Does Your Home Make You Wealthy?

Alexandra Killewald* and Fangsheng Zhu

Harvard University

DRAFT – NOT FOR CITATION OR CIRCULATION

* Draft prepared for the 2015 annual meeting of the Population Association of America. Direct all correspondence to Alexandra Killewald, 436 William James Hall, Harvard University, 33 Kirkland St, Cambridge, MA 02138; killewald@fas.harvard.edu; 617-495-3818.

Abstract. Wealth inequality in the United States is vast, and homeownership is hypothesized to be one key mechanism by which wealth accumulates unequally. Evaluating the effect of homeownership on later-life wealth is challenging, however, because prior wealth affects transitions to homeownership, at the same time that homeownership in turn is expected to affect subsequent wealth. Thus, conventional regression models that predict current wealth as a function of prior homeownership are likely to overestimate the causal effect of homeownership. We provide a more rigorous estimate of the effect of homeownership on later-life wealth by using NLSY79 data and inverse probability of treatment weights to model dynamic selection processes into and out of homeownership across the life course. We find that, as expected, conventional regression models overstate the effect of homeownership patterns on midlife wealth, by almost 20 percent. However, even after adjustment, we find that each additional year of homeownership is associated with an increase in midlife wealth of about $8,000. We also find that the wealth benefits of homeownership are highly heterogeneous by race and ethnicity; while whites benefit almost $12,000 for each additional year of homeownership, the benefit is only $8,000 for Hispanics and less than $5,000 for blacks. Thus, our results suggest that black and Hispanic Americans are doubly disadvantaged in wealth accumulation by homeownership processes: they are both less likely to be homeowners, even net of other characteristics, and they experience smaller wealth returns from each year of homeownership. Our results confirm that homeownership is an important mechanism for both wealth accumulation and the construction of racial and ethnic disparities in wealth holdings.

In the United States, net worth is a highly unequally distributed resource (Budrıa, Diaz-Giménez, Quadrini and Rios-Rull 2002; Piketty 2014), with strong persistence across generations (Charles and Hurst 2003) and massive racial disparities (Kochhar, Fry and Taylor 2011; Oliver and Shapiro 2006). Wealth disparities are consequential because wealth facilitates a variety of life chances, including marriage (Charles, Hurst and Killewald 2013; Schneider 2011), health (Smith 1995), and children's educational and labor market outcomes (Conley 1999; Conley 2001a; Orr 2003).

Homeownership is hypothesized to be a key mechanism for asset accumulation and, therefore, for the construction and reproduction of asset inequalities. Estimating the contribution of homeownership to wealth at midlife, however, poses substantial methodological and conceptual challenges, because wealth is itself a determinant of transitions to homeownership (Charles and Hurst 2002). The positive association between homeownership and wealth, therefore, may merely reflect that wealthier individuals are more likely to purchase (and keep) homes. Conventional regression models that estimate the association between current wealth and homeownership (or a history of homeownership) are therefore likely to overestimate the casual role of homeownership in wealth accumulation.

We produce a more accurate estimate of the effect of homeownership patterns on midlife wealth, incorporating how prior wealth shapes transitions to homeownership and the likelihood of remaining a homeowner across the life course. If homeownership itself does not cause wealth accumulation (compared to alternative uses of the same starting wealth), then it cannot be a source of asset gaps by race or the intergenerational transmission of wealth, although it is certainly an indicator of racial and socioeconomic disparities. On the other hand, if homeownership is wealth-generating, we can estimate both race differences in the estimated benefits of homeownership and explore how the wealth gap by race at midlife would change if homeownership trajectories did not diverge by race.

THEORETICAL FRAMEWORK

Social scientists often study wealth in much the same way we study point-in-time life outcomes, like income, with a collection of cross-sectional measures of predictors. For example, it is common to estimate models of current net worth that consider how current traits of individuals – such as income, race, and education, and potentially the traits of their family – are associated with net worth (Barsky, Bound, Charles and Lupton 2002; Conley 1999; Keister 2003; Killewald 2013; Yamokoski and Keister 2006). Such an approach, however, is not well suited to the study of wealth, which is a measure of accumulated resources – a product of long-ago experiences as well as current circumstances. Furthermore, wealth is a cumulative advantage process: an individual's current wealth is affected not only by her history of wealth determinants, like income and education, but by previous levels of wealth itself (DiPrete and Eirich 2006). As a result, wealth gaps magnify across the life course, particularly by race (McKernan, Ratcliffe, Steuerle and Zhang 2013).

Thus, the study of wealth is inherently the study of wealth accumulation. Individuals' current wealth holdings are the product of an unfolding set of pathways by which new resources are set aside in assets and previous assets increase in value. Previous research examining wealth growth has considered the effects of portfolio composition, savings rates, income, and inheritances (Conley 2001b; Gittleman and Wolff 2004). Yet, for most Americans, home equity is the largest component of household wealth (Gittleman and Wolff 2004). Homeownership itself need not affect net worth: if the returns to housing investments are the same as to other uses of the same financial capital, then homeownership will change the form in which the asset is held, but not net worth. However, homeownership may be asset-promoting if the returns to housing are higher than for other investments. Furthermore, the rise of rental prices (Collinson 2011) may have made homeownership a less expensive option than renting, increasing disposable income that can be set aside for savings. Monthly mortgage payments might even act as a commitment device that encourages saving. As a result, homeownership is often conceptualized as a key pathway by which wealth accumulation occurs. Housing markets are also a key site for the generation of race gaps in wealth (Oliver and Shapiro 2006), as blacks are less likely to own homes, are at higher risk of return to renting, and experience fewer housing upgrades (Boehm and Schlottmann 2004; Horton 1992).

Despite homeownership's key position in Americans' asset portfolios and hypothesized pathways of wealth accumulation, evaluations of the unique effect of homeownership on wealth accumulation are rare (Di, Belsky and Liu 2007), and housing wealth, like net worth more generally, is rarely considering in dynamic context (Boehm and Schlottmann 2008). In part, this limitation stems from the challenge of modeling causal relationships in dynamic, cumulative advantage processes. Although homeownership is hypothesized to affect wealth, wealth is also a predictor of subsequent home purchase (Charles and Hurst 2002). Thus, a cross-sectional examination of the association between current wealth and cumulative homeownership does not reveal the effect of home purchase on subsequent wealth, but will be confounded with the selection into homeownership on the basis of previous wealth. Prior research that has examined the association between cumulative years of homeownership and wealth suffers from this limitation as well (Shapiro, Meschede and Osoro 2013; Turner and Luea 2009).

Recognizing this limitation, Boehm and Schlottmann (2008) take a dynamic approach to the study of housing wealth accumulation. They estimate transition probabilities across housing statuses for individuals over a nine-year period and then estimate the housing wealth accumulation experienced by race and income subgroups, in comparison to nonhousing wealth accumulation for the same groups. They find that, for all groups, housing wealth accumulation is larger than nonhousing wealth accumulation. These results are descriptively informative about the sources of wealth accumulation for different groups, but do not answer the question of whether, for example, an individual randomly prohibited from purchasing a home would experience lower total wealth growth than an otherwise similar individual who purchased a home.

Di, Belsky and Liu (2007) seek to estimate the causal effect of homeownership on net worth over a twelve-year period, controlling for both prior wealth and the household's wealth accumulation in the five prior years prior to the observation window. In this way, their analysis both adjusts for the effect of wealth on transitions to homeownership and also seeks to control for unobserved differences between households that may be correlated with both homeownership and their tendency for wealth accumulation. They find that time spent in homeownership has a substantial and positive effect on net worth at the end of the period.

Our approach seeks to address the same concerns raised in Di, Belsky and Liu (2007), but with greater attention to patterns of homeownership within the observation period. Di et al. control only for circumstances prior to the twelve-year window of wealth accumulation and average circumstances within the window (average income, total inheritance received during the period, percent of period spent married, etc.). This represents an improvement on prior research, but does not consider the evolving nature of individuals' circumstances across the period studied.

For example, covariate values at the end of the period may themselves be in part a function of homeownership status earlier in the period. For example, homeownership is consistently associated with a diminished risk of divorce (Cooke 2006; Ono 1998; South 2001), suggesting that the percent of the period spent married may be endogenous with prior homeownership. As a result, the cumulative effect of homeownership on wealth may be *underestimated*, because a portion of the pathway by which homeownership has its effects may be controlled away. This illustrates that standard regression models cannot properly account for the ongoing, reciprocal relationship between homeownership and other wealth-related characteristics. If these factors are controlled, homeownership's effect is likely to be understated, but if they are ignored the estimate of homeownership's effect is likely to be biased upward.

We conceptualize individuals as exposed to a trajectory of homeownership, with the cumulative experience potentially affecting midlife wealth. Incorporating the full history of homeownership experiences is particularly important because transitions from homeownership back to renting are nontrivial, particularly for low-income and minority households (Boehm and Schlottmann 2008). Using marginal structural models and inverse probability of treatment weights, we account for the fact that homeownership both affects and is affected by other family characteristics, including previous wealth. Our approach will provide the most accurate estimate to date of the cumulative effect of homeownership on adults' wealth outcomes.

We also test the possibility that whites receive a larger wealth return from homeownership than do blacks and Hispanics. Racial segregation in the United States is severe (Massey and Denton 1993), and there is some evidence that homes in predominantly minority neighborhoods experience slower rates of appreciation (Oliver and Shapiro 2006). Thus, racial and ethnic minorities may be disadvantaged both by spending less time as homeowners and by receiving smaller benefits when they own their homes.

DATA AND METHODS

To illustrate the challenges of estimating the cumulative effect of homeownership on midlife wealth, consider only the relationship between homeownership status and wealth across time. As discussed previously, wealth in a given period is likely to affect homeownership decisions. Therefore, in any given year, wealth in previous periods should be considered a spurious factor (that has its own effect on later wealth) and should be controlled out. Yet wealth in later periods will be in part the product of earlier homeownership decisions and, thus, controlling for interim wealth values would control away a portion of the indirect effect of homeownership on midlife wealth that operates via short-term wealth gains. Thus, conventional regression models cannot appropriately account for the endogeneity of homeownership in dynamic context: either controlling or not controlling for wealth in earlier periods will lead to a biased estimate of the cumulative effect. The same logic applies to other covariates that may also be determined in part by prior homeownership and shape subsequent homeownership decisions.

Marginal structural models offer an appropriate way to account for the intersecting causal relationships in dynamic processes (Robins, Hernán and Brumback 2000). Applications have been most common in the epidemiological literature (Hernán, Brumback and Robins 2000; Mortimer, Neugebauer, van der Laan and Tager 2005; Sato and Matsuyama 2003; VanderWeele 2009) and, within sociology, in the study of crime (Sampson, Laub and Wimer 2006; Sharkey and Sampson 2010) and neighborhood or classroom disadvantage (Lauen and Gaddis 2013; Sampson, Sharkey and Raudenbush 2008; Sharkey and Elwert 2011; Wodtke, Harding and

Elwert 2011). We apply this novel method to a new domain: the effect of homeownership on wealth.

To account for the dynamic process by which individuals select into and out of homeownership, we use inverse probability of treatment weights (IPTW). The IPTW approach estimates the probability that an individual would have experienced her actual pattern of homeownership between the first and last years of wealth data from the National Longitudinal Survey of Youth 1979 (NLSY79), 1985 and 2008. Thus, homeownership is the "treatment," and it occurs as a series of statuses across the 24-year period. We can express the probability that an individual (i) experiences a particular 24-year homeownership pattern as the product of annual conditional probabilities:

$$w_i^{-1} = \prod_{t=1985}^{2008} w_{ti}^{-1} = \prod_{t=1985}^{2008} P\left(E_t = e_{ti} \middle| \overline{E}_{t-1} = \overline{e}_{(t-1)i}, \overline{X}_t = \overline{x}_{ti}\right)$$

In each period (t), we estimate the probability (w_{ti}) that the homeownership status was the actual status experienced by the individual (e_{ti}), given the history of homeownership ($\overline{e}_{(t-1)i}$) and other confounders, such as income, marital status and, most critically, prior wealth ($\overline{x}_{ti}$).

Multiplying across all years gives the probability that the individual experiences the observed sequence of homeownership outcomes. The IPTW (w_i) is the inverse of this probability. Regression models that weight the sample by the IPTWs create a pseudo-population in which homeownership status in each period is independent of prior confounding variables, making it unnecessary to condition on these variables (Robins 1999; Robins, Hernán and Brumback 2000).

Consistent with prior research (Ingram, Yue and Rao 2010; Pontikes, Negro and Rao 2010; Sharkey and Elwert 2011; Wodtke, Harding and Elwert 2011), we use stabilized IPTWs to improve the properties of the weights. The stabilized IPTWs are centered at one, follow an approximately normal distribution, and have smaller variance than the unstabilized weights (Hernán, Brumback and Robins 2002; Robins, Hernán and Brumback 2000). The stabilized weights can be expressed as:

$$\mathrm{sw_i} = \prod_{t=1985}^{2008} \frac{P\left(E_t = e_{ti} \middle| \bar{E}_{t-1} = \bar{e}_{(t-1)i}, X_0 = x_0\right)}{P\left(E_t = e_{ti} \middle| \bar{E}_{t-1} = \bar{e}_{(t-1)i}, \bar{X}_t = \bar{x}_{ti}\right)}$$

$$= w_i \prod_{t=1985}^{2008} P\left(E_t = e_{ti} \middle| \bar{E}_{t-1} = \bar{e}_{(t-1)i}, X_0 = x_0\right)$$

The denominator of the stabilized weight is the unstabilized weight. The numerator conditions on time-invariant baseline traits and prior homeownership statuses, but not other time-varying confounding variables. To compute the numerator, we follow the same logit model described previously, but exclude all time-varying predictors other than homeownership.

Following Wodtke, Harding and Elwert (2011), we also create stabilized weights that account for sample attrition in the same way. The product of the stabilized treatment weight and the stabilized attrition weight is the final weight for the individual.

We subsequently estimate regression models weighted by the final weight. The key independent variable in these regressions is the number of years between ages 1985 and 2008 the individual owned a home. Because the stabilized weights include baseline covariates in both the numerator and denominator, the homeownership experiences of the weighted pseudo-population are not independent of these baseline traits and they must also be included in the final outcome model (Wodtke, Harding and Elwert 2011). In addition to models that pool across races, we also estimate models entirely separately for whites, blacks, and Hispanics, to evaluate whether there are race differences in the wealth returns to homeownership.

With this general model in mind, we turn to specifying the models of year-specific probabilities of different homeownership statuses. Rather than using a pooled, cross-sectional model, we follow the logic of event-history models to estimate the risk of entry into homeownership in the next (survey) year for an individual who does not currently own a home, and out of homeownership for someone who does. Because we have data on homeownership only at the survey wave level (typically every one to two years), we use discrete-time hazard models, which we specify with the logit link function.

This event-history model approach has several advantages. First, it allows that the determinants of transitions into and out of homeownership may not be simply mirror images of one another. Second, the framework of event-history models brings to the forefront the question of how to measure time, a relevant question given that, conceptually, our models should allow the entire history of prior experiences to influence an individual's current transition probability. We argue that the relevant time process is different between different transitions. For transitions into first-time homeownership, our fundamental measure of time is age: we assume that, just as the transition to own residence is an important part of the transition to adulthood, homeownership is also a natural step in this transition for many Americans. For transitions out of homeownership, we use the number of years since entry into this spell of homeownership (a transition from one home to another with no break in ownership would count as one spell). We also consider repeat transitions into homeownership, which we assume depend in part on the duration of time since the last homeownership spell ended. In this way, prior homeownership patterns affect both which risk set (first-time homeownership, exit from homeownership, repeat homeownership) an individual is in, and the core measure of time in the hazard models (time since last transition in/out of homeownership, or, for first-time homeownership, the prior history of homeownership is by definition entirely non-owning).

Data

We use data from the National Longitudinal Survey of Youth 1979 (NLSY79), which includes 12,686 men and women first interviewed in 1979, when they were ages 14-22. They have subsequently been interviewed annually or biannually (U.S. Department of Labor Bureau of Labor Statistics 2012), with the response rate remaining over 80 percent (National Longitudinal Surveys 2013). Respondents are ages 20-28 in the first year asset information was collected (1985) and 43-51 in the most recent year (2008). Thus, the NLSY79 provides a good source of information for the evolution of assets from young adulthood through middle age. We limit the sample to the subsamples of NLSY79 that were followed throughout the entire survey period. We estimated models pooled by race and also separate models for Hispanics, non-Hispanic blacks, and non-Hispanic whites. We do not have sufficient sample size to estimate race-specific models for other racial groups. All financial variables are adjusted to 2012 dollars using the Consumer Price Index (CPI).

We deal with missing data in the following way. First, we impute all variables with values from the subsequent available year, up to four years forward. If homeownership status in any year 1985-2008 is missing after this imputation, we lack full information on homeownership patterns, so we drop this person entirely and consider them to have attrited following the last wave in which information was available. We also consider individuals to have attrited who did not provide information on wealth in 2008 – our outcome variable. For individuals who lack compete homeownership histories, we include pre-attrition observations in the hazard models, for statistical power. For individuals with complete homeownership histories (after imputation) at each wave, we create year-by-year histories by assuming no status transitions between waves (even when years are biennial). Likewise, to create the estimated probability of a particular homeownership status in an inter-wave year, we use the most recent available set of covariate values for prediction. For item-missing data, we use missing data flags. The exception is education, which is missing for only three individuals, whom we drop from the sample.

Our analytic sample for the hazard models includes 6,820 respondents and 145,736 person-year observations, and our IPTW models include 5,919 individuals. Our three race-specific IPTW models include 1,804 blacks, 2,612 whites, and 888 Hispanics.

Our ultimate dependent variable, the measure of wealth at midlife, is an individual's net worth in 2008, when respondents are ages 43-51. Our final models are weighted regressions with years of homeownership as the main independent variable. Because we use stabilized weights, we also include baseline controls for all variables that go into our hazard models, measured in 1985. We estimate both conditional mean, ordinary least squares (OLS) regressions and median regressions. We prefer the results of the median regressions, because they are less sensitive to outliers – a particularly important property for an outcome like wealth, which is heavily skewed.

Hazard Model Specification

In each wave of the NLSY79, individuals are asked to report whether they and/or their spouse or partner own or are making payments toward owning their home. We define individuals to be homeowners if they answer in the affirmative. The phrasing of the NLSY79 is helpful: individuals living with their parents are not homeowners if it is the parents who own the home. For each transition type we examine, we estimate a discrete-time hazard model, estimating the risk of change in homeownership status between waves. Standard errors are clustered at the individual level in each model, to account for the non-independence of observations from the same individual.

In our model of transition to first-time homeownership, our outcome variable is a dummy variable set to one if the individual is observed at the next observation to have become a homeowner. The risk set is all individuals who have never previously been observed to be a homeowner. Although our period of wealth accumulation begins in 1985, we have information on homeownership status since the first wave in 1979. Because of the young age of the sample in 1979, we assume that anybody not observed to own a home between 1979 and 1985 has never previously owned a home.

To model transitions out of homeownership, the risk set is all individuals who currently own their homes, and we define a dummy variable set to one if an individual in this group is not a homeowner in the next observation. Because we use time since entry into this spell of homeownership as our key measure of time in this model, individuals who already owned their home in 1979 (less than 5 percent of the sample) are left-censored. For this situation, we assume respondents started homeownership at age 18 if they were older than 18 in 1979. If they were younger than 18 in 1979, we assume they became homeowners in 1979.

Lastly, we model transitions into a repeat spell of homeownership, using as the risk set all individuals who have previously been observed to own their own home, but do not currently report owning their home. We define a dummy variable set to one if the individual is observed to have re-entered homeownership at the next observation.

In all models, we account for the fact that the inter-wave period differs across observations. This is particularly true because the NLSY79 was annual between 1979 and 1994 and has been biennial since then. However, individuals also occasionally miss waves. Therefore, we control for the number of calendar years between observations (up to four years), but constrain the model coefficient such that the hazard rate scales linearly with exposure time (this is referred to as an "offset"). In other words, we do not use imputed homeownership status as the outcome in the hazard models, only in the calculation of the IPTWs.

For each hazard model, we experimented with a range of functional form specifications. We estimated baseline models with no controls and a fully flexible set of dummy variables and graphically examined the shape of the relationship between, for example, age and each transition. We also used formal model fit statistics to experiment with alternative specifications of key control variables, such as income. Because we are concerned with the predictive validity of our hazard models, rather than a particular substantive question, we specify many continuous variables more flexibly than a single linear term.

Age. In the models of first-time and repeat transition to homeownership, current age is specified as a linear spline with a knot at age 35 (again, as with all specification decisions, with the location of the knot chosen by experimentation and/or visual inspection). In the model of exit from homeownership, it is modeled with a linear term, top-coded at age 44, after which point the risk of exit appears roughly stable, at least through the early 50s when our respondents are last observed.

Race. Race is captured with binary variables for whether the respondent is black, white, Hispanic, Asian American and Pacific Islander (AAPI), or another race.

Education. Education has been hypothesized to affect asset accumulation via improved financial decision-making and is consistently positively associated with net worth (Yamokoski and Keister 2006). We measure educational attainment in the current year in five categories: less than a high school degree, exactly a high school degree, some college education, a four-year college degree, and an advanced degree.

Social origins. Respondents' social origins are measured with parental education, parental age, parental occupation, whether the respondent was born in the south, and the respondent's number of siblings, all measured at baseline in 1979. Parental education is positively associated with wealth, net of individuals' own traits (Yamokoski and Keister 2006), while siblings are negatively associated with net worth (Keister 2003; Yamokoski and Keister 2006). Parental education is measured in the same categories as the respondent's own education. Parental education is measured as the maximum among the respondent's residential parents, if there is more than one. Respondents report their parents' occupation when respondents were 14 years old, if at the time the parents worked for pay and lived with the respondent. If either parent misses this information and is reported to live with the respondent at age 14, we impute with parental occupation in 1978, which is reported by respondents in 1979 if the parent worked for pay at all in 1978. For each occupation, we obtain wage and salary annual income from a random subsample of 1980 Census respondents aged 40 to 50 and employed full-time (more than 35 hours per week) (Ruggles et al. 2010). Even in the large Census sample, some occupations are uncommon, making sample means unreliable estimates of typical earnings. Therefore, we exploit the fact that three-digit detailed occupation codes are grouped into broader occupation categories (we use 19) and estimate a hierarchical linear model, borrowing information on the wages of other, similar occupations. In order to purge the measure of occupational average income of composition effects, we control in these models for gender, race and age. The random effects that we recover from this model are then estimates of how the income of the occupation in question compare to the typical occupation, net of composition differences. We experimented with measures of parental occupational prestige, but found that it was never statistically significantly associated with homeownership transitions and therefore do not include it in our models. Parental occupational income is measured as the maximum among respondents' residential parents, if both parents report working. Parental age is measured as the average, if there is more than one residential parent. For respondents not living with any parent at age 14, maternal values are used, when available. Otherwise, paternal values are used. A dummy variable is set to one if the respondent reports having been born in the American south.

Independent residence. By definition, homeowners must have established independent residence, since homeownership is defined by the respondent's report of whether she or her spouse owns or are making payments to own the home. Therefore, we include a measure of independent residence only in our models of transitions to homeownership. We define the respondent as living independently if in the current survey she reports no parental figures in the household and she is not living in a group home (e.g., fraternity/sorority, juvenile detention center, or hospital). In all models, we also include a measure of the number of years since the respondent last lived non-independently. Individuals who already lived independently in 1979 are left-censored. Similar to our assumptions with left-censored homeownership histories, we assume respondents started to live independently at age 18 if they were older than 18 in 1979 and living independently. If they were younger than 18 at 1979, we assume they entered independent residence in 1979. For the model of transitions to repeat homeownership, this term is specified as a linear spline with a knot at 13 years. For the other two models, it is specified as a constant linear term.

Marriage and gender. We use the respondent's report of her current marital status in each wave to create a binary variable for whether the respondent is married in that period. We incorporate marital status in our models in two ways. First, we allow current marital status to be associated with the risk of transition into or out of homeownership. We specified married as the reference group and create dummy variables for whether the individual is an unmarried male or an unmarried female, thus also incorporating the possibility for a gender gap in homeownership. Second, we recognize that residential transitions are particularly likely at a time of marital transition. Therefore, we also include dummy variables for whether the individual either marries or separates between the current period and the next period (when homeownership is measured). Although we cannot determine precisely the temporal ordering of marital and homeownership transitions, we assume that, in the short-term, marriage decisions drive homeownership decisions, rather than the other way around, although we acknowledge the possibility for reverse causality. We also use interaction terms to allow different effects of marriage and marital transitions by sex.

Prior homeownership experiences. In the model of repeat homeownership, we include a linear term measuring the number of years since the individual was last a homeowner. For the model of exit from homeownership, we control for the number of years the individual has spent in the current homeownership spell, top-coded at 20. We also include a dummy variable to indicate whether the individual has ever previously experienced a transition out of homeownership, to capture unobserved traits that may be associated with enduring risk of homeownership exit.

Income. In each wave, NLSY79 ascertains total family labor income since the previous wave. We assume that income received *prior* to the current period will affect homeownership transitions only through wealth – described below. Therefore, we include only the current measure of income in the hazard model. For the transition to first-time homeownership and exit from homeownership models, we specify income with a linear spline, with a knot at the 75th percentile of the distribution in the risk set. For transitions to repeat ownership, we use a constant linear term.

Wealth. In most years, the NLSY79 has collected information on the respondent's net worth (1985-1990, 1992-1994, 1996, 1998, 2000, 2004, and 2008). Although the specific wealth questions vary somewhat across years, in each wave in which asset information is collected, NLSY79 creates a measure of the respondent's total net worth. Net worth is generally the sum of: housing equity (market value less debt), vehicle(s) value, cash savings, stocks and bonds, ownership of a farm, business, or property, and other (residual) valuable items or debts. Beginning in 1988, respondents were also asked to report the value of any rights they hold to estates or trusts. NLSY79 imputes missing values for specific assets, and we employ these imputed values. We also top code positive wealth and income at the 99th percentile for each year, to avoid unduly influential outliers. To reduce skew, we log wealth for those with positive net worth and include separate dummies for zero and negative net worth. We experimented with a specification that included the log of the amount of debt, among net debtors, but we found that this did not improve model fit compared to the simple dummy for negative net worth. For the model of exits from homeownership, we specify the log of positive wealth as a linear spline with a knot at the top quartile of the risk set. Log of positive wealth is specified with a constant linear term for the two models of transitions into homeownership.

RESULTS

Table 1 shows descriptive statistics in the sample of individuals and person-year observations used in the IPTW regressions. As expected, race differences in 2008, at midlife, are vast: whites average $425,000 in net worth, compared to $263,000 for Hispanics and $139,000 for blacks. Homeownership patterns also differ substantially; whites spend, on average, over 15 years in homeownership during the 24-year period, compared to 11 for Hispanics and 8 for blacks. Whites are also advantaged compared to Hispanics and blacks in their social origins; they are less likely to have been born in the south, have fewer siblings, and having parents with higher average education. In terms of achieved characteristics, whites again are most advantaged, having the highest average family incomes, highest probabilities of independent residence, highest marriage rates, and highest education.

[Table 1 about here]

Table 2 shows estimates for the log hazard of transitioning into first-time homeownership (first column), into repeat homeownership (second column), and out of homeownership (third column). Critically for our analysis, prior wealth is strongly associated with the risk of homeownership transitions. Among net wealth holders, greater wealth is associated with greater risk of transition into either first or repeat homeownership. Specifically, a one percent increase in wealth is associated with an increase of about 0.28 percent in the odds of transition to first-time homeownership, and about 0.19 percent in the odds of transition to repeat homeownership. For the risk of exit from homeownership, high levels of wealth are especially protective: wealth is not statistically significantly associated with exits from homeownership over the bottom three quarters of the wealth distribution (in fact, the coefficient goes in the opposite of the expected direction), but, among those in the top quartile of the wealth distribution (above $76,923), higher wealth significantly reduces the risk of homeownership exit. These strong associations demonstrate the importance of controlling for prior wealth when considering the association between homeownership patterns and later-life assets.

The dummy variables for zero and negative net worth have different patterns than we might expect: they are positively associated with transitions into homeownership and negatively associated with homeownership exits. To put these coefficients in context, they are compared to the reference group of someone with $1 of wealth (log(1) = 0). We suspect that these coefficients reflect specification error in the functional form of the association between wealth and homeownership: if close to zero net worth the association between wealth and homeownership transitions is weaker than at other points in the distribution, we are likely to tend to underestimate the risk of transition to homeownership (or overstate the risk of transition out of homeownership) for those at $1 of net worth. Thus, it may appear that those with zero or negative net worth are comparatively advantaged. This pattern of differently signed coefficients for dummy variables for wealth-holding (compared to the coefficient on the log of positive wealth) appears in prior literature as well (Conley 1999; Killewald 2013).

As expected, African-Americans and Hispanics are disadvantaged in all transition types. They are less likely to enter both first and repeat homeownership and are at greater risk of exiting homeownership. The associations for Asian American and Pacific Islanders and members of other races are more mixed and are imprecisely estimated. Education is positively associated with entry into first-time homeownership and negatively associated with exit from homeownership. In particular, those with less than a high school degree have much lower rates of entrance and much higher rates of exit than other groups, and the advantage conferred by a four-year college degree or more is also strong. Differences between those with a high school degree and some college, and between those with a four-year degree and an advanced degree, are more modest. Compared to married couples, single men and women are at lower risk of transitioning into either first or repeat homeownership and at higher risk of exiting homeownership. The marital status transition measures are not statistically significant.

Income facilitates entrance into homeownership, both first and repeat, and diminishes the risk of exit. Notably, for both entrance into first-time homeownership and risk of exit, the association is much stronger in the top quartile of the risk set ($32,040 for the first-time entrance risk set, $63,308 for the exit risk set). This finding is consistent with prior evidence of a highly non-linear association between income and wealth more generally (Barsky et al. 2002).

Perhaps unexpectedly, parental characteristics have relatively little association with homeownership transitions, net of individuals' own characteristics. Being born in the south is associated with higher risk of transitioning into either first-time or repeat homeownership, perhaps reflecting lower housing prices in this region. Parental education is associated with *reduced* risk of entering first-time homeownership and *higher* risk of exit from homeownership, contrary to predictions. Thus, there is no evidence that parental resources are used as a safety net to prevent homeownership exits.

Transitions into first-time homeownership rise gradually with age until age 35, but then decline sharply. Some of this may reflect changing selectivity: those who have not yet purchased homes by age 35, for reasons not explained by the covariates, may have unobserved characteristics that make homeownership either undesirable or infeasible. Increasing age is also associated with diminished risk of exiting homeownership and of entering repeat homeownership. Thus, transitions of all kind become less common with age, at least after age 35, highlighting that transitions into and out of homeownership are not mirror images in all cases: some characteristics reduce or heighten transitions of all kinds.

Young adults currently living independently are, surprisingly, at *lower* risk for transition to first-time homeownership. However, those who have been living independently for longer are at higher risk of entrance to repeat homeownership. Finally, those who have owned their home for longer are at lower risk of exit from homeownership, although having previously exited homeownership is not associated with heightened risk of doing so in the future (in fact, the association goes in the opposite direction and is statistically significant).

We estimated a similar model with the outcome being attrition from the sample. The results are shown in Appendix Table 1.

[Table 2 about here]

The properties of the IPTWs are shown in Table 3. For both the full sample and the race subsamples, we show the mean of the stabilized weights. Recall that stabilized weights, both treatment and attrition, should have mean one and be approximately normally distributed. Although our attrition weights have mean close to one, this is not the case for the treatment weights. The reason for this is that we estimate the hazard models on a somewhat broader sample than the sample of individuals who eligible to have an IPTW constructed. Our hazard models are greedy: we use observations from individuals even if they subsequently attrit from the sample prior to 2008 and, therefore, cannot be used in the IPTW-adjusted regression. When we restrict the hazard models to observations from only those individuals who will eventually contribute to the regression, the average weights are much closer to one. Prior to analysis, we also top- and bottom-code the final weights at the 1st and 99th percentiles of the distribution, to reduce the potential for unduly influential outliers.

[Table 3 about here]

Finally, Table 4 presents the results of our regression models. For comparison, we present the results of unweighted regressions as well as our preferred weighted results. We anticipate that weighting will reduce the estimated association between homeownership and subsequent wealth, as the weights remove any association between midlife wealth and homeownership that is due to the effect of the time-varying variables in our model on both. We also show, for comparison, the results of OLS models, in addition to our preferred median regressions.

In the pooled sample, we estimate that each additional year of homeownership is associated with $8,000 more in midlife wealth. Failure to adjust for time-varying spurious characteristics generates an estimate of homeownership's effects that is 18 percent larger - $9,500 per year. The results of the comparison OLS regressions also demonstrate how sensitive the results are to outliers: both weighted and unweighted estimates are more than 60 percent larger when conditional mean rather than conditional median models are used.

[Table 4 about here]

Our race-specific results show substantial disparities in the wealth returns to homeownership. These estimates are based on entirely race-specific hazard models that are subsequently used to estimate race-specific IPTWs and weighted regressions. At the median, whites are estimated to accumulate $11,800 more in wealth for every year of homeownership, compared to $7,900 for Hispanics and only $4,900 for blacks.

CONCLUSIONS AND NEXT STEPS

The results presented in the previous section are preliminary, but they confirm that homeownership has substantial wealth benefits, although models that fail to account for the dynamic relationships between wealth, homeownership, and other wealth-enhancing characteristics are likely to overstate its benefit. Furthermore, we find that, compared to whites, blacks and Hispanics are disadvantaged in three distinct ways. First, as shown in our descriptive results, they have, on average, characteristics that are less likely to facilitate entering and maintaining homeownership. As a result, they participate less in this wealth-generating state. Second, even when holding other determinants of homeownership constant, blacks have lower rates of entry into homeownership, further depressing their accumulated years of homeownership. Third, for every year that they spend as a homeowner, blacks and Hispanics receive a lower median wealth return than do whites.

We plan several additional analyses in the coming months. First, we plan to refine our models in several ways. Our race-specific hazard models, used to generate the race-specific IPTW estimates, suggest that the determinants of transitions into and out of homeownership may differ by race. We plan to more formally test this possibility by estimating hazard models with full interactions by race and then assuming homogeneous associations by race only when it is not possible to statistically reject this assumption. This will improve our pooled estimate of the median wealth returns to homeownership, but will also allow us to investigate substantively whether, for example, blacks and Hispanics receive a smaller asset return on characteristics like income and education, as has been shown for net worth more generally (Addo and Lichter 2013; Altonji and Doraszelski 2005; Conley and Glauber 2008).

We also plan to use multiple imputation to account for item-missing data and to check the sensitivity of our results to estimating the hazard models on the same sample used in the regressions. We also plan to estimate models that use the log of net worth as the outcome, rather than its raw value. From a conceptual standpoint, it is unclear whether we would expect the effects of homeownership to be heterogeneous in dollar terms across the wealth distribution, but it is possible that homeownership has larger absolute benefits, but similar relative benefits, at higher wealth levels. In this case, we may find that some of the race differences in the benefits of homeownership are reduced when we consider relative gains.

We also plan simulations to describe how much of the wealth gap by race and ethnicity at midlife would be closed if each of the previously-described sources of disadvantage were eliminated. While simulations of this kind do not translate directly to causal inference, they can provide a sense of the relative contributions of disparities in different aspects of the wealth-generating process.

Finally, we plan to investigate in more detail the sources of the lower returns to homeownership for blacks and Hispanics compared to whites. One possibility is that returns to homeownership are heterogeneous by other characteristics, such as income or education, that vary in their distributions by race. We could create a matched sample of young adults in 1985, equalizing the distribution of baseline characteristics by race, and explore whether within this sample the wealth returns to homeownership are similar by race.

Although homeownership is only one possible mechanism for wealth accumulation, it has been the subject of much speculation, because of the prominence of home equity in most Americans' asset portfolios. Our results suggest that, indeed, homeownership has large wealth consequences and may be a significant source of asset differences by race. This, in turn, suggests that expanding and strengthening programs that support low-income Americans' homeownership, such as HUD's HOME Investment Partnerships and Community Development Block Grants, may narrow the asset gap (Fudge, Bulka and Sage Computing staff 2012), but not all of the race gap in either homeownership or its benefits can be explained by class differences.

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Table 1. Descriptive statistics for the IPTW sample

	All	Black	White	Hispanic
Persons	5,919	1,804	2,612	888
Wealth as of 2008 ($100,000)	3.73	1.39	4.25	2.63
	(6.66)	(3.88)	(7.02)	(5.45)
Years of homeownership during 1986-2008	14.19	8.36	15.27	11.14
	(7.78)	(7.45)	(7.33)	(8.03)
Female	.50	.50	.50	.49
South	.32	.62	.24	.32
Number of siblings	3.29	4.61	3.01	4.39
	(2.23)	(2.96)	(1.89)	(2.99)
Parental age in 1978	45.32	44.53	45.52	44.48
	(6.91)	(7.54)	(6.71)	(7.17)
Parental education				
Less than a high school degree	.24	.45	.17	.58
Exactly a high school degree	.42	.36	.45	.23
Some college education	.13	.10	.14	.08
Four-year college degree	.12	.06	.14	.06
Advanced degree	.08	.03	.10	.04
Person-years	142,056	43,296	62,688	21,312
Age	35.53	35.56	35.51	35.42
	(7.30)	(7.30)	(7.30)	(7.33)
Family income ($100,000)	.44	.27	.48	.35
	(.39)	(.28)	(.41)	(.33)
Positive wealth ($100,000)	1.67	.64	1.86	1.13
	(3.85)	(2.04)	(4.04)	(2.97)
Zero wealth	.03	.13	.02	.06
Negative wealth dummy	.09	.12	.08	.11
Independent residence	.91	.83	.93	.89
Years since last dependent residence	13.56	10.59	13.87	12.66
	(11.19)	(10.74)	(10.93)	(11.53)
Years of homeownership	5.53	2.48	6.10	12.66
	(6.80)	(4.63)	(6.96)	(11.53)
Ever lost homeownership before	.30	.30	.30	.32
Years since last homeownership	.68	1.16	.57	.99
	(2.45)	(3.41)	(2.19)	(3.12)
Male single	.14	.21	.13	.15
Female single	.14	.23	.12	.15
Entering marriage	.03	.03	.03	.03
Female entering marriage	.02	.01	.02	.02
Exiting marriage	.02	.02	.02	.02
Female exiting marriage	.01	.01	.01	.01
Education				
Less than a high school degree	.09	.12	.07	.16
Exactly a high school degree	.55	.63	.52	.58
Some college education	.12	.12	.12	.13
Four-year college degree	.17	.10	.20	.08
Advanced degree	.07	.03	.09	.05

Note: All samples are weighted by the 2008 NLSY79 weight.

Table 2. Discrete-time hazard models of entry to and exit from homeownership

	(1) First ownership	(2) Repeat ownership	(3) Exit ownership
Age			
<=35	.020*	-.021***	
	(.009)	(.010)	
>35	-.209***	-.224***	
	(.013)	(.012)	
top coded at 44			-.174***
			(.005)
Race			
White			
Black	-.591***	-.619***	.768***
	(.066)	(.088)	(.072)
Hispanic	-.322***	-.294*	.482*
	(.074)	(.096)	(.075)
AAPI	-.094	.756	.592
	(.263)	(.616)	(.365)
Other races	.168*	.438	.109
	(.082)	(.098)	(.079)
Family income ($10,000s)		.071***	
		(.013)	
top quartile in risk set	.259***		-.133***
	(.024)		(0.014)
bottom three quartiles in risk set	.033*		.012
	(.013)		(.013)
Log wealth, wealth>0	.284***	.191***	
	(.018)	(.022)	
top quartile in risk set			-.209***
			(.023)
bottom three quartiles in risk set			.078
			(.074)
Zero wealth	1.740***	1.194***	-0.608
	(.178)	(.233)	(.353)
Negative wealth dummy	2.101***	1.345***	-1.605***
	(.171)	(.227)	(.252)
Independent residence	-.228***	-.155	
	(.059)	(.108)	
Years since last reported non-independent residence	.001		-.001
	(.004)		(.003)
<=13		.020*	
		(.010)	
>13		.007	
		(.005)	
Years of homeownership (top coded at 20)			-.058***

			(.009)
Ever lost homeownership before			-.092**
			(.068)
Years since last homeownership		-.016	
		(.012)	
Male single	-1.144***	-.704***	1.027***
	(.061)	(.085)	(.071)
Female single	-.773***	-.430***	.469***
	(.059)	(.081)	(.073)
Entering marriage	-.170	-.320	.206
	(.094)	(.167)	(.124)
XFemale	.150	.293	-.244
	(.123)	(.226)	(.172)
Exiting marriage	.147	-.113	.202
	(.202)	(.153)	(.179)
XFemale	.336	-.153	.231
	(.263)	(.206)	(.244)
Education			
Less than a high school degree			
Exactly a high school degree	.219**	-.059	-.279***
	(.078)	(.095)	(.080)
Some college education	.303***	.195	-.343***
	(.091)	(.124)	(.103)
A four-year college degree	.578***	.257	-.738***
	(.097)	(.140)	(.111)
An advanced degree	.741***	.523**	-.662***
	(.131)	(.195)	(.139)
Social origins			
Parent – Less than a high school degree			
Parent – exactly a high school degree	-.032	-.038	.121
	(.061)	(.078)	(.063)
Parent – Some college education	-.021	-.099	.252**
	(.080)	(.111)	(.088)
Parent – A four-year college degree	-.286**	-.098	.152
	(.091)	(.138)	(.104)
Parent – An advanced degree	-.256*	-.071	.329**
	(.107)	(.159)	(.123)
Parent education missing	-.157	-.011	.336*
	(.130)	(.189)	(.145)
Log of average wage of parental occupation	.026	-.208	.163
	(.141)	(.191)	(.135)
Parental age	-.000	-.005	-.017
	(.003)	(.014)	(.011)
Born in south	.328***	.279**	.028
	(.052)	(.067)	(.053)
Number of siblings	.012	.005	-.017

	(.010)	(.014)	(.011)
N (person-years)	44,546	12,945	42,315

** p < .05, ** p < .01, *** p < .001*

Note: The model also includes missing flags for being born in the south, number of siblings, wealth, parental occupation, and parental age. Two dummy variables are included for whether the parents are not working and whether the respondent didn't know the parental occupation or didn't know whether the parent was working.

Table 3. Properties of stabilized treatment and attrition weights

Weight	Mean	Mean*	SD	SD*	Percentiles 1st	25th	75th	99th
All (n=5,919)								
Stabilized treatment weight (SW)	1.52	(1.28)	6.27	(2.10)	.0065	.32	1.21	15.16
Stabilized attrition weight (CW)	1.00	(1.00)	.11	(.05)	.96	.98	.99	1.38
SW × CW	1.53	(1.28)	6.22	(2.11)	.0065	.32	1.20	15.00
Black (n=1,804)								
Stabilized treatment weight (SW)	1.57	(1.33)	6.54	(1.93)	.0066	.37	1.24	12.98
Stabilized attrition weight (CW)	1.00	(1.00)	.06	(.03)	.96	.99	1.00	1.21
SW × CW	1.57	(1.33)	6.45	(1.95)	.0069	.37	1.23	13.17
White (n=2,612)								
Stabilized treatment weight (SW)	1.41	(1.17)	5.40	(1.78)	.0046	.29	1.16	12.55
Stabilized attrition weight (CW)	1.00	(1.00)	.13	(.05)	.95	.99	.99	1.36
SW × CW	1.42	(1.18)	5.44	(1.86)	.0047	.29	1.15	13.23
Hispanic (n=888)								
Stabilized treatment weight (SW)	1.55	(1.44)	4.00	(2.96)	.0078	.27	1.20	23.20
Stabilized attrition weight (CW)	1.34	(1.05)	5.31	(.46)	.93	.97	.98	4.74
SW × CW	1.85	(1.59)	6.58	(3.50)	.0090	.26	1.21	26.52

*top and bottom coded at 1%

Table 4. Estimated effects of homeownership on wealth

Model	Coef	RSE	95% Confidence Interval
All (n=5,919)			
Unadjusted OLS	15553.11	906.84	(13775.37, 17330.84)
Stabilized IPT-weighted OLS	13742.47	1596.57	(10612.61, 16872.32)
Unadjusted quantile regression	9520.57	301.84	(8928.85, 10112.29)
Stabilized IPT-weighted quantile regression	8044.92	482.38	(7099.28, 8990.57)
Black (n=1,804)			
Unadjusted OLS	13617.65	1431.56	(10809.92, 16425.39)
Stabilized IPT-weighted OLS	12726.74	3423.46	(6012.28, 19441.20)
Unadjusted quantile regression	6389.42	402.09	(5600.79, 7178.05)
Stabilized IPT-weighted quantile regression	4940.18	539.72	(3881.64, 5998.74)
White (n=2,612)			
Unadjusted OLS	17972.84	1622.46	(14791.39, 21154.29)
Stabilized IPT-weighted OLS	17583.19	2996.64	(11707.12, 23459.25)
Unadjusted quantile regression	11754.62	638.29	(10503.00, 13006.24)
Stabilized IPT-weighted quantile regression	11755.82	931.37	(9929.52, 13582.12)
Hispanic (n=888)			
Unadjusted OLS	15853.69	2080.00	(11771.16, 19936.21)
Stabilized IPT-weighted OLS	14285.84	4327.15	(5792.71, 22778.97)
Unadjusted quantile regression	9686.09	768.37	(8177.97, 11194.21)
Stabilized IPT-weighted quantile regression	7872.55	1199.18*	(5518.85, 10226.25)*

IPT weights (SW × CW) are top and bottom coded at 1%

*IPT weights (SW × CW) top and bottom coded at 5% to obtain meaningful estimates

Appendix Table 1. Discrete-time hazard model of attrition

	Attrit from the sample in the following year
Age	.097*
	(.043)
Race	
White	
Black	-.047
	(.129)
Hispanic	.189
	(.145)
AAPI	-.203
	(.864)
Other races	.076
	(.159)
Family income ($10,000s)	.003
	(.012)
Log wealth, wealth>0	.055*
	(.027)
Zero wealth	.906***
	(.266)
Negative wealth dummy	.627*
	(.274)
Independent residence	.094
	(.115)
Years since last reported non-independent residence	-.022**
	(.008)
Owns a home	-.454***
	(.112)
Transitioned into homeownership from last year	.164
	(.124)
Transitioned out of homeownership from last year	.072
	(.143)
Male single	.448***
	(.107)
Female single	-.260*
	(.121)
Entering marriage	-.070
	(.215)
XFemale	-.433
	(.313)
Exiting marriage	-.376
	(.244)
XFemale	.575
	(.338)
Education	

Less than a high school degree	
Exactly a high school degree	-.116
	(.137)
Some college education	-.097
	(.176)
A four-year college degree	-.064
	(.186)
An advanced degree	-.159
	(.228)
Social origins	
Parent – Less than a high school degree	
Parent – exactly a high school degree	-.012
	(.121)
Parent – Some college education	-.042
	(.170)
Parent – A four-year college degree	-.015
	(.196)
Parent – An advanced degree	-.236
	(.233)
Parent education missing	-.054
	(.246)
Log of average wage of parental occupation	-.231
	(.248)
Parental age	-.013
	(.008)
Born in south	-.315***
	(.113)
Number of siblings	.008
	(.020)
N (person-years)	99,806

* $p < .05$, ** $p < .01$, *** $p < .001$

Note: The model also includes missing flags for being born in the south, number of siblings, wealth, parental occupation, and parental age. Two dummy variables are included for whether the parents are not working and whether the respondent didn't know the parental occupation or didn't know whether the parent was working.