Impact of Homeownership on Children's Academic Outcomes: A Critical Methodological Review of Literature

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Abstract: The primary goal of this paper is to provide a critical methodological review of literature on homeownership and child academic outcomes. It focuses on the strengths and weaknesses regarding causal inference of different research findings. We first build a conceptual framework on the relations between homeownership and child academic outcomes. In the body of the review, six empirical studies using different quasi-experimental techniques were critically analyzed and compared regarding their strengths and weaknesses of establishing causal inference. A final comment on the quality of literature and future directions are also discussed.
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1. Introduction

Homeownership has been connected to wide-ranging benefits in the literature (Boehm & Schlottmann, 1999; Hepp, 2010). Enormous amounts of money from the public and private have been invested in increasing the homeownership rate as an important strategy to regenerate distressed urban communities (Harkness & Newman, 2003a). This includes the 2 trillion dollar "American Dream commitment" of Fannie Mae, multimillion-dollar homeownership programs of the Enterprise Foundation, and the millions of dollars of programs under the control of U.S. Department of Housing and Urban Development (HUD) (Harkness & Newman, 2003a) just to name a few.

However, as a result of the collapse of the subprime mortgage market in 2007, the widely believed benefits of homeownership and policies designed to encourage homeownership have come under great scrutiny. The perspective on promoting homeownership among low-income population as an unmitigated goal is no longer universally held (Lerman & McKernana, 2008). This paper thus focuses on the potential effects of homeownership on children's academic outcomes. Does homeownership influence children's educational outcomes? How big is the effect size? These questions are crucial in policy evaluation and future policy formulation.

Our primary goal is to provide a critical methodological review of literature on homeownership and child academic outcomes with focus on the strengths and weaknesses regarding causal inference. We first build a conceptual framework on the relations between homeownership and child academic outcomes. In the body of the review, six empirical studies using different quasi-experimental techniques were critically analyzed and compared regarding their strengths and weaknesses of establishing causal inference. A final comment on the quality of literature and future directions are also discussed.

2. Conceptual Framework

2.1 Links between homeownership (T) and children's academic outcomes (Y)

The treatment (t) in this paper refers to homeownership. One can either be in the treatment (i.e., is a home owner), or in the comparison (i.e., is not a home owner, they could be renters, staying with others, or homeless). Homeownership has been linked to many positive child outcomes including better health, fewer behavioral problems, greater academic achievement (e.g., in math and reading), lower high school dropout rates, are less likely to be teen parents, higher level of educational attainment by age 25, and are more likely to graduate from high school (Boehm and Schlottman 1999; Green and White, 1997; Haurin D., Parcel, & Haurin R., 2002)

Homeownership can exert control on child outcome through several pathways including parenting practices and assets (Harkness & Newman, 2003a). There is some evidence on improved parenting of homeowners due to either their greater investment in their properties or residential stability. Homeowning parents are found to provide a more stimulating and emotionally supportive environment, which improves children's cognitive abilities (Haurin, D., Parcel, &

Haurin, R., 2002).

Another explanation is that homeownership improves life satisfaction and/or self-esteem for adults, which in turn produces a more positive home environment for children (Balfour and Smith 1996). Assets theory also stipulates homeownership as an important type of assets that can generate psychological benefits for adults (Sherraden, 1991). Additional explanations include the improved personal and management skills associated with home-owning experiences which are then transferred to children (Green and White, 1997).

Homeownership can also have an impact on children's academic outcomes through the effects of assets. Housing is the primary asset held by most American families and assets can be leveraged during times of need to benefit children. For example, homeowners can apply for home equity loans to pay for their children's college education. Empirical evidence has linked net worth of equity to better child outcomes (e.g., college attendance) (Aaronson 2000; Boehm & Schlottman, 1999). Homeowners also enjoy some tax advantages, which could lead to better outcomes of their children. However, Harkness and Newman (2003a) argue that the effect of homeownership as a function of assets could have negative impact on children in poor families if the down payment and maintenance costs absorb resources that could have been invested on children.

2.2. The existence of confounding covariates (Xs and Us)

Despite the abundance of evidence in favor of the links between homeownership and child outcomes, estimating the true effect of homeownership is not an easy task due to its endogeneity. Homeownership is often associated with a collection of parental and neighborhood characteristics that are difficult to disentangle. Instead of benefiting from growing up in an owned house itself, children might benefit from the factors often accompanying homeownership.

Demographic characteristics are important parental and household characteristics that could influence homeownership and child academic outcomes. It does not require much education to figure out the direct link between family income and homeownership. Family with higher income and more savings are more likely to purchase houses simply because they can afford it. And there is substantial evidence linking family wealth to child academic outcome directly (Duncan & Brooks-Gunn, 1997; Shanks, 2007).

Parents' education level may also attribute to their decision of purchasing a house, an activity that involves certain financial skills. Highly educated parents, on the other hand, may provide a more

cognitively stimulating environment or have higher expectations, which in turn produce better academic outcomes of their children. There is also some evidence suggesting racial differences in home ownership and child outcomes (Coulson, 1999).

Behavioral and psychological characteristics of the parents such as saving behavior, nurturing abilities, propensity to invest, and goal attainment (Hepp, 2010) are other parental level covariates. It is reasonable to believe that certain characteristics of the parents may lead them to purchase housing property since a home purchase is a large financial commitment. For example, homeowners could be more responsible and/or have better financial management skills than renters considering the duties associated with home maintenance and mortgage payments. Responsible parents may pay more attention to monitor their children's behavior and bring up children with better outcomes. Or perhaps parents who are more involved with their children are also more likely to purchase a home. Unlike demographic characteristics, however, these personal traits of homeowners' characteristics are most often not measured in surveys.

Neighborhood characteristics are another constellation of confounding covariates. Homeowners are most often found in communities characterized by higher incomes, higher rates of homeownership, and greater residential stability (Harkness & Newman, 2003a). The available amenities such as a good school in a community might attract parents to permanently settle down and buy a house there. Going to a good school is also likely to produce better academic outcomes of children. Neighborhood safety is another potential factor that can influence the relationship between homeownership and child outcomes. High crime rates in a neighborhood could reduce the market value of its prosperities and thus discourage parents to purchase houses there. Living in an unsafe neighborhood and being exposed to crimes at an early age could greatly affect children's academic outcomes. In short, the observed and/or unobserved characteristics let it be parents or neighborhood, could lead to biased estimates of homeownership effects.

## 2.3 Independent predictors of child academic outcomes (Ws)

The existence of independent predictors—variables that influence child academic outcomes but are not associated with homeownership—can increase the precision of the treatment effect estimate. Since Ws are not associated with the treatment status, their existences will not bias the estimate of treatment effect. Some child characteristics that can arguably be Ws include child's gender, cognitive and intellectual ability, physical and mental health status, motivations and other unobserved

characteristics that influence its academic outcomes. Peer influence, certain teacher and school characteristics may also add the explanatory power to child academic outcomes. For example, nurturing or more experienced teachers may lead to better students' academic outcomes through adequate stimulation and proper teaching methods.

However, it is much less evident that experienced teachers could have somehow affect parents' decision of purchasing a house. Similarly, children who have positive peer networks at school (e.g., connected to pro-social peers, have little disruptions at school) may feel less distressed and thus function better academically. But it is much less obvious that their peer relations would affect parents buying a house or not (though in some rare cases, it might be true).

See Figure 1 for a graphic representation of the conceptual framework of the effects of homeownership and child academic outcomes.

Figure 1. Conceptual Framework of the effects of homeownership and child academic outcomes.
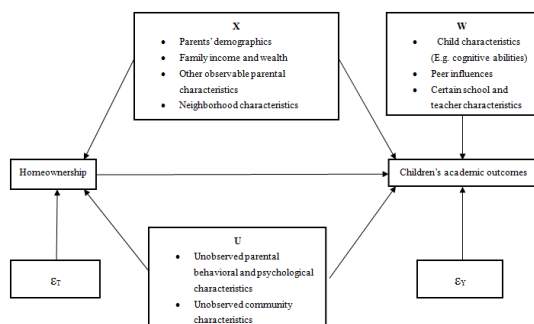
3. Methods

In this paper we present critical analyses of six empirical studies examining the causal relationship between homeownership and child academic outcomes. If multiple outcomes were examined in a single study, only child academic outcomes were reported in this paper. Techniques used in the selected studies ranges from statistical control in regression using longitudinal dataset, instrumental variable estimation, difference-in-difference, fixed effect, and differential effect. Studies using different methods were deliberately selected to provide a wide coverage and a comparison between methods.

4. Discussion
4.1 "Statistical control" in multiple regressions
Study 1: Boehm & Schlottmann (1999)

This study uses the Panel Study of Income Dynamics (PSID) dataset, which collected data from a national representative sample of American families during the period between 1968 and 1992, to test whether homeownership by parents have an impact on their child's highest educational attainment. The population of sample (POS) was restricted to children who left their parents' households between 1975 and 1982 to allow a 10-year interval in which to observe subsequent children's outcome after leaving their parent (n=911). In other words, the analysis tested the relationship between homeownership by parents and family situation during the 7 year prior to the year in which the children left their parents' home, and child highest education attained within the next 10 years period. Assuming correct function form, regression results should be able to be generalized to the whole POS.

In regression analysis, controlling for selection bias depends heavily on statistical control to partial out the influences of covariates. In their study, Boehm and Schlottmann (1999) controlled a set of different family covariates, including family size, parents' asset income and non-asset income, average house value, and parents' educational background. According to our literature review, these are indeed very important covariates that could greatly influence homeownership status and child education outcomes. To improve the precision of estimate, they also included four child characteristics (i.e., gender, race, veteran status, and time disabled). They found that after controlling for other covariates, the regression coefficient of homeownership remained significant.

This study has several strengths. It uses a national representative sample. More importantly, a clear time order was established for making any causal conclusions. In addition, the authors included some of the most important covariates into their model.

However, the list of family characteristics they "controlled for" is no way an exhaustive list of potential confounders. The presence of systematic differences between homeowners and non-homeowners in terms of expectations, sense of responsibility, and other psychological and behavioral characteristics could distort or explain away the significant impact of homeownership found in this study. Other techniques are in need in order to control the systematic differences (i.e., selection bias).

4.2 Instrumental variable estimation (IVE)

Instrumental variable estimation represents one of such techniques. To put it simple, an instrument variable is a variable closely related to the treatment, but has no direct impact on the outcome variable (i.e., the only way for an instrument to exert control on the outcome is through the effect of the treatment). IVE approach carves out the influences of unobserved covariates (Us) on outcomes and is able to provide unbiased estimate of treatment effect, provided that the instrument is a valid one.

Study 2: Green & White (1997)— endogenous switching model

In this often cited paper, Green & White (1997) examined whether homeownership by parents predicts their 17 or 18-year-old child's schooling status. Three datasets including PSID, the Public Use Microsample of the 1980 Census of Population and Housing (PUMS), and High School and Beyond (HSB) were used to cross-validate homeownership estimates. Population of sample was restricted to children of PSID households who were 17 years old in any of the years 1980-1987; households that contained a 17 year old in 1980 in PUMS; and children of 18 years old in a follow-up survey in 1982 of the HSB data.

The authors first tested three probit models with the above-mentioned datasets respectively. The dependent variable for all three models was the same— a binary variable equaling one if a 17 or 18-year-old is still in school or have already graduated from high school and equaling zero if they have dropped out of school. A set of household characteristics and homeownership status were entered as the predictors, with slight variations across three datasets, depending on the availability of relevant information in each dataset.

More specifically, household explanatory variables (other than homeownership status) included race of the household head, family size, family income, household head's educational level, marital status, and employment history in PSID sample. In PUMS, the length of tenure (i.e., the number of years the household has lived in its present housing unite) and housing quality were also included other than the fore-mentioned predictors in the PSID sample. The inclusion of tenure length and housing quality could help reduce the omitted variable biases. With the HSB data, some additional variables included were youth disability status and language spoken in their household. Three models yielded similar results by supporting the hypothesis that home-owning by parents have a statistically significant impact on whether their children stay in school.

The authors were not completely satisfied with regression results and they were aware of the endogeneity of homeownership. In order to have a better control for selection bias, they estimated a bivariate probit (endogenous switching) model with PSID dataset, which explains both parents' tenure decision and children's stay-in-school decision. This procedure bears the spirit of instrumental variable estimation. The equations were shown below (copied from Barker & Miller, 2009).

$$I^* = \boldsymbol{\gamma}' \mathbf{Z} + \alpha P + \mu$$

$$J_0^* = \boldsymbol{\beta}_0' X + \varepsilon_0 \ if \ I = 0$$

$$J_1^* = \boldsymbol{\beta}_1' X + \varepsilon_1 \ if \ I = 1$$

$$I = \begin{cases} 1 & if \ I^* > 0 \\ 0 & if \ I^* \leq 0 \end{cases}$$

$$J_i = \begin{cases} 1 & if \ J_i^* > 0 \\ 0 & if \ J_i^* \leq 0, \end{cases}$$

I^* is parents' propensity to purchase a house, Z is a vector affecting parents' tenure choice (other than the instrument), P is an instrument that captures the relative cost of owning versus renting, whereas J^* is child's propensity of staying at school. The instrument—relative housing cost should affect parents' propensity of home purchasing directly but not child education outcomes.

The idea was that if there were uncontrolled unobserved covariates (Us) in their original probit model, the residual terms ε and μ should be correlated. And this would bias the homeownership effect estimates obtained in their earlier analysis. In order to rule out this selection bias, they estimate the Corr (ε_0, μ) and Corr (ε_1, μ) using full information maximum likelihood and found that the correlations were not statistical significant. Therefore, they concluded that selection bias is not a problem in their sample (i.e., that their regression estimates were most likely not biased).

However, their conclusion may not be valid after all because the particular instrument variable, which is the ratio of the average user cost of owner-occupied housing to the average rent on rental housing in the year of the household most recent move, may not capture the full picture of the economic cost of home-owning. Other economic determinates of home purchasing such as local property tax rate, expected house price, and interested rates may also influence home purchasing and will be reflected in the residual term μ. Since these purely economic factors are uncorrelated to personal characteristics, the correlation estimate between ε and μ will most likely be smaller. In other words, the insignificant correlation in their analysis might be caused by the inability to include relevant instruments rather than the absence of selection bias.

Instead of testing for the correlations between residuals, we could use regular IVE approach to first regress the treatment status on the instrument and covariates Xs; and then regress the outcome on the predicted value of treatment along with Xs and Ws to obtain unbiased treatment effect estimate for the

compliers. This could address the problems identified above and strengthen the internal validity. However, it limits the population of causal inference (POCI) to compliers, whereas in Green & White's original paper, generalizability was not compromised (i.e., POCI is the virtually the same as POS). The question is whether to have a stronger internal validity with limited generalizability or have good external validity but to live with a strong assumption for internal validity.

Other weaknesses of the probit estimates are that family wealth, home environment or neighborhood effects were not controlled for, and that the same functional form was assumed for the whole POS when different sub-groups (e.g., low and high income family) might have different functional forms.

Study 3: Aaronson (2000) – Instrumental variable estimation

With a critical attitude toward the assumptions made in Green & White's work, Aaronson (2000) re-examined the PSID dataset utilizing instrumental variable approach. He expanded his sample to children who reached the age of 17 between 1975 and 1993 in the PSID dataset. The dependent variable, which measures whether the child graduated from high school by age 19, is also slightly different from Green & White's study. He further matched PSID data to geocode database to capture some residential mobility of the family and their neighbors and tested how residential mobility influenced homeownership effects.

Aaronson first estimated a probit regression including observable family characteristics (e.g., child's gender, race, parents' age, head's education, family size, etc.) on the outcome variable and obtained homeownership effect comparable to that in Green & White's study. He then added measures of residential mobility including frequency of residential moves, the duration of residential and neighborhood residence into the regression model, and found half of the homeownership effect obtained in previous model disappeared. This led him to question the distinctive contribution of homeownership. He suspected that the "effect" of homeownership could merely reflect its association with third factors such as residential mobility, home equity, or some other latent conditions, rather than its unique impact on the outcome.

To address the endogeneity problem of homeownership, Aaronson utilized an instrumental variable approach. He constructed two instrumental variables: one is group average homeownership rate (formed by taking state-year average homeownership rates by race and income quintile using the March CPS surveys) for homeownership; the other one is residential mobility (measured by family mobility rates prior to the child turning age 5). He argued that average homeownership rates could pick up regional variation that is driven by housing costs, property tax rates and other secular trend in housing and would be unrelated to the error terms of child educational outcomes. As for residential mobility, he suspected pre-school moves would not influence child's school progress. Two sets of two-stage regressions, one that includes the homeownership instrument and one that includes both homeownership and residential mobility instruments were tested with five samples: the full, the low and high income neighborhood, and the low and high mobility neighborhood samples. As expected, the estimates were smaller than the probit model tested earlier and were statistically insignificant after controlling for residential mobility (with one exception for low mobility neighborhood sample). For low mobility neighborhood sample, homeownership effect remained significant.

Aaronson's study, compared to Green & White's article, has two major strengths. First, he utilized two instruments to control for the endogeneity of homeownership and residential mobility, which should be able to yield unbiased estimate provided the instruments were valid. The assumptions for the instrumental variable approach were comparatively weaker than that in Green & White's article. The second strength is that he did not impose the same functional form on the whole sample; instead, he estimated homeownership effects with different sub-groups respectively (i.e., high and low income group, high and low neighborhood mobility group). Again, this requires weaker assumptions as compared to Green & White's study in which they assumed a single function form for the whole sample.

The validity of the two instruments, however, can be challenged. An instrument is not supposed to be directly related to the outcomes. It is plausible that frequent pre-school movement could influence children's psychological wellbeing and cognitive ability, therefore disqualifies the residential mobility variable as a valid instrument. A good instrument should also be strongly related to the treatment. The partial R^2 from the instruments ranges from .049 to .088, not exactly the strongest predictor of homeownership.

4.3 Difference-in-difference
Study 4: Barker & Miller (2009)

Barker and Miller used a "difference in difference" method to estimate homeownership effect on children's reading and math test scores. They restricted their population of sample to families that changed from renters to owners or owners to renters. They argued that if an independent effect of homeownership exists, one would expect an improvement in test scores of children in families that move from renting to owning and a decline in test scores of children in families that

changed from owning to renting, holding other family characteristics constant.

The authors looked into the Early Childhood Longitudinal Study (ECLS) data. The identified families with changes of tenure during the times that the child was in first grade and third grade. Descriptive statistics showed that children of homeowners consistently scored higher than children of renters, and children who moved from rental to owner occupied housing improved more than children who moved in the other direction. They then regressed the improvement in test scores from first to third grade on dummy variables indicating whether the family stayed as owners for the whole time, whether it moved from renting to owning, whether it moved from owning to renting, and whether it stayed as renters (always renting is the reference group). No significant homeownership effect was found.

However, their findings should be interpreted with caution. First of all, the effective sample size was small because not so many families changed tenure within a three-years period (79% of the families were owners in 1st grade and 82% were owners in 3rd grade, exact numbers were not provided in the article). Moreover, the analysis was restricted to those who changed tenure within a three-year period, which may be too short to detect any long-term effect of homeownership.

The major problem with the DID estimate in this study is that the conditional independence assumption seems implausible. In the analysis, the effects of always-owners, owner-to-renter, and renter-to-owner were compared to the effect of always-renters. To get an unbiased DID estimate, there should be no unobserved covariates (Us) after differencing the effect of always-renters. This assumption may not hold. For example, families who switched from renting to owning could be more likely to move to a better neighborhood, they might decide to own because they were getting remarried or having another baby, or they might care more about their children and therefore they decided to purchase a house as the child gets older. Although the authors controlled several family changes during this period, it was unlikely that these change measures were exhaustive. In short, it takes a strong assumption to believe the observed homeownership effect is unbiased that it is not merely a reflection of its association with other covariates.

In addition, the authors imposed same functional form for those who moved from renting to owning and those who moved from owning to renting by putting them into the same regression model. This may not hold because families who switched from owning to renting and those who switched from renting to owning could have very different experiences. In other words, they could be systematic differences between these two types of families and the relationships between the treatment, covariates, and the outcome for these families could be different.

4.4 Fixed Effect
Study 5: Boyle, Georgiades, Mustard & Racine (2007)

An alternative method to control for family-specific and neighborhood-specific unobserved factors is fixed effect approach. Conditional fixed effect models allow identification of within-neighborhood and/or within-family variation in homeownership status. Utilizing fixed effect to control for family-specific factors requires sibling data and variation in tenure status within siblings. Such cases are difficult to identify. To our knowledge, no study has ever utilized fixed effect to control for family-specific unobserved factors to estimate homeownership effect on children's educational outcomes. Studies that estimated neighborhood fixed effect do exist. Though not an ideal case, here we present one study that estimated homeownership effect when controlling for neighborhood-specific fixed effect.

In this study, Boyle et al. (2007) examined a set of neighborhood and family characteristics on participants in 1983 (when they were between 4 and 16 years old) and their total years of education (excludes grades repetition) in 2001 when they were 22-34 years old from the Ontario Child Health Study (OCHS). They used three-level multilevel modeling since the data was nested (i.e., children nested within family, family nested within community). Homeownership (renting versus owning) was one of the several family characteristics included to predict child education attainment. Since homeownership was not their focal predictor and treated as a fixed factor at the family level, it was not possible to rule out family-specific unobserved characteristics for homeownership effect estimate. However, neighborhood-specific unobserved characteristics were controlled for provided that there were within community variation on tenure status. The results suggested that children living in renting families when they were young had lower educational attainment when they were young adults.

The major problem with the estimate is that family-specific unobserved characteristics were not controlled for. It is possible that parents who owned were systematically different from those who rented in the same neighborhood. Although several family demographic, physical, behavioral and psychological characteristics were included in the regression analysis, there is no guarantee that additional confounders do not exist.

4.5 Differential Effects
Study 6: Harkness & Newman (2003b): IVE differential effects

A common weakness of the afore-evaluated

studies is that their findings were based on samples that included families from across the income spectrum (Harkness & Newman, 2003b). To test if the benefits of homeownership differ across income levels, Harkness and Newman utilized differential effects approach and estimated homeownership effects in low-income and high-income groups separately using IVE. The population of sample came from the PSID data. Individuals who were born between 1957 and 1973 and who had family data available for each year when they were between ages 11 and 15 in the PSID data were included into the analysis. The two groups included children from families with parental earnings less or more than 150% of the federal poverty threshold for at least three of the five years the children were between age 11 and 15. Children's educational outcomes were measured by whether they graduated from high school at age 20, years of education at age 20, and whether they had any post-secondary education at age 20.

For instruments for homeownership labeled as highway stock (annual change in state's per capital highway investment), cost ratio (ratio of owner to renter costs in census region), metropolitan area (ratio of median property value of median rent), and state homeownership rate were tested to compare their robustness. All four instruments significantly predicted homeownership in low-income group, but only two significantly predicted homeownership in high-income group. Two-stage regression results suggested significant homeownership effects for all three educational outcomes in low-income group; however, no significant effects were detected for high-income group after covariates and the instruments were included.

The major strength of this study is that it does not impose the same functional form on low-income and high-income groups. Instead, IVE approach was utilized on each sub-group and differential effects were detected. Another strength is that four different instruments were used to provide comparisons among instruments.

However, two instruments (highway stock and cost ratio) were not exactly valid instruments for high-income groups as they failed to predict homeownership within that group. Another weakness is that by operationalizing homeownership as years living in home-owning families, the authors assume the outcome changes as a linear function of homeownership. This might not be valid.

5.0 Conclusion
Based on our review, homeownership appears to have only small independent effect on children's educational outcomes. A significant proportion of homeownership effect observed in cross-sectional studies can be explained away once other important

covariates are included in the model. This is not surprising given the endogeneity of homeownership. In order to obtain an unbiased estimate of homeownership effect, future research should either adopt true experiment design or utilize appropriate quasi-experimental methods to deal with selection bias.

If plausible instrument(s) could be constructed, IVE appears to be the best choice because the assumptions required to make valid causal inference are relatively weaker compared to other methods. Besides, it does not require much on the specific nature of the data and/or the original research design. Because of its efficiency, IVE is the most popular quasi-experimental method in homeownership literature. However, future studies should take caution in selecting instruments. The validity of instruments should be explicitly evaluated theoretically and empirically. Using multiple measures to construct an instrument or using multiple instruments seem to be a good way to cross-validate the results of IVE.

When a plausible instrument does not exist, fixed effect may be a good choice when the data is nested. The ideal situation would be sibling data where one sibling lived with renting parents at one time and the other(s) lived with home-owning parents at another time. Theoretically, using fixed effect with sibling data should be able to eliminate the effects of unobserved family characteristics. However, the results should be interpreted with caution since other family changes over time that affect the educational outcomes of the siblings differently may be correlated with switches in homeownership. Family changes (e.g., divorce, being laid-off, etc) that could be related to change of tenure and children's educational outcomes should be controlled for when estimating homeownership effect.

Another quasi-method that could be utilized to estimate homeownership effect, though less commonly applied, is difference-in-difference (DID). The endogeneity of homeownership makes the conditional independence assumption in DID vulnerable to criticism. When a panel of people who switch their tenure voluntarily (i.e., without any policy incentives) is used to obtain DID estimate, important family changes should be controlled for. Another way to implement DID is to utilize existing policy changes.

When DID and fixed effect does not apply, propensity score matching (PSM) is another alternative to estimate homeownership effect. Compared to DID and fixed effect, PSM has little requirements on the data structure and can be easily implemented as long as a comparison group can be formed. To our knowledge, no study has ever utilized PSM to estimate homeownership effects on children's educational outcomes. The validity of PSM largely relies upon whether all the important covariates are included at the first step to predict homeownership status. If one can

make the case that important covariates are included, then PSM seems to be a convenient and efficient choice.

Evidence also suggests that homeownership effects differ across groups (e.g., low-income, high-income group). Future studies should take this into consideration and avoid imposing the same functional form for different groups of people.

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