Social Learning under Limited Experience: Evidence from High-Rise Housing Adoption

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Abstract

This paper studies how individuals make high-stakes investment decisions when some product attributes are only imperfectly observed prior to purchase. We examine the role of social interactions as a channel of learning, providing causal evidence on how peer networks shape high-rise housing adoption—a relatively unfamiliar form of housing for many households. Using population-wide administrative data from Sweden, we exploit quasi-random variation in peer exposure arising from school-based interactions among families. We find that households are more than 8 percent more likely to adopt high-rise housing when exposed to peers with prior experience. Event-study evidence shows that adoption increases only after exposure, with no pre-trends, supporting a causal interpretation. To interpret these findings, we develop a Bayesian learning framework in which households update imperfect prior beliefs through peer-provided signals. Consistent with this mechanism, peer effects are stronger when interactions are more likely to occur frequently and when prior beliefs are less precise.

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JEL Classification: D83, D84, D91, R21, C31

1 Introduction

How do individuals make high-stakes investment decisions when they have limited prior experience with a product and some of its key attributes are only imperfectly observed prior to purchase? In many markets, individuals must form beliefs about aspects of quality that are learned only through use, such as reliability, convenience, or day-to-day experience. In such settings, social interactions can provide an important source of information, allowing individuals to learn from the experiences of others and update their beliefs. Understanding how this form of network-based learning shapes decision-making is central to a wide range of contexts, including durable consumption, technology adoption, and housing choices.

This paper studies the role of social learning in housing investment decisions. We focus on the adoption of high-rise residential properties, which represent a relatively unfamiliar form of housing for many households. Compared to more common low-rise environments, individuals often have limited direct or indirect experience with high-rise living. As a result, beliefs about key product attributes—such as reliance on shared infrastructure, privacy, building management, patterns of social interaction, and day-to-day living conditions—are often imprecise and heterogeneous. This creates a setting in which information derived from peers' experiences can play a central role in shaping choices.

This setting is particularly relevant in the context of ongoing urban transformation. Across both developed and developing economies, residential high-rise buildings are becoming increasingly prevalent, driven by urbanization pressures and the need to accommodate population growth in dense cities (Glaeser, 2011; Romer, 2020).[1] As a result, many households will increasingly face the decision of whether to adopt high-rise living. However, this transition involves more than a change in physical structure: it requires adapting to a distinct living environment characterized by vertical density, reliance on shared infrastructure, and new patterns of daily interaction.

Despite these trends, households often appear reluctant to adopt high-rise housing.[2] In many settings, even when high-rise housing offers objective advantages, such as improved energy efficiency or upgraded amenities, individuals remain hesitant to transition from more

[1]They can aid in providing access to affordable housing, expanding sustainable and affordable transportation systems, generating productivity benefits, and mitigating the environmental impact of urban areas (Brueckner, 1987; Brueckner and Singh, 2020; Liu, Rosenthal, and Strange, 2018; Liu, Rosenthal, and Strange, 2020; UN-Habitat, 2020).

[2]Survey and case-study evidence points to a range of concerns, including reliance on elevators and building systems, reduced privacy, perceived safety risks, social isolation, and the suitability of such environments for families (Gifford, 2007; Gehl, 2013; Nguyen, van den Berg, Kemperman, and Mohammadi, 2024). Additionally, due to their scale, high-rise structures heavily rely on technology, which may be met with resistance by potential residents (Ng, 2017).

familiar low-rise environments.[3] This tension raises a central question: how do households form beliefs about an unfamiliar form of housing, and what role does social learning play in this process? Our empirical approach involves estimating the impact of having peers already living in high-rise apartment buildings on the likelihood of a mover choosing to purchase an apartment in a high-rise building instead of another nearby, available, affordable, and similar option in a low-rise building. We leverage quasi-random exposure to peers by using information on the school attended by each child in the country. In our analysis, a peer group consists of families with children of the same age and gender who attend the same school as the mover family's children. Through shared activities, such as sports games, children facilitate casual interactions among parents. We indeed provide evidence that the likelihood of having a peer family living in a high-rise building is unrelated to the observable characteristics of one's own family, conditional on the residential neighborhood.

Our analysis relies on detailed administrative data at the micro-level, covering the entire population of Sweden and tracking individuals' residential choices from 2012 to 2020. This data provides precise information about the location and characteristics of residential homes, along with extensive details about individuals, households, and schools. To enhance our data set, we incorporate supplementary information on real estate listing and sales from the major online platform *Hemnet*, covering the entire country's housing stock. We also merge in data on energy use and building characteristics for the entire spectrum of buildings in Sweden, including information at the apartment level. This level of data granularity enables us to identify all homes available on the market at any given time, along with their specific attributes. For each home, we have information about when it was listed for sale, how long it remained on the market, the asking price, the final sale price and the fee to the condominium together with the size of the home, number of rooms, which floor it's located on, and the energy consumption and energy class of the property the home is located in.

Beyond the richness of our data, Sweden offers a unique opportunity to study residential decisions related to high-rise buildings for several reasons. First, the proportion of Sweden's population residing in urban areas has steadily increased since the 1970s and is one of the highest in the world (United Nations, 2019). Second, Sweden responded to this challenge by building tall buildings.[4] Third, considerations regarding natural light, safety, and community well-being have led urban planners to maintain a harmonious mixture of high-rise and low-

[3]In many developing countries, managing population growth involves redeveloping slum areas and relocating residents to high-rise buildings. Despite the lack of basic amenities in slum environments, residents often resist the change, apprehensive about adapting to unfamiliar high-rise living conditions (see Gechter and Tsivanidis (2023); Hart and King (2019); Grey (2023)).

[4]Using data from the last two decades, Appendix Figure A1 shows that new high-rise constructions are growing taller each year and are characterized by an increasing number of floors (see also Haupt, Pont, Alstäde, and Berg, 2020).

rise residential buildings in many neighborhoods, where apartments in high-rise and low-rise buildings exhibit similar characteristics.[5]

The paper provides the first evidence that social interactions between early-movers in high-rises and their social contacts influence the decision to invest in high-rise housing. We establish a strong, causal, and robust social influence effect: the likelihood of buying an apartment in a high-rise building in a given area increases by more than 8 percent if the relocating family has at least one peer who already resides in a high-rise building. This effect remains stable in various scenarios, disappears in several placebo tests, and exhibits patterns in line with an information-sharing mechanism.

Our results cannot be attributed to correlated unobservables or spatial clustering of demographically similar families with a preference for high-rise living—for example, families moving into high-rises disproportionately originating from nearby areas and resembling the demographic profile of current high-rise residents. Balance tests indicate that the likelihood of having peers living in a high-rise is unrelated to a family's observable characteristics, conditional on the residential neighborhood. Additionally, the results cannot be explained by school-related shocks affecting all families with children attending the same school. This is evidenced by estimates using placebo peer groups within schools, which fail to replicate the observed patterns. Finally, further simulation experiments involving random peer assignments across schools demonstrate that the estimated effects are unique and cannot arise by chance.

To guide interpretation and discipline the discussion of mechanisms, we develop a Bayesian learning model of housing choice under uncertainty, in the spirit of social learning models of technology adoption and information diffusion (e.g., Foster and Rosenzweig, 1995; Conley and Udry, 2010). In the model, mover households hold imperfect prior beliefs about the net utility of high-rise living and update these beliefs through peer interactions that serve as informative private signals. The framework yields clear comparative statics: adoption increases upon exposure to an informed peer, the strength of peer effects rises with interaction intensity, and effects are larger for households with less precise prior beliefs. We test each of these implications directly in the data. Event-study evidence further shows that adoption increases sharply following first exposure to a high-rise peer, with no evidence of pre-trends, supporting a learning-based response to new information.

In line with the model's prediction that peer effects increase with interaction intensity, the effects are stronger when families moving into high-rises are exposed to peers with children of

[5] In Sweden, the sun barely rises above the horizon for several months each year. High-rises cast long shadows that block sunlight and contribute to a drop in temperature. Additionally, they create wind tunnels that are uncomfortable for the pedestrians (Al-Kodmany, 2018; Nugroho, Triyadi, and Wonorahardjo, 2022). Maintaining a mix of high-rise and low-rise buildings helps mitigate these problems.

the same gender and substantially weaker when exposed to peers with children of the opposite gender.[6] Because families with same-gender children are more likely to interact through shared activities, these findings suggest that heterogeneity in treatment intensity, rather than shared preferences, is the more likely explanation. Similarly, peer effects are stronger among geographically closer peers, further supporting an interaction-based mechanism. We also find larger peer effects among households with limited prior exposure to high-rise living, consistent with the prediction that social signals have a stronger impact when prior beliefs are less precise.

This paper contributes to several strands of literature. First, it relates to work on social learning and peer effects in economic decision-making, which documents how individuals use information from others when forming beliefs and making choices (e.g., Banerjee, 1992; Bikhchandani, Hirshleifer, and Welch, 1992; Foster and Rosenzweig, 1995; Conley and Udry, 2010). While this literature has established the importance of peer influence across a range of settings, identifying causal effects and isolating learning from correlated preferences and common shocks remains challenging. Using Scandinavian data, De Giorgi, Frederiksen, and Pistaferri (2020) document important peer effects in consumption behavior using quasi-random exposure to co-workers of similar characteristics. We contribute by providing causal evidence on social learning using quasi-random variation in exposure to household with similar children and by directly linking peer effects to belief updating in a setting where prior experience is limited.

Second, the paper contributes to the literature on decision-making under uncertainty and information acquisition. A growing body of work studies how individuals form beliefs when product attributes are not fully known ex ante, often emphasizing the role of signals, experimentation, or market-based information (e.g., Nelson, 1970; Ackerberg, 2003; Erdem and Keane, 1996; Kogan, Schneider, and Weber, 2025). We complement this work by highlighting the role of social interactions as a decentralized and endogenous source of information, and by showing how the importance of this channel depends on the precision of prior beliefs and the intensity of interactions. In particualr, our analysis is related to evidence that individuals hold systematically biased or simplified beliefs in complex environments (e.g., Rees-Jones and Taubinsky, 2020), as well as to work documenting systematic mistakes in real-world household financial decisions (e.g., Agarwal, Driscoll, Gabaix, and Laibson, 2009), and shows how social interactions can help mitigate such information frictions in the context of housing choices.

[6] Gender plays a key role in school-based friendship formation, with children showing a strong preference for same-gender peers. This pattern is confirmed by Smith, Maas, and van Tubergen (2014) and Kretschmer, Leszczensky, and McMillan (2025), who analyze classroom friendship networks from Sweden (among other countries) and find that students were significantly more likely to nominate same-gender classmates as friends.

Third, we contribute to the literature on housing markets and urban economics. While prior work has examined housing demand, neighborhood choice, and the valuation of housing attributes (e.g., Rosen, 1974; Bayer, Ferreira, and McMillan, 2007), relatively little is known about how individuals form beliefs about less familiar forms of housing and how these beliefs affect adoption decisions. Bailey, Cao, Kuchler, and Stroebel (2018) and Bailey, Dávila, Kuchler, and Stroebel (2019) leverage distant Facebook friendships to study social influences in the housing market. Bailey et al. (2018) and Bailey et al. (2019) show that individuals with Facebook friends in distant markets experiencing substantial house price increases are more likely to transition from renting to homeownership, purchase larger and more expensive homes, and adopt lower mortgage leverage. By focusing on high-rise housing, a setting characterized by limited prior experience and substantial heterogeneity in perceived attributes, we provide new evidence on how learning shapes housing investment decisions. A key challenge in this literature is the identification of causal peer effects, as homophily often drives the formation of social groups. The most compelling research designs address this issue by exploiting variation within narrowly defined or quasi-randomly formed peer groups. Our empirical strategy builds on this approach by leveraging granular administrative data and exploiting quasi-random peer exposure arising from school-based interactions. In this respect, our work is closely related to Bayer, Ross, and Topa (2008) and Bayer, Mangum, and Roberts (2021), who exploit limited sorting within neighborhoods to identify social interactions. We extend this framework by incorporating detailed information on building characteristics and focusing on learning in a setting where prior experience is limited.

The remainder of the paper is organized as follows. Section 2 describes the data and institutional setting. Section 3 draws on an original survey of high-rise apartment owners to examine whether peer interactions plausibly transmit information in this setting. Section 4 presents descriptive evidence on high-rise housing and key market patterns. Section 5 outlines the empirical design and reports the main results. Section 6 develops a theoretical framework to interpret the findings and derives testable implications, which we bring to the data. Section 7 concludes.

2 Data and Sample

We gather an extensive range of data for our analysis. Besides geographically detailed longitudinal data for the entire population of Sweden from 2012 to 2020, we assemble a novel real estate data set including building attributes as well as sales transactions for the apartment market in the nation. We complement our data collection with administrative information on population-level school enrollment, auxiliary information on education in environmental degrees, and information from an original survey of high-rise apartment owners.[7]

Individual Data. The individual data covers everyone in Sweden between 15-74 years of age and follows these individuals on a yearly basis between 2012 and 2020. The base for the individual data is the Longitudinal Integrated Database for Health Insurance and Labor Market Studies (LISA), which includes a wide array of demographical and economic variables. This database provides family identifiers that allow us to link different individuals to the same family.[8] To include individuals who are younger than 15 years, we use the child register. This register captures all children in Sweden and links them to parents. In addition to this information, we obtained access to the following data. First, we have detailed information on the level and specialization of education. This allows us to identify individuals who have environmental education and related occupation. Second, we merge this data with the housing register. This register provides detailed accommodation information: exact home-ID over time, type of accommodation, rental or owned, square meters, number of rooms, type of kitchen, and property ID. As a result, we obtain detailed information about the type of home residence each individual occupies during each year.

Geographical Units. For each individual, the data provide home location at different levels of geographical disaggregation. At the highest level, we have information at a grid-level, where each grid square is 250×250 meters in urban areas, and 1000×1000 meters in rural areas. This information is available for the whole time period (i.e., between 2012 and 2020). In our primary analysis, we define neighborhoods using the RegSO disaggregation. This is a statistical area created by Statistics Sweden that divides the country into 3,363 areas.[9]

Building Attributes. We obtained information on building attributes from the energy declaration register (*Boverket*). By law, every apartment building in Sweden is required to submit an energy declaration, which allows us to observe the universe of apartment buildings in Sweden at the beginning of 2023.[10] Apartment buildings in Sweden consists of either only rental apartments or only owned apartments. Mixed rental and owned apartments in the same building are rare, but both rental and owner-occupied buildings vary in height,

[7] The survey data collection is detailed in Section 3.

[8] Refer to Section C in Appendix for details on the LISA database.

[9] RegSO areas correspond to census tracts in the United States. Census tracts are statistical subdivisions of a county with an average of 4,000 inhabitants (https://www.federalregister.gov/documents/2018/11/13/2018-24567/census-tracts-for-the-2020-census-final-criteria). In Stockholm county, each RegSO area has an average of 3,527 residents aged 15 or older (own calculations using the individual data from LISA).

[10] The Boverket dataset is cross-sectional and includes all apartment buildings existing at the time of data access. Since we obtained access to this data in 2023, it covers all apartment buildings associated with the main sample of movers observed between 2012-2020.

including low-rise and high-rise structures.[11]

The *Boverket* data set offers detailed information on the universe of apartment buildings in Sweden, both rental and owner-occupied apartment buildings. Specifically, there is detailed information on each building's energy consumption, ventilation, and other environmental variables. This includes total energy consumption, the type of energy mix used by the building, the presence of environmentally friendly installations (such as solar panels), and the building's energy classification.[12] The underlying concept is to provide prospective buyers/renters with a quick indication of how the building's heating and other energy usage may impact the cost of living (through rent or condominium fees), offering a brief overview for environmentally conscious individuals. For each building, the data also include the number of floors, number of apartments per floor, total livable area, construction year, building ID, and property ID. Although the building ID is not provided in the individual-level data discussed above for confidentiality reasons, we merge the two data sets using the property ID. The reason this is an appropriate way to incorporate building attributes is illustrated in Appendix Figure A2, which displays the distribution of apartment properties (properties with at least one apartment building) by number of buildings in the entire country. It shows that about three out of four apartment properties in Sweden have one building only, and more than 85 percent have at the most two.[13] In addition, the relevant information for our analysis is whether a building is high-rise or low-rise. We observe that fewer than 2 percent of properties have a mixture of high-rise and low-rise buildings (as defined in Section 3). We thus exclude these properties from the sample and safely assign the number of floors from the *Boverket* data at the building level to each remaining property.

Housing Market Transactions. As in many other countries, the Swedish market for both detached houses and apartment purchases work in the following way. A prospective buyer starts by looking for interesting objects. Previously, this was mainly done by looking through apartment/house-advertisements in the local newspaper but since the beginning of the 21st century this activity has moved online. In particular, one early actor took the entire market of online advertisements of apartment/houses, namely the online service *Hemnet*. While competitors since then have entered the market as well (such as the similar service, *Booli*), *Hemnet* has retained their first mover advantage and still advertises 9 out

[11]See, for example, the building heights in the Million Programme policy listed here https://www.boverket.se/sv/samhallsplanering/stadsutveckling/miljonprogrammet/

[12]The energy classification ranges from A to G, where A presents the best grade (i.e. most environmentally friendly/energy efficient) and G is the worst. This classification system aligns with that used by the rest of the European Union.

[13]For a total of 66,004 apartment properties in Sweden, the mean is 1.95 buildings/property with a SD of 2.69. The 25th percentile and median are both 1 building, the 75th percentile is 2 buildings, the 90th percentile is 4 buildings, and the 95th percentile is 6 buildings.

of 10 apartments that are sold in Sweden.[14] In each *Hemnet* listing, various elements such as home information and pictures, condominium fees, and the asking price are provided.[15] Additionally, the listing includes dates for scheduled home viewings facilitated by the realtor for prospective buyers.[16] After the home has been shown, prospective buyers who remain interested in the home are invited to participate in an open auction. Through a bidding-process, managed by the realtor, the final price of the home is set, which is (usually) the highest bid, given that the seller accepts this price.[17] In most cases therefore, the final price is higher than the asking price, and the difference is the price development, or the "bidding" on the home.

We have collected unique data on the universe of apartments listings made at the online service *Hemnet* between 2012-2022.[18] This data includes the first listing date, the date the apartment is sold, asking price and final price, the condominium fee, exact address and geocode of the apartment, which floor the apartment is located in and the total number of floors of the building that the apartment is located in. In Section 4, we focus our analysis of high-rise apartment transactions on the 2020–2022 sample because the necessary floor variables have only been included since 2020.[19] This sample includes 165,087 apartments from every county in Sweden, most of which are sold in Stockholm county (see Appendix Table A3).

Movers. We consider the sample of families who relocated between 2012 and 2020. We exclude families that move to rental properties and those who move to types of residences

[14]In practice, most sellers advertise their apartments/houses on both *Hemnet* and *Booli*, but very few choose to only list their apartments at *Booli* since *Hemnet* is still the most known brand in the market and the go-to service for prospective buyers.

[15]The asking price is decided by the seller, but is strongly influenced by the recommendation of the realtor and his/her understanding of the local market. In general, the realtor wants to set as low an asking price as possible to attract more bidders and potentially incite a bidding war, as more bidders result in a higher final price, on which the realtor's commission is based.

[16]Unlike in some other countries, buyers in Sweden do not work with a specific realtor to search for a property. Instead, they use online platforms—primarily Hemnet—to independently search for available apartments. Once they identify a listing of interest, they contact the assigned realtor (who represents the seller) to schedule a viewing. This means that realtors generally do not guide buyers across multiple listings or steer them toward specific property types.

[17]In addition to price, other aspects of the deal are also negotiated, such as the timing of access to the apartment. Sometimes, the seller may opt for the second-highest bid if the prospective buyer offers more favorable terms, such as greater flexibility regarding access dates.

[18]We exclude apartments that are sold directly by developers (around 1.5 percent of the listings), i.e., newly built homes, since their sale process is different. The market for these apartments operates on a first-come, first-served system where buyers accept the price predetermined by the developers. Additionally, apartments are listed very early on *Hemnet*, often selling before the building is completed, and in some cases, even before the construction process begins.

[19]This time period is only considered for the analysis of the high-rise housing market in Section 4. In the main analysis, the sample of movers between 2012-2020 is the sample of interest, described in more detail in the next paragraph.

different from apartments. The first exclusion comes from the intricacies of the rental market in Sweden. Rental apartment buildings are typically owned by private companies, with the largest rental companies often owned by the local municipality. Due to rent regulation, the selection process for rental apartments is in most cities based on a queue-point system: individuals who have been in the queue the longest are the ones that get the rental apartment (given that they meet the basic financial requirements of the landlord).[20] As a result, renters have a much more limited choice set than those buying their apartments. This also means renters are less likely to be able to choose the height of the building they live in. We exclude families that move to detached houses or other types of residences that are not apartments because such dwellings lack a high-rise counterpart. Specifically, we define a family i as a *mover* if the following criteria are satisfied. First, a *mover* must have changed their home residence during the year, as determined by the exact apartment-ID. Secondly, the *mover* must have purchased the new residence, which must be located in an apartment building.

Peer Groups. We leverage quasi-random exposure to peers by merging another administrative data set into our data: records of the schools attended by each child in the country from the school register. The school register contains information about the school each child attends each year, including the precise school ID and the corresponding grid location of the school.

In our analysis, a peer group is defined as families with children of the same age and gender who attend the same school as the children of the mover family in the year before the move. As children participate in shared activities like sports games and enrichment classes, parents naturally form casual interactions. The fact that parents of schoolmates of the same gender and age are effectively randomly matched peers is carefully examined and confirmed by the data. Our results show that the likelihood of encountering a family living in a high-rise building among peers is essentially unrelated to the observable characteristics of one's own family, conditional on the residential area (see Section 5.1). Our assumption is that although these encounters are not deliberately chosen but rather random, the interactions provide valuable insights into living standards, which are taken into account when making housing investment decisions.[21] Over the entire sample period, we observe 403,134 peer groups in total, with a mean of 21 peer families per peer group, and a median of 17.[22]

[20] Owned apartments are administrated through organizations called condominiums. Renting out these apartments, whether to long-term tenants or through platforms like Airbnb, is generally prohibited.

[21] The housing market in Sweden operates at a rapid pace. As shown in Section 4, most apartments are sold within two weeks of being listed, with new owners typically moving in within three months of signing the contract (own calculations based on *Hemnet* data). Therefore, interactions with high-rise peers in the year prior to a move are more likely to occur during the search period or beforehand, rather than after the contract is signed.

[22] We exclude groups with less than 5 peer families. According to our contact at Statistics Sweden, the

3 Exploratory Small Scale Survey

We begin our exploration by presenting evidence from an original survey of high-rise apartment owners. The purpose of the survey is twofold. First, it allows us to assess whether social interactions are a plausible channel for information transmission in this context. Second, it provides direct evidence on households' beliefs and perceptions regarding high-rise living, which are not observable in administrative data. Together, these insights help interpret the reduced-form evidence presented later in the paper and clarify the role of social learning.

Target Population and Distribution Process. We target households living in purchased high-rise apartments within Uppsala municipality, one of the largest municipalities in Sweden. We identified 31 high-rise buildings containing a total of 1,378 owner-occupied apartments.[23] The survey distribution began in May 2024, using two main channels. First, since owned apartments are managed by organizations known as condominiums, we contacted the condominium administrators and requested their collaboration in distributing the survey to apartment owners. To locate relevant contact information, we accessed condominium websites, which often provide details for both current residents and prospective buyers. Our primary source for identifying these websites was a web service called Allabrf.se (translated as "All Condominiums.se"). Through this process, we obtained contact information for administrators managing 25 of the 31 buildings. Based on input from these administrators, we distributed the survey using either a leaflet with a QR code and survey link or physical surveys accompanied by response envelopes, or a combination of both, depending on their preference. The condominium administrators facilitated the distribution directly to the apartment owners. Some condominiums either lacked websites or restricted contact information to a login service for owners. Additionally, some administrators did not respond to our requests. In these cases, we sent letters with the aforementioned leaflet directly to households in the remaining high-rise buildings. Using two additional web services, Booli.se and Hitta.se, we located address information for the owners of 413 out of the remaining 820 apartments. Altogether, we distributed the survey between May and June 2024 to 971 of the 1,378 apartments, reaching over 70 percent of the target population. In December 2024, we sent a round of reminder letters to non-respondents.

Survey Structure and Incentives. The survey comprised 18 questions across two modules. The first module asked about apartment use and pre-purchase discussions. Specif-

school register may include hospital schools with small classes, or some other special schools that have their own arrangements for small classes. Results remain roughly unchanged if we include these groups.

[23]To identify the universe of high-rise buildings with owner-occupied apartments, we utilized Hemnet data to pinpoint all high-rise buildings where at least one apartment was sold between 2020 and 2022. This selection was validated through our administrative data and Google Maps.

ically, this module explored how respondents used their apartments (e.g., for accommodation, as a financial investment, or other purposes), when they purchased them, and how frequently they discussed the experience of living in high-rises with friends or acquaintances before buying. Respondents were also asked whether these discussions focused more on concerns or benefits. In the second module, we explored the specific concerns and benefits respondents discussed in greater detail, providing examples and allowing free-text responses. Additionally, we asked whether they would recommend high-rise living to friends planning a move and whether they had prior experience living in high-rise buildings.[24] The full survey can be found in Appendix Section D. To encourage participation, we included a financial incentive: one respondent from each condominium was randomly selected to receive a 500 SEK (roughly 45 USD) gift card to ICA, Sweden's largest grocery chain. To ensure accessibility, participants could choose to complete the survey in either Swedish or English.

Response Rates and Final Sample. Out of the 971 households contacted, we received 367 complete responses, achieving a response rate approximately 38 percent. After excluding a small number of respondents who were renting or had inherited their apartments, the final sample comprised 349 responses.[25]

Survey Results. Table 1, Panel A examines the extent to which households exchange information about high-rise living prior to purchase. Consistent with the presence of an active information channel, the majority of respondents report having discussed high-rise living with friends or acquaintances before buying their apartment.[26] These conversations are not superficial: they involve detailed exchanges about both benefits (e.g., views, energy efficiency) and concerns (e.g., safety, privacy, and maintenance) (see Appendix Table A2). This evidence supports the interpretation that peer interactions can plausibly transmit relevant information. Panel B in Table 1 turns to the content of beliefs. A key finding is that concerns about high-rise living are more prevalent among households without prior experience. In particular, concerns are reported by 25 percent of households without high-rise experience, compared to only 16 percent among those with prior experience. This gap suggests that negative perceptions are, at least in part, driven by lack of direct exposure, consistent with the presence of pessimistic priors among inexperienced households. Importantly, these concerns do not translate into uniformly negative ex-post evaluations. Among

[24] We also collected information about household composition, including the number of individuals, their ages, the presence of children (particularly those in primary or middle school), education levels, and household income. However, the sample sizes are too small to explore heterogeneous results along these dimensions.

[25] While apartment owners in Sweden are prohibited from renting out their units for long-term use or through platforms like Airbnb (See Section 2) short-term rentals may be approved for specific reasons, such as temporary work or study in another city or trying out cohabitation with a partner.

[26] A share of nearly 70 percent is relatively high, as discussions of personal housing choices and living experiences are typically limited outside the family in Sweden.

respondents who report having shared concerns with others, only 8.7 percent indicate that they would not recommend high-rise living to a relocating friend. The remaining respondents would either definitely recommend it, recommend it under certain conditions, or express indifference between building types. This pattern indicates that, even among households who initially hold reservations, realized experiences are often sufficiently positive to justify recommending the product to others. Taken together, these findings suggest that households may enter the housing market with concerns that are not fully aligned with experienced outcomes. Peer interactions, by transmitting first-hand information, can help update these beliefs. This mechanism is consistent with a setting in which prior beliefs are imperfect and potentially biased toward pessimism, and where social learning plays a role in correcting these perceptions.

These insights provide a useful lens through which to interpret the descriptive evidence in Section 4. As we document there, high-rise apartments exhibit objective advantages, such as greater energy efficiency, yet face a persistent market discount and slower sales. The coexistence of favorable attributes and lower demand is difficult to reconcile with fully informed preferences. The survey evidence presented here suggests that this pattern may reflect, at least in part, misperceptions or uncertainty about high-rise living. In this context, peer exposure can serve as a mechanism for belief updating, helping to bridge the gap between perceived and realized quality.

4 High-Rises: Descriptive Evidence

Definition of High-Rises. We consider both an absolute and a relative definition of high-rises. In the case of the absolute definition, the threshold must be sufficiently high to classify a building as tall anywhere in Sweden. To identify this threshold, we look at the distribution of buildings by the number of floors in Stockholm county, which is the county with the highest population density in Sweden and where the majority of tall buildings are situated. We define as high-rises the buildings falling in the top 10 percent of the distribution. Under this definition, buildings with 8 or more floors are classified as high-rises, a category that captures the top 4 percent of buildings by height across Sweden.[27]

For the relative definition of high-rises, we rely on the distribution of apartment buildings based on their number of floors at the municipal level. We establish a threshold for what qualifies as a relative high-rise building by setting it at two standard deviations above the

[27]See Appendix Table A1. Our results remain robust when employing alternative high-rise definitions, such as considering buildings taller than nine floors across the entire country, or taller than the 95th county-specific percentile.

municipal mean. Since Sweden has an uneven distribution of the built environment, this threshold ranges from 2.71 to 9.97 floors across municipalities.

Our definition of high-rises as buildings with 8 or more floors reflects how tall buildings are typically classified in Sweden, in many other European cities, and in urban areas with similar structures. According to the EEA (2019), recent urban development in Europe often promotes tall buildings in the range of 6 to 15 floors to support compact and sustainable cities.[28]

High-Rises as Environmental Choices. A traditional prediction of urban land use models is that because the horizontal expansion is constrained by commuting costs, a larger city size maps into taller buildings (Brueckner, 1987). The general consensus in the literature is that denser areas have significantly lower emissions and energy consumption per capita (Glaeser and Kahn, 2010, Borck and Brueckner, 2018, Larson and Yezer, 2015; Borck, 2016). However, these studies are based on either numerical simulations of calibrated models or on aggregated data at a city level. Our data provides evidence on the energy consumption of tall buildings based on micro-level data.

Using the whole stock of apartment buildings in Sweden in the beginning of 2023, we compare the level of environmental friendliness between high-rises and low-rises by comparing their energy consumption per square meter of livable area, as reported in the energy declaration register (the *Boverket* data set). This variable reflects building-level energy consumption, beyond the direct control of individual apartment owners.[29] Table 2 shows the results when controlling for municipality fixed effects, time fixed effects and the construction year of the building. The first column shows the results using our main definition of absolute high-rises, i.e., buildings that are 8 floors or taller. On average, high-rises are using 8 percent less energy per square meter than low-rises.[30] In columns 2 to 5, we use higher thresholds for the definition of high-rises and show that the energy saving increases as the high-rise definition becomes more stringent, showing that it is indeed the tallness of the building that

[28]A clear example is Ørestad in Copenhagen, a newly developed district with buildings in this height range, planned along transit lines. In contrast, cities like New York or Shanghai have much taller buildings. As shown by Barr and Jedwab (2023), these cities are outliers, with very tall skylines and different cost and construction dynamics.

[29]The measured energy consumption includes energy for heating/cooling and hot water, which are generally included in the rent or fee to the condominium (apartment dwellers cannot affect the heating individually above a certain temperature, this is a building level decision taken by the condominium board or landlord). It also includes electricity used for communal areas within the building. Household electricity, which includes usage for home appliances like television, fridge, lighting, etc., is excluded from the measurement. Similarly, electricity consumed by businesses within the building is not considered

[30]This is in line with the model of Borck and Brueckner (2018) where residential energy use depends on a building's exposed surface area. The surface area increases less rapidly than floor space as the height of the building increases (because the roof area stays constant), which implies that energy use per square meter of floor space falls as height increases.

is driving the energy savings.[31]

High-Rises Housing Market. For each sold apartment, we consider three features: the final price, the price development from the asking price (the bidding on the apartment) and the number of days in the market, that is the days between the initial listing date and the eventual sale date.[32]

Table 3 Panel (a) presents the regression results of the differences in these three variables for apartments sold in high-rise and low-rise buildings, when the absolute high-rise definition (8 floors and above) is used. In all regressions, we consistently include RegSO-by-year fixed effects to account for expectations about local housing market dynamics. Additionally, we control for various characteristics of the sold apartment, including floor level, top-floor status, bracket controls for price per square meter (except for the final price estimation), construction year, a dummy variable for apartments larger than 50 square meters, and the monthly condominium fee.[33]

The results show that the price increase during the bidding process is, on average, approximately 12 percent lower for high-rises compared to low-rises, with a 4 percent reduction in the average final price per square meter if the apartment is located in high-rise buildings.[34] In addition, a high-rise apartment spends on average 1.1 days longer time on the market than low-rise apartments. This is a non trivial number of days since half of the apartments in the sample are sold within 16 days of being listed on *Hemnet*. It is worth noting that in Sweden, while high-rise and low-rise buildings differ in the environmental and market-related features discussed earlier, the apartments themselves are quite similar in terms of size and amenities. Our data show that apartments in high-rise buildings have an average floor area of approximately 68.24 square meters, compared to 69.46 square meters in low-rise buildings. Additionally, both types of buildings have a similar number of apartment units per floor, with high-rises averaging 2.76 units per floor and low-rises 3.31 units per floor. In our analysis, they also share comparable access to local amenities such as green spaces, parking areas, and schools, as they are located within the same neighborhood. Given that

[31] Similar results are found when using the energy classification (A to G) as the dependent variable.

[32] We exclude the top 2 percent of the listings with implausibly high values of days on the market (more than 294 days). These are likely to be apartments that are withdrawn from *Hemnet* and then relisted at a later time with the same first-listing date.

[33] As a sensitivity check, we also add the control variables sequentially. The results are reported in Appendix Tables A4 and A5 for the absolute and relative definition, respectively.

[34] Construction costs per square meter are comparable between high and low buildings (Boverket, 2014). According to Statistics Sweden's data on prices for newly produced dwellings, land costs account for no more than twenty percent of the total construction costs for multi-dwelling buildings (https://www.statistikdatabasen.scb.se/pxweb/en/ssd/START__BO__BO0201/). Additionally, in regressions using price development as the dependent variable, we control for final price brackets, which capture the value of the land. Consequently, the lower price development for high-rise apartments cannot be attributed to lower land costs.

more attractive apartments are generally expected to command higher final prices and sell more quickly (for instance, requiring fewer showings by the realtor), all else being equal, our results indicate a reluctance among households to purchase apartments in high-rise buildings compared to low-rise buildings.

Table 3 Panel (b) presents the results from using the relative definition of high-rises. The findings indicate a notable reduction in the high-rise penalty, suggesting that households tend to have a stronger aversion to living in tall buildings in an absolute sense rather than in comparison to the average height. This is especially troubling as an increasing number of households may have to opt for very tall buildings in the coming decades given the global trend of constructing taller buildings in response to the environmental crisis. Our analysis focuses on the absolute definition of high-rises for the remainder of the paper.

To ensure that our findings are not driven by supply-related factors, such as a difference in the number of apartments available for sale between high-rise and low-rise buildings, we conduct a supplementary analysis. This involved narrowing our sample to areas where the quantity of sold apartments in low-rise buildings equals or exceeds those in high-rise buildings. The results, detailed in Appendix Table A6, show that the evidence remain qualitatively unchanged.

In Appendix Table A7, we further show that the prices of apartments in a high-rise buildings varies nonlinearly with the floor number. For example, we find that while prices are higher for apartments on higher floors, the increase in value diminishes when the apartment is situated in a high-rise building (with the exception of the top floors).[35] This evidence holds true for both absolute and relative definitions.[36]

5 Social Interactions and Housing Investment Decisions

5.1 Empirical Design and Identification Strategy

Our sample includes all *movers* as defined in Section 2. To quantify the importance of social interactions on the movers' housing investment decisions, we examine the propensity of families to purchase an apartment in a high-rise building, comparing such propensities for families with and without peers who are living in high-rise buildings. We restrict choices to buildings *within* the neighborhood that contains the new home. This is done purpose-

[35] Perhaps unsurprisingly, interactions of the top-floor dummy and the high-rise building dummy show a premium for top-floor apartment in high-rise buildings (see Appendix Table A8).

[36] As mentioned in Section 2, confidentiality restrictions preclude access to precise addresses within Statistics Sweden's granular, grid-level individual data. As a result, we cannot merge the transaction data with the individual records.

fully. Residential choices are influenced by many factors, including work-related motivations, preferences for local amenities, and intra-household bargaining for married couples, many of which cannot be directly controlled in our framework. Limiting choices within neighborhoods reduces the extent of heterogeneity across choices and allows us to better isolate the effect of social interactions from competing explanations of location choice.[37]

A long-standing challenge in the literature on housing investment decisions is that only actual sales are observed. The range of available choices is usually not captured in observational data. Consequently, it's entirely plausible that the purchased house was the sole option available in the chosen area at the time of the search, or it was the only affordable one within the family's price range. This scenario might not necessarily reflect preferences for specific attributes over nearby alternatives. Leveraging the richness and structure of our data, we implement the following empirical design. To make sure that i) apartments in both high-rises and low-rises are available at the time of the search, and that ii) both options are affordable to the searching family, we restrict choices of apartment buildings to neighborhoods where there are comparable opportunities in both types of buildings available in the same time period. In particular, we restrict our analysis to movers in neighborhoods where, during the same period, at least one other family in the same narrowly defined income bracket purchased an apartment in a low-rise building and at least one purchased an apartment in a high-rise building.[38] This symmetric restriction ensures that both building types were transacted by comparable families in the mover's neighborhood, providing evidence that both options were part of the relevant market environment.

Our final sample of families with similar opportunities nearby consists of 18,150 families, out of which approximately 85 percent and 15 percent moved out of low-rise and high-rise buildings, respectively.[39] Given the importance of studying how to facilitate the transition from low-rise to high-rise residential buildings, our primary analysis focuses on the 85 percent of families who moved out of low-rise buildings (15,283 families). While the majority of them opted to buy an apartment in a low-rise building again, about 25 percent of them chose to buy

[37] Additionally, by examining a mover family's choice between low-rise and high-rise apartments in the same narrowly defined neighborhood ensures that these housing alternatives share comparable local amenities and disamenities, including green spaces, commercial activities, and crime rates.

[38] We use the total income of all income types to construct the family annual income variable. We develop income brackets that characterize families living in urban areas. For instance, using the real 2010 value, the distribution of household primary income in Stockholm county has a median of 43,686 Euros with the 25th and 75th percentile values being 6,650 and 83,713 Euros, respectively. We construct five equally-sized brackets for the family annual income between zero and 100,000 Euros and an extra bracket for an annual income above 100,000 Euros. In our sample, each income bracket contains an average of 3,025 families, with the count ranging from 1,565 to 4,533.

[39] We refer to this sample of 18,150 mover families as the broader sample. See Section C in the Appendix for the detailed step-by-step sample construction.

an apartment in a high-rise building.[40] Appendix Table A10 shows summary statistics for this sample of families that moved out of low-rise buildings, distinguishing between families who bought apartments in low-rise and high-rise buildings. On average, the characteristics of low-rise and high-rise families are broadly similar.

We estimate the model:

$$M_{i,t} = \beta HR\, Peer_{i,t-1} + \mathbf{X_{i,t-1}}\gamma + \lambda_{\tilde{c},c,t} + \epsilon_{i,t}$$

where $M_{i,t}$ is a dummy that takes the value one if family i moves to an apartment in a high-rise building as owner in time period t. $HR\, Peer_{i,t-1}$ captures the treatment. Figure A3 shows the distribution of mover families in our primary sample by the number of peer families residing in high-rise buildings. The graph shows that half of them had fewer than two peer families in high-rises, roughly 30 percent had no peer families living in high-rises, and the majority had fewer than seven high-rise peers, with noticeable dispersion in the right tail. We define $HR\, Peer_{i,t-1}$ as a dummy variable for having at least one peer family that already lives in a high-rise building in our baseline specification, and investigate non-linear effects and alternate definitions in Section 6. The coefficient of interest is β, which captures the social influence effect. The control vector $X_{i,t-1}$ captures family, peers, and apartment attributes. We also include i's total number of social contacts (peer group size) as well as number of children to control for a mechanical correlation stemming from larger groups having a higher probability to feature families living in high-rises. In addition, we include one dummy to capture whether the family moved to a different neighborhood or remained in the same one.[41] We estimate several specifications of the above model, with different sets of fixed effects $\lambda_{\tilde{c},c,t}$ where $\tilde{c}$ denotes the old residential neighborhood or school, c the new residential neighborhood, and t indicates the year of the move. Our preferred specification includes old neighborhood and new neighborhood-by-year fixed effects

$$\lambda_{\tilde{c},c,t} = \lambda_{\tilde{c}} + \lambda_{c,t}. \tag{1}$$

The first component in the above set of fixed effects accounts for factors specific to the old neighborhood that do not change over time. It captures baseline differences in wealth and lifestyle preferences associated with a family's previous location. The second component $\lambda_{c,t}$ creates a unique dummy variable for each combination of new neighborhood and

[40] We refer to this set of 15,283 families originating from low-rise buildings as the primary sample. Appendix Table A9 shows the entire transition matrix.

[41] In our primary sample, about 30 percent of the mover families moved within the same neighborhood, and 70 percent moved to a different one. We code the dummy as equal to one if the family moved to a different neighborhood and zero otherwise.

year. It absorbs all time-varying local housing market shocks (e.g., price fluctuations, new construction, and amenities) that might simultaneously drive high-rise availability and peer clustering. This specification relies on the identifying assumption that, conditional on a family's old neighborhood and the specific market conditions of their new neighborhood at the time of purchase, the remaining variation in peer exposure is orthogonal to unobserved preferences. As mentioned before, both the new and the old neighborhoods are defined at the granular RegSO level.[42] As an alternative, we replace origin neighborhood fixed effects with school fixed effects. This requires restricting the sample to families whose children attend the same school, but yields nearly identical estimates. This equivalence is expected because in Sweden most children attend the school closest to their home.[43] Furthermore, the composition of families in high-rise and low-rise apartments is similar (see Table A10), making it unlikely that deviations from proximity-based assignment correlate with high-rise peer exposure. In addition, in many smaller schools, no family has a peer residing in a high-rise building—this is the case for approximately 42 percent of schools in our broader sample. We therefore retain neighborhood fixed effects as our baseline specification.

For robustness, we add the *interaction* of the old and new neighborhood dummies

$$\lambda_{\tilde{c},c,t} = \lambda_{\tilde{c},c} + \lambda_{c,t}. \tag{2}$$

In this model specification, we introduce a rigorous set of paired origin-destination neighborhood fixed effects, denoted as $\lambda_{\tilde{c},c}$, which creates a unique dummy variable for each specific transition from an old neighborhood ($\tilde{c}$) to a new neighborhood (c), both defined at the granular RegSO level. The inclusion of the paired-RegSO fixed effects accounts for possibility that families sorting into specific origin-to-destination flows likely share unobserved traits (e.g., lifestyle preference changes, commuting patterns) that correlate with their peer networks. By absorbing these relocation-flow specific time-invariant characteristics, the paired-RegSO fixed effects ensures that the key coefficient β is estimated from variation in peer exposure among families making the exact same locational transition. While this is a demanding specification that restricts the sample to only populated location pairs, the estimated peer effect remains almost identical as that from our model specification (1). This stability suggests that our main results are unlikely to be driven by endogenous sorting. The fact that controlling for the origin-destination neighborhood pairs does not diminish the effect validates the identifying assumption of model specification (1) that utilizes a larger sample.

[42] In the city of Stockholm, the average land size of RegSO areas is 1.47 square kilometers. In 2020, more than 60 percent of the RegSO areas had both high-rise and low-rise buildings.

[43] According to Statistics Sweden, the average distance between home and school for students aged 6–15 is 0.7 km in major cities and their suburbs, and 0.9–1 km in smaller cities and towns (https://www.scb.se/hitta-statistik/artiklar/2024/barn-i-friskolor-reser-langre-till-skolan/).

Evidence in Support of the Identification Strategy. The main threats to a causal interpretation is that a correlation may arise in a scenario where peers are chosen because of characteristics similar to own characteristics and related to the decision in question. In our analysis, we tackle this issue by designing peer groups that are quasi-random, that is random conditionally to observed controls. While individuals are able to choose their residential neighborhood (and school), they have no ability to choose the precise composition of the characteristics of the families with children of same gender and age in the school. We investigate the validity of the identification strategy by examining whether variation in the key peer variables is related to variation in a number of predetermined family characteristics. Finding significant correlations would suggest a violation of the identifying assumption that parents do not choose which other parents to interact with based on the likelihood of finding a family living in a high-rise with children of the same gender and age as their own, conditional on observables. We consider a comprehensive set of family characteristics: annual family income in thousands of euros, the average age of working-age adults in the family, family size, the presence of higher education, environmental education, or self-employment within the family, and whether at least one parent is not working full-time. We run separate regressions with each of these as alternative dependent variables and the key peer variables as the independent variables, always including old neighborhood fixed effects, new neighborhood-by-year fixed effects, peer group size, and the number of children in the family. The results of these diagnostic tests are presented in Table 4. As shown in Table 4, none of the estimated correlations are significant.[44] In addition, the magnitudes of the estimates are close to zero. Further evidence in support of our identification strategy is provided by the event-study Figure 3 and Table 7 in Section 6.2.

5.2 Results

We begin our analysis by estimating our empirical model on the broader sample of families, regardless of whether they moved from high-rise or low-rise buildings, using different sets of fixed effects. The results are reported in Table 5. Column 1 presents estimates from a specification with old and new neighborhood fixed effects. Column 2 adds local time trends. Column 3 replaces old neighborhood fixed effects with school fixed effects. The standard errors are two-way clustered at the old and new neighborhood level, though the statistical significance of key parameter estimates is robust to the choice of clusters. The results show that controlling for local time trends (Column 2) and replacing old neighborhood fixed effects

[44] Altonji, Elder, and Taber (2005) suggest that the degree of selection on observables can provide a good indicator of the degree of selection on unobservables. In light of this argument, the evidence of Table 4 supports the hypothesis that the model specification identifies an exogenous source of variation.

with origin school fixed effects (Column 3) each modestly reduce the estimated effect, but all three specifications yield qualitatively similar estimates. We adopt Column 2 as our baseline specification because the inclusion of new neighborhood-by-year fixed effects absorbs time-varying local shocks, while the alternative specification with school fixed effects relies on a smaller sample, as discussed above in Section 5.1.

Table 6 reports the results for our sample of interest, families that moved out of low-rise buildings, under progressively richer sets of controls. The social influence coefficient in Column 1 is 0.0215, indicating a substantial and precisely estimated effect. In our primary sample, about 25 percent of movers from low-rise buildings purchased an apartment in a high-rise building (see Appendix Table A9). Relative to this baseline, exposure to at least one peer already living in a high-rise increases the adoption probability by more than 8 percent.[45]

Columns 2, 3 and 4 add in additional family controls, peer characteristics and apartment characteristics, respectively. The coefficient for the social influence effect remains stable across columns, confirming the pseudo-random nature of our key variable. In subsequent discussions, we refer to Column 4's estimate of the social influence effect (0.0196) as the baseline estimate. Appendix Table A11 shows the results remain robust when controlling for origin-destination neighborhood flows (model specification (2)).

Our empirical design conditions on movers. If peer exposure also affects the propensity to move, then restricting the sample to movers could induce a spurious correlation between peer exposure and unobserved determinants of high-rise choice. To assess the empirical relevance of this channel, we re-estimate specification (1) on the full population of families living in low-rise buildings at $t-1$, including non-movers, using an unconditional adoption indicator that equals one if the family transitions from low-rise at $t-1$ to high-rise at t and zero otherwise. Appendix Table A12 shows that high-rise peer exposure remains a statistically significant predictor of high-rise adoption in this full-sample specification.

To increase confidence in the estimation results, we conduct a simulation exercise to determine whether the coefficient on our key peer variable could have occurred by chance (see Athey and Imbens, 2017). Randomness in exposure to peers living in high-rise apartments is generated by regressing families' $HR\ Peer_{i,t-1}$ on old neighborhood and new neighborhood-by-year fixed effects to generate predicted values for each family. Next, we assign each family the average residual among families in a randomly drawn school within the same municipality. We then obtain random variation in each family's $HR\ Peer_{i,t-1}$ by combining

[45] Conditional on the new neighborhood, the probability of choosing an apartment in a high-rise building is 0.05. Relative to this conditional mean, as reported in Column 1 of Table 6, having at least one peer family already living in a high-rise building increases a mover family's probability of choosing high-rise housing by more than 40 percent.

the predicted values with these randomly assigned residuals. All other variables remain at their true values. Figure 1 shows the estimated coefficients of the regressions when the placebo variables are used and the procedure is repeated 1,000 times. The solid line represents the actual treatment coefficient. The share of estimates that is larger in absolute value than the actual treatment represents the randomization-based p-value. Only three of the placebo estimates of $HR\ Peer_{i,t-1}$ are larger in absolute value than the actual treatment coefficient (0.0196), providing evidence that it is highly unlikely to have occurred by chance.

Placebos. We further examine whether our results are driven by sorting into the same school based on unobservables. In particular, we implement a Monte Carlo experiment where peers are randomized within the same school. Similarly to Figure 1, to generate predicted values for each family, we regress families' $HR\ Peer_{i,t-1}$ on old neighborhood fixed effects and new neighborhood-by-year fixed effects. Next, we assign each family the average residual among families in a randomly drawn peer group within the same school. We then obtain random variation in each family's $HR\ Peer_{i,t-1}$ by combining the predicted values with these randomly assigned residuals. All other variables remain at their true values. Figure 2 shows the estimated coefficients of the regressions when the placebo peers are employed and the procedure is repeated 1,000 times. The solid line represents the actual treatment coefficient (0.0196). Only five of the placebo estimates are larger in absolute value than the actual treatment coefficient. The results of this exercise suggest that our main findings are unlikely to be driven by within-school clustering of unobservables, providing greater confidence in a genuine social influence effect.

6 Theoretical Framework and Testable Implications

To guide interpretation of our baseline results and discipline the discussion of mechanisms, we develop a model of housing choice under uncertainty. Our model focuses explicitly on the decision problem of mover households who have already decided to relocate and must now choose between high-rises and low-rises. This restriction mirrors our empirical design, which conditions on the sample of movers to isolate the choice between building types.

Consistent with our descriptive evidence that high-rise apartments offer objective advantages (e.g., energy efficiency) but face a market penalty, we assume that novice buyers begin with a pessimistic prior over high-rise living quality, rationalizing the observed discount. Building on the literature in social finance and technology adoption (e.g., Mobius and Rosenblat, 2014; Conley and Udry, 2010; Foster and Rosenzweig, 1995), we formalize social interactions as a Bayesian learning process where peers provide private signals that allow households to update their beliefs and potentially overcome the initial market stigma.

In our model, mover households who have peers living in high-rise buildings receive a noisy private signal about the true net benefit of high-rise living and update beliefs via Bayes' rule, so that peer exposure shifts the posterior mean and, in turn, the probability of choosing high-rises. Crucially, the precision of this social signal is increasing in interaction intensity, so the model delivers a transparent mapping from the heterogeneity patterns in the data to differences in the informativeness of peer exposure.

6.1 Model Setup

Consider a continuum of mover households indexed by i who must choose between a high-rise option (HR) and a low-rise option (LR). We normalize the utility of the standard option (LR) to zero. The utility from choosing HR is determined by the true quality θ^* and an idiosyncratic preference:

$$U_i(HR) = \theta^* + \epsilon_i,$$

where θ^* is the true net benefit of high-rise living and $\epsilon_i \sim N(0, \sigma_\epsilon^2)$ is a taste shock. Based on our descriptive evidence regarding energy efficiency, we assume the true quality is positive ($\theta^* > 0$) for the relevant population of movers.[46]

Prior Beliefs and Market Stigma. Households do not know the true quality θ^*. Instead, they hold a prior belief that the quality is drawn from a distribution:

$$\theta \sim N(\mu_0, \sigma_0^2).$$

Consistent with the observed market penalty for high-rise units, we assume the prior mean is pessimistic relative to the truth ($\mu_0 < \theta^*$) and negative relative to the outside option ($\mu_0 < 0$). This implies that, without additional information, $E[\theta] < 0$ so the adoption of the high-rise option is relatively unlikely. The precision of the prior is denoted by $\tau_0 = 1/\sigma_0^2$.

Social Signals and Information Neighborhoods. Let J denote the number of peers in high-rise apartments. Household i only receives a private signal s_i generated by the true

[46]The assumption $\theta^* > 0$ is sufficient for the comparative statics derived below and rationalizes the observed market discount as reflecting, at least in part, imprecise beliefs. However, the model's testable implications are driven by the gap $\theta^* - \mu_0$ rather than the absolute sign of θ^*. All that is required is that peer signals, which are centered on θ^*, convey information that is on average more favorable than households' prior beliefs. This weaker condition accommodates the possibility that high-rise living involves genuine disamenities, such as reduced privacy or reliance on shared infrastructure, while still generating the prediction that peer exposure shifts beliefs upward and increases adoption.

quality θ^* if they have at least one high-rise peer (i.e., $J > 0$). Therefore

$$s_i = \begin{cases} \theta^* + \eta_i & \text{if } J > 0, \quad \text{where } \eta_i \sim N(0, \sigma_\eta^2) \\ 0 & \text{if } J = 0. \end{cases} \tag{3}$$

We model the signal precision $\tau_\eta = 1/\sigma_\eta^2$ as a function of the interaction frequency, $\tau_\eta = \lambda I_i$, where $\lambda > 0$ represents the baseline efficiency of communication. Here, I_i captures the treatment intensity. A higher treatment intensity reduces social frictions in communication, effectively lowering the noise variance σ_η^2. If $J = 0$, the household does not observe signals. We can set $I_i = 0$ and therefore $\tau_\eta = 0$, so the posterior equals the prior.[47]

Belief Updating. Households update their beliefs using Bayes' rule. The posterior mean μ_i is a precision-weighted average of the prior mean and the observed signal:

$$\mu_i = (1 - w_i)\mu_0 + w_i s_i, \quad \text{where } w_i \equiv \frac{\tau_\eta}{\tau_0 + \tau_\eta}.$$

The weight w_i represents the influence of the social signal relative to the prior. Because $\theta^* > \mu_0$, when $J > 0$ the social signal s_i (which is centered on θ^*) will, on average, pull the posterior belief μ_i upward, correcting the initial pessimism.

Derivation of Adoption Probability. We seek the probability that a household chooses HR, conditional on the true quality θ^*. A household adopts if their expected utility (posterior mean plus taste shock) is positive:

$$\mu_i + \epsilon_i > 0 \implies \frac{\tau_0\mu_0 + \tau_\eta s_i}{\tau_0 + \tau_\eta} + \epsilon_i > 0.$$

For treated households ($J > 0$), we substitute the active signal generation process ($s_i = \theta^* + \eta_i$) and rearrange terms to isolate the stochastic components:

$$Z_i \equiv \tau_\eta \eta_i + (\tau_0 + \tau_\eta)\epsilon_i > -(\tau_0\mu_0 + \tau_\eta\theta^*).$$

The composite error term Z_i is normally distributed with mean zero and variance $\sigma_Z^2 = \tau_\eta + (\tau_0 + \tau_\eta)^2\sigma_\epsilon^2$.[48] Using the symmetry of the normal distribution, the probability of adoption is:

$$P(HR \mid \theta^*, \tau_\eta) = \Phi\left(\frac{\tau_0\mu_0 + \tau_\eta\theta^*}{\sqrt{\tau_\eta + (\tau_0 + \tau_\eta)^2\sigma_\epsilon^2}}\right). \tag{4}$$

[47] Equivalently, we could let $s_i = 0$ be a normalization because $\tau_\eta = 0$ makes the signal irrelevant.

[48] We calculate the variance of Z_i, denoted as $\sigma_Z^2 = \mathrm{Var}(\tau_\eta\eta_i) + \mathrm{Var}((\tau_0 + \tau_\eta)\epsilon_i)$. Because $\mathrm{Var}(\eta_i) = \sigma_\eta^2 = 1/\tau_\eta$ and $\mathrm{Var}(\epsilon_i) = \sigma_\epsilon^2$, we obtain $\sigma_Z^2 = \tau_\eta^2\left(\frac{1}{\tau_\eta}\right) + (\tau_0 + \tau_\eta)^2\sigma_\epsilon^2 = \tau_\eta + (\tau_0 + \tau_\eta)^2\sigma_\epsilon^2$.

For households with no high-rise peers ($\tau_\eta = 0$), the probability of adopting HR collapses to the baseline adoption rate $\Phi(\mu_0/\sigma_\epsilon)$, which is low given the pessimistic prior $\mu_0 < 0$. For households with at least one high-rise peer ($\tau_\eta > 0$), because the true quality is positive but the prior is negative ($\theta^* > 0 > \mu_0$), the adoption probability $P(HR \mid \theta^*, \tau_\eta)$ is increasing in signal precision τ_η.[49]

6.2 Timing and Causal Interpretation

A central implication of the learning framework concerns the timing of how adoption responds to peer exposure. In the model, the adoption probability $P(HR \mid \theta^*, \tau_\eta)$ changes only when a household transitions from the untreated state ($J = 0$), in which there is no informative high-rise peer signal ($\tau_\eta = 0$), to the treated state ($J > 0$), in which exposure generates a signal of finite precision ($\tau_\eta > 0$). Absent changes in peer exposure, the model does not generate systematic changes in high-rise adoption over time. We examine this prediction using an event study design centered on the first year in which a peer group acquires its first high-rise peer.

Even though we do not observe communication links and therefore cannot measure the effect of direct interactions with high-rise peers, the richness of our data allows us to identify all families residing in low-rise buildings whose peer groups either transitioned from having no high-rise residents to having at least one during the sample period (switcher groups) or never acquired a high-rise resident throughout (never-treated groups). Employing such an event-study sample, which includes both movers and non-movers, we examine the effect of high-rise peer exposure on the cumulative adoption of high-rise living and the timing of adoption.[50] We center the event time on the arrival year of the first high-rise resident in switcher groups and use an indicator that equals one if the family has ever transitioned from a low-rise to a high-rise building by year t, and zero otherwise, as the outcome variable to capture cumulative adoption.[51] Exploiting variation in the specific year the peer group gained its first high-rise resident, Figure 3 shows flat pre-trends prior to exposure, followed by a sharp and statistically significant increase in the probability of having ever adopted high-rise housing. This pattern is consistent with adoption responding to the arrival of new information rather than reflecting anticipatory behavior or pre-existing trends.[52]

[49] See proof in Appendix Section A.

[50] Families whose peer groups had high-rise residents from the start of the observation window are excluded because their treatment onset is unobservable in our data.

[51] Families in never-treated grous are pooled into the omitted reference period $t - 1$.

[52] A potential concern for event study estimation relying on fixed effects under staggered treatment is that already-treated groups may serve as implicit controls for later-treated groups, producing biased estimates when treatment effects vary across event time (Goodman-Bacon, 2021). In our setting, this concern is substantially mitigated by two features of the design. First, the comparison pool is dominated by never-

As an additional falsification test, we augment the baseline regression with a future high-rise peer indicator that equals one if the mover has a high-rise peer in the year after the mover's move ($t + 1$). Because households cannot respond to peer exposure that has not yet occurred, the learning framework predicts a null effect of future peers. If sorting on unobservables were driving the baseline estimate, this future-peer indicator would predict high-rise choices at the time of the move. Table 7 shows that the coefficient on the future-peer indicator is statistically indistinguishable from zero, while the lagged high-rise peer coefficient remains stable, providing evidence against pre-move selection into destinations where high-rise peer exposure would emerge shortly after the move. Taken together, the event study and placebo evidence support a causal interpretation in which the estimated social multiplier reflects information transmission from peers rather than pre-move sorting on unobservables.

6.3 Signal Relevance and Effective Treatment

Equation (4) implies that the adoption probability $P(HR \mid \theta^*, \tau_\eta)$ is increasing in signal precision τ_η. Because we parameterize precision as $\tau_\eta = \lambda I_i$ with $\lambda > 0$, the model predicts stronger peer effects when interaction intensity I_i is higher. Interpreting I_i as the effective relevance of peer communication, exposure that is plausibly low-intensity should generate negligible effects, whereas exposure that is high-intensity should generate larger effects that increase with the number of relevant peers.

This prediction naturally lends itself to an empirical test based on heterogeneity in the strength of social interactions. In our setting, interactions are likely to be stronger among families with children of the same age and gender—groups that are more likely to share activities, communicate frequently, and exchange relevant information. By contrast, interactions across opposite-gender peers are plausibly weaker and less informative. We exploit this variation to construct proxies for interaction intensity: we treat exposure through same-gender peers as a high-intensity channel and exposure through opposite-gender peers as a low-intensity channel. Moreover, within the set of same-gender peers, variation in the number of high-rise peers provides a natural way to capture differences in the intensity of exposure.

treated families. These families are pooled into the reference period and serve as the primary comparison group in every event-time contrast. Second, the yearly treatment cohorts are roughly balanced in size, so no single cohort dominates the estimation. These features imply that in our setting, the fixed effects-based event study with a large never-treated comparison group and balanced group sizes approximates the estimand of Callaway and Sant'Anna (2021). We interpret the event study result as evidence that the timing of adoption is consistent with a response to peer exposure, complementing the cross-sectional identification strategy based on balance tests, placebo simulations, and the future high-rise peer falsification test.

To implement this test, we redefine the peer group to include same-age children attending the same school regardless of gender and decompose exposure to high-rise peers into (i) cases where the mover has only opposite-gender children high-rise peers and (ii) cases where the mover has at least one same-gender children high-rise peer. We then further stratify the latter by terciles of the number of high-rise peers. Table 8 reports the corresponding estimates. In Column 1, we obtain a sample based on redefined peer groups without requiring children in the same peer group are of the same gender, and applies model specification (1) to this restructured sample.[53]

Column 2 of Table 8 decomposes the peer effect by distinguishing between high-rise peers whose children are exclusively of the opposite gender and those with at least one child of the same gender as the mover family. Column 2 shows that exposure exclusively through opposite-gender children high-rise peers yields an estimate that is small and statistically indistinguishable from zero, consistent with this channel representing low-intensity interactions (i.e., low I_i and hence low τ_η). In contrast, exposure that includes at least one same-gender children high-rise peer is positive and statistically significant, indicating that same-gender ties transmit more informative signals. Column 3 further sharpens this pattern by showing that, conditional on having at least one same-gender children high-rise peer, the estimated effect increases monotonically with the number of high-rise peers.[54] The effect for families in the bottom tercile is smaller than that for families in the middle tercile and smaller than that for families in the top tercile.[55]

Taken together, these results provide direct evidence in support of the model's mechanism: peer effects operate through interaction intensity, with stronger and more numerous relevant ties increasing the precision of social learning and, in turn, adoption.

6.4 The Social Learning Effect

Having established that informative exposure operates primarily through same-gender peers, we can define and empirically implement a clean measure of the adoption gain generated by receiving a usable social signal relative to the no-signal baseline. In the model, moving from

[53]The estimated effect in Column 1of Table 8 is larger in magnitude than the estimate reported in Column 4 of Table 6. The key difference is that families exposed to opposite-gender children high-rise peers are categorized as part of the reference group in Table 6, while in Column 1 of Table 8 the reference category is families with no high-rise peers of any gender.

[54]Because we do not observe actual communication links, our baseline coefficient captures the effect of exposure to potential high-rise peers. For social interactions to generate the observed effects, influence must operate through actual peer connections. Our findings are therefore consistent with the interpretation that a larger pool of potential high-rise peers increases the likelihood of forming such connections, thereby strengthening peer effects.

[55]The estimated effect for mover families in the bottom tercile differs from that for families in the middle tercile at the 10 percent significance level and from that for families in the top tercile at the 5 percent level.

the untreated state ($J = 0$, interpreted as $\tau_\eta = 0$) to the treated state ($J > 0$, $\tau_\eta > 0$) increases the high-rise adoption probability

$$\Delta P(\tau_\eta) \equiv P(HR \mid \theta^*, \tau_\eta) - P(HR \mid \theta^*, 0) > 0.$$

Intuitively, when $\theta^* > \mu_0$ the social signal shifts beliefs toward the true net benefit, raising the posterior mean and thereby increasing the probability of choosing a high-rise. Empirically, Table 8 indicates that exposure through opposite-gender peers is statistically negligible, so the relevant no-signal comparison corresponds closely to movers with no same-gender high-rise peers. With this interpretation, the implication maps naturally to our baseline estimates in Table 6. When peer groups are defined by relevant (same) gender and same age children attending the same school, movers with at least one high-rise peer are significantly more likely to move into a high-rise than otherwise similar movers without such peers. We interpret this as a social learning operating through belief updating, with peer exposure shifting adoption upward from a low baseline demand driven by pessimistic priors.

6.5 Geographic Proximity and Localized Interactions

Motivated by evidence that social interactions are highly localized (Bayer et al., 2008; Barwick et al., 2023), we posit that geographic proximity reduces communication frictions and increases I_i. Consequently, the incremental effect of peer exposure should be largest when peers are immediate neighbors and should attenuate as the definition of a neighbor broadens to include more distant peers.

We test this prediction by estimating whether the effects are stronger when the peers in high-rises are also neighbors. The results are shown in Table 9 that uses different neighborhood definitions at an increasing level of geographical detail.[56] They show a clear pattern: the effect becomes stronger as the areas decrease in size. In Columns 1 and 2, the interaction terms for the coarser definitions are small and statistically insignificant, suggesting that residing in the same broad district adds little incremental information beyond the baseline peer relationship. In contrast, Column 3 shows a large and statistically significant incremental effect when at least one high-rise peer is located in the same grid cell. The effective interaction intensity I_i appears to be highest at very fine geographic distances, and coarser spatial aggregation dilutes the relevant social signal, which is consistent with highly localized information transmission.

[56]We achieve this goal by using RegSO, DeSO and grid statistical units. Sweden is divided into 3,363 RegSO areas, 5,984 DeSO areas, and approximately 220,000 grids. In Stockholm county, while RegSO areas have an average of 3,527 residents aged 15 or older, DeSO areas and grid areas have average populations of 1,411 and 95 residents aged 15 or older, respectively.

6.6 Prior Effectiveness and Uncertainty Resolution

In the learning model, the posterior weight placed on the peer signal is $w_i = \frac{\tau_\eta}{\tau_0+\tau_\eta}$, which is decreasing in the precision of the household's prior τ_0. Consequently, the effect of peer exposure on high-rise adoption should be smaller when households enter the market with more precise priors and larger when priors are diffuse.

We test this prediction using two proxies for prior precision. First, we use market familiarity, because long-distance movers face greater uncertainty about how high-rise living is going to be in the destination region. Differences in urban density, building standards and neighborhood conditions may make prior beliefs less precise. In terms of our learning model, less precise priors increase the posterior weight placed on signals from peers. Second, we use private experience, as households previously living in high-rises are likely to hold more precise beliefs about high-rise living quality than novice movers from low-rises. Table 10 implements both tests by interacting the high-rise peer indicator with indicators for moving distance in Columns 2 and 3, and with the previous building type in Column 4. In Column 4, we enlarge our sample to include families who moved out of high-rise buildings. Those families possess firsthand knowledge of high-rise living. As a result, if information is an important source of the social influence effect we should observe that social interactions with other families living in high-rises have no effect for their decisions to buy apartments in high-rise buildings.

Columns 2 and 3 show that peer effects are substantially larger for longer-distance moves. In Column 2, the estimated peer effect for cross-municipality movers (0.0543) exceeds that for within-municipality movers (0.0081). In Column 3, the peer effect is especially large for cross-county movers (0.1140) compared to within-county movers (0.0163). Column 4 shows that the estimated peer effect is concentrated among novice movers from low-rises, while it is small and statistically indistinguishable from zero for movers coming from high-rises. The results in Column 4 suggest that when priors are more precise due to direct experience, the value of peer signals is attenuated, which is consistent with private information substituting for social learning. Overall, these comparisons support the model mechanism that peer signals are most influential when households face greater uncertainty about the relevant market environment and thus enter with less precise priors.

6.7 Ruling Out Alternative Mechanisms

While our preferred interpretation emphasizes information transmission through peers, the baseline peer coefficient could in principle reflect other channels. A preference for co-locating with friends would predict a stronger peer effect when peers are present in the mover's destination area.[57] A positional-externality or neighborhood-composition channel would predict that the peer effect varies systematically with the prevalence of high-rise housing in the destination neighborhood.[58] Finally, a pure social-norms channel would suggest that adoption responds primarily to the share of peers living in high-rises rather than to the presence of any high-rise peer.[59]

Appendix Table A13 implements three complementary tests. Column 1 of this table reproduces Column 4 of the baseline results table. In Column 2, we interact the high-rise peer indicator with an indicator for whether at least one high-rise peer resides in the mover's destination RegSO.[60] The results provide little support for a co-location preference mechanism. The interaction between peer exposure and having a high-rise peer in the destination RegSO is small and statistically indistinguishable from zero, while the baseline peer coefficient remains similar in magnitude. This suggests that the estimated peer effect is not driven primarily by movers choosing destinations in order to live near their peers. Column 3 interacts the high-rise peer indicator with the share of high-rise housing in the destination neighborhood. The results indicate no evidence that the peer effect is systematically stronger in destinations with a higher prevalence of high-rise housing. The interaction between peer exposure and the destination high-rise share is close to zero and statistically insignificant, which is inconsistent with a channel operating through destination neighborhood composition or positional concerns tied to the local high-rise environment. Finally, Column 4 replaces the high-rise peer dummy with the share of peers in the mover's peer group who live in high-rises. The estimates remain statistically significant but are economically modest when expressed in terms of changes in peer group shares.[61] Relative to the discrete peer exposure estimates, this pattern suggests that a social norm channel, as captured by peer group high-rise shares, is unlikely to be the primary driver of the social influence effect in our setting. More broadly, while we cannot fully exclude a role for conformity effects, the heterogeneity patterns across interaction channels, geographic proximity, and prior experience are jointly most consistent with a learning-based mechanism. Overall, these results reinforce the interpretation that

[57] Park (2019) uses a field experiment and shows that people are willing to forego six percent of wage to work with friends. Büchel, Ehrlich, Puga, and Viladecans-Marsal (2020) show that movers prefer neighborhoods where they already know more people nearby.

[58] Bellet (2024) finds that new construction at the top of the house size distribution in a neighborhood lowers the satisfaction that other residents derive from their own homes.

[59] Studying the adoption of new products, Bollinger and Gillingham (2012) show that an additional installation of a solar panel within the same zip code area increases the probability of adoption by 0.78 percent.

[60] We leverage the richness of our data and identify cases where the peers living in high-rises reside in the new RegSO. If preferences to be with friends are an important driver in our results, then we should expect a strong effect if the peers living in high-rise buildings reside in the new RegSO.

[61] Increasing the share of high-rise peers by 1 percent increases the probability of buying a high-rise apartment by barely 0.1 percent.

the main estimates reflect learning from informative peer exposure rather than co-location preferences, destination neighborhood composition, or conformity.

7 Concluding Remarks

This paper studies how individuals make high-stakes investment decisions when they have limited prior experience with a product and must form beliefs about attributes learned through use. Using high-rise housing investment as our empirical setting, we provide causal evidence that social interactions play an important role in shaping adoption decisions. Exposure to peers with relevant experience substantially increases the likelihood that households adopt high-rise housing, consistent with a learning-based mechanism. Event-study evidence shows that adoption increases only after exposure, with no evidence of pre-trends, reinforcing the causal interpretation of our estimates.

We interpret these findings through a framework in which individuals hold imperfect prior beliefs and update them using signals obtained from their social network. The empirical patterns support this interpretation: peer effects are stronger when interactions are more likely and when prior beliefs are less precise. Together, these results point to social networks as a decentralized information system that helps individuals evaluate less familiar alternatives.

Our results speak directly to the ongoing transformation of urban environments. As high-rise living becomes increasingly prevalent, households must adapt to a form of housing that differs from more familiar alternatives. Imprecise or biased beliefs may slow this transition, even when high-rise housing offers objective advantages. Our findings suggest that social learning can mitigate these frictions by facilitating the transmission of experience-based information.

These insights also inform how information about less familiar alternatives diffuses in practice. Interventions that leverage network structure, such as targeting early adoption among socially connected individuals or facilitating the dissemination of experience-based information, may accelerate diffusion. More broadly, information campaigns that address common concerns, such as safety, costs, and daily living conditions, which we document using survey evidence, may help reduce misperceptions and improve the efficiency of investment decisions in environments where key product attributes are learned through use.

More generally, our findings extend beyond the housing market. In many settings, individuals evaluate products or environments with which they have limited direct experience. In such contexts, social interactions can play a central role in shaping adoption patterns, particularly when other sources of information are incomplete or difficult to interpret. Understanding these mechanisms is important for predicting the diffusion of new technologies, products, and lifestyles.

Several directions for future research remain. An important open question is how different sources of information, such as social interactions, market signals, and formal information campaigns, interact in shaping belief formation and adoption. Another is how social networks themselves evolve in response to new environments, and how this evolution affects learning and diffusion dynamics.

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Tables and Figures

Table 1: Survey Results, Key Questions

	N	Percent
Panel A: Talked with friends or not (Q3)		
All Respondents		
Talked (i.e., very often/often/sometimes/rarely)	230	65.90
Never talked	119	34.10
Total	349	100.00
Panel B: Talked about concerns or benefits (Q5)		
Previously not lived in HR		
Concerns	32	25.40
Benefits	94	74.60
Subtotal	126	100.00
Previously lived in HR		
Concerns	14	16.47
Benefits	71	83.53
Subtotal	85	100.00
Overall		
Concerns	46	
would recommend HR or indifferent		91.30
would not recommend HR		8.70
Benefits	165	
would recommend HR or indifferent		97.58
would not recommend HR		2.42
Total	211	100.00

Notes: This table presents survey results for two key questions: (1) whether respondents discussed high-rise living with friends or acquaintances before purchasing their current apartment (Q3, Panel A); and (2) whether those discussions primarily focused on concerns or benefits (Q5, Panel B), reported separately by prior high-rise experience. Panel B additionally cross-tabulates discussion content with willingness to recommend high-rise living to a relocating friend (Q8), where "would recommend HR or indifferent" combines respondents who would definitely recommend, recommend with caveats, or express indifference between building types. The sample in Panel B (211) is smaller than in Panel A (230) because 19 respondents chose not to answer Q5. All survey questions are detailed in Appendix D.

Table 2: Descriptive Evidence from *Boverket*

Dep. Variable: kWh/m^2	(1)	(2)	(3)	(4)	(5)
HR Building $\geq$ 8 floors	-9.735***				
	(0.477)				
HR Building $\geq$ 9 floors		-9.705***			
		(0.619)			
HR Building $\geq$ 10 floors			-10.37***		
			(0.818)		
HR Building $\geq$ 11 floors				-11.67***	
				(1.009)	
HR Building $\geq$ 12 floors					-14.51***
					(1.230)
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Construction year	Yes	Yes	Yes	Yes	Yes
Average value dep. var	125	125	125	125	125
Observations	129,165	129,165	129,165	129,165	129,165
R-squared	0.203	0.202	0.202	0.202	0.202

Notes: This table reports the average differences in annual energy consumption per square meter (kWh/m^2) between high-rise (HR) and low-rise (LR) buildings using a dummy variable taking the value one for HR buildings and zero otherwise, for increasingly stringent definitions of HR buildings. Municipality fixed effects, year fixed effects (for the year the energy declaration was conducted), and the construction year of the building are included in all regressions. The regression sample is all apartment buildings in Sweden at the beginning of 2023. Robust standard errors in parentheses. *p<0.10, **p<0.05, ***p<0.01.

Table 3: Descriptive Evidence from *Hemnet*

	Panel (a)			Panel (b)		
	(1) Bidding/ sqm	(2) Final price/ sqm	(3) Days on market	(4) Bidding/ sqm	(5) Final price/ sqm	(6) Days on market
HR Building, abs. def.	-35.9606***	-152.7797***	1.1068***			
	(4.758)	(9.669)	(0.411)			
HR Building, rel. def.				-27.6629***	-31.4121***	1.8916***
				(4.141)	(9.381)	(0.467)
Floor	Yes	Yes	Yes	Yes	Yes	Yes
Top-floor	Yes	Yes	Yes	Yes	Yes	Yes
Price per sqm. brackets	Yes	No	Yes	Yes	No	Yes
Construction year	Yes	Yes	Yes	Yes	Yes	Yes
Large apartment	Yes	Yes	Yes	Yes	Yes	Yes
Monthly fee	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-RegSO FEs	Yes	Yes	Yes	Yes	Yes	Yes
Average value dep. var.	289	3922	36	289	3922	36
Observations	165,087	165,087	165,087	165,087	165,087	165,087
R-squared	0.463	0.918	0.096	0.463	0.918	0.096

Notes: This table reports average differences in bidding (i.e., the change between asking price and final price) per square meter, final price per square meter, and days on the market between sold apartments in high-rise (HR) and low-rise (LR) buildings, for the absolute and relative definitions of HR buildings respectively, after controlling for a rich set of covariates. All columns include Year-by-RegSO fixed effects. Our control variables are the floor number the sold apartment is located at, a dummy variable indicating whether this floor is the top floor in the building, brackets for final price per square meter (4 brackets are used, but excluded from the final price per square meter estimation), the construction year of the building the apartment is located in, a dummy variable for whether the apartment is bigger than 50 square meters and the apartment's monthly fee to the condominium. The unit of the bidding and final price per square meter variables are inflation adjusted Euros. The regression sample is all apartments in Sweden sold through Hemnet between 2020 and 2022, as detailed in footnote 32. Robust standard errors in parenthesis. *p<0.10, **p<0.05, ***p<0.01.

Table 4: Balance Tests

	(1) Family Income	(2) Avg. Age of Adults in Family	(3) Family Size	(4) Higher Edu. in Family	(5) Env. Edu. in Family	(6) Self-employed in Family	(7) At Least One Parent Not Full-time
HR peer	-0.0928	0.1834	0.0180	0.0056	-0.0033	-0.0017	0.0017
	(1.085)	(0.159)	(0.0188)	(0.0113)	(0.00565)	(0.00619)	(0.00654)
Old neighb FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15283	15283	15283	15283	15283	15283	15283
R-squared	0.346	0.314	0.624	0.329	0.226	0.210	0.222

Notes: This table reports parameter estimates and standard errors (in parentheses) for regressions of family characteristics on the HR peer dummy. All columns include old neighborhood fixed effects, new neighborhood × year fixed effects, peer group size, and number of children in the family. Standard errors are two-way clustered at the old and new neighborhood level. * for p<0.10, ** for p<0.05, and *** for p<0.01

Table 5: Main Results, Broader Sample

	Families with similar housing opportunities in LRs nearby		
Dep. Variable: Probability of moving to HRs	(1)	(2)	(3)
HR peer	0.0290***	0.0258***	0.0223*
	(0.00904)	(0.00947)	(0.0126)
Old neighb FEs	Yes	Yes	No
Origin school FEs	No	No	Yes
New neighb FEs	Yes	No	No
New neighb × Year FEs	No	Yes	Yes
Observations	18,150	18,150	15,536
R-squared	0.279	0.372	0.390

Notes: This table reports parameter estimates and standard errors (in parentheses) for the general sample of mover families, including those who moved out of both low-rise and high-rise buildings. Column 1 includes old neighborhood and new neighborhood fixed effects. Column 2 replaces new neighborhood fixed effects with new neighborhood × year fixed effects, allowing for local time-varying shocks in the housing market. Column 3 replaces old neighborhood fixed effects with origin school fixed effects and restricts the sample to families whose children attend a single school (about 85 percent of the broader sample). This ensures an unambiguous mapping between each family and its school-defined peer group, as the remaining families have children attending multiple schools. All columns include the full set of family, peer, and apartment controls as in Column 4 of Table 6. Standard errors are two-way clustered at the old and new neighborhood level. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Main Results, Primary Sample

Dep. Variable: Probability of moving to HRs	Families with similar housing opportunities in LRs nearby			
	(1)	(2)	(3)	(4)
HR peer	0.0215**	0.0213**	0.0202**	0.0196**
	(0.00912)	(0.00908)	(0.00901)	(0.00904)
Baseline controls	Yes	Yes	Yes	Yes
Controls for family characteristics	No	Yes	Yes	Yes
Controls for peer family characteristics	No	No	Yes	Yes
Controls for apartment attributes	No	No	No	Yes
Old neighb FEs	Yes	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes	Yes
Observations	15283	15283	15283	15283
R-squared	0.394	0.396	0.396	0.396

Notes: This table reports parameter estimates and standard errors (in parentheses) for model (1). HRs and LRs stand for high-rise and low-rise buildings, respectively. All columns include old neighborhood fixed effects and new neighborhood × year fixed effects. Baseline controls include peer group size, number of children in the family, and income brackets. Additional family characteristics are the average age of adults, family size, higher education in the family, self-employed in the family, and at least one parent working less than 40 hours a week. Peer family controls include the average age of adults and family size within peer families, the proportion of peer families with university degrees, self-employed family members, and with at least one parent who does not work full-time. Apartment attributes include apartment size, captured by a dummy variable indicating whether the area exceeded the median value of 85 square meters, and a measure of apartment layout, captured by a dummy variable indicating whether the number of rooms exceeded the median value of three. All regressions also include one dummy indicating whether the family moved to a different neighborhood. Appendix Table A14 reports complete estimation results for controls. Standard errors are two-way clustered at the old and new neighborhood level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 7: Falsification Tests with Future HR Peers

Dep. Variable: Probability of moving to HRs	Families with similar housing opportunities in LRs nearby			
	(1)	(2)	(3)	(4)
HR peer	0.0196**	0.0196**	0.0187**	0.0182*
	(0.00950)	(0.00946)	(0.00939)	(0.00942)
Future HR peer	0.0086	0.0078	0.0067	0.0066
	(0.0110)	(0.0109)	(0.0110)	(0.0110)
Baseline controls	Yes	Yes	Yes	Yes
Controls for family characteristics	No	Yes	Yes	Yes
Controls for peer family characteristics	No	No	Yes	Yes
Controls for apartment attributes	No	No	No	Yes
Old neighb FEs	Yes	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes	Yes
Observations	15283	15283	15283	15283
R-squared	0.394	0.396	0.396	0.396

Notes: This table reports parameter estimates and standard errors (in parentheses) for a modified model (1), $M_{i,t} = \beta \text{HR Peer}_{i,t-1} + \rho \text{Future HR peer}_{i,t+1} + X_{i,t-1}\gamma + \lambda_{\tilde{c},c,t} + \varepsilon_{i,t}$, in which Future HR peer$_{i,t+1}$ equals one if the mover family has at least one HR peer in the year after their move. HRs and LRs stand for high-rise and low-rise buildings, respectively. All columns include old neighborhood fixed effects and new neighborhood × year fixed effects. Baseline controls include peer group size, number of children in the family, and income brackets. Additional family characteristics are the average age of adults, family size, higher education in the family, self-employed in the family, and at least one parent working less than 40 hours a week. Peer family controls include the average age of adults and family size within peer families, the proportion of peer families with university degrees, self-employed family members, and with at least one parent who does not work full-time. Apartment attributes include apartment size, captured by a dummy variable indicating whether the area exceeded the median value of 85 square meters, and a measure of apartment layout, captured by a dummy variable indicating whether the number of rooms exceeded the median value of three. All regressions also include one dummy indicating whether the family moved to a different neighborhood. * p<0.10, ** p<0.05, *** p<0.010

Table 8: Exposure by Peer Children's Gender

Dep. Variable: Probability of moving to HRs	Families with similar housing opportunities in LRs nearby (1)	(2)	(3)
HR peer	0.0379***		
	(0.0120)		
Only opposite-gender HR peers		0.0133	0.0163
		(0.0305)	(0.0305)
Any same-gender HR peer		0.0375***	
		(0.0120)	
Same-gender HR peers: bottom tercile			0.0292**
			(0.0124)
Same-gender HR peers: middle tercile			0.0476***
			(0.0142)
Same-gender HR peers: top tercile			0.0533***
			(0.0167)
Comprehensive controls	Yes	Yes	Yes
Old neighb FEs	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes
Observations	15422	15422	15422
R-squared	0.397	0.397	0.397

Notes: This table reports parameter estimates and standard errors (in parentheses) for model (1). HRs and LRs stand for high-rise and low-rise buildings, respectively. All columns include old neighborhood fixed effects and new neighborhood × year fixed effects. Baseline controls include peer group size, number of children in the family, and income brackets. Additional family characteristics are the average age of adults, family size, higher education in the family, self-employed in the family, and at least one parent working less than 40 hours a week. In this table, we redefine the peer group as same-age children attending the same school. This alternative definition allows us to distinguish peer effects by whether the children of HR peer families are of the same or the opposite gender as the mover family's children. Column 1 applies model specification (1) to this new sample and estimates the average effect of having any HR peer under the expanded peer definition. Column 2 replaces the single HR peer indicator with two mutually exclusive indicators: (i) only opposite-gender HR peers and (ii) at least one same-gender HR peer. Column 3 extends the Column 2 specification and reports the estimated effect of having only HR peer families with opposite-gender children, as well as separate estimates for mover families that have at least one same-gender HR peer family, stratified by terciles of the number of HR peer families. In Column 3, the estimated effect for mover families in the bottom tercile of high-rise peers differs from that for families in the middle tercile at the 10% significance level, and it differs from that for families in the top tercile at the 5% significance level. All regressions include a constant term and one dummy indicating whether the family moved to a different neighborhood while controlling for whether the mover family has children of both genders. Standard errors are two-way clustered at the old and new neighborhood level. *p<0.10, **p<0.05, ***p<0.01.

Table 9: Peer Effects by Geographical Proximity

Dep. Variable: Probability of moving to HRs	Families with similar housing opportunities in LRs nearby		
	(1)	(2)	(3)
HR peer	0.0190**	0.0186**	0.0183**
	(0.00928)	(0.00914)	(0.00905)
HR peer × Same RegSO HR peer	0.0032		
	(0.00878)		
HR peer × Same DeSO HR peer		0.0130	
		(0.0110)	
HR peer × Same grid HR peer			0.0412***
			(0.0135)
Comprehensive controls	Yes	Yes	Yes
Old neighb FEs	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes
Observations	15283	15283	15283
R-squared	0.396	0.396	0.397

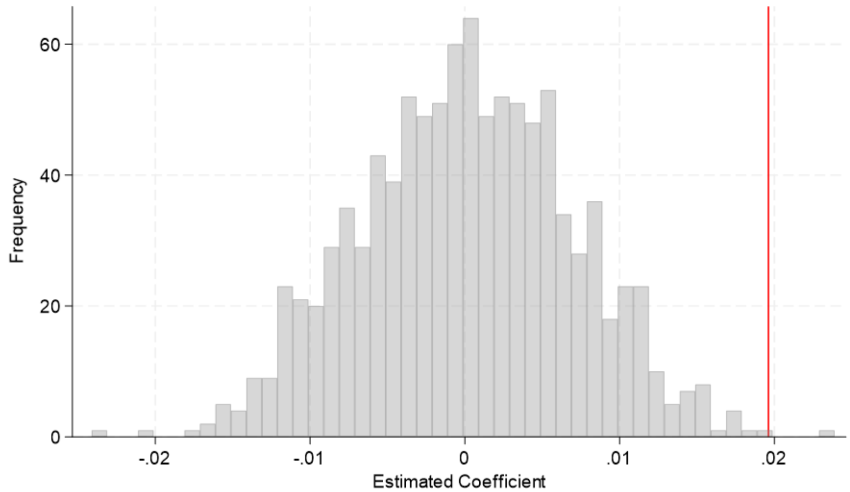
Notes: This table reports parameter estimates and standard errors (in parentheses) for model (1). HRs and LRs stand for high-rise and low-rise buildings, respectively. All columns include old neighborhood fixed effects and new neighborhood × year fixed effects. Baseline controls include peer group size, number of children in the family, and income brackets. Additional family characteristics are the average age of adults, family size, higher education in the family, self-employed in the family, and at least one parent working less than 40 hours a week. Columns 1, 2, and 3 include interactions of the HR peer dummy and a dummy indicating there is at least one HR peer in the same old neighborhood, in which different neighborhood definitions are used where grid being the finest. All regressions also include one dummy indicating whether the family moved to a different neighborhood. Standard errors are two-way clustered at the old and new neighborhood level. * p<0.10, ** p<0.05, *** p<0.01

Table 10: Prior Effectiveness and Uncertainty Resolution

Dep. Variable: Probability of moving to HRs	Families with similar housing opportunities in LRs nearby			
	(1)	(2)	(3)	(4)
HR peer	0.0196**			
	(0.00904)			
HR peer × cross-municipality movers		0.0543**		
		(0.0211)		
HR peer × same-municipality movers		0.0081		
		(0.00902)		
HR peer × cross-county movers			0.1140**	
			(0.0500)	
HR peer × same-county movers			0.0163*	
			(0.00904)	
HR peer × Mover from LRs				0.0217**
				(0.00904)
HR peer × Mover from HRs				0.0059
				(0.0393)
Comprehensive controls	Yes	Yes	Yes	Yes
Old neighb FEs	Yes	Yes	Yes	Yes
New neighb × year FEs	Yes	Yes	Yes	Yes
Observations	15283	15283	15283	18150
R-squared	0.396	0.397	0.397	0.388

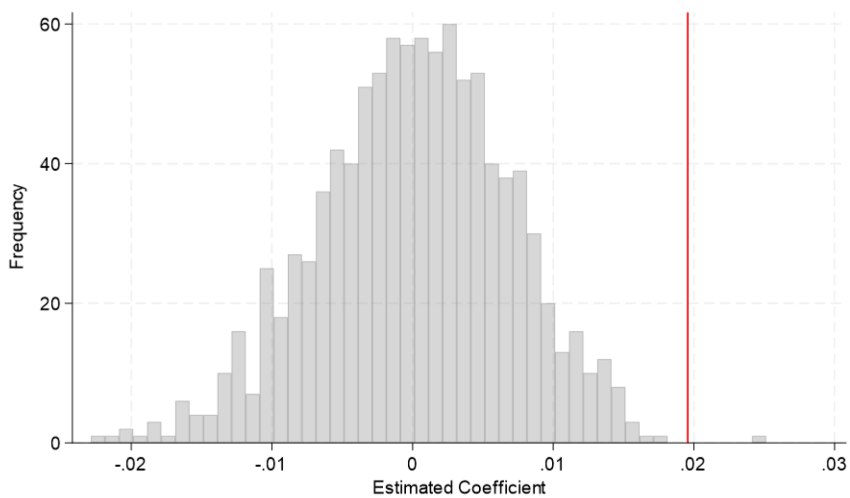
Notes: This table reports parameter estimates and standard errors (in parentheses) for model (1). HRs and LRs stand for high-rise and low-rise buildings, respectively. All columns include old neighborhood fixed effects and new neighborhood × year fixed effects. Baseline controls include peer group size, number of children in the family, and income brackets. Additional family characteristics are the average age of adults, family size, higher education in the family, self-employed in the family, and at least one parent working less than 40 hours a week. Apartment attributes include living area and number of rooms in brackets. Column 1 reproduces Column 4 of the baseline results table. Column 2 analyzes the peer effects separately for cross-municipality and within-municipality mover families by interacting the HR peer dummy with subgroup indicators. The full specification in Column 2 also includes the overall constant term and a dummy variable for families who moved to a different municipality. Column 3 applies the Column 2 specification to analyze the peer effects separately for cross-county and within-county mover families. Column 4 expands the sample to include families who moved out of high-rise buildings. It analyzes the effects separately for families who moved out of HR buildings and those who moved out of low-rise buildings. This separation is achieved by interacting the HR peer dummy with dummy indicators specific to these two types of mover families. The full specification in Column 4 also includes the overall constant term and a dummy variable for families who moved out of high-rise buildings. All regressions also include one dummy indicating whether the family moved to a different neighborhood. Standard errors are two-way clustered at the old and new neighborhood level. * p<0.10, ** p<0.05, *** p<0.01

Figure 1: Placebo Experiments, Random Schools



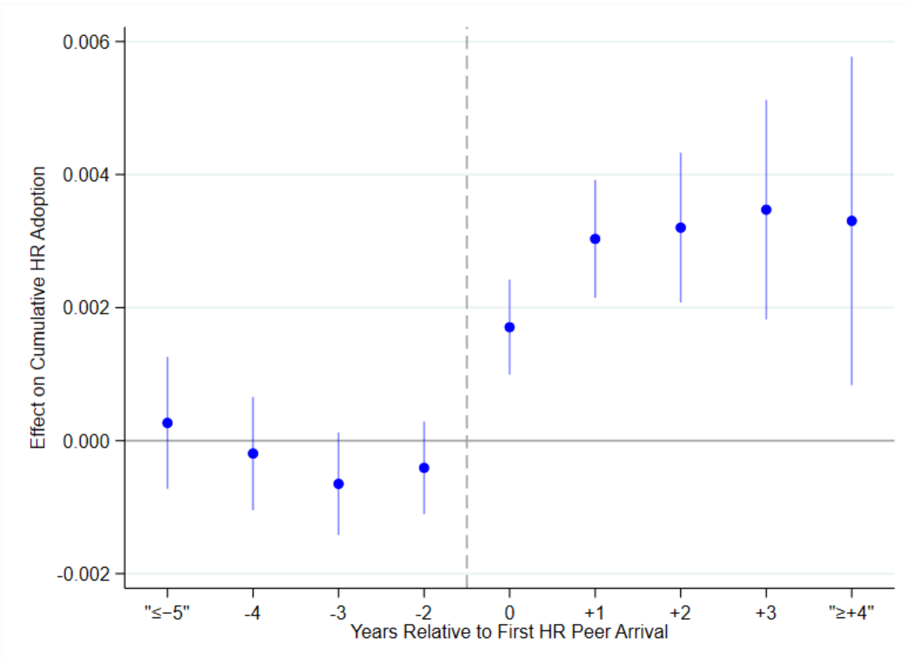
Notes: This figure shows the distribution of coefficients obtained from the specification in Column 4 of Table 6 when the residual of $HR\ Peer_{i,t-1}$ is replaced by the mean residual of a randomly drawn school within the same municipality and added to the fitted value. The solid line represents the actual estimate (0.0196) obtained after removing three observations in municipalities that have too few schools for a thorough randomization.

Figure 2: Placebo Experiments, Random Peer Groups within Schools



Notes: This figure shows the distribution of coefficients obtained from the specification in Column 4 of Table 6 when the residual of $HR\ Peer_{i,t-1}$ is replaced by the mean residual of a randomly drawn peer group within the same school and added to the fitted value. The solid line represents the actual estimate (0.0196) obtained after removing three mover families associated with schools that have too few peer groups for a thorough randomization.

Figure 3: Timing of High-Rise Adoption Relative to First HR Peer Exposure



Notes: This figure plots event-study coefficients from a linear probability model in which the dependent variable equals one if the family has ever transitioned from a low-rise to a high-rise building by year t, and zero otherwise. The sample consists of all families originally residing in low-rise buildings whose peer groups either transitioned from zero to at least one high-rise peer during the sample period (switcher groups, 69,828 families) or never acquired a high-rise peer (never-treated groups, 36,879 families). Families whose peer groups had high-rise members from the start of the observation window are excluded. Event time is centered on the year the peer group acquires its first high-rise peer. The coefficient for the year prior to peer exposure ($t = -1$) is normalized to zero. Families in never-treated groups are pooled into the omitted reference period ($t = -1$). The specification includes old neighborhood and new neighborhood-by-year fixed effects, matching the fixed-effect structure of the baseline estimates in Table 6. Observations are binned at $t \leq -5$ and $t \geq +4$. An indicator for families who returned to low-rise housing after an initial high-rise adoption is included as a control. Standard errors are clustered at the family level. Solid dots represent point estimates, and vertical line segments indicate 95 percent confidence intervals.