THE EFFECT OF MARITAL STATUS ON HOMEOWNERSHIP AMONG LOW-INCOME HOUSEHOLDS

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ABSTRACT

We investigate the causal relationship between marital status and the transition to homeownership among a sample of low- to moderate-income renters. Using data from the Community Advantage Panel Study (*N* = 1,530), we use discrete-time survival analysis with propensity score matching to explore this relationship. Results indicate married couples have higher odds of buying a home, and do so at faster rates, than their unmarried counterparts. These findings were robust to the control of selection-bias between the married and unmarried groups using propensity score matching. The findings suggest efforts to encourage marriage among low-income couples may be associated with subsequent economic mobility through homeownership.

Keywords: homeownership, life course, low-income families, marital status, propensity score matching

The Transition into Low-Income Homeownership: Does Marital Status Matter?

The transition in tenure from renting to homeownership is highly valued in the United States and is considered part of the American Dream. Research suggests that homeownership is associated with many benefits for individuals, families, and their communities. For example, evidence supports that owning a home is associated with increased savings and wealth, particularly among low-income families (Di, Yang, & Liu, 2003; Skinner, 1989). Further, research has established an association between owning a home and the household's greater social and civic involvement in local activities such as voting, volunteer work, and neighborhood associations (DiPasquale & Glaeser, 1999; Drier, 1994; Manturuk, Lindblad, & Quercia, 2009). Studies have also shown a link between homeownership and positive child outcomes such as higher educational attainment (Boehm & Schlottmann, 1999; Haurin, Parcel, & Haurin, 2002), lower teenage pregnancy and fewer behavioral problems (Haurin et al., 2002).

Similar to homeownership, marriage is associated with upward mobility and prosperity and is perceived by many as a normative milestone in the life course with numerous associated benefits (Waite, 1995; Waite & Gallagher, 2000). Evidence suggests that married couples in high quality relationships are more likely to have positive psychological outcomes (Williams, 2003; Frech & Williams, 2007), fewer health complications (Hughes & Waite, 2009), and greater economic stability and wealth (Grinstein-Weiss, Zhan, & Sherraden, 2006; Lupton & Smith, 2003; Wilmoth & Koso, 2002). The socioeconomic benefits to marriage include an increased likelihood of higher income, greater affluence, and less material hardship (Hirschl, Altobelli, & Rank, 2003; Lerman, 2002, White & Rogers, 2000).

Despite significant public policy efforts on the part of the federal government to promote homeownership and marriage, little attention has been given to the relationship between the two.

Specifically, the extent to which marriage affects the transition from renting to homeownership remains unclear. Although marital status has been included in previous studies, it has primarily been used as a control variable to examine tenure change. Therefore, the purpose of this study is to explore the causal relationship between marital status and homeownership among a group of low- and moderate-income renters. To address this topic, we use propensity score matching and event history analysis to answer the following question: To what extent does marital status influence whether and when low- and moderate-income individuals purchase a home?

This research makes three major contributions to the literature. First, the study employs a rigorous approach to draw causal inference by using discrete-time survival analysis with propensity score matching. The study examines important covariates affecting the selection into marriage compared to remaining unmarried and controls for these covariates through a series of matched samples using several approaches. The application of propensity score matching surpasses the conventional covariance control approach which is limited and cannot yield valid conclusions about causal inference (Guo & Fraser, 2009). Second, the samples of low-income households used in this study are of particular interest to policy makers. Low-income households are the primary population targeted by social policies aimed at increasing rates of both marriage and homeownership. Poverty reduction strategies that encourage marriage and homeownership continue to be a major priority in the current administration as part of an effort to strengthen and support society's most disadvantaged families. Third, this study has important implications for asset-building policy and marriage promotion initiatives. In the face of criticism about marriage promotion as an asset building policy, our findings suggest that efforts to support marriage among disadvantaged couples may have unforeseen economic benefits that make this policy approach especially valuable.

Background and Theoretical Framework

Although extensive research has been conducted on the transition to homeownership, few studies have focused on the relationship between marital status and homeownership among lower-income households. One exception is the recent study by Hendershott, Ong, Wood, and Flatau (2009) who examined marital status and marital history as determinants of tenure choice among a sample of Australian couples. These researchers found that as compared to continuously married couples, people younger than the age of 35 years who were not married (i.e., single, divorced, or separated) were 36% less likely to own their homes, and those 35 years and over were 23% less likely to own. Being a widow further lowered the probability of owning a home by 7%.

Other studies that have used marriage as a control variable but not as a primary predictor variable have drawn similar conclusions. In both the United States and Britain, marriage increases the likelihood of home purchase compared to being single (Andrew, Haurin, & Munasib, 2006). Married and unmarried couples with and without children are also more likely to transition into homeownership than either single parents or unpartnered individuals (Clark, Deurloo, & Dieleman, 1994). Further, the transition into owner-occupied quarters occurs in a relatively short period after a transition in household composition (i.e., after the couple unit has formed or the couple has started having children). This finding suggests that the social, psychological, and economic stability associated with couple and family formation may increase the likelihood of a transition to homeownership.

The literature on transitions from renting to homeownership suggest that income, education, race, number of children, and age are associated with the move from renting to owning (Andrew et al., 2006; Mulder & Wagner, 1998). Further, when marriage is included as a control variable, marriage is often linked to the shift from renting to homeownership. However, only a few studies have examined the relationship of marriage to homeownership using data from low-income populations. Taken together, the literature on marital status and homeownership is fragmented. No single study that we could find explicitly examined the affect of marriage on tenure change with a low-income U.S.-based sample using a rigorous analytical approach. The purpose of this research is to address this gap.

Drawing from the sociological and economics literature, we argue that people become homeowners for financial and non-financial reasons. Although financial considerations are clearly important, family life-cycle factors play a crucial role in influencing whether people choose to become homeowners and, if so, whether they can do so. Each of these influences, while both well-substantiated, have often been used in isolation in assessments of tenure change (Clark et al., 1994). However, this study relies on a framework that uses both perspectives.

First, households have financial constraints, such as limited assets or limited income, which can impede the transition to homeownership (Hendershott et al., 2009; Linneman & Wachter, 1989; Plaut, 1987). However, not all people who have the financial ability to become homeowners elect to do so. Why do some people prioritize homeownership when making decisions about how to allocate limited financial resources? We argue that answering this question requires a life course approach to understanding homeownership across the life span.

People often change from renting to homeownership after experiencing a significant life transition such as having children or getting married (Clark et al., 1994; Deurloo, Clark, Dieleman, 1994; Robison & Moen, 2000). Similar to Clark and colleagues (1994) and Huang (2004), we contend that life-cycle factors, specifically marital status and household income, play a role in tenure choice.

In addition, we use a framework outlined by Hendershott and colleagues (2009). In examining the effect of marital status and history on homeownership, Hendershott et al. suggested that unmarried people might be more apt to rent because they tend to be transitory and less likely to be able to afford the high costs associated with home purchase. Moreover, married couples are more likely to require larger living spaces than unmarried individuals; a need that can be better accommodated in the homeownership market. Finally, divorced or separated individuals are less likely to buy a home because of saving constraints that impede house purchase. We draw from this framework, as well as economic and life course perspectives, to guide our study. As such, we hypothesize that married renters will have faster rates of transitioning to homeownership than never married, cohabiting, separated, divorced, or widowed renters.

Method

Data

This study uses a subset of data from the Community Advantage Panel Study (CAPS). CAPS is an annual survey of homeowners in the Community Advantage Program (CAP), a secondary market mortgage loan program. To compare these homeowners with renters, a panel of renters was assembled to function as a control group. The renters were randomly selected within the same neighborhoods as CAPS homeowners, and renters had to meet the same criteria for panel eligibility. To be selected into the renter sample, respondents could not own their primary residence, had to be between 18 and 65 years of age, and had to have an annual income of less than 80% area median income (AMI) or 115% AMI in a predominantly-minority neighborhood. This research used only the panel of 1,530 renters. Prior analysis has demonstrated that the demographic composition of the renter panel is very similar to other nationally representative random samples of lower-income renters (Riley & Ru, 2009).

This analysis used five waves of data from the CAP renter panel, collected annually from 2004 to 2008. As with any longitudinal survey, CAP renters experienced sample attrition. As of 2008, the CAP renter panel consisted of 982 (34%) of the original 1,530 respondents. Of the 982 cases present at baseline and at the fifth data wave, 642 cases had no missing data on marital status and important predictors of homeownership such as income, race, and age and comprised the final analytic sample used for propensity scoring and discrete time analyses. Those excluded from the sample due to attrition and missing data were, at baseline, more likely to be married, more likely to be male, younger, less educated, and have more children than those included in the analytic sample.

Measures

The key outcome measure in this study is time to home purchase. Because each case began the study as a renter, we measured our outcome variable in discrete units (1-4) as the number of data waves that elapsed before a respondent purchased a house. Because our focus was time elapsed to homeownership, if a case was missing homeownership status information at any time point the respondent was right censored, or excluded from the analysis.

The key independent variable of this study is marital status. In CAP, marital status was recorded with six categories: "*Living with a Partner, Married, Widowed, Divorced, Separated, or Never Married*." Respondents who indicated they were separated were excluded from the analysis because it was not clear whether to classify them as married. The remaining respondents were classified as either married or not married. Not married respondents included those who were divorced, widowed, cohabiting, or never married.

All models include socio-demographic control variables. Characteristics used in propensity score matching are measured at Year 1 of CAP participation. Respondents self-reported gender (1 = *female*, 0 = *male*) and race. Race was measured using four categories that simultaneously captured race/ethnicity and entered models as indicator variables for African American, Hispanic, and Other. White was the reference category. Age at baseline was measured in years and recorded as an integer. CAP collected data on education by level of education completed. This variable is treated as an ordinal variable in the analysis because small cell counts for some tiers made indicator variables problematic in models. Education level answers ranged from "*11th grade or less*" (1) to "*graduate degree*" (8). Four respondents who indicated their education was "non-traditional" were recoded to missing. Number of children in the household was constructed using the respondent reported household roster, which was a count of all household members aged 0 to 17 years. Employment was recoded into an indicator variable (1 = *employed*, 0 = *non-employed*) from four response categories: employed, unemployed, out of labor force, retired. Income was measured in $1,000s of nominal dollars. The analysis controlled for the characteristics of the respondent's Census tract at baseline including median tract house value, median tract rent, and tract disadvantage score. The tract neighborhood disadvantage score was constructed from several other tract level items in the 2000 Census: percent unemployed, percent in poverty, percent on public assistance, and percent single-headed households with children (Caughy, Hayslett-McCall, & O'Campo, 2007). A well-established relationship exists between neighborhood characteristics and an individual's decision to purchase a home. We included tract-level characteristics to enable isolation of the effect of marriage on homeownership from the effect of neighborhood context. Although most of the variables discussed above are fixed characteristics, income and employment change over time. For the logistic models used to create the propensity scores, we measured all characteristics at baseline. For the subsequent survival analysis, we used time-varying measures of employment and income.

Analysis

The research hypothesis central to this study aimed to test a causal relationship: Does marital status cause transition into homeownership? In the past 30 years, researchers have recognized the need to develop more efficient approaches for assessing treatment effects from studies based on observational data. This growing interest in seeking consistent and efficient estimators of causal effects led to a surge in work focused on estimating average treatment effects under various sets of assumptions (e.g., Heckman, 1978, 1979; Rosenbaum and Rubin, 1983).

Researchers have found that the conventional covariance control approach has numerous flaws and should be replaced by more rigorous methods to draw causal inference. For instance, Sobel (1996) criticized the common practice in sociology that uses a dummy variable (i.e., treatment versus nontreatment) to evaluate the treatment effect in a regression model (or a regression-type model) using survey data. In this paper, we use the term “treatment” and “nontreatment/control” in a broad sense, that is, they are used under the setting of observational studies and refer to conditions associated with the central “cause” being studied. More specifically, treatment in this study denotes being married, and nontreatment or control denotes not being married. The primary problems of covariance control approach discussed in the literature may be summarized as follows: (1) the dummy treatment variable is specified by these models as exogenous, but in fact it is not, and determinants of incidental truncation or sample selection should be explicitly modeled first, and selection effects should be taken into consideration when estimating causal impacts on outcomes (Heckman, 1978, 1979); (2) the strongly ignorable treatment assignment assumption (i.e., conditional upon covariates, the treatment assignment is independent from outcomes under both treatment and control conditions) is prone to violation in observational studies; under such condition, the presence of the endogeneity problem leads to a biased and inconsistent estimation of the regression coefficient (Berk, 2004; Imbens, 2004; Rosenbaum & Rubin, 1983); and (3) covariance control does not automatically correct for nonignorable treatment assignment (Guo & Fraser, 2009).

To draw valid causal inference, this study applies the Neyman-Rubin counterfactual framework (Morgan & Winship, 2007; Neyman, 1923, Rubin, 1974, 2006) as a conceptual model to guide the data analysis. Under this setting, a counterfactual is a potential outcome, or would have happened in the absence of the cause (Shadish, Cook, & Campbell, 2002); and a *counterfactual framework* emphasizes that individuals selected into either the treatment or the nontreatment group have potential outcomes in both states: that is, the one in which they are observed and the one in which they are not observed. The Neyman-Rubin framework offers a practical way to evaluate counterfactuals. Working with data from a sample that represents the population of interest, the standard estimator for the average treatment effect is seen as the difference between two estimated medians from the sample data as:

$$\hat{\tau} = Median\ \ (\hat{T}_1 \mid w = 1) - Median\ \ (\hat{T}_0 \mid w = 0)\,,$$

where $\hat{T}_1$ is the survival time under the treated condition, $\hat{T}_0$ is the survival time under the control condition, and w is binary variable indicating treatment receipt (i.e., w = 1, treatment; and w = 0, control).

Specifically, this study used the following methods to balance data to draw a valid causal inference: (a) a discrete-time survival analysis applied to the original sample without matching;

(b) a propensity score greedy matching (i.e., the nearest neighbor within caliper matching) followed by a discrete-time survival analysis; (c) a propensity score optimal pair matching that uses the generalized boosted regression to estimate the propensity score and a follow-up discrete-time survival analysis; and (d) a propensity score optimal full matching that uses the generalized boosted regression to estimate the propensity score and a follow-up Hodges-Lehmann aligned rank test. Key features of these analytic methods are described below.

The Discrete-Time Survival Model. The outcome variable in this study (i.e., timing to house purchase) is a time-to-event variable that involved data censoring. That is, within the 4-year study period, we knew the exact times of homeownership for only a portion of the study participants, and the remainder of the event times was known only to exceed (or to be less than) the maximum 4 years. Specifically, our study data were interval censored (Hosmer & Lemeshow, 1999), because study participants were contacted every 12 months, and time was accurately measured only in multiples of 12 months. To analyze this type of censored data, we used the discrete-time survival model (Allison, 1982). In accordance with censoring pattern embedded in the data collection, we used 1 year as a discrete time unit. The probability of house purchasing based on the person-time data is a proxy of hazard rate of house purchasing.

In this study, participant’s employment status, number of children, and income were analyzed as time-varying covariates. In addition, the discrete-time model specifies the following time-fixed covariates: age at baseline; gender; dummy variables measuring race (i.e., the dummy variable “African American” and the dummy variable “Hispanic and others”), with “White” used as a reference; education; median house value of the participant’s neighborhood at baseline; median rent value of the participant’s neighborhood at baseline; the disadvantage score of the participant’s neighborhood at baseline; and the key variable to test the research hypothesis, which was the binary marital status. In this study, all neighborhood variables refer to the Census tract where a study participant was living at baseline.

The discrete-time survival model is the key analytic approach for studying the outcome difference between married and unmarried people. This model is applied to the original unmatched sample as well as to matched samples designed to correct for selection bias. When applied to the original sample, the method is the survival model using traditional covariance control. When applied to matched samples, the effect of marital status on timing of house purchasing more accurately represent the causal impact of interest.

The optimal pair matching strategy used in this study creates multiple pairs or strata, and each pair/stratum contains one married and one non-married participant. Therefore, study participants in the matched sample are also nested within matched pairs. This study follows Kalbfleisch and Prentice (2002) to control for "*pairwise dependency*" (or within-pair correlated survival times), and employs the Huber-White method to estimate robust standard errors for the logistic regression model, where the participant's pair membership is specified as a cluster variable.

Propensity Score Greedy Matching. To create valid counterfactuals for treated participants, this study used the propensity score greedy matching technique (Rosenbaum & Rubin, 1983, 1985), which involved the following steps.

First, it uses the binary logistic regression to estimate propensity score of receiving treatment (i.e., being married). By definition, a propensity score is a conditional probability of a participant receiving treatment given observed covariates. More precisely, the propensity score is a balancing score representing a vector of covariates or the so-called "conditioning variables". The advantage of the propensity score matching is its reduction of dimensions: the conditioning variables the study aims to match may include many covariates. The propensity score approach reduces all this dimensionality to a one-dimensional score. Doing so, it eases the burden of finding matches within the study sample. Following Rosenbaum and Rubin (1985), this study employs the logit of the predicted probability from the logistic regression as a propensity score

$$(\text{i.e., } \hat{q}(x) = \log[(1 - \hat{e}(x)) / \hat{e}(x)]$$

where $\hat{e}(x)$ is the predicted probability from the logistic regression), because the distribution of $\hat{q}(x)$ approximates to normal. The logistic regression employs the same set of independent variables as those used in the discrete-time model, except that the three time-varying variables (i.e., participant’s employment status, number of children, and income) are specified as time-fixed variables and measured at the time point of baseline.

Second, it matches the treated participants to controls on the estimated propensity scores to make the estimate of counterfactuals (i.e., outcome values of the comparison group) more valid. This study employs the nearest neighbor within a caliper matching (Rosenbaum & Rubin, 1985). The method selects a control participant *j* as a match for treated participant *i*, if and only if the absolute distance of propensity scores between the two participants (i.e., the difference between propensity scores P_i and P_j) meets the following condition:

$$\| P_i - P_j \| < \varepsilon,$$

where ε is a prespecified tolerance for matching, or a caliper. Rosenbaum and Rubin (1985) suggest using a caliper size of a quarter of a standard deviation of the sample estimated propensity scores (i.e., $\varepsilon \leq .25\sigma_P$, where σ_P denotes standard deviation of the estimated propensity scores of the sample).

Finally, based on the matched sample, the study conducts the discrete-time model to study outcome difference between treated and control participants. As depicted earlier, this analysis is expected to provide more valid estimate of causal effect, because it utilizes a sophisticated control of selection bias.

Propensity Score Optimal Matching. As discussed by Guo and Fraser (2009), a greedy matching method has several limitations. In dividing matching into a series of discreet decisions, it fails to account for the effect of a given match on the overall efficiency of matching. It can sometimes produce too many unmatched cases or too many inexactly matched cases. Finally, it requires a sizable common supported region to work efficiently. To overcome these limitations, this study applies the optimal matching method (Rosenbaum, 2002).

The optimal matching method uses the network flow theory to optimize the creation of matched sample. A primary feature of network flow is that it concerns the cost of using *b* for *a* as a match, where a *cost* is defined as the effect of having the pair of (*a*, *b*) on the total distance of propensity scores.

Initially, we have two sets of participants: the treated participants are in a set *A* and the controls are in a set *B*, with $A \cap B = \varnothing$. The initial number of treated participants is $|A|$ and the number of controls is $|B|$, where | . | denotes the number of elements of a set.

For each $a \in A$ and each $b \in B$, there is a distance, δ_{ab} with $0 \leq \delta_{ab} \leq \infty$. The distance measures the difference between *a* and *b* in terms of their observed covariates, such as their difference on propensity scores. Matching is a process to develop *S* strata (A_1, ...A_s; B_1, ...B_s) consisting of *S* nonempty, disjoint participants of *A* and *S* nonempty, disjoint subsets of *B*, so that $|A_s| \geq 1$, $|B_s| \geq 1$, $A_s \cap A_{s'} = \varnothing$ for $s \neq s'$, $B_s \cap B_{s'} = \varnothing$ for $s \neq s'$, $A_1 \cup \ldots \cup A_S \subseteq A$, and $B_1 \cup \ldots \cup B_S \subseteq B$.

By this definition, a matching process produces S matched sets, each of which contains $|A_1|$ and $|B_1|$, $|A_2|$ and $|B_2|$, ...and $|A_S|$ and $|B_S|$. Notice that, by definition, within a stratum or matched set, treated participants are similar to controls in terms of propensity scores. Depending on the structure (i.e., the ratio of number of treated participants to control participants within each stratum) the analyst imposes on matching, we may classify matching into the following three types: pair matching, matching using a variable ratio, and full matching. Optimal matching is the process of developing matched sets ($A_1, \ldots A_s$; $B_1, \ldots B_s$) with size of (α,β) in such a way that the total sample distance of propensity scores is minimized. Formally, optimal matching minimizes the total distance Δ defined as

$$\Delta = \sum_{s=1}^{S} \omega(|A_s|,|B_s|)\delta(A_s, B_s),$$

where $\omega(|A_s|,|B_s|)$ is a weight function.

Using the R program *optmatch* (Hansen, 2007) and following the guidelines suggested by Rosenbaum (2002), this study employs optimal pair matching and optimal full matching. Using the matched sample created by optimal pair matching, we conducted the discrete-time survival analysis as previously described. Next, using the matched sample created by optimal full matching, we conducted the Hodges-Lehmann aligned rank test, which is described later in this article.

Generalized Boosted Regression. Our analyses also used generalized boosted regression (GBR) to estimate the propensity scores (McCaffrey, Ridgeway, and Morral, 2004) created using a program developed by Schonlau (2007) for the optimal matching. One of the problems with the binary logistic regression is specifying an unknown functional form for each predictor. If specifying functional forms can be avoided, then the search of a best model involves fewer subjective decisions, and therefore, may lead to a more accurate prediction of treatment probability.

GBR is a general, automated, data-adaptive algorithm that fits several models by way of a regression tree, and then merges the predictions produced by each model. As such, GBR can be used with a large number of pretreatment covariates to fit a nonlinear surface and predict treatment assignment. GBR is one of the latest prediction methods that have been rapidly adopted by the machine learning community as well as mainstream statistics research (Guo & Fraser, 2009). From a statistical perspective, the breakthrough in applying boosting to logistic regression and exponential family models was made by Friedman, Hastie, and Tibshirani (2000).

Checking Covariate Imbalance. An important task for propensity score matching is to check covariate imbalance before and after matching. The analyst hopes that after matching, most study covariates are balanced between treated and control groups. This study employs chi-square test and independent sample t test to check covariate imbalance before and after greedy matching, and the imbalance indexes developed by Haviland, Nagin, and Rosenbaum (2007) to check covariate imbalance before and after optimal matching. The study employs a Stata ado program *imbalance* developed by Guo (2008a) to conduct this analysis. d_X and d_{Xm} are the two statistics generated by the imbalance check, and can be interpreted as the difference between treated and control groups on X in terms of standard-deviation unit of X. d_X indicates imbalance on X before matching, and d_{Xm} indicates imbalance on X after matching. Typically, the analyst expects to have $d_X > d_{Xm}$, because she or he expects the need to correct for imbalance before matching, and the sample balance improves after matching.

The Hodges-Lehmann Aligned Rank Test. The outcome analysis following optimal full matching is complicated because the survival model analyzing event times for a matched sample needs to consider correlated survival times within matched stratum. Ideally, the analyst might use a frailty model that included random effects to represent extra heterogeneity of the unit that gives rise to the dependence of event times (Hougaard, 2000; Kalbfleisch & Prentice, 2002). Although a frailty model is fruitful in matched studies with a randomized experiment, that approach has not yet been developed for observational studies such as the current study. Thus, we used the Hodges-Lehmann aligned rank test (Hodges & Lehmann, 1962) to evaluate outcome differences between treated and control participants for the sample generated by the optimal full matching.

The aim of the Hodges-Lehmann aligned rank test is to produce a crude estimate of the difference on the time-to-event data between study groups, and to gauge whether that difference is statistically significant. However, the Hodges-Lehmann approach has three inherent limitations: (a) it treats the length of time-to-event as uncensored; (b) it analyzes mean difference rather than differences of other statistics that better account for skewed distribution of survival times; and (c) it is bivariate and fails to control for covariates of the survival times.

Using the Hodges-Lehmann method, we evaluated the sample average treatment effect by assessing a weighted average of the mean differences between treated and control participants of all matched sets, as:

$$\hat{\delta} = \sum_{i=1}^{b} \frac{n_i + m_i}{N} \left[\overline{T}_{0i} - \overline{T}_{1i} \right],$$

where i indexes the b matched strata, N the total number of sample participants, n_i the number of treated participants in the i^{th} stratum, and m_i the number of controls in the i^{th} stratum, and $\overline{T}_{0i}, \overline{T}_{1i}$ the mean survival times corresponding to the control and treated groups in the i^{th} stratum.

The Hodges-Lehmann aligned rank test (Hodges & Lehmann, 1962; Lehmann 2006, pp.132-141) further tests whether this average treatment effect differs to a statistically significant degree. The study employs a Stata ado program *hodgesl* developed by Guo (2008b) to conduct this analysis.

Findings

Using listwise deletion, cases with missing data were removed from the sample, producing an analytic sample that contained 642 participants. Of those, 163 (25.1%) participants were married, that is, they were either married at baseline or married during the 4-year study period, and the remaining 479 (74.6%) participants were not married at any point during the study period.

<Insert Table 1 about here>

Table 1 presents sample descriptive statistics and imbalance checks before and after matching. As it shows, the overall sample before matching is not balanced on various covariates. For example, the table shows that the original sample contains more married males than married females, and the difference is statistically significant ($p < .001$). Other significant covariates predicting differences on marital status include race, age, education, number of children, employment status, income, and participant's census tract disadvantage score. Table 1 also shows statistically significant results of chi-square tests for categorical covariates and an independent sample *t* test for continuous covariates. Had these differences in marital status not been taken into consideration in causal inference about marital status on transition to homeownership, the findings would be biased.

The sample sizes after greedy matching and optimal matching are also presented in Table 1. Greedy matching paired 146 married participants with 146 unmarried participants. After optimal pair matching, the matched sample contained 163 married and 163 unmarried participants. Optimal full matching retained all 163 married and 479 unmarried participants (the full analytic sample) and grouped these participants in matched strata. The ratio of treated (married) participants to controls (unmarried) participants varied by stratum, but the married participants within each stratum shared a propensity score similar to that of the unmarried participants within the stratum.

As indicated in the table, both greedy matching and optimal matching improved sample balances. After greedy matching, none of the 10 covariates showed significant differences. Before optimal matching, the absolute standardized difference in covariate means before optimal matching (i.e., d_X) generally has a higher value than the index after optimal matching (i.e., d_{Xm}). Taking gender as an example, before optimal matching, d_X of gender was 0.375, meaning that the treated and control groups were almost half a standard deviation apart on gender. After optimal pair matching, d_{Xm} of the same covariate is 0.151, meaning that the difference between the two groups is 15% of a standard deviation for gender. After optimal full matching, d_{Xm} of gender is 0.036, meaning that the difference between the two groups is 4% of a standard deviation for gender. The value of most covariates decreases from d_X to d_{Xm}, suggesting that optimal matching indeed improves balances. The worst-case scenario for the optimal pair matching occurs for the covariate "Census tract median house value" for where d_X equals .224, and the worst-case scenario for the optimal full matching occurs for the covariate "age at baseline" for where d_X equals .255. Given that these imbalances can be considered as small, we concluded that matching improved sample balances and all matched samples were acceptable.

When conducting propensity score matching, it is important to take into account whether the treatment (married) and control (unmarried) groups have similar propensity scores. Similarity in these is an important aspect of matching because the two groups must have sufficient overlap in their propensity scores in order to match cases and create a matched sample. Figure 1 shows the distribution of propensity scores we estimated using binary logistic regression. The two groups had a sizable common support region; as result of this similarity, the greedy matching procedure resulted in only a small reduction in the matched sample. Figure 2 shows the box-plots and histograms of the estimated propensity scores derived using generalized boosted regression. As the figure indicates, the two groups differed on the distribution of estimated propensity scores, sharing a very narrow common-support region. This narrow region of common-support is especially problematic because, if we applied nearest-neighbor matching within caliper or other types of greedy matching, the narrow common-support region would produce a great loss of matched participants. However, this is not a problem when using optimal matching: both pair matching and full matching created matches for all 163 treated participants. Further, the reduction of the sample size occurred only with number of controls in the pair matching where, by design, each participant matches only one control.

<Insert Figure 1 and Figure 2 about here>

Table 2 presents results of the survival analysis. For this study, all four models estimating differences in timing of house purchase between married and unmarried people show consistent findings. The estimated odds ratio of purchasing a home for the original unmatched sample is 1.979 ($p < .001$), indicating the odds of homeownership for married participants was 97.9% higher than odds of homeownership for unmarried participants [i.e. (1.979-1)*100=97.9%]. Similarly, the odds ratio of house purchase for the matched sample created by greedy matching was 1.985 (98.5% higher; $p < .01$) and for the matched sample created by the optimal pair matching is 1.611 (61.1% higher; $p < .05$); both findings indicating that married participants purchased houses during the study period at a faster speed than the unmarried participants.

<Insert Table 2 about here>

Using the optimal full matched sample, we found the length of time married participants took to purchase a house was 0.646 year shorter (approximately 7.75 months shorter) than the unmarried participants. The Hodges-Lehmann aligned rank test showed that this was a statistically significant difference ($p < .001$).

Other significant predictors of timing of house purchase included participant's age at baseline ($p < .01$), educational attainment, ($p < .01$), and income ($p < .001$). In general, participants who were younger, had attained higher levels of education, and had higher income than their study counterparts were also more likely to purchase houses at a faster time-to-event rate.

In summary, this study in general confirms the research hypothesis and shows positive impact of marriage on homeownership. This conclusion is valid given that the models include important covariates affecting the selection between getting married and not getting married, and we control for these covariates through a series of matched samples using various propensity score analytic approaches.

Discussion

The purpose of this research was to explore the causal relationship between marriage and the transition into homeownership among low- and moderate-income renters. Using 5 years of data from the CAPS, this study addressed the question of whether marital status causes more frequent and faster transitions into homeownership. Our findings suggest that even after using propensity score analysis to control for selection bias between married and unmarried groups, low- to moderate-income married couples have higher odds of buying a home and purchase homes at faster rates when compared to their nonmarried counterparts.

Our findings are aligned with the theoretical framework used to develop the study's hypothesis. Economic and life course perspectives suggest that married individuals are more apt to purchase a home because of fewer borrowing constraints (Linneman & Wachter, 1989) and specific life-course events (Clark et al., 1994) that make owning more likely. Further, unmarried households are less likely to buy a home because of the transitory nature of their lives and the reduced need for large living quarters (Hendershott et al., 2009).

Most who give our results a superficial examination will suggest our findings capture only the ability of married participants to pool their social and economic resources with those of their partner to generate sufficient capital for investment in homeownership. Although that phenomenon is surely present in our study, our analyses controlled for both wealth and income as the cases were matched. Thus, we suggest that other mechanisms were also influential. Three alternative explanations exist that may help account for our findings. First, we contend that our results reflect the strong normative pressures exerted on individuals within our society to time life course events in specific patterns. There is an expectation that marriage precedes home purchase in the sequence of life stages (Townsend, 2002). Individuals respond to this social pressure by shaping their expectations, their aspirations, and, ultimately, their behavior.

Second, we suspect that institutional and environmental factors condition the ability of unmarried people to purchase homes relative to that of married couples. For example, unmarried individuals may have more difficulty receiving mortgage loans or may receive less attractive loan terms because mainline banking institutions are more likely to perceive unmarried persons as less responsible and less worthy credit risks than their married peers. Similarly influenced by social norms, real estate agents may be less likely to show top properties to unmarried clients, and instead interact with them in ways that discourage home purchase. Finally, given that homeowners are usually married, the housing stocks may be more closely aligned with the tastes and needs of married couples, thus leaving a portion of unmarried individuals unable to find a suitable property. Any of these scenarios reduce the likelihood of homeownership for unmarried persons relative to married persons beyond the measured individual characteristics.

It is equally important to consider what aspects of the marital relationship drive our findings. Few would suggest that the legal proceedings that define marriage, per se, make a couple more likely to buy a home. Instead, we suggest that the changing social status and life course position of the relationship alter the social norms faced by the couple, as discussed above. In addition, common attributes of marriage, such as stability, may be the crucial determinate that explains our results rather than the marriage ritual itself.

Several limitations to our study are worth acknowledging. The first is that propensity score matching fails to correct for selection bias due to unmeasured variables. Unlike randomized clinical trials that balance data on both measured and unmeasured variables, propensity score matching cannot correct for hidden selection bias (Guo & Fraser, 2009). Thus, if there are important variables predicting marriage omitted by our matching process, our study findings are prone to errors. The fact that our findings are consistent across all the samples—matched and unmatched—is not uncommon in observational studies in which the cause has a strong impact on effect. Although it is likely that the matched studies may have missed important covariates affecting sample selection, and therefore, the findings from the matched studies are still prone to bias (precisely, prone to hidden selection bias), the results of the impact of marital status on transition into homeownership for this low-income sample are revealing.

Our study makes three substantial, unique contributions to the field. First, it offers one of the first attempts to address self-selection into marriage using propensity score analysis. Most studies on the impact of marriage fall short because they not only fail to model the choice individuals make to be married but also fail to model how that choice affects the outcome of interest. Instead of the conventional covariance control approach, which cannot adjust for selection bias, we used an innovative and rigorous method that allows us to draw causal inference between marital status and the transition to homeownership. The second major contribution of our study is our focus on a low- and moderate-income population. This population is of great interest to policymakers and has been the core recipient of social policies aimed at increasing and sustaining both marriages and homeownership. However, no study to date has examined the relationship between marriage and homeownership among lower-income households.

Third, our findings have important implications for asset-building policy and marriage promotion initiatives. The past decade has witnessed an increasing number of calls to promote marriage as an asset-building strategy among the poor (Solot & Miller, 2002). However, critics have countered that the benefits of marriage have been oversold and that marriage promotion has not translated to reductions in poverty. Further, women's increasing economic independence and access to employment opportunities have been cited as evidence that promoting marriage is an outdated approach to asset building (Seefeldt & Smock, 2004).

Although this study focused specifically on homeownership rather than asset-building in general, we nonetheless conclude that one benefit of marriage is its function as a catalyst to homeownership for lower-income families. Previous studies have provided strong evidence that responsible homeownership can yield meaningful wealth returns, even for low-income families, when purchased as a long-term investment (Riley, Freeman, & Quercia, 2009). Given the social and economic gains that may be gleaned from marriage, and in turn from homeownership, policy makers may want to formulate well-coordinated policies aimed at concurrently increasing marriage and homeownership opportunities. Such policies could be of significant benefit to disadvantaged families who struggle to achieve economic and familial stability.

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Table 1. Sample Description and Imbalance Check before and after Matching

	% of Married People or Mean (*SD*) of the Covariate by Group[a]			Absolute Standardized Difference in Covariate Means after Matching (d_{xm})	
Covariate	**Overall Sample before Matching (N = 642)**	**Sample after Greedy Matching (N = 292)**	**Absolute Standardized Difference in Covariate Means before Matching (d_x)**	**Sample after Optimal Pair Matching (N = 326)**	**Sample after Optimal Full Matching (N = 642)**
Number of married people	163	146		163	163
Number of non-married people	479	146		163	479
Number (%) of married people lost after matching		17(10.4%)		0(0%)	0(0%)
Gender			.375	.151	.036
Male	37.7%***	50.5%			
Female	21.1%***	49.7%			
Race					
White	26.7%***	51.0%			
African American	16.6%***	48.1%	.361[b]	.063[b]	.048[b]
Hispanic	42.0%***	47.7%	.335[b]	.052[b]	.132[b]
Other	42.9%***	57.1%			
Age at baseline			.621	.112	.255
Married people	34.96(10.96)***	35.40(11.15)			
Non-married people	42.46(13.09)***	34.18(10.20)			
Education at baseline (measured as an ordinal variable)			.296	.120	.017
Married people	3.71(2.08)***	3.63(2.00)			
Non-married people	3.12(1.86)***	3.77(2.14)			
Number of children at baseline			.285	.089	.058
Married people	1.00(1.17)**	.90(1.07)			
Non-married people	.68(1.07)**	.95(1.30)			
Employment status at baseline			.296	.100	.226
Working	29.7%**	47.5%			
Not working	18.4%**	57.5%			
Income at baseline (in $1,000)			.437	.039	.097
Married people	24.83(13.85)***	24.39(14.03)			
Non-married people	19.08(12.41)***	23.81(13.83)			
Census tract's median house value			.008	.224	.171
Married people	91794.5(37914.8)	92892.5(39022.5)			
Non-married people	91489.6(38533.7)	95115.1(38950.0)			
Census tract's median rent value			.166	.174	.184
Married people	483.07(127.21)	483.97(128.38)			
Non-married people	461.38(134.39)	490.68(140.47)			
Census tract's disadvantage score			.224	.032	.001
Married people	.09(.54)*	.10(.54)			
Non-married people	.23(.67)*	.06(.57)			

[a]Each entry is the percent of homeowners in the categorical covariate, or the mean [SD] of the continuous covariate by group;
***p < .001, **p < .01, *p < .05, chi-square test or independent-sample t test two-tailed.
[b]Race is recoded as two dummy variables (i.e., African American, Hispanic and Others) using White as a reference.

Figure 1(a). Histograms of Propensity Scores Estimated by Logistic Regression

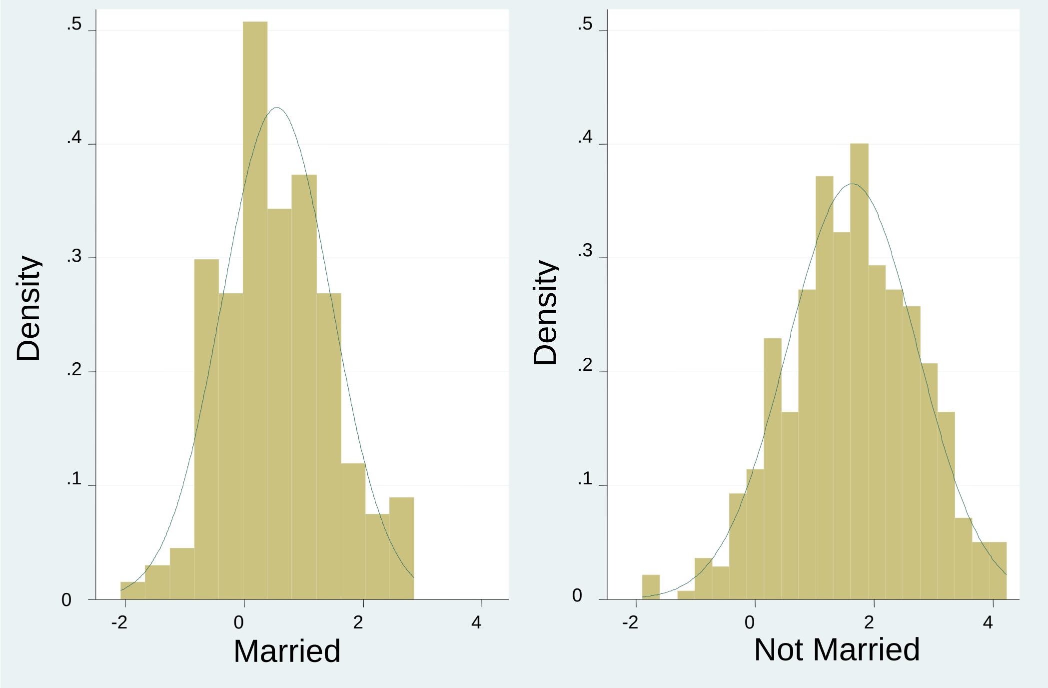

Figure 1(b). Boxplots of Propensity Scores Estimated by Logistic Regression

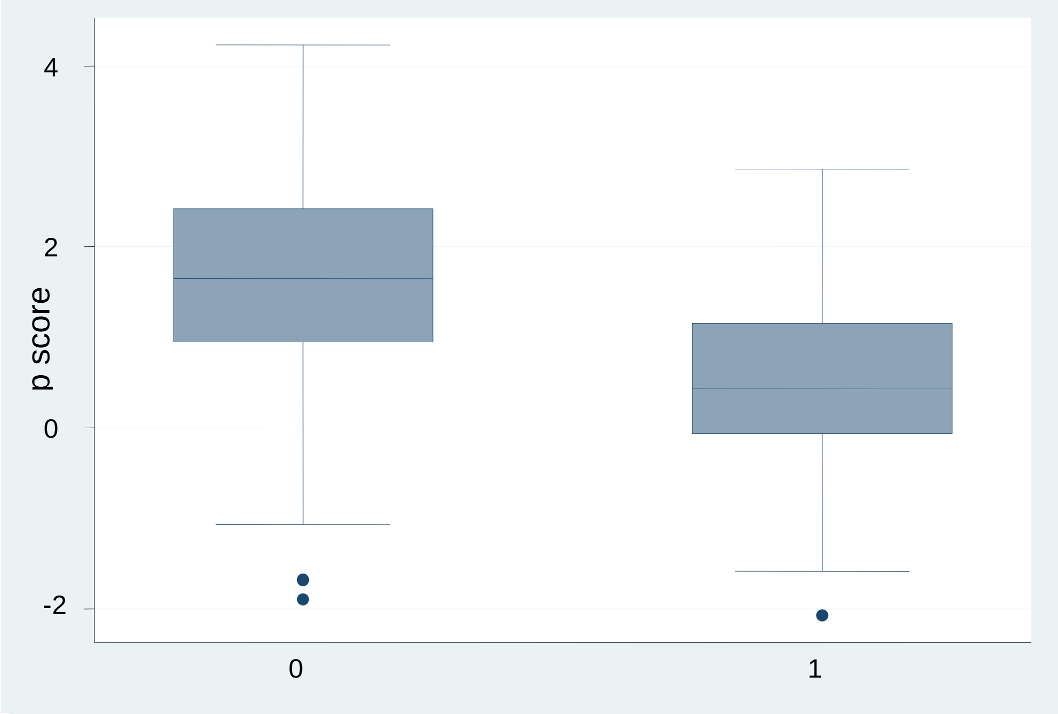

Figure 2(a). Histograms of Estimated Propensity Scores by Generalized Boosted Modeling

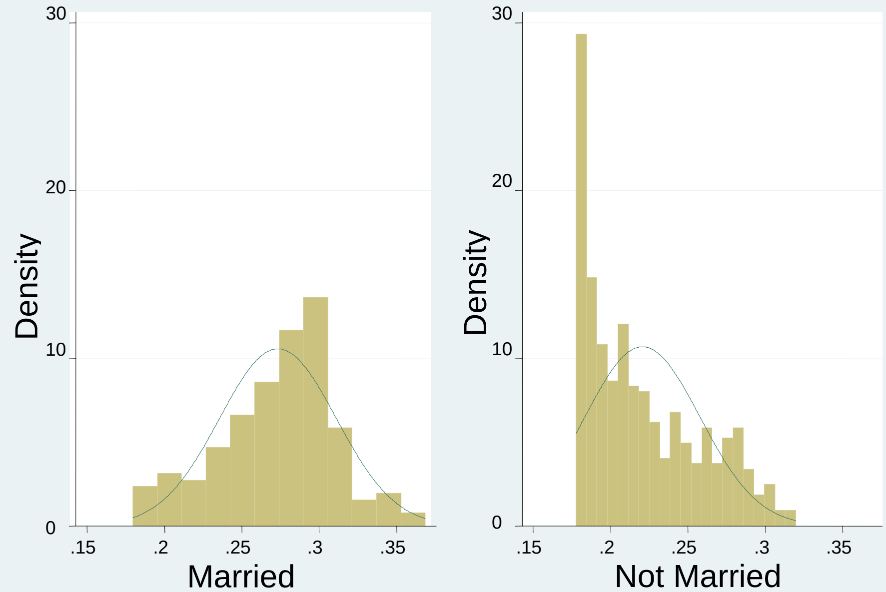

Figure 2(b). Boxplots of Estimated Propensity Scores by Generalized Boosted Modeling

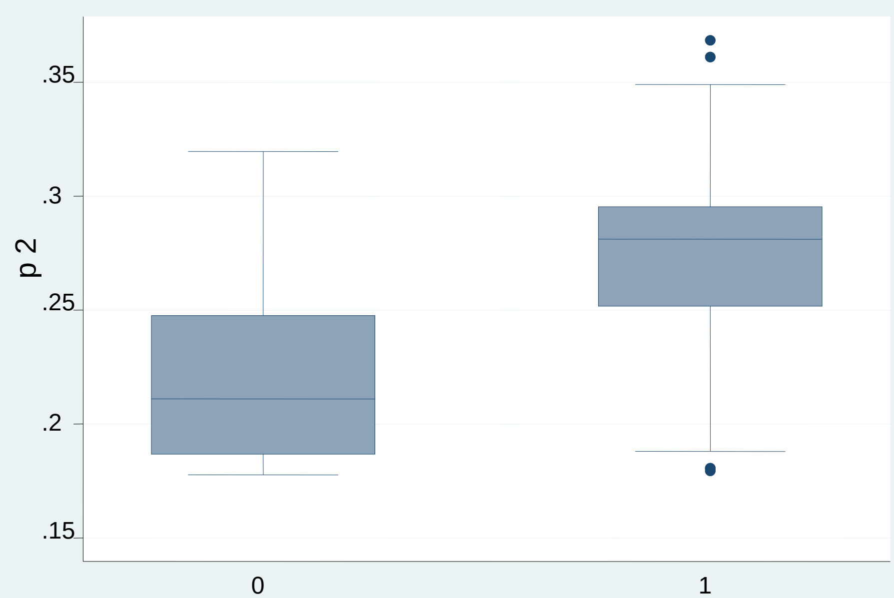

Table 2. Results of the Discrete-Time Models and the Hodges-Lehmann Aligned Rank Test

Covariate	**Estimated Odds Ratio from the Disrete-Time Model for the Overall Sample before Matching (*N* = 642)**	**Estimated Odds Ratio from the Discrete-Time Model for the Sample after Greedy Matching (*N* = 292)**	**Estimated Odds Ratio from the Discrete-Time Model for the Sample after Optimal Pair Matching (*N* = 326)**	**Mean Difference of Time-to-Event with the Hodges-Lehmann Aligned Rank Test for the Sample after Optimal Full Matching (*N* = 642)**
Marital status (Non-married)				
Married	1.979***	1.985**	1.611*	-.646***
Gender (Female)				
Male	.835	.930	.853	
Race (White)				
African American	.908	.976	1.212	
Hispanic and Other	1.167	.943	1.045	
Age at baseline	.973**	.973*	.977*	
Education at baseline (measured as an ordinal variable)	1.159**	1.225**	1.171**	
Number of children -- time-varying covariate	1.108	1.083	1.090	
Employment status -- time-varying covariate (Not working)				
Working	1.249	.841	1.008	
Income (in $1,000) -- time-varying covariate	1.027***	1.027***	1.022***	
Year Indicator Variable (Year 4)				
Year 1	1.075	1.254	.998	
Year 2	1.774*	1.883+	1.527	
Year 3	1.250	1.596	1.059	
Model Chi-square (df)	134.47(12)***	64.85(12)***	54.94(12)***	
Pseudo R-square	.120	.101	.074	

Note: Reference group is shown in the parenthesis for the categoridal covariate.
***p < .001, **p < .01, *p < .05, +p < .1; two-tailed test.