What Are the Social Benefits of Homeownership?
Experimental Evidence for Low-Income Households

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Abstract

We estimate the social benefits of homeownership using an exogenous instrument based on randomly assigned treatment status from a field experiment that subsidized saving for home purchase for low-income renters through Individual Development Accounts (IDAs). This approach attempts to eliminate the potential correlation present in previous analyses between unobserved individual characteristics leading to homeownership and traits leading to provision of social capital or local amenities. Consistent with previous work, we show that homeownership positively affects political engagement in simple probits. Instrumental variable probits, however, show no impact of homeownership on political involvement. IV results for other social outcomes are less conclusive. The analysis suggests that with the use of an exogenous instrument, it is possible to generate results that are different from the previous literature. Our results also suggest that being eligible to open an IDA did not spur households to provide more social capital or local amenities.

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1. Introduction

Federal, state and local governments in the United States subsidize household investment in owner-occupied housing.[1] Beyond the provision of private benefits and redistribution of income, subsidies are often justified on efficiency grounds—namely, that homeownership generates significant social benefits. In the terminology of DiPasquale and Glaeser (1999), homeowners may provide local amenities—which improve the quality of neighborhoods through classic externalities—or social capital—which improves social connections to neighbors. Some even have argued there are positive impacts on child well-being (Green and White, 1997; Boehm and Schlottmann, 1999; Haurin et al., 2002; Harkness and Newman, 2003).[2]

Most of the previous literature has concluded that homeowners generate both local amenities and social capital. A recurring issue, however, is the extent to which studies have successfully addressed the potential biases created by unobserved correlation among individual characteristics that encourage homeownership and those that lead to provision of social capital.[3]

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[1] The portfolio of policies includes the non-taxation of imputed rents, favorable tax treatment of capital gains, local land-use restrictions, exemption of housing from means-tested social insurance programs, subsidized mortgage insurance, and the sponsorship of secondary mortgage-market enterprises. While the mortgage interest deduction (MID) is probably the most well known tax or spending policy toward homeownership, the deduction itself is not a subsidy in the context of a well-designed income tax system, in which all interest and capital income were taxed and all interest payments were deductible. The federal income tax subsidy hence derives from the non-taxation of the imputed income, not from the MID.

[2] We make a distinction between these social benefits from homeownership at the household level versus those at the group level. The latter are due to spillovers and occur to the extent that neighborhood homeownership rates affect household-level behavior independent of household-level homeownership status. Such spillovers are a broader form of externality from homeownership. Haurin et al. (2003) review this literature. Additional aspects of household-level homeownership that have been studied, but for which the benefits are more likely private, are the impacts of homeownership on health, happiness, and personal efficacy. In addition, Oswald (1996) has conjectured that homeownership may generate negative labor-market externalities by raising mobility costs. This has been explored empirically by Green and Hendershott (2001), Coulson and Fisher (2002, 2009), Van Leuvensteijn and Koning (2004), and Munch et al. (2006, 2008), among others. We do not address any social costs of homeownership in our study.

[3] For example, households with (unobserved) low rates of time preference have greater incentive to invest in both housing and social capital (Glaeser et al., 2002), so that it is difficult to identify the impact of homeownership separately from this unobserved heterogeneity. The presence of such heterogeneity in this case would upwardly bias standard estimates of the social benefits of homeownership.

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The most sophisticated contributions to date recognize this endogeneity and employ instrumental variables (IV) techniques to isolate the true impact of homeownership. DiPasquale and Glaeser (1999) and Aaronson (2000) use the group average homeownership rate, based on a household's race, income group, calendar year, and state of residence, as an instrument for homeownership in their social outcome regressions. Green and White (1997), Haurin et al. (2002), and Harkness and Newman (2003) use the relative user cost of owner-occupied housing as an instrument for homeownership in their child-outcome specifications.[4] These studies found social benefits of homeownership, but the instruments may not be exogenous, because unobservable characteristics that influence the social and child outcomes may be correlated with either group membership or parental investment in children, respectively.[5]

Against this backdrop, our paper makes three contributions. First, we attempt to identify the social benefits of homeownership using an exogenous instrument for homeownership. Our instrument is the randomly assigned treatment status for low-income households from a field experiment that subsidized saving for home purchase through Individual Development Accounts (IDAs) conducted in Tulsa, Oklahoma, from 1998 to 2003. Using this instrument, we generate new estimates of the impact of homeownership for a wide variety of social capital measures and

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[4] Green and White (1997) also used weeks worked and marital status of the parent as instruments. Harkness and Newman (2003) also used the state homeownership rate, the metropolitan area or county ratio of median rent to median value, and the annual change in state per capita highway investment as instruments. Coulson and Fisher (2009) used the state marginal tax rate and the percentage of households in the MSA in multifamily housing as instruments for homeownership. Van Leuvensteijn and Koning (2004) used the regional homeownership rate as an instrument. Similarly, Munch et al. (2006) used the labor-market level aggregate homeownership rate as an instrument for individual-level homeownership. Munch et al. (2008) used as instruments the regional homeownership rate in both the current area of residence and the birth area of residence, as well as the parents' homeownership status.

[5] This point was first made by Glaeser and Shapiro (2003). Also, differential tax treatment, which may generate a wedge between the average cost of owning and average rents, will confer income effects, so that, in general, it is not possible to separately identify homeownership effects from income effects using relative user-cost measures as instruments. An alternative to IV is to estimate fixed-effects models with panel data, as in DiPasquale and Glaeser (1999), but this approach requires that households who change housing tenure are not also the ones predisposed to change their investments in social capital and local amenities. Munch et al. (2008) also exploited longitudinal data.

for a classic externality, exterior home maintenance. The primary advantage of this instrument is that it is exogenous via random assignment.[6] In a companion paper (Mills et al., 2008), we show that, four years after randomization, treatment-group renter households had a 7-11 percentage point higher homeownership rate than control-group renter households. This represented a 25-30% increase in homeownership, which is sizable relative to baseline.

Second, our analysis focuses on low-income households. Although most homeownership subsidies accrue to middle- or high-income households, low-income households are an interesting subgroup to study for policy purposes. Homeownership rates are already quite high for upper-income households, so if there are externalities to expanding homeownership rates, they would arise in the lower-income population studied here.

Third, we add to the small literature on experimental evidence on the effects of Individual Development Accounts (IDAs). IDAs are saving accounts designed to provide matching contributions for withdrawals that are used for particular purposes, such as home purchase. Although IDA programs have grown in popularity in the United States, there has been little formal analysis of their effects.[7]

Our broad conclusion is that use of an exogenous instrument for homeownership casts doubt on the previously-found positive effect of homeownership on local amenities and social capital. Three sets of results support this conclusion. First, the impact of homeownership on measures of political involvement—voting, giving time or money to a candidate, or calling or writing a public official—figures prominently in the previous literature. Similar to previous work, we find that simple probit estimates show positive effects of homeownership on political

[6] Recent studies that use randomization to study social interactions and social capital include Katz et al. (2001), Sacerdote (2001), and Hastings et al. (2007), among others.
[7] Publicly sponsored IDAs have been adopted in 34 states, Washington, D.C., and enabled through a series of federal laws. See Mills et al. (2008), Grinstein-Weiss et al. (2007), and Schreiner and Sherraden (2006).

involvement. In contrast, our IV estimates show negative effects. The difference is substantively large (around 50 percentage points) and highly statistically significant (p=0.0001). This suggests the simple probit estimates are biased upward substantially by unobserved characteristics that are correlated with both the propensity to be politically engaged and the propensity to own a home. The results do not appear to be explained by mobility necessitated by the transition from renting to owning, which might break households' ties to the political system in the short run. Overall, there is no evidence that homeowners are more politically involved than renters.

Second, although we find that homeowners are more likely to perform home maintenance, statistically significant effects arise only for interior repairs, which confer private benefits, rather than for external maintenance, which is the classic public externality. Third, our results imply that the treatment status itself – being eligible to open an IDA – did not spur households to provide more of any type of social capital or local amenity.

Although our research design represents a substantial improvement over earlier work, the results should be qualified in three ways. First, the IV estimates for outcomes other than political involvement – such as neighborhood involvement and giving to the community – are less precise, generally not statistically different from the probit estimates, and in some cases are actually larger than the probit estimates. Second, our sample sizes are not very large, which makes it more difficult to obtain conclusive estimates. Third, because the field experiment lasted only four years, we estimate only the short-run impact of homeownership on social benefits. Whether such benefits emerge in the long run is an open question. It is also worth noting that our sample is not a random sample of all low-income households; however, it does consist of households who are motivated to buy homes, which may make it more relevant than a random sample to the analysis of the marginal social benefits of increasing homeownership rates.

The paper is organized as follows. Section 2 briefly reviews the key findings in the previous literature. Section 3 describes the experimental design and program rules. Section 4 examines attrition and characteristics of the treatment and control groups at baseline. Section 5 presents the main empirical results. Section 6 concludes.

2. Existing Findings

The empirical literature on the social benefits of homeownership is summarized by Rosen (1985), Rohe et al. (2002), and Dietz and Haurin (2003), among others. We focus on a widely cited and influential study, DiPasquale and Glaeser (1999), who highlight the key difficulties in identifying the impact of homeownership on social outcomes and focus on social outcomes roughly similar to those measured in our data.[8] In their model, renters invest less than owners in social capital and local amenities because renters are more geographically mobile and the returns on their investment accrue to the landlord. They use micro data measured in the U.S. General Social Survey (GSS) and 1990 Census IPUMS on: number of memberships in nonprofit organizations, whether knows school head, whether knows U.S. representative, whether votes in local elections, whether helps solve local problems, whether gardens, whether attends church, and whether owns a gun. Based on a sample of households of all income levels, their OLS estimates suggest that homeownership raises activity in each outcome. The impact of homeownership on voting in elections—the only outcome of theirs that also is measured in our data—is to raise participation by 15.3 percentage points, relative to the mean voting rate of 68.6% in their sample.[9]

[8] Unfortunately, our data do not measure child outcomes that would allow for a comparison of findings with Green and White (1997), Haurin et al. (2002), and Harkness and Newman (2003).
[9] Rossi and Weber (1996) and Rohe and Stegman (1994) obtain similar results.

Recognizing the potential for correlation between unobserved individual characteristics leading to homeownership and traits leading to the provision of social capital or local amenities that might bias the OLS estimates, DiPasquale and Glaeser (1999) instrument for homeownership using the individual's group-average homeownership rate based on race, income group, calendar year, and state of residence. The IV estimates of homeownership on social outcomes are *larger* than the OLS estimates. For example, the impact of homeownership on voting in elections rise to 28.4 percentage points in the IV estimates. A key concern, however, is that this instrument may not be exogenous, because unobservable characteristics that influence the social outcomes may be correlated with group membership and, hence, the instrument. To circumvent such concerns, we use randomly assigned treatment status for low-income households from an IDA field experiment, as explained in the next section. In the empirical analysis below, we compare estimates using our instrument and an instrument based on group-average homeownership rates.

3. Experimental Design and Program Rules[10]

The instrument is based on randomly assigned treatment status for low-income households from a field experiment conducted in Tulsa, Oklahoma, from 1998-2003, as part of the American Dream Demonstration (ADD), a set of 14 privately funded local IDA programs initiated in the late 1990s. They are described in detail in Schreiner and Sherraden (2006). The Tulsa program, administered by the Community Action Project of Tulsa County (CAPTC) and the Bank of Oklahoma, was the only ADD site to adopt an experimental design.

To be eligible for the Tulsa program, individuals had to be employed, with a prior-year

[10] The discussion in this and the next section draws heavily on Mills et al. (2008).

family income below 150 percent of the federal poverty level. Eligible households were randomly assigned to a treatment group, which was allowed to open an Individual Development Account (IDA), or to a control group, which was not. An IDA is a savings account that provides matching contributions when balances are withdrawn for qualified uses (Sherraden, 1991).

The Tulsa program matched IDA saving for new home purchase at the rate of 2:1 and for other qualified uses (business capitalization, home repair, postsecondary education, or retirement saving) at the rate of 1:1, where up to $750 in annual deposits were eligible for the match for three consecutive years. Combining deposits and matching funds, participants could accumulate $6,750 for home purchase or $4,500 for the other allowable assets during the course of the experiment. Given a median house price of $89,000 in Tulsa in this period, the program offered a substantial inducement to become a homeowner, especially for low-income households, who have relatively greater difficulty in saving for home purchase and likely would purchase a home below the median value (Engelhardt, 1996, 1997; Engelhardt and Mayer, 1998).[11]

The Individual Development Account itself was a regular passbook saving account at the Bank of Oklahoma. Participants could not make a matched withdrawal until six months after having opened the account and received the required financial education. The match was provided in the form of a check made out to the vendor (e.g., a home mortgage lender). IDA deposits made within 36 months after 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.[12]

[11] Data from the 2000 Census IPUMS indicates that among the sample of employed Tulsa homeowners with income less than 150 percent of the poverty line, the median house value was $59,000.

[12] The experiment ended after four years. Remaining balances could be rolled over (at the participant's request) into a Roth IRA with a 1:1 match. Participants who contributed more than $750 in one year could carry forward the difference as a matchable contribution for the following year.

Enrollment in the Tulsa program occurred between October 1998 and December 1999. Information about the IDA Matched Savings Program was distributed through several channels: media outreach; CAPTC's existing social services, tax assistance, and homeownership assistance programs; and mailings to other local social service agencies, current and former CAPTC clients, and people who called to ask about the program. Interested individuals submitted an application and were interviewed to establish eligibility. Participants were informed that if assigned to the control group, they would be unable to open an IDA during the four-year study period. Overall, those who enrolled in the experiment were not a random sample of low-income households; they were motivated savers. The implications of this for the external validity of the experimental findings are discussed in the conclusion.[13]

Eligible individuals participated in a baseline survey that collected information on household income, finances, demographics, housing and social capital prior to randomization. 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, therefore, is the *offer* to participate in the IDA program and receive financial education.[14] The Wave 2 survey occurred for each case about 18 months

[13] The households in Tulsa IDA sample are not a representative sample of low-income households. In particular, Tables A1 and A2 in the appendix to Mills et al. (2008) compare the combined IDA sample (treatments and controls) to households who matched the IDA eligibility requirements – i.e., they were employed and had income below 150 percent of the poverty level – taken from the 1998 Survey of Consumer Finances and from a Tulsa-area subsample of the 2000 Census Public-Use Microdata Sample. The three samples show roughly similar average age, but differ markedly in other respects. IDA sample members have slightly higher average income and are less likely to be married, have health insurance, or own a business or home. They are far more likely to be female and African-American, to receive government assistance, and to have completed at least some college. They also have far lower levels of average wealth than the SCF sample members, but this is largely due to a few outliers in the SCF with extremely high wealth. IDA sample members are more likely to have bank accounts. The differences between the Tulsa IDA sample, a random national sample of low-income households, and a random sample of low-income households in the Tulsa area emphasize the importance of having a randomized control group in analyzing IDA behavior.

[14] Treatment group members were required to attend at least four hours of general financial education before opening an account. Prior to a matched withdrawal, participants had to have taken 12 hours of general financial

after random assignment, between May 2000 and August 2001; the Wave 3 survey occurred about 48 months after random assignment, from January to September 2003. These surveys also collected information on household income, finances, demographics, housing and social capital.

4. Data and Descriptive Statistics

There were 1,103 individuals at baseline for the experiment. As documented in Mills et al. (2008), there were significant differences in attrition rates across baseline treatment and control group members who were in subsidized rental housing. To avoid potential problems from differential attrition, we limit our analysis to the 437 renters who were in non-subsidized housing at baseline (where "subsidized" is defined as residing in a public housing project or Section 8 housing) and appear in the wave 3 survey. Mills et al. (2008) also show that, among this group, four years after randomization, treatment-group households had a 7-11 percentage point higher homeownership rate than control-group households. There were no economically or statistically significant treatment effects for the purchase of other qualified assets. Although it was designed to stimulate asset purchases broadly, the IDA program was *de facto* a first-time homebuyers' program for households who were renting when the demonstration began and had never owned before.

Table 1 presents sample economic and demographic characteristics as of the baseline survey. There were a few significant differences in baseline characteristics of treatment and control group members. Relative to controls, treatment group members were less likely to be

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education as well as additional training specific to the type of intended asset purchase. During the experiment, control cases were restricted from participating in any other matched savings or homeownership program from CAPTC, including a pre-existing program that provided 1:1 matching funds for down payment and closing costs. Control group members could receive homeownership counseling, and those who requested information about financial assistance for homeownership were referred to other Tulsa-area providers. Control and treatment cases could participate in CAPTC programs that provided loans for micro-enterprise and heating assistance.

single, had slightly more children, and had somewhat higher monthly income.[15]

In order for treatment status to be a valid instrument, it must be uncorrelated with any underlying propensity for social involvement. Hence, Table 2 presents similar statistics on social outcomes as of the baseline survey. Panel A covers political involvement: voting, supporting a candidate with time or money, and calling or writing to a public official. Panel B covers neighborhood involvement: volunteering and fund raising for a church, school, or neighborhood organization, working on a neighborhood project, or participating in a community association. Panel C shows a variety of measures of assistance given to others in the community in the past month. The table shows no significant differences in baseline social involvement between treatment and control group members.

The most widely cited efficiency rationale given for government subsidies for homeownership is that homeowners improve the quality of neighborhoods by taking better care of their homes, specifically, on the exterior (Galster, 1983; Rosen, 1985; Harding et al., 2000; and Dietz and Haurin, 2003, among others). Panel D shows that there were no significant differences at baseline between the treatment group and control group for exterior or interior repairs.[16]

Overall, the results in Tables 1 and 2 suggest that, conditional on marital status, income,

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[15] Mills et al. (2008) provide an extensive discussion of participation, summarized briefly here. Participation rates were quite high across all of the economic and demographic groups and relatively insensitive to typical determinants of saving behavior, such as age, income, or net worth, consistent with results in other IDA projects (Schreiner and Sherraden, 2006). Relative to others, blacks, divorced household heads, and later cohorts of sample members were less likely to participate.

[16] Exterior repairs include roof improvements or replacement; exterior painting; new or repaired windows or screens; built or enlarged porch/deck; landscape/yard work; siding; installing/repairing fence; installing/repairing gutters; and, adding a garage or shed. Interior repairs include interior painting; new carpeting; replaced and/or finished floor/ tile flooring; remodeled rooms, removed walls, enlarged space; new or upgraded appliances; new or upgraded furniture; replaced and /or repaired heating and/ or cooling systems; wall repair, patching, sheet rock; plumbing; new fixtures, lights, ceiling fan, doorknobs, blinds, doors, faucets; shampooing/cleaning carpet; installing insulation; electrical repairs; general maintenance and repairs; and, repairs after a fire/disaster.

and number of children, randomly assigned treatment status is orthogonal to the underlying proclivity for social involvement and local amenity provision. Therefore, treatment status will be a valid instrument for homeownership in our social outcome estimation.

5. Empirical Results

Our central results are reported in Tables 3-6, which give estimates of the effects of homeownership on a variety of social outcomes and have a common structure. The first row in each table shows the simple probit parameter estimate of the impact of homeownership on the social outcome:

(1) $$Y_{3i} = \beta_0 + \beta_1 H_{3i} + \theta \mathbf{X}_i + u_i \,,$$

with the standard error in parentheses and the marginal effect in brackets. The subscript *i* refers to the household, Y_{3i} is a dummy dependent variable for the social outcome four years after randomization (in Wave 3), H_{3i} is a dummy variable that is one if the household owns a home four years after randomization (in Wave 3) and zero otherwise, and u is the household-specific error term. We include a vector ($\mathbf{X}$) of control variables for marital status, income, number of children, and race to account for the economically and statistically significant differences between the treatment and control groups at baseline, as indicated in Table 1. We use the results from equation (1) as a benchmark against which to measure the impact of the IV procedure. The equation is estimated on the control group only, and thus exhibits the behavior that would be observed in non-experimental survey data.

The second row shows the IV parameter estimate from the bivariate probit estimation of β_1 in (1), based on the pooled sample for the treatment and control groups, using treatment status as the instrument, under the assumptions that homeownership is endogenous and the errors u in (1) and ε in (3) are jointly normally distributed.[17]

The third row reports Hausman test significance levels for the null hypothesis that the simple probit marginal effects in row 1 and the IV marginal effects in row 2 are equal. The fourth row reports the sample mean of the social outcome for the control group.

The final row in the tables shows the probit parameter estimate of the *treatment* (being eligible for an IDA) on the social outcome in question:

(2) $$Y_{3i} = \delta_0 + \delta_1 Z_i + \theta \mathbf{X}_i + v_i ,$$

where Z is the treatment-status indicator, and where the exogeneity of the instrument is generated by the randomization. The equation is estimated on the pooled treatment and control group. Equation (2) serves two important purposes. First, it provides direct evidence on how being eligible for an IDA affects social outcomes, an issue of specific interest for IDA program design (Schreiner and Sherraden, 2006). Second, it provides reduced-form estimates of the instrument on the social outcome. Angrist and Krueger (2001) argue that the absence of a reduced-form relationship typically implies no relationship in the IV. However, this is not always the case, because the variance of the IV estimator also depends on the precision of the first-stage estimation. Chernozhukov and Hansen (2008) formally consider the conditions under which inference on the reduced-form implies inference on the IV results, even in the presence of weak instruments, and draw similar conclusions as Angrist and Krueger (2001). Therefore, in our case, the lack of a reduced-form relationship lends credence to, but does not definitively rule out, the hypothesis that there are no social benefits from homeownership.

[17] An alternative to probit and bivariate probit maximum likelihood (ML) estimation is to use linear OLS and IV estimators. We illustrate the results with those estimators in Appendix Table 1. In general, those results are similar to the ones based on bivariate probit. However, because we achieve a substantial efficiency gain from using maximum likelihood, we present the bivariate probit results in Tables 3-6 as our preferred estimates.

The first stage of the instrumental variable regression, which shows the effect of IDA eligibility on homeownership and is common across the tables, and is given by

(3) $$H_{3i} = \alpha_0 + \alpha_1 Z_i + \theta \mathbf{X}_i + \varepsilon_i ,$$

with an estimated $\hat{\alpha}_1 = 0.092$ with a robust standard error of 0.046. That is, the treatment is associated with a 9.2 percentage-point rise in the homeownership rate. This replicates the earlier analysis in Mills et al. (2008) on the effects of the IDA program on homeownership.

Political Involvement

Table 3 shows the results for political involvement. The simple probit estimate in the first row of the first column indicates that, among control group members, becoming a homeowner by wave 3 raises the likelihood of having called or written a public official by 17.2 percentage points. This is both statistically significant and economically large. The key concern with this estimate, of course, is that the types of individuals who become homeowners also might be more inclined to contact public officials, even after controlling for observable characteristics. The second row shows the IV marginal effect, based on the bivariate probit estimation over the pooled sample for the treatment and control groups. In contrast to the simple probit effect in the first row, the IV estimate is negative, suggesting that becoming a homeowner reduces the likelihood of contacting a public official substantially. The difference in point estimates between the simple and IV probit is large, and the *p*-value for the Hausman test in row three shows that the results are statistically significantly different from one another.

Columns 2 and 3 show similar patterns of results for two other measures of political involvement: supporting a candidate with time or money, and voting in an election. While the simple probit estimates in the first row suggest that homeownership increases political involvement, the IV results in the second row suggest the opposite. The Hausman test results in the third row show that equality of coefficients can be rejected at conventional levels for both measures.[18]

For all dependent variables, the reduced-form results in the bottom row provide no evidence that IDAs have a positive effect on political involvement; all three of the point estimates are negative and all are insignificantly different from zero. Overall, the table provides no evidence to suggest that homeowners are more politically engaged than renters and, for all three measures, the evidence indicates that the simple probit estimates are biased upward by statistically significant margins relative to the IV estimates.

We extend the results in two ways. First, one reason for the lack of impact of homeownership on political involvement could be that the transition from renting to owning generally entails moving, which might break households' ties to the (local) political system in the short run. Hence, mobility might be a confounding factor in the IV analysis. As a specification check, Appendix Table 2 presents probit estimates of the reduced-form relationship between mobility and the instrument for two measures of mobility asked in Wave 3—whether the household moved in the previous 30 months and the number of times moved during the previous 30 months—and a third measure of mobility based on any change in zip codes since baseline. The estimates show that the instrument is uncorrelated with these measures of geographic mobility. Therefore, it does not appear that mobility is confounding the IV probit estimates in Table 3.

[18] Much of the empirical impact of homeownership on social outcomes in, for example, Green and White (1997), DiPasquale and Glaeser (1999), and Aaronson (2000), worked through longer duration. In particular, the greater the length of time spent as a homeowner, the greater the level of investment in social capital and local amenities. We examined this and found no effect of greater duration as a homeowner on social outcomes. Average homeowner tenure length for those baseline renters who bought houses during the demonstration was 2.2 years.

Second, we compare our estimates with estimates based on specifications in DiPasquale and Glaeser (1999). We are able to broadly replicate their OLS estimates for voting behavior in our data as shown in the simple probits in the first row. As noted above, they use the group average homeownership rate, based on a household's race, income quartile, calendar year, and state of residence in the IPUMS, as an instrument for homeownership in their social outcome regressions. Because all of our sample households entered the experiment in the same time frame and are from the Tulsa metropolitan area, we construct the group average homeownership rate, based on a household's race and income quartile in an analogous manner using data from the 2000 IPUMS for Tulsa. For the purposes of comparison, Appendix Table 3 presents the IV marginal effects from the bivariate probit estimation of the impact of homeownership on the three measures of political engagement outcomes, controlling for the same set of additional regressors used in DiPasquale and Glaeser (1999). The IV marginal effects using the group average homeownership rate as an instrument are of the opposite sign (positive) and statistically different from the IV effects based on our exogenous instrument in Table 3 for two of the three outcomes. These results cast some doubt on the ability of instruments based on group average homeownership rates to generate consistent estimates of social benefits.

<u>Neighborhood Involvement and Community Giving</u>

Table 4 provides a parallel analysis for measures of neighborhood involvement in the past year. The first row results indicate that, in the control group, homeowners are about 10 percentage points more likely than renters to volunteer in their neighborhoods (column 1). However, the IV marginal effect in row two flips sign and suggests that becoming a homeowner reduces the likelihood of volunteering by 51 percentage points. The difference between the estimates is significant, with a *p*-value of 0.0001. This is consistent with the results in Table 3 suggesting no positive impact of homeownership on the provision of social benefits.

Results for the other two measures of neighborhood involvement, as well as community giving in Table 5, are imprecisely estimated. However, we note that the reduced-form results in the bottom row of Tables 4 and 5 are not significant.

<u>Home Maintenance and Repairs</u>

Table 6 reports the results for external and internal repairs. Column 1, row 1 shows that in the control group, homeowners were 13.2 percentage points more likely than renters to engage in exterior repairs, which is statistically significant at the 2% level. This marginal effect increases in value to 27 percentage points in the IV estimates. The difference between these estimates, however, is not statistically significant at conventional levels. The estimates in column 2 for expenditures on exterior repairs are positive, but also imprecisely estimated and not statistically different than zero.

The results in column 3 are more definitive for interior repairs, which confer private benefits. The simple probit results suggest that homeowners were 27 percentage points more likely to engage in interior repairs than renters. The IV probit results show even larger effects and are statistically different from the OLS estimates. Both the OLS and IV estimates in column 4 show a substantial impact of homeownership on expenditure on interior repairs, which generate private benefits. The only statistically significant impact of eligibility for IDAs in any of the tables is reported for expenditures on internal repairs, in the fourth column of the table.

We caution that, because withdrawals for home maintenance expenditures were a qualified use of the IDA, the instrument (treatment status) may have had a *direct* effect on maintenance. While this does not affect the reduced-form probit and OLS estimates, respectively, shown in Table 6, this would render treatment status invalid as an instrument, such that the IV probit and IV estimates in the table would not be consistent estimates of the true impact of homeownership on maintenance. In this case, because the first-stage correlation between IDA eligibility (the instrument) and homeownership is positive and the direct effect of IDA eligibility on maintenance as a qualified use would be positive, it can be shown quite easily that the asymptotic bias on the IV estimates is upward. This suggests that the IV estimates in Table 6 are likely upper bounds on the true impact of homeownership on home maintenance.

6. Conclusion

The primary contribution of this paper is to use randomly assigned treatment status to generate new instrumental variable estimates of the impact of homeownership on a wide variety of social outcomes for low-income households. Because our instrument is exogenous, we expunge the analysis of any unobserved correlation between propensities to own homes and to invest in social capital or local amenities.

Our clearest set of results is for the impact of homeownership on political involvement. These results imply that simple probit estimates overstate the impact of homeownership on political involvement and that the true effect, provided in the IV estimates, is zero or negative in our sample. We find a qualitatively similar pattern for two of three measures of neighborhood involvement.

For most of the other outcomes, the results are not conclusive. It is an open question as to whether these effects would become statistically significant if the sample size were substantially larger.

There is some evidence that homeowners are more likely to perform home maintenance. However, it is worth noting that, for interior repairs, which do not provide externalities, we find strong and significant impacts of homeownership in both the simple probit and IV probit estimates and that the IV estimates are larger and significantly different than the OLS estimates. The current sample size is not impeding precise estimates of at least some outcomes.[19]

We also find little evidence of any impact of Individual Development Accounts on various forms of social capital or local amenities. The only statistically significant impact of IDA eligibility in the data examined here occur for funds spent on interior repairs, which, as noted above, do not generate externalities. Moreover, interior repairs were a directly subsidized use of IDA funds.

In closing, several aspects of the design and implementation of the experiment should be kept in mind. First, the demonstration had a limited time horizon; the difference in time between the first and last wave was 4 years, and most people who bought homes did not do so until well after the program began. Thus, the results provide estimates of the short-term effects of homeownership on social capital and local amenities, but not long-term effects. Currently, there is a follow-up survey in the field designed to assess the longer term impacts of IDAs. Eventually, we hope to use the new survey results to assess the impact of homeownership on social outcomes over a longer horizon.

Second, the ideal empirical strategy is to randomize ownership status, holding economic resources constant. However, because the treatment involved matching contributions on saving for home purchase, the treatment itself conveyed a small wealth effect. It is not possible to

[19] Whether there are group- or neighborhood-level spillovers from homeownership is a separate question not addressed in this study. See Haurin et al. (2003), Coulson et al. (2003a, 2003b), and Ioannides (2002), among others.

separate this wealth effect from the pure effect of ownership (holding wealth constant).

Unfortunately, our data do not measure child outcomes, so we were not able to address whether homeownership improves these outcomes, a potentially important social benefit. This has been an important area of study in this literature, e.g., Green and White (1997), Haurin et al. (2002), and Harkness and Newman (2003).

Finally, the external validity of the results should be considered carefully. The experiment took place in a metropolitan area with low housing prices. In particular, the sample was restricted to households employed at the time of randomization, living at or below 150 percent of the poverty level, and residing in the general Tulsa, Oklahoma area. These households are neither geographically representative nor representative of the overall low-income population, because they were motivated savers. Although the random assignment of sample members to treatment and control appears to have been successfully completed, the overall sample is not representative of low-income households. Specifically, those attractive to the IDA program were likely to be savers who were motivated to buy homes. However, for purposes of examining the externalities associated with marginal purchases of homes, using a sample of motivated savers may be an advantage.

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Table 1. The Relationship Between Treatment Status and Demographic and Economic Characteristics at Baseline

Characteristic	(1) Treatment Group (N=217)	(2) Control Group (N=220)	(3) Difference (T - C)
Age	35.3	34.7	0.6
Monthly Household Income ($)	1,544	1,388	156**
Female (%)	76.0	76.8	-0.8
# of Children in Household	1.7	1.4	0.3**
Never Married (%)	35.5	49.1	-13.6***
Married (%)	29.0	23.6	5.4
Divorced or Separated (%)	33.6	25.4	8.2*
Widowed (%)	1.8	1.8	-0.0003
Caucasian, Non-Hispanic (%)	47.5	53.2	-5.7
African-American, Non-Hispanic (%)	40.6	34.1	6.5
Other (%)	11.9	12.7	-0.8
Less than High School (%)	3.2	3.6	-0.4
High School Diploma or GED (%)	24.0	29.6	-5.6
Less than BA (%)	57.6	54.1	3.5
BA or more (%)	15.2	12.7	2.5
Receive Government Assistance (%)	31.8	33.8	2.0
Has Health Insurance (%)	59.9	55.3	4.6
Poverty Rate (%)	30.9	31.8	-0.9

Note: Statistical significance is indicated as follows: ***=p<0.01; **=p<0.05; and *=p<0.10.

Table 2. The Relationship Between Treatment Status and Social Outcomes at Baseline

Outcome (%)	(1) Treatment Group (N=217)	(2) Control Group (N=220)	(3) Difference (T - C)
A. Political Involvement			
Voted in an Election	46.1	41.8	4.3
Supported a Candidate for Office with Time or Money	11.1	10.9	0.2
Called or Written a Letter to a Public Official	23.0	21.4	1.6
B. Neighborhood Involvement			
Volunteered or Helped Raise Money for a Church, School, or Neighborhood Organization	65.0	60.0	5.0
Worked on a Neighborhood Project	19.8	16.8	3.0
Participated in a Neighborhood Association or Similar Community Organization	29.0	31.3	-2.3
C. Giving to the Community			
Cared for another Adult or Provided Child Care	60.0	60.0	-0.09
Watched a home or cared for a pet	34.1	34.1	0.01
Made calls or written/read letters for Someone Else	50.7	44.1	6.6
D. Home Repairs			
Performed Any Exterior Repairs	6.5	5.5	1.0
Performed Any Interior Repairs	12.0	13.6	-1.6

Note: Statistical significance is indicated as follows: ***=p<0.01; **=p<0.05; and *=p<0.10.

Table 3. Parameter Estimates of the Impact of Homeownership on Political Involvement in the Past Year, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)
	Dependent Variable:		
Estimator	Called or Wrote a Letter to a Public Official	Supported a Candidate for Office with Time or Money	Voted in an Election
Probit (Control Group)	0.497	0.235	0.682
	(0.195)	(0.226)	(0.203)
	[0.172]	[0.053]	[0.237]
IV (Bivariate Probit)	-1.178	-1.566	-1.306
	(0.216)	(0.109)	(0.075)
	[-0.366]	[-0.396]	[-0.486]
p-value on Hausman Test	0.0001	0.0001	0.0001
Sample Mean (Control Group)	0.286	0.136	0.636
Reduced-Form Probit	-0.245	-0.326	-0.009
	(0.140)	(0.171)	(0.132)
	[-0.077]	[-0.062]	[-0.003]

Note: Each cell of the table shows a parameter estimate from a different specification. The first row shows the probit estimates based on the sub-sample of control group observations four years after randomization. The second and last rows show estimates based on the full sample four years after randomization. Each specification controls for income, marital status, race, and number of children at baseline. Standard errors are reported in parentheses; marginal effects are in square brackets.

Table 4. Parameter Estimates of the Impact of Homeownership on Neighborhood Involvement in the Past Year, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)
	Dependent Variable:		
Estimator	Volunteered or Helped Raise Money for a Church, School, or Neighborhood Organization	Worked on a Neighborhood Project	Participated in a Neighborhood Association or Similar Community Organization
Probit (Control Group)	0.273	0.279	0.099
	(0.193)	(0.239)	(0.199)
	[0.099]	[0.056]	[0.032]
IV (Bivariate Probit)	-1.370	0.297	0.824
	(0.077)	(1.818)	(0.789)
	[-0.507]	[0.064]	[0.297]
p-value on Hausman Test	0.0001	0.993	0.344
Sample Mean (Control Group)	0.641	0.132	0.259
Reduced-Form Probit	-0.060	-0.021	0.277
	(0.130)	(0.161)	(0.136)
	[-0.023]	[-0.005]	[0.096]

Note: Each cell of the table shows a parameter estimate from a different specification. The first row shows the probit estimates based on the sub-sample of control group observations four years after randomization. The second and last rows show estimates based on the full sample four years after randomization. Each specification controls for income, marital status, race, and number of children at baseline. Standard errors are reported in parentheses; marginal effects are in square brackets.

Table 5. Parameter Estimates of the Impact of Homeownership on Non-Monetary Assistance Given to Others in the Community in the Past Month, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)
	Dependent Variable:		
Estimator	Cared for Another Adult or Provided Child Care	Watched a Home or Cared for a Pet	Made Calls or Written/Read Letters for Someone Else
Probit (Control Group)	0.343	0.144	0.236
	(0.192)	(0.192)	(0.190)
	[0.129]	[0.050]	[0.092]
IV (Bivariate Probit)	0.066	1.554	-0.157
	(1.466)	(0.394)	(1.265)
	[0.025]	[0.557]	[-0.061]
p-value on Hausman Test	0.867	0.160	0.767
Sample Mean (Control Group)	0.600	0.291	0.418
Reduced-Form Probit	0.103	0.112	-0.035
	(0.130)	(0.136)	(0.129)
	[0.040]	[0.038]	[-0.014]

Note: Each cell of the table shows a parameter estimate from a different specification. The first row shows the probit estimates based on the sub-sample of control group observations four years after randomization. The second and last rows show estimates based on the full sample four years after randomization. Each specification controls for income, marital status, race, and number of children at baseline. Standard errors are reported in parentheses; marginal effects are in square brackets.

Table 6. Parameter Estimates of the Impact of Homeownership on Exterior and Interior Repairs in the Past 30 Months, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)	(4)
	Dependent Variable:			
Estimator	Any Exterior Repairs	Expenditure on Exterior Repairs	Any Interior Repairs	Expenditure on Interior Repairs
Probit (Control Group)	0.513 (0.211) [0.132]		0.721 (0.189) [0.269]	
OLS (Control Group)		126.31 (144.70)		618.43 (146.86)
IV (Bivariate Probit)	1.012 (1.718) [0.272]		1.610 (0.486) [0.578]	
IV		733.58 (1302.73)		3312.49 (1866.95)
p-value on Hausman Test	0.815	0.441	0.0364	0.188
Sample Mean (Control Group)	0.161	363.23	0.341	478.44
Reduced-Form Probit	0.087 (0.145) [0.022]		0.195 (0.125) [0.074]	
Reduced-Form OLS		67.37 (121.62)		304.21 (126.87)

Note: Each cell of the table shows a parameter estimate from a different specification. Rows 1 and 2 show probit and OLS parameter estimates of the impact of homeownership on repairs for the control group subsample, respectively. Rows 2 and 3 show IV probit and IV parameter estimates on the full sample. Rows 6 and 7 show probit and OLS reduced-form estimates. Each specification controls for income, marital status, race, and number of children at baseline. Standard errors are reported in parentheses; marginal effects are in square brackets.

Appendix

The tables in this appendix provide results on additional robustness checks and extensions of the analysis in Tables 3-6. Specifically, Appendix Table 1 compares IV estimates for a variety of estimators. The left-most column of the table lists the outcomes examined in Tables 3-6. Column 1 of the appendix table shows the marginal effect from the IV estimate of β_1 in equation (1) in the text, based on the bivariate probit estimator. Column 2 shows the linear IV estimate of β_1 in equation (1) under the assumption of a linear probability model. Column 3 shows the linear IV estimate of β_1 from a modified version of equation (1),

$$Y_{3i} = \beta_0 + \beta_1 H_{3i} + \theta \mathbf{X}_i + \gamma Y_{1i} + u_i \,, \tag{4}$$

in which the baseline (Wave 1) value of the outcome, Y_1 , is added as an explanatory variable to control for any differences in baseline social participation. Finally, column 4 shows the linear IV estimate of β_1 from

$$Y_{3i} - Y_{1i} = \beta_0 + \beta_1 H_{3i} + \theta \mathbf{X}_i + u_i \,, \tag{5}$$

in which the social outcome is in first-differenced form. The only difference between (4) and (5) is that (5) implicitly restricts $\gamma = 1$. All specifications in the table are estimated on a comparable sample of 436 observations, so that the sample is not changing across columns, hence, isolating the impact of the estimator.[20] Robust standard errors are shown in parentheses for columns 2-4.

In principle, all four estimators will yield consistent estimates of β_1 if the errors in (1) and (3) are jointly normal and the instrument is exogenous. However, the bivariate probit estimates will be relatively more efficient because the maximum likelihood estimator achieves the Cramer-Rao lower bound if the parametric assumptions hold. In practice, those results across the linear IV estimators in the appendix table are in general similar to the ones based on bivariate probit. In fact, normality of the errors cannot be rejected based on formal specification tests. Therefore, in the text and in Tables 3-6 we focus on the bivariate probit estimates as our preferred estimates, because, as is illustrated in the appendix table, we achieve a substantial efficiency gain from using maximum likelihood. The reduced-form estimates associated with this table (not shown) are similar across estimators as well.

Appendix Tables 2 and 3 extend the analysis in two ways. Because one reason for the lack of impact of homeownership on political involvement in Table 3 could be that the transition from renting to owning generally entails moving, mobility might be a confounding factor in the IV analysis in Tables 3. As a specification check, Appendix Table 2 presents probit estimates of the reduced-form relationship between mobility and the instrument for two measures of mobility asked in Wave 3—whether the household moved in the previous 30 months and the number of times moved during the previous 30 months—and a third measure of mobility based on any

[20] This is one observation less than the sample size of 437 observations in Tables 3-6, because there is one individual with missing social outcome information at baseline. Therefore, there is a slight difference between the bivariate probit estimates in column 1 of the appendix table and those in Tables 3-6.

change in zip codes since baseline. The estimates in this appendix table show that the instrument is uncorrelated with these measures of geographic mobility. Therefore, it does not appear that mobility is confounding the IV probit estimates in Table 3.

Appendix Table 3 compares our estimates with estimates based on specifications in DiPasquale and Glaeser (1999). We are able to broadly replicate their OLS estimates for voting behavior in our data as shown in the simple probits in the first row of Appendix Table 3. Also, as noted above, they use the group average homeownership rate, based on a household's race, income quartile, calendar year, and state of residence in the IPUMS, as an instrument for homeownership in their social outcome regressions. Because all of our sample households entered the experiment in the same time frame and are from the Tulsa metropolitan area, we construct the group average homeownership rate, based on a household's race and income quartile in an analogous manner using data from the 2000 IPUMS for Tulsa. For the purposes of comparison, Appendix Table 3 presents the IV marginal effects from the bivariate probit estimation of the impact of homeownership on the three measures of political engagement outcomes, controlling for the same set of additional regressors used in DiPasquale and Glaeser (1999). The IV marginal effects using the group average homeownership rate as an instrument shown in this appendix table are of the opposite sign (positive) and statistically different from the IV effects based on our exogenous instrument in Table 3 for two of the three outcomes. These results cast some doubt on the ability of instruments based on group average homeownership rates to generate consistent estimates of social benefits.

Appendix Table 1. Comparison of Estimates of the Impact of Homeownership from Alternative IV Estimators on a Consistent Sample, Robust Standard Errors in Parentheses

Outcome	(1) Bivariate Probit	(2) Linear IV	(3) Linear IV, Conditioning on Outcome's Baseline Value	(4) Linear IV in First-Differences
Called or Wrote a Letter to a Public Official	-0.365	-0.681	-0.982	-0.827
	(0.076)	(0.504)	(0.676)	(0.572)
Supported a Candidate for Office with Time or Money	-0.397	-0.449	-0.563	-0.456
	(0.024)	(0.345)	(0.443)	(0.401)
Voted in an Election	-0.487	0.195	0.028	-0.167
	(0.024)	(0.424)	(0.462)	(0.492)
Volunteered or Helped Raise Money for a Church,	-0.508	-0.405	-0.871	-0.842
School, or Neighborhood Organization	(0.024)	(0.478)	(0.678)	(0.645)
Worked on a Neighborhood Project	0.068	0.066	-0.050	-0.203
	(0.397)	(0.304)	(0.354)	(0.413)
Participated in a Neighborhood Association or Similar	0.300	0.689	0.860	0.931
Community Organization	(0.284)	(0.508)	(0.626)	(0.623)
Cared for Another Adult or Provided Child Care	0.044	0.146	0.069	0.116
	(0.550)	(0.434)	(0.498)	(0.516)
Watched a Home or Cared for a Pet	0.552	0.179	0.357	0.085
	(0.153)	(0.412)	(0.514)	(0.535)
Made Calls or Written/Read Letters for Someone Else	-0.060	0.072	-0.337	-0.531
	(0.478)	(0.437)	(0.583)	(0.563)

Note: Each cell of the table shows a parameter estimate from a different specification. Each specification controls for income, marital status, race, and number of children at baseline.

Appendix Table 2. Estimated Impact of Treatment on Mobility, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)
		Dependent Variable:	
Estimator	Moved During Previous 30 Months	Number of Times Moved during Previous 30 Months	Changed Zip Codes Since Baseline
Reduced-Form	0.016	-0.104	-0.037
	(0.120)	(0.105)	(0.123)
	[0.006]		[-0.014]
Sample Mean (Control Group)	0.468	0.443	0.355

Note: This table shows marginal effects from the probit estimation of the correlation of the instrument and geographic mobility.

Appendix Table 3. Comparison of Instrumental Variable Estimates of the Impact of Homeownership on Political Involvement in the Past Year, Standard Errors in Parentheses, Marginal Effects in Brackets

	(1)	(2)	(3)
	Dependent Variable:		
Estimator	Called or Wrote a Letter to a Public Official	Supported a Candidate for Office with Time or Money	Voted in an Election
Probit (Control Group)	0.178	0.138	0.255
	(0.140)	(0.174)	(0.138)
	[0.056]	[0.025]	[0.092]
Bivariate Probit with Treatment as IV	-1.153	-0.896	-0.119
	(0.291)	(0.582)	(1.608)
	[-0.352]	[-0.173]	[-0.044]
Bivariate Probit with Group Homeownership Rates by Race and IPUMS Income Quartile	-0.568	0.461	1.131
	(0.749)	(1.352)	(0.578)
	[-0.177]	[0.089]	[0.382]
p-value for Test that the Two Sets of IV Estimates are Equal	0.386	0.372	0.567
Sample Mean of Outcome (Control Group)	0.288	0.137	0.635

Note: The first row shows the probit estimates based on the sub-sample of control group observations four years after randomization. The next two rows show different instrumental variable estimates based on the full sample four years after randomization using the specification from DiPasquale and Glaeser (1999) that controls for age categories, race, sex, married, education categories, real income, and number of children in the household.