

Homeownership Subsidies and the Marriage Decisions of Low-Income Households

Michael D. Eriksen*
Department of Real Estate
Terry College of Business
University of Georgia
eriksen@terry@uga.edu
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Abstract

This paper estimates the impact of being randomly assigned down payment assistance with a home purchase on the marriage and divorce decisions of low-income households using unique data from a field experiment. 1,103 participants in Tulsa, Oklahoma were randomly assigned in 1998 to either a treatment group eligible to receive a 2:1 match on saving for a down payment, or a control group that was not eligible. Using data collected on treated and controls 18 and 48 months after randomization, it is shown the offer of the subsidy had important impacts on participants' marriage and divorce decisions. Treated participants who reported being unmarried prior to randomization were 42% more likely to be married 48 months after opening an account than similar control group members. The offer to receive the subsidy is also shown to substantially increase the divorce rates for originally married participants, with the most pronounced effect occurring among women with children or those who reported poor spousal relations prior to randomization. Although the exact mechanisms for how the subsidy affects such decisions are unclear, homeownership is shown to have an important role.

Keywords: Marriage, Divorce, Homeownership, Individual Development Accounts
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* Please send correspondence to eriksen@terry.uga.edu or 206 Brooks Hall, Athens, GA 30602. All errors and opinions are those of the author and should not be taken to represent the views of the University of Georgia.

I. Introduction

Homeownership and asset building policies, more broadly, have emerged as important policy tools over the last two decades targeted to low-income households. These policies seek to build wealth among the poor and provide a pathway to self-sufficiency, in a way that differs from traditional income-support programs that typically provide disincentives for asset accumulation due to means testing. The asset-building policy that arguably has garnered the greatest attention is the Individual Development Account (IDA), pioneered by Sherraden (1991). IDAs are saving accounts that provide low-income households with matching payments when the balances are withdrawn and used for special purposes, such as home purchase, business start-up, and investment in education. They also have been touted as a policy tool that generates substantial social benefits and fosters independence (Schreiner and Sherraden, 2006). By the end of 2006, more than 400 IDA programs were in operation in the United States, and publicly sponsored IDA programs have been enacted in 34 states and the District of Columbia.

This paper reports on the impact of being randomly offered the opportunity to open an IDA on the marital decisions of low-income households using data collected from the first ever randomized field experiment of the program. More specifically, the effects of the offer are evaluated on the marriage and divorce outcomes of an IDA program that took place in Tulsa, Oklahoma, between 1998 and 2003 as part of the American Dream Demonstration. Eligible applicants, those who were employed with prior-year family income below 150% of the poverty level, were randomly assigned to a treatment group, which was allowed to open an IDA, or to a control group, which was not. The program matched IDA withdrawals for new home purchase at the rate of 2:1 and withdrawals for other qualified uses (business start-up or expansion, education, home improvement, and retirement saving) at a 1:1 rate. For each of three years, up

to \$750 in deposits were subject to match, or up to \$6,750 could be accumulated for a down payment related to a home purchase. Participants were interviewed immediately prior to random assignment and approximately 18 and 48 months after assignment.

The prior literature in urban economics has typically assumed marital status is exogenous in modeling tenure choice decisions (e.g., Bourassa, 1995; Hendershott et al., 2009; Painter and Lee, 2009), or looked to differences across race in marriage rates to partially explain homeownership gaps (Charles and Hurst, 2002; Haurin and Rosenthal, 2007). Prior studies modeling formation decisions have primarily focused on decisions faced by young adults (Boersch-Supan, 1986; Hendershott, 1987; Haurin, Hendershott, and Kim, 1993, 1994; Ermisch, 1999).

While prior research on how homeownership subsidies directly influence household formation decisions is rather limited, a robust literature in public economics and demography generally suggests public welfare subsidies discourage marriage and encourage divorce decisions at the margin (Moffit, 1990; Hoffman and Duncan, 1995; Hoynes, 1997; Fitzgerald and Ribar, 2004; Bitler, Gelbach, Hoynes, and Zavodny, 2004).¹ This study's experimental design and focus on a low-income population is most similar to Keeley (1987) that used data from the Seattle and Denver Income Maintenance Experiments (SIME/DIME) and found a substantial increase in divorce rates attributable to receiving subsidies associated with a negative income tax.

A companion paper (Mills et al., 2008) demonstrated using data from the same field experiment that treatment-group renter households had a 25-30% higher homeownership rate than control-group renter households four years after randomization. This paper illustrated the

¹ As marriage was increasingly seen as a route out of poverty towards self-sufficiency during the 1990's, disincentives provided against it by previous policies served as a partial motivating force behind the large-scale welfare reform eventually enacted through the 1996 Personal Responsibility and Work Opportunity Act (Bitler, Gelbach, Hoynes, and Zavodny, 2004).

program had an important impact on the marriage and divorce decisions of participants as well. First, treatment group members who reported being unmarried at the time of randomization were 5.85 percentage points, or 42.1%, more likely than those the control group to be married four years after randomization. Second, originally married participants randomly offered to open the account were 7.41 percentage points, or 51.4%, more likely than those in the control group to be divorced at after randomization. This treatment effect was most pronounced 18 months after randomization and especially for treated females with children who reported a 422.7% increase in divorce rates as compared to similar control group members. In particular, the prospect of independent living in an owner-occupied residence provided by the matched saving incentive increased dissolution; this impact was most pronounced for individuals reporting poor spousal relations prior to randomization.²

As a final exercise, I follow Engelhardt et al. (2010) and use randomization as an instrumental variable to exploit the exogenous variation in homeownership rates caused by the field experiment to examine the causal role of homeownership on marital status.³ A robust literature in urban economics has investigated the consequences of becoming a homeowner on subsequent behaviors; however it has been difficult to address empirical issues related to reverse causality and omitted factors that may confound such an analysis (see Dietz and Haurin (2002) for a review).⁴ If direct wealth effects caused by being randomly offered the subsidy are negligible on marital status, then randomized assignment to the treatment group is a potentially

² As described later in the paper, Mills et al. (2008) did not find a significant difference in homeownership rates between treated and control participants at 18 months suggesting increased divorce rates occurred as treated participants prepared to become homeowners.

³ Engelhardt et al. (2010) used the instrumental strategy to examine the external social benefits of homeownership among low-income populations. Despite such benefits often being used to justify the increasing subsidization of homeownership among this population, the authors found little evidence of their existence.

⁴ For example, Bracher et al. (1993) found homeowners were less likely to get a divorce than renters in a sample of Australian panel of women, but the direction of the causation or whether some other unobserved factor (e.g., wealth) caused both are unclear.

valid instrumental variable. To minimize concerns about weak instrument bias, I focus on baseline renters and find homeownership was a significant determinant of being married 48 months after randomization. More importantly, I find evidence that previous analysis without a valid instrument has understated the causal role of homeownership on marital status.

The paper is organized as follows. Since the analysis relies on the Tulsa experiment, Section II provides a description of the design and implementation of the field experiment. Section III then examines attrition and characteristics of the treatment and control groups at baseline. Much of the material in these two sections draws extensively from and paraphrases the organization and exposition in Mills et al. (2008) and Engelhardt et al. (2010). Section IV briefly reviews previous research findings from the Tulsa experiment. Section V traces out the basic economic framework and econometric strategy. Section VI presents the estimated treatment effects of IDAs on marriage and divorce; Section VII examines the indirect role of homeownership on such decisions. A brief conclusion follows describing the caveats and implications of the research.

II. Experimental Design

The Tulsa IDA program was based on an experimental design.⁵ The program was administered through a partnership between the Bank of Oklahoma, which provided the financial

⁵ The American Dream Demonstration (ADD) was a set of 14 privately funded local IDA programs that began in the late 1990's. They were organized by the Corporation for Enterprise Development (CFED), with technical guidance and research oversight provided by the Center for Social Development (CSD) of Washington University in St. Louis, evaluation funding from the Ford Foundation and the Charles Stewart Mott Foundation, and operational funding from a broad consortium of foundations. The overall ADD evaluation includes a wide array of other non-experimental research activities, conducted by (or under the direction of) the Center for Social Development of Washington University in St. Louis. These include an implementation assessment, participant in-depth interviews and case studies, cross-sectional participant survey, community-level assessment, and benefit-cost analysis. For details, see Schreiner et al. (2002). The Tulsa site was the only experimental site in the ADD program. Abt Associates (2004) provides additional details on the structure of the evaluation, as well as information on the financing, implementation, and management of the project.

services associated with the demonstration, and the Community Action Project of Tulsa County (CAPTC), a multi-service community action agency serving low-income residents in the Tulsa metropolitan area.

The timing of the demonstration was as follows. Enrollment occurred between October, 1998 and December, 1999. Information about the program was distributed through several channels including local media, CAPTC's existing social services, and mailings to other local social service agencies, current and former CAPTC clients, and people who inquired about the program. Individuals submitted an application and were interviewed to establish eligibility, which required that participants were employed with prior-year family income below 150% of the poverty level. There were no limits on assets. Applicants were informed that, if assigned to the control group, they would be unable to enter the IDA program during the four-year study period, provided informed consent with regard to random assignment, and authorized the release of financial information for research purposes.

Once eligibility was determined, individuals were then administered a baseline survey (Wave 1) that collected information on household income, finances, demographics, and other characteristics. Within a week *after* the baseline (Wave 1) interview, applicants were randomly assigned to either the treatment group, which was allowed to participate in the IDA program, or the control group, which was not. The treatment analyzed in the experiment, therefore, is the *offer* to participate in the IDA program.

There were two follow-up surveys.⁶ The first occurred about 18 months after random assignment, from May to August in 2000, and is referred to throughout the analysis below as the

⁶ The mode of the follow-up interviews was telephone. If telephone attempts were unsuccessful, a field interviewer attempted to arrange an in-person interview at the respondent's residence. The average interval between the baseline and Wave 3 interviews was 1,449 days for treatment cases and 1,456 days for controls; the difference is statistically insignificant.

Wave 2 survey. The second follow-up survey occurred approximately 48 months after random assignment, from January to September, 2003, and is referred to below as the Wave 3 survey. Both surveys collected socio-economic information similar to that in the baseline and had retention rates of 69.2% and 76.0%, respectively.

The IDA itself was a regular passbook saving account at the Bank of Oklahoma. Interest rates were about 2 to 3% during the experiment. Fees to open and maintain accounts were waived, except that a participant who made three withdrawals within a twelve-month period was charged \$3 for each additional withdrawal during the period. Participants could not make a matched withdrawal until six months after opening the account.

The experiment provided for matching contributions on up to \$750 of saving annually and gave different incentives for different types of saving.⁷ The largest incentive was saving for home purchase: account withdrawals used for purchase of a primary residence were matched at 2:1. Withdrawals for repair/improvement of a primary residence, post-secondary education, micro-enterprise expansion or startup, or contributions to an IRA were matched 1:1. The qualifying educational uses included (for the participant or the participant's spouse, child, grandchild, or other dependent) the cost of attending a vocational and technical training institution, community college, four-year college, or university; the cost of obtaining a professional certificate or license; or the fees for obtaining a General Educational Development certificate. Therefore, in principle, IDA saving could promote housing, business, or human capital formation.

Importantly, the participant never had direct access to the matching funds, because the match was provided in the form of a check made out to the vendor (e.g., a home mortgage

⁷ Participants who contributed more than \$750 in one year could carry forward the difference as a matchable contribution for the following year. However, individuals who contributed less than \$750 in a year were not allowed the following year to make "catch-up" deposits retrospectively.

lender). IDA deposits made within 36 months of the account opening and used for qualified purposes were eligible for the match. The accountholder had up to six additional months to make final matched withdrawals. Remaining balances could be rolled over (at the participant's request) into a Roth IRA with a 1:1 match. IDA balances in this program did not affect eligibility for Temporary Assistance to Needy Family programs, but could affect eligibility for other public assistance, such as food stamps and Medicaid.

III. Data and Descriptive Statistics

Columns 1-3 of Table 1 reports sample sizes by sex and baseline marital status for each of the three survey waves of the experiment, respectively. As shown in the bottom row of the table, of the 1,103 individuals in the baseline survey 865 or 78%, were women. Hence, the experiment was undertaken on primarily women, the great majority of which had children in the household at baseline. We return to this characteristic of the experimental setting when we discuss the external validity of our findings in the conclusion.

Approximately 76 percent, 838, of baseline members completed interviews at Wave 3.⁸ Retention rates by characteristic are shown in column 4. Column 5 shows the difference in retention rates (as of Wave 3) between the treatment group (T) and the control group (C). For example, in row 1 of panel A, retention rates did not differ at all for females in the treatment versus the control group. In row 1 of panel B, the retention rate was 5.4 percentage points higher for men in the treatment group than men in the control group, but this difference was not statistically significant at conventional levels.

⁸ The relatively high retention rate may be due in part to extensive tracking efforts and the incentives provided, equally for both treatment and control cases. Six tracking letters were sent between the various surveys; sample members received \$10 for each letter to which they responded. At Waves Two and Three, respondents received \$35 for completing the interview.

The other rows in the table show attrition by sex and the presence of children in the household at baseline. While there are few statistically significant differences in the sub-samples beyond that expected from chance alone, the economic magnitude of attrition among some subsets of men was quite large. Therefore, in our analysis below, results are presented for the whole sample as well as for sub-samples of women and women with children that did not feature differential attrition.

Table 2 presents baseline demographic and economic characteristics for single (i.e., never married, divorced, and widowed) and married participants, respectively. In particular, the first three columns of Table 2 present baseline means for the combined, treatment-group, and control-group samples of single individuals. As reported in column 1, single participants on average were 36 years old and had a monthly household income of \$1,346 with 35.5% living below federal poverty limits. About 90% of sample members were female, 42% were non-Hispanic Caucasian, and 45% were African-American. About 6% had no high school diploma or GED, 25% had a high school diploma or GED, and about 69% had attended at least some college. In comparison, married participants at baseline were approximately half men (50.2%), had a monthly household income that was approximately \$400 more per month, and reported slightly more children and incidence of poverty for the household unit.

For each baseline characteristic, the fourth and eighth column of Table 2 shows the differences between the treatment and control groups. Most differences are economically small and none were statistically different from each other at the 10% level. Overall, the lack of substantial differences between each group at baseline suggests that the randomization of

treatment status was implemented successfully.⁹

The other main conclusion to be drawn from this table is that the households in the Tulsa IDA sample were *not* a representative sample of low-income households. Using the 1998 Survey of Consumer Finances and the Tulsa-area subsample of the 2000 Census Public-Use Microdata sample, Mills et al. (2008) showed those who volunteered to be randomized had a slightly higher household income and were overall less likely to be married, have health-insurance, or have income below federal poverty guidelines than potentially eligible participants. While the randomization appears to have been implemented effectively, the non-representativeness of those who selected into the randomization process has implications for the external validity of our findings. This is discussed further in the conclusion.

IV. Previous Results from the Tulsa Experiment

Mills et al. (2008) showed 89% of participants randomly assigned the offer to open an IDA were successful in eventually opening an account. Almost half of these participants opened their IDA in the first three months after random assignment. Participants kept their accounts open for an average of 38 months. Among treatment-group members, cumulative matchable IDA contributions averaged \$1,110; 53% made the maximum annual contribution of \$750 at least once, and 21% contributed the three-year maximum of \$2,250. As of October 2003, 40% of treatment group members had taken a matched withdrawal, and 77% had taken at least one unmatched withdrawal. Unmatched withdrawals accounted for 79% of all withdrawal transactions and 54% of all withdrawn funds. Among treatment group members, 39% made contributions, withdrew all of the deposits in unmatched withdrawals, and closed the account.

⁹ Mills et al. (2008) and Abt Associates (2004) present comprehensive analyses of participant characteristics and treatment versus control group comparisons at baseline along many other characteristics than those shown in Table 2, all of which indicate that randomization was implemented effectively.

Combined with the fact that 11% did not open an account, this implies that half of all treatment group members made no matched withdrawals.

Average matched and unmatched withdrawals (per transaction) were \$636 and \$194, respectively. Among matched withdrawals, 24% of transactions and 31% of funds withdrawn were for housing down payments. The average matched withdrawal was \$844 for down payments and \$576 for other allowed uses. Thus, the average withdrawal including the match was \$2,532 and \$1,152, respectively.

Finally, it was shown that treatment-group renter households had a 7-11 percentage point higher homeownership rate than control-group renter households four years after randomization. Relative to the control group over the same time span, this represented a sizable 25-30% increase in propensity to be homeownership. Importantly, we found no evidence of increased saving as compared to the controls for the other qualified uses of the IDA matching funds: business start-up, retirement saving, and educational expenses. Therefore, despite its breadth, the IDA program appears to have been *de facto* a program that subsidized saving for home purchase. This is not altogether surprising given the most generous match rate (2:1) was for this form of saving.

V. Methodology

The economic approach to household formation (Becker, 1973) emphasizes the trade-off between the present value of benefits versus the present value of costs in the decision to marry and divorce, respectively. An individual's utility from marriage, for example, will depend on the individual's economic resources (income and wealth), the potential spouse's resources, the resource-sharing rule in the (potential) household, economies of scale in household production and consumption, the degree of specialization in home versus market production, and tastes. In

this framework, an IDA and its matching contributions confer a wealth effect on eligible individuals. Unfortunately, the standard utility-maximizing approach generates ambiguous theoretical predictions as to the sign of the wealth effect on household formation. In particular, depending on the resource-sharing rule and tastes, an increase in wealth may increase or decrease the incentives to form households.

There are two channels for how wealth may impact such decisions that have garnered particular interest in previous research. The first is the complementarity between homeownership and marriage decisions due to economies of scale in the consumption of housing services. A key empirical challenge is identification of the causal impact of incentives for homeownership on household-formation decisions due to unobserved heterogeneity. In particular, individuals who have a taste for (i.e., face strong economic incentives for) marriage may also either have a taste for (i.e., face strong incentives for) homeownership. Unobserved heterogeneity in either tastes for saving or incentives would render typical estimates of the impact of homeownership on household formation biased and inconsistent.

A second channel of particular interest in the existing demography literature is the “independence” effect, whereby an increase in wealth results in less household formation, either through decreased marriage or increased divorce. This occurs because greater economic resources allow individuals to purchase greater autonomy (where independence is a normal good). Again, a fundamental challenge in this literature is that unobserved heterogeneity may render estimates of the impact of wealth on independence biased and inconsistent.

Overall, because of the theoretical ambiguity, the impact of IDAs on marriage and divorce decisions is an empirical exercise. To this end, we estimate the effect of being eligible for an IDA, which is referred to in the program evaluation literature as “intent to treat” (ITT)

estimates.¹⁰ Specifically, we estimate ordinary least squares equations of the form:

$$(1) \quad Y_{ki} = \beta_0 + \beta_1 T_i + \theta X_i + \varepsilon_{ki},$$

where the subscript i refers to the individual sample member, Y_{ki} is the value of an household-formation outcome variable in wave k of the survey ($k=2$ for 18 months after randomization; $k=3$ for 48 months after randomization). T_i takes the value of 1 for treatment group members and zero otherwise, ε is the individual-specific error term, and the β 's are parameters to be estimated with the treatment effect of the offer given by β_1 . To improve the efficiency of the estimated treatment effects and to further account for any random differences between treatment and control groups, X_i is a vector of baseline demographic and economic characteristics listed in Table 2. These characteristics measured at baseline include: age (30–39, 40–49, 50+, with < 30 omitted); income (in thousands: 10–20, 20–30, 30+, with < 10 omitted); educational attainment (some college, 4-year degree or more, with high school graduate or less omitted); female; receipt of government assistance; health insurance status; race/ethnicity (Black non-Hispanic, other non-Caucasian, with Caucasian non-Hispanic omitted); and ownership of a car or a home. To simplify and focus the analysis, I examine the likelihood of marriage conditional on being single (never married, divorced, or widowed) at the time of randomization and being divorced conditional on being married at baseline.

There are two distinguishing features of this analysis from those in previous studies. First, our treatment-effect estimates are a measure of the impact of homeowner incentives on marriage and divorce decisions (to the extent that the Tulsa IDA program was de facto a

¹⁰ The effect of participation, or the treatment on treated (TOT), of receiving the subsidy may also be of interest to the reader. If the treatment effect on eligible non-participants is zero and if the ITT is the overall impact effect evaluated at the sample mean, the TOT estimate is ITT / p , where p is the IDA take-up rate (Bloom, 1984). In this experiment, however, this formula should probably be viewed with caution since it is not obvious that effect on non-participants is zero or there is a homogenous treatment effect (Mills et al., 2008).

homeownership program). Second, due to the randomized experimental design, any unobserved factors potentially influencing formation decisions are evenly biased among treated and control group members, creating consistent estimates of such effects. Hence, the empirical difficulties most likely encountered in previous studies are effectively circumvented with the parsimonious specification.

VI. Results

A. Effects on Marriage

Table 3 presents the treatment-effect estimates from (1) where the dependent variable is defined as a 0,1 indicator of being married. The sample is restricted in the first column to the 620 participants not married at the time of randomization (i.e., never married, divorced, and widowed). Panel A shows the treatment effect of being offered the subsidy 18 months after randomization. Approximately 9.5% of the control group was married 18 months into the experiment as compared to 12.3% of the treatment group. The raw difference between those two estimates, 2.78 percentage points (0.1227-0.0949), is almost identical to the estimated treatment effect obtained from (1) using the linear probability model with the additional control variables listed in Table 2. Compared to the control-group mean (of 9.5%), this is a 28.9% increase in the marriage rate, which is economically large, but based on the associated standard error shown in parentheses of 0.0268 is not statistically different than 0 at conventional levels. Therefore, there is little to no statistical evidence of the treatment in 18 months following randomization.

The results in panels B and C, which measure the treatment effect over 48 months, yield a different conclusion. In particular, there appears to be substantial treatment effects four years after randomization. In particular, in panel B, approximately 15.2% of the control group was

married 48 months into the experiment as compared to 21.3% of the treatment group. Thus, the treatment group had a 6.37 percentage-point higher marriage rate, or 42.1% higher than the control-group rate of 15.2%, and with a standard error of 0.0311 is statistically different than zero at conventional significance levels. Panel C, which examines ever being married during the demonstration period conditionally on being single at baseline, yields a similar finding.

Columns 2 and 3 limit the estimation sample to the dominant demographic groups documented in Tables 1 and 2, females and females with children at the time of randomization. The findings of substantial treatment effects for marriage, in the range of 30-50%, continue to hold for these groups 48 months after randomization. Overall, the results in Table 3 suggest a substantial impact of IDAs on marriage.

The first two columns of Table 4 examines differential treatment responses according to a selected set of baseline socio-economic characteristics: the presence of at least one additional adult in the household, which is a measure of co-residence; whether the participant is female; or whether the participant is female and has a child in the household.¹¹ Each coefficient listed in Table 4 is β_2 from estimating via a linear probability model,

$$(2) \quad Y_{ki} = \beta_0 + \beta_1 T_i + \beta_2 T_i \times D_i + \theta X + \varepsilon_{ki},$$

where D_i is a 0,1 indicator for each baseline attribute listed in the table.¹² That coefficient can be interpreted as the differential treatment effect associated with that specific attribute. To assess the statistical significance of differences, p-values for the test whether that coefficient equals 0 is listed below each estimate in brackets.

¹¹ Unfortunately, there is not enough information in the baseline and follow-up surveys to determine where co-residence is intimate or not.

¹² Each baseline attribute (D) was also included as a control variable in X if it wasn't already included.

Overall, there appears to be little evidence of statistically significant differences between groups, although the power to detect small differences is limited attributable to the small samples sizes.¹³ There does, however, appear to a slight difference for those participants that were previously living with at least 1 additionally adult in the household. Although it is unclear whether the participant was involved in a romantic partnership with that adult, they were 9.7 percentage points more likely to be married at 18 months after randomization than those treated participants who did not.

B. Effects on Divorce

In a manner parallel to Table 3 for marriage, Table 5 shows treatment-effect estimates from (1) for being divorced at 18 and/or 48 months for participants who were originally married at the time of randomization. Panel A shows the treatment effect 18 months after randomization and, in contrast to marriage, there seemed to be an immediate impact on such outcomes. In column 1, 6.45% of the control group, who were previously married at baseline, were divorced 18 months into the experiment as compared to 16.82% of the treated. After controlling for baseline attributes, this resulted in an estimated treatment effect of 9.61 percentage points, or a 149% increase in the likelihood of being divorced for the treatment group. This result, with a standard error of 0.0452, is statistically different than 0 at the 5% level. When the sample is further restricted to either the 110 female participants or 97 females with children at the time of randomization, the effects grow to as high as a 422.7% increase in the likelihood of being divorced 18 months after randomization. Each result is statistically different than 0 at the 1% level of significance.

¹³ In addition to the baseline attributes listed in Table 4, differences in treatment effects were also estimated for all of the attributes listed in Table 2, although no significant difference for being married were found for those attributes as well.

The results in panels B and C, which measure the treatment effect for divorce over 48 months, show a similar treatment effect in magnitude to that in panel A. There is almost a near equal magnitude increase in divorce rates between 18 and 48 months for treated and control participants suggesting that the bulk of the difference occurred relatively shortly after randomization (i.e., within the first 18 months).¹⁴ In the case of females, it appears being offered the treatment may have temporarily sped up the dissolution rate of prior marginal marriages as control group members were more likely to receive a divorce between 18 and 48 months.

These results are consistent with a substantial “independence” effect provided to women from the IDA program. This hypothesis is further supported by estimated differences in treatment effects on divorce listed in the last two columns of Table 4. The largest differences in divorce rates for treated participants occurred for females and females with children. Furthermore, those participants who reported being married in the baseline survey were additionally asked about the health of their relationship with their spouse. Treated married participants who reported they were in a very good relationship or seldom argued heatedly reported much lower divorce rates at 18 and 48 months after randomization than control participants. In contrast, those treated participants who were not in such healthy relationships were 16.7 and 16.0 percentage points more likely to be divorced at 18 months, respectively.

The next section will investigate further the underlying mechanism(s) leading to such large observed differences in the likelihood of marriage and divorce for treated participants.

VII. Role of Homeownership

While it is clear that the randomized chance of being offered a homeownership subsidy had a

¹⁴ In absolute terms, the treatment effects remain constant as time in the demonstration elapsed. In relative (or percentage terms relative to the control-group mean), the treatment effects decline over time, because the control-group mean divorce rate rose over time.

sizable impact on resulting marriage and divorce decisions of participants, the mechanism of how the subsidy affected those outcomes is not necessarily clear. The influence of the subsidy could either have a direct or indirect effect on those decisions depending on how the subsidy is perceived by recipients and their current or potential mates. For example, the subsidy could have a direct effect on a participant's potential wealth and their resulting value on the marriage market. As the maximum change in wealth directly attributable to the subsidy was modest (at most \$4500), the large observed effects on marital status could potentially also be explained by the indirect role of becoming a homeowner.

As mentioned in the Introduction, the previous literature typically assumes that homeownership is exogenous when considering its effect on marital status. A challenge to this assumption is created by the possibility of reverse causality or omitted factors simultaneously correlated with homeownership and being married. For example, it is unclear whether becoming a homeowner causes marriage or does marriage lead to a higher incidence of homeownership? If the direct effect of being offered the subsidy on marriage decisions is assumed to be negligible, then it is possible to use the randomized offer of the subsidy as an instrumental variable to identify the causal impact of homeownership on such decisions. To minimize concerns of biases resulting from the use of weak instruments, I follow Engelhardt et al. (2010) and focus on the two samples, baseline renters and baseline renters who were not currently receiving a governmental subsidized rent (hereafter referred to as baseline unsubsidized renters), where the impact of the offer of the subsidy was estimated by Mills et al. (2008) to be the largest 48 months post-randomization.

Results for the two samples where a 0,1 indicator variable for being married 48 months post randomization is the dependent variable are displayed in Table 6. Each regression in the Table

includes the same set of control variables discussed in Section V and listed in Table 2, with the addition of baseline marital status. The first panel of the Table displays OLS regression of homeownership on being married for the two samples. As expected, a strong correlation exists between homeownership and being married 48 months after randomization. Baseline renters who became homeowners were 13.25% more likely to be married and those baseline unsubsidized renters were 10.07% more likely. Both results, with respective standard errors of 0.0353 and 0.0430, are statistically different than 0 at the 5% level.

Panel B is the first-stage regression of treatment status on marital status and accordingly replicates the original results in Mills et al. (2008) discussed in Section IV.¹⁵ Treated baseline renters were 5.63 percentage points more likely to be homeowners 48 months after the initial offer of opening an IDA. The strongest effect of the offer was for baseline unsubsidized renters with a 9.32 percentage point difference in homeownership rates between treated and control groups. While both results are large in the economic sense and show being offered the subsidy is efficacious in encouraging homeownership, the lack of statistical precision on treatment status, perhaps due to the relatively small sample sizes, makes it less than an ideal instrumental variable due to concerns about weak instrumental bias.

Weak instruments, or having instruments not sufficiently correlated with endogenous variables, have been a growing concern in economics dating back to at least Bound, Jaeger, and Baker (1995). The concern arises because 2nd stage results derived through the use of weak instruments using linear IV are potentially biased in the same direction as OLS, and thus no longer necessarily consistent (Chao and Swanson, 2005). F-statistics of each of the first-stage

¹⁵ Differences between results displayed in Panel B of Table 6 and Mills et al. (2008) are attributable to how baseline characteristics used as control variables enter the estimation equation. In Table 6, all controls enter the equation directly, whereas Mills et al. (2008) used a propensity score. Results are qualitatively similarly regardless of which methods is used.

regressions are displayed in brackets in Panel B of Table 6. They are 2.46 and 3.93 for baseline renters and baseline unsubsidized renters, respectively. While they are below the standard threshold of 10 indicating they are weak instruments, a growing literature has suggested they are potentially still informative of the underlying simultaneity bias present in the 2nd stage since the direction of bias attributable to weak instruments is known (see Murray (2006) for a review).

Panel C of Table 6 displays two-stage least squares results of homeownership on being married 48 months after randomization. They are 87.99% and 77.50% with standard errors of 0.7291 and 0.5480 for the two samples, respectively. Both point-estimates are substantially larger, albeit less precise, than their OLS counterparts suggesting omitted factors provide a downward bias, or understate, the causal impact of homeownership on becoming married.

In effort to gain more precision of resulting IV estimates using binary variables, one solution is to provide more structure to the error term and estimate the first- and second-stage simultaneously through estimating coefficients via a bivariate probit estimator.¹⁶ Panel D of Table 6 reports the partial marginal effects of such an estimator of homeownership on marital status. Both IV point-estimates, at 36.67% and 47.24%, are likewise larger than their OLS counterparts, with the last for baseline unsubsidized renters now statistically different than 0 at the 1% level.

VIII. Conclusion and Implications

This paper presents the first experimental evidence for how homeownership subsidies and IDAs, an important asset-building policy for low-income households, affect marriage and divorce decisions. Overall, the Tulsa IDA program was a de facto home ownership saving

¹⁶ The bivariate probit estimator assumes the idiosyncratic errors in the traditional first- and second-stages are jointly normally distributed. See the appendix of Engelhardt et al. (2010) for the advantages and disadvantages of using such an estimator.

program for participants and had important effects on marriage and divorce decisions. First, baseline unmarried individuals who were randomly assigned to open an IDA were 40.3% more likely to be married at 48 months after randomization. Second, married individuals at baseline given the offer of the subsidy, were 158% more likely to be divorced 18 months after randomization. In particular, the prospect of independent living in an owner-occupied residence from the matched saving incentive increased dissolution, as this impact was most pronounced for individuals reporting poor spousal relations at baseline. Finally, it was shown that homeownership has a direct causal affect on marriage and that previous analysis without a plausibly valid instrument has understated the role of homeownership on such decisions.

However, several aspects of the design and implementation of the experiment raise issues of interpretation. First, as discussed in Mills et al. (2008), treatment group members had incentives to accelerate home purchases into the sample period, and control group members had incentives to delay purchases until the sample period ended. For the treatment group, the incentive to accelerate arose because the program matched contributions that were made during the four-year period and used for a down payment within that time at a 2:1 rate. Down payments made in future years were effectively matched at a 1:1 rate (if the IDA funds were rolled over into a Roth IRA and then used for home purchase sometime in the future). A treatment group renter who was planning to buy a home at some point in the future therefore may have accelerated the buying decision due to the program. For the control group, the incentive to delay home purchase stemmed from the program requirement that control group members not participate in other homeownership programs at CAPTC during the evaluation. This implies that the homeownership subsidy options for control group members were less attractive during the experiment than the options faced by typical low-income households, and that the options would

improve once the experiment ended.

To the extent that either incentive influenced the timing of home purchases, they would be expected to affect household formation decisions, with the treatment group having incentives to possibly accelerate those decisions and the control group possibly to delay those decisions. Therefore, it seems plausible that some of the household formation decisions represent inter-temporal substitution of marriage decisions that would have occurred in the future even in the absence of the program. Ideally, more information would have been collected from participants at baseline and in the subsequent surveys on cohabitation of romantic partners and long-term separations.

Second, as we have discussed already, there are concerns with the external validity of the results. In particular, the analysis sample is not a random draw of all low-income households, but those individuals who volunteered to participate in matched savings program for asset accumulation in Tulsa, Oklahoma in 2008. Most likely volunteers differ remarkably along both observed and unobserved dimensions from representative low-income households. Their self-identified motivation perhaps represents those individuals most likely to participate if IDAs and other similar homeownership subsidies were made available nationwide.

There are two key open research questions. The first concerns a finer analysis of the economic channels and mechanisms through which marriage and divorce occur. While the subsidy, and homeownership indirectly, has been shown to have an important role influencing such decisions, the net social welfare gains are unclear. The second concerns the long-term effects of IDAs, which are intended to be more than simply saving accounts; they are intended to induce behavioral changes (education, buying a home, or starting a business) that fundamentally alter households' lifetime prospects, generate economic mobility, and foster independence. Such

gains may ultimately take time, and perhaps even generations, to develop. Currently, a follow-up survey is going into the field to help assess the impact of the program a decade after the baseline intervention. This assessment will be an important avenue for future research.

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Table 1.
Number of Participants and Retention Rates Split by Marital Status at Baseline

Sample	(1)	(2)	(3)	(4)	(5)
	Wave 1	Wave 2	Wave 3	Wave 3 Retention Rates	Retention Rates Difference T - C ^a
<i>A. Females</i>	865	620	669	77.3%	0.0%
Married	155	103	110	71.0%	-8.1%
w/ Children	135	91	97	71.9%	-10.2%
Divorced or Widowed	315	240	256	81.3%	-2.3%
w/ Children	244	183	197	80.7%	3.5%
Single, Never Married	395	277	303	76.7%	5.4%
w/ Children	313	220	242	77.3%	2.8%
<i>B. Males</i>	238	144	169	71.0%	5.4%
Married	152	97	109	71.7%	-16.1% **
w/ Children	130	82	92	70.8%	-15.4% *
Divorced or Widowed	37	20	24	64.9%	8.3%
w/ Children	20	12	14	70.0%	12.5%
Single, Never Married	49	27	34	69.4%	16.7%
w/ Children	13	5	8	61.5%	40.4%
<i>C. Total Sample Size (n)</i>	1103	764	838	76.0%	-1.1%

^a. Statistical significance is indicated as follows: *** = $p < .0.01$; ** = $p < 0.05$; * = $p < 0.10$.

Table 2.
Baseline Demographic and Economic Characteristics by Marriage Status at Baseline

Sample Characteristics	Single (Never Married, Divorced, Widowed)				Married			
	Combined Sample (n = 620)	Treatment Group (n = 296)	Control Group (n = 324)	Difference T - C^a	Combined Sample (n = 218)	Treatment Group (n = 115)	Control Group (n = 103)	Difference T - C^a
Age	36.4	36.6	36.3	0.3	36.0	35.5	36.5	-1.0
Monthly Household Income	\$1,346	\$1,381	\$1,314	\$67	\$1,750	\$1,769	\$1,727	\$42
Female (%)	90.3%	89.9%	90.7%	-0.9%	50.2%	50.9%	49.5%	1.3%
# of Children in Household	1.5	1.6	1.4	0.2	2.1	2.1	2.2	0.0
Race/Ethnicity (%)								
Caucasian, Non-Hispanic	42.4%	39.2%	45.4%	-6.2%	59.8%	59.5%	60.2%	-0.7%
African-American, Non-Hispanic	44.5%	47.0%	42.3%	4.7%	31.1%	32.8%	29.1%	3.6%
Other	13.1%	13.9%	12.4%	1.5%	9.1%	7.8%	10.7%	-2.9%
Educational Attainment (%)								
Less than High School	5.5%	5.7%	5.3%	0.5%	5.5%	7.8%	2.9%	4.8%
High School Diploma or GED	25.2%	25.7%	24.7%	1.0%	26.9%	23.3%	31.1%	-7.8%
Less than BA	59.2%	58.5%	59.9%	-1.4%	51.6%	51.7%	51.5%	0.3%
BA or more	10.0%	10.1%	9.9%	0.3%	16.0%	17.2%	14.6%	2.7%
Receive Gov't Assistance (%)	44.4%	43.9%	44.8%	-0.8%	37.0%	39.7%	34.0%	5.7%
With Health Insurance (%)	58.2%	58.5%	58.0%	0.4%	58.5%	60.3%	56.3%	4.0%
Poverty Rate (%)	35.5%	35.5%	35.5%	0.0%	41.1%	41.4%	40.8%	0.6%

^a. Statistical significance is indicated as follows: *** = p<.001; ** = p<0.05; * = p<0.10.

Table 3.
Estimated Impact of Treatment on Marriage Decisions for Participants Not
Married at Baseline, Standard Errors in Parentheses

	(1)	(2)	(4)
Estimate/Group	All	Female	Female w/ Children
<i>A. Married 18 months After Randomization</i>			
Treatment Group	0.1227	0.1260	0.1224
Control Group	0.0949	0.0959	0.1014
Treatment Effect	0.0274 (0.0268)	0.0275 (0.0282)	0.0281 (0.0324)
	28.9%	28.7%	27.7%
<i>B. Married 48 months After Randomization</i>			
Treatment Group	0.2128	0.2143	0.2290
Control Group	0.1517	0.1536	0.1511
Treatment Effect	0.0637 (0.0311)	0.0591 (0.0327)	0.0837 (0.0383)
	42.1%	38.6%	55.7%
<i>C. Ever Reported Being Married at 18 or 48 months</i>			
Treatment Group	0.2399	0.2444	0.2570
Control Group	0.1827	0.1843	0.1822
Treatment Effect	0.0585 (0.0326)	0.0547 (0.0344)	0.0789 (0.0400)
	32.1%	29.8%	43.5%
Sample Size (n)	619	559	439

Table 4.
Difference in Treatment Effects by Baseline Characteristics, P-Values in Brackets^a

Sample	Single at Baseline...		Married at Baseline ...	
	Married @ 18 Months (n = 564)	Married @ 48 Months (n = 617)	Divorced @ 18 Months (n = 200)	Divorced @ 48 Months (n = 219)
<u>Baseline Characteristic</u>				
Female	-0.013 [0.882]	-0.059 [0.585]	0.308 [0.001]	0.211 [0.041]
Female with Children	-0.032 [0.589]	0.030 [0.662]	0.358 [0.001]	0.226 [0.037]
Additional Adult in HH	0.097 [0.131]	0.048 [0.526]	-0.117 [0.526]	0.340 [0.051]
Very Good Relationship	.	.	-0.167 [0.051]	-0.122 [0.231]
Seldom Argue Heatedly	.	.	-0.160 [0.125]	-0.066 [0.608]

^a. The difference in treatment effects is obtained through estimating a linear-probability model with each above listed baseline characteristic separately interacted with treatment status as illustrated by equation (2) in the text. The number in brackets indicates the p-value represented by an F-test of the significance of difference in treatment effects.

Table 5.
Estimated Impact of Treatment on Divorce Decisions for Participants Married At
Baseline, Standard Errors in Parentheses

	(1)	(2)	(4)
Estimate/Group	All	Female	Female w/ Children
<i>A. Divorced 18 months After Randomization</i>			
Treatment Group	0.1682	0.3333	0.3600
Control Group	0.0652	0.0833	0.0750
Treatment Effect	0.0961	0.2613	0.3094
	(0.0452)	(0.0794)	(0.0868)
	149.0%	320.2%	422.7%
<i>B. Divorced 48 months After Randomization</i>			
Treatment Group	0.2414	0.3898	0.4000
Control Group	0.1456	0.1961	0.1905
Treatment Effect	0.0741	0.2109	0.2119
	(0.0518)	(0.0867)	(0.0929)
	51.4%	109.7%	113.9%
<i>C. Ever Reported Being Divorced at 18 or 48 months</i>			
Treatment Group	0.2672	0.4407	0.4545
Control Group	0.1553	0.2157	0.2143
Treatment Effect	0.0741	0.2109	0.2119
	(0.0518)	(0.0867)	(0.0929)
	51.4%	109.7%	113.9%
Sample Size (n)	219	110	97

Table 6.
Estimated Impact of Homeownership on Marital Status at 48
months, Standard Errors in Parentheses

Estimate/Group	(1)	(2)
	Baseline Renters	Baseline Unsubsidized Renters
<i>A. OLS of Homeownership on Marital Status</i>		
Homeownership	0.1325 (0.0353)	0.1007 (0.0430)
<i>B. First-Stage of Treatment on Homeownership, F-Stat in Brackets</i>		
Treatment Status	0.0563 (0.0359) [2.4637]	0.0932 (0.0470) [3.9281]
<i>C. 2SLS IV Estimates of Homeownership on Marital Status</i>		
Homeownership	0.8799 (0.7291)	0.7750 (0.5480)
<i>D. Bivariate Probit IV Estimates of Homeownership on Marital Status</i>		
Homeownership	0.3667 (0.2295)	0.4724 (0.1565)
Sample Size (n)	641	437