Childhood Housing Tenure and Young Adult Educational Outcomes: Evidence from Sibling Comparisons in Norway

Kristin Aarland, PhD

Senior Researcher

NOVA, Norwegian Social Research

Oslo Metropolitan University, Oslo, Norway

Kristin.Aarland@oslomet.no

Anna Maria Santiago (corresponding author)

Professor

School of Social Work

Michigan State University, East Lansing, MI

santia63@msu.edu

George C. Galster

Emeritus Clarence Hillberry Professor of Urban Affairs

Department of Urban Studies and Planning

Wayne State University, Detroit, MI

george_galster@wayne.edu

Viggo Nordvik

Research Professor

NOVA, Norwegian Social Research

Oslo Metropolitan University, Oslo, Norway

viggo.nordvik@oslomet.no

DOI: http://dx.doi.org/10.1016/j.jhe.2021.101772

ABSTRACT

We investigate how childhood housing careers affect young adults' secondary school and college educational attainments, focusing on the role played by cumulative exposure to homeownership. We analyze Norwegian census and administrative data using extensive controls for youth, household, housing, mobility, and neighborhood characteristics and employ, as a methodological first in this domain, family fixed effects. We find that, compared to an otherwise-comparable sibling experiencing identical residential contexts, a youth who lived one more year in a home owned by the parent(s) had a 1.4 percentage-point higher probability of completing high school by age 21 and a 1.7 percentage-point higher probability of enrolling in college by age 20. These effects arise from homeownership per se, independent of its relationship with dwelling type, mobility, or neighborhood.

HIGHLIGHTS

- Examine how cumulative childhood exposure to homeownership affects young adults' secondary school and college educational attainments.
- We analyze Norwegian census and administrative data using extensive controls for youth, household, housing, mobility, neighborhood and employ, as a methodological first in this domain, family fixed effects.
- Compared to an otherwise-comparable sibling experiencing identical residential contexts, a child who lived one more year in a home owned by the parent(s) had a 1.4 percentage-point higher probability of completing high school by age 21 and a 1.7 percentage-point higher probability of enrolling in college by age 20.
- These effects arise from homeownership per se, independent of its relationship with dwelling type, mobility, or neighborhood.

Childhood Housing Tenure and Young Adult Educational Outcomes:

Evidence from Sibling Comparisons in Norway

Introduction

The determinants of children's development of human capital have long been of interest to economists. Underlying much of the existing child development literature is Bronfenbrenner's ecological systems model, which postulates that children's outcomes are influenced by both characteristics of family of origin as well as the characteristics of dwelling units and neighborhoods in which children live and interact (Bronfenbrenner and Morris, 1998). This latter realm of dwelling and neighborhood context brings housing economics to the forefront. To what degree does the childhood housing career – the cumulative exposure to particular dwellings, neighborhoods, mobility patterns and tenures experienced by children – independently affect educational outcomes in early adulthood?

There is a broad consensus based on strong research designs regarding impacts of the first three elements of childhood housing careers. Dwellings that suffer from substandard structural quality (e.g. damp, cold), toxic substances (e.g., lead, mold) and/or overcrowding harm health and cognitive-behavioral development and thereby educational outcomes (Prescott and Vesbo, 1999; Goux and Maurin, 2005; Lien, Wu and Lin, 2008; Newman, 2008; Mohanty and Raut, 2009; Bourassa, Haurin and Hoesli, 2016, Loopoo and London, 2016; Clair, 2018; Cordes, Schwartz, and Stiefel, 2019).[1] Neighborhoods manifesting violence, pollution, deviant peers, concentrations of disadvantaged populations and weak schools retard cognitive and educational achievements (Mohanty and Raut, 2009; Galster et al., 2016; Chetty, Hendren and Katz, 2016; Galster and Santiago, 2017; Chetty and Hendren, 2018; Chyn, 2018; Laliberté, forthcoming).[2] Similar harms arise if children change residences frequently (Aaronson, 2000;

[1] See Dunn (2000) and Easterlow, Smith and Mallinson (2000) for review of the associations between housing and health.
[2] For reviews of the theory and evidence related to neighborhood and peer effects on education, see Ross (2012), Nieuwenhuis and Hooimeijer (2016) and Galster and Sharkey (2017).

Lien, Wu and Lin, 2008; Mohanty and Raut, 2009; Gasper et al., 2009; Chen, 2013; Yun and Evangelou, 2016; Cordes, Schwartz, and Stiefel, 2019), partially as a result of intervening impacts on substance use, social functioning, mental health, and sexual and deviant behaviors (Astone and McLanahan, 1994; Pettit and McLanahan, 2003).

Unfortunately, no such consensus exists in the literature regarding the human capital developmental impacts of the last element of housing careers: rental vs. owner tenure. There are many putative benefits to children living in a home owned by their parents, including superior physical conditions, stability of tenure, skill development and wealth, plus more developmentally supportive role-modeling, behavioral supervision, and self-esteem of parents. Whether these benefits are manifested empirically has been the source of much contention, however. Persuasive statistical evidence has been difficult to obtain due to the challenges of selection, omitted variables, endogeneity, and heterogeneity by income and ethnicity (Newman and Holupka, 2013). Newman and Holupka (2013: 235) summarize the state of knowledge as follows:

> "For those interested in the effects of homeownership on children's well-being, the empirical literature offers good news and bad news. The good news is that the topic has generated a sizable body of serious research by highly respected researchers. (…) The bad news is that this research has not produced consistent evidence about whether the effects of homeownership are positive, negative, or nonexistent. (…) Most researchers agree, however, that a (or perhaps *the*) major challenge in estimating the net effects of homeownership on child well-being is separating the effects of the characteristics of parents who select into homeownership, which are highly correlated with child outcomes, from the effects of homeownership per se."

In this paper, we aim to contribute to this literature by answering the research question: To what degree does the cumulative childhood experience of living in a dwelling owned by parents affect the probability of completing secondary school and attending a post-secondary (college) institution as a young adult? We use data from Norwegian census and administrative registers that provide extensive controls for family, child, dwelling, residential mobility and neighborhood characteristics. We attempt to surmount the daunting methodological challenge of disentangling the selection effect from the causal effects of homeownership by employing an intra-family analysis, comparing educational outcomes across full siblings who have spent differential amounts of time in owner-occupied housing during their childhood. To our knowledge, this is the first time this family fixed-effect method has been employed in estimating the effect of homeownership on young adult educational outcomes.

The family fixed effects model effectively controls for any time-invariant family background effects that may affect both parents' tenure choice and children's educational attainment. Siblings who grow up in the same household with the same parents share the same home environment as well as neighborhood and social environment at large. Although parental behavior towards each child may not be exactly the same, the extent to which they value education and emphasize educational attainment as a life goal, is likely to be conveyed to all children in the household. Moreover, parental practices such as reading to their children, attending parent-teacher conferences, helping with homework and, in general, taking an interest in their children's schoolwork, that have all been found to benefit children's educational outcomes, are also likely to benefit all children similarly. However, while the family-fixed effects remove time-invariant unobservable parental characteristics, there may be other time-variant family characteristics at play that still produce a correlation between unobservable characteristics subsumed in the error term and the homeownership variable and therefore bias the coefficient of interest. First, changes in family income may influence both the choice of housing tenure as well as access to enrichment activities that boost children's academic performance. We therefore explicitly control for parental income during childhood. Second, family dissolution will often cause both a drop in income and a move in addition to the emotional turmoil brought on by the event itself, all of which are likely to have a negative impact on children's educational outcomes. We include controls for both moves as well as time not spent with both parents in order to control for this event.

Based on the lack of consensus about the existence and magnitude of homeownership effects on young adult educational outcomes and the fact that we are using an identification strategy new to the field, we prioritize rigor and internal validity over generalizability in our approach. Our sample consists of siblings for whom we have exact information on timing of moves and tenure transitions. For reasons articulated more fully in the data section of the paper, we employ a low-mobility sample where both variation in homeownership exposure and its presumed impact are expected to be lower than in a population-representative sample. Still, we find evidence that cumulative exposure to homeownership raises the probability of completing secondary school and enrolling in college, even after controlling for family characteristics, dwelling type, mobility and neighborhood.

Review of the Theoretical and Empirical Literature

The Theory of Homeownership and Children's Educational Outcomes

How might parental homeownership produce positive outcomes for resident children?[3] Galster and colleagues (2007) identified seven direct causal mechanisms associated with parental homeownership on child outcomes from the extant literature. First, the quality of housing maintenance is higher in dwellings owned and occupied by homeowners relative to those occupied by renters (Galster, 1983, 1987) which has been found to affect the health, cognitive and social development of resident children (Parcel and Menaghan, 1994).[4] Second, children may benefit from the transfer of tangible skills associated with homeownership, including home repair, contract negotiation, and home financing, and more generally the

[3] Dietz and Haurin (2003) provide an exhaustive review of the potential consequences of homeownership on children.
[4] Holupka and Newman (2012) could not detect any significant homeownership effect on health after controlling for mobility, neighborhood and wealth.

capacity to make and adhere to long-term plans (Green and White, 1997; Boehm and Schlottmann, 1999). Third, homeowners desiring to maintain high property values and quality of life are more likely to monitor and control anti-social behaviors of resident adults and children in the neighborhood including those that might negatively influence academic outcomes (Haurin, Parcel, and Haurin, 2002; Hoff and Sen, 2005).[5] Fourth, homeowners and their children tend to be more engaged in neighborhood activities and socially connected to their neighbors, which benefits resident children in a variety of ways (Cox, 1982; Rohe and Stegman, 1994b; Rossi and Weber, 1996; DiPasquale and Glaeser, 1999; Hilber, 2010).[6] Fifth, homeowners often report higher levels of personal satisfaction and self-esteem, both of which have been found to produce more supportive, positive socio-psychological environments for children (Rohe and Stegman, 1994a; Rohe, Van Zandt, and McCarthy, 2013: Rossi and Weber, 1996; Santiago, Roberts and Lee, 2014). Sixth, the equity earned through home appreciation generally affords homeowners with substantially higher levels of wealth relative to renters (Herbert and Belsky, 2006), thereby increasing the likelihood that homeowners will invest more in their children's education and other aspects of their environment.[7] Seventh, the greater security and residential stability associated with homeownership have been found to reduce environmental stress among homeowners, which, in turn, may yield more positive behavioral and cognitive outcomes for their resident children (Cairney and Boyle, 2004).

The effects of homeownership on educational outcomes may also occur indirectly through increased residential stability and the bundle of neighborhood attributes associated with purchasing a home. High transaction costs associated with home sale and purchase (Haurin,

---

[5] The evidence on whether homeownership has behavioral impacts on children is mixed, however; see Haurin, Parcels and Haurin (2002), Grinstein-Weiss et al.(2012), and Holupka and Newman (2012).
[6] However, see the contrary empirical evidence for low-income homeowners' social capital in Engelhardt et al. (2010).
[7] Haurin, Parcel and Haurin (2002) report that after controlling for homeownership status and other parental characteristics, wealth was unrelated to either cognitive or emotional dimensions of the home environment, children's math and reading test scores, or an index of children's behavioral problems. Chen (2013) also found an insignificant wealth effect in his Swedish study of homeownership effects.

Hendershott and Ling, 1988) tend to increase the residential tenure of homeowners in any given dwelling unit relative to that of renters (Rohe and Stewart, 1996). In turn, increased residential stability has been associated with higher levels of educational achievement and credential attainment for children, as noted above.

The Four Challenges of Measuring the Impact of Homeownership

Though there are persuasive theoretical reasons why parental housing tenure might influence children in the household, producing equally persuasive statistical information is difficult due to four challenges: (1) selection; (2) omitted variables; (3) endogeneity; and (4) heterogeneity by income and ethnicity (Newman and Holupka, 2013). The selection issue arises because parents who have certain (unmeasured) motivations and skills related to their children's upbringing also systematically tend to choose a certain form of housing tenure, thus biasing any observed relationship between tenure and child outcomes and threatening causal deductions. The omitted variable challenge arises because many characteristics of parents, children, dwellings, neighborhoods, and schools affect the educational achievements of young adults; failure to control adequately for them will bias the measured relationship with tenure to the degree that they are correlated with tenure. If tenure choice affects other housing career decisions like mobility, including these endogenous characteristics in a model of young adult educational outcomes may "over-control," thereby understating the full influence of tenure through these indirect causal pathways (Angrist and Pischke, 2009; Cinelli, Forney and Pearl, 2020).[8] Finally, both the direct and indirect (mediated) impacts of tenure choice on children's educational outcomes are likely heterogeneous across household groups categorized by socioeconomic status, race, or ethnicity (Harkness and Newman, 2003; Holupka and Newman,

[8] If choice of tenure, mobility and neighborhood are assumed to be mutually causal, the problem becomes more challenging; cf. Aaronson (2000), Galster et al. (2007) and Chen (2013) for alternative strategies.

2012; Chen, 2013; Sharkey and Faber, 2014); more aggregated samples thus will provide misleading estimates of relationships.

The Empirical Literature

Over the last quarter century, there has been a distinct evolution in the conclusions of studies examining the relationship between homeownership and the cognitive development, health, behavior and educational attainment of children; see the reviews by Dietz and Haurin (2003), Barker and Miller (2009), Newman and Holupka (2013), Barker (2013) and Bourassa, and Haurin and Hoesli (2016). What was previously a strong consensus about the benefits of homeownership has now morphed into considerable skepticism. In our view, this evolution can be tied to a growing recognition of the aforementioned empirical challenges and limitations of the techniques for overcoming them. Green and White's (1997) seminal work was the first to confront the selection challenge by employing instrumental variables (IV) for tenure choice; it has since become the methodological standard in this realm: see Aaronson (2000), Haurin, Parcel and Haurin (2002), Harkness and Newman (2003), Galster et al. (2007), Mohanty and Raut (2009), Green, Painter and White (2012), Holupka and Newman (2012), Chen (2013) and Bourassa, Haurin and Hoesli (2016).[9] Despite its widespread use, the IV technique has well-known limitations, the problem of weak instruments being paramount (cf. Galster et al., 2007 and Holupka and Newman, 2012). The sensitivity of results to alternative IVs (Harkness and Newman, 2003) and to IV vs. propensity score matching has also been revealed (Holupka and Newman, 2012).

[9] The commonly employed instruments are housing price indices and homeownership rates for the state or metropolitan area in which the observed household lives. Propensity score matching (Holupka and Newman, 2012; Grinstein-Weiss et al., 2012), difference-in-differences, i.e., transitions between tenures (Barker and Miller, 2009) and fixed-effects for small geographic areas (Lien, Wu and Lin, 2008) also have been used to control for selection in this realm of research. For a detailed review of the these studies through 2012, see Newman and Holupka (2013: Table 1).

Several studies controlling for selection with IVs concluded that benefits for cognitive development or educational credentials accrue to children who grow up in homes owned by their parents (Green and White 1997; Aaronson, 2000; Haurin, Parcel and Haurin, 2002;Galster et al. (2007; Green, Painter and White, 2012; Chen, 2013). However, caveats to this broad conclusion began to mount. Several studies have found that the magnitude of the homeownership effect (and sometimes its statistical significance) depended on what other aspects of housing career were controlled. In particular, homeownership effects were often strongly attenuated or rendered insignificant when mobility was controlled (Green and White, 1997; Aaronson, 2000;Galster et al., 2007; Mohanty and Raut, 2009; Chen, 2013).[10] Others found a statistically significant homeownership effect on educational attainment only for particular socioeconomic, racial or ethnic strata (Harkness and Newman, 2002; 2003; Holupka and Newman, 2012). Perhaps the most skeptical conclusions were reached by Barker and Miller (2009), who failed to replicate the results of Green and White (1997) and Haurin, Parcel and Haurin (2002) when additional controls and an alternative method for discerning selection were used and could not find any positive homeownership effect on test scores when analyzing a new dataset.[11]

After surveying the unsettled state of this literature, Newman and Holupka (2013) recommended a way forward involving: (1) another strategy to addressing selection beyond the IV approach; (2) a broad set of household covariates; (3) avoiding over-controlling endogenous variables; (4) testing relationships for different socioeconomic and ethnic strata. We take these recommendations seriously in this paper. Specifically, our study contributes to this literature in four ways. First, we address selection by exploiting inter-sibling differences in housing careers (i.e., family fixed effects), the first time this method has been employed in testing

[10] Even though this likely represents “over-controlling” (Angrist and Pischke, 2009).
[11] The nascent international literature on this topic is similarly inconsistent, with Lien, Wu, and Lin (2008) and Chen (2013) finding positive educational impacts of homeownership in Taiwan and Sweden, respectively, but Bourassa, Haurin and Hoesli (2016) finding none in Switzerland.

homeownership effects. Second, we employ administrative data from Norwegian social registers that provide a rich set of household covariates for a large sample of families. Third, we employ a “pseudo-stepwise regression” approach to ascertain upper and lower bounds for the influence of homeownership working through the endogenous (mediating) mobility variable. Fourth, we explore how relationships might differ between youth from lower- and higher-income households, as well as other heterogeneity tests.

The Norwegian Context

The educational and housing market systems in Norway present many substantial differences from their U.S. counterparts. Since these differences affect our specification of outcome variables and interpretation of results, we briefly discuss them at the outset.

Educational System in Norway

The educational system in Norway has three components: elementary, secondary and postsecondary (college/university). Elementary education is compulsory and free for all children between the ages of 6 and 16. Secondary education is also free but not compulsory; however, all young people between the ages of 16 and 21 are entitled to secondary schooling of up to four years. Students complete their secondary studies in one of three general education/college preparatory tracks or in one of the twelve vocational education and technical training programs aimed at preparing students for the labor force (Statistics Norway, 2019). Though not compulsory, in 2019 97.3% of 16 year olds enrolled in secondary education directly after completing elementary education (Statistics Norway, Table 11964: *Transitions to and from upper secondary education, by transition, region, contents and year*).

Although 92% of all 16-18 year olds are enrolled in secondary education (Statistics Norway, 2019), the completion rate is widely seen in Norway as a concern (Bunting and

Moshuus, 2017) because of the association between secondary school completion and the economic performance of young adults. Barth and von Simpson (2013) showed that only 70% of Norwegians who had not completed secondary school were either working or studying at age 31, compared to 85% among those who had received a diploma. The Norwegian five-year secondary school completion rate hovered between 67% and 71% for many years, although it has trended upwards recently and is currently at 73% (Norwegian Directorate for Education and Training, 2017). There are, however, large differences in completion rates across tracks (86% in academic tracks vs. 59% in vocational tracks[12]), gender (80% for women vs. 71% for men) and region (ranging from 63% to 78%). In particular, the high dropout rates among young men in vocational tracks has been a major policy concern.

As a general rule, one needs to have graduated from secondary school with a college preparatory diploma in order to attend college or university in Norway.[13] College attainment in Norway is slightly above the OECD average with 49% of the population age 25-35 and 33% of those age 55-64 having completed a college/university degree, compared to the OECD averages of 43% and 27%, respectively. In comparison to the United States, the share of college graduates is similar among the younger cohorts (49% vs. 48%), but lower among the older cohorts (33% vs. 42%, respectively) (OECD Education Statistics, 2020). Post-secondary studies in Norway are free of charge at any public college or university. Moreover, the Norwegian State Educational Loan Fund allocates grants and lends money to students accepted to any accredited institution. Hence, lack of funding is rarely a major reason for not attending postsecondary studies in Norway. However, as in other countries, children's educational attainment in Norway is positively correlated with their parents' education and their

[12] The graduation rate also varies across vocational tracks, from 45% in food-and restaurant trades to 81% in media and mass communications.
[13] A high school diploma from a vocational track can be supplemented with a one-year course, however, to gain college admittance.

overall socioeconomic status (Kristensen and Bjorkedal, 2010; Markussen, Frøseth, and Sandberg, 2011; Björklund and Salvanes, 2011; Black, Devereux and Salvanes, 2005).

Housing in Norway

Since 1945, the Norwegian government has promoted universal individual or cooperative homeownership as one of the foundations of the Norwegian welfare state. Both the central and municipal governments supported homeownership, primarily through advantageous taxation of owner-occupied housing capital, subsidized mortgage loans for new construction offered through the State Housing Bank, and a regulated supply of land (Sandlie and Gulbrandsen, 2017). The housing market was deregulated in the 1980s, abolishing general subsidies for housing construction and eventually restructuring the housing policy from a general to a targeted approach aimed at supporting vulnerable groups (Bengtsson, Ruonavaara and Sørvoll, 2017). Homeownership is no longer subsidized directly, but homeowners still enjoy preferential treatment in the form of tax deductions of mortgage interest, no taxation of imputed rental income, below-market valuation of owner-occupied homes for wealth tax purposes, low or non-existent property taxes, and tax-exempt capital gains in home appreciation and in property inheritance. The sustained focus on homeownership has naturally resulted in a high homeownership rate, currently at 81%, including 14% cooperative homeownership prevalent in Oslo and other large cities (see Eurostat, 2020 for homeownership rates).

In contrast, rental housing in Norway is often viewed as a step in the path to homeownership, and the rental sector is characterized by a high degree of transiency. Overall, renters in Norway tend to be younger, single, less affluent and disproportionately of immigrant background compared to homeowners (Gulbrandsen and Nordvik, 2007). Contrary to other Scandinavian nations, Norway does not have a large, purpose-built rental sector. The private rental market is dominated by small-scale landlords who rent out parts of their own home or a small number of dwellings.[14] These units are often leased temporarily and may be withdrawn from the rental market and converted back to owner-occupation at short notice (Nordvik, 2000). Moreover, the selection of rental units is more limited compared to owner-occupied units and concentrated in smaller dwellings. Importantly, and in contrast to other Northern European countries, the public housing sector plays a minor role in Norway. Comprising less than 5% of the housing stock, it caters to groups that have difficulties securing housing on their own. Additionally, many municipalities have an explicit goal of having a high turnover in their public housing units, granting only time-limited contracts and viewing public housing as a safety net during a temporary period of distress rather than offering long-term housing. In sum, the Norwegian rental market is not geared towards long-term renting as large segments of it do not offer sufficient tenure security and the selection of units offered do not match the changing needs of families over the life course. Consequently, homeownership remains the generally preferred tenure, especially for families with children looking for a stable home in which to raise their children (Aarland and Reid, 2018).

Although housing conditions depend critically on tenure, spatial segregation along tenure lines is not pronounced, as most neighborhoods contain both renter and owner households and few neighborhoods are majority rental.[15] Hence, for the most part renters and owners share local amenities and, more importantly, their children typically attend the same schools.[16] Moreover, curricula, student-teacher ratios, and funding for schools are determined at the national level, hence the potential for large, cross-tenure differences in access to quality schools is much less in Norway than in the United States.

[14] The share of small-scale landlords was 65% in Census 2001, which is the most recent figure available.
[15] The median share of renter households across census tracts was 20% in the 2011 Census. Only 5% of census tracts had at least 50% renters (authors' calculations).
[16] 96.3% of students at the elementary and lower secondary school level attend public schools in Norway; in most cases their local neighborhood school (Statistics Norway, 2019, p. 8). Retrieved from https://www.ssb.no/en/utdanning/artikler-og publikasjoner/_attachment/373651?_ts=16813a35da0

Data and Methodology

Data

In this paper, we employ administrative data from different public registries in Norway, merged with census data from 1990, 2001, and 2011, respectively. The administrative data cover the whole population of Norway from 1990 until 2014 and include a wide range of demographic and socioeconomic characteristics for both parents and their children, as well as detailed moving history and census tract location information as of January 1st of each year. The decennial census data provide detailed information on housing conditions of individual households, including tenure, our main variable of interest. Data from the different registries were merged using a unique individual-level identifier that ensures exact matching.[17]

Sampling Strategy

We first compiled a subset of nine birth cohorts, from 1986 through 1994, where each cohort contains about 60,000 persons. We imposed several restrictions on which children we would analyze in this combined sample of cohorts. For tractability, we included only children in the 1986-1990 cohorts whose parents were members of the same household in the 1990 census. For children in the 1991-1994 cohorts, we included only those whose parents were members of the same household in the year the child was born. Children who left the parental home before turning age 18 were excluded from the sample as were children from peripheral areas of Norway.[18] In these cases, we would not have a reliable indicator of the exposure to parental housing type, tenure and urban neighborhood during all 18 years of their childhood.

[17] In compliance with the regulation of the Norwegian Data Protection Authority, the official personal number was replaced by a project-specific individual identifier before the data were released to the project team.

[18] In remote areas of Norway, many adolescents leave home before the age of 18 to attend secondary school as there is no local school.

Finally, since our identification strategy relied on inter-sibling variation in exposure to homeownership, we included only children with at least one full sibling.

The nature of the data contained in the Norwegian decennial censuses resulted in two further reductions in the sample size of children meeting the aforementioned criteria. First, the 1990 census was a 28% sample as opposed to a complete census. Second, information on housing type and tenure of the household is only available in the census. Thus, we could only analyze children whose households were present in all three censuses and who only moved once (if at all) between pairs of these censuses. The latter condition was imposed because we wanted a precise measure of years of childhood exposure to housing type and tenure; with multiple intra-census moves we could not be sure how and when these characteristics changed. While the first restriction preserves the representativeness of our sample, the second most certainly does not as we retain only low-mobility observations.[19] However, this is not necessarily a threat to internal validity, as we compare low-mobility owners with similarly low-mobility renters. On the other hand, it does imply that the external validity of our results may be compromised. In combination, our screening criteria and limitations imposed by the censuses left us with a final sample of 25,091 children in 11,385 families.[20] Relevant to our family fixed-effect model, this translates into a total of 16,283 sibling pairs.[21] Our data selection and matching process is illustrated schematically in Figure 1.

[Figure 1 about here]

[19] Including only children whose parents lived in the same household in the 1990 census or whose parents lived together when they were born similarly reduces the representativeness of the final sample.
[20] Over 99% of these children are born to native Norwegian parents.
[21] Our family fixed-effect strategy exploits all pairwise differences in homeownership exposure between siblings. In our sample the number of sibling pairs per family varies between one (in two-children families) to 15 (in six-children families). A total of 16,283 sibling pairs are included in our final sample.

The mobility data register documents the exact date of moves for each family/household member. Coupled with the month and year of birth for each child, all exposure variables (tenure, type of housing, residency with both/each parent) have been calculated in months and converted back to years. This technique was employed instead of using year-of-birth and year-of-move data, which have a margin of error of up to 22 months,[22] thereby allowing us to calculate any duration indicators with considerably more accuracy. However, there are two important limitations to our moving data. First, local moves (across census tracts within the same municipality or within census tract[23]) were not registered in the moving file prior to 1999. Relying instead on the child/parents' location on January 1st of each year, we classified children or their parents as movers if their census tract changed from one year to the next during the period from 1990 through 1998,[24] and we set their moving date to July 1st of that year.[25] Second, for a number of observations we could not identify any move although the reported tenure changed from one census to the next. That suggests that the moves were extremely local (within the same census tract between1990 and 1998) or that a tenure conversion took place without a move. For those households who were reported to be homeowners in the 2001 census, we exploited the available geocoding to look up the transaction history for their homes to find the exact dates that the properties changed hands.[26] This transaction date was used to both indicate a move and to calculate any exposure variables pertaining to the house (length of exposure to tenures and types of housing).

---

[22] For example, a child is born in January of year one and moves in December of year two, compared with another child born in December of year one and who moves in January of year two.

[23] As in the United States, Norwegian census tracts are specified to be homogeneous in multiple aspects. Unlike in the U.S., their populations are roughly one-quarter as large, on average.

[24] We cross-referenced the location data with annual indicators of whether the parents stay together, and family type for both parents in cases where the family members have different census tract locations. All inconsistent observations are dropped.

[25] Assuming that moves happen with equal probability on any day throughout the year, using July 1st as the moving date for all local moves prior to 1999 ensures that we err equally on both the positive and negative side when calculating length-of-exposure variables.

[26] Alternatively, if the house was built after the property was bought, we used July 1st of the reported construction year as the moving date.

Outcome measures. Two educational outcomes – completion of secondary school and enrollment in post-secondary (college) education – are the focus of this study. Data for these outcomes are extracted from the Norwegian central register of completed and ongoing education, which is updated annually every October. When we measure these educational outcomes is influenced by the nature of the Norwegian educational system (see Appendix Table A.1 that shows these outcomes measured at different ages). The college preparatory tracks in secondary schools are three-year programs, while vocational or technical training programs usually consist of two years of schooling and two years of apprenticeship followed by a practical exam. Thus, all secondary school students receiving a diploma within a year of being on-time (regardless of track) will have graduated by June of their 21st calendar year (i.e., between ages 20.5 and 21.5).[27] This is the measure we use in operationalizing secondary school completion, which also coincides with official statistics. Operationalizing when college enrollment is measured is a bit more arbitrary, given the variability in when college prep students graduate from secondary school and when (if ever) they begin college. We choose a measure that is analogous to that employed above: secondary school students receiving a diploma within a year of being on-time in the college prep track, and currently enrolled in college, will have started in August of their 20th calendar year (i.e., between ages 19 years and 7 months, and 20 years and 7 months).[28] We provide sensitivity analyses for these two outcomes measured at different ages in the Appendix Tables A.2 and A.3.

Predictors of Educational Outcomes

[27] In Norway, the academic year runs approximately from the 20th of August until the 20th of June.
[28] We recognize that this group includes three categories of secondary school graduates: (1) graduated during their 19th calendar year and in college both 19th and 20th years; (2) graduated during their 19th calendar year and in college only 20th year; (3) graduated during their 20th calendar year and in college 20th year.

In this section, we describe housing, individual, household and neighborhood indicators used to predict our two educational outcomes. For quick reference, these also have been provided in our supplementary materials and can be found in Appendix Table A.15.

Housing indicators. Three indicators measure housing tenure, housing type and housing mobility. Housing tenure (residing in housing owned or rented by parents) is ascertained from the decennial census data as described above. In our analysis, we utilize the cumulative number of years between the ages of 4 and 18 an individual resided in a home owned by their parents.[29] A second variable indicates the cumulative number of years during childhood between the ages of 4 through 18 an individual lived in housing that was not a single-family, detached dwelling. Finally, we employ two dummy variables to capture one and two or more residential moves during childhood, respectively.[30]

Individual characteristics. We control for the youth's gender, birth order, and birth cohort. In our analyses, female is coded as 1; male=0. Three dummy variables indicating birth order -- second born, third born, or fourth or higher born in the family are also included in the model; the reference category is first born. Birth cohort is operationalized using dummy variables for years 1987 to 1994; the reference category is 1986.

Household characteristics. Seven indicators are employed to control for parental and household characteristics that may have varied among siblings during their childhoods. Mother's age at youth's birth is used as a proxy for a variety of age-related maternal characteristics (health, psychological maturity, etc.) that may affect child-rearing practices and home environment. The cumulative number of years during childhood that the youth did not reside with both parents and a dummy variable denoting if the youth was living with a stepparent

[29] Exposure starts at age 48 months as this was the earliest age for which we have complete tenure information for all children in the sample. All exposure variables are measured at monthly intervals.
[30] Because of our low-mobility sample requirement, an alternative specification using the number of moves did not prove as powerful.

at age 18 are included to capture the impacts of family instability. Additionally, we include the total number of people and the total number of siblings in the household when the youth is age 18 as proxies for interior crowding, individualized adult supervision, private study space and other unspecified social dynamics that could affect educational achievement. Finally, the natural log of average household income between ages 1 to 18 and the natural log of average household wealth between ages 7 and 18 are used to control for variations in household resources that siblings may have experienced that affected their cognitive development and educational enrichment activities. We note that the ability to control for parental wealth is unique to our study of tenure effects. Finally, to control for inter-family differences in unobserved characteristics that may affect tenure choice, residential mobility, and neighborhood selection, as well as youth educational outcomes, we employ family-level fixed effects.

Neighborhood Characteristics. In this study, we are not interested in which particular features of neighborhoods might affect youth's educational performance; this has been widely studied elsewhere.[31] Instead, we only seek to ascertain how the relationship between parental housing tenure and youth's educational outcomes may change when neighborhood conditions are controlled. Thus, we employ neighborhood fixed effects by including a series of geographical dummy variables at the level of clusters of (typically between four and eight adjacent) census tracts.[32] We specify the cluster when each youth is age 14.

Descriptive Statistics. Table 1 provides descriptive statistics about completed secondary and ongoing college education by birth cohort, gender and housing tenure. For young adults born between 1986 and 1994, approximately 81% completed high school by age 21, and 39%

---

[31] See the reviews in Sharkey and Faber (2014); Nieuwenhuis and Hooimeijer (2016); Galster and Sharkey (2017).

[32] These clusters yield areas that typically range in population from one to two US census tracts. This is the closest we can come to controlling for unobserved school quality in our sample, as children in the same neighborhood are likely to attend the same school, at least at the primary level (grades 1-10). Moreover, neighborhood fixed effects will pick up any local attitudes towards school and "culture" of education, as well as neighborhood stability and socioeconomic status.

were enrolled in college by age 20, although note the trend for younger cohorts having superior educational attainments.[33] While there were slight variations depending upon birth cohort, there were considerable differences by gender. By age 21, on average, 76% of young men had graduated from secondary school and about 26% had enrolled in college by age 20. These rates were markedly higher for young women: 86% had graduated from secondary school by age 21 and nearly 53% were enrolled in college by age 20.[34]

As would be expected from the aforementioned dominance of homeownership in Norway, approximately 92% of the young adults in our sample resided in homes owned by their parents for their entire childhood; only 1% lived in rented housing throughout their childhood, and the remaining 7% belonged to families that lived in both rental and owned housing.[35] Normally such a paucity of variation in the variable of interest would prove problematic for identification. Fortunately, this proves untrue in our case because of our access to datasets with an unusually large number of observations.

Young adults who always lived in homes owned by their parents also had higher secondary school completion and college enrollment rates relative to those who had lived in rental housing during some period of childhood. Secondary school completion rates at age 21 were 82% for young adults who always resided in homes owned by their parents compared to 78% for those who always lived in rental housing, and 79% for those whose parents had mixed housing tenures (switcher families). College enrollment at age 20 was 39% for young adults always residing in homes owned by their parents, 35% for those who always lived in rental

[33] All outcomes are measured in October of the person's 20th or 21st calendar year, respectively (calendar year = birth year + 20, or 21).
[34] The large gender discrepancy in college enrollment rates at age 20 is partly due to military service, which is currently completed by approximately 20% of each male cohort and 6% of each female cohort. For our oldest cohorts, more males and fewer females served in the military.
[35] The two former groups are designated as non-switcher families and latter group as switcher families. In the non-switcher families, there is no inter-sibling difference in homeownership exposure during childhood, while for the switcher families there is difference in the homeownership exposure for at least one sibling pair.

housing, and 37% for those who had mixed housing tenures. Of course, the key question is whether these raw differences persist when we control for household, individual, housing type, neighborhood, and mobility characteristics.

Table 2 provides descriptive statistics for the covariates used in our empirical models, stratified by housing tenure. As compared to young adults who resided in homes owned by their parents throughout childhood, young adults who always lived in rental housing or resided in families with mixed housing tenures (switchers) had mothers who were younger, spent more time without both parents, were more likely to live with a step-parent at age 18, were less affluent, had higher mobility rates throughout childhood, and were less likely to live in a single-family home. None of these differences are surprising and reinforce the need to control for these observed systematic differences in households by tenure to obtain an unbiased estimate of homeownership effects.

[Table 2 about here]

Methodology

Model. As noted above, theory suggests that tenure, dwelling type, and neighborhood characteristics are three conceptually and empirically distinct sets of attributes intrinsically associated with the act of residential selection; the household jointly chooses to rent or buy a particular dwelling in a particular neighborhood. In turn, these three sets of attributes subsequently affect the household's residential stability in the future.[36] Finally, tenure, dwelling, neighborhood, and mobility across the life course affect youths' educational outcomes. In sum,

[36] This framing is supported by the bivariate correlations in our dataset showing that cumulative years of residing in a home owned by parents was only moderately correlated with not living in a single-family dwelling (-.07), ln neighborhood median income (-.05), moving once (-.14), and moving more than once (-.10).

we specify three treatment variables (tenure, dwelling, neighborhood) and one mediating variable (mobility).

Formally, our model of the probability of an educational outcome (*E*) observed during early adulthood for individual *i* reared in family *j* in neighborhood *n* can be expressed as follows:

$$pr(E_{ijn}) = \alpha + \beta O_i + \gamma C_i + \delta F_{ij} + øD_i + \varphi M_i + \zeta F_j + \mu_n + u_j + \varepsilon_i \qquad [1]$$

where:

$E_{ijn} = \begin{cases} \text{Completion of secondary education, or} \\ \text{Enrollment in post-secondary (college) education} \end{cases}$

O_i = observed exposure to homeownership during individual's childhood

C_i = observed characteristics during individual's childhood

F_{ij} = observed youth-specific family characteristics

D_i = observed exposure to type(s) of dwelling(s) during individual's childhood

M_i = observed residential mobility during individual's childhood

F_j = observed family characteristics that are constant across siblings

μ_n = unobserved neighborhood characteristics (tract cluster fixed-effects)

u_j = unobserved family characteristics that are constant across siblings

ε_i = a random error term

We specify [1] as a linear probability model because the oft-used logit model cannot be estimated with both family and neighborhood fixed effects.

Identification Strategy. Although we have interest in the dwelling and mobility dimensions of the housing career, our primary goal is to obtain an unbiased estimate of β, the average causal (treatment) effect of housing tenure on an educational outcome observed during early adulthood. Housing tenure is, however, suspected to be correlated with unobserved family-level characteristics u_j that affect both tenure choice and children's educational outcomes, for example parents' orderliness and self-discipline along with their parental ability, child rearing practices or educational aspirations for their children. To surmount this selection challenge threatening internal validity, we rely upon a sibling comparison (family fixed-effect) strategy that is well-known in the realm of measuring neighborhood effects on children (Aaronson, 1998; Plotnick and Hoffman, 1999; Vartanian and Walker-Buck, 2005) and subsidized housing (Anderson et al., 2018), but to our knowledge has not been applied to impacts of homeownership. By differencing across siblings in the same family, we remove all family characteristics that siblings share, both the observed in F_j and the unobserved in u_j, thus, in principle, eliminating any bias due to the correlation between unobserved family characteristics, tenure choice, and educational outcomes. We obtain inter-sibling differences in exposure to homeownership by the family moving and switching tenures during the siblings' childhoods.

There are several potential drawbacks in employing this identification strategy we must overcome. One is that identification is based on families with two or more children who experience differences in exposure to homeownership because they have changed tenure as part of a move.[37] Fortunately, with the large sample afforded by the Norwegian register data this requirement is easily met: We have 797 families with a total of 1,783 sample children in which there is inter-sibling difference in the time between the ages 4 and 18 spent in homes owned by their parent(s) for at least one sibling pair. The between-sibling variation in exposure to homeownership in this subsample is shown in Figure 2. We found that 9.7% of 1,199 sibling pairs in these switcher families experienced no difference in homeownership exposure. For

[37] Another shortcoming is that the family fixed effects strategy remedies the selection problem only as long as the endogenous unobserved family characteristics are constant across siblings. That is, we effectively ignore the possibility that families choose tenure based on their desire to foster the educational attainment of only one or some of their children, e.g., if one child is particularly gifted.

46.8% of the sibling pairs, the difference in homeownership exposure was two years or less. The difference in homeownership exposure was between 2 and 3 years for 22.5% of the sibling pairs; another 9.9% had a between 3 and 4-year difference in homeownership exposure. The remaining 10.7% of sibling pairs experienced differences in homeownership exposure of more than 4 years.

[Figure 2 about here]

Note that the average treatment effected identified via family fixed effects is estimated for the sub-sample selected into identification (Miller et.al, 2019). This yields a possible threat to external validity arising if true treatment effects differ between those selected into identification and others in the sample about which we may want to draw inference. This will be the case if the subsample that is employed for the identification of the causal effects systematically deviates from the sample at large, and if the causal effect of the variable of interest is heterogeneous across groups. However, in our case we find that our identifying subsample is not different from our overall sample, neither with respect to family size nor parental education levels, which were the two background variables that Miller et. al. (2019) emphasized.[38] Although there may be systematic differences between the identifying subsample and the sample at large along some other underlying dimension, this gives us confidence in our family fixed effect estimates of the effect on homeownership on offspring's educational outcomes.[39]

[38] We formally test for the number of siblings in the sample, the number of siblings overall, and education levels of mother and father in 2001, respectively, being different for families that do and do not change tenures while their children are ages 4-18. Neither test statistic is close to being statistically significant. The results of these tests are available upon request.

[39] Another potential source of bias inherent in the family-fixed effects approach is the automatic omission of any lone child from the identifying subsample. While that implies that our results cannot necessarily be extrapolated to children without siblings, we consider this a minor issue inasmuch as the share of lone children in the population birth cohorts we employ is only 4.8%.

Other Methodological Issues

As noted above, Newman and Holupka (2013) argue that two common shortcomings of the field are over-controlling and heterogeneous treatment effects. To explore over-controlling, we examine the impact on β when we exclude and then include our mobility mediator M_i in model [1]. With the mobility variable excluded, we can view β as an upper-bound estimate of the full (i.e., direct plus mediated) impact of homeownership; with it included, β can be interpreted as a lower-bound estimate (i.e., direct impact). Although this estimate is likely biased by over-controlling (Angrist and Pischke, 2009; Cinelli, Forgey and Pearl, 2020), we present it because it is conventional to do so in the literature. To explore heterogeneity, we stratify [1] by lower- and higher-income families differentiated at the national median, as well as other exploratory stratifications.

A final methodological issue acknowledges that there may be systematic differences between siblings besides their exposure to homeownership. We investigate this possibility by conducting balance tests to ascertain if our individual and household covariates are significant predictors of cumulative years of homeownership, mobility, and housing type once family fixed effects are controlled. The results presented in Appendix Table A.4 show that family and sibling-specific variables are not jointly statistically significant after controlling for the family fixed effects.

Results

Core Model

Parameter estimates for the linear probability models of secondary school completion and college enrollment are presented in Tables 3 and 4, respectively.[40] Beginning with the

[40] Note that sample sizes differ between Tables 3 and 4 because the outcome is measured at different ages. Given that our most recent educational outcome data are for October 2014, the cohort born in 1994 will not have had an educational outcome recorded for them during their 21st year. Also, note that selected missing data render a slightly smaller sample size in Table 4 than in Tables 1 or 2.

uncontrolled relationships between the cumulative years spent in a home owned by parent(s) and educational outcomes as shown in the first columns of these two tables, we find that each additional year of homeownership is associated with a .005 ($p < .01$) greater probability of a youth completing secondary school by their 21st calendar year and a (statistically insignificant) .003 greater probability of a youth enrolling in college by their 20th calendar year.

[Tables 3 and 4 here]

Of more interest is what occurs when we control for unobserved and observed family and parental characteristics, thereby obtaining an upper-bound estimate of the total effect of homeownership. Identifying effects only based on between-sibling variations (i.e., using family fixed effects) increases the magnitude of the estimated homeownership effect on both outcomes, but also increases the standard errors to the point where neither parameter is statistically significant (column 2 in Tables 3 and 4). The effects of introducing sibling and sibling-specific family characteristics as controls in our models are shown in the third columns of Tables 3 and 4. We note that the probabilities of completing secondary school and enrolling in college were greater for females in our sample, youth who were born earlier in the sibling sequence, and had younger mothers at time of birth. Additionally, youth from later birth cohorts had a higher probability of completing high school and enrolling in college. However, living with a stepparent at age 18 decreased the probability of completing secondary school while living with higher fractions of siblings decreased the probability of enrolling in college. We suspect that the lack of explanatory power of income and wealth variables is likely due to their insufficient variation among siblings, as opposed to their unimportance in shaping educational outcomes. An alternative interpretation is that any deviation from the long-term (permanent) household income does not affect youth's educational outcomes. More remarkable, however, is that with family controls both point estimates of homeownership effects more than double in magnitude and become statistically significant. Each additional year of living in a home owned by one's parent(s) is associated with an increased probability of a youth both completing secondary school and enrolling in college by .015. This unexpected result of family controls increasing the apparent impact of homeownership is driven by the introduction of the dummy variables representing the youth's birth order in the family.[41] Children always experiencing homeownership come from larger families (hence are later in the birth sequence, on average) and have older mothers (by almost two years, on average); see Table 2. Failure to control for these family factors that retard educational attainments in the Norwegian context leads to an apparent underestimation of the salutary impacts of homeownership.

Next, we turn to the results from adding dwelling type, mobility and neighborhood variables that arguably are endogenous to the choice of homeowner tenure; see columns 4-6 in Tables 3 and 4. Residing in a single-family dwelling appears to have no impact compared to residing in a different structure type. Greater residential instability is associated with a lower probability of completing secondary school, though it is unrelated to college enrollment. Compared to youth who never moved while growing up, those who moved twice or more were .068-.079 less likely to complete secondary school, a result consistent with a wide range of prior research cited above.[42] Census tract fixed effects as a set were statistically significant predictors of both educational outcomes, again consistent with most neighborhood effects literature cited above.

---

[41] A version of the models presented in Column 3 on Tables 3 and 4 without family controls produces similar results. We ran stepwise regression models entering the family and household control measures in individually to examine change in the homeownership coefficient. The main drivers of change in the homeownership coefficient are the parity dummy variables that indicate the youth's birth order with 4 reflecting 4^{th} or higher birth order. Further, the homeownership coefficient remains the same with or without the including of the family income variables in the model. As an additional check, we ran correlations for exposure to homeownership against income and wealth across all sibling pairs in the sample and found that these correlations were weak. These results remain the same when only the sibling pairs with a non-zero difference in homeownership exposure are included. These regression results are available upon request from the authors.

[42] We explored whether the impacts of mobility differed when households did not change tenure compared to when they changed from renting to owning, using interaction terms. We could not discern any statistically significant differences.

Of more interest to our core research question, the addition of housing type and mobility barely changes the point estimates of the homeownership effect for both outcomes, compared to the models with only family fixed effects and other family covariates. Both remain statistically significant at the $p < .05$ level. The addition of census tract fixed effects does not reduce the point estimate of this effect for secondary school completion, but slightly increases it in magnitude for college enrollment. These results are consistent with those of Holupka and Newman (2012), who concluded that neither residential stability nor neighborhood conditions were powerful mediators of homeownership effects on youth's cognitive development. Our estimated parameter indicates that, compared to an otherwise-comparable sibling experiencing identical residential contexts, a youth who lived one more year in a home owned by their parent(s) had a 1.4 percentage-point higher probability of completing high school and a 1.7 percentage-point higher probability of enrolling in college. Our findings suggest that it is homeownership *per se* that generates these external benefits to young adults, not dwelling type, mobility, or neighborhood characteristics. We cannot identify the source of this independent impact, but above offered several plausible mechanisms we will not repeat here. Regardless, we think that the magnitude of the measured effect is substantial, given that the means for our sample are: 82% completed high school by age 21, and 39% were enrolled in college by age 20. Youth raised in homes that were owned by their parents every year while they were between the ages of four through 18 would be predicted to have a .196 higher probability of completing secondary school by age 21 and a .238 higher probability of being enrolled in college by age 20, compared to other youth with identical family backgrounds and housing careers who always lived in rental housing.

We investigated the degree to which these apparent effects of homeownership varied by child developmental stage, given previous work on neighborhood effects demonstrating such variability (e.g., Sharkey and Faber, 2014; Chetty, Hendren and Katz, 2015; Galster and Santiago, 2017). We split the cumulative years in homeownership into two variables—ages 4 to

12 and 13-18—and found provocative differences; see Appendix Table A.5. Experiencing homeownership during ages 4 to 12 proved roughly twice as powerful a predictor of finishing high school by age 21 as experiencing it as an older youth, but exactly the opposite relationship emerged when considering enrolling in college by age 20.

Income-Stratified Model. Given prior research findings that both the direct and indirect (mediated) impacts of housing tenure on children's educational outcomes are likely heterogeneous across household groups categorized by socioeconomic status, race, or ethnicity (Green and White, 1997; Harkness and Newman, 2003; Holupka and Newman, 2012; Chen, 2013; Sharkey and Faber, 2014), we explore how core results might vary by household income. We cannot explore the racial-ethnic dimension with our dataset since more than 99% of the youth analyzed are born to native Norwegian parents.[43] Specifically, we re-estimate the fully controlled models shown in the last columns of Tables 3 and 4 for two sets of youth: those growing up in households with below- and above-national median incomes of households with children.[44] Results are presented in Table 5.

[Table 5 about here]

The first conclusion to draw from Table 5 is that, with the exception of gender, most of the previously significant individual and household predictors of educational outcomes have heterogeneous impacts across income groups, with secondary school completion of lower-income youth generally being affected more strongly. Moving twice or more during childhood

[43] We have only 225 non-native youth in our analysis sample.

[44] We determined a household's stratum by measuring total household income when the youngest child is age 18 and then comparing it with the median household income of all Norwegian households with children who are 18 years old that year. Chow tests revealed that for both outcomes we could not reject the null hypothesis that the estimated parameters were identical in the two income strata. Nevertheless, the different findings related to parental homeownership were sufficiently strong and interesting to warrant discussion here.

and being later in the birth order all evince more powerful negative effects on the probability of completing secondary school for lower-income youth, while higher-income youth are affected by mother's age and birth cohort. Coefficient estimates are more similar across income strata in the case of college enrollment. Lower income youth are, however, slightly more affected by birth cohort, while higher-income youth are slightly more affected by birth order.

The second conclusion is of more relevance to our research focus. Homeownership has a stronger impact on lower-income youth in the case of secondary school completion, but in the case of college enrollment homeownership has a stronger impact on higher-income youth. More specifically, the point estimates of the homeownership effects differ by a factor of greater than two between the two income strata for both outcomes, though in neither case are these inter-strata differences statistically significant. Our Norwegian findings that point estimates of homeownership effects on secondary school completion are larger for lower-income youth replicate the findings in the U.S. by Green and White (1997) and Harkness and Newman (2003), and in Sweden by Chen (2013). Our findings that homeownership effects on college enrollment are weaker (and not statistically significant) for lower-income youth are, however, opposite those of Harkness and Newman (2003). We think it likely that this difference can be traced to the fact that college tuition is free in Norway, whereas in the U.S. the parental wealth implicit in homeownership can prove crucial for lower-income students being able to afford college.

Further Heterogeneity Tests

Our explorations uncovered several other dimensions of heterogeneous impacts. When we stratified by educational attainment of the father (bachelor's degree or more vs. less) we found that only for youth in families with less-well educated fathers did homeownership matter, with statistically significant coefficients of .017 and .020 for high school completion and college enrollment, respectively; see Appendix Table A.6. Families with mothers with college degrees evinced the largest (statistically significant) homeownership coefficient .028 for college enrollment. Stratifications by family size indicated that homeownership only was significantly predictive for youth with only one sibling graduating from high school (coefficient of .017) and attending college (coefficient of .019), not for those with two or more siblings; see Appendix Table A.7. Among these families with only two children, only those with children born three or fewer years apart experienced any apparent benefit from homeownership in terms of enrolling in college (significant coefficient of .048); see Appendix Table A.8. The only families evincing significantly positive relationships between home-owning and youth's high school and college attainments were those in which mothers began childrearing after the sample median age of 25 (coefficients of .016 and .021, respectively), and only those with both male and female youth (coefficients of .020 and .022, respectively); (coefficients of .016 and .021, respectively); see Appendix Tables A.9 and A.10. Only families in which the first-born was male evinced a significantly positive relationship between home-owning and youth's high school completion (coefficient of .030); see Appendix Table A.11. We do not have ready explanations for these results, but they have not been reported in previous research and thus clearly require further investigation.[45]

Robustness Checks

We subjected our core model to four robustness checks. First, we altered the timing of when educational outcomes were measured between calendar years 19 and 23 and re-estimated the fully controlled linear probability model [1]. We began the alternatives for both

---

[45] We also estimated the male-oldest and female-oldest sample partition regressions including an interaction effect for own_cum48 and female. In none of the regressions is this interaction statistically significant. We also included this interaction term in the d_mixed sample where we partitioned by all-male/all-female/mixed gender and it is not statistically significant either. It appears that homeownership benefits all youth equally in households where the oldest sibling is a male and in the mixed-gender households, but for some reason not at all in the households where the oldest sibling is a female. In the all-male and all-female households, the sample sizes are very small however, and the lack of statistical significance may therefore also stem from low sample size and little across-sibling variation.

outcomes with calendar year 19 of each youth's life, since that is the earliest feasible year for a Norwegian student to graduate from secondary school in the college prep track (in June) and immediately enroll in college that year (in August). Analogously, we ended the alternatives with calendar year 23 because virtually all Norwegian students who ever earn a secondary school diploma do so by then and all who eventually enroll in college will have begun by then, typically after deferring admission while they complete their 12-month military service.[46] Results of this test are presented in Appendix Tables A.2. and A.3. In interpreting these trials, note that results would not be expected to be identical across all outcome age measures because we have progressively smaller sample sizes as we proceed through 21, 22 and 23 calendar years.[47] Appendix Table A.2. shows that the point estimate of cumulative years of homeownership is very small and statistically insignificant when secondary school completion is measured at age 19 or 20, but is stable in the range of .013 to .017 when it is measured at ages 21, 22 or 23. The point estimates for college enrollment in Appendix Table A.3 are more sensitive to age of measurement: by far the largest point estimates (.017 and .018) are found for ages 20 and 23, though the latter is not statistically significant at any conventional level. We think this unusual pattern can be attributed to differences in patterns of 12-month military service, for which all young Norwegians are required to register, regardless of gender.[48] Some who are keen to go to college immediately after graduation (i.e., age 20) can defer service until college completion. Others may "find themselves" as a result of military service and enroll in college after they are discharged (i.e., at age 23). In sum, we attribute the fragility of our results to when the outcome is measured to the idiosyncrasies of the educational and military service systems in Norway.

---

[46] Pushing measurement beyond age 23 is inappropriate because it will increasingly miss students who have completed their college educations.
[47] Given that our most recent educational outcome data are for October, 2014, the cohort born in 1994 will not have had an educational outcome recorded for them during their 21st year, for example.
[48] The share of males undertaking military service was 35% for the 1986 birth cohort, declining to about 20% for the 1988 birth cohort and staying there subsequently. The corresponding share for females was 2%, rising to 6% after 2015. This service does not necessarily follow the academic year, potentially delaying the pursuit of higher education by two years.

Second, we re-estimated a partially controlled version (i.e., omitting census tract fixed effects) of [1] as a logit model instead of a linear probability model.[49] The results of these model trials are presented in column (1) of Appendix Tables A.12 and A.13 and should be compared to those from the core models presented in the fifth columns of Tables 3 and 4. This test reveals that, compared to an otherwise-comparable sibling, a youth who lived one more year in a home owned by their parent(s) had 11% higher odds of completing secondary school and 9% higher odds of enrolling in college, though the former estimate is statistically significant only at $p<.10$. When we estimate this logit model for below-median income and above-median income samples, as shown in columns (2) and (3) in Appendix Tables A.12 and A.13, we qualitatively replicate the earlier findings from the linear probability model that the homeownership point estimates are slightly higher for the lower-income youth in the case of high school completion and vice versa in the case of college enrollment. For neither stratum are the estimates statistically significant, however.

Third, we explored the sensitivity of the estimate of β to excluding the 168 youth whose families switched from owning to renting and the 33 who made multiple tenure switches. Identifying solely on the rent-to-own subsample had virtually no effect on the size or precision of the parameter estimates for either outcome.

Finally, we altered the age at which the geographical fixed effects are defined and re-estimated the fully controlled linear probability model [1] for geographical fixed effects based on census tract residency for all ages between 13 and 18. The corresponding estimated coefficients on the cumulative homeownership variable for both outcomes are shown in Appendix Table A.14. We found that the estimated coefficients vary between 0.012 and 0.014 for the completion of secondary school at age 21, however, they are statistically significant at

---

[49] As mentioned above, Stata cannot estimate a logit model with both family and geographic area fixed effects.

the p<0.5 level only for age 14 and age 18, and significant only at p<0.1 for the remaining ages. For the enrollment in higher education by age 20, the coefficients vary between 0.015 and 0.017, however all homeownership exposure coefficients are significant at the p<0.05 level except when the geographical fixed effects are defined at age 18, in which case the coefficient is only significant at the p<0.1 level.

In conclusion, our core findings are reasonably robust. Though we think that the ages at which we measured our educational outcomes for our core model are the most defensible *a priori*, the results are somewhat sensitive to this choice, as would be expected given the interplay between the Norwegian educational and military service systems. Statistically significant homeownership impacts on secondary school completion are revealed regardless of whether completion is measured at age 21 or 22. The only time we observe a statistically significant homeownership impact, however, is in the case of college enrollment at age 20, though the point estimate is virtually the same at age 23. Estimates of a logit homeownership model without neighborhood fixed effects do not produce as precise estimates but reproduce qualitatively the results obtained for our linear probability models.

Caveats

Though our analysis has taken advantage of an unusually large and rich dataset that permits us to identify for the first time causal impacts of homeownership based on inter-sibling comparisons, there are several shortcomings that must be acknowledged. First, given our necessary reliance on decennial Norwegian censuses to identify housing tenure and structural characteristics, we confined our analysis to those households that moved no more than once a decade. This means that we probably do not fully capture the impacts of extreme residential instability on young adult educational outcomes. Second, to preserve sample sizes we chose not to measure tenure during the first 48 months of a youth's life. To the extent that this unobserved variable is not correlated with cumulative tenure observed during ages four through

18 years, measurement error will be introduced that erodes the precision of our estimates. Third, we have no way to measure several aspects of the residential environment that have been linked to young adult's educational outcomes, such as housing quality, interior healthiness or overcrowding, neighborhood pollution and violence, and school quality. To the extent that these omitted variables are correlated with tenure but not causally related to it, our estimates of the benefits of homeownership will be biased upward. Fourth, the paucity of youth from minority/immigrant ethnic groups in our analysis sample prevented us from exploring the potential heterogeneity of impacts in this realm, as has been observed in the United States (Harkness and Newman, 2003). Fifth, although we have intentionally employed a research design providing high internal validity, given the unsettled state of empirical research in the field, it potentially sacrifices external validity. We cannot be sure that our findings generalize beyond the set of relatively stable Norwegian households with two or more children born during the years 1986 through 1994. In particular, given the idiosyncrasies of the Norwegian housing and educational systems, our results may not be generalizable internationally. Noteworthy features on the Norwegian context are that: (1) the rental sector is not geared toward long-term renting, particularly for families, because of the limited selection of units and the lack of tenure security; (2) neighborhood deprivation and violence is neither extreme nor concentrated; (3) schools are centrally funded with a formula that compensates (to some degree) primary and secondary schools for socio-economic disadvantage of their student population, and (4) qualifying young adults have access to tuition-free college education. What these Norwegian-specific features imply for whether the observed educational impacts of tenure would apply to the U.S. context is ambiguous. Moving from rental to owned housing likely is associated with a greater improvement of dwelling physical size and quality but less change in neighborhood or school quality in Norway than it does in the United States. However, we must emphasize again our core finding that there appears to be something about homeownership per se that facilitates the educational attainment of children. Although we cannot identify which particular mechanism(s)

are at work, we have no reason to believe that the relevant socio-economic-psychological dynamics internal to the home-owning household are systematically different for other types of Norwegian families or between the two countries.

Conclusion

Norway, like the United States, prides itself on its high rate of homeownership and sees attaining this tenure as the apex of the life course. Whether such tenure conveys substantial external benefits that might justify the generous public subsidies lavished on this sector by both nations has been the subject of considerable debate. Unlike in the United States, where considerable, albeit inconsistent, research can be brought to bear on this topic, no previous scholarship has focused on the Norwegian case. Our research into the impact of homeownership on educational attainments of Norwegian students is not only unique in its national scope. Our unusually large dataset spanning the period 1990 to 2014 also facilitates the first application in this field of inter-sibling comparisons to identify causal impacts. Although this method gives us confidence in the internal validity of our analyses, it comes at a cost. We are constrained to analyze a low-mobility sample in which the between-siblings variation in the cumulative experience of homeownership probably is lower than in the general population. Nevertheless, we find a homeownership effect—and a potentially large one—on educational outcomes, independent of familial income, wealth and other household characteristics, dwelling type, mobility, and neighborhood context. However, it appears that this effect is not general across families; rather, it is strongest for those with lower incomes, less-educated fathers, and mothers who after age 25 first bear a son and then a daughter within a span of three years.

We think it appropriate to close with some reflections on the larger issue to which these results speak: housing career as an element of national economic development. Aggregate educational attainment is clearly a major contributor to a country's productivity, inventiveness, and quality of life. As noted in the introduction, there is now a substantial, international body of literature supporting the thesis that multiple dimensions of the residential environment children cumulatively experience—dwelling conditions, mobility, and neighborhood—independently and substantially influence their physical, mental and social development, health and well-being in ways that shape their human capital attainments. Assuming our findings have some generality, they not only provide further evidence on the power of those dimensions, but also suggest that homeownership is another key component of childhood housing career that is worthy of policymakers' attention.

Acknowledgements

This work has been funded by the Norwegian Research Council, grant no. 237028.

References

Aarland, K. and C.K. Reid. 2018. Homeownership and Residential Stability: Does Tenure Really Make a Difference? *International Journal of Housing Policy* 19: 1-27. doi:10.1080/19491247.2017.1397927

Aaronson, D. 1998. Using Sibling Data to Estimate the Impact of Neighborhoods on Children's Educational Outcomes. *Journal of Human Resources* 33: 915-946.

Aaronson, D. 2000. A Note of the Benefits of Homeownership. *Journal of Urban Economics* 47: 356-369.

Anderson, F., J. Haltiwanger, M. Kutzbach, G. Palloni, H. Pollakowski, and D. Weinberg. 2018. *Childhood Housing and Adult Earnings: A Between-Siblings Analysis of Housing Vouchers and Public Housing.* NBER working paper 22721. Cambridge, MA: National Bureau of Economic

Research. https://www.nber.org/papers/w22721

Angrist, J. and J.-S. Pischke. 2009. *Mostly Harmless Econometrics: An Empiricist's Guide.* Princeton: Princeton University Press.

Astone, N.M., and S. McLanahan. 1994. Family Structure, Residential Mobility and School Dropout: A Research Note. *Demography* 31: 575–584.

Barker, D. 2013. The Evidence Does Not Show That Homeownership Benefits Children. *Cityscape* 15: 231-234.

Barker, D. and E. Miller. 2009. Homeownership and Child Welfare. *Real Estate Economics* 37: 279–303.

Barth, E. and K. von Simson. 2013. Ulike veier gjennom videregående: Hva skjer de neste ti årene? [Different Paths through Upper Secondary Education: What Happens During the Next Ten Years?]. *Søkelys på Arbeidslivet* 30: 313-333.

Bengtsson, B., H. Ruonavaara, and J. Sørvoll. 2017. Home Ownership, Housing Policy and Path Dependence in Finland, Norway and Sweden. In *Housing Wealth and Welfare* by C. DeWilde and R. Ronald, Eds. (pp. 60-84). Cheltenham, UK: Edward Elgar Publishing.

Björklund, A. and K.G. Salvanes. 2011. Education and Family Background: Mechanisms and Policies. In *Handbook of the Economics of Education*. Vol. 3 (pp. 201-247). New York, NY: Elsevier.

Black, S.E., P.J. Devereux and K.G. Salvanes. 2005. Why the Apple Doesn’t Fall Far: Understanding Intergenerational Transmission of Human Capital. *The American Economic Review* 95: 437-449.

Boehm, T. P. and A. M. Schlottman. 1999. Does Home Ownership by Parents Have an Economic Impact on Their Children? *Journal of Housing Economics* 8: 217-232.

Bourassa, S., D. Haurin and M. Hoesli. 2016. What Affects Children’s Outcomes: House Characteristics or Homeownership? *Housing Studies* 31: 427-444.

Bronfenbrenner, U., and P. Morris.1998. The Ecology of Developmental Processes. In R. M. Lerner, Ed., *Handbook of Child Psychology: Vol. 1. Theoretical Models of Human Development*, *5th ed.* (pp. 993-1028). New York: Wiley.

Bunting, M. and G. H. Moshuus. 2017. Young Peoples’ Own Stories about Dropping Out in Norway: An Indirect Qualitative Approach. *Acta Didactica Norge* 11: 1-20.

Cairney, J. and M. Boyle, 2004. Home Ownership, Mortgages and Psychological Distress. *Housing Studies* 19: 161-174.

Chen, J. 2013. Housing Tenure, Residential Mobility and Adolescents’ Education Achievement: Evidence from Sweden. *Annals of Regional Science* 50: 275–294.

Chetty, R., N. Hendren, and L. Katz. 2015. The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment. *American Economic Review* 106: 855-902.

Chetty, R., and N. Hendren. 2018. The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects. *The Quarterly Journal of Economics* 133: 1107-1162.

Chyn, E. 2018. Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children. *American Economic Review* 108: 3028–3056.

Cinelli, C., A. Forney, and J. Pearl. 2020. A Crash Course in Good and Bad Controls. Los Angeles: UCLA Technical Report R493. Retrieved from https://ftp.cs.ucla.edu/pub/stat_ser/r493.pdf

Clair, A. 2018. Housing: An Under-explored Influence on Children's Well-Being and Becoming. *Child Indicators Research* 12: 609-626. Retrieved from https://doi.org/10.1007/s12187-018-9550-7.

Cordes, S., A. Schwartz, and L. Stiefel. 2019. The Effect of Residential Mobility on Student Performance: Evidence From New York City. *American Education Research Journal* 56 (4): 1380-1411.

Cox, K. 1982. Housing Tenure and Neighborhood Activism. *Urban Affairs Quarterly* 18:107-129.

Dietz, R. and D. Haurin. 2003. The Social and Private Micro-Level Consequences of Homeownership. *Journal of Urban Economics* 54: 401-450.

DiPasquale, D. and E. Glaeser. 1999. Incentives and Social Capital: Are Homeowners Better Citizens? *Journal of Urban Economics* 45: 354-384.

Dunn, J. 2000. Housing and Health Inequalities: Review and Prospects for Research. *Housing Studies* 15: 341-366.

Easterlow, D., S. Smith, and S. Mallinson. 2000. Housing for Health: The Role of Owner Occupation. *Housing Studies* 15: 367-386.

Eurostat. 2020 August 26. *Distribution of Population by Tenure Status, Type of Household and Income Group - EU-SILC Survey*. Retrieved at https://data.europa.eu/euodp/en/data/dataset/fAtiyJCKzLXUhNX3wZUw

Galster, G. 1983. Empirical Evidence on Cross-Tenure Differences in Home Maintenance and Conditions. *Land Economics* 59: 107-113.

Galster, G. 1987. *Homeowners and Neighborhood Reinvestment*. Durham, NC: Duke University Press.

Galster, G., D. Marcotte, M. Mandell, H. Wolman, and N. Augustine. 2007. The Impacts of Parental Homeownership on Children’s Outcomes during Early Adulthood. *Housing Policy Debate* 18: 785-827

Galster, G.C. and A. M. Santiago. 2017. Do Neighborhood Effects on Low-Income Minority Children Depend on Their Age? Evidence from a Public Housing Natural Experiment. *Housing Policy Debate* 27: 584-610.

Galster, G. C., A. M. Santiago, L. Stack, and J. Cutsinger. 2016. Neighborhood Effects on Secondary School Performance of Latino and African American Youth: Evidence from a Natural Experiment in Denver. *Journal of Urban Economics* 93: 30-48.

Galster, G. C., and P. Sharkey. 2017. Spatial Foundations of Inequality: An Empirical Overview and Conceptual Model. *RSF: The Russell Sage Journal of the Social Sciences* 3(2): 1-34.

Gasper J., S. DeLuca and A. Estación. 2009. Coming and Going: The Effects of Residential and School Mobility on Delinquency. *Social Science Research* 39:459–476.

Goux, D. and E. Maurin. 2005. The Effect of Overcrowded Housing on Children's Performance at School. *Journal of Public Economics* 89: 797–819.

Green, R. K. and M. J. White. 1997. Measuring the Benefits of Homeowning: Effects on Children. *Journal of Urban Economics* 41: 441-461.

Green, R. K., G. Painter, and M. J. White. 2012. *Measuring the Benefits of Homeowning: Effects on Children Redux*. Washington, DC: Research Institute for Housing America.

Grinstein-Weiss, M., C. Key, Y. Yeo, J. Yoo, K. Holub, A. Taylor and J. Tucker. 2012. Homeownership, Neighbourhood Characteristics and Children's Positive Behaviours among Low- and Moderate-Income Households. *Urban Studies* 49: 3545–3563.

Guldbrandsen, L.P. and V. Nordvik. 2007. Tilbudet av leide boliger. [The Supply of Rental Housing]. Oslo, NOR: NOVA-Report 6/07.

Harkness, J. M. and S. J. Newman. 2002. Homeownership for the Poor in Distressed Neighborhoods: Does It Make Sense? *Housing Policy Debate* 13: 597-630.

Harkness, J. M. and S. J. Newman. 2003. Differential Effects of Homeownership on Children from Higher- and Lower-Income Families, *Journal of Housing Research* 14:1-19.

Haurin, D. R., P. Hendershott, and D. Ling. 1988. Homeownership Rates of Married Couples: An Econometric Investigation. *Housing Finance Review* 7: 85-108.

Haurin, D. R., T. L. Parcel, and R. J. Haurin. 2002. Does Home Ownership Affect Child Outcomes? *Real Estate Economics* 30: 635-666.

Herbert, C. E. and E. S. Belsky. 2006. *The Homeownership Experiences of Low-Income and Minority Families: A Review and Synthesis of the Literature.* Cambridge, MA: Abt Associates.

Hilber, C. 2010. New Housing Supply and the Dilution of Social Capital. *Journal of Urban Economics* 67: 419–437.

Hoff, K. and A. Sen. 2005. Homeownership, Community Interactions, and Segregation. *American Economic Review* 95: 1167-1189.

Holupka, S. and S. J. Newman. 2012. The Effects of Homeownership on Children's Outcomes: Real Effects or Self-Selection? *Real Estate Economics* 4: 566–602.

Kristensen, P. and T. Bjerkedal. 2010. Educational Attainment of 25 year old Norwegians According to Birth Order and Gender. *Intelligence* 30: 123-136.

Laliberté, J-W. (forthcoming). Long-Term Contextual Effects in Education: Schools and Neighborhoods. *American Economic Journal: Economic Policy.* Retrieved from https://www.aeaweb.org/articles?id=10.1257/pol.20190257&&from=f

Lien, H-M., W-C. Wu, and C-C. Lin. 2008. New Evidence on the Link between Housing Environment and Children's Educational Attainments. *Journal of Urban Economics* 64: 408–421.

Loopoo, L. M. and A. S. London. 2016. Household Crowding during Childhood and Long-Term Education Outcomes. *Demography* 53: 699-721.

Markussen, E., Frøseth, M. W. and N. Sandberg. 2011. Reaching for the Unreachable: Identifying Factors Predicting Early School Leaving and Non-completion in Norwegian Upper Secondary Education. *Scandinavian Journal of Educational Research* 55: 225-253.

Miller, D.L., N. Shenhav and M.Z. Grosz. 2019. Selection into Identification in Fixed Effects Models, with Application to Head Start. NBER Working Paper Series, Working paper 26174. Retrieved from https://www.nber.org/papers/w26174

Mohanty, L. and L. Raut. 2009. Home Ownership and School Outcomes of Children: Evidence from the PSID Child Development Supplement, *American Journal of Economics and Sociology* 68: 465-489.

Newman, S. 2008. Does Housing Matter for Poor Families: A Critical Summary of Research and Issues Still to be Resolved. *Journal of Policy Analysis and Management*, 27: 895-925.

Newman, S. and S. Holupka. 2013. Looking Back to Move Forward in Homeownership Research. *Cityscape* 15: 235-246.

Nieuwenhuis, J. and P. Hooimeijer. 2016. The Association between Neighbourhoods and Educational Achievement: A Systematic Review and Meta-Analysis. *Journal of Housing and the Built Environment* 31: 321-347.

Nordvik, V. 2000. Tenure Flexibility and the Supply of Private Rental Housing. *Regional Science and Urban Economics* 30: 59-76.

Norwegian Directorate for Education and Training. 2017. Gjennomføring i videregående opplæring. [Completion of Upper Secondary Education] Retrieved from https://www.udir.no/tall-og-forskning/finn-forskning/tema/gjennomforing/

OECD. 2020. *Education at a Glance: OECD Indicators.* Retrieved from https://doi.org/10.1787/69096873-en

Parcel, T. L. and E. G. Menaghan. 1994. Early Parental Work, Family Social Capital, and Early Childhood Outcomes. *American Journal of Sociology* 99: 972-1009.

Pettit, B. and S. McLanahan. 2003. Residential Mobility and Children’s Social Capital: Evidence from an Experiment. *Social Science Quarterly* 84:632–649

Plotnick, R.D. and S. D. Hoffman. 1999. The Effect of Neighborhood Characteristics on Young Adult Outcomes: Alternative Estimates, *Social Science Quarterly* 80: 1-18

Prescott, E. and J. Vestbo. 1999. Socioeconomic Status and Chronic Obstructive Pulmonary Disease. *Thorax* 54: 737–741.

Retsinas, N. P. and E. S. Belsky, Eds. 2002. *Low Income Homeownership: Examining the Unexamined Goal.* Washington D.C.: Bookings Institution Press.

Retsinas, N. P. and E. S. Belsky. 2005. *Building Assets, Building Credit: Creating Wealth in Low-Income Communities*. Washington D.C.: Brookings Institution Press.

Rohe, W. and M. Stegman. 1994a. The Effects of Home Ownership on the Self-Esteem, Perceived Control, and Life Satisfaction of Low-Income People. *Journal of the American Planning Association* 60:173-184.

Rohe, W. and M. Stegman. 1994b. The Impact of Home Ownership on the Social and Political Involvement of Low-Income People. *Urban Affairs Quarterly* 30:152-172.

Rohe, W. and L. Stewart. 1996. Home ownership and Neighborhood Stability. *Housing Policy Debate* 7: 37-81.

Rohe, W., S. van Zandt and G. McCarthy, 2013. The Social Costs and Benefits of Homeownership. In Tighe, J. Rosie and Elizabeth J. Mueller, Eds., *The Affordable Housing Reader* (pp. 196-213). New York, NY: Routledge/Taylor and Francis.

Ross 2012. Social Interactions Within Cities: Neighborhood Environments and Peer Relationships. In Brooks, Nancy, Kieran Donaghy, and Gerrit-Jan Knaap, Eds., *The Oxford Handbook of Urban Economics and Planning* (pp. 203-229). Oxford and NY: Oxford University Press.

Rossi, P. H. and E. Weber. 1996. The Social Benefits of Homeownership: Empirical Evidence from National Surveys. *Housing Policy Debate* 7: 1-35.

Sandlie, H. C. and L. Gulbrandsen. 2017. The Social Home Ownership Model: The Case of Norway. *Critical Housing Analysis* 4:52-60. Retrieved from http://dx.doi.org/10.1360/23362839.2017.4.1.324

Santiago, A. M., A. R. Roberts, and E. Lee. 2014. Not Just Buying a Home: The Meaning of Home, Homeownership, and Neighborhood in Low-income Communities. In C. Renzetti, and R. Bergen (Eds.), *Understanding Diversity* (pp. 220-241). Boston: Allyn and Bacon.

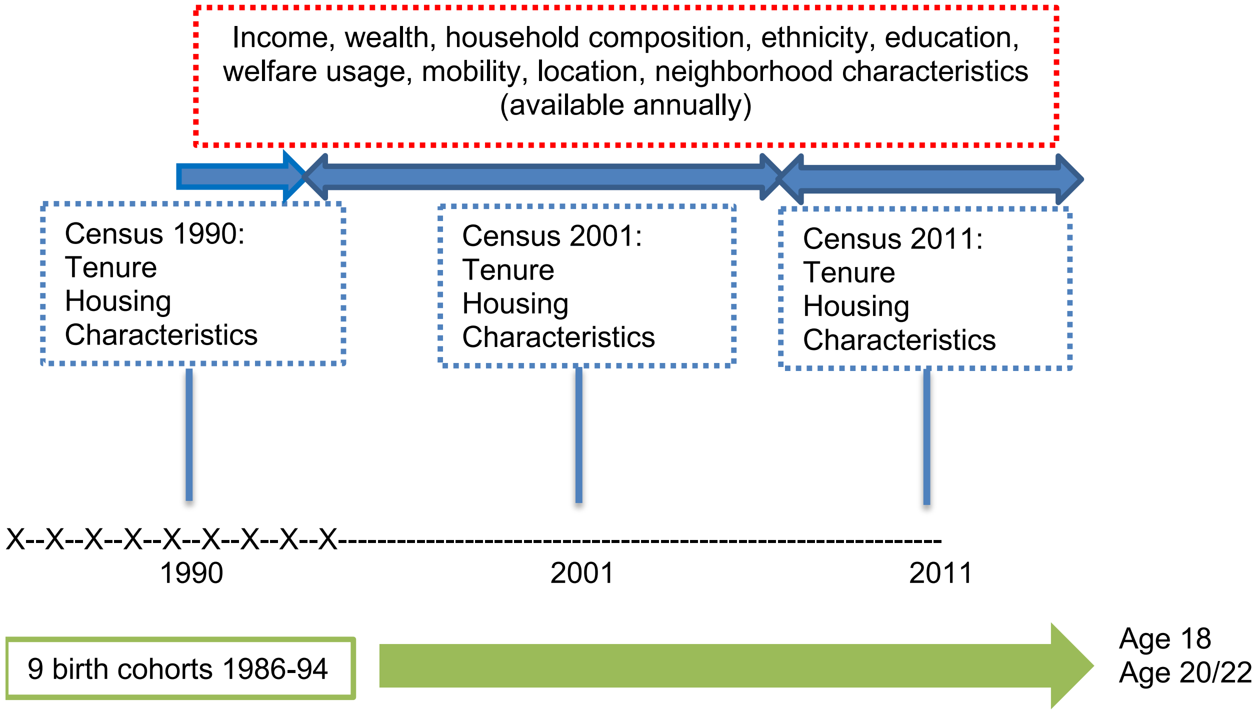
Sharkey, P. and J. Faber. 2014. Where, When, Why, and For Whom Do Residential Contexts Matter? Moving Away from the Dichotomous Understanding of Neighborhood Effects. *Annual Review of Sociology* 40: 559-79.

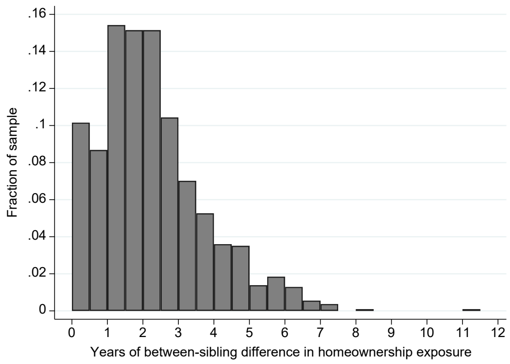
Statistics Norway. 2019. *Facts about Education in Norway 2017*. Statistics Norway, Division for Education Statistics. Retrieved from https://www.ssb.no/en/utdanning/artikler-og publikasjoner/_attachment/373651?_ts=16813a35da0

Vartanian, T.P. and P. Walker-Buck. 2005. Childhood and Adolescent Neighborhood Effects on Adult Income: Using Siblings to Examine Differences in OLS and Fixed Effect Models, *Social Service Review* 79: 60-94

Yun, L. and N. Evangelou (December 2016). *Social Benefits of Homeownership and Stable Housing.* Research Division, National Association of Realtors. Retrieved from: https://realtoru.edu/wp-content/uploads/2014/06/Homeownership-Stable-Housing.pdf

Figure 1. Data matching process for Norwegian census and register data





N=1,083 sibling pairs in 797 families (6.7% of sample)

Figure 2. Between-sibling difference in homeownership exposure between ages of 4 and 18

Table 1. Educational Outcomes by Birth Year, Gender, and Housing Tenure

	Birth cohort									
	1986	1987	1988	1989	1990	1991	1992	1993	1994	**Avr. all cohorts**
All youth (N=25,091 in 13,385 families)										
High school completion by age 21	82%	80%	81%	84%	81%	81%	81%	81%		81%
Postsecondary enrollment by age 20	39%	37%	37%	40%	39%	39%	39%	41%	42%	39%
Gender										
Male										
High school completion by age 21	78%	76%	76%	81%	77%	78%	78%	77%		76%
Postsecondary enrollment by age 20	26%	24%	25%	27%	28%	26%	26%	27%	27%	26%
Female										
High school completion by age 21	87%	85%	85%	87%	86%	86%	83%	86%		86%
College enrollment by age 20	54%	50%	51%	54%	52%	53%	53%	55%	57%	53%
Housing tenure										
Always in home owned by parents (23,087 children in 10,485 families)										
High school completion by age 21	83%	81%	81%	84%	81%	82%	81%	82%		82%
College enrollment by age 20	40%	36%	37%	40%	39%	39%	39%	41%	43%	39%
Always in rented home (221 children in 103 families)*										
High school completion by age 21	72%	85%	86%	69%	79%	88%	67%	75%		78%
College enrollment by age 20	36%	35%	52%	23%	29%	35%	42%	30%	43%	35%
Housing tenure switchers, inter-sibling difference in homeownership exposure (1,783 children in 797 families)										
High school completion by age 21	77%	79%	75%	84%	80%	76%	78%	81%		79%
College enrollment by age 20	36%	41%	30%	39%	39%	34%	37%	42%	36%	37%

NOTE: Switcher families are defined as those in which there is some inter-sibling difference in homeownership exposure. Not all children in these families are necessarily themselves switchers. Included are a handful (38 children in 19 families to be exact) that switch between tenures but where there is no inter-sibling difference.

Table 2 **Descriptive statistics by housing tenure**

	Full Sample (25,091 children; 11,385 families)				Always in Home Owned by Parents (23,087 children; 10,485 families)				Always in Rented Home* (221 children; 103 families)				Housing Tenure Switchers (1,783 children; 797 families)			
	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max	Mean	Std. Dev	Min	Max
Predictor measures																
Individual Characteristics																
Female	0.479	0.500	0	1	0.478	0.500	0	1	0.543	0.499	0	1	0.481	0.500	0	1
Birth order	2.04	0.90	1	4	2.05	0.91	1	4	2.03	0.89	1	4	1.96	0.87	1	4
Household Characteristics																
Mother's age at youth's birth	28.76	4.33	16.75	46.83	28.87	4.31	17.00	46.83	28.28	4.63	18.42	40.00	27.45	4.37	16.75	43.58
Number of years between ages 4-18 not residing with both parents	0.34	1.59	0	14.08	0.30	1.49	0	14.08	1.29	3.06	0	12.52	0.66	2.31	0	14.08
Living with stepparent at age 18	0.013	0.113	0	1	0.012	0.108	0	1	0.045	0.208	0	1	0.022	0.148	0	1
Number of persons living in the household when youth was age 18	1.84	0.90	0	11	1.83	0.89	0	11	1.84	0.84	0	4	1.97	0.99	0	9
Number of siblings living in the household when youth was age 18	1.64	0.96	0	11	1.63	0.95	0	11	1.62	0.80	0	4	1.73	1.03	0	9
Natural log of average median family income between ages 1-18	13.34	0.35	11.25	15.43	13.35	0.35	11.25	15.43	13.25	0.39	12.14	14.40	13.24	0.36	11.78	14.59
Natural log of average parental wealth between ages 7-18	13.82	0.75	9.05	19.03	13.84	0.74	10.38	19.03	13.29	1.28	9.77	16.98	13.68	0.81	9.05	17.53
Housing Characteristics																
Number of years between ages 4-18 not residing in single family housing	2.10	4.62	0	14.08	2.04	4.59	0	14.08	4.38	6.03	0	14.08	2.56	4.80	0	14.08
Mobility control variables																
Number of residential moves during childhood (omitted=none)																
1 move	0.286	0.452	0	1	0.245	0.430	0	1	0.199	0.400	0	1	0.818	0.386	0	1
2 or more times	0.029	0.168	0	1	0.021	0.144	0	1	0.217	0.413	0	1	0.104	0.306	0	1

NOTE: Switcher families are defined as the families in which there is some inter-sibling difference in homeownership exposure. Not all children in these families are necessarily themselves switchers.

* Included are a handful (38 children in 19 families to be exact) that switch between tenures but where there is no inter-sibling difference.

Table 3 High school completion by age 21

	Homeownership only		Family fixed effects		Household controls		Housing controls		Mobility controls		Neighborhood fixed effects	
	(1)		(2)		(3)		(4)		(5)		(6)	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Homeownership												
Number of years between ages 4-18 residing in home owned by parents	0.005 ***	(3.148)	0.008	(1.248)	0.015 **	(2.199)	0.015 **	(2.189)	0.014 **	(2.068)	0.014 **	(2.042)
Individual Characteristics												
Female (omitted=male)					0.085 ***	(13.386)	0.085 ***	(13.382)	0.085 ***	(13.383)	0.086 ***	(13.351)
Birth order (omitted=first born)												
Second born					-0.054 ***	(-3.365)	-0.054 ***	(-3.376)	-0.054 ***	(-3.399)	-0.056 ***	(-3.502)
Third born					-0.083 ***	(-2.762)	-0.083 ***	(-2.770)	-0.084 ***	(-2.798)	-0.088 ***	(-2.902)
Fourth or higher born					-0.130 ***	(-2.854)	-0.130 ***	(-2.859)	-0.131 ***	(-2.886)	-0.136 ***	(-2.955)
Birth year cohort (omitted=1986)												
1987					-0.005	(-0.218)	-0.005	(-0.214)	-0.004	(-0.173)	-0.002	(-0.102)
1988					0.036	(1.186)	0.036	(1.189)	0.038	(1.251)	0.040	(1.305)
1989					0.095 **	(2.372)	0.095 **	(2.373)	0.098 ***	(2.435)	0.097 **	(2.414)
1990					0.093 *	(1.840)	0.093 *	(1.842)	0.096 *	(1.908)	0.097 *	(1.910)
1991					0.121 **	(1.964)	0.121 **	(1.965)	0.124 **	(2.013)	0.124 **	(1.993)
1992					0.163 **	(2.235)	0.163 **	(2.236)	0.166 **	(2.277)	0.167 **	(2.267)
1993					0.170 **	(2.017)	0.171 **	(2.020)	0.173 **	(2.054)	0.173 **	(2.033)
Household Characteristics												
Mother's age at youth's birth					-0.021 *	(-1.751)	-0.021 *	(-1.751)	-0.021 *	(-1.791)	-0.020 *	(-1.711)
Number of years from 4-18 youth not residing with both parents					0.003	(0.557)	0.004	(0.581)	0.005	(0.758)	0.005	(0.815)
Living with stepparent at age 18					-0.130 ***	(-2.693)	-0.130 ***	(-2.694)	-0.124 ***	(-2.572)	-0.113 **	(-2.235)
Number of persons in household under 18 years old when youth was age 18					-0.002	(-0.168)	-0.002	(-0.169)	-0.003	(-0.177)	-0.005	(-0.346)
Number of siblings (full, half, step) in same household when youth was age 18					0.009	(1.087)	0.009	(1.084)	0.009	(1.014)	0.009	(1.060)
Natural log of average median family income between ages 1-18					0.054	(1.323)	0.054	(1.317)	0.054	(1.317)	0.034	(0.825)
parental wealth between ages 7-18					-0.011	(-0.407)	-0.011	(-0.424)	-0.012	(-0.439)	-0.009	(-0.346)
Housing Characteristics												
Number of years between ages 4-18 not residing in single family housing							-0.001	(-0.477)	-0.001	(-0.428)	-0.001	(-0.224)
Mobility Characteristics												
Number of residential moves during childhood (omitted=none)												
1 move									-0.012	(-0.728)	-0.013	(-0.803)
2 or more moves									-0.068 **	(-2.027)	-0.079 **	(-2.303)
Number of observations	23,206		23,206		23,206		23,206		23,206		23,206	
Number of families			11,382		11,382		11,382		11,382		11,382	
rho			0.438		0.472		0.472		0.473		0.555	
sigma			0.462		0.472		0.472		0.473		0.515	
sigma_e			0.347		0.343		0.343		0.343		0.343	
sigma_u			0.306		0.324		0.324		0.325		0.383	
R^2 (between)			0.001		0.000		0.000		0.000		0.000	
R^2 (within)			0.000		0.021		0.021		0.022		0.034	
R^2 (overall)			0.000		0.001		0.001		0.001		0.001	

* p < .1; ** p < .05; *** p < .01 NOTE: Family fixed-effects are present in all models except Column 1.

Table 4 **Enrollment in post-secondary (college) education by age 20**

	(1) Homeownership only		(2) Family fixed effects		(3) Household controls		(4) Housing controls		(5) Mobility controls		(6) Neighborhood fixed effects	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Homeownership												
Number of years between ages 4-18 residing in home owned by parents	0.003	(1.492)	0.008	(1.045)	0.015 **	(2.045)	0.015 **	(2.070)	0.015 **	(2.054)	0.017 **	(2.245)
Individual Characteristics												
Female (omitted=male)					0.268	(37.907)	0.268	(37.912)	0.268	(37.920)	0.267	(37.459)
Birth order (omitted=firstborn)												
Second born					-0.018	(-1.073)	-0.018	(-1.046)	-0.018	(-1.064)	-0.018	(-1.015)
Third born					-0.025	(-0.794)	-0.025	(-0.772)	-0.025	(-0.779)	-0.023	(-0.710)
Fourth or higher born					-0.046	(-0.948)	-0.045	(-0.936)	-0.046	(-0.945)	-0.044	(-0.900)
Birth year cohort (omitted=1986)												
1987					-0.032	(-1.290)	-0.033	(-1.300)	-0.032	(-1.281)	-0.033	(-1.305)
1988					0.030	(0.886)	0.029	(0.879)	0.031	(0.915)	0.029	(0.849)
1989					0.086	(1.950)	0.086	(1.947)	0.088	(1.984)	0.086	(1.935)
1990					0.121 **	(2.176)	0.121 **	(2.171)	0.123 **	(2.211)	0.124 **	(2.211)
1991					0.175	(2.577)	0.175 **	(2.573)	0.177 **	(2.607)	0.176 **	(2.564)
1992					0.217	(2.695)	0.217	(2.693)	0.219	(2.716)	0.219	(2.694)
1993					0.275	(2.945)	0.274	(2.939)	0.276	(2.955)	0.276	(2.936)
1994					0.331	(3.128)	0.331	(3.124)	0.332	(3.137)	0.333	(3.120)
Household Characteristics												
Mother's age at youth's birth					-0.037	(-2.849)	-0.037	(-2.848)	-0.037	(-2.873)	-0.037	(-2.809)
Number of years from 4-18 youth not residing with both parents					0.001	(0.159)	0.001	(0.102)	0.001	(0.188)	0.003	(0.463)
Living with stepparent at age 18					-0.025	(-0.495)	-0.025	(-0.478)	-0.021	(-0.417)	-0.010	(-0.187)
Number of persons in the household when youth was age 18					0.048	(3.116)	0.048	(3.118)	0.048	(3.120)	0.051	(3.209)
Number of siblings in the household when youth was age 18					-0.031	(-3.366)	-0.031	(-3.351)	-0.032	(-3.395)	-0.031	(-3.287)
Natural log of average median family income between ages 1-18					0.025	(0.578)	0.026	(0.594)	0.024	(0.559)	0.014	(0.324)
parental wealth between ages 7-18					0.001	(0.048)	0.003	(0.095)	0.002	(0.084)	0.000	(-0.017)
Housing Characteristics												
Number of years between ages 4-18 not residing in single family housing							0.003	(1.040)	0.003	(1.053)	0.003	(1.192)
Mobility Characteristics												
Number of residential moves during childhood (omitted=none)												
1 move									-0.020	(-1.171)	-0.019	(-1.144)
2 or more moves									-0.018	(-0.495)	-0.010	(-0.274)
Number of observations	25087		25087		25087		25087		25087		25087	
Number of families			11385		11385		11385		11385		11385	
rho			0.430		0.506		0.505		0.506		0.604	
sigma			0.570		0.582		0.581		0.582		0.650	
sigma_e			0.431		0.409		0.409		0.409		0.409	
sigma_u			0.374		0.413		0.413		0.414		0.505	
R^2 (between)			0.000		0.002		0.002		0.002		0.001	
R^2 (within)			0.000		0.100		0.100		0.100		0.110	
R^2 (overall)			0.000		0.014		0.015		0.014		0.006	

* $p < 0.05$; ** $p < 0.01$; $p < 0.001$ NOTE: Family fixed effects present in all models but the one in Column (1).

Table 5 Completion of secondary (high) school and enrollment in post-secondary (college) education stratified by income

	Secondary (high) school completion by age 21				Enrollment in post-secondary education by age 20			
	Below median family income		Above median family income		Below median family income		Above median family income	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Homeownership								
Number of years between ages 4-18 residing in home owned by parents	0.019 *	(1.752)	0.009	(1.018)	0.011	(1.058)	0.022 **	(1.992)
Individual Characteristics								
Female (omitted=male)	0.092 ***	(8.127)	0.082 ***	(10.587)	0.267	(24.052)	0.267	(28.834)
Birth order (omitted=first born)								
Second born	-0.108 ***	(-4.110)	-0.019	(-0.925)	-0.038	(-1.540)	0.009	(0.373)
Third born	-0.184 ***	(-3.774)	-0.018	(-0.445)	-0.054	(-1.170)	0.023	(0.509)
Fourth or higher born	-0.260 ***	(-3.521)	-0.045	(-0.744)	-0.087	(-1.246)	0.018	(0.266)
Birth year cohort (omitted=1986)								
1987	0.011	(0.276)	-0.015	(-0.518)	0.022	(0.578)	-0.084 **	(-2.423)
1988	0.025	(0.468)	0.041	(1.100)	0.095	(1.843)	-0.030	(-0.659)
1989	0.065	(0.914)	0.109 **	(2.214)	0.118	(1.720)	0.046	(0.784)
1990	0.060	(0.661)	0.111 *	(1.803)	0.179 **	(2.047)	0.070	(0.950)
1991	0.036	(0.328)	0.165 **	(2.211)	0.228 **	(2.138)	0.119	(1.323)
1992	0.087	(0.666)	0.203 **	(2.297)	0.255 **	(2.015)	0.172	(1.611)
1993	0.052	(0.343)	0.230 **	(2.256)	0.322 **	(2.190)	0.220	(1.789)
1994					0.395 **	(2.367)	0.268	(1.917)
Household Characteristics								
Mother's age at youth's birth	0.013	(0.603)	-0.041 ***	(-2.867)	-0.038	(-1.857)	-0.036 **	(-2.126)
Number of years from 4-18 youth not residing with both parents	0.005	(0.648)	0.002	(0.104)	0.000	(0.019)	-0.008	(-0.455)
Living with stepparent at age 18	-0.198 ***	(-2.725)	-0.008	(-0.096)	-0.038	(-0.548)	0.054	(0.549)
Number of persons in household under 18 years old when youth was age 18	-0.022	(-0.939)	0.007	(0.371)	0.012	(0.546)	0.089	(3.914)
Number of siblings (full, half, step) in same household when youth was age18	0.028 *	(1.910)	-0.004	(-0.398)	-0.033 **	(-2.382)	-0.028 **	(-2.221)
Natural log of average median family income between ages 1-18	0.027	(0.375)	0.038	(0.726)	0.025	(0.390)	0.050	(0.809)
Natural log of average median parental wealth between ages 7-18	-0.047	(-0.923)	0.016	(0.482)	-0.024	(-0.532)	0.025	(0.677)
Housing Characteristics								
Number of years between ages 4-18 not residing in single family housing	0.001	(0.301)	-0.002	(-0.557)	0.004	(0.858)	0.003	(0.795)
Mobility Characteristics								
Number of residential moves during childhood (omitted=none)								
1 move	-0.034	(-1.197)	0.000	(0.017)	-0.011	(-0.426)	-0.027	(-1.203)
2 or more moves	-0.106 *	(-1.916)	-0.062	(-1.399)	-0.026	(-0.498)	0.002	(0.035)
Number of observations	8671		14535		9379		15708	
Number of families	4257		7125		4258		7127	
rho	0.497		0.568		0.574		0.540	
sigma	0.526		0.493		0.600		0.619	
sigma_e	0.373		0.324		0.392		0.420	
sigma_u	0.371		0.372		0.455		0.455	
R^2 (between)	0.003		0.001		0.000		0.002	
R^2 (within)	0.043		0.032		0.121		0.108	
R^2 (overall)	0.007		0.000		0.008		0.011	

* p < 0.1; ** p < 0.05; *** p < 0.01

NOTE: OLS models include family and neighborhood fixed effects.

Appendix Table A-1 **Descriptive statistics for educational outcomes by age***

	Observations	Mean	Std. Dev.	Min	Max
Secondary (high) school completion					
By age 19	25,053	0.566	0.496	0	1
By age 20	25,055	0.720	0.448	0	1
By age 21	23,206	0.815	0.388	0	1
By age 22	21,000	0.847	0.360	0	1
By age 23	18,416	0.863	0.344	0	1
Enrollment in college					
By age 19	25,087	0.198	0.399	0	1
By age 20	25,087	0.391	0.488	0	1
By age 21	23,214	0.478	0.500	0	1
By age 22	21,003	0.519	0.500	0	1
By age 23	18,417	0.543	0.498	0	1

* For secondary school completion, on-time graduation is by June in the 21st calendar year. For enrollment in college, it is by August in the 20th calendar year. The upper half of the table shows the proportion of youth completing secondary school by different ages while the bottom half of the table shows the proportion of youth enrolling in college by different ages.

Appendix Table A-2. Age robustness check for high school completion

	High school completion by age:				
	19	20	21	22	23
Number of years between ages 4-18 residing in home owned by	-0.002	0.005	0.014 **	0.017 **	0.013
Youth Characteristics					
Female (omitted=male)	0.252 ***	0.178 ***	0.086 ***	0.057 ***	0.055 ***
Sibling parity (omitted=first born)					
Second born	-0.048 ***	-0.036 **	-0.056 ***	-0.055 ***	-0.063 ***
Third born	-0.043	-0.041	-0.088 ***	-0.086 ***	-0.101 ***
Fourth or higher born	-0.043	-0.082 *	-0.136 ***	-0.133 ***	-0.159 ***
Household Characteristics					
Mother's age at youth's birth	-0.063 ***	-0.038 ***	-0.020 *	-0.021 *	-0.012
Number of years from 4-18 youth was not residing with both parents	0.007	0.002	0.005	0.000	0.000
Living with a stepparent at age 18	-0.054	-0.069	-0.113 **	-0.063	-0.071
Number of persons residing in household when youth was age 18	0.015	0.008	-0.005	-0.005	-0.016
Number of siblings (full, half, step) in same household when youth was age 18	-0.021 **	-0.003	0.009	0.010	0.009
Natural log of average median family income between ages 1-18	0.020	0.048	0.034	0.019	-0.021
Natural log of average median parental wealth between ages 7-18	-0.018	-0.029	-0.009	-0.024	-0.035
Housing Characteristics					
Number of years between ages 4-18 not residing in single family housing	-0.003	-0.002	-0.001	0.002	0.005
Number of residential moves during childhood (omitted=none)					
1 move	-0.033 *	-0.028 *	-0.013	-0.012	-0.032
2 or more moves	-0.036	-0.083 **	-0.079 **	-0.078 **	-0.084 *
Observations	25,053	25,055	23,206	21,000	18,416
Groups	11,385	11,385	11,382	11,356	11,124
Rho	0.679	0.621	0.555	0.579	0.568
Sigma	0.734	0.642	0.515	0.49	0.469
Sigma_e	0.416	0.395	0.343	0.318	0.308
Sigma_u	0.605	0.506	0.383	0.373	0.353
R^2 between model	0.002	0	0	0	0
R^2 within model	0.102	0.066	0.034	0.027	0.027
R^2 overall model	0	0.001	0.001	0	0

Legend: * p<.1; ** p<.05; *** p<.01

Table A-3. Age robustness check for college enrollment

	College enrollment by age:				
	19	20	21	22	23
Number of years between ages 4-18 residing in home owned by parents	-0.000	0.017 **	0.009	0.010	0.018
Youth Characteristics					
Female (omitted=male)	0.157 ***	0.267 ***	0.268 ***	0.254 ***	0.240 ***
Birth order (omitted=firstborn)					
Second born	-0.030 **	-0.018	-0.001	0.002	-0.007
Third born	-0.032	-0.023	0.034	0.034	0.011
Fourth or higher born	-0.031	-0.044	0.039	0.046	0.014
Household Characteristics					
Mother's age at youth's birth	-0.020 *	-0.037 ***	-0.029 **	-0.032 **	-0.054 ***
Number of years from 4-18 youth not residing with both parents	-0.010 *	0.003	0.001	-0.011	-0.006
Living with a stepparent at age 18	0.028	-0.010	-0.033	-0.007	-0.021
Number of persons in household when youth was age 18	0.019	0.051 ***	0.048 ***	0.037 *	0.024
Number of siblings (full, half, step) in same household when youth was age18	-0.005	-0.031 ***	-0.025 **	-0.032 ***	-0.041 ***
Natural log of average median family income between ages 1-18	-0.028	0.014	-0.064	-0.068	-0.132 **
Natural log of average median parental wealth between ages 7-18	0.025	0.000	0.000	0.002	-0.045
Housing Characteristics					
Number of years between ages 4-18 not residing in single family housing	-0.002	0.003	0.000	0.004	0.000
Mobility Characteristics					
Number of residential moves during childhood (omitted=none)					
1 move	-0.022	-0.019	-0.035 *	-0.046 *	-0.061 *
2 or more moves	0.052	-0.010	-0.015	-0.064	-0.072
Observations	25,087	25,087	23,214	21,003	18,417
Groups	11,385	11,385	11,382	11,356	11,124
Rho	0.648	0.604	0.588	0.61	0.672
Sigma	0.601	0.650	0.639	0.640	0.687
Sigma_e	0.356	0.409	0.410	0.400	0.393
Sigma_u	0.484	0.505	0.490	0.500	0.563
R^2 between model	0.000	0.001	0.000	0.000	0.007
R^2 within model	0.065	0.110	0.108	0.101	0.098
R^2 overall model	0.002	0.006	0.004	0.001	0.002

Legend: * p<.1; ** p<.05; *** p<.01

Appendix Table A.4. Balance tests

	(1) Number of years between ages 4-18 residing in home owned by parents		(2) Number of years not residing in single family home		(3) Moved once		(4) Moved multiple times	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Individual Characteristics								
Female (omitted=male)	-0.015	(-1.820)	-0.012	(-0.530)	0.006	(1.450)	-0.001	(-0.780)
Birth order (omitted=firstborn)								
Second born	0.062 **	(3.100)	-0.163 **	(-3.040)	-0.015	(-1.570)	-0.001	(-0.230)
Third born	0.079 *	(2.120)	-0.244 *	(-2.420)	-0.007	(-0.360)	-0.005	(-0.590)
Fourth or higher born	0.121 *	(2.140)	-0.210	(-1.370)	-0.017	(-0.620)	-0.006	(-0.510)
Birth year cohort (omitted=1986)								
1987	0.006	(0.200)	0.083	(1.050)	0.016	(1.140)	0.008	(1.250)
1988	0.013	(0.330)	0.085	(0.810)	0.048 *	(2.560)	0.016	(1.830)
1989	0.015	(0.290)	0.052	(0.370)	0.063 *	(2.530)	0.027 *	(2.280)
1990	0.037	(0.570)	0.106	(0.600)	0.089 **	(2.850)	0.034 *	(2.340)
1991	0.024	(0.310)	0.095	(0.440)	0.095 *	(2.490)	0.029	(1.600)
1992	0.045	(0.470)	0.058	(0.230)	0.062	(1.380)	0.036	(1.710)
1993	-0.024	(-0.220)	0.194	(0.660)	0.044	(0.850)	0.038	(1.540)
1994	0.021	(0.170)	0.138	(0.410)	0.037	(0.630)	0.038	(1.360)
Household Characteristics								
Mother's age at youth's birth	0.002	(0.110)	-0.004	(-0.090)	-0.013	(-1.770)	-0.005	(-1.330)
Number of years from 4-18 youth not residing with both parents	0.000	(0.060)	0.125 ***	(6.320)	0.018 ***	(5.120)	0.011 ***	(6.680)
Living with stepparent at age 18	-0.050	(-0.830)	-0.305	(-1.880)	0.095 ***	(3.310)	0.069 ***	(5.060)
Number of persons living in the household when youth was age 18	0.014	(0.790)	-0.011	(-0.230)	-0.001	(-0.140)	0.003	(0.800)
Number of siblings living in the household when youth was age 18	0.007	(0.630)	-0.050	(-1.710)	-0.018 ***	(-3.430)	-0.006 *	(-2.300)
Natural log of average median family income between ages 1-18	0.245 ***	(4.830)	-0.263	(-1.930)	-0.072 **	(-2.990)	-0.007	(-0.660)
Natural log of average median parental wealth between ages 7-18	0.180 ***	(5.480)	-0.474 ***	(-5.370)	-0.011	(-0.710)	-0.006	(-0.840)
Number of observations	25091		25091		25091		25091	
Number of families	11385		11385		11385		11385	
rho	0.923		0.925		0.772		0.651	
sigma_e	1.664		4.548		0.421		0.148	
sigma_u	1.664		4.548		0.421		0.148	
R^2 (between)	0.013		0.013		0.005		0.027	
R^2 (within)	0.038		0.018		0.032		0.011	
R^2 (overall)	0.014		0.013		0.006		0.022	
F stat	26.24		25.84		7.12		3.95	
Prob > F	0.000		0.000		0.000		0.000	

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Appendix Table A.5 **Educational outcomes by timing of homeownership exposure during childhood**

	(1) High school completion by age 21		(2) College enrollment by age 20	
	Coefficient	t-stat	Coefficient	t-stat
Homeownership				
Number of years between ages 4-12 residing in home owned by parents	0.014	(1.821)	0.015	(1.790)
Number of years between ages 13-18 residing in home owned by parents	0.016	(0.905)	0.027	(1.407)
Number of youth	23206		25087	
Number of families	11382		11385	

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note: All OLS models include all individual and household controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A.6 **Educational outcomes stratified by parental educational attainment**

	(1)		(2)		(3)		(4)	
	Father's educational attainment				Mother's educational attainment			
	High school or lower		Bachelor's or higher degree		High school or lower		Bachelor's or higher degree	
	Coefficient	t-stat	t	t-stat	Coefficient	t-stat	Coefficient	t-stat
Educational Outcomes								
High school completion by age 21	0.017	(1.898)	0.010	(0.922)	0.014	(1.496)	0.014	(1.381)
Enrollment in post-secondary education by age 20	0.020 *	(2.296)	0.011	(0.707)	0.011	(1.249)	0.028 *	(2.146)
Number of observations (completion)	16465		6741		14819		8387	
Number of families (completion)	8124		3258		7304		4078	
Number of observations (enrollment)	17851		7236		16001		9086	
Number of families (enrollment)	8124		3261		7304		4081	

* p < 0.05; ** p < 0.01; *** p < 0.001

NOTE: Coefficient presented is on number of years of homeownership exposure between ages 4 and 18.

All OLS models include all individual and household controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A-7. **Stratification by number of siblings in family**

	(1) Two siblings		(2) Three or more siblings	
	Coefficient	t-stat	Coefficient	t-stat
Outcome				
High school completion by age 21	0.017 *	(1.999)	0.008	(0.651)
Enrollment in post-secondary education by age 20	0.019 *	(2.114)	0.090	(0.676)
Number of children (completion)	17361		5845	
Number of families (completion)	9281		2101	
Number of children (enrollment)	18564		6523	
Number of families (enrollment)	9283		2102	

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

NOTE: Coefficient presented is on number of years of homeownership exposure between age 4 and 18.

NOTE: All OLS models include all individual and household controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A.8 **Stratifications by age gap between siblings**

	Families with only two children				First two children born to mother			
	(1)		(2)		(3)		(4)	
	Age gap 36 months or less		Age gap more than 36 months		Age gap 36 months or less		Age gap more than 36 months	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Outcome								
High school completion by age 21	0.021	(1.523)	0.014	(1.356)	0.021	(1.433)	0.023	(1.766)
Enrollment in post-secondary education by age 20	0.048 **	(2.971)	0.060	(0.561)	0.048 **	(2.741)	0.009	(0.634)
Number of children (completion)	9076		8285		7357		6362	
Number of families (completion)	4742		4539		4316		4162	
Number of children (enrollment)	9487		9077		7707		6869	
Number of families (enrollment)	4744		4539		4318		4163	

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

NOTE: Coefficient presented is on number of years of homeownership exposure during childhood.
NOTE: All OLS models include individual and family controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A.9 **Stratification by mother's age at first and last birth**

	Mother's age at first birth				Mother's age at last birth			
	(1)		(2)		(3)		(4)	
	25 years of age or younger		Older than 25 years		32 years of age or younger		Older than 32 years	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Outcome								
High school completion by age 21	0.013	(1.302)	0.016	(1.665)	0.016	-1.661	0.012	-1.182
Enrollment in post-secondary education by age 20	0.014	(1.436)	0.021	(1.736)	0.019 *	-1.99	0.013	-1.09
Number of children (completion)	11341		11865		12081		11125	
Number of families (completion)	5523		5859		5919		5463	
Number of children (enrollment)	12238		12849		12916		12171	
Number of families (enrollment)	5523		5862		5921		5464	

* p < 0.05; ** p < 0.01; *** p < 0.001

NOTE: Coefficient presented is on number of years of homeownership exposure.

Note: All OLS models include all individual and family controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A.10 **Stratification by gender uniformity of siblings for all siblings by mother and only full siblings**

Table 1

	All siblings by mother						Only full siblings					
	(1)		(2)		(3)		(4)		(2)		(3)	
	All Male		All Female		Mixed Gender		All Male		All Female		Mixed	
	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat	Coefficient	t-stat
Outcome												
High school completion by age 21	0.010	(0.597)	-0.018	(-0.802)	0.200 *	(2.328)	0.015	(0.943)	-0.019	(-1.320)	0.025 **	(2.642)
Female dummy					0.085 ***	(13.062)					0.085 ***	(12.947)
Enrollment in post-secondary education by age 20	0.006	(0.360)	0.003	(0.127)	0.022 *	(2.446)	0.006	(0.424)	0.005	(0.273)	0.025 *	(2.480)
Female dummy					0.267 ***	(37.160)					0.267 ***	(36.650)
Number of children (completion)	3911		3150		16145		5554		4665		12987	
Number of families (completion)	1971		1615		7796		2824		2402		6156	
Number of children (enrollment)	4216		3430		17441		4216		3430		17441	
Number of families (enrollment)	1971		1615		7799		1971		1615		7799	

* p < 0.05; ** p < 0.01; *** p < 0.001

NOTE: Coefficient presented is on number of years of homeownership exposure during ages 4-18.

NOTE: All OLS models include all individual and household controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A-11. **Stratification by gender of oldest sibling -- all siblings of same mother and full siblings only**

	Gender of oldest sibling (all siblings of same mother)						Gender of oldest sibling (full siblings only)					
	(1)			(2)			(3)			(4)		
	Male			Female			Male			Female		
	Coefficient		t-stat	Coefficient		t-stat	Coefficient		t-stat	Coefficient		t-stat
Outcome												
High school completion by age 21	0.030	**	(3.049)	-0.003		(-0.312)	0.032	***	(3.149)	-0.005		(-0.493)
Female dummy	0.077	***	(7.056)	0.095	***	(8.590)	0.076	***	(6.439)	0.093		(7.985)
Enrollment in post-secondary education by age 20	0.017		(1.645)	0.017		(1.546)	0.015		(1.522)	0.018		(1.579)
Female dummy	0.268	***	(22.859)	0.260	***	(20.597)	0.268	***	(21.933)	0.225	***	(18.599)
Number of children (completion)	12106			11049			12152			11003		
Number of families (completion)	5931			5431			5941			5421		
Number of children (enrollment)	13071			11956			15152			11902		
Number of families (enrollment)	5933			5431			5941			5421		

* p < 0.05; ** p < 0.01; *** p < 0.001

NOTE: Coefficient presented is on number of years of homeownership exposure during ages 4-18.

NOTE: All OLS models include all individual and household controls, housing controls, mobility controls, family fixed effects, and neighborhood fixed effects.

Appendix Table A-12. Logistic Regression Specification Predicting High School Completion by Age 21

	Full Sample		Below Median Income		Above Median Income	
	Odds ratio	z-stat	Odds ratio	z-stat	Odds ratio	z-stat
Number of years between ages 4-18 residing in home owned by parents	1.113 *	1.88	1.123	1.54	1.100	1.06
Female (omitted=male)	1.991 ***	12.34	1.858 ***	7.44	2.106 ***	9.79
Sibling parity (omitted=first born)						
Second born	0.638 ***	-3.36	0.497 ***	-3.68	0.774	-1.29
Third born	0.506 ***	-2.73	0.312 ***	-3.35	0.744	-0.79
Fourth or higher born	0.350 ***	-2.77	0.193 **	-3.09	0.534	-1.11
Birth year cohort						
1987	0.915	-0.47	1.020	0.07	0.870	-0.51
1988	1.291	0.99	1.060	0.15	1.533	1.18
1989	2.118 **	2.17	1.371	0.61	3.012 **	2.30
1990	2.104 *	1.69	1.272	0.37	3.135	1.89
1991	2.578 *	1.77	1.126	0.15	5.046 **	2.20
1992	3.604 **	2.02	1.506	0.43	7.241 **	2.26
1993	4.059 *	1.89	1.242	0.19	10.237 **	2.29
1994						
Family control variables						
Mother's age at youth's birth	0.847	1.61	1.099	0.61	0.684 ***	-2.72
Number of years from 4-18 youth not residing with both parents	1.046	0.92	1.017	0.29	1.013	0.09
Living with a stepparent at age 18	0.480 *	-1.78	0.125 ***	-2.66	1.381	0.45
Number of persons in household when youth was age 18	1.010	0.08	0.904	-0.63	1.064	0.35
Number of siblings (full, half, step) in same household when youth was age 18	1.043	0.57	1.193	1.59	0.937	-0.66
Natural log of average median family income between ages 1-18	1.643	1.41	1.659	1.01	1.660	0.96
Natural log of average median parental wealth between ages 7-18	0.887	-0.52	0.774	-0.71	1.011	0.04
Housing control variables						
Number of years between ages 4-18 not residing in single family housing	0.995	-0.23	1.000	-0.01	0.990	-0.36
Number of residential moves during childhood (omitted=none)						
1 move	0.950	-0.34	0.857	-0.73	1.079	0.33
2 or more moves	0.656	-1.41	0.523	-1.60	0.626	-1.00
Observations	5,885		2,610		3,275	
Groups	2,642		1,166		1,476	
Log likelihood	-1954.695		-862.524		-1080.224	
Chi2	241.8		114.77		150.26	
Prob > chi2	0		0		0	

Legend: * p<.1; ** p<.05; *** p<.01

Table A.14 Geography Fixed Effects Robustness Check

	Completed HS by age 21	Enrolled in college by age 20
Coefficient on "Number of years between ages 4-18 residing in home owned by parents" when geography fixed effects are based on residential census tract at age		
13	0.012 *	0.017 **
14	0.014 **	0.017 **
15	0.013 *	0.016 **
16	0.014 *	0.015 **
17	0.012 *	0.015 **
18	0.014 **	0.015 *
Observations	23,206	25,087
Groups	11,382	11,385

Legend: * p<.1; ** p<.05; *** p<.01

Appendix Table A-15. Indicator Names and Descriptions

Indicator Name	**Variable Description**
Outcome Measures	
Completion of secondary school	Receipt of secondary school diploma by October 1 during their 21st calendar year (between age 20.5 and 21.5 years). Source: Norwegian Education Registry.
Enrollment in post-secondary education (college)	Enrollment in post-secondary education or college if they started in August of their 20th calendar year (between ages 19.7 and 20.7 years). Source: Norwegian Education Register.
Housing Indicators	
Housing tenure	Cumulative number of years between the ages of 4 and 18 that a youth resided in a home owned by their parents. Source: 1990, 2001, 2011 Decennial Censuses, Norway.
Housing type	Cumulative number of years between the ages of 4 and 18 that a youth lived in a single family, detached dwelling. Source: 1990, 2001, 2011 Decennial Censuses, Norway.
Housing mobility	Two dummy variables indicating one and two or more moves during childhood. Source: Norwegian Mobility Register; 1990, 2001, 2011 Decennial Censuses, Norway.
Individual Youth's Characteristics	
Gender	Female=1; male=0
Birth order	Three dummy variables 2nd born, 3rd born, 4th or higher born; reference category=first born.
Birth cohort	Dummy variable for year of birth 1987 to 1994; reference category=1986.
Household Characteristics	
Maternal age	Mother's age at child's birth.
Not living with both parents	Cumulative number of years during childhood that youth was not living with both parents.
Living with stepparent	Dummy variable indicating residence with stepparent at age 18.
Household size	Total number of people in household when youth was age 18.
Number of siblings	Total number of siblings in household when youth was age 18.
Natural log of average household Income	Average household income between ages 1 and 18
Natural log of average household wealth	Average household wealth between ages 7 and 18
Family-level fixed effects	
Neighborhood Characteristics	
Neighborhood-level fixed effects	Geographic dummy variables measured at the level of clusters, typically 4-8 census tracts.