The Price of Inattention: Evidence from the Swedish Housing Market *

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[First Version: February 11, 2016]
This Version: December 7, 2017

Abstract

Do behavioral biases affect prices in a high-stake market? We study the role of left-digit bias in the purchase of an apartment. Left-digit bias is the inability to fully process digits after the first, perceiving prices just below a round number (such as $3.99) as cheaper than their round counterpart ($4). Apartments listed at just-below asking prices are sold at a 3-5% higher final price after an auction. This effect appears not to be driven by differences in observables or in real estate agents' behavior. Auctions for apartments listed just-below are more competitive and attract more bidders and bids.

Keywords: Housing market, auctions, inattention, first-digit bias
JEL codes: R31, D44, D83, C78

*Thanks to Matz Dahlberg and Mattias Nordin for their invaluable help during the project. Thanks to Ina Blind, Matz Dahlberg, and Gustav Engström for the Mäklarstatistik dataset, whose purchase was made possible by a Handelsbanken grant. We would also like to thank Stefano DellaVigna, Rosario Macera, and seminar participants at Uppsala University, UCFS, Linnaeus university, ERSA conference, Stockholm School of Economics, Stockholm University, Lund University, London School of Economics, Norwegian School of Economics in Bergen, the European meeting of the Urban Economic Association, IIES Brown Bag, Stockholm Behavioral Workshop for valuable comments and suggestions.

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1 Introduction

Behavioral economics has challenged rational-agent models, showing that decision makers are prone to mistakes (Simon, 1955; Tversky and Kahneman, 1974). Individuals use heuristics or cognitive shortcuts to process large amounts of information, often leading to sub-optimal decisions. Psychologists and economists have shown a variety of such behavioral biases in laboratory experiments and, more recently, in real market settings, such as the financial market, used car sales, auctions, and supermarket purchases (Lacetera, Pope and Sydnor, 2012; Englmaier, Schmoller and Stowasser, 2017; Malmendier and Lee, 2011; Chetty, Looney and Kroft, 2009). Despite growing evidence, we still have a limited understanding of how these biases affect high-stakes markets.

In this paper, we study a form of inattention known as left-digit bias, which refers to the inability of some buyers to process prices correctly. Left-digit bias is the propensity to focus on the leftmost digit of a number, while partially neglecting the other digits. Sellers exploit these biases by setting prices just below a round number (e.g., $3.99), which the buyer perceives to be much lower than the round price ($4). Most of the empirical evidence on the importance of these heuristics comes from settings in which stakes are low. However, in high-stakes markets, rational consumers have strong incentives to avoid heuristics because the potential welfare losses are significant.

We study how left-digit bias affects prices in the Swedish housing market. The housing market offers an ideal setting in which to test the prevalence of heuristics because of its high-stakes and low search costs. First, a home is one of the most important financial assets in a household's portfolio, accounting for as much as two thirds of its total wealth (Iacoviello, 2011).[1] Second, information about units for sale is abundant at a relatively low cost. There are websites that collect and organize information, and provide tools to assist buyers in their search.

Most transactions are mediated by a real estate agent, who advertises the dwelling and manages a public ascending price auction. The most salient element in the process is the asking price that appears in the ad and usually serves as the starting price in the auction. The main focus of this paper is to investigate the effect of inattention to the asking price on the final sale price. To this end, we compare the final prices of apartments with asking prices that are very similar but that differ in the first (or second) digit.

We find that the average final price of comparable apartments drops discontinuously by 3-5% when the first digit of the asking price changes (e.g., from 1,995,000 to 2,000,000 SEK). This change in the final price amounts to about $13,000, equivalent to

[1] Moreover, the size of the housing market has important implications for the aggregate economy, since the value of this market is larger than the stock market (see Shiller, 2014).

five months of the disposable income of the median Swedish household. An effect of this magnitude is difficult to reconcile with models of optimal search or rational inattention. Additionally, when studying the second digit, we find that just-below prices yield a slightly smaller premium of 1.1-3.4%, consistent with the existence of both a first- and a second-digit bias.

We have administrative information on about 350,000 apartment sales, collected by Sweden's real estate agents' association, which covers about 90% of all transactions between January 2010 and December 2015. The data contain a rich set of characteristics of each apartment (e.g., size, number of rooms, floor, etc.), exact address, date of transaction, and asking and final prices. Moreover, we merge this dataset with three other sources of information to test mechanisms. Specifically, we collect data to link sales to real estate agents; we download information from the largest real estate firm's web page to obtain the complete history of bids for all of their auctions; and, finally, we survey real estate agents.

Our empirical strategy relies on estimating discontinuous jumps in the final price as a function of the asking price, assuming local linearity of the conditional expectation function. Because we expect the final price to be a continuous function of the asking price, discontinuities found at each 1 million threshold can be interpreted, in absence of selection, as evidence of first-digit bias. Similarly, the discontinuities at the 100,000 thresholds suggest the existence of second-digit bias. The richness of the data allows us to control for several observable characteristics and an extensive set of fixed effects, to account for seasonality, common macroeconomic shocks, and unobserved amenities in the neighborhood or building. Our estimates are robust to restricting the sample to different years and regions, to varying the bandwidth, and to alternative estimation methods.

To rule out that our effect is driven by endogenous sorting of apartments around the threshold, we use several strategies. First, to ensure that apartments are comparable, we inspect the averages of each observable characteristic around the thresholds. Observables are balanced around the common threshold obtained by pooling all 1 million marks together. When refining the analysis to each 1 million threshold separately, we observe some differences. In most cases, however, these imbalances suggest that we are underestimating the true effect. We also construct a predicted final price using observables and various sets of fixed effects, and show that it does not exhibit any discontinuity at asking price thresholds.

A second form of sorting arises if apartments on either side of the threshold systematically differ in the ability of the real estate agent – for instance because more competent agents use just-below prices more often. We rule out this possibility by including real estate agent fixed effects, which allow us to compare apartments sold by the same agent, thus controlling for her ability and any other unobserved time-invariant characteristic.

Additionally, we inspect the time on the market, defined as the period between the advertisement date and the contract date. First, the time in the market is roughly constant at around 30 days along the whole distribution of the asking price and does not show any discontinuity, suggesting that agents' incentives are the same across the threshold. This is also indirect evidence against both types of sorting described above and is not consistent with the alternative explanation that our result is driven by impatient sellers choosing to list apartments at round numbers as "cheap talk" to signal a weak bargaining position as in, for example, Backus, Blake and Tadelis (2016).

Finally, we perform a set of robustness checks to confirm our hypothesis. First, we rule out that our effect is due to the design of the search engine by exploiting a change in the web interface of the main web portal for apartment ads, *Hemnet.se*. Previously, users of this website could restrict their search by entering a price interval manually. In 2011, the system was replaced by pre-set price brackets that coincide with our 1 million thresholds. A simple diff-in-diffs shows that our baseline effect is the same before and after the reform. Second, we implement the bias correction method proposed by Oster (2016, forthcoming) to correct for potential omitted variable bias. While the estimated effect decreases slightly, it remains large and statistically significant for a reasonable range of values of the parameter that governs the degree of bias. Third, we confirm our main result using an alternative estimation method based on Abadie and Imbens (2006) nearest neighbor matching algorithm.

We propose a mechanism to interpret our results in which inattentive buyers, when deciding which apartment to bid on, perceive those with just-below prices as cheaper, inducing them to participate in these auctions. As a consequence, auctions for apartments with just-below prices have more bidders and receive more bids, leading to a higher final price.[2] Using additional data on over 27,000 completed auctions from Sweden's largest real estate firm, we find that apartments using just-below prices have, on average, 0.72 more bidders and receive 2.7 more bids, in line with our hypothesis. This corresponds to an increase of 25% of the average number of bidders and 30% of the average number of bids per auction, respectively.

Our paper contributes to the behavioral economics literature described by DellaVigna (2009) by documenting that consumers use heuristics even when making important

[2]The final price in ascending price auctions with independent valuations will be the second-highest willingness to pay, which is an increasing function of the number of bidders (Krishna, 2009). Moreover, participants in very popular auctions may be affected by herding effects or the "bidder's heat" and bid above their valuations (Malmendier and Lee, 2011).

decisions, such as buying a home. The closest paper to ours is by Lacetera, Pope and Sydnor (2012), who document the existence of a first-digit bias in the wholesale used car market. In their context, professional dealers have extensive experience from participating repeatedly in the market, but, in general, it is the final consumer who is more likely to suffer from behavioral biases (see, e.g., List, 2003 and List, 2011). In this respect, we contribute by analyzing the behavior of the final consumer in a high-stakes market in which individuals have limited experience. Moreover, we are able to present evidence on some of the mechanisms.

Our results also relate to the literature on behavioral finance. Dwellings are an example of an asset that is indivisible, illiquid, and heterogeneous (Campbell, Giglio and Pathak, 2011), whose price can deviate from fundamentals and be affected by behavioral components, such as loss aversion (Genesove and Mayer, 2001), herd behavior (Bayer, Mangum and Roberts, 2016), and anchoring (Northcraft and Neale, 1987; Bucchianeri and Minson, 2013). Our contribution lies in documenting a behavioral bias in the process of pricing of an asset.

We also contribute to the behavioral industrial organization literature by documenting an anomaly in the search process. The question of how to search for the best alternative among several choices is a central element in industrial organization (see, e.g. Weitzman, 1979; Salop and Stiglitz, 1977; and Varian, 1980). When consumers have little or no experience or when they face complicated pricing schemes, they search too little, get confused by the different price schemes, and switch too seldom from past decisions or default options (Grubb, 2015). Consumers behave in this way because searching and switching are costly, and firms respond by shrouding attributes and hiding information on, for example, add-ons or shipping costs (see, for example, Gabaix and Laibson, 2006, Brown, Hossain and Morgan, 2010). Our paper contributes to this literature by showing that even when search costs are low, consumers appear to suboptimally restrict their search.

Finally, this paper relates to the marketing and real estate literatures on the effectiveness of just-below pricing strategies (Allen and Dare, 2004; Thomas and Morwitz, 2005). Most empirical studies from the housing market use data from negotiations and not from auctions. The evidence in these papers is mixed, possibly because of the difficulties in properly controlling for unobserved apartment and seller traits.[3] Our paper contributes to this literature by i) being able to control for neighborhood and real estate agent unobservables; ii) providing evidence on mechanisms through analyzing the

[3]While, for instance, Palmon, Smith and Sopranzetti (2004) find that just-below asking prices yield lower final prices, Beracha and Seiler (2013) show the opposite result. Recent laboratory evidence suggests that just-below strategies do not generate the highest profits for the seller in bilateral negotiations (Cardella and Seiler, 2016).

auction process, and iii) showing the existence of both a first- and second-digit bias.

At a late stage in the writing of this paper, we became aware of a study similar to ours (Chava and Yao, 2017) who document, using US data, a very small effect of just-below asking prices on the final price (0.1%) and, contrary to our findings, a positive effect on the time on the market. The apparent conflict with our results may stem from the different sale mechanisms in the two markets. In the US, properties are generally sold in private negotiations which, in the vast majority of cases, end below the asking price (see, e.g., Beracha and Seiler 2013). In this setting, the buyer will try to negotiate the price down from the asking price. Therefore, sellers may choose a high asking price to serve as an anchor (see, for example, Northcraft and Neale 1987), in order to limit the scope for significant discounts. If buyers suffer from left-digit bias, they will perceive a just-below asking price as lower than it actually is, hence they will be anchored to a lower price. For example, a fully inattentive buyer would perceive 3.9 as 3, and therefore will start negotiating downwards from 3. On the contrary, a starting price of 4 can serve as an anchor to elicit offers around 4. Therefore, sellers choosing a just-below asking price need to weight the potential benefits from attracting more buyers with the potential downside of setting a weaker anchor. In an ascending price auction, instead, the role of the asking price as anchor is arguably weaker. For example, in US data, the final price is 1-3% lower, on average, than the asking price, while in our data it is 10-13% *higher*, suggesting that the anchoring effect is lower in auctions, possibly because the asking price is less informative about the reservation price. In fact, comparing the distribution of the asking prices in our dataset from the results in Chava and Yao (2017) and Pope, Pope and Sydnor (2015), it appears that just-below asking prices are not nearly as common in the US as in Sweden, suggesting that the sellers recognize that just-below pricing might be a less attractive strategy in negotiations.

2 Background and data

2.1 The Swedish housing market

The Swedish housing market is competitive and liquid. Anyone is able to buy or sell freely, and prices are set by the interaction of supply and demand.[4] Due to a relatively strong economy – only marginally affected by the financial crisis – and an intense immigration flow, housing prices almost doubled between 2005 and 2015.

The vast majority of dwellings are sold in auctions organized by real estate agents, who act as intermediaries between sellers and potential buyers. Agents are in charge of

[4]The rental counterpart, however, operates very differently. Prices are regulated by the government and dwellings are assigned on the basis on a queue system.

advertising the property, organizing open houses, and setting up the auction process, usually in the form of an anonymous, ascending price auction. Agents advertise the properties through local newspapers, descriptive brochures, and web ads. These ads are then usually posted on the agency's website and on a centralized search engine, *Hemnet.se*, that is collectively owned by the real estate agencies.

This search engine offers several tools for narrowing the search according to the needs of the potential buyer, who can specify the city or neighborhood, the type of dwelling (houses, apartments, etc.), a price interval, and some characteristics, such as the number of rooms, bathrooms, living area and monthly fee.[5]

Each ad describes the property, with pictures and information about characteristics (square meters, year of construction, elevator availability, address, etc.), open houses, and the auction starting date.[6] Note that the ad shows the asking price, which is an important element of our analysis, as we will describe shortly. To further facilitate the search, *Hemnet.se* also provides information on past sales, which is easily accessible and can be tailored to the buyer's interests. Moreover, several other websites compile statistics and provide historical data that are released to the public free of charge. As a consequence, potential buyers have access to a large amount of information at an arguably low cost.

Before the auction, agents typically show the property once or twice during open houses and register potential bidders. After the viewings have finished and the auction has started, bidders interact with the real estate agent using several platforms: SMS, email, phone calls or bids placed directly into the web system. In most cases, the whole auction process is also made public on the agency's website.

For our purposes, the most relevant characteristic of a property on sale is its asking price, which the owner and the real estate agent decide together. This asking price, while related to the price that the seller is willing to accept, should not necessarily be interpreted as a reservation price and, rather, serves as a starting point for the auction. In fact, the seller is not obliged to sell, even after receiving offers exceeding the asking price (see Osterling 2016 for a more detailed discussion of the role of the asking price in the Swedish market). Similarly, potential buyers may withdraw a bid and walk away from the auction with no consequences, although this occurs very rarely. There is no fixed auction time, and the bidding continues until the seller accepts an offer.

Real estate agents must hold a government license to be able to act as intermediaries between sellers and buyers. As of 2015, there were 6,700 registered agents in Sweden. The seller pays them a commission, either a fixed amount or a percentage of the final

[5]The monthly fee is a payment, proportional to the size of the apartment, done from the owner to the housing association to cover shared expenses in the building and past mortgages.

[6]For an example of how a typical ad looks see Figure 9 in the Appendix.

price, upon a successful sale.

2.2 Data

We combine three sources of data to perform our analysis. Our main data source is administrative information on the sales brokered by real estate agents. We complement this dataset with information from three other sources. The identity of the real estate agent in charge of each sale was collected from the web page *Hemnet.se*. To obtain information about the auction process, we gathered all the bidding histories of the auctions run by the market-leading real estate firm. Finally, we ran an email survey among real estate agents.

Main Dataset

Our main source is *Mäklarstatistik AB*, a private company that provides transaction data, including sale price, for the housing market in Sweden. According to *Mäklarstatistik*, they cover around 90% of all sales of houses and apartments that are mediated by a broker. The data contain information on the asking price, the final selling price, the date when the ad was posted and the date of the transaction for the period between 2010 and 2015. In addition, we observe a number of characteristics of the dwelling, such as the exact address, the year of construction, living area, number of rooms, number of floor, the presence of an elevator in the building and whether or not the unit has a balcony. For apartments, we also observe a unique housing association identifier and the monthly fee. The housing association, sometimes referred to as the "co-op" is an organization of neighbors that is the formal, legal owner of the apartment block and manages common areas and provide basic services.

In order to be able to compare units that are as similar as possible, we restrict our analysis to apartments units, hence excluding villas, cottages and summer houses. The main reason for this choice is that, by comparing apartments within the same housing association, we are able to control for several potential confounders, such as the architectural style, year of construction, proximity to amenities, and quality of the neighborhood. We also exclude from the sample apartments identified as new construction since they are often sold at fixed prices and not in an auction. Finally, we drop apartments with asking prices greater than 5.5 million SEK because such instances are extremely rare. Additional details on the construction of the final dataset are available in Appendix A. The final dataset consists of 349,476 apartments. The first column of Table 1 shows that the average apartment in our sample has an asking price of 1,514 million SEK (about $165,000) and is sold after the auction at a 10.4% higher price. It is relatively small, with 2.5 rooms (including the living room) and a living area of 66.9 square meters. The monthly fee due to the housing association is, on average, substantial, at 3,600 SEK (corresponding to roughly $400). Finally, the average time between advertising and sale is slightly more than one month.

As Figure B in the Appendix shows, the period covered by our sample is one of expansion. We also observe the substantial seasonal component in both the average asking and sale prices and the large, positive gap between asking and sale price. This gap, amounting to about a 15% increase in 2010, declined substantially to less than 4% in December 2011, when real estate agents committed to set the asking price to numbers close to the seller's reservation price. In recent years, the gap began to increase again and has reached pre-2011 levels.

Hemnet **subsample**

Given that information on real estate agents is absent from the main dataset, we obtain the history of past sales available on the *Hemnet.se* website. This dataset contains the same type of basic information available in the main dataset and, in addition, information on the identity and affiliation of the real estate agent in charge of each sale. To collect the universe of the data, covering the period between late 2012 and 2015, we used a Python script to download the information directly from the web page.

We are able to merge 98,451 transactions with our main dataset.[7] We present descriptive statistics in Panel B of Table 1. On average, the two datasets are comparable with respect to observable characteristics of the dwelling. However, possibly because observations in the second sample are from more recent years, both the asking and the sale price in the latter are higher, on average. In addition, apartments in the subsample appear to be sold slightly faster. In the empirical analysis, we use the identity of the agent to control for all the unobserved traits, such as innate ability, that are time-invariant.

Auctions subsample

Finally, we complement our dataset with a third data source in order to test mechanisms. We gathered detailed information on complete auctions from the real estate agency *Fastighetsbyrån*, which is the largest broker in Sweden, with a market share of 25%. Again, we use a web script to download the information from the agency's web-

[7]The main reasons for the relatively low merge rate are: i) we do not have unique identifiers in the *Hemnet* subsample, so the merge is performed using asking and final price, date of sale, number of rooms, surface area and monthly fee; and ii) the *Hemnet* dataset essentially has no information at all for the years 2010-2012.

site, which includes several complete auction histories containing bids, their timing and a bidder identifier. However, the coverage at the beginning of our sample period is scarce and increases substantially only in 2014 and 2015. We merge this information with our main dataset to obtain apartment characteristics and geographic identifiers, leading to a final dataset of 27,173 complete auctions. Panel C of Table 1 shows some descriptive statistics for apartments in this dataset, together with information on the auction outcomes. Apartments in this subsample were sold more recently and were, on average, slightly more expensive than those in the main dataset, but comparable regarding the other observable characteristics. On average, 2.7 bidders participated in the auction, placing about nine bids.

Real estate agents survey

Our final source of information comes from an online survey to real estate agents. In February 2017, we contacted all real estate agents who had at least one sale in the previous year, as recorded in our dataset. We sent out a total of 4,456 e-mail invitations to participate in a survey, of which 301 were returned with a complete answer.[8] Our main goal was to shed light on whether agents had any belief about the relative advantages of a just-below versus a round-number pricing strategy for the property's visibility, number of interested buyers and final price. We also asked them how often, and why, the seller intervenes in setting the asking price. Finally, we left room for comments. Results from the survey are reported in Appendix B and throughout the paper.

3 Empirical analysis

3.1 Graphical analysis

We start by showing that the discontinuities in the final price around 1 million asking price thresholds are visible from the raw data. In Figure 1, we show the average final price for each bin of size 10,000 SEK (about $1,100) of the asking price for apartments with an asking price around the 1 million mark. The size of each circle is proportional to the number of apartments in each bin.

We immediately notice that a large number of apartments are listed at a price just below the 1 million threshold. However, there is still a significant number of apartments listed at the threshold or just above. The relationship between asking and final price is

[8]Since some addresses were inactive, it is challenging to assess what the actual response rate was. According to the web platform we used for the survey, 103 addresses were invalid; thus, once we removed these, we were left with a 6.9% response rate.

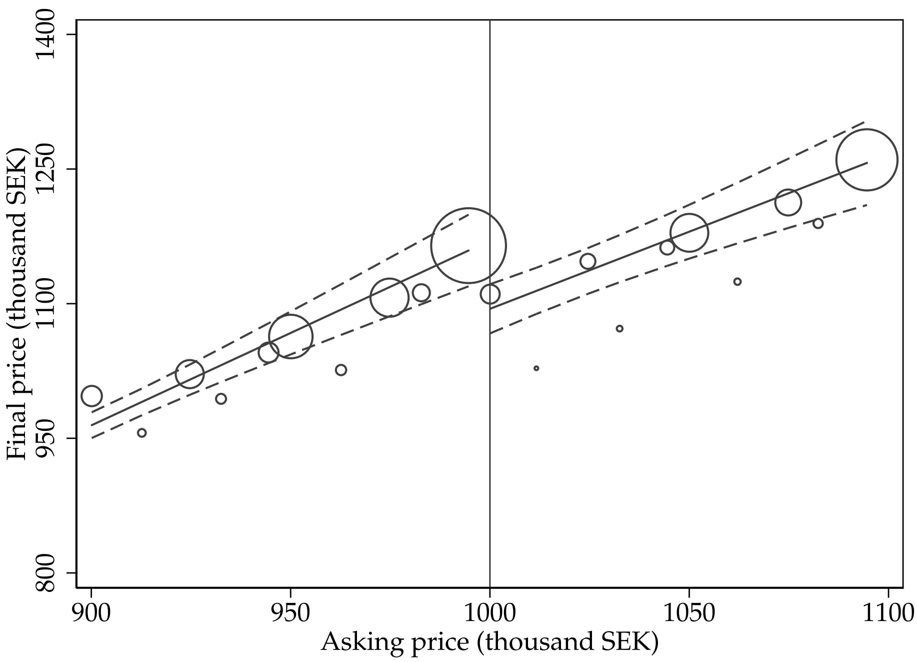
Table 1: Descriptive statistics for the main dataset and the two subsamples

	Main dataset 2010-2015	**Hemnet** 2013-2015	**Auctions** 2012-2015	**Main dataset** 2013-2015
Asking price	1513.7	1608.1	1767.0	1665.4
	(1039.7)	(1061.9)	(1106.5)	(1083.5)
Sale price	1654.4	1790.2	1972.4	1822.1
	(1126.5)	(1163.8)	(1208.9)	(1171.5)
Sale price (per m2)	27.2	30.1	34.0	30.3
	(19.7)	(21.7)	(23.1)	(21.5)
% increase over asking price	10.4	12.8	13.5	10.7
	(15.2)	(15.0)	(15.0)	(14.3)
Days on the market	34.4	29.3	24.5	35.0
	(80.4)	(71.6)	(64.2)	(90.1)
N. of rooms	2.5	2.5	2.5	2.5
	(1.0)	(1.0)	(1.0)	(1.0)
Living area (m2)	66.9	66.1	65.0	66.5
	(23.8)	(23.2)	(23.0)	(23.6)
Year of construction	1963.5	1965.1	1964.9	1964.2
	(28.3)	(27.4)	(28.8)	(28.7)
Elevator	0.5	0.4	0.5	0.5
	(0.5)	(0.5)	(0.5)	(0.5)
Floor	2.4	2.4	2.5	2.4
	(1.8)	(1.7)	(1.7)	(1.8)
Monthly fee	3605.9	3668.0	3568.9	3653.4
	(1361.0)	(1341.3)	(1352.2)	(1353.1)
N. bids			8.9	
			(8.7)	
First bid			1788.2	
			(1127.2)	
N. bidders			2.7	
			(1.8)	
Bid increment (%)			2.2	
			(3.0)	
Observations	349,476	98,451	27,173	189,952

Notes: Prices are in thousand SEK, with 1000 SEK corresponding to roughly $110 as of June 2017. Standard deviations in parentheses. In the first column, the main dataset is used. The second column reports descriptives for the *Hemnet* subsample, for which real estate agent identifiers are available. The third column uses only the subsample for which we also have auction information. Finally, for comparability, the rightmost column uses the main dataset restricted to 2013-2015.

positive and approximately linear at both sides of the 1 million threshold. However, when the asking price crosses the threshold, the final price drops sharply, with an estimated discontinuity of 77 thousand SEK (about $8,500), corresponding to a 7.7% price drop, suggesting that when the first digit of the asking price changes – in this case, from zero to 1 million – the final price is affected negatively.

Figure 1: The discontinuity in final prices around the 1-million asking price threshold

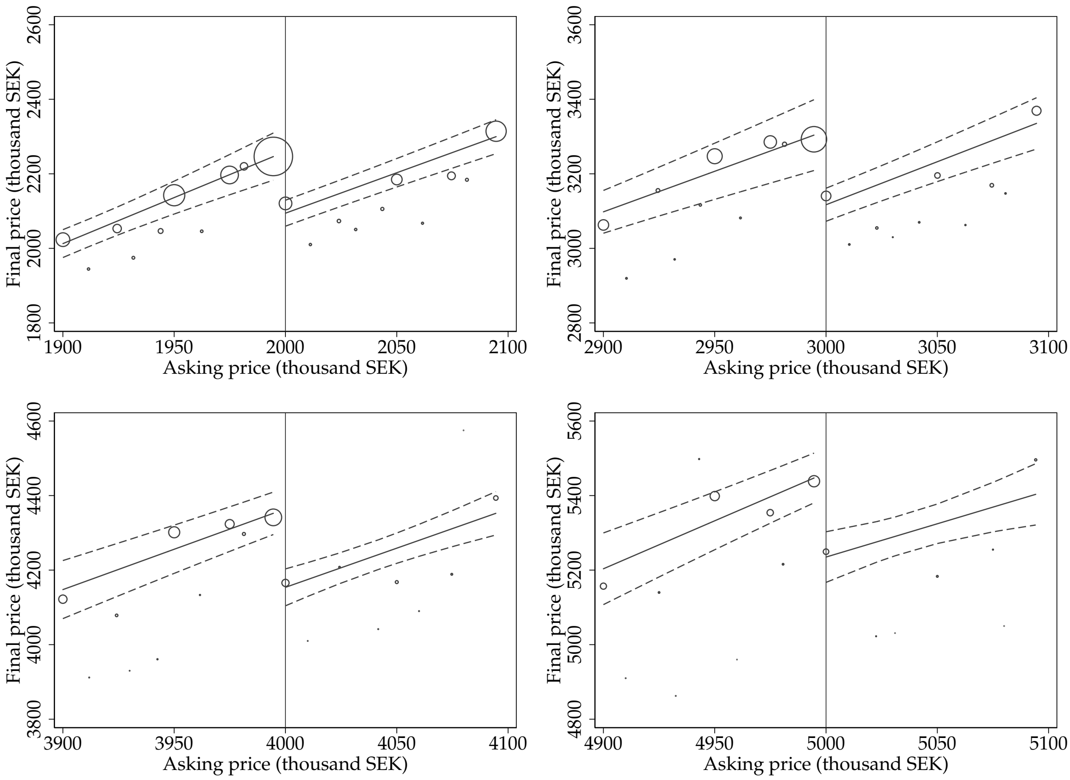


Notes: The figure plots the average final price for each bin of size 10,000 SEK of the asking price for apartments with asking price around the 1 million mark. Circles represent averages in 10,000 SEK bins, and their size is proportional to the number of transactions in each bin. Lines are fitted values from a regression. Dashed lines represent 95% confidence intervals (s.e. clustered at the municipal level).

This discontinuity is not peculiar to the 1 million threshold. Inspecting all the other thresholds reveals a very similar pattern, as Figure 2 shows. The relationship between asking and final price remains approximately linear, and there are sizable discontinuities in the final price also around the 2, 3, 4 and 5 million marks. While these drops in price are even larger in absolute value than the one observed around 1 million, they remain comparable in percentage terms.

Taken at face value, these results suggest that it is profitable to choose an asking price just below 1 million marks relative to using round-number pricing or prices just above. Experienced sellers and real estate agents should take this effect into account when choosing the asking price. Hence, we expect to observe substantial bunching just below each of the 1 million marks. Figure 3 presents the distribution of asking prices,

Figure 2: The discontinuity in final prices around the other 1-million thresholds



Notes: The figure plots the average final price for each bin of size 10,000 SEK of the asking price for apartments with asking price around the 2, 3, 4 and 5 million marks. Circles represent averages in bins of 10,000 SEK width, and their size is proportional to the number of transactions in each bin. Lines are fitted values from a regression. Dashed lines represent 95% confidence intervals (s.e. clustered at the municipal level).

showing the percentage of apartments listed at a given asking price in histogram bins of 20,000 SEK width. There is substantial bunching at several points of the asking price distribution. In particular, there is an excessive density just below even millions and a corresponding "hole" at even millions and just above. Also, bunching occurs around half-millions (dotted lines) and 100,000 SEK round numbers.

When looking, instead, at the distribution of the final prices, the pattern is different. In Figure 4 we show the distribution of the final prices. Contrary to what we observe for asking prices, for final prices, the bunching appears on apartments listed exactly at the threshold or just above (Palmon, Smith and Sopranzetti 2004 have shown this phenomenon using U.S. data). For example, around the 5 million threshold, there are approximately seven times more apartments with a final price between 5 and 5.19 million than between 4.8 and 4.99 million. In the case of negotiations, Pope, Pope and Sydnor (2015) and Backus, Blake and Tadelis (2016) have documented the fact that round numbers serve as focal points. Interestingly, their result also holds in the presence of

Figure 3: Distribution of asking prices, main dataset, 2010-2015

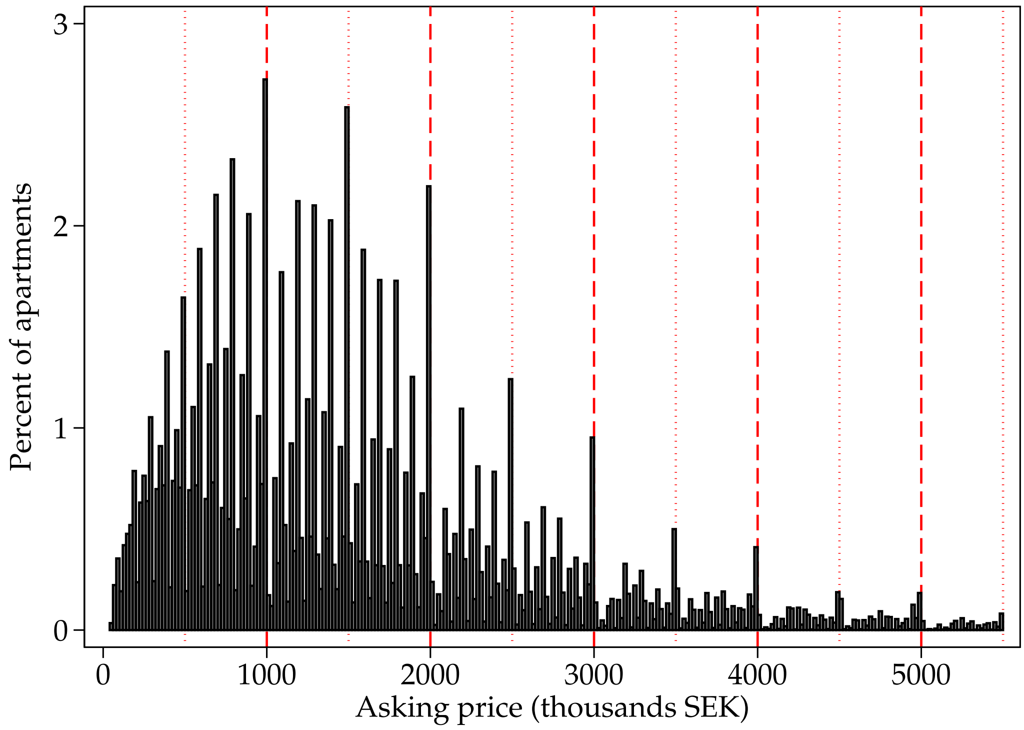
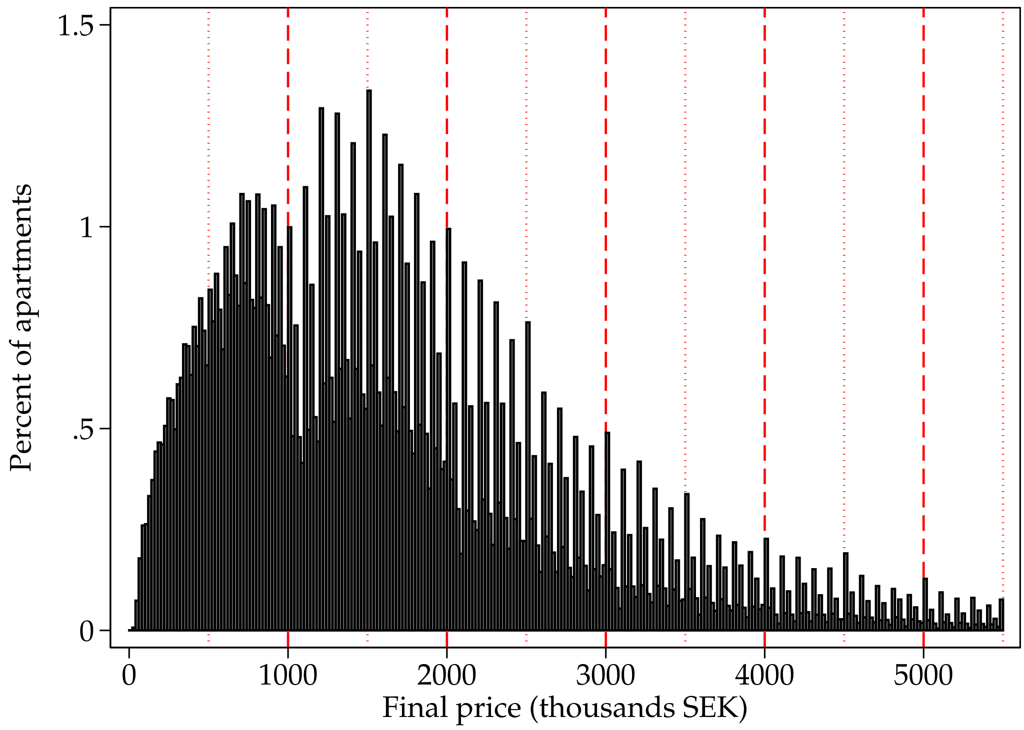


Figure 4: Distribution of final prices, main dataset, 2010-2015



ascending-price auctions.

If apartments at either side of 1 million thresholds are equivalent, the final price discontinuities documented above suggest that buyers are behaving sub-optimally by overpaying for apartments to the left of the threshold. One explanation for this phenomenon is that they are subject to first-digit bias; that is, they incorrectly perceive these apartments as cheaper because they tend to ignore, at least partially, the part of the number to the right of the first digit.

In order to interpret our graphical evidence as the causal effect of the asking price on final prices, however, we must be able to reasonably rule out that there are no unobservable characteristics of apartments that are correlated with the decision to list them at either side of the threshold. For example, this issue arises if some sellers (or real estate agents) systematically choose just-below asking prices because they are more knowledgeable about the housing market or have better apartments. In the following, we will consider these selection issues in detail, but first we must introduce a formal empirical model.

3.2 Regression analysis

A natural way of testing for the presence of left-digit bias in our context is to compare final prices of apartments that have an asking price just below to those listed exactly at a 1 million threshold, but that are otherwise equivalent in terms of characteristics and location. In the absence of such bias, the average final price should reflect the quality of the apartments and, hence, be a smooth and continuous function of the asking price. By contrast, a discontinuity at points where the first digit of the asking price changes could be attributed to the presence of potential buyers suffering from first-digit bias. In order to give these discontinuity estimates a causal interpretation, however, it is important to control for all observed and unobserved determinants of the final price that are correlated with the decision to sort at either side of the threshold.

We start by pooling all observations, assuming that the discontinuity in the final price is the same around all 1 million thresholds:

$$p_i = \beta_j + \gamma \cdot \mathbb{1}(a_i \geqslant c_j) + \theta_j(a_i - c_j) + \phi_j(a_i - c_j) \cdot \mathbb{1}(a_i \geqslant c_j) + \delta' \mathbf{X}_i + \epsilon_i, \qquad (1)$$

where p_i is the logarithm of the final sale price of apartment i, and $a_i - c_j$ is the running variable, defined as the distance between the asking price, a_i, and the j-th relevant threshold, c_j (e.g., 1 million SEK, 2 million, etc.). Because we pool observations around five different thresholds, we include threshold-specific intercepts β_j. The running variable is assumed to have a linear effect on the final price, but the slope can be different at each side of each threshold. Finally, $\mathbf{X}_i$ is a vector of controls and fixed effects, to ensure that we are comparing apartments that are as similar as possible in terms of observable characteristics. The coefficient of interest is γ, which captures the discontinuity in the final price (in percentage terms) as apartments cross any of the 1-million thresholds. Later, we will relax the assumption of a homogeneous effect, presenting results for each threshold separately. A similar approach will also allow us to test for the presence of a second-digit bias by comparing apartments at either side of the 100,000 SEK thresholds.

Although we are effectively estimating discontinuities, our setup differs from the traditional regression-discontinuity design, in which interpreting the discontinuity as a causal effect relies on the assumption that agents are unable to perfectly manipulate the running variable (Lee and Lemieux, 2010; McCrary, 2008). In our setting, the running variable is perfectly manipulable by the seller, something that appears evident by inspecting Figure 3. This systematic bunching of apartments around thresholds can potentially be a consequence of selection, which can take two forms: selection based on apartment characteristics and selection due to real estate agents. To deal with these issues, we will develop several different strategies in Section 4.

We start by estimating model 1 without any controls or fixed effects, pooling observations from all thresholds and restricting the sample to a bandwidth of 100,000 SEK around the common threshold. Column 1 of Table 2 shows that the average drop in final prices at 1-million thresholds is sizable and equal to about 6.4%, consistent with the graphical evidence presented in Figure 1. Adding controls and month-year fixed effects reduces the point estimate to approximately 5.6%, while including municipality-year fixed effects does not alter the magnitude of the coefficient. In column 4, we go further and compare apartments sold in the same parish – an administrative entity smaller than the municipality – in the same year, and the estimate does not vary significantly. Column 5 is the most demanding, as it requires apartments to be sold in the same year, within the same housing association. Housing associations usually include one or two buildings, generally on the same street and often contiguous. Apartments sold in the sample used in Column 5 belonged to 5,552 different associations. Thus, including association-year fixed effects in addition to controls ensures that we are comparing apartments that are effectively very similar in terms of observable and unobservable characteristics.

Although it is standard to assume that the conditional expectation of the final price given the asking price can be approximated by a continuous function, in this case, and despite the fact that the asking price is, in principle, a continuous variable, most asking prices are clustered at multiples of 5,000 SEK. Therefore, the running variable is, in fact, discrete, and we would not observe apartments in a small vicinity of the threshold even

Table 2: The effect on final prices, pooling all thresholds

	Linear specification & bandwidth = 100k SEK				
	(1)	(2)	(3)	(4)	(5)
Above the threshold	-6.45***	-5.63***	-5.63***	-5.54***	-5.13***
	(1.20)	(0.97)	(0.56)	(0.55)	(0.64)
Obs.	57,956	57,788	57,788	57,538	57,455
R^2	0.944	0.952	0.961	0.965	0.990
Controls		✓	✓	✓	✓
Fixed Effects			Year × Municip.	Year × Parish	Year × Assoc.

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK) from equation 1, pooling all 1 million thresholds together and using a bandwidth of 100,000 SEK. We use a local linear control function allowing for different slopes at each side of each threshold. Standard errors are clustered at the municipality level. Controls include living area, the number of rooms, monthly fee, and year of construction, plus different sets of fixed effects. Month-year fixed effects are also included in all columns but the first.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

if we could increase the sample size indefinitely. To address the uncertainty generated by a discrete running variable, one possibility is to cluster the standard errors at each discrete value of the running variable (Lee and Card, 2008). Another natural alternative is to cluster at the municipal level, allowing for correlation in the unobserved component of prices within each municipality of any form. Given that we found that clustering at the municipal level yields standard errors that are three to four times larger, we decided to be conservative and report results for this last specification throughout the paper.

In Figure 5, we explore the sensitivity of our baseline result to different choices of bandwidth by showing point estimates and confidence intervals for bandwidths between 100,000 and 0 in steps of 10,000 SEK. We use the most demanding specification used in column 5 of Table 2, which includes housing association-year effects and standard errors clustered at the municipal level. The estimated effect is remarkably stable across various bandwidth choices. In the limiting case – informally displayed as having zero bandwidth in the figure – we use only observations with a value of the running variable of -5 or 0 – that is, apartments sold at a price 5,000 SEK below (about $550) or exactly at a 1 million threshold. Although the standard errors increase slightly, the point estimate remains virtually unchanged.

3.2.1 Heterogeneity analysis

We now relax the assumption of a common effect and show the effects in separate regressions for each threshold. Regression estimates from this specification show that

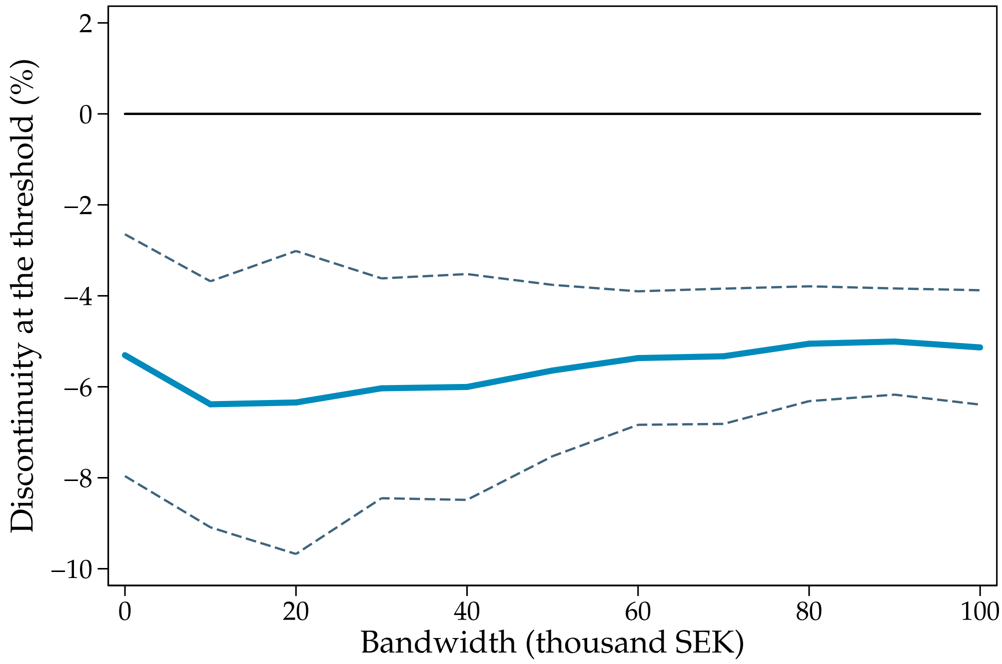


Figure 5: Baseline result - different bandwidths

Notes: The figure shows the effect for different bandwidths using a linear control function, including controls and year - housing association fixed effects. The running variable is the distance between the asking price and the closest threshold (defined as the closest million integer). The control function is allowed to have different slopes for each side of each threshold. Standard errors are clustered at the municipal level. Dashed lines represent 95% confidence intervals.

discontinuities in the final price distribution appear around each 1 million mark. Table 3 shows results for three different samples. Specifically, Panel A reports the estimates for the full sample, while panels B and C split the sample between Stockholm and the rest of Sweden to capture geographical differences. Column 1 presents regression estimates assuming a common effect across thresholds, while columns 2 to 6 show the estimates threshold by threshold. Parish-year, month-year fixed effects and controls are included in all specifications, and clustering is at the municipal level except in Panel B, where we use parishes.[9]

The results for the full sample are entirely consistent with the graphical evidence presented in Figure 2, with the largest discontinuities found, in percentage terms, around the 2 and 3 million thresholds, where they range from 5.9 to 6.4%. Panel B shows that, in general, the effects for Stockholm –the biggest metropolitan area in the country, ac-

[9]Given the reduced sample in some of the specifications in this section, the inclusion of housing association-year fixed effects would be too demanding; hence, we choose to use parish-year effects. Results including association-year effects are, however, qualitatively similar and available upon request.

counting for 30% of the sample – tend to be larger. However, Panel C shows that the discrete jump in the final price at round numbers is not peculiar to this market and also appears to be large in the sample of the other municipalities.

Finally, by estimating the model year by year for each threshold, we explore whether the effect varies over time. Table 15 in the Appendix shows that while point estimates vary in magnitude, the effect is negative and significant in all years. Overall, the evidence in this section shows that the effects appear to be pervasive, and are not unique to a particular threshold, year, or geographical area.

Table 3: The effect on final prices for each threshold, by geographical area.

	Pooled	C=1M	C=2M	C=3M	C=4M	C=5M
	(1)	(2)	(3)	(4)	(5)	(6)
A. Full sample						
Above the threshold	-5.54***	-4.65***	-6.37***	-5.92***	-4.74***	-3.62***
	(0.55)	(0.81)	(0.30)	(0.41)	(0.72)	(0.66)
Obs.	57,538	28,759	16,832	6,999	3,260	1,688
R^2	0.965	0.526	0.478	0.445	0.424	0.442
B. Stockholm						
Above the threshold	-6.31***	-8.61***	-6.39***	-5.91***	-4.04***	-3.51**
	(0.64)	(1.33)	(0.54)	(0.78)	(0.70)	(1.45)
Obs.	17,031	3,240	6,623	3,626	2,240	1,302
R^2	0.96	0.56	0.50	0.44	0.40	0.40
C. Rest of Sweden						
Above the threshold	-4.88***	-4.06***	-6.04***	-5.55***	-7.00***	-6.01
	(0.59)	(0.65)	(0.51)	(1.06)	(1.59)	(3.88)
Obs.	40,507	25,519	10,209	3,373	1,020	386
R^2	0.95	0.49	0.44	0.42	0.51	0.64

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK). In column 1, we pool observations from all thresholds, whereas in columns 2 through 6, we estimate the effect around each individual threshold separately. We use a local linear control function allowing for different slopes at each side of each threshold. Controls include living area, the number of rooms, monthly fee, and year of construction, plus month-year and year-parish fixed effects. Standard errors are clustered at the municipality level in panels A and C and at the parish level in panel B.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

3.2.2 Second-digit Bias

Having established the existence of a significant discontinuity in the final price when the first digit in the asking price changes, we investigate similar effects for similar asking prices that differ in the second digit, such as multiples of 100,000 SEK. The bunching observed in Figure 3 just before multiples of 100,000 SEK suggests that these numbers might, indeed, be relevant.

Following the methodology described in Section 3.2, we re-estimate the baseline model comparing apartments with asking prices that are similar but have different second digits. For instance, we compare units listed at 1,200,000 with those listed at 1,195,000 SEK, and similarly for all multiples of 100,000. To this end, we redefine the running variable appropriately as the distance to the closest multiple of 100,000 SEK.[10] Again, the running variable enters linearly, allowing different slopes at either side of each threshold, but we assume that the discontinuity parameter is common to all thresholds.

Table 4: Second digit bias, for each year and for each million.

	Pooled	0-1M	1-2M	2-3M	3-4M	4-5M	5-5.5M
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Full sample							
Above the thresh.	-3.40***	-2.27***	-3.82***	-3.80***	-3.12***	-2.19***	-2.72***
	(0.50)	(0.32)	(0.71)	(0.39)	(0.21)	(0.23)	(0.32)
Obs.	308,158	120,805	115,601	46,128	16,868	7,313	1,443
R^2	0.98	0.94	0.80	0.70	0.58	0.51	0.46
B. Stockholm							
Above the thresh.	-4.53***	-7.00***	-5.97***	-4.21***	-3.12***	-1.99***	-2.88**
	(0.44)	(1.11)	(0.40)	(0.40)	(0.38)	(0.38)	(1.06)
Obs.	65,946	2,234	26,101	20,930	10,087	5,445	1,149
R^2	0.96	0.70	0.76	0.71	0.56	0.51	0.43
C. Rest of Sweden							
Above the thresh.	-2.72***	-2.18***	-2.92***	-3.22***	-2.80***	-2.69***	-3.51
	(0.30)	(0.31)	(0.36)	(0.53)	(0.58)	(0.78)	(2.52)
Obs.	242,212	118,571	89,500	25,198	6,781	1,868	294
R^2	0.98	0.94	0.80	0.69	0.60	0.50	0.77

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK). In column 1 we pool observations from all 100,000 thresholds, whereas in columns 2 through 7 we estimate the effect for apartments with an asking price between 0 and 1 million, between 1 and 2, and similarly for all millions separately. Controls include living area, the number of rooms, monthly fee, and year of construction, plus month-year and year-parish fixed effects. Standard errors are clustered at the municipality level in panels A and C and at the parish level in panel B.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

To avoid overlap, we restrict the sample to a bandwidth of 50 thousand SEK. Also, we discard observations around those thresholds that coincide with the 1 million marks.

[10] For example, an apartment with an asking price of 1,203,000 SEK corresponds to a running variable of 3,000, and so on.

Table 4 reports estimates including control, month-year, and parish-year fixed effects. Again, in Panel A, we show results for the full sample, while in Panels B and C, we separate Stockholm from the rest of the country. Column 1 shows the effects assuming a common effect around each threshold, while column 2 through 6 show results by grouping apartments by the first digit of the asking price. For example, Column 2 shows the estimated discontinuity obtained by pooling the nine 100,000 thresholds between 0 and 1 million.

Column 1 in Panel A shows that, on average, apartments listed at 100,000 SEK thresholds are 3.4% cheaper than their counterparts just below. Compared to the first-digit discontinuity estimates, the jump around 100,000 SEK is smaller, at slightly more than half the size. This finding is in line with the inattention model in DellaVigna (2009) and Lacetera, Pope and Sydnor (2012), where, in presence of inattentive individuals who discount all digits past the first, one expects to see discontinuities at each digit threshold, with smaller discontinuities for smaller thresholds.[11] Once again, the same conclusions hold for different thresholds and by restricting the analysis to Stockholm or the rest of Sweden, as shown in panels B and C.

4 Sorting around the thresholds

Given that the asking price is essentially a choice variable, it is possible that its choice is systematically related to characteristics of the apartment or the seller. For instance, those who choose a just-below strategy might be sellers with better apartments or more experienced agents. This endogenous sorting of apartments around the threshold is problematic for a causal interpretation of the discontinuity in final prices.

There are two main types of sorting that are relevant in our context. The first is sorting based on apartment characteristics, which arises if sellers with better apartments systematically choose to locate just below a threshold. A second type of sorting stems from real estate agents choosing different pricing strategies based on their ability, with more skilled or experienced agents being aware of the advantages of pricing just below a round number. While entirely ruling out either type of sorting is challenging without conducting a randomized experiment, we can perform several tests that are informative on its importance.

To start, if better apartments are systematically sorted just below a threshold, we should observe a discontinuous jump in observable characteristics when crossing such

[11]This is true regardless of whether one assumes that individuals are equally inattentive to each digit (so that, for example, they always perceive a digit to be a fraction of what it is) or if individuals are assumed to be progressively more inattentive to digits past the first.

a threshold. We show that this is not the case in general. In addition, we use apartment characteristics, location-, and agent-year fixed effects to predict the final price and show that it does not jump discontinuously at the threshold, further reassuring that apartments are comparable in terms of observables. Ruling out that more skilled agents sort around the threshold is more difficult without having a measure of such ability. To tackle this issue, we introduce agent fixed effects in estimation, which allows us to control for ability by comparing apartments at and just below a threshold that are sold by the same agent.

4.1 Sorting on apartment characteristics

To explore the importance of sorting by characteristics in our case, we study how each of the apartment's observed variables differs, on average, at either side of the threshold. Although we are not in an RDD design – so neither continuity of the potential outcomes nor local randomization can be invoked – and our identification relies, instead, on including these variables as controls, it is informative to look at their distribution at the threshold to detect large imbalances that might be a direct consequence of sorting.

In Table 5, we estimate the baseline model (but with no controls or fixed effects), having as the dependent variable each of the covariates that we use as controls in the main specification. Additionally, we use an indicator for the building having an elevator, one for the presence of a balcony, and a variable counting the number of floors.[12] We estimate the model by pooling all thresholds and for each threshold separately. Column 1 shows that, once we pool observations from all thresholds together, none of the seven covariates in our dataset jumps when crossing the threshold, suggesting that, on average, apartments at either side are comparable with respect to all the characteristics we observe.

Columns 2 through 6 show results around each 1 million mark. Out of the 35 possible cases, in 12 we observe a discontinuity, either positive or negative, that is statistically significant at least at the 10% confidence level. Given that our primary concern is that apartments with better characteristics are systematically priced just below the threshold, the problematic cases arise when the imbalance goes in that direction.

Regarding the living area, measured in square meters, we find a negative coefficient around the 3 million threshold that could, at least partially, explain the discontinuity in the final price. However, the situation is exactly the opposite at the 5 million threshold, where we find a positive discontinuity. However, in both cases, the baseline effect on the final price is negative and statistically significant, suggesting that the discontinuity

[12]These variables are not used as controls in our baseline specification because they are missing for a significant fraction of the observations.

Table 5: Balance of covariates

	Pooled	C=1M	C=2M	C=3M	C=4M	C=5M
	(1)	(2)	(3)	(4)	(5)	(6)
Squared meters	-0.60	2.27	-2.47	-5.69***	-1.52	5.40**
	(0.90)	(1.67)	(1.51)	(1.51)	(1.88)	(2.09)
Obs.	57,918	28,925	16,932	7,066	3,289	1,706
Year Constr.	1.46	3.52*	-1.40	1.38	0.30	4.55*
	(1.39)	(1.99)	(2.39)	(1.93)	(2.01)	(2.29)
Obs.	57,956	28,939	16,946	7,074	3,291	1,706
No. Rooms	-0.032	0.074	-0.11	-0.21***	-0.023	0.11**
	(0.035)	(0.067)	(0.069)	(0.042)	(0.074)	(0.048)
Obs.	57,893	28,913	16,921	7,064	3,291	1,704
Monthly fee	-31.7	99.4	-90.0	-232.6***	-163.9**	112.5
	(41.0)	(85.3)	(76.7)	(64.4)	(62.1)	(86.6)
Obs.	57,841	28,907	16,912	7,047	3,275	1,700
Floor	-0.053	-0.15**	-0.089	0.32***	-0.24	0.25**
	(0.047)	(0.061)	(0.075)	(0.077)	(0.17)	(0.11)
Obs.	48,118	24,498	14,049	5,673	2,569	1,329
Elevator	-0.0092	-0.022	-0.010	-0.014	0.045***	0.019
	(0.017)	(0.019)	(0.026)	(0.023)	(0.017)	(0.016)
Obs.	50,649	24,614	14,852	6,460	3,086	1,637
Balcony	0.010	0.010	-0.017	0.060	0.024	0.033
	(0.022)	(0.030)	(0.027)	(0.055)	(0.059)	(0.061)
Obs.	18,857	9,623	5,689	2,183	937	425

Notes: Regression estimates of the effect of the asking price on different apartment characteristics. We report the coefficients of an indicator for the asking price being equal or above the threshold. In column one we pool observations from all thresholds, whereas in columns 2 to 6 we estimate the effect around each individual threshold separately. We use a local linear control function allowing for different slopes at each side of each threshold. No controls or fixed effects included. Standard errors are clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

does not vary with the degree of imbalance. We observe a similar pattern with other covariates.

Apartments at the threshold appear to be, on average, slightly older, although in most cases the difference is not significant. The effect of this variable on final prices is, however, ambiguous, as older apartments may be poorly maintained but are often more centrally located. The monthly fee is, in general, balanced, and the negative coefficients at the 3 and 4 million thresholds indicates that apartments at the thresholds could actually better that those just below. In this case, the imbalance suggests that our baseline effect is underestimated. The number of rooms is well balanced, on average. Again, we find a negative coefficient at the 3 million threshold and a positive one at the 5 million one. Some imbalances also appear in the floor variable, although the direction of its effect on the final price is also unclear. The elevator variable exhibits only one significant, positive, coefficient that again goes against finding our effect, while the balcony indicator is always balanced.[13]

If we consider each million threshold independently, results suggest that there is no selection on observable characteristics around the 2 million mark. The 1 and 4 million marks show a small amount of negative sorting that suggest that our baseline estimates might be a lower bound. Finally, thresholds 3 and 5 shows significant differences in four characteristics out of seven. For the 3 million mark, two of those four imbalances are worrisome. Apartments just below this threshold are larger than their threshold counterparts. However, they also have a more expensive monthly fee, so the two effects might compensate each other. For the 5 million mark, apartments just below the threshold are smaller and older and are located on a lower floor. Again, these differences would imply an underestimation of the true effect.

In sum, the analysis of the covariates in Table 5 does not suggest a clear sorting in characteristics around the threshold, on average. Reassuringly, as Table 2 shows, we find large and negative discontinuities in the final price around all thresholds, even when the imbalanced covariate would suggest a positive effect. Is is also worth remembering that we are controlling for these covariates in the estimation.

As a complement to this analysis, we predict the (logged) final price using our baseline covariates and different sets of fixed effects. Then, we use the fitted values from this regressions as the dependent variable in our baseline model's equation 1.[14] This allows us to investigate whether the predicted final price jumps discontinuously at 1-million thresholds of the asking price. Because their effects are already accounted for in the prediction stage, we do not include any controls of fixed effects in the second stage. Finding a negative and significant effect would be evidence that apartments just below the threshold are systematically better in terms of observable characteristics or geographical location. Reassuringly, table 6 shows that in all specifications the predicted final price does not significantly differ at either side of the threshold. When including housing association-year fixed effects, in column 4, the point estimate is equal to -0.63%, suggesting that differences in observable characteristics or in attributes controlled by

[13] Results around 100,000 SEK thresholds are similar and reported in Table 17 in the Appendix.

[14] A similar approach to test for covariate balancing has recently been used by Kirkeboen, Leuven and Mogstad (2016).

Table 6: The effect of the asking price on the predicted final price

	Linear & bw=100				
	(1)	(2)	(3)	(4)	(5)
Above the threshold	-1.00	-0.67	-0.37	-0.63	0.12
	(0.68)	(1.09)	(1.17)	(0.99)	(1.19)
Obs.	57,788	57,788	57,788	57,788	16,499
R^2	0.14	0.13	0.10	0.11	0.09
Hedonic regression includes:					
Controls	✓	✓	✓	✓	✓
Fixed Effects	-	Year × Municip.	Year × Parish	Year × Assoc.	Year × Agent

Notes: Estimates from equation 1 using, as dependent variable, the predicted logged final price, pooling all 1 million thresholds together and using a bandwidth of 100,000 SEK. We use a local linear control function allowing for different slopes at each side of each threshold. No controls or fixed effects are included in these specifications. The predicted final prices are obtained as the fitted values from a hedonic regression of the final price on our controls, month-year effects (only for columns 2 through 5), and different sets of fixed effects, as specified in each column. Standard errors are clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

the fixed effects are unable to explain our baseline effect. It is worth noting that real estate agents' answers to our survey questions indirectly confirm these results. When asked whether they think that apartments sold at either side of the threshold differ in their market value, most answered negatively (see Table 14).

Of course, given that apartments are complex, multifaceted goods, we would be unable to control for all possible characteristics, regardless of how detailed the data are. For example, even apartments of the same size and floor in the same building might differ because of exposure to sunlight or interior design. However, failing to control for these more subtle determinants of price would lead to biased estimates only if those characteristics were systematically related to being located at either side of the threshold. This possibility may arise if, for example, better agents, who understand pricing strategies, systematically get involved in sales of better apartments. We consider this possibility in the next section.

4.2 Selection on real estate agents' characteristics

Even when comparing apartments that are very similar in location and characteristics, a concern arises if real estate agents representing apartments at the threshold or just below differ systematically in their ability or effort. If more sophisticated agents know of the potential higher gains from setting the asking price just below a round number (while naïve ones do not), it is possible that more competent agents will systematically

Table 7: The effect on sale prices, controlling for differences in real estate agents

	Linear & bw=100					
	(1)	(2)	(3)	(4)	(5)	(6)
Above the threshold	-3.32***	-3.07***	-3.84***	-3.05***	-3.11***	-2.96***
	(0.85)	(0.76)	(1.26)	(0.77)	(0.78)	(0.92)
Obs.	16,499	16,460	16,474	16,499	16,217	16,217
R^2	0.96	0.97	0.99	0.97	0.97	0.98
Controls	✓	✓	✓	✓	✓	✓
Fixed Effects	Year × Municip.	Year × Parish	Year × Assoc.	Year × Agency	Agent	Year × Agent

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK) from equation 1, pooling all 1 million thresholds together and using a bandwidth of 100,000 SEK. We use a local linear control function allowing for different slopes at each side of each threshold. Standard errors are clustered at the municipality level. Controls include living area, the number of rooms, monthly fee, and year of construction, plus different sets of fixed effects. Month-year fixed effects are also included in all columns.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

mediate sales of apartments just below the threshold. As a consequence, the discontinuity would capture not only the effect of a round asking price, but also differences in agents' quality. To rule out this concern, we downloaded all the historical transaction data available in the *Hemnet* website, comprising information on transactions from late 2012 onwards. This dataset allows us to assign, for a subsample of transactions, information on the identity of both the real estate firm that managed the sale and of the agent in charge. As Table 1 shows, the subsample compares reasonably well with the main dataset.

Using the *Hemnet* subsample, the first three columns of Table 7 replicate our baseline specifications with municipal-year, parish-year and housing association-year fixed effects that we estimated in columns 3 through 5 of Table 2, respectively. The estimated jump at the threshold for the most demanding specification with association-year fixed effects is -3.8%, which is smaller than the full sample estimate of -5.1% but consistent with the fact that the effect is smaller in more recent years (see Table 15 in the Appendix).

Taking this estimate as our baseline, we proceed to control for the identity of the real estate agency by including agency-year fixed effects. Column 4 shows that, when requiring apartments to be sold by the same agency in the same year, we still observe a sizable discontinuity of -3.05%. In column 5, we include identifiers for each real estate agent, hence controlling for all unobserved characteristics of the agent that are fixed over time. Even so, the point estimate remains large and statistically significant at −3.11%. The most demanding specification is the one in column 6, where we in-

Table 8: The effect around 100,000 SEK thresholds, controlling for differences in real estate agents

	Linear & bw=100					
	(1)	(2)	(3)	(4)	(5)	(6)
Above the threshold	-1.44***	-1.46***	-1.57***	-1.55***	-1.16***	-1.09***
	(0.40)	(0.31)	(0.40)	(0.32)	(0.35)	(0.33)
Obs.	86,721	86,561	86,534	86,721	85,306	85,306
R^2	0.98	0.98	0.99	0.98	0.98	0.98
Controls	✓	✓	✓	✓	✓	✓
Fixed Effects	Year × Municip.	Year × Parish	Year × Assoc.	Year × Agency	Agent	Year × Agent

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK) from equation 1, pooling all 100,000 SEK thresholds together and using a bandwidth of 50 thousand SEK. We use a local linear control function allowing for different slopes at each side of each threshold. Standard errors are clustered at the municipality level. Controls include living area, the number of rooms, monthly fee, and year of construction, plus different sets of fixed effects. Month-year fixed effects are also included in all columns.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

clude agent-year interactions. Here, estimation relies solely on variation across sales completed by the same agent in the same year; yet the point estimates are essentially unchanged.

In Table 8, we report analogous estimates for the second-digit bias. We pool all 100,000 SEK thresholds and allow for the asking price to have a linear effect – possibly with different slopes – at each side of each threshold. The point estimates using this subsample are, as in Table 7, smaller than the ones using the full sample. Interestingly, however, we note that the average discontinuity around 100,000 thresholds is between half and one third of the size of the one found around the 1 million marks. After including real estate agent fixed effects, the effect decreases to about −1.1% but remains statistically significant at the 1% level.

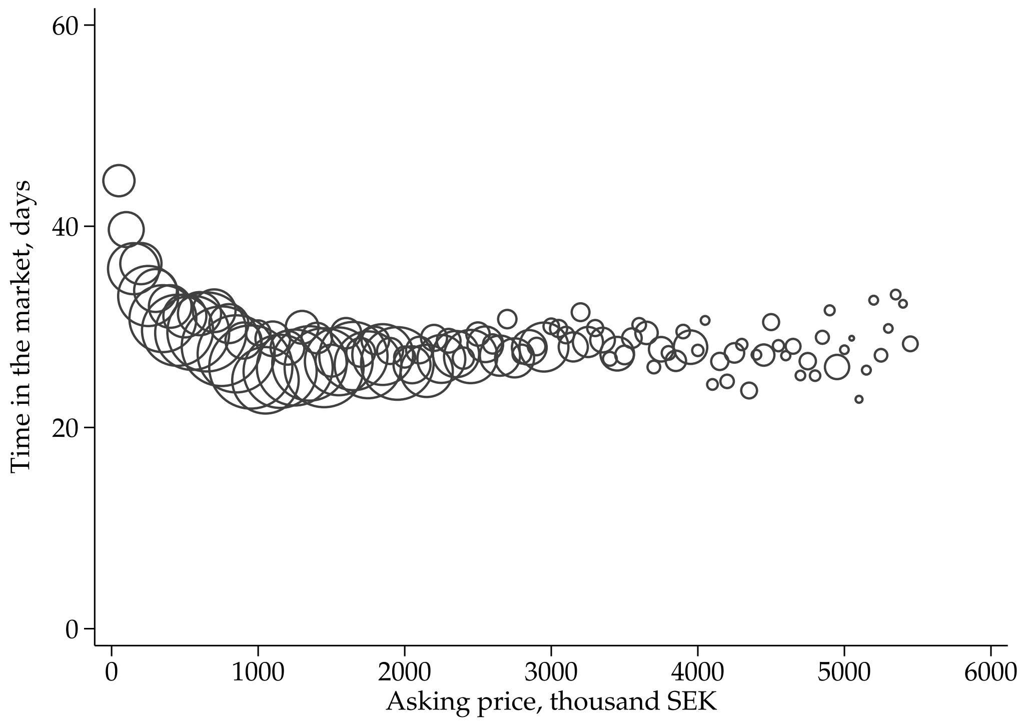
The results from Tables 7 and 8 are reassuring for our concerns about selection based on agent quality. First, even when including progressively more demanding sets of fixed effects by controlling for agent-year indicators, the discontinuity estimate preserves its sign and remains statistically significant. Furthermore, its magnitude is essentially unchanged, suggesting that if agents differ in terms of quality and effort, these differences are not related to sorting around the threshold. A further piece of evidence in this direction is given by column 5 of table 6, in which we first predict the final price using covariates and real estate agents fixed effects, and then use this predicted price instead of the final price in our baseline model. The estimated discontinuity at round-number thresholds of the asking price is, in fact, positive and very small, suggesting that apartments at either side of the threshold are not different in terms of agents' quality.

Naturally, these approaches can only control for the unobserved characteristics of real estate agents – such as effort and ability – as long as they can be thought of as being roughly fixed over time. However, it is still possible that the same agency (or their agents) change the pricing strategy depending on the quality of the apartment in a way that changes over time and is correlated with effort. If this were the case, agency fixed effect would not completely control for this unobservable confounding factor.

4.3 The time on the market

The final price is not the only relevant outcome of the sale. In fact, some sellers could choose the asking price not to maximize profit but to sell as quickly as possible (Levitt and Syverson, 2008). In Figure 6, we plot time on the market – measured as the number of days between the advertising date and the signing of the contract – against the asking price, grouped in 10,000 SEK bins.

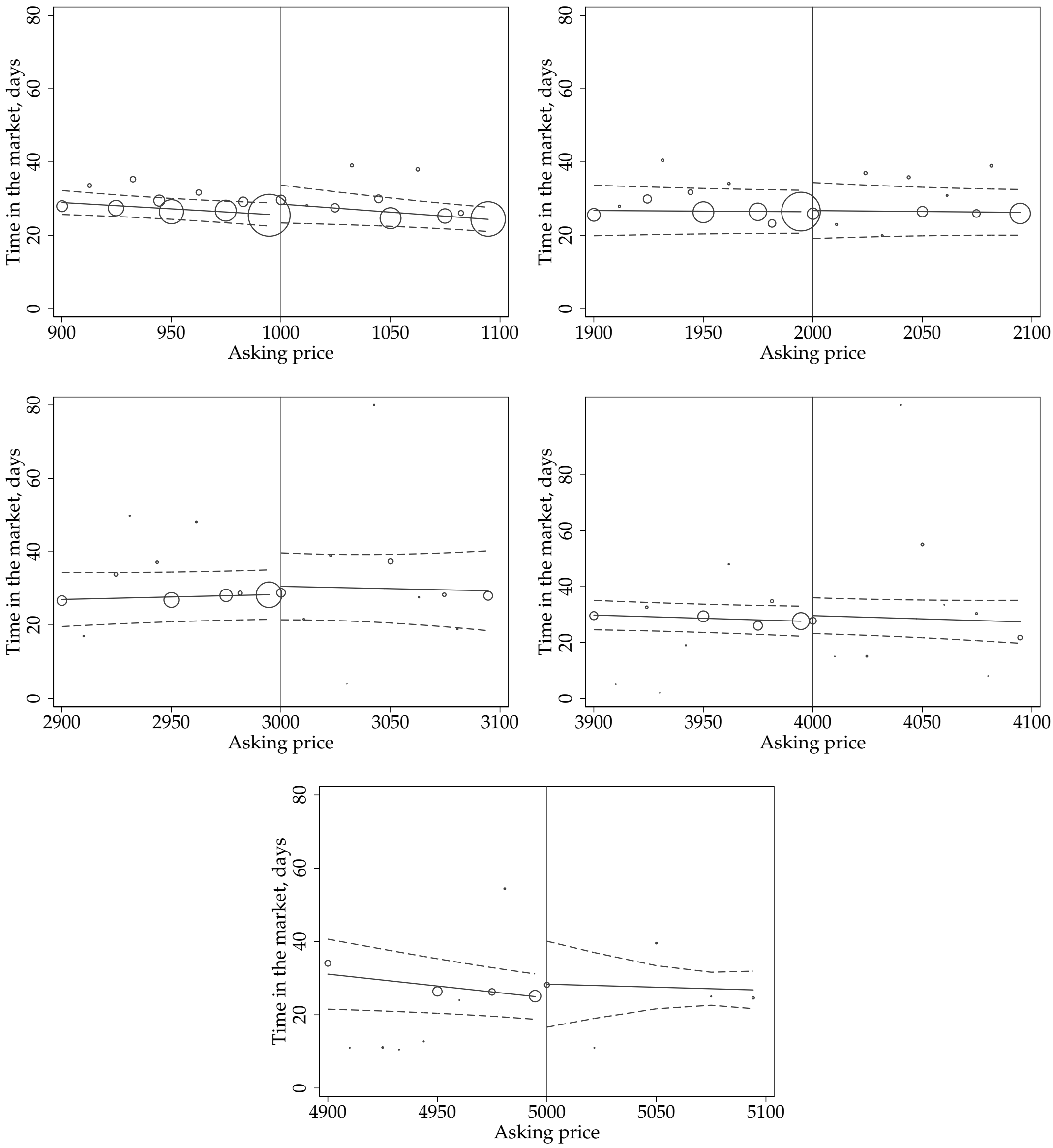
Figure 6: Time in the market and asking price



Notes: Time on the market (measured in days), average in 50,000-SEK bins of the asking price. Main dataset (2010-2015).

Cheaper apartments tend to sell more slowly, but the relationship between time on the market and asking price flattens around 1 million SEK, and stays remarkably stable at around 30 days, even at higher prices. While the graph suggests that the time on the market is essentially unrelated to the asking price, it is interesting to investigate its behavior in more detail around the 1 million marks for the presence of discontinuities.

Figure 7: Discontinuity estimates in time on the market around each 1 million threshold



Notes: The figure plots the average time on the market (in days) for each 10,000 SEK bin of the asking price for apartments with an asking price around each 1 million mark. Circles represent averages in 10,000 SEK bins, and their size is proportional to the number of transactions in each bin. The observations above the 99^{th} percentile of the time on the market (equal to 340 days) are excluded from the graph for visualization purposes. Lines are fitted values from a regression. Dashed lines represent 95% confidence intervals (s.e. clustered at the municipal level).

Figure 7 shows the average time on the market around each threshold using the same procedure described in Section 3.1.[15] There is no appreciable difference at either

[15]To aid visualization, observations exceeding the 99^{th} percentile (equal to 340 days) are excluded from

side of the threshold around each 1 million mark, with the average time on the market being stable around 30 days in all cases. Although the time on the market is an outcome of the auction process, rather than a characteristic of the apartment, the results in Figure 7 are informative because they show that just-below asking price strategies do not affect how quickly the apartment will be sold, hence indirectly providing additional evidence against both types of sorting detailed earlier. In fact, if apartments just below the threshold had better characteristics, or if their agents were more skilled, we may expect them to sell more quickly, but we do not observe such a pattern in the data.

5 The role of inattention in the housing market

The evidence in the previous section shows that there are large discontinuities in the final price when the asking price crosses either a 1 million or a 100,000 SEK threshold. In the absence of sorting of the type described in Section 4, these results imply that buyers overpay for apartments with an asking price just below round numbers. One possibility is that they do so because they are partially inattentive to the asking price. Buyers who tend to focus mainly on the leftmost digit of the price may perceive apartments listed just below a round number as being cheaper than those listed exactly at a round number. If the asking price is a salient characteristic, sellers can choose it to be just below round numbers to induce inattentive buyers to believe that the apartment is cheaper than the competition.[16] In principle, given that an auction determines the final price, it is not immediately obvious that inattention to the asking price should have a significant effect on the final price. However, inattentive buyers, when deciding which apartments to view, may disproportionately choose to visit those with an asking price just below the threshold because they appear cheaper. Therefore, more prospective buyers will view these apartments and, as a consequence, more bidders will participate in the auction.

The final price in an ascending price auction corresponds to the second highest willingness to pay which, under reasonable assumptions, is an increasing function of the number of bidders N. As an illustrative example, consider the case in which bidders have independent willingnesses to pay x, distributed uniformly over the interval $[0, \overline{x}]$. The expected final price of the auction, $E[P_N]$, is the second highest order statistic equal to

$$E[P_N] = \frac{N-1}{N}\overline{x},$$

the graphs. Including these observations does not change the conclusions in this section but hinders clarity.

[16] This phenomenon is in line with the theoretical predictions by Bordalo, Gennaioli and Shleifer (2016), where, in the presence of a salient characteristic – that is, one that consumers overvalue in their decisions – sellers compete for buyers' attention by emphasizing either quality or price.

Table 9: Threshold discontinuities in auction outcomes

	Final price	N.bidders	N.bids	Bids per bidder
A. Effect at 1-million SEK thresholds				
Above the threshold	-6.12***	-0.72***	-2.70***	-0.58***
	(1.01)	(0.082)	(0.46)	(0.13)
Obs.	4,822	4,822	4,822	4,822
R^2	0.97	0.26	0.25	0.21
B. Effect at 100 thousand SEK thresholds				
Above the threshold	-4.24***	-0.45***	-2.33***	-0.49***
	(0.31)	(0.043)	(0.22)	(0.053)
Obs.	23,738	23,738	23,738	23,738
R^2	0.98	0.19	0.17	0.12
Controls	✓	✓	✓	✓
Fixed Effects	Year × Parish	Year× Parish	Year× Parish	Year× Parish

Notes: Panel A shows discontinuity estimates from equation 1 around 1 million SEK thresholds for the logarithm of the final price, the number of bidders and bids, and the number bids per bidder, respectively, using a local linear control function and a bandwidth of 100,000 SEK at each side of each threshold. Panel B reports similar estimates for 100,000 SEK thresholds, using a bandwidth of 50,000 SEK. Standard errors are clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

which is an increasing function of the number of bidders. It is challenging to directly test the hypothesis that the effect we find is due to inattentive buyers participating in auctions for apartments listed just below a round number. However, there is an important implication that we can test. Specifically, we should observe that apartments with an asking price just below the 1 million (or 100,000) marks attract more bidders and more bids. To test this hypothesis, we use the subset of the data for which we have full auction information (described in Section 2.2).

In Panel A of Table 9, we start by estimating our baseline model with parish-year fixed effects. As column 1 shows, the estimated discontinuity around 1-million thresholds is -6.12%, slightly larger than what we found using the full sample. Columns 2-3 show that apartments listed just below the threshold attract, on average, 0.72 more bidders and 2.7 more bids than those listed exactly at the threshold, which is consistent with the hypothesis that inattentive buyers disproportionately participate in auctions for apartments listed just below 1 million thresholds. Additionally, these bidders make, on average, 0.58 more bids, again suggesting that the competition for these apartments is fiercer.[17]

[17] The presence of more bidders may also affect the final price indirectly by triggering other mechanisms that are positively correlated with N, such as herding behavior (Simonsohn and Ariely, 2008) or “bidder’s

Panel B shows the same result for 100,000 SEK thresholds. Consistent with the evidence in Section 3.2.2, inattention to the second digit also appears to be present – although the magnitude is, as expected, smaller – and apartments listed just below multiples of 100,000 SEK also attract fewer bidders and bids.

While it should be noted that these results might also be consistent with other hypotheses – for example, they could be partially driven by selection in case we were unable to control for all confounders – they suggest that auctions for just-below apartments are more competitive. Our interpretation that inattentive buyers are disproportionately participating in just-below auctions is also supported by evidence from the real estate agents' survey. To elicit agents' beliefs on the effect of the two pricing strategies, we pictured the following hypothetical scenario where there was no selection bias:

"Suppose that two identical objects, A and B, are sold at the same time in the same area. The asking price is 1,995,000 SEK for object A and 2,000,000 SEK for object B."

Then, we asked which object would perform better in the market in terms of time on the market, web ad clicks, number of bids, open house visitors, and final price (see question 6 in Table 13 in the Appendix). Virtually no agent believed that the apartment listed at exactly 2 million would do better. Instead, roughly a third of agents expected the just-below strategy to yield a faster sale and a highest price, with the rest expecting no difference. As much as two-thirds, however, anticipated an increase in the web ad views.

Additional evidence from the survey also corroborate our hypothesis of no sorting on apartment characteristics. In question 7 (reported in Table 14), we asked how much they agreed with the statement that apartments listed just below (or exactly at) a round number threshold were artificially under-priced. Interestingly, only 24% agreed or strongly agreed that just-below apartments were under-priced, while 14% suspected it for apartments listed exactly at the threshold.[18] Rather, as many agents wrote in the comments to the survey, the reason for just-below pricing lies in the presence of "psychological effects" and the fact that they "sound cheaper", often drawing the analogy with supermarket pricing schemes such as 99 cent pricing.

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heat" (Malmendier and Lee, 2011). Attracting more bidders to an auction is a sufficient condition to have a higher expected final price.

[18]In general, there was very little difference in the fractions of agents who expressed a particular belief regarding under-pricing (such as strongly agree, agree, etc.) in the two cases. This suggests that, if sorting is present, it is the same above and below the threshold.

6 Robustness checks and alternative explanations

We begin this section by exploring the robustness of our results to two alternative estimation methods. We start with a nearest-neighbor matching approach and then implement the bias-corrected estimator suggested by Oster (2016, forthcoming), which attempts to learn about the omitted variable bias by analyzing how the coefficient estimate changes when adding controls. In the second part of the section, we investigate and rule out two alternative explanations for our results. First, we consider the possibility that a design feature of the market – the interface of the search engine on *Hemnet.se* – explains the observed discontinuity in the final price. Then, we look for support in our data for the alternative "cheap talk" hypothesis, according to which impatient sellers use round numbers to signal a weak bargaining position.

6.1 Robustness checks

Nearest-neighbor matching

One concern arises from the fact that the linear specification in equation 1 may be too restrictive and may fail to control for nonlinear effects of the covariates on the final price. This issue is well known in the treatment effects literature, in which practitioners often worry about OLS results when treatment and control groups differ too much regarding observables. One common solution to this issue is to use matching methods instead. These methods estimate the effect of being at the threshold by first finding, for each apartment just below the threshold, another apartment that is as similar as possible in observable characteristics but that has an asking price exactly *at* the threshold. Then, the treatment effect is estimated as the difference in average price between treated and matched control groups; therefore, it does not rely on linearity to control for observable characteristics.

Table 10 shows results from several variations of the nearest-neighbor matching method developed by Abadie and Imbens (2006). To have a clear distinction between treated and non-treated observations, we restrict the sample to apartments with an asking price just below or exactly at the common threshold. Apartments listed at round millions are, therefore, "treated", while those listed exactly 5,000 SEK below are not. In column 1, we report, as a reference, the OLS coefficient obtained by estimating the discontinuity in equation 1 on the sample restricted as before and including parish-year fixed effects.

The nearest neighbor-matching algorithm finds the closest neighbor in terms of surface area, number of rooms, monthly fee and year of construction. Following Abadie and Imbens (2006), we also use these variables to implement the bias correction. The

Table 10: The effect on sale prices, estimates from nearest-neighbor matching

	OLS	Nearest-neigbour matching			
	(1)	(2)	(3)	(4)	(5)
Above the cutoff	-4.90***	-4.87***	-6.24***	-5.47***	-3.48***
	(0.47)	(0.33)	(0.87)	(1.02)	(1.25)
Obs.	21,964	22,070	21,156	17,255	12,197
R^2	0.97				
Controls	✓	-	-	-	-
Fixed Effects	Year × Parish	-	-	-	-
Exact match on:	-	-	Mun.	Year & Mun.	Year & Parish

Notes: In all columns, the sample is restricted to apartments with a listing price just below or exactly at the cutoff (i.e., with running variable equal to -5,000 SEK or 0). In the first column, we report OLS estimates including year-parish and year-month effects. Columns 2-5 use a different specification of Abadie and Imbens (2006) nearest-neighbor matching with bias-correction using all the covariates used for matching. In column 2, we match on square meters, the number of rooms, monthly fee and year of construction. In addition, in column 3, we require observations to match exactly on municipality; in columns 4 and 5, instead, we require exact matching on year and municipality, and on year and parish, respectively. Standard errors are clustered at the municipality level in column 1 and are heteroskedasticity-robust in the remaining ones.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

matching results show that the coefficient drops slightly, but remains significant.[19] To make the comparisons more stringent, in column 3, we require apartments to match exactly on the municipality, and the coefficient remains negative and significant and even increases slightly in magnitude. When we require apartments to match exactly on municipality and year (column 4), the estimated coefficient decreases but remains larger in absolute value than the OLS estimate. In the most demanding specification of column 5, when we require an exact match on parish and year, the point estimate decreases to about -3.5. The drop in the number of observations is due to the fact that the algorithm needs at least two observations in each cell. We cannot go further than year-parish cells by, for instance, requiring exact matching on housing association and year because the number of observations drops to a prohibitively low level. The results from the specification in column 5, which uses comparisons within parish-year, are roughly of the same magnitude as those obtained by controlling for agent-year fixed effects in Table 7, suggesting a conservative estimate of the effect of about 3-3.5% of the final sale price.

Coefficient stability when adding controls

Oster (2016, forthcoming) shows that, if observables (W_1) and unobservables (W_2) are

[19] Standard errors in columns 2-5 are heteroskedasticity-robust and not clustered because the option is not available in the STATA command *teffects*.

related in the same way to the treatment variable X, in the sense that the regression coefficient of each on X yield the same result (up, at most, to a proportionality factor δ), then the following is a consistent estimator of the effect of X on Y:

$$\beta^* \approx \tilde{\beta} - \delta[\hat{\beta} - \tilde{\beta}]\frac{R_{max} - \tilde{R}}{\tilde{R} - \hat{R}}, \tag{2}$$

where $\tilde{\beta}$ is the OLS estimator from regressing Y on X and W_1, while $\hat{\beta}$ is the estimator from regressing Y on X. Clearly, δ – the relative degree of selection on observed and unobserved variables – and R_{max} – the R^2 of the regression that also includes W_2 – are not estimable and are chosen by the researcher for sensitivity analysis.

Inspection of the baseline results in Table 2 reveals that, in our, case, the more controls we add, the less, negative our estimates become – hence, $\hat{\beta} < \tilde{\beta}$. If we assume that the unobservables and the observables are related in the same way to X (a plausible assumption here), then $\delta > 0$, so that the true effect is less negative that our estimate, $\tilde{\beta}$. The size of this bias also depends on our assumption on R_{max}. Given that the value of R^2 in our estimates is very close to 1, it seems natural to set R_{max} at its most conservative level – that is, 1.

According to equation 2, the bias correction that we need to implement in this case is larger i) the greater the change in the slope when we include controls; and ii) the greater the corresponding change in the R^2 are. Given that, in our case, both changes are small, the bias correction is minimal. Looking at Table 2, we see that when we move from the specification without controls (column 1) to the one with controls and association-year fixed effects, the coefficient changes from $\hat{\beta} = -6.45$ to $\tilde{\beta} = -5.13$, while the R^2 increases from 0.944 to 0.990. Assuming that $\delta = 1$, we can calculate an upper bound using equation 2 with $R_{max} = 1$, $\delta = 1$ and the above values as $\beta^* \approx -5.13 + 1.32 \times 0.046 = -5.07$.

Equation 2 is derived under some restrictive assumptions, so it should be used only to have a first approximation to the size of the bias correction. The more general version of the estimator does not, in general, have a simple formula like 2; thus, to implement it we use STATA 14.1 and the user-written command *psacalc* made available by the author. To implement the estimator correctly, we first residualize the dependent variable and the covariates by regressing them on the running variables and the group indicators, each equal to one if the observation is close to one of the five 1 million thresholds (following the advice in section 3.3.3 of Oster 2016, forthcoming). Our uncontrolled baseline effect $\hat{\beta}$ is then obtained by regressing the residualized log price on the threshold dummy, as reported in the first column of Table 11.

The controlled effect, and its corresponding R^2, are obtained by including the full

Table 11: Robustness of the main effect to selection on unobservables

	Baseline effect	Controlled effect	Bias adjusted		
			$\delta = 0.75$	$\delta = 1$	$\delta = 1.25$
Above the threshold	-6.43***	-5.65***	-4.92***	-4.41***	-3.52
	(0.22)	(0.84)	(0.71)	(1.14)	(2.22)
Observations	57,616	57,455	57,455	57,455	57,455
R^2	0.015	0.806			

Notes: Bias-corrected OLS estimates for the effect of the asking price on the logarithm of the final transaction price (in thousands SEK). We use a local linear control function and a bandwidth of 100,000 SEK at each side of each cutoff. Estimation is performed in STATA 14.1 with the *psacalc* command by Oster (2017) using different values of δ and setting $R^{max} = 1$. Standard errors are bootstrapped by resampling at the municipality level (500 replications).

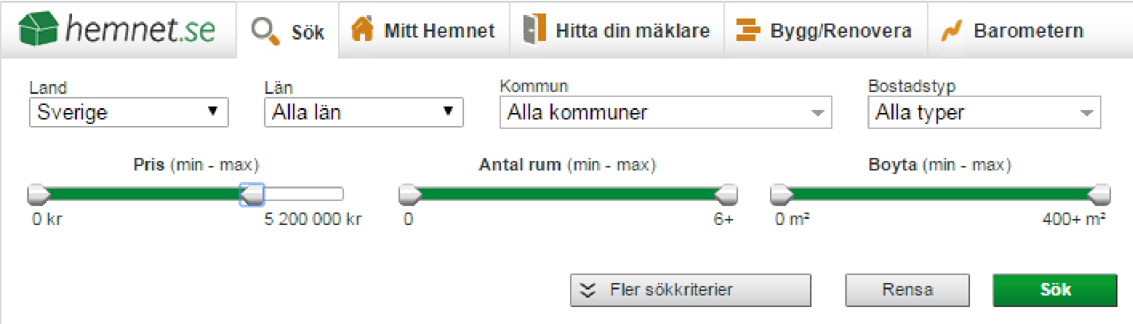
set of year-month and association-year fixed effects used in our most demanding baseline specification and reported in the second column. In the rightmost part of Table 11, we estimate the bias-corrected discontinuity in the final price around the round million thresholds for different values of δ and assuming $R^{max} = 1$. Standard errors are bootstrapped using municipalities as clusters and 500 replications. The residualization that we performed at the beginning causes the R^2 of the uncontrolled regression to be very small. The increment in R^2 due to the inclusion of the fixed effects is so dramatic (from 0.015 to 0.8) that, in principle, the bias-corrected estimator could be very different from the uncontrolled one. However, as we can see in column 3, for a value of $\delta = 0.75$, the point estimate decreases only slightly to -4.9. When setting this parameter to the suggested value of 1, the estimated discontinuity is reduced to -4.41 but is still statistically significant. It is only when imposing that δ be 1.25 that significance is lost, although the sign and, partially, the magnitude – of the coefficient are preserved. The estimated value of δ for which the effect eventually is zero is 1.58, a number that appears rather extreme compared to the recommended value of 1.

6.2 Alternative explanations

6.2.1 Institutional features of the market

Given that the vast majority of apartments on sale are also listed on *Hemnet*, it is possible that, if its interface makes apartments listed just below more visible than those listed at a round number, our results could be driven by a feature of the interface and not by inattention by the buyers. Indeed, the *Hemnet* search engine allows us to restrict the search results to apartments with prices within some predetermined brackets (each usually 250,000-500,000 SEK apart; see Figure 11 in the appendix). In large cities

Figure 8: The *Hemnet* interface in 2011



like Stockholm, it is almost unavoidable to restrict the search results in some way because of a large number of units for sale. Simply enlarging the search by including one more bracket increases the number of results by hundreds, and the cost of processing those additional results may become too burdensome for potential buyers. Although reasonable in principle, the hypothesis that these brackets are driving the result is unlikely because apartments listed exactly at the ends of a bracket are always shown in the search results (for instance, an apartment listed at 2 million SEK appears both in a 0-2 million search and in a 2-4 million search). This fact means that apartments listed at round numbers are, if anything, *more* visible that those listed just below. Even with this in mind, we can test this hypothesis formally by taking advantage of the fact that the search engine in *Hemnet* was changed on March 12, 2011 (as a quick investigation using the Internet archives shows, see https://archive.org/web/). Before that date, *Hemnet* did not have price brackets but a slider with 100,000-SEK increments, as Figure 8 shows.

We can use this change in the interface to estimate whether the price discontinuity at round numbers was different before and after using a diff-in-diffs strategy. To this end, we augment our baseline model described in section 3.2 by including an indicator *Post* for transactions of apartment advertised after the date of the interface change, as well as its interaction with our threshold indicator. In Table 18 in the Appendix, we see that the discontinuity was present even before the introduction of the brackets. The result is the same even when using observations from 2011 only (Panel A), suggesting that this particular feature of the search engine does not have an effect on final prices.[20]

Cheap talk

An alternative explanation for the effect that we document is that using round asking prices is one of the two optimal strategies that arise in a separating equilibrium. In such a model, some sellers are impatient, in the sense that they are willing to forgo a

[20]Notice that the *Post* indicator is not collinear with the year-month fixed effects because it is defined as being 1 for apartments listed after March 12, 2011 and, hence, it varies within a month.

higher final price in exchange for a quicker sale. In order to signal their weak bargaining position to buyers, they use round numbers as a distinctive pricing strategy. In this framework, sellers are behaving rationally and behavioral biases play no role. This is the approach followed by Backus, Blake and Tadelis (2016), who argue, using a large dataset of eBay negotiations, that patient sellers use round number for their asking price to signal that they are willing to accept a lower price in exchange for a quicker transaction. Items listed at multiples of $100 receive offers that are 8-12% lower but they are 15-25% more likely to sell than items listed at any other number. They also complement their evidence with data on apartment sales in the US, showing that apartments listed at round numbers are sold more at a lower price (although they have no information on the time on the market before sale).

It might be the case that a similar mechanism drives our result, and that round numbers are used as "cheap-talk" signaling to let potential buyers know of a weak bargaining position. However, in our case, the evidence goes against this hypothesis. In fact, even if round-million priced apartments are sold at a 3.5-5% lower price than apartments listed just below a round million, they do not differ in the amount of time they stay on the market, as shown in Figures 6 and 7. This result violates the incentive-compatibility constraint because round-number pricing appears to be a dominated strategy, as it yields lower prices without a faster sale.[21]

Given the number of apartments that are sold at round numbers, one might ask why sellers choose to incur such a large loss by picking this price. One possibility is that, as DellaVigna (2009) points out, behavioral biases are likely to be large in markets where players have little experience, as it happens for most buyers in the housing market and even for some sellers, especially when not guided by a real estate agent. In fact, although agents are often experienced professionals, the apartment owners ultimately decide the listing price. As the survey evidence reported in Tables 12, 13, and 14 shows, over 70% of the agents reported some interference by the seller in the choice of the final price, and 9.2% of them even reported that the sellers decided the price in all of their last five sales. Furthermore, when asked why someone would pick an asking price of 2 million SEK, 65.7% of agents declared that this was the price the seller required, although only 3% of agents said that they believed it would yield the highest sale price. When asked the same question regarding a 1.95 million price instead, only 6.4% declared that the seller would have chosen this amount. The vast majority believed that just-below pricing will generate the highest final price. This suggests that, while agents know that the best

[21]One important difference between Backus, Blake and Tadelis (2016)'s case and ours is that they study descending price negotiations and not ascending price auctions. In negotiations, the role of the asking price might be different because, for example, it is often set much above the reservation price and used as an "anchor".

strategy is to price just below round numbers, sellers often disagree, possibly because of their lack of experience in the market.

7 Conclusions

The tendency to use cognitive heuristics is deep-rooted in the human nature and is amply documented in laboratory experiments and, to some extent, in the field. In this paper, using a large and detailed dataset of transactions, we investigate the effect of partial inattention to the asking price of an apartment on the final sale price. We find that apartments with an asking price just below a 1 million threshold are sold at a 3-5% premium compared to similar apartments listed exactly at the threshold. A similar, but smaller, effect is found around 100,000 thresholds. Our estimates are robust to several specification checks and sample restrictions, suggesting that the effect is ubiquitous. To ensure that this result is not driven by sorting around the threshold, we control for a large set of covariates and increasingly more demanding fixed effects, restricting the comparison to apartments sold in the same building or by the same agent in the same year.

Turning to mechanisms, the effect appears to be caused by potential buyers who are inattentive to the asking price. Consistent with this hypothesis, apartments with just-below asking prices receive more attention, and their auctions have more bidders and more bids. Overall, the size of the effect, equivalent to roughly to five months of disposable income, appears to be hard to reconcile with predictions from an optimal search model, in which individuals stop searching when the marginal cost of searching is equal to the marginal benefit (Weitzman, 1979). Rather, buyers appear to pay a large price for their inattention.

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A Appendix - Data and sample restrictions

Main dataset

Data from Mäklarstatistik for all sales brokered by their agents (covering 90% of brokered sales in Sweden), 2010-2015. The original dataset was provided by Ina Blind, Matz Dahlberg, and Gustav Engström. The dataset has some inconsistencies and errors that we fix, as follows:

- Drop 51 duplicate transactions.
- Fix some unreasonable years of constructions when possible.
- Drop apartments with a missing, unreasonable or unclear year of construction.
- Drop new constructions, as they are generally not sold in auctions.
- Keep only apartments.
- Drop apartments with zero or missing asking or final sale price.
- Drop apartments with asking price larger than 5.5M SEK (very few).
- Drop apartments with inconsistent characteristics: e.g., zero rooms/square meters, floor higher than 30, or with missing municipal ID.

Outliers: Given that the housing association fee and the surface area have very few large outliers in both tails, we drop 579 apartments for which the value of one of these two variables is either larger than the 0.995 or lower than the 0.005 quintiles. We use the same trimming for the price increase, dropping 367 apartments sold at a final price that far exceeds the asking price or sold at a fraction of the asking price. We also drop 119 apartments with a number of rooms exceeding the 0.995 quintile (equal to 12 rooms), and 37 apartments with a negative time on the market. The final dataset comprises 349,476 transactions, with some missing values in some of the variables.

***Hemnet* subsample**

This dataset was downloaded from the *Hemnet.se* web page with the help of a Python script. We obtained information on the real estate firm and agent with every transaction that was available on 25/4/2016. The coverage of this dataset is small in 2010-2012 and becomes satisfactory only for 2013-2015, where the overlap with our primary dataset ranges from 40 to 57%.

Due to the absence of a unique identifier, the merge with the main dataset is based on asking and final price, sale date, rooms, surface area, and monthly fee. For this reason, we had to drop transactions that are indistinguishable from one another (for example, because they are apartments sold in the same building on the same date but on different floors). Also, possibly because the coverage differs in the two datasets, we were unable to merge all the *Hemnet* transactions to our main dataset. However, for the years 2013-2015, we could match about 52% of them (98,451).

Auctions subsample The largest real estate agency in Sweden (with about 20% market share), *Fastighetsbyrån*, publishes several auction results on its web page. Although the coverage is not perfect and some items are missing, the vast majority of auctions managed by *Fastighetsbyrån* can be obtained directly from the website with a script. We clean the dataset by dropping auctions for apartments sold abroad (in Euros), and auctions in which one or more bids lack the bidder identifier. We also drop 400 bids below 10,000 SEK and above 200 million SEK. In addition, we exclude a few auctions (0.14%) in which there is at least one bid that exceeds the previous one by more than 100% or is less than half of it, or for which there is a bid that is 1 million SEK lower than the previous one, as those are usually coding errors. We drop cases in which the auction lasted more than 60 days (1.7% of the total).

We drop transactions that are observationally equivalent to be able to merge them – based on geographical coordinates, price, and date of the auction – to the main dataset. We can match 27,173 apartments of the main dataset to the bidding information. Finally, we identify auctions in which a participant bids below the first bid. These cases can happen in reality when the first bidder leaves the auction, and the auction remains open, sometimes for months, until a new bidder arrives. These are not dropped but are identified by an indicator variable named "tag_lower_bid."

B Appendix - Survey results

Table 12: Survey results - I

1) Which are the main arguments you would say there are in favor of choosing an asking price of 1,995,000 SEK? It is possible to select multiple answers.		
	Fraction	Frequency
a. It generates the highest sales price.	0.23	70
b. It is the asking price that is usually used.	0.34	102
c. It attracts the most people during the open days.	0.35	104
d. Such a starting price is essentially when the seller insists on.	0.07	22
e. It generates the highest number of views on Hemnet	0.44	130
f. Other reason (please specify)	0.32	95
Answers		298
No answer		2

2) Which are the main arguments you would say there are in favor of choosing an asking price of 2,000,000 SEK? It is possible to select multiple answers.		
	Fraction	Frequency
a. It generates the highest sales price.	0.04	10
b. It is the asking price that is usually used.	0.09	26
c. It attracts the most people during the open days.	0.01	2
d. Such a starting price is essentially when the seller insists on.	0.66	190
e. It generates the highest number of views on Hemnet	0.02	7
f. Other reason (please specify)	0.31	90
Answers		289
No answer		12

3) If you think of your five most recent sales, in how many of these it was the owner who essentially decided the asking price?		
	Fraction	Frequency
None	0.30	87
1	0.28	82
2	0.17	49
3	0.11	33
4	0.06	17
5	0.09	27
Answers		295
No answer		6

Table 13: Survey results - II

4) In situations where the seller insists on a different asking price than the one the broker suggests, would you say it is more often lower or higher than the broker's suggestion?		
	Fraction	Frequency
a. More often lower	0.14	41
b. More often higher	0.86	249
Answers		290
No answer		11

5) What would you say are common reasons for the seller to insist on a different asking price than the one the broker suggests? It is possible to select multiple answers.		
	Fraction	Frequency
a. The seller wants the asking price to be the same he/she originally bought the item for.	0.03	10
b. The seller believes to know which strategy gives the highest sales price	0.60	181
c. The seller wants it to be the lowest price at which he/she is willing to sell the item for.	0.29	86
d. Other reason (please specify)	0.33	100
Answers		299
No answer		2

6) Suppose that two identical objects, A and B, are sold at the same time in the same area. The asking price is 1,995,000 SEK for object A and 2,000,000 SEK for object B.			
	Object A	Object B	No difference
a. Which item will sell faster?	0.34	0.01	0.65
b. Which item will get the most views on Hemnet?	0.65	0.02	0.33
c. Which item will get more visitors in the open days?	0.47	0.02	0.52
d. Which item will get the most bids in the auction?	0.45	0.02	0.53
e. Which item will sell at the highest price?	0.39	0.05	0.56
Answers			299
No answer			2

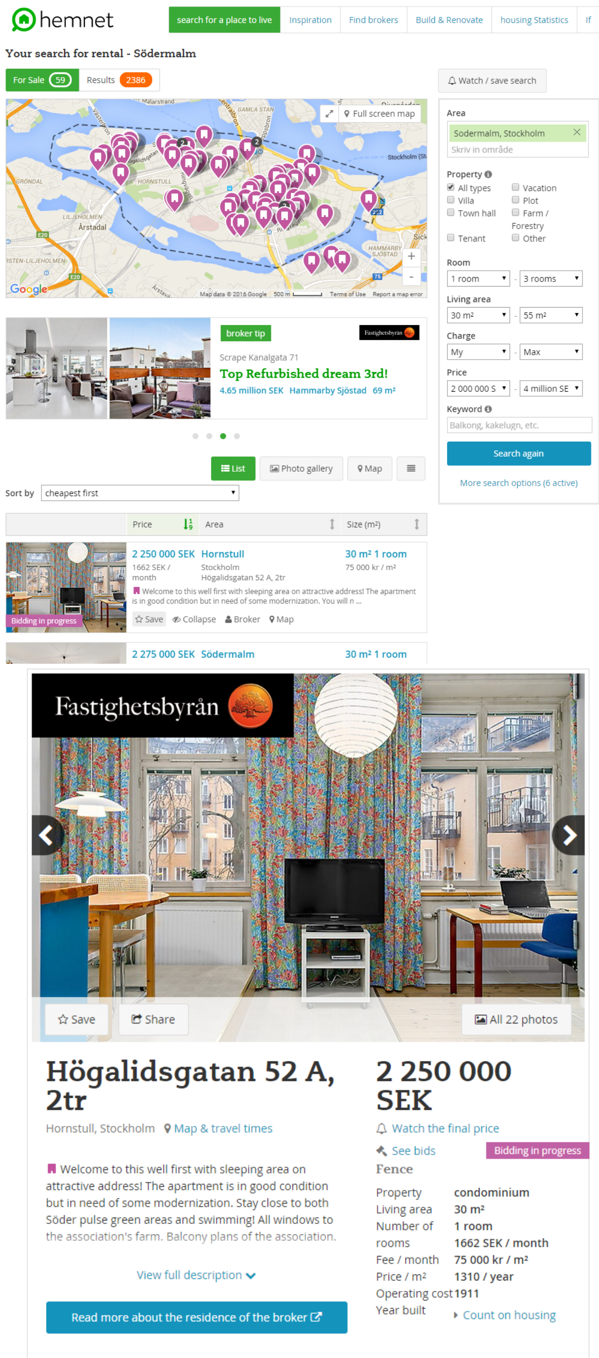
Table 14: Survey results - III

7) There has been extensive of discussion about asking prices set far below market value. To what extent do you agree with the following two statements?					
	Fully disagree		Indifferent		Fully agree
a. An object with an asking price of 1,995,000 SEK is likely to have an asking price much lower than market value	0.42	0.15	0.19	0.12	0.12
b. An object with an asking price of 2,000,000 SEK is likely to have an asking price much lower than market value	0.45	0.21	0.21	0.06	0.08
Answers					290
No answer					11

8) How many years of experience do you have as a real estate agent?		
	Fraction	Frequency
Less than 1 year	0.04	12
Between 1 and 3 years	0.14	42
Between 3 and 6 years	0.16	47
Between 6 and 10 years	0.23	70
More than 10	0.43	130
Answers		299
No answer		2

Appendix - Additional figures and tables

Figure 9: *Hemnet* main interface and an example of an ad



Notes: The top picture shows the main search engine in *Hemnet*. It shows the results for a search in central Stockholm. The map shows the different units available, which are also listed to the end (partially visible in the picture). The search can be narrowed down to different criteria listed on the right of the picture. The bottom picture shows the page that appears by clicking on an ad, showing the relevant characteristics of the unit, pictures, real estate information, etc.

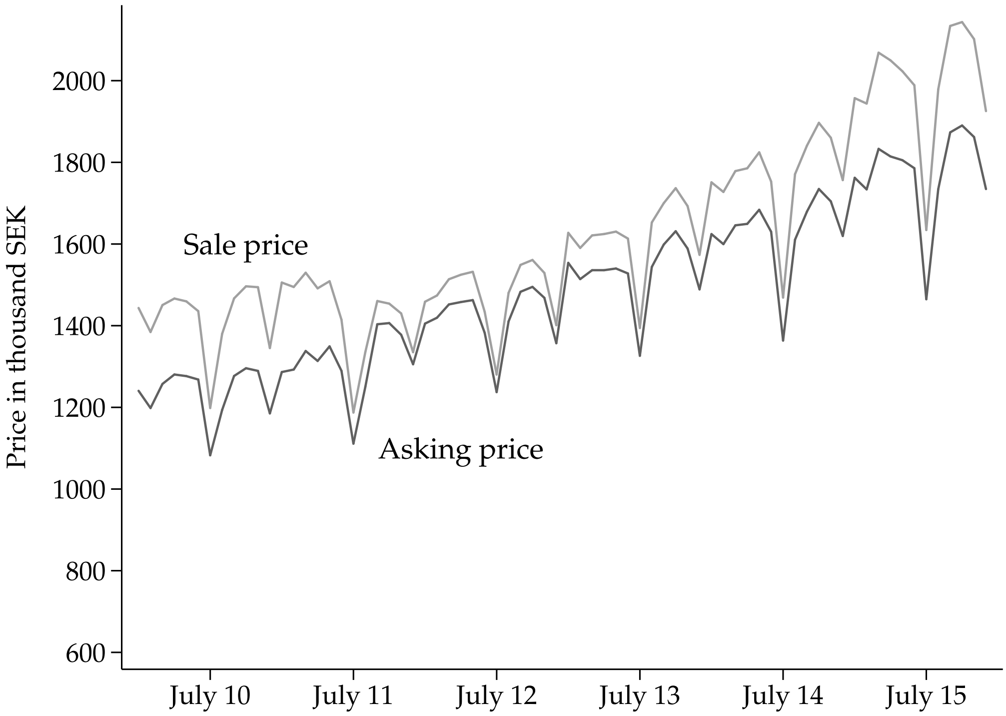


Figure 10: Evolution of average asking and sale prices for apartments sold in the whole of Sweden, 2010-2015.

Figure 11: The *Hemnet.se* interface

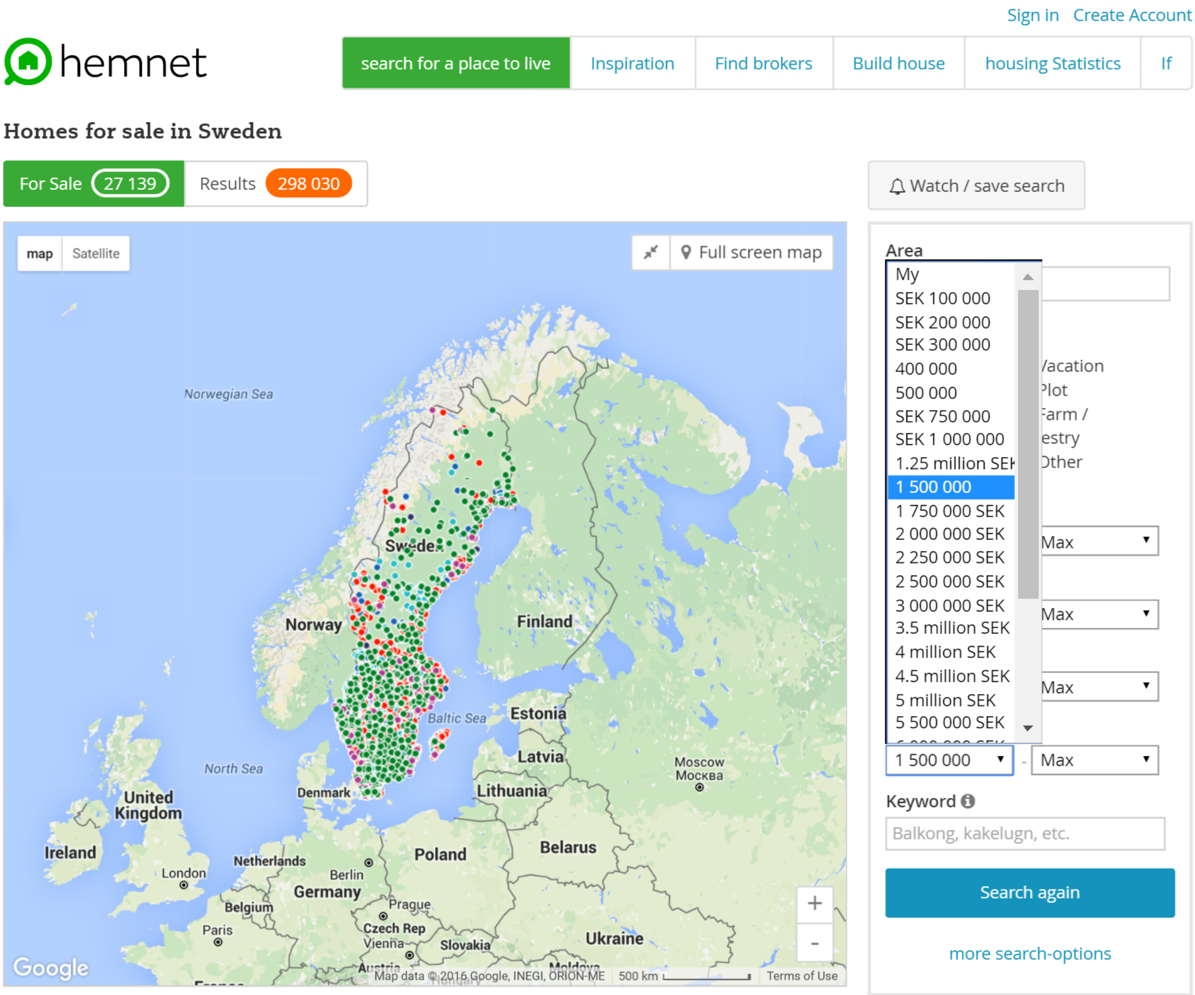


Table 15: The effect on sale prices for each year and at each million.

	Pooled	C=1M	C=2M	C=3M	C=4M	C=5M
	(1)	(2)	(3)	(4)	(5)	(6)
A. Year 2010						
Above the threshold	-7.68***	-5.55***	-11.5***	-8.17***	-8.71***	-5.38**
	(2.24)	(1.87)	(2.93)	(1.22)	(2.31)	(2.02)
Obs.	8,836	5,990	1,705	687	302	152
R^2	0.94	0.54	0.44	0.41	0.37	0.56
B. Year 2011						
Above the threshold	-6.58***	-5.93***	-7.49***	-5.74***	-4.46***	-4.13*
	(1.12)	(1.83)	(0.71)	(1.64)	(1.12)	(2.24)
Obs.	8,069	4,879	1,866	821	328	175
R^2	0.96	0.53	0.48	0.52	0.54	0.52
C. Year 2012						
Above the threshold	-2.70***	-3.78***	-2.00*	-1.53	-2.56	-0.92***
	(0.87)	(0.70)	(1.02)	(1.11)	(3.01)	(0.25)
Obs.	8,538	4,644	2,385	870	420	219
R^2	0.97	0.46	0.34	0.32	0.21	0.40
D. Year 2013						
Above the threshold	-3.65***	-4.64***	-3.68***	-2.67***	-2.14***	0.53
	(0.51)	(0.77)	(0.47)	(0.51)	(0.72)	(0.38)
Obs.	9,319	4,385	2,968	1,107	585	274
R^2	0.97	0.46	0.35	0.30	0.30	0.36
E. Year 2014						
Above the threshold	-5.39***	-4.83***	-6.71***	-5.85***	-3.70***	-2.25***
	(0.55)	(0.96)	(0.95)	(0.46)	(0.28)	(0.66)
Obs.	10,273	4,424	3,343	1,433	740	333
R^2	0.97	0.51	0.38	0.35	0.27	0.34
F. Year 2015						
Above the cutoff	-6.90***	-2.42*	-8.48***	-8.46***	-7.03***	-7.66***
	(1.07)	(1.42)	(0.70)	(0.71)	(0.81)	(2.19)
Obs.	12,503	4,437	4,565	2,081	885	535
R^2	0.96	0.49	0.45	0.38	0.38	0.30

Notes: Dependent variable is sale price in logarithms. All specifications use local linear regressions in a bandwidth of 100,000 SEK around the threshold. All regressions include covariates and allow the running variable to have a different slope at either side the cutoff. Year times parish fixed effects are included in all regressions. In column 1 we pool all round number thresholds and include indicators for the closest million. Standard errors are clustered at the municipality level.
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

Table 16: The effect on sale prices at 100,000 SEK cutoff, for each year and for each million.

	Pooled	0-1M	1-2M	2-3M	3-4M	4-5M
	(1)	(2)	(3)	(4)	(5)	(6)
A. Year 2010						
Above the thresh.	-5.57***	-3.13***	-6.99***	-6.97***	-5.46***	-1.54
	(1.61)	(0.64)	(2.30)	(1.62)	(0.77)	(1.33)
Obs.	47,568	23,448	17,351	4,530	1,535	578
R^2	0.98	0.94	0.75	0.64	0.52	0.53
B. Year 2011						
Above the thresh.	-4.75***	-1.89***	-6.38***	-4.75***	-3.02***	-3.08***
	(1.23)	(0.56)	(1.64)	(0.60)	(0.33)	(0.64)
Obs.	45,426	21,200	16,764	4,853	1,770	696
R^2	0.98	0.95	0.77	0.68	0.59	0.58
C. Year 2012						
Above the thresh.	-1.24***	-1.44**	-1.40***	-0.91*	-0.79	-0.59***
	(0.37)	(0.57)	(0.39)	(0.47)	(0.72)	(0.21)
Obs.	49,307	20,636	18,493	6,557	2,437	990
R^2	0.98	0.95	0.84	0.74	0.52	0.59
D. Year 2013						
Above the thresh.	-1.88***	-2.48***	-2.01***	-1.58***	-1.11***	-1.01***
	(0.23)	(0.58)	(0.27)	(0.40)	(0.22)	(0.29)
Obs.	51,838	19,410	20,023	7,913	2,957	1,289
R^2	0.98	0.95	0.83	0.73	0.66	0.49
E. Year 2014						
Above the thresh.	-2.85***	-2.10***	-2.94***	-3.70***	-2.28***	-2.18***
	(0.33)	(0.51)	(0.50)	(0.35)	(0.24)	(0.18)
Obs.	54,083	18,210	21,053	9,521	3,415	1,612
R^2	0.98	0.94	0.81	0.70	0.59	0.38
F. Year 2015						
Above the thresh.	-4.42***	-2.60***	-4.05***	-5.35***	-5.63***	-3.28***
	(0.54)	(0.68)	(0.64)	(0.62)	(0.73)	(0.52)
Obs.	59,936	17,901	21,917	12,754	4,754	2,148
R^2	0.98	0.93	0.79	0.66	0.55	0.49

Notes: Dependent variable is sale price in logarithms. All specifications use local linear regressions in a bandwidth of 50,000 SEK around the threshold. All regressions include covariates and allow the running variable to have a different slope at either side the cutoff. Year times parish fixed effects are included in all regressions. In column 1 we pool observations from all 100,000 SEK thresholds, whereas in columns 2 through 7, we estimate the effect only for apartments with an asking price between 0 and 1 million SEK, between 1 and 2, and similarly for all millions separately. Standard errors are clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

Table 17: Balance of covariates - around 100,000 SEK thresholds

	Pooled	0-1M	1-2M	2-3M	3-4M	4-5M	5-5.5M
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Squared meters	-0.078	2.45***	0.017	-1.66	-3.27**	0.12	0.91
	(1.05)	(0.78)	(1.54)	(1.48)	(1.45)	(1.92)	(3.74)
Obs.	310,048	121,457	116,258	46,471	17,018	7,386	1,458
R^2	0.06	0.00	0.00	0.01	0.01	0.01	0.01
Year Constr.	-0.23	2.37***	1.16	-1.38	-6.75***	-5.72**	-6.77**
	(1.56)	(0.87)	(2.14)	(1.31)	(1.69)	(2.26)	(2.73)
Obs.	310,287	121,558	116,353	46,505	17,025	7,388	1,458
No. Rooms	-0.0011	0.094***	0.017	-0.064	-0.16***	-0.0075	-0.039
	(0.047)	(0.030)	(0.069)	(0.055)	(0.046)	(0.044)	(0.092)
Obs.	309,888	121,373	116,210	46,454	17,011	7,384	1,456
Monthly fee	-14.1	118.9**	19.5	-101.6	-218.0***	-138.8*	-82.8
	(60.3)	(47.6)	(75.6)	(77.9)	(75.7)	(81.0)	(165.6)
Obs.	309,775	121,443	116,129	46,400	16,988	7,364	1,451
Floor	-0.070	-0.095*	-0.074	-0.066	-0.036	0.019	-0.11
	(0.053)	(0.052)	(0.061)	(0.061)	(0.12)	(0.073)	(0.20)
Obs.	258,633	102,783	97,530	38,054	13,348	5,780	1,138
Elevator	-0.0076	-0.012	-0.0017	-0.013	-0.015	0.0036	0.012
	(0.021)	(0.013)	(0.023)	(0.042)	(0.031)	(0.012)	(0.014)
Obs.	267,140	101,248	99,885	41,805	15,783	7,024	1,395
Balcony	0.0028	-0.0023	0.0046	0.014	0.0083	-0.075*	0.053
	(0.016)	(0.020)	(0.018)	(0.024)	(0.038)	(0.039)	(0.048)
Obs.	99,917	36,396	41,068	15,106	5,005	1,988	354

Notes: Regression estimates of the effect of the asking price on different apartment characteristics around 100,000 SEK thresholds. We report the coefficients of an indicator for the asking price being to or above a threshold. In column 1 we pool observations from all 100,000 SEK thresholds, whereas in columns 2 through 7, we estimate the effect only for apartments with an asking price between 0 and 1 million SEK, between 1 and 2, and similarly for all millions separately. We use a local linear control function allowing for different slopes at each side of each threshold. No controls or fixed effects included. Standard errors are clustered at the municipality level.

* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$

Table 18: The effect of the introduction of price brackets on *Hemnet.se*

	(1)	(2)	(3)	(4)
A. Using observations for 2011 only				
Above the threshold	-6.27***	-6.38***	-6.77***	-6.73***
	(1.88)	(1.85)	(1.31)	(1.39)
Above the threshold*Post	-0.19	-0.11	0.021	0.17
	(0.76)	(0.71)	(0.72)	(0.73)
Post	-4.87***	3.86***	2.93***	2.68***
	(1.15)	(0.79)	(0.67)	(0.71)
Obs.	8,123	8,101	8,101	8,069
R^2	0.93	0.94	0.95	0.96
B. Using all sample, 2011-2015				
Above the threshold	-5.63***	-5.61***	-5.70***	-5.79***
	(1.35)	(1.31)	(0.91)	(0.94)
Above the threshold*Post	-0.036	-0.066	0.077	0.31
	(0.51)	(0.48)	(0.47)	(0.54)
Post	0.71	1.19**	0.43	-0.14
	(0.81)	(0.59)	(0.38)	(0.43)
Obs.	57,956	57,788	57,788	57,538
R^2	0.95	0.95	0.96	0.96
Controls		✓	✓	✓
Fixed Effects			Year × Municip.	Year × Parish

Notes: Regression estimates of the effect of the asking price on the logarithm of the final transaction price (in thousands SEK) from equation 1, pooling all 1 million thresholds together and using a bandwidth of 100,000 SEK. We use a local linear control function allowing for different slopes at each side of each threshold. Standard errors are clustered at the municipality level. Controls include living area, the number of rooms, monthly fee, and year of construction, plus different sets of fixed effects. Month-year fixed effects are also included in all columns but the first. *Post* is one for apartments sold after the change in the *Hemnet.se* interface in March 12, 2011 (see Section 6 in the text for details).
* $p < 0.1$, ** $p < 0.05$, *** $p < .0.01$