the first quarter of 2000 normalized to 100 for each ZIP. A linear regression of the DataQuick dollar-denominated time series against the Case-Shiller index time series, with fixed effects at the ZIP code level, produces an R^2 of 0.86.

respondents who do not own their housing, and cut short the followup period for those who sell. Endogenous attrition of this type would greatly complicate the interpretation of our results, especially if the dynamics determining respondents' risk of selling their houses are correlated with any unobserved health-related characteristics (as is likely). Second, errors in the self-reported estimate of home values are likely to be correlated with health-relevant characteristics. For example, more "optimistic" individuals may be more likely to report higher estimates. This sort of non-classical measurement error would complicate interpretation of observed relationships. The external valuations may be at lower risk for this problem. As figure 1 shows, the self-reported estimates and DataQuick's estimates are highly correlated.[14]

There is no free lunch; these advantages come at a cost. First, the external valuations are specific to each ZIP code, not to the home of each HRS respondent. At best, that reduces the power of our analyses by reducing our effective sample size. Second, like any estimate, these valuation estimates almost certainly contain error themselves, but the error is difficult to characterize. A critical assumption underlying our interpretation of the results is that measurement error in the valuations is classical, in that there is no statistical relationship between errors in the third-party estimates and unobserved health-related characteristics of homeowners or their communities. This assumption is plausible; if diligent arbitrageurs could turn a profit by learning more about homeowners or their communities, they would have incentive to do so, and thereby bid up the initial price, implicitly building that information into the observed sales prices that are used to generate the estimates in the first place.

Our first set of results below lends further credence to the assumption. However, the proprietary nature of these firms' estimation techniques limits how specifically we can know what

[14]Relatedly, Benítez-Silva, Eren, Heiland, and Jiménez-Martín (2010) compare self-reported home valuation estimates and *actual* sales prices for those respondents who sold their homes during the HRS follow-up period. They find that self-reported valuations are a powerful predictor of subsequent sales prices, but they observe a small degree of overestimation on the part of the average respondent.

is implied by our assumption of classical measurement error.

3.3 Sample

Our main analytical sample consists of owners of detached houses, born between the years 1924 and 1960, in those communities where data on transactions of detached houses are available to allow for value estimation.[15] Table 2 illustrates how the sample restriction criteria may affect the representativeness of our results. The first column describes the universe of HRS respondents. Moving across the columns, the sample restriction criteria accumulate.

A quarter of the HRS respondents were born before 1923, and half were born before 1934. Almost a third did not finish high school. A quarter of them report less than $1000 in total assets, and almost a third died during the course of the study. Comparison to the second column illustrates that by restricting to younger respondents, we eliminate a substantial fraction of attrition due to mortality. Comparing the second column to the third indicates that the communities in which housing transactions are reported in the public record differ from those where they are not; by restricting to communities where external valuation estimates are available, we select a slightly more educated, slightly wealthier subsample. Comparison with the next column reflects the fact that homeowners are more educated and wealthier, on average, than respondents who did not own their housing.

The most important factor determining the completeness of followup was mortality. Below, we analyze how housing price appreciation predicts incomplete followup; table 2 illustrates that those who survived and remained in the sample through the followup period were slightly younger, wealthier, and better educated than those who did not.

[15] In the vast majority of cases where a community is excluded, it is because the community is located in a "non-disclosure state," so that housing transactions are not recorded in the public record.

As we have discussed, some of our analyses use externally measured health indicators (including for example anthropometrics). For these analyses, we must restrict our sample to those respondents who were measured. Measurements were meant to be collected on a *randomly* selected half of available respondents; comparing the last two columns in the table indicates that those who were measured were similar, on average, to those who were not measured. However, the poorest and wealthiest tails of the distribution may have been lost from the biomeasures sample.

Taken together, the table indicates that our analytical sample is about 6 years younger, 18 percentage points more likely to have attended college, and nearly three times as wealthy as the overall HRS sample. Therefore, our results speak to the combined impact of changes in wealth and changes in local conditions among the relatively wealthier and more educated; we remain duly cautious about generalizing our results to other socioeconomic strata.

Figures 2 and 3 report the proportion lost from our analytical sample over the followup period. As we discussed in section 3.2.1 above, the year of baseline interview depended primarily on the target respondent's year of birth.

Figure 2 reports the fraction of homeowners born 1924-1960 in each study cohort that was still alive at each subsequent follow-up interview. The "War Babies" and "Early Baby Boomers" cohorts are sufficiently young that mortality rates were low over the course of the followup period. This, together with the fact that their baseline interviews were relatively late, explains why over 90% of respondents in these cohorts survived from the baseline interview to the end of our follow up in 2006. By contrast, the longer period of follow-up and older age of the core HRS sample combine to produce much more substantial attrition due to mortality–nearly a quarter of that cohort died before the end of our current followup period.

Figure 3 includes attrition due not only to mortality, but also refusal, and all the other reasons for unsuccessful followup (although, mortality and refusal account for most of the attrition). The pattern is similar, although non-mortality related attrition appears to be a more important factor for the cohorts with the later baseline interviews than for the core HRS cohort.

As we discuss in section 3.5, all regressions reported below include a dummy variable for study cohort, to capture any mean-shifting effects of dynamics like these. We have also repeated all of our analyses separately for each cohort; everything we report below is consistent with the results of those stratified analyses.

3.4 Outcomes of Interest

We first examine the association between housing appreciation and respondents' economic expectations and general optimism, which may indicate how appreciation impacts perceived economic stress. These measures included the probability that the respondent assigned to various future outcomes. We focus on the probability that, at the end of the followup period, respondents believed:

1. their income would keep up with their cost of living
2. they would bequeath more than $10,000 to their children
3. they would transfer more than $5000 to others over the decade subsequent to the interview
4. a major economic recession would occur within the next decade
5. "tomorrow" (at the time of the interview) would be a rainy or snowy day.

Responses took the form of a probability in percentage terms (0-100). Responses to survey questions of this form typically heap heavily on a small number of salient values (Lillard and

Willis, 2001); responses in HRS conform with this pattern.[16] We estimate linear regressions, treating reported probabilities as continuous. As others have discussed (Bassett and Lumsdaine, 2001, Lillard and Willis, 2001), the linear regression approach may rely on questionable assumptions about how respondents process information and think probabilistically. We take two precautions to ensure that our results are not driven by these assumptions. First, we follow Lillard and Willis (2001), who argued that "ambiguity" or epistemic uncertainty about a probability may drive some types of respondents to report the focal values 0, 50, or 100. Accordingly, we include in all regressions flexible controls for the number of answers in the expectations module at the time of the *baseline* interview that took one of these three values, in order to "control" for the type of respondents who react to epistemic uncertainty in this way. Second, we repeat the analyses, relaxing functional form assumptions. Specifically, in a robustness check we use the reported probabilities to construct two coarse categories for each question. Specific to each question, we choose a salient value (10, 25, 50, and so on) that cuts the sample into a "more optimistic" bin containing three fifths to three quarters of respondents, and a "more pessimistic" one containing the rest. We then estimated linear probability models for ending up in the "more pessimistic" bin. The results of those analyses were consistent with the results we report here.

We make use of the question about the weather, but will interpret the result with appropriate caution. In the 1994, 2004, and 2006 waves of the HRS, respondents were asked a "warm-up" question at the beginning of the expectations module, in order to give them a chance to practice assigning probabilities to uncertain future outcomes. In 1994 and 2004, this "warm up" question asked them to report the likelihood that tomorrow would be a sunny day. The "sunny day" question has become a subject of substantial interest in its own right. Bassett and Lumsdaine (1999) first reported that respondents who reported higher

[16] To take a typical example–when respondents were asked to report the likelihood of rain or snow tomorrow, three fifths of the answers were accounted for by four focal values (0, 10, 50, or 100).

probabilities in this question (net of geographically-driven climate variation) were also more likely to answer other expectations questions "optimistically." Later, they introduced evidence that the question may reflect not merely "optimism" but rather a propensity to report higher probabilities, regardless of whether the event in question is "good" or "bad" (Bassett and Lumsdaine, 2001); interpretation of responses has consequently become more nuanced. In part in order to differentiate "optimism" from a propensity to inflate probabilities, the question was changed in 2006 so that respondents reported the likelihood of precipitation tomorrow, rather than the likelihood of sun.[17]

We next examine the relationship between housing appreciation and both self-reported and measured physical functioning. In 2006, respondents were taken through a list of about eight activities of daily living (ADLs) including walking, climbing stairs, carrying moderately heavy objects like a bag of groceries, and so on. For each activity, they were asked to indicate whether they did it as part of their everyday lives, or whether they were "able" to do it at all. If they did do it, they were asked whether or not they found it "difficult," and to indicate how "difficult" it is. We classify a person as having "limited capacity" in each activity if the respondent reports that he or she finds it "difficult," that he or she "can not" do it, or that he or she "does not" do it. The "does not" response is the most ambiguous. For example, what if homes in the fastest appreciating neighborhoods are more likely to have more than one floor? In that case, individuals in the slower appreciating neighborhoods might be more likely to report that they "do not" climb stairs routinely, not because the activity is difficult, but simply because it is unnecessary in their lives. However, recoding the variable so that only the answers "can not" and "does, but with difficulty" count as functional limitations has

[17]Perhaps unsurprisingly, answers overall tend to correlate negatively between the baseline "sunny day" formulation and the 2006 "rainy day" formulation; however, when a fixed-effects regression is used to restrict comparisons to within metropolitan areas, the relationship becomes an imprecisely-estimated zero. Insofar as variation within a locality is driven by individual characteristics, these characteristics are likely to be time-varying, and to be more nuanced than either pure "optimism/pessimism" or pure fixed personal propensity to inflate/deflate probabilities.

no substantive effect on the results we report below.

One of the elements of the innovative module on biomeasures that was added in 2006 (see section 3.2.1) involved tasks assessing functional capacity. One of these tasks involved respondents being asked to balance on one leg for 60 seconds (older respondents were only asked to balance for 30 seconds). We assess externally measured functional capacity with a dichotomous variable, equal to 1 if the respondent was able to balance for the entire time, and 0 otherwise.[18]

Finally, we test the association between housing appreciation, measured anthropometrics and biomarkers of health risk. Another component of the biomeasures module included a series of measured anthropometrics, including waist circumference and blood pressure. We code hypertension as equal to one if measured systolic blood pressure exceeds 140 *and* measured diastolic blood pressure exceeds 90, or if the respondent reported that he or she was taking blood pressure medication as prescribed by a doctor. By this measure, a full 57 percent of the analytical sample qualified as hypertensive.[19]

3.5 Analytical Specifications

We will begin by shoring up our identifying assumption in a series of placebo regressions. In these regressions, the conditional variable is the (log of) home value *in 2006*, as estimated by DataQuick. In each placebo regression, we include as the conditioning variable of interest a single respondent characteristic from the *baseline* year, along with a flexible spline for

[18]There were three functional capacity measures we opted not to analyze. Respondents were asked to blow into a spirometer (to assess lung capacity), to grip a dynamometer (to assess hand strength), and to walk a short distance while being timed. The lung capacity and grip strength tasks demonstrated substantial variation across repeated trials, and included a nontrivial fraction of outlier values. The timed walk was only administered on older respondents.

[19]According to the Centers for Disease Control and Prevention, about 56 percent of the US population in this age group qualified as hypertensive based on these criteria (NCHS, 2009)

house valuation in the baseline interview year (notched at each quartile). As we discuss, our identifying assumption is that price appreciation is external to any potential buyer or potential seller; under that assumption, the coefficient on 2006 valuation should be zero.

For our main analyses, the conditional variable for each regression is one of the *year 2006* health outcomes described in section 3.4, and our conditioning variable of interest is the (log of) home values in 2006, as estimated by DataQuick. In addition, the right hand side in our main analyses will include flexible controls for the following baseline wealth characteristics: house valuation as estimated by DataQuick, non-housing wealth, housing and non-housing debt. We also include indicators for self-rated health in the year of the baseline interview, metropolitan area[20] fixed effects, and indicators for birth year, race, sex, and study cohort. Since a flexible spline for house valuation in the baseline interview year (notched at each quartile) is included separately on the right hand side of the regression, the coefficient on house valuation in 2006 will indicate the effect of the *appreciation* in home values over the followup period.

Specifications of particular robustness checks and extensions are elaborated at the appropriate point in our discussion of results.

4 Results

In this section, we report the results of our main analyses. More detailed discussion on all measures, our regression specification, and more complete definitions and descriptions of all variables, can be found in section 3.

[20]We identify a "metropolitan area" as a census core based statistical area (CBSA).

4.1 Are price changes quasirandom?

Our results identify the causal impact of housing value appreciation only if, after matching flexibly on estimated market valuation as baseline, subsequent appreciation is uncorrelated with relevant observed or unobserved characteristics. To explore the plausibility of this critical identifying assumption, we perform a series of placebo regressions.

Results of these placebo regressions are shown in table 1. Each cell in the table represents a different regression. An observation is an HRS respondent in our main analytical sample (the rightmost column of table 2). The conditional variable in each regression is DataQuick's estimate of the value of the median home in the ZIP code at the end of the followup period. In the left column, the conditioning variables consist only of indicators for baseline year of interview and the variable indicated in the row title. In the right column, flexible controls for DataQuick's valuation estimate in the baseline year are added.

The first row, left column indicates that in 2006, houses were valued about 0.05 logs lower if their owners had owned them outright at the time of their baseline interview. This almost surely reflects the fact that housing valuation *levels* reflect any number of individual and community characteristics that would remain unobserved in our analysis. However, the first row, right column shows that after controlling flexibly for initial valuation, any subsequent *change* in valuation is uncorrelated with previous equity stake. If our identifying assumption holds, we should observe similar patterns for *any* relevant characteristic. Regardless of whether it is correlated with valuation levels in 2006, our identifying assumption is that any baseline characteristic should be uncorrelated with changes in valuation over the followup period.

We tested a total of 27 relevant characteristics using this approach, including those reported in table 1. When baseline values were *not* included in the regression, 23 of these 27 were correlated in a statistically and substantively significant manner with valuations at the end of the followup period. But then when flexible controls for baseline valuation were *added*, in every case the magnitude of this relationship diminished dramatically. In almost every case, the relationship between appreciation and baseline characteristics was statistically indistinguishable from zero.[21]

These results imply that housing markets are sufficiently effective in building information about the expectations of buyers and sellers into current prices. This process almost certainly takes time; even in very active markets, home sales are many times less frequent than, for example, sales of equities on a trading floor. Therefore, communities with similar prices on any given date may be converging to very different "long run" prices. The results in table 1 indicate that in these communities over this followup period, any lags between changes in buyers' and sellers' expectations and changes in observed transaction prices would not confound casual interpretations of our main results. Since this assumption is absolutely critical to the rest of our analyses, we take two further steps to shore it up.

1. In results not reported here, we repeat all of our analyses (including the placebo regressions) controlling not only for baseline price *levels*, but also for past rates of *change* in prices, going back up to a quarter century prior to baseline. If our results are confounded by lags in market adjustment, then including pre-baseline trends in prices should be expected to change our results. It does not.

2. Due to data limitations, the health outcomes we analyze are measured in terms of their

[21]The characteristics that were still statistically significantly correlated with 2006 valuation, even after introducing flexible controls for initial price, are all shown in the table: white race, education beyond high school, "poor" self-rated health (1 on the 5 point scale), respondents' expectations that housing values would appreciate, and having ever been diagnosed as hypertensive. The relationships in every case are very small, but nonetheless statistically distinguishable from zero. These may reflect types of forecastable errors in valuation that do not get arbitraged away, or they may reflect failures of even the very flexible functional specification we use for initial price. As an added precaution, we include all of these baseline characteristics in all the regressions reported in this paper.

prevalence in 2006; they do not represent change over the followup period. Most of these outcomes were not measured at the beginning of the followup period. Subject to our identifying assumption, initial levels of the outcomes would be balanced between the more rapidly and less rapidly appreciating communities, so that differences measured only at the end of the followup period would be sufficient to measure change over time. In results not reported, we used a handful of relevant outcomes that *were* measured at the beginning of the followup period, and compare analyses of outcome *levels* in 2006 to analyses of *change* in outcomes over the followup period. The two approaches yielded the same results, further implying that our identifying assumption is plausible.

4.2 Housing values, mortality, and attrition

Our main analyses begin with table 3, in which we test for relationships between local housing price appreciation and attrition from the sample (which, as we illustrate in figures 2 and 3, is mainly due to mortality). Our full regression specification is described in section 3.5.

Homeowners who resided in faster appreciating neighborhoods may have been slightly less likely to die during the course of followup, or to be lost to followup for other reasons. However, these effects are extremely poorly estimated, and are indistinguishable from zero at any traditional confidence level. This null result may simplify interpretation of the results in the rest of our analyses, which are done conditional on survival and successful followup.

4.3 Housing values and economic expectations

Table 4 examines the relationship between the evolution of housing values and the self-reported economic expectations and optimism of homeowners.

The first three columns analyze responses to questions specific to the economic situation of the respondents themselves. The first indicates that homeowners in faster appreciating neighborhoods assign a higher probability to the prospect that their income will keep up with their cost of living. Specifically, the results imply that moving across the interquartile range in terms of housing price appreciation rate increases the reported probability of staying ahead of inflation by about 2.5 percentage points, or one-twentieth of the average reported probability.[22] The second and third columns indicate that those homeowners in faster appreciating communities reported a higher probability that they would be in a position to transfer wealth to their children, either through bequest or during their lifetime.

The fourth column analyzes respondents' assessments of an aspect of the broader macroeconomy. In the spring and summer of 2006, only a year before the subprime mortgage crisis set off an implosion of the housing bubble, about a quarter of respondents thought a major economic depression had a 60 percent or better chance of occurring within the next decade. The results in the fourth column of table 4 indicate that those who were at the 75th percentile in terms of housing value appreciation assigned a nearly 3 percentage point lower probability to that outcome than those who were at the 25th percentile. Either the economy looked stronger to these individuals, or they rationalized the expanding bubble into an indication of strong fundamental growth. Notwithstanding this result, the evidence is mixed regarding the impact of housing price appreciation on optimism about the broader macroeconomy. In other analyses (not shown), we find no relationship between housing price appreciation and reported probabilities that the real value of a typical mutual fund would increase over the next decade, or that Congress would revise the Social Security system to make it less

[22] Nominal housing values in the 25th percentile of ZIP codes in terms of appreciation increased by about 0.29 logs over the followup period; those in the 75th percentile increased by about 0.96 logs. These rates of increase translate to growth rates of about 3.7% per year at the 25th percentile, and 9.0% per year at the 75th percentile. Inflation over the period from 1992 to 2006 averaged about 2.6% per year; thus, by the valuations we use, the real value of the houses of about 83% of the analytical sample increased over the period.

generous.

The fifth column in table 4 indicates that homeowners in the fastest appreciating neighborhoods reported lower likelihoods that "tomorrow" (at the time of the interview) would be a rainy day. With appropriate caution (discussed in greater detail in section 3), we interpret this to indicate that wealth accumulation or neighborhood change may have led these homeowners to feel more optimistic overall.[23]

4.4 Housing values and functional capacity

Table 5 examines the relationship between housing price appreciation and respondents' functional capacities. The first column relies on respondents' self reports of difficulties with activities of daily living. Sixty-two percent of respondents report a functional limitation of some kind, but moving up through the interquartile range in terms of housing value appreciation reduces the likelihood of having such a limitation by about 5.4 percentage points. The second column shows results from the externally measured indicator of functional capacity– the timed balance task. The conditional variable in the second column of table 5 takes the value 1 if the respondent successfully maintained his or her balance for the entire allotted time, and 0 otherwise. The results based on this measure of functional capacity corroborate those based on the self-report; 64 percent of respondents were able to complete the balance task successfully, and moving up through the interquartile range in terms of housing price appreciation increased this probability by 6 percentage points.

[23]This result is robust to restricting the analysis to only those homeowners still living in their baseline home, and also to inclusion of controls for the response to the baseline "sunny day" question. Nor does appreciation between 1992 and 2006 predict answers to the "sunny day" question in 1994. Taken together, these findings may lend credence to an (albeit cautious) interpretation of this result as an indication that housing value appreciation drives improvements to respondents' overall outlook.

4.5 Housing values, anthropometrics, and biomarkers

We next analyze the effects of appreciation on waist circumference and blood pressure in table 6. The first column indicates that moving up through the interquartile range in terms of housing price appreciation reduces waist circumference by about an inch. The next two columns indicate similar patterns for weight (whether or not we control for height). In the second column, the conditional variable is our dichotomous indicator of hypertension. Moving up through the interquartile range reduced the likelihood of hypertension by 5 percentage points. We interpret the hypertension result with due caution, however. It has a large standard error–only distinguishable from zero at a ten percent size of test–and we noted in table 1 that changes in valuations were already weakly correlated with diagnosed hypertension at the beginning of the followup period.

Taken together, the results in table 6 imply more rapidly increasing cardiovascular risk in communities where housing prices were more stagnant (or, equivalently, more rapidly improving health in communities where they were appreciating). We tested for evidence that this might be driven by changes in glucose metabolism or diet by examining whether concentrations of glycosylated hemoglobin or cholesterol levels covaried systematically with housing value appreciation. They did not.[24]

Central adiposity as measured by waist circumference is associated with excess cortisol (Anagnostis, Athyros, Tziomalos, Karagiannis, and Mikhailidis, 2009, Björntorp, 2001), and blood pressure has well established acute and chronic associations with stress (Steptoe, Brydon, and Kunz-Ebrecht, 2005). This may suggest that the observed differences in weight, waist circumference, and blood pressure may be the result of subtle differences in stress exposure, diet, or exercise that compound over the long term, even though they are not acute

[24]Glycosylated hemoglobin is an indicator of average blood sugar levels over a period of several weeks to a few months. Poorly managed diabetes is associated with high quantities of glycosylated hemoglobin, as are other diet and exercise related factors that are detrimental to cardiovascular health.

enough to affect less sensitive short term or medium term outcomes like blood sugar levels or cholesterol.

5 Extensions

5.1 Exploring Sources of Impact: Wealth effects and local improvements

The effects of housing price appreciation are likely driven by at least two types of mechanisms– the first, because appreciation augments the wealth of home owners, and the second, because appreciation itself may be a summary indicator of a broad array of local improvements. In this section, we take a first pass at differentiating between effects driven by these two types mechanisms.

We stratify our main analyses based on the fraction of respondents' wealth that was represented by their houses at their baseline interview. While recognizing that the composition of respondents' asset portfolios almost certainly reflects any number of unobserved characteristics, we note that price appreciation represents a more important form of wealth augmentation for those who rely more on their houses as their primary store of wealth. Therefore, we speculate that effects operating more through the first type of mechanism should be concentrated primarily on those holding asset portfolios weighted more heavily toward housing, whereas effects driven more by the second type of mechanism should be felt more evenly by residents across the community, regardless of the composition of their wealth portfolio.

Interpretation of the stratified analyses may nonetheless be complicated by the obvious fact that asset portfolios are endogenously determined, which means we cannot to rule out the possibility that any differences in effect between the strata may be mediated not by the portfolio composition itself, but rather by some unobserved characteristic that *covaries with* portfolio composition. Nonetheless, we will interpret the results (albeit with appropriate caution) as potentially illustrative of the different roles played by wealth itself and by improvements in local conditions.

5.1.1 Expectations

The first column in table 7 disaggregates the result from the first column in table 4. Housing price appreciation had a powerful impact on the economic optimism of those respondents whose wealth portfolios were weighted very heavily toward their houses. In this stratum, moving up through the interquartile range in terms of housing price appreciation increases reported expectation that their income would keep pace with inflation by 8 percentage points, or one sixth of the average reported probability. By contrast, price appreciation had substantially and significantly smaller impacts on the economic optimism of those for whom one expect it to be a less salient source of wealth augmentation.

The other indicators of expectations regarding personal economic circumstance from table 4 (bequeathing and transferring wealth to offspring) show similar patterns, although in the interest of parsimony they are not shown in the table.

By contrast, the second column disaggregates the "rainy day" result in the last column of table 4. The point estimates imply that the effect of housing price appreciation on this outcome declines with the fraction of a respondent's wealth portfolio that is represented by housing; however, the differences are small, and are not individually or jointly significant at any traditional confidence level. Insofar as this outcome represents an improvement in respondents' overall outlook, it may be driven more by improvements in local conditions that are *reflected* in price appreciations, rather than the price appreciation *itself*. In that case, it would not necessarily be surprising that effects on this outcome are more widely shared than more specifically economic outcomes like the one in the first column.

5.1.2 Functional limitations

In analyses not shown in the interest of parsimony, we decomposed the effects reported in table 5 based on the fraction of respondents' wealth portfolio represented by their homes. We found no evidence that the effects vary systematically across the strata. Taken together with the results in table 5, these results may imply that neighborhood improvement in itself (rather than the wealth-augmenting effects it can have for homeowners) play an important role in slowing the rate of functional decline.

5.1.3 Anthropometrics and biomarkers

In table 8, we decompose the effects of price appreciation on cardiovascular outcomes based on the share of respondents' wealth portfolio represented by their homes. The first column indicates that with regard to weight gain, homeowners benefited from housing price appreciations while renters did not. The second column indicates a similarly graded pattern for the effects on hypertension. The health-improving effects of house price appreciation were concentrated on those who were relied heavily on their homes as a store of wealth. Moving up through the interquartile range in terms of housing price appreciation cut the probability of hypertension nearly in half for those who began the followup period holding 95 percent or more of their wealth in their homes. By contrast, it had no appreciable impact on homeowners who were less reliant on their houses as a store of wealth, or on renters.

The results in the first two columns of table 8 imply that mechanisms relating to wealth augmentation may play an important role in driving the cardiovascular effects of housing price appreciation. Among these types of mechanisms are ones that involve financial stress. The third and fourth column sheds further light on the potential role played by (possibly financial) stress-mediated mechanisms. Respondents were asked four questions about their quality of sleep, including how frequently they had trouble falling asleep at night, and how frequently awoke too early in the morning. The conditional variable in the third column of table 8 is binary; it takes a value 1 if a respondent said any of the four indicators of poor sleep happened "most of the time," and 0 otherwise. Using this metric, about 40 percent of respondents in the analytical sample had poor sleep (most often, because they reported that "most of the time," they woke during the night and had trouble falling back to sleep). Housing price appreciation has no effect on sleep quality among homeowners, but significantly reduces sleep quality among renters. Moving up through the interquartile range in terms of housing price appreciation increases the likelihood of reporting poor sleep quality by 10 percentage points. Appreciating housing values may represent a challenge for renters, to the extent that it is reflected in rising rent and other costs of living.

Elsewhere in the survey, respondents were taken through a series of statements, and were asked to indicate (using "yes" or "no") whether each was true "for much of the time during the past week." The conditional variable in the fourth column of table 7 is binary; it takes a value of 1 if the respondent answered "yes" to any of the following statements: "You felt depressed"; "You felt sad"; "You could not get going." Otherwise, it took a value of 0. By this metric, 28 percent of respondents in the analytical sample experienced negative feelings "much of the time" over the previous week. Appreciation in housing values, however, significantly reduced the likelihood of these feelings for those who relied more heavily on their houses as a store of wealth. It was substantially and significantly less protective for renters or those homeowners who were less reliant on their houses.

5.2 Comparing self-reported and externally assessed valuations

In this study, we have constructed an original dataset from complementary sources, combining private sector market valuations of housing assets with the extensive health and economic information available from the Health and Retirement Survey. The HRS itself includes a question (posed only to homeowners) on the market valuation of respondents' houses.[25] It remains an empirical question whether the source of the valuation estimate has any effect on our results. In this subsection, we explore that question.

One important difference between the private sector and self-reported valuation estimates is that the latter are only reported by *current* homeowners, about their *current* homes. Therefore, self-reported price appreciation can only be measured for those respondents who remained in the same home throughout the followup period. As figure 4 illustrates, our analytical sample is highly mobile, a pattern typical for Americans around retirement age. For example, about two fifths of the analytical sample from the core HRS study cohort had moved from their baseline homes over the course of followup.

In the first and third columns of table 9, we disaggregate the relationship between housing value appreciation and functional limitations, by stratifying the analyses reported in table 5. The top row indicates the effect of housing price appreciation among those who remained in the same home throughout the followup period, while the second row indicates the effect among those who moved out of their homes during the follow-up period. Since we are almost certainly stratifying on an endogenous variable, we do not necessarily attribute observed heterogeneity to the move itself. This stratification is necessary only because it allows us to compare against the second and fourth columns, where we estimate the effect of *self-reported*

[25] The specific wording of the question is as follows: The respondent is first asked if he or she owns the home. For those who answer yes, a followup question is: "What is its present value? I mean, what would it bring if it sold today?" In the context of the questionnaire, there is no ambiguity that the pronoun "it" refers to the home.

housing value appreciation.

Comparing the first row in the first and second columns, we observe that the effect of appreciation on self reported functional limitations is very similar, regardless of whether appreciation is measured using the self reported or the DataQuick valuation estimates. By contrast, comparing the first row in the third and fourth columns, we observe that for externally measured limitation, the choice of valuation measure has a very important impact on the estimated effect.

In further analyses (not shown here), we observe this pattern consistently. For self-reported measures of health (negative feelings, restless sleep, self-rated health), estimated effects of appreciation were as strong or stronger when measured using self-reported valuations, as compared with the DataQuick valuations. For the externally assessed measures of health (waist circumference, measured blood pressure), the opposite is true. Deeper exploration of these patterns is beyond the scope of this paper, but is a subject of ongoing analysis.

6 Conclusion

Our results indicate impacts of long-run changes in housing prices on an array of stress-related health outcomes. Comparing homeowners in late middle age and beyond who lived in more rapidly appreciating communities to those who lived in the same metropolitan area, but in communities that were more price stagnant, we find that those in the former group report higher levels of economic optimism–and perhaps a more positive "general" outlook as well. They reported higher levels of functionality in their activities of daily living, and displayed higher levels of functionality in an externally measured task, administered under controlled conditions. They were relatively protected from externally measured cardiovascular risk factors.

Most of these effects were graded; housing price appreciation most powerfully improved indicators of economic expectations, sleep quality, more general indicators of "affect," and cardiovascular risk factors for those who relied most heavily on their houses as a store of wealth. This may indicate that wealth augmentation driven by price appreciation itself may play a role–rather than solely the local improvements that drive price appreciations. A few of the effects, however, were not significantly graded; appreciation had similar effects on functional outcomes and responses to the "rainy day" question (arguably, an indicator of general "affect"), regardless of how heavily respondents relied on their houses as a store of wealth. This pattern may imply that local improvements reflected in the price played a more important role in driving the effects on these outcomes.

Furthermore, our results, taken with previous findings relating wealth outcomes to health outcomes, illustrate important potential nuances in these relationships. Housing wealth is highly illiquid, especially when compared with financial wealth invested in stock portfolios, or wealth that may be bequeathed. It also has been observed to function differently from more liquid forms of wealth–for example, it is an important buffer against economic shocks (Venti and Wise, 2000, 2001; Walker, 2004). Home ownership may represent an form of precautionary saving, whereas financial capital may be more important as a source of non-labor income. These differences in the roles that different types of wealth may play could be an important part of the explanation for differences between our results and those reported in Smith (2004, 2007), for example.

Our results point to several directions for future research. While we have provided some preliminary explorations, further innovation will be necessary to disentangle the wealth effect of price change from the effects of changes in local conditions which *drive* these price changes.

We have observed that a sustained medium-run appreciation in housing values has important impacts on economic expectations, and health. This raises the question about whether shorter run changes in housing values like the sharp decline associated with the implosion of the housing bubble should be expected to have analogous effects to those reported here. As more recent waves of HRS data become available covering the period after the implosion, we will be able to examine this question in more detail.

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Tables and Figures

Table 1: Placebo regressions

Conditional variable: 2006 valuation (each cell is a separate regression)		No control for baseline valuation	Flexible control for baseline valuation
Personal **baseline** characteristics	Owned house outright	-0.05 [0.02]	-0.007 [0.007]
	Had $\geq \frac{2}{3}$ equity stake in house	-0.007 [0.01]	-0.005 [0.007]
	Says major macro-economic depression likely	-0.05 [0.01]	-0.008 [0.007]
	Says local housing appreciation likely	0.06 [0.02]	0.02 [0.01]
	Says social security likely to become less generous	0.02 [0.01]	0.000 [0.007]
	"Poor" self-rated health ("1" on 5 point scale)	0.11 [0.01]	0.02 [0.01]
	Difficulty with ≥ 1 ADL	-0.05 [0.01]	-0.01 [0.01]
	Exercises regularly	0.04 [0.01]	0.003 [0.006]
	Accurately subtracted 7s 5 times	0.07 [0.02]	0.00 [0.01]
	Accurately recalled 7 words after short delay	0.06 [0.02]	0.009 [0.008]
	Accurately recalled 5 words after longer delay	0.05 [0.02]	0.002 [0.012]

Table 1 continues on the next page...

Table 1–continued

Conditional variable: 2006 valuation (each cell is a separate regression)		No control for baseline valuation	Flexible control for baseline valuation
Diagnosed conditions (baseline)	Diabetes	-0.08	-0.00
		[0.02]	[0.01]
	Arthritis or	-0.03	-0.00
	rheumatism	[0.01]	[0.01]
	Hypertension	-0.05	-0.02
		[0.01]	[0.01]
	Heart	-0.01	-0.01
	condition	[0.02]	[0.01]
	Lung	-0.04	-0.006
	condition	[0.02]	[0.013]
Fixed characteristics	Height	0.005	0.001
	(inches)	[0.002]	[0.001]
	White	0.27	0.05
	race	[0.03]	[0.02]
	Educ:	0.10	0.02
	$\geq$ 12 years	[0.02]	[0.01]
	Educ:	-0.09	-0.01
	$<$ 12 years	[0.02]	[0.01]

Notes for table 1: Each cell in this table reports on a different regression. The regression in the left column of each row differs from the one in the right column of the same row in exactly one respect: the one on the right includes flexible controls for DataQuick's estimate of detached house values in the ZIP code, whereas the left column includes no controls for baseline valuation. Each regression also includes fixed effects at the level of the metropolitan area, and each regression sample consists of the 5255 respondents in the first column from the right in table 2. For more details, see text.

Table 2: Comparing subsamples

		All Respondents	Born 1924-1960	Areas with price info	Detached house owner	Completed followup	External measures
Birth year	*median*	1934	1938	1938	1938	1940	1940
	IQR	1923 1942	1932 1945	1932 1945	1932 1945	1934 1947	1933 1947
Education	<12 *years*	0.31	0.26	0.23	0.18	0.13	0.12
	>12 *years*	0.37	0.41	0.45	0.49	0.55	0.57
Total Wealth (1000s of $)	*median*	21	25	28.2	50	58.5	64.8
	IQR	1 110	1.5 120	2 130	8 174	10.2 189	12.5 182
Self-rated health	*median*	Good	Good	VG	VG	VG	VG
	IQR	VG Fair	VG Good	VG Good	Exc Good	Exc Good	Exc Good
Mortality by 2006		0.30	0.16	0.16	0.14	0	0
N		28,943	21,189	12,764	8522	5255	2422

Notes for table 2: This table reports differences in terms of basic demographic characteristics between the overall HRS sample and the analytical sample for this study (represented in the right two columns). The columns reflect the bases on which the analytical sample is selected–HRS respondents who are owners of detached houses, and who were born between 1924 and 1960; with the exception of mortality regressions, inclusion in the analytical sample also requires survival and successful followup all the way to the 2006 wave. The difference in sample size between the penultimate column and the last primarily reflects the fact that external measures (like anthropometrics, pulse rate, and other biomeasures) were only collected for a randomly selected half of respondents.

Table 3: Follow-up and housing value appreciation

Conditional variable: As given in column header	Respondent died before 2006	Lost to follow-up in 2006 (death/refusal)	Lost **or** incomplete data in 2006
Log, 2006 valuation	-0.02 [0.02]	-0.03 [0.03]	-0.04 [0.03]
Fraction lost/died	*0.12*	*0.28*	*0.38*

Notes for table 3: This table reports one of the coefficients from each of three linear probability models; conditional variables are as given in the column header. In addition to those reported in the table, conditioning variables in each regression also include metropolitan area fixed effects, and flexible controls for birth year, race, baseline interview year, sex, and house valuation, non-housing wealth, and housing and non-housing debt in the year of the baseline interview, as well as self-reported health in the year of the baseline interview. Each regression sample consists of the 8522 respondents in the third column from the right in table 2.

Table 4: Expectations and housing value appreciation

Conditional variable: Reported prob of event in col header	Income will keep up with inflation	Bequeath $\geq \$10,000$	Give help totalling $\geq \$5000$	Major econ depr next decade	Tomorrow will be a rainy day
Log, 2006 valuation	3.7 [2.0]	3.8 [2.1]	10.2 [2.9]	-4.2 [1.8]	-7.8 [2.2]
Average reported probability	*45.6*	*82.3*	*40.8*	*43.7*	*33.7*

Notes for table 4: This table reports one of the coefficients from each of five linear probability models; conditional variables are as given in the column header. In addition to those reported in the table, conditioning variables in each regression also include metropolitain area fixed effects, and flexible controls for birth year, baseline interview year, sex, and house valuation, non-housing wealth, and housing and non-housing debt in the year of the baseline interview. Each regression sample includes the 5255 respondents represented in the first column from the right in table 2. For more details, see section 3 in the text.

Table 5: Functional outcomes and housing value appreciation

Conditional variable: As given in column header	Difficulty with ≥ 1 ADL	Success with balancing task
Log, 2006 valuation	-0.07 [0.03]	0.09 [0.04]
Fraction $y = 1$	*0.62*	*0.64*
N	*5255*	*2206*

Notes for table 5: This table reports one of the coefficients from each of two linear probability models; conditional variables are as given in the column header. The regression specification and sample are the same as in table 4. The sample in the second column is smaller because the biomeasures module was only administered on a randomly selected half of respondents. For more details, see section 3 in the text.

Table 6: Cardiovascular outcomes and housing value appreciation

Conditional variable: As given in column header	Waist circ (inches)	Weight (lbs)	Weight (control for height)	Hypertension (measured or diagnosed)
Log, 2006 valuation	-1.6 [0.57]	-8.9 [4.0]	-9.4 [3.9]	-0.077 [0.046]
Average y	*39.1*	*0.57*	*179*	*176*
N	*2422*	*2422*	*2407*	*2422*

Notes for table 6: This table reports one of the coefficients from each of four linear regressions; conditional variables are as given in the column header. The regression specification is the same as in table 4. For more details, see section 3 in the text.

Table 7: Effect heterogeneity: fraction of wealth in home

Conditional variable: As given in column header		Income will keep up with inflation	Tomorrow will be a rainy day
	≥ 95% wealth in house	12.3 [5.1]	-11.1 [4.9]
Log, 2006 valuation	≥ 95% wealth in house	1.4 [2.4]	-7.5 [2.7]
	did not own house	3.9 [4.8]	-4.7 [5.8]
Δ: (≥ 95% - < 95%)		*10.8* *[5.7]*	*-3.5* *[5.9]*
Δ: *(did not own - < 95%)*		*2.5* *[5.2]*	*2.8* *[6.3]*
N (≥ 95% in house)		*1041*	
N (< 95% in house)		*4214*	
N (did not own house)		*826*	

Notes for table 7: This table reports selected coefficients from each of two linear probability models; conditional variables are as given in the column header. The categories down the rows are defined based on the fraction of net worth represented by housing equity at the time of the baseline interview. For more details, see section 5 in the text.

Table 8: Effect heterogeneity: fraction of wealth in home

Conditional variable: As given in column header		Waist circ (inches)	Hypertension (measured or diagnosed)	Poor sleep quality	Negative feelings past week
	$\geq$ 95% wealth in house	-1.6 [1.5]	-0.37 [0.10]	0.05 [0.08]	-0.17 [0.07]
Log, 2006 valuation	< 95% wealth in house	-1.5 [0.6]	-0.05 [0.05]	-0.04 [0.04]	-0.03 [0.03]
	did not own house	1.2 [1.3]	0.12 [0.12]	0.15 [0.07]	0.03 [0.1]
Δ: ($\geq$ 95% - < 95%)		*0.05* *[1.5]*	*-0.32* *[0.11]*	*0.10* *[0.09]*	*-0.15* *[0.07]*
Δ: *(did not own - < 95%)*		*2.8* *[1.3]*	*0.16* *[0.12]*	*0.19* *[0.08]*	*0.05* *[0.08]*
N ($\geq$ 95% in house)		*460*		*1041*	
N (< 95% in house)		*1962*		*4214*	
N (did not own house)		*398*		*826*	

Notes for table 8: This table reports selected coefficients from one linear regression and each of three linear probability models; conditional variables are as given in the column header. The categories down the rows are defined based on the fraction of net worth represented by housing equity at the time of the baseline interview. For more details, see section 5 in the text.

Table 9: Effect heterogeneity: movers and stayers

Column indicates conditional variable (and measure of housing price appreciation)		ADL difficulty *(DataQuick valuation)*	ADL difficulty *(Self-reported valuation)*	Successful balancing task *(DataQuick valuation)*	Successful balancing task *(Self-reported valuation)*
Log, 2006 valuation	stayers	-0.08 [0.04]	-0.06 [0.03]	0.11 [0.05]	-0.00 [0.03]
	movers	-0.06 [0.05]		0.10 [0.07]	
Number of stayers		*3148*		*1332*	
Number of movers		*2107*		*874*	

Notes for table 9: This table reports one coefficient from each of four linear probability models. The categories down the rows are defined based on whether or not a respondent moved out of his or her baseline home at any point during the followup period. In the odd columns, the conditioning variables include ZIP-code specific estimates of housing values provided by DataQuick for the beginning and end of the followup period. In the even columns, they are the valuation estimates reported by resondents themselves. These latter data are only available for those respondents who remained in their homes over the entire followup period, since movers were not asked about the current value of their former homes. For more details, see section 5 in the text.

Figure 1: External valuation estimates and self-reported housing values at baseline

Notes for figure 1: Depending primarily on birth year, baseline interviews occurred in 1992, 1998, or 2004; home owners were asked to estimate the sales value of their houses (for specific wording, see footnote 25). The estimate of the sales value of the median home reported by the DataQuick for each respondent's ZIP code at the time of the baseline interview is plotted along the x-axis; each data point indicates the self-reported estimate of valuation and this external estimate. The dashed lines indicate sample averages, and the dash-dotted line represents the 45 degree line. The solid line is a splined linear regression with notches at the quintiles. This regression generates an R^2 of 0.53. There are 5091 data points, each corresponding to the owner of a detached house who was born between 1924 and 1960 and who reported a value estimate for their homes. Some respondents said they did not know the value, and refused to even estimate a range of possible values.

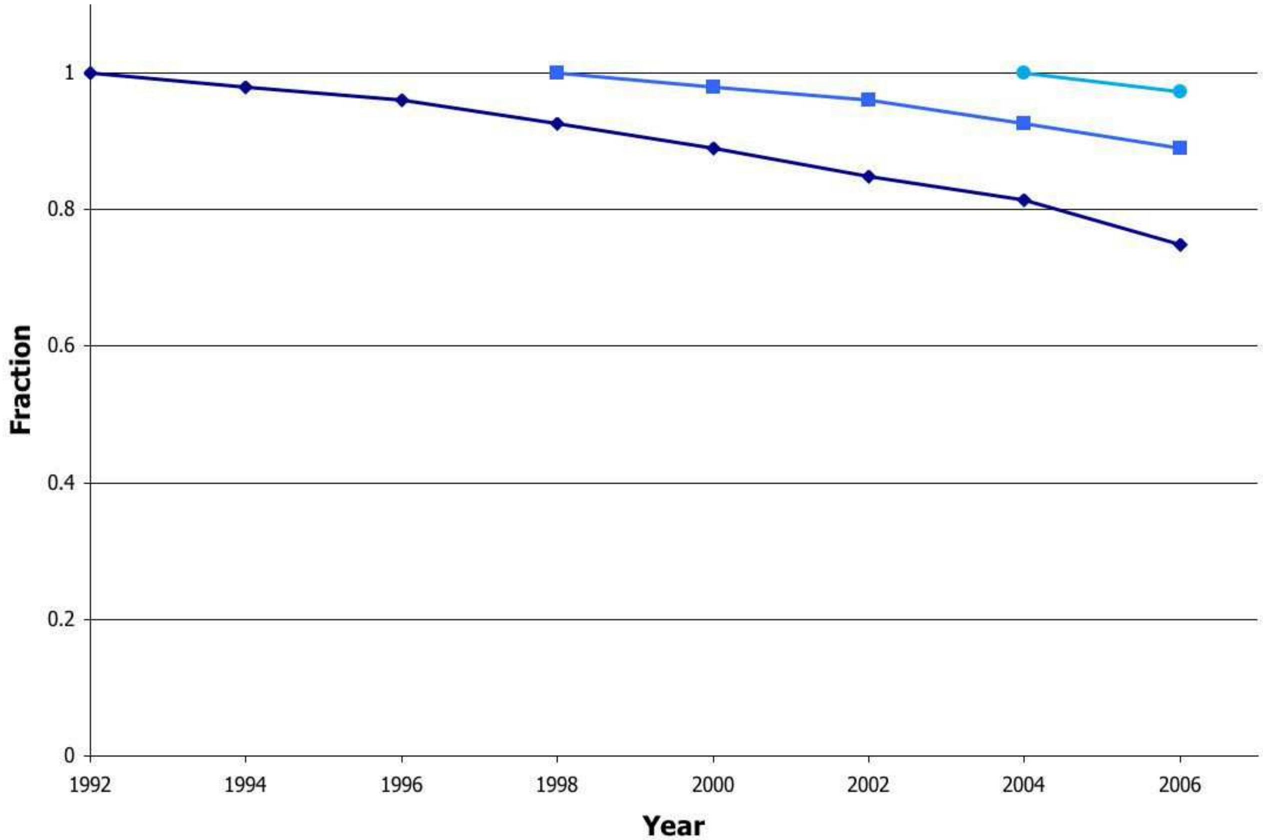

Figure 2: Survival of sample respondents

Notes for figure 2: Depending primarily on birth year, baseline interviews occurred in 1992, 1998, or 2004; each study cohort was reinterviewed every two years. For each study cohort, the lines in this graph indicate the fraction still alive in each followup interview year. The sample includes only owners of detached houses, born between the years 1924 and 1960.

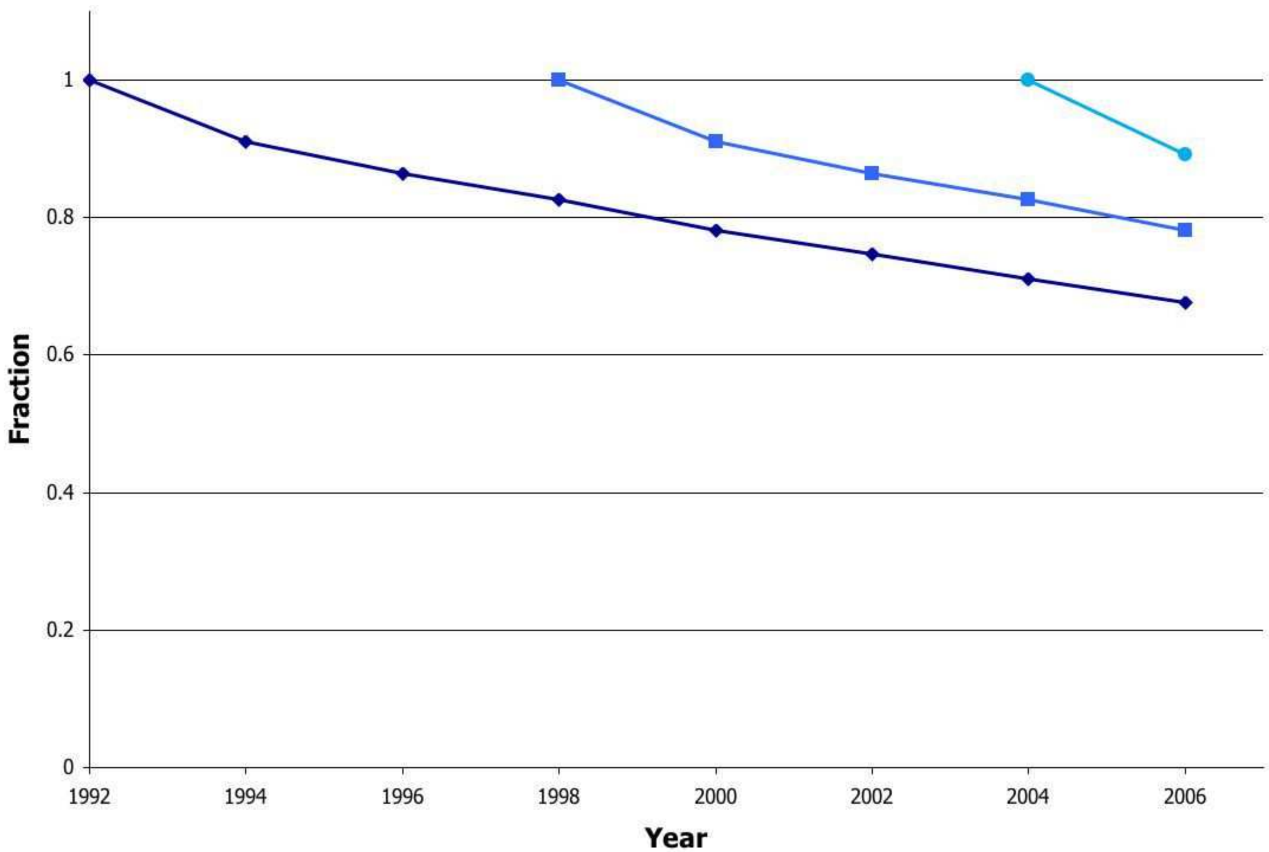

Figure 3: Reinterview rates of sample respondents

Notes for figure 3: Depending primarily on birth year, baseline interviews occurred in 1992, 1998, or 2004; each study cohort was reinterviewed every two years. For each study cohort, the lines in this graph indicate the fraction retained in the study in each followup interview year. Loss to followup occurred primarily because of death or respondent refusal. The sample includes only owners of detached houses, born between the years 1924 and 1960.

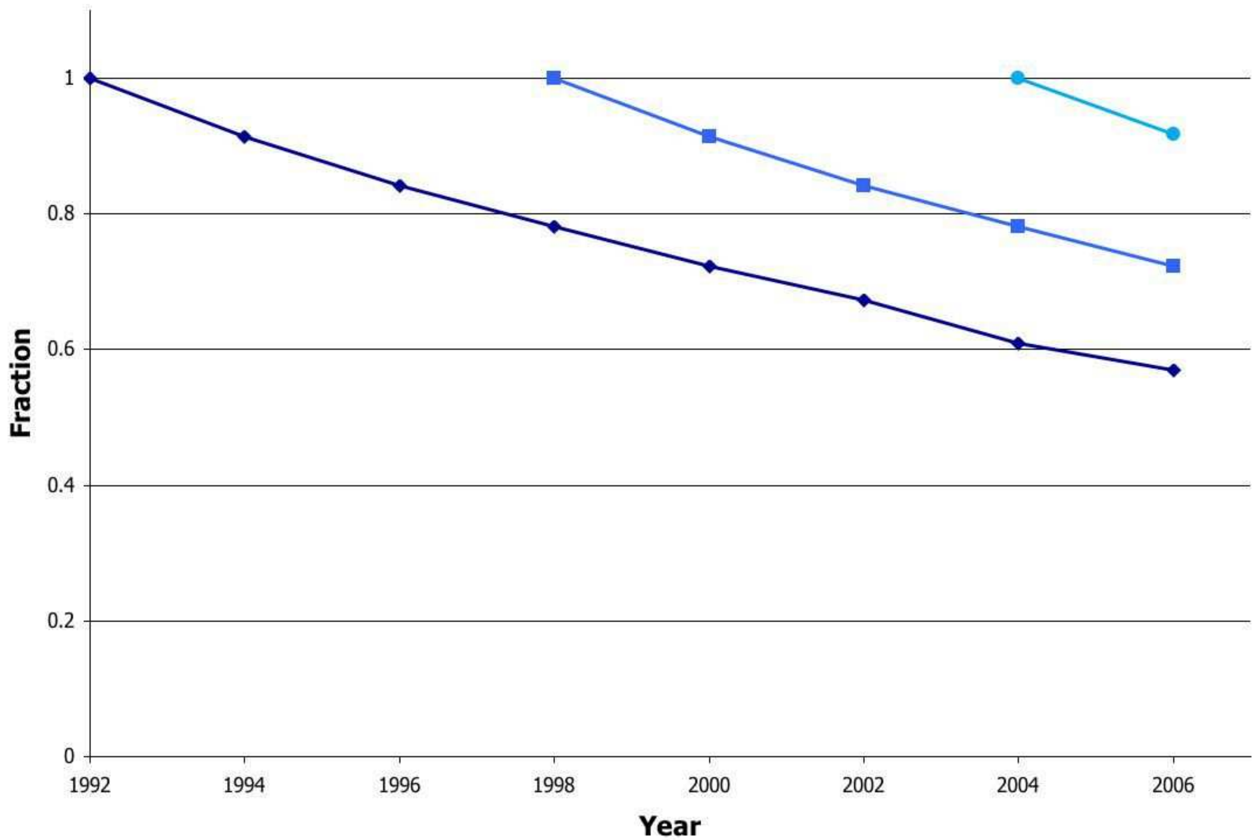

Figure 4: Persistence of sample respondents living in baseline home

Notes for figure 4: Depending primarily on birth year, baseline interviews occurred in 1992, 1998, or 2004; each study cohort was reinterviewed every two years. For each study cohort, the lines in this graph indicate the fraction of the analytical sample still living in the same house where they were living at the baseline interview. The sample includes only owners of detached houses born between the years 1924 and 1960, who were living in communities where external valuation if housing assets was available, and who were retained in the study up to the end of the followup period.