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Working Paper 13796
http://www.nber.org/papers/w13796

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 2008

This paper was previously circulated under the title "How Much Value Do Real Estate Brokers Add? A Case Study." We are grateful to April Blaine, Shirley Campbell, Betty Oen, Jan Thomson, and members of Stanford University's Faculty and Staff Housing Office. Stephan D. McBride, Sriniketh S. Nagavarapu, Harvey S. Rosen, and members of Stanford University's Labor Reading Group provided helpful comments. Zhihao Zhang provided valuable research assistance. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

Do Sellers' Brokers Raise Or Reduce Home Prices? Evidence Of Agency Costs From An Unusual Real Estate Market

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NBER Working Paper No. 13796
February 2008, Revised March 2010
JEL No. D14,R21,R31

ABSTRACT

This paper measures the effects of real estate brokerage services provided to sellers, other than MLS listings, on the terms and timing of home sales. Because the relationship between the homeowner and the broker resembles a classical principal-agent problem, the broker may not deploy services in ways that promote the seller's interests. However, because the seller presumably benefits when a home is included in the Multiple Listing Service (MLS) database, the combined effects of MLS listings and other services likely obscures the agency costs. The separate effects of those other services bear directly the recent policy debate over the unbundling of MLS listings. With few exceptions, previous studies make no attempt to separate the effects of MLS listings from those of other services, precisely because bundling has been so prevalent. We estimate the effect of a seller's decision to use a broker on list prices, selling prices, and speed of sale for a real estate market with an unusual and critical characteristic: it has a single open-access listing service that is used by essentially all sellers, regardless of whether they employ brokers. The market consists of single homes and condominiums located on the Stanford University cam-pus. Our central finding is that, when listings are not tied to brokerage services, a seller's use of a broker reduces the selling price of the typical home by 5.9 to 7.7 percent, which is consistent with the presence of a fairly severe principal-agent problem. There is also some indication that a seller's use of a broker reduces the initial asking price and accelerates the sale.

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1. INTRODUCTION

Real estate brokers typically provide sellers with bundles of services, most of which are ostensibly aimed at improving the terms at which homes are sold and helping homeowners find buyers more quickly.[1] Traditionally, access to the Multiple Listing Service (MLS), a pooled database of homes for sale that is owned and operated by an association of local real estate brokers, has been an important component of that bundle. Other components of the traditional brokerage bundle that potentially affect the terms and timing of a sale include market information and recommendations pertaining to the appropriate asking price,[2] promotional services (preparing a home for sale, circulating flyers, placing advertisements, holding open houses, and recommending the home to individual buyers), the screening of prospective buyers, the facilitation and acceleration of matches between buyers and sellers,[3] and assistance with negotiations.[4]

The object of this paper is to measure the effects of real estate brokerage services provided to sellers, *other than MLS listings*, on the terms and timing of home sales. The homeowner presumably benefits when the home is included in the MLS database. However, because the relationship between the homeowner and the broker resembles a classical principal-agent problem, the broker may not deploy *other* services in ways that promote the seller's interests. While appropriately structured compensation schemes may alleviate the principal-agent problem to some degree (see, e.g., the recent survey by Miceli, Pancak, and Sirmans, 2007), significant conflicts remain. As Levitt and Syverson (2008) emphasize, an agent "has strong incentives to sell the house quickly, even at a substantially lower price." Their empirical analysis shows that homes owned by agents sell for nearly four percent more and stay on the market roughly nine days longer than other comparable houses. Similarly, Rutherford, Springer, and Yavas (2005) find that "real estate agents receive a premium of 3.0–7.0% when selling their own condominiums in

[1] Brokers frequently provide other services that do not directly impact the terms or timing of sales. For example, they help with paperwork and legal documentation, and provide referrals to mortgage lenders. However, those services tend to be secondary.

[2] Brokers argue that they "offer professional advice and objective insight" (Evans, 2003), while homes sold by owner "often are priced too high and may not sell until the price is reduced, which can turn into an unnecessarily long drawn-out process" (Kossen, 2000).

[3] See, e.g., Salant (1991).

[4] According to the National Association of Realtors, brokers are "trained to negotiate the best possible prices and terms" (Evans, 2003).

comparison to similar client owned condominiums" by waiting longer to sell.[5] While those studies may shed light on the extent to which the deployment of brokerage services departs from the first-best, they do not tell us how the use of brokerage services by the typical buyer impacts that buyer's selling price or time to sale. Consequently, they do not answer the question posed at the outset of this paragraph.

The importance of the question addressed in this paper is most readily apparent within the context of the recent policy debate over service bundling in real estate brokerage. Because the traditional tie between MLS listings and other brokerage services is both artificial and contrary to the desires of some homeowners, a small but growing number of brokers now offer to place clients' homes in the MLS for a fixed fee.[6] Yet in many jurisdictions established brokers have resisted pressures to unbundle MLS listings from other services. In some cases they have pressed for "minimum service" laws that prevent agents from offering MLS-only options. As of 2007, eight states had such laws in place.[7] Some states have also adopted licensing requirements that impede the entry of non-traditional real estate brokers. Where such laws are not in force, some MLSs have adopted their own rules and procedures to prevent or at least discourage unbundling.[8] Though the internet increasingly provides alternatives to MLS databases (at least in some jurisdictions), the U.S. Federal Trade Commission observed as recently as November 2009 that "[t]he MLS is generally acknowledged to be a superior platform for matching home buyers and sellers."[9] Thus, the practice of bundling MLS listings with other real estate brokerage services handicaps homeowners who wish to sell their homes either completely independently ("for sale by owner" or FSBO transactions) or without the ancillary services of a listing broker. As a result, that practice has become the subject of debate, litigation, and legislative action. Both the Federal Trade Commission and the Department of Justice have taken active roles in challenging

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[5] In a similar vein, Huang and Rutherford (2007) find that properties sold by realtors – that is, members of the National Association of Realtors – on the MLS sell for more (and more quickly) than those sold by agents without that designation.

[6] The fee is generally in the range of $200 to $900. The homeowner may then contract with the listing broker for other sales services (which are usually provided in exchange for a smaller commission) or sell the home without those services. In the latter case, the homeowner may pay a "Buyer's Agent Commission" to a broker who brings in a buyer, or avoid commissions altogether by finding a buyer independently.

[7] Alabama, Idaho, Illinois, Indiana, Iowa, Missouri, Texas, and Utah had minimum service laws. New Mexico passed and then rescinded a minimum service law, and other states have actively debated that alternative. See Magura (2007) and Federal Trade Commission (2007).

[8] For further discussion, see Magura (2007) and Federal Trade Commission (2007).

[9] This passage appears in a recent decision by the Commissioners of the Federal Trade Commission concerning the practices of Realcomp II Ltd.; see Kovacic (2009).

the practice and the laws that support it.[10] They argue that bundling reduces competition from non-traditional channels of home sales, and essentially compels many homeowners to purchase unwanted services (other than MLS listings). Plainly, if it turns out that those services lead homeowners to sell their homes at lower prices contrary to their apparent interests, the latter allegation would be particularly serious.

The task of quantifying the separate impact of brokerage services provided to sellers, other than MLS listings, is challenging precisely because bundling has been so prevalent. While a number of previous studies have examined the impact of real estate brokerage,[11] they generally make no attempt to separate the effects of MLS listings from those of other services. For example, Hendel, Nevo, and Ortalo-Magné (2009) compare sales of MLS-listed homes sold through traditional full-service brokers to sales of homes listed on a web-based FSBO service. Their analysis is noteworthy because their data set is reasonably large, contains many FSBO transactions, and spans a seven year period, which allows them to control for both home and household fixed effects.[12] However, analyses of that type inherently cannot reveal the *separate* effects of non-MLS brokerage services.[13] Notably, the working paper version of the Hendel *et al.* study mentions that the relative scarcity of listings by limited-service brokers as of year-end 2004 precluded an analysis of MLS-only versus full-service listings.

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[10] For example, the U.S. District Court for the District of South Carolina (Columbia Division) ruled in favor the government in U.S. v. Consolidated Mutliple Listing Services (Case No. 3:08-CV-01786-SB, Final Judgement issued August 27, 2009) overturning the discriminatory rules of an MLS in Columbia, SC, that prevented various practices including unbundling. See http://www.justice.gov/atr/cases/f249600/249614.htm.

[11] Doiron, Shilling, and Sirmans (1985) and Frew and Jud (1987) find that homes sell for more when the seller uses a broker. Kamath and Yantek (1982), Colwell, Lauschke, and Yavas (1992), and Hendel, Nevo, and Ortalo-Magné (2009) find no effect. Based on a matching model, Yavas and Colwell (1995) argue that the effects of using a broker should be heterogeneous across sellers. Liao and Chang (2005) find that broker price effects are indeed heterogeneous, raising the price of homes at the lower end of the distribution but lowering the price of more expensive ones.

[12] Prior studies generally examined cross-sectional correlations, a strategy that offers little opportunity to control convincingly for the fact that the use of a broker is highly correlated with the characteristics of homes and homeowners. In most markets, FSBO sellers constitute a small, highly selected group with potentially unusual characteristics and inclinations: only 17% of sellers forego the use of an agent (National Association of Realtors, 2003); during the first quarter of 2004, 44% of all FSBO homes were never placed on the open market, as the buyer and seller knew each other in advance (Evans, 2003); and FSBO sellers tend to be older and less wealthy (National Association of Realtors, 2002). Some prior studies employ sample selection corrections, but identification is driven entirely by functional form assumptions rather than exclusion restrictions. Also, some earlier studies employed data samples that were extremely small and somewhat peculiar. For example, Doiron, Shilling, and Sirmans (1985) examined 134 transactions in two condominium complexes, while Kamath and Yantek studied 118 transactions.

[13] If one is willing to accept without evidence the assumption that MLS-listings lead to higher sales prices than the non-MLS listings studied by Hendel, Nevo, and Ortalo-Magné (2009), then their finding that the use of a seller's broker has no impact on sales price would imply that services other than MLS listings collectively have an offsetting negative impact on sales price. Of course, the confidence interval of the effect they estimate includes positive values, so the preceding implication only follows for their point estimate.

The analysis of Johnson, Springer, and Brockman (2005) attempts to speak more directly to the question that motivates our study, in that they compare the selling prices of MLS-listed homes and "non-traditional" broker-marketed homes that were not listed in the MLS. On its face such a comparison would appear to reveal the separate value of an MLS listing. However, it is not clear whether brokerage contracts are otherwise similar for MLS-listed and non-MLS-listed homes; hence, measured differences in sales prices may reflect a combination of effects. It also readily apparent that the non-traditional transactions, which represent less than five percent of their sample, constitute a highly selected subsample, and the authors' cross-sectional regressions make no allowance for the likely selectivity bias. Thus, the proper interpretation of the study's central finding – that selling prices for brokered homes are six percent higher when the home is *not* MLS-listed – is obscure.

In this paper, we estimate the effect of a seller's decision to use a broker on list prices, selling prices, and speed of sale for a real estate market with an unusual and critical characteristic: it has a single open-access listing service that is used by essentially all sellers, regardless of whether they employ brokers.[14] The market consists of roughly 800 houses and condominiums located in a collection of largely contiguous neighborhoods on Stanford University land. Because ownership of the homes is limited to Stanford faculty and some senior staff,[15] the MLS plays no role. Instead, the Faculty Staff Housing (FSH) Office maintains a free listing service, so that listings are inherently unbundled from brokerage services. As in other markets, brokers are compensated through standard commissions (historically in the range of five to six percent), so the usual principal-agent problems are present. Consequently, by analyzing this market, we can identify the separate effects of brokerage services other than MLS listings.

Studying Stanford University housing offers several additional advantages. We have data on all home sales over a 27 year period, during which the quality of the neighborhoods remains approximately constant due to the special nature of the market. The data also span a major regime shift: brokered transactions were relatively rare during the 1980s but became increasingly common during the 1990s and have accounted for roughly half of all sales in recent years (see Figure 1). As a result, neither FSBO nor brokered transactions are exceptional. There is anecdo-

[14] Throughout this paper, we use the term "market" somewhat loosely, and not in the formal sense employed, for example, in antitrust analyses.

[15] Stanford enforces this restriction by retaining ownership of the land. Stanford provides the homeowner with a long-term land lease involving modest monthly payments.

tal evidence that this transition was driven by the aggressive marketing efforts of several realtors rather than by a shift in sellers' preferences. Because the data cover a relatively long time period, they include multiple transactions not only for the same home, but also for the same party. In many cases, the pertinent transactions span the regime shift. Therefore, we are able to assess whether correlations between the use of a broker and the terms or timing of a sale reflect spurious relationships with unobserved characteristics of sellers or their homes, and to purge our estimates of such effects. Concerns about unobserved heterogeneity are also ameliorated to some extent by the fact that the population of buyers and sellers is relatively homogeneous, at least in comparison with the general population.

Our central finding is that a seller's use of a broker *reduces* the selling price of the typical home by 5.9 to 7.7 percent (based on point estimates), which is consistent with the presence of a fairly severe principal-agent problem. Those estimates are obtained from specifications that include home fixed effects; some also allow for the possibility that prices may have changed over time at different rates in different market segments (e.g., that the prices of high-end homes, which are more frequently sold through brokers, may have risen or fallen relative to those of low-end homes). In all such specifications we reject the hypothesis that the broker effect equals zero at a high level of confidence.[16] We find no evidence that the lower prices received by sellers who use brokers are attributable to correlations with unobserved household characteristics such as preferences or negotiation skills. Our analysis also suggests that a seller's use of a broker may reduce the initial asking price and accelerate the sale, but those findings are supported with less statistical confidence.

Why do sellers use brokers if doing so reduces a home's selling price? One possibility is that sellers place sufficient value on convenience or the speed of sale. Given the magnitude of the measured effect plus the broker's commission (which total nearly $100,000 for the average home in our sample), we doubt that this explanation is valid. Another possibility is that sellers are poorly informed about the effects of brokers' services. There is some anecdotal support for that view. An earlier version of this paper was provided to the FSH Office in late 2006 and circulated among homeowners. As seen in Figure 1, the fraction of sellers using brokers plummeted from 59.5% in 2006 to only 28.6% in 2007.

[16] For all but one such specification we reject that hypothesis at the 95% level of confidence or higher. For the remaining (and most restrictive) specification, which does not allow prices to follow different time paths based on home quality, we reject it at the 90% level of confidence.

As with all studies that focus on usefully unusual settings, there is a question of generalizability. While we would not suggest that our estimates can be applied directly to the general population, there are good reasons to think that the special features of this market would, if anything, limit broker opportunism: home sellers are particularly well-educated, affluent compared to the general population, and less likely to be at an informational disadvantage concerning market conditions given that the scope of the market is limited and information is available through the FSH Office. It is also worth noting that, conditioning only on the year of sale, the average selling price is 43 percent higher for homes sold through brokers than for FSBO homes, a figure that lines up well with the national average.[17] Another concern is that our data sample is relatively small (755 usable observations out of 1,064 total transactions), which to some degree limits our investigation. For example, we cannot successfully estimate a model with buyer or seller fixed effects. Even so, we are able to evaluate the potential importance of household heterogeneity diagnostically, and our sample is sufficiently large to generate reasonably precise estimates for the specifications we do present.

The remainder of this paper is organized as follows. Section 2 describes our data. Sections 3, 4, and 5 evaluate the effects of a seller's broker on, respectively, selling prices, initial asking prices, and time to sale. Section 6 describes some sensitivity analyses and Section 7 concludes.

2. DATA

The data used in this paper were generously provided by Stanford University's FSH Office. Sales data and certain home characteristics are available as far back as the 1940s, but information relating to the use of brokers is available only through monthly sales circulars distributed by the FSH office, which are archived back to January 1980. We infer the use of a broker from the contact information provided in the housing listings. Altogether, 1,064 sales were recorded between January 1980 and January 2008, of which 794 appear in the sales circulars. This discrepancy is attributable primarily to two factors: some sales involved land used for new construction, and some were sold off-market without being listed. We dropped twenty observations with incomplete data for purchase price, construction date, or home characteristics. We also

[17] See specification (1) in Table 1. According to the National Association of Realtors (2002), the median selling price of homes sold through brokers is 37 percent higher than that of FSBO homes.

dropped nineteen observations listing Stanford University as the buyer or seller; including those observations does not significantly alter our results, but their prices appear to be atypical.[18] These exclusions leave us with 755 observations, of which 133 involved brokers.[19] Some homes were removed from the FSH listings prior to a sale, only to reappear in subsequent listings, most within one year. If the home reappeared in the listings within 36 months of withdrawal, we treated it as having remained on the market since its initial listing. Roughly a dozen homes were re-listed after 36 months; we treated those as new listings.

Other variables used in our analysis measure characteristics of the property, including the number of bedrooms and bathrooms, site acreage, square footage, dummies indicating the presence of a study or a pool, the age of the home at the time of sale (calculated using its date of construction), and neighborhood indicators.[20] We include a dummy variable indicating sales through estates, as well as year dummies to account for variations in market conditions. In some specifications, we also control for the length of time the seller had lived in the home at the time of sale.[21] That variable presumably proxies for the seller's age or attachment to the home, and possibly for the condition of the property. Its use further limits our sample to 691 transactions, of which 129 involved a broker. Generally, our results are robust with respect to the combination of variables used.

We also have some information on the characteristics of the buyers and sellers. We were able to determine the ages of 585 sellers and 720 buyers, as well as the academic department affiliation for 625 sellers and 739 buyers. We do not observe directly whether buyers were represented by brokers.[22]

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[18] Among other things, the selling prices for those homes rarely differ from the asking prices.

[19] Eighteen buyers switched from FSBO to using a broker over the course of listing their property, while five switched from using a broker to FSBO. For our analysis of the initial asking price, we treat the seller as using a broker if it did so when initially listing the property; for our analysis of the selling price and time on the market, we treat the seller as using a broker if it did so at the time of sale. Dropping these observations does not significantly alter our results.

[20] Numerous studies (MacDonald, 1996; Palmquist, 1984; Parsons, 1986, and others) have demonstrated the importance of these characteristics in determining the price of a home. Two of the neighborhood indexes correspond to condominium complexes. Because all condominiums are in one of these two complexes, it is not necessary to include a separate dummy variable indicating whether the home is a house or condominium.

[21] We calculate this variable by determining the last date of sale for the same property. In some cases, that information is unavailable.

[22] Levitt & Syverson (2005) found that the absence of a buyer's agent "has a negligible impact on sale price." Similarly, Zumpano, Elder, and Baryla (1996) model the buyer's decision to use a broker and, accounting for this selection, find that there is no effect on sale price.

Table 1 reports summary statistics. Figure 1 shows the fraction of sellers using a broker by year. Notice that the proportion of brokered transactions remained quite low through the 1980s and early 1990s, but rose steadily from 1996 through 2006, at which point it hit 59.5%. It then fell sharply once the first version of this paper was circulated.

3. SELLING PRICES

First we examine the relationship between the log selling price and the use of a broker. Table 2 contains OLS regression results, reported with robust standard errors, clustered at the home level. Specification (1) includes only a broker dummy and year effects. The coefficient of the broker dummy (0.357) implies that brokered homes sold for approximately 43 percent more on average than homes sold without brokers. The difference is highly statistically significant, with a t-statistic of more than 5.

Naturally, the broker coefficient in specification (1) tells us nothing about the effect of using a broker on a home's selling price. As a first step toward measuring that effect, we control for the characteristics of a home that are correlated both with the home's value and with the likelihood that it is listed through a broker. Specification (2) adds the home characteristics discussed in Section 2, as well as dummy variables for eight Stanford neighborhoods. Notice that the coefficient of the broker dummy drops to 0.0009 with a standard error of 0.0232; it is both economically negligible and statistically indistinguishable from zero. Anecdotal evidence suggests that the usual commission in the Stanford housing market has fallen over time from 6 percent to 5 percent; to cover even a 5 percent commission, the use of a broker would need to increase a home's selling price by 5.26 percent, which corresponds to a broker coefficient of 0.0513. Notably, we can confidently reject the hypothesis that the broker coefficient equals 0.0513 (p-value = 0.030). Other coefficients generally have the expected sign.

Specification (3) adds a measure of the length of time the seller had lived in the home prior to listing it for sale (as well as its square). Adding this variable reduces the size of our sample from 755 to 691. The broker coefficient rises a bit to 0.0203, with a standard error of 0.0238. The measured effect is now larger economically, but still less than half of the standard broker's commission, and it remains statistically indistinguishable from zero at conventional levels of confidence. We can no longer reject the hypothesis that the coefficient is 0.0513 at conventional levels of confidence (here, the p-value is 0.194); consequently, on the basis of this es-

timate, we cannot rule out the possibility that brokers pay for themselves. The difference between the broker coefficient in specifications (2) and (3) is partly attributable to the smaller sample size.

In interpreting our estimates of specifications (2) and (3), one should bear in mind that the use of a broker may be correlated with unobserved factors that influence transactions prices. Such factors fall into two main categories: characteristics of the home and characteristics of the seller. We experimented with a number of potential instruments, including the recent incidence of brokered sales within a home's neighborhood and among members of the seller's academic division. Unfortunately, none of the instruments we examined had a great deal of explanatory power. As a result, the associated IV estimates were imprecise and unstable. One instrument did have explanatory power in the first stage: the use of a particular university loan program that incidentally subsided brokers' commissions upon sale in certain situations. The second-stage point estimates of the key parameter were consistent with those reported in Table 2, but the standard errors were large. We were therefore compelled to address these concerns through different methods.

A. Unobserved characteristics of homes

Many aspects of home quality are, of course, observable to sellers, buyers, and brokers, but unobservable to us. The sharp contrast between the broker coefficients in specifications (1) and (2) indicates that the use of a broker is positively correlated with observed characteristics that enhance a home's value. For example, larger homes are more likely to sell through brokers than smaller homes. Since brokers earn more from the sale of more valuable homes, this pattern is consistent with their incentives, and may reflect targeted efforts to obtain valuable listings. If the same pattern holds for *unobserved* characteristics that contribute to a home's value, then specifications (2) and (3) will tend to overstate the effect of using a broker on a home's selling price.

Many of the relevant unobserved characteristics of a home – location, views, architectural style, and so forth – remain reasonably stable over time. In specification (4), we immunize our estimates against the influence of such unobserved characteristics by including home fixed effects. This strategy is feasible because our sample period covers a reasonable long period of time (27 years), during which many homes were sold multiple times. Our 755 observations pertain to 466 separate homes. Of those, 277 were sold once during our sample period, 116 were sold twice, 51 were sold three times, and 22 were sold four or more times. In total, there are 478

observations on the 189 homes that were sold multiple times. Due to the regime shift that occurred during the 1990s, virtually all of the early sales occurred without brokers, while the later sales were fairly evenly split between brokered transactions and FSBOs (see Figure 1). Therefore, the sample provides good opportunities to, in effect, compare the changes in selling prices for homes that transitioned from FSBO to brokers with the changes in selling prices for homes that remained FSBOs.

With home fixed effects included (specification (4) in Table 2), the broker coefficient falls to -0.0603 with a standard error of 0.036 (implying a price impact of -5.9 percent). The measured effect is now negative and significantly different from zero at the 90% confidence level ($p = 0.096$), consistent with the hypothesis that brokers have incentives to expedite sales, even at a lower price. We also decisively reject the hypothesis that the brokers pay for themselves, i.e., that broker coefficient equals 0.0513. Notice that for this specification, many of the other control variables are absorbed into the home fixed effect.[23]

Specifications (1) through (4) do not allow for the possibility that the prices of different types of homes may evolve differently over time. If, for example, the prices of high-end homes were falling (rising) over time relative to those of low-end homes (as a result of, say, shifts in the distribution of income), then the observed concentration of brokered sales among high-end homes during the latter portion of our sample period would imply that the broker coefficient is biased downward (upward). To examine this possibility, we estimated a probit regression explaining the likelihood that the seller used a broker as a function of all time-invariant home characteristics plus year dummies. We used the estimated equation to compute fitted probabilities (propensity scores) for each home in a fixed year (2000). Finally, we re-estimated specification (4), adding interactions between this propensity score and each of the year dummies (specification (5) in Table 2). This specification allows for the possibility that the prices of the types of homes sold through brokers evolved differently over time than the prices of the types of homes sold without brokers, and it places minimal structure on the manner in which those paths differed. To account for the fact that propensity scores are estimated, we bootstrap the standard errors.

[23] Though home renovations can lead to changes in certain variables such as the number of bedrooms, bathrooms, and square footage, such changes are relatively rare in our data, and their effects are not usefully identified.

The coefficients of the year-propensity interaction terms (not shown in the table) exhibit a general tendency to rise over time, which implies that the coefficient of the broker indicator in specification (4) is likely biased *upward*. Indeed, the coefficient of the broker indicator in specification (5) is -0.0782, implying a price impact of -7.5 percent. The negative price effect is even larger (in absolute value) than the corresponding effect in specification (4), and it is significantly different from zero at a higher level of confidence (p = 0.0361). Specification (6) is the same as (5), except that we also control for the amount of time the owner lived in the home (by adding linear and quadratic terms), which as before reduces our sample size. The coefficient of the broker indicator changes only slightly to 0.0796, implying a price impact of -7.7 percent. We reject the hypothesis that the coefficient is equal to zero at a similar level of confidence (p = 0.035). Variants of specifications (5) and (6) that use propensity scores constructed from estimates of a simple linear probability model rather than a probit regression yield similar results.

The disadvantage of using a fitted propensity score is that it introduces additional estimation error and thereby reduces precision. An alternative is to allow for differential time trends by interacting the year dummies with a single aspect of home quality. Typically, that strategy does indeed yield sharper estimates, and we reject the hypothesis that the broker coefficient is equal to zero at an even higher level of confidence. Focusing on specification (6), the broker coefficient is −0.0573 (s.e. = 0.0244, p = 0.019) when we use square feet as our measure of home quality, −0.1032 (s.e. = 0.0276, p=0.000) when we use acreage, and −0.0723 (s.e. = 0.0333, p = 0.029) when we use the number of bedrooms.

The possibility remains that the quality of a given home may have varied over time, and that short-term variations in quality may be correlated with the use of a broker. We have seen, however, that brokers tend to sell higher quality homes, and that as a result improvements in our controls for quality tend to reduce the broker coefficient. Consequently, it would appear unlikely that such considerations explain why the measured effect is negative.

B. Unobserved characteristics of sellers

Each seller chooses whether to engage a broker. Consequently, the use of a broker may be correlated with unobserved characteristics of the seller that influence the selling price. Conceptually, the direction of the resulting bias is unclear. A seller who is more concerned about his net yield (and who is therefore more likely to obtain a higher price with or without a broker) may be either more or less likely to use a broker, depending on whether he finds brokers' claims cred-

ible. A seller who has more confidence in his own negotiating abilities may be less likely to use a broker, as well as more likely to obtain a higher price, unless his confidence is unwarranted.[24]

If unobserved seller characteristics are reasonably stable over time, then it would be possible in principle to remove their influence by including seller fixed effects. Unfortunately, only 166 observations in our sample involved sellers who sold at least one other home. After controlling for seller fixed effects and home characteristics, too few degrees of freedom remain to measure the broker coefficient with useful precision.

The available data do, however, permit us to conduct an informative diagnositc investigation of seller heterogeneity. First we examine correlations between fitted residuals across observations involving the same household. If unobserved heterogeneity manifests itself in the form of household fixed effects (e.g., some individual is a particularly effective negotiator or consistently more sensitive to price), we would expect to observe a strong positive correlation between the residuals for pairs of observations where the same household is on the same side of both transactions (i.e., it is the buyer in both instances or the seller in both instances), and a strong negative correlation between the residuals for pairs of observations where the same household is on opposite sides of the two transactions (i.e., the buyer in one instance and the seller in the other). Based on specification (2) in Table 2, we find that residuals are effectively uncorrelated across pairs of observations where the same household is the buyer in both instances ($\rho = 0.033$, s.e. = 0.103, N = 65), *negatively* correlated across pairs of observations where the same household is the seller in both instances ($\rho = -0.112$, s.e. = 0.090, N = 76), and *positively* correlated across pairs of observations where the same household is the seller in one instance and the buyer in the other ($\rho = 0.078$, s.e. = 0.074, N = 136).[25] Thus, there is no evidence that selling prices depend on persistent household heterogeneity.

The possibility remains that a household's decision to use a broker may be spuriously related to transitory changes in its economic status or preferences. For example, if the inclination to engage a broker is negatively correlated with the inclination to negotiate aggressively (e.g.,

[24] A substantial body of evidence suggests that people tend to be overconfident (see, e.g., Ehrlinger and Dunning, 2003, or Meer & Van Wesep, 2007). Those with low competence are particularly likely to overestimate their abilities (see, e.g., Kruger and Dunning, 1999).

[25] Because the correlations between these fitted residuals depend on fitted coefficients, we bootstrapped the standard errors for the correlation coefficients. Using residuals created by specification (3) and specification (4) results in qualitatively similar correlations.

because short-term financial pressure reduces the first inclination and enhances the second), then the estimates of brokers' effects on selling prices in Table 2 are presumably biased downward.

To investigate that possibility, we ask whether sellers who use brokers obtain better terms when acting as *buyers* than sellers who do not use brokers. In our sample, we have 125 observations (the "paired buyer sample") for which the buyer is a seller in some other paired observation (the "paired seller sample"). Of the 125 transactions in the paired seller sample, 108 did not involve brokers while 17 did. While the latter group is quite small, it provides a meaningful basis for some comparisons. Notably, more than 61 percent of the buyer/seller observation pairs (77 of 125) involve transactions separated by less than 12 months. Typically, these are cases in which a household moved from one campus home to another. Presumably, any factor – whether permanent or transitory – that enhances a household's proclivity to negotiate aggressively when acting as a seller creates a similar proclivity when the household acts as a buyer in a roughly contemporaneous transaction.

To gauge the buyer's success at negotiating the terms of each deal, we compute the discount received from the (log) asking price; i.e.,

$$\text{Discount} = \log(\text{asking price}) - \log(\text{selling price})$$

Our strategy is to compare the magnitudes of discounts across the following three groups:

Group A: the observation belongs to the paired buyer sample and the buyer used a broker when acting as a seller in the paired seller observation

Group B: the observation belongs to the paired buyer sample and the buyer did *not* use a broker when acting as a seller in the paired seller observation

Group C: the observation does *not* belong to the paired buyer sample

If those who use brokers are more aggressive or effective negotiators than those who do not, we should observe the largest discounts on average in group A, the second largest in group C (which is not selected based on broker usage), and the smallest in group B. If those who use brokers are less aggressive or effective negotiators than those who do not, that ranking should be reversed.

Using our full sample, we regress the discount on two dummy variables, one for group A observations and the other for group C observations, as well as year and home fixed effects (to control for the likely possibility that discounts vary systematically over time and across types of

homes).[26] The estimated value of the group A coefficient is 0.0227 (s.e. = 0.0275). The point estimate implies that the types of households who employ brokers when acting as sellers tend to be more aggressive and effective negotiators when acting as buyers than those who do not. The estimated value of the group C coefficient is 0.000589 (s.e. = 0.0181). The point estimate indicates that, when acting as buyers, households who do not employ brokers in other transactions where they act as sellers tend to be slightly less effective negotiators than the group C households, who are not selected based on whether they used brokers in other transactions where they sold campus homes. Thus, group A receives the largest discounts on average, group C the second largest, and group B the smallest by a slim margin, but the differences are extremely small and statistically significant.

We also estimate a second specification in which we control for the home's initial asking price (in logs) in addition to all the aforementioned variables. The asking price potentially acts as a proxy for considerations that may systematically influence the discount the buyer receives, such as transitory elements of home quality that are not subsumed by the fixed effect, or the degree to which the property is overpriced. With this variable added, the estimated value of the group A coefficient is -0.00692 (s.e. = 0.0311), while the estimated value of the group C coefficient is -0.00569 (s.e. = 0.0171). Thus, from this specification it appears that group B receives the largest discounts on average, group C the second largest, and group A the smallest, but the differences are tiny both economically and statistically.

We see no basis in these results for an inference that those who use brokers are significantly less aggressive or effective negotiators than those who do not, and hence no grounds for concern that that the estimates of broker effects in Table 2 are biased downward. While we acknowledge that our ability to draw definitive conclusions is limited by small group sizes (particularly for group A), which reduces precision, we note that the 95% confidence interval for the negotiating efficacy differential between groups A and B does not contain differentials large enough to offset the estimated broker effects from specifications (5) or (6) in Table 2.

[26] The regression employs 755 observations, of which 277 are effectively dummied out by the home fixed effects (i.e., the correspond to homes for which we have only one observation).

4. INITIAL ASKING PRICES

In this section we examine the possibility that brokers influence transaction prices in part by encouraging sellers to set lower initial asking prices. We estimate the same six specifications as in Table 2, except that we use the log of the initial asking price as the dependent variable. Results appear in Table 3. Not surprisingly, specification (1), which includes only a broker dummy and year effects, indicates that initial asking prices tend to be significantly higher for homes that are sold through brokers than for those that are not. Specification (2) controls for the home characteristics discussed in Section 2, as well as for neighborhood effects. Notice that the coefficient of the broker dummy becomes negative (-0.0265, s.e. = 0.0219). Although the point estimate is economically significant, we cannot rule out the possibility that the true effect is zero. The addition of controls for the length of time the seller has lived in the home (specification (3), which is based on a smaller sample) moves the coefficient toward zero (-0.00760, s.e. = 0.0238). However, with home fixed effects (specification (4)), the broker coefficient becomes considerably more negative and significant, both economically and statistically (-0.0614, s.e. = 0.0325). Adding interactions between the brokerage propensity score and the year dummies (specification (5)) does not noticeably alter that finding: the broker coefficient falls slightly to -0.0639 (s.e. = 0.0338). For specifications (4) and (5), the coefficient is significantly different from zero at roughly the 94% level of confidence. However, adding controls for the seller's tenure in the home on top of the interaction terms (specification (6), also based on a smaller sample) moderates the measured effect (-0.0401, s.e. = 0.0320). Notably, the size of the estimated effects on initial asking price and sale price are roughly comparable in most specifications. Though the estimates are not sufficiently precise to permit a definitive inference, they suggest that much of the effect of brokers on selling prices may reflect their influence on asking prices.

5. TIME ON THE MARKET

Does the use of a broker lead the homeowner to sell his or her home more quickly? To address this question, we estimate the same six specifications as Tables 2 and 3, except that we use the log of the amount of time on the market (between initial listing and sale) as the dependent variable. Results appear in Table 4. In specification (1), which controls only for year effects, the coefficient of the broker dummy is –0.192 (s.e. = 0.081), which implies that brokered homes sell

17.5 percent faster than homes that are not brokered. That difference is significant both economically and statistically. Adding controls for home characteristics and Stanford neighborhoods has a relatively minor effect on the estimated coefficient (-0.168) and its standard error (0.088). When we add controls for the seller's tenure in the home, we find that brokered homes sell about 19 percent faster than homes that are not brokered (the coefficient of the broker dummy is –0.211, and its standard deviation is 0.093); evaluated at the mean of our sample, this finding implies that brokered homes are sold nearly 1.9 months more quickly than non-brokered homes. However, with home fixed effects, the measured effect is much smaller – only 5.7 percent (the coefficient is -0.0651) – and no longer statistically significant (the standard deviation is 0.112). Adding interactions between the brokerage propensity score and the year dummies (specification (5)) weakens the effect further: the broker coefficient falls to -0.0448 (s.e. = 0.134). However, adding controls for the seller's tenure in the home on top of the interaction terms (specification (6), also based on a smaller sample) restores the effect, albeit at a reduced level of statistical significance (-0.191, s.e. = 0.142).

When the homeowner is a reluctant seller, a home can remain on the market for an extended period of time. Such sellers may also be disinclined to use brokers, who they know will seek quick sales. The effects discussed in the previous paragraph are not, however, attributable to such considerations. For example, when the sample is limited to homes selling within twelve months (n = 595), the coefficient of the broker dummy in specification (2) remains reasonably similar: -0.183 (s.e. = 0.067). Further limiting the sample to those selling within six months (n = 463) yields a coefficient of -0.179 (s.e. = 0.061).

We can obtain additional insights concerning the effect of using a broker on time-to-sale by examining monthly hazard rates. Specifically, we estimate a series of probit models describing the probability of selling a home during the *t*-th month after placing it on the market, conditional on reaching the start of that month without a sale. Column (1) of Table 5 reports the marginal effects of using a broker – in other words, the impact on the probability of a sale. To conserve space, we have omitted the coefficients for other variables, which include a full set of home characteristics, neighborhood effects, and year effects. The results indicate that the use of a broker is associated with a slightly higher probability of sale during the first month on the market (0.0253, s.e. = 0.0160, p = 0.070), and a substantially higher probability during the second

month (0.175, s.e. = 0.056, p = 0.00). [27] Beyond the second month, there is no clear pattern. The measured effects are positive and reasonably large in the third and sixth months, but not statistically significant. However, they are negative, large, and statistically significant in the fourth and fifth months. Thus, to the extent the use of a broker reduces time-to-sale, the effect appears to involves quick sales (i.e., within two months) rather than persistently elevated probabilities.

Columns (2) and (3) of Table 5 reports the *cumulative* probability of a sale for the average home in our sample, conditional on using or not using a broker, implied by the probit regressions shown in column (1). Notice that the use of a broker raises the cumulative probability for every month. As a result of the inversion of relative hazard rates in the fourth and fifth months, the probability of selling a home without a broker nearly catches up with the probability of selling a home with a broker by the end of the fifth month, but these probabilities diverge once again in the sixth month.

Thus, we conclude that brokered homes likely sell somewhat faster than similar homes that are not brokered, owing mostly to an increased likelihood of sale within the first two months after being placed on the market. We note, however, that the specifications with home fixed effects yield ambiguous results.

6. SENSITIVITY

The qualitative results reported in this paper are robust with respect to a wide range of alternative specifications. Here we briefly summarize some of the alternatives we examined. Full results are available upon request.

Variations in market conditions. Our basic specifications control for variations in market conditions through the inclusion of year effects. We also estimated specifications with seasonal effects, half-year indicators, and quarterly indicators. Seasonal effects are marginally significant in some specifications but change the estimated effect of using a broker only slightly, as do half-year and quarterly indicators.

Buyer and seller characteristics. Additional characteristics of buyers and sellers, including age and departmental affiliation, are available for most observations. To preserve sample

[27] Due to the inclusion of year effects, all observations within a given year are dropped if all of the associated homes either sold or failed to sale within a given month after listing. That is why the sample size is smaller for the first month after listing than for the second month after listing.

size, we did not include those variables in our basic specification. Adding them sacrifices some precision, but does not meaningfully alter our findings, even though the coefficients of the additional variables are sometimes statistically significant individually and/or jointly.

Data from 2007 and 2008. As noted in the introduction, the frequency with which buyers used brokers dropped sharply from 2006 to 2007 after we circulated an early version of this paper through the FSH Office. Though we see no reason to suspect that this development would impart any particular bias, it is nevertheless arguable that the data from January 2007 through January 2008 are somehow contaminated. The coefficients of interest change slightly when those observations are removed from the sample, but our conclusions are qualitatively unaltered.

Heterogeneity across brokers. Different people may respond differently to the incentives present in principal-agent problems. It is therefore of interest to determine whether the effects of brokerage are reasonably uniform, or if they differ across companies and agents. [28] One company handled 54 of the 133 brokered sales in our sample, and another handled 39. One broker with the first company accounted for 34 transactions, and another broker with the second company accounted for 25. Accordingly, we re-estimated various specifications with additional dummy variables, either for the two lead companies or for the two lead brokers. The results suggest that the effects of interest may indeed differ across some brokers. In particular, both the selling price and the initial asking price tended to be noticeably higher when one particular broker handled transactions, and those differences were significant both economically and statistically. In specifications otherwise analogous to equation (2) in Tables 2 and 3, the estimated impact on selling price for the broker in question is 0.0856 (s.e. = 0.0374), and the estimated impact on list price is 0.0509 (s.e. = 0.0390). Otherwise, broker effects on both asking prices and selling prices were fairly uniform. In addition, the acceleration of sales appears to be almost entirely attributable to transactions handled by the two lead companies. With company dummies added to specification (2) in Table 4, the main broker effect becomes positive and statistically insignificant (0.0970, s.e. = 0.119). In contrast, the coefficients of the two company indicators are large and negative (-0.560, s.e. = 0.168, and -0.309, s.e. = 0.167). Possibly the companies with the most experience in this particular market have an advantage in selling homes quickly.

[28] Notably, Palmon and Sopranzetti (2008) find that broker quality matters in the sale of a home.

7. CONCLUSION

We have employed a unique data set to examine the separate effects of real estate brokerage services provided to sellers, *other than MLS listings*, on a home's selling price, initial asking price, and time on the market. Because a seller presumably benefits from an MLS listing, measuring the effects of real estate brokerage services *including* MLS listings (as a number of other studies have done) likely obscures the significance of agency costs. Our central finding is that a seller's use of a broker *reduces* the selling price of the typical home by 5.9 to 7.7 percent, which is consistent with the presence of a fairly severe principal-agent problem. Those estimates are statistically significant, and are obtained from specifications that include home fixed effects; some also allow for the possibility that prices may have changed over time at different rates in different market segments (e.g., that the prices of high-end homes, which are more frequently sold through brokers, may have risen or fallen relative to those of low-end homes). We find no evidence that the lower prices received by sellers who use brokers are attributable to correlations with unobserved household characteristics such as preferences or negotiation skills. Our analysis also suggests somewhat more tentatively that a seller's use of a broker may reduce the initial asking price and accelerate the sale. These results are of direct relevance to the recent policy debate over the traditional practice of bundling MLS listings with other brokerage services. They suggest that bundling may indeed compel many homeowners to purchase unwanted services (other than MLS listings) contrary to their interests.

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Figure 1
Proportion of Homes Sold Using a Broker

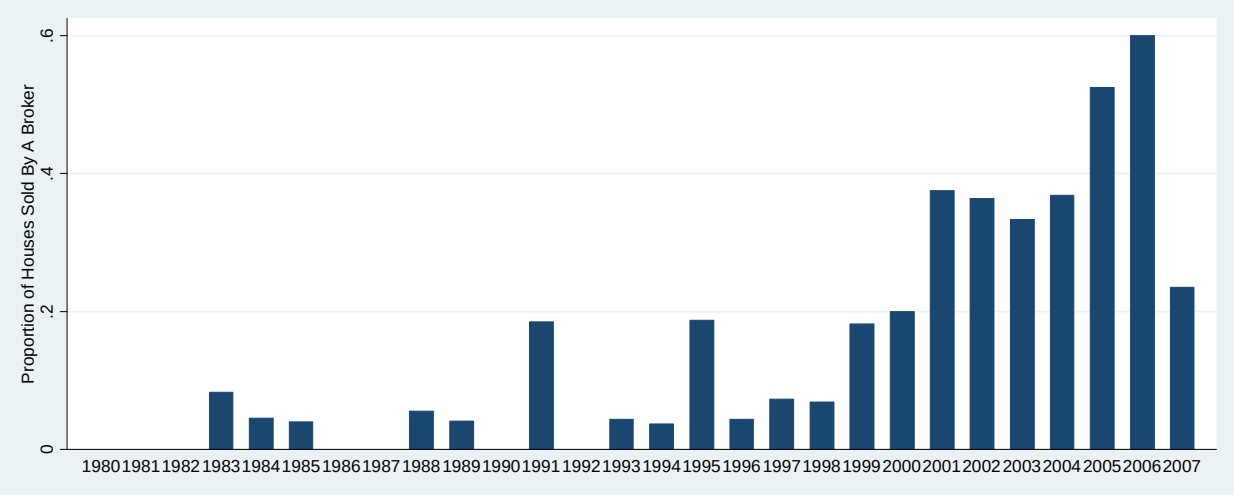

Table 1: Summary Statistics

Variable	Mean	Median	Standard Deviation	Minimum	Maximum
Selling price (2008 dollars, thousands)	795.80	659.48	507.82	162.34	3089.22
Initial asking price (2008 dollars, thousands)	855.38	712.95	562.65	168.83	4422.72
Months between initial listing and close of escrow	9.79	5	14.89	1	217
Whether the seller used a broker	0.176	0	0.381	0	1
Age of the home at the date of initial listing (in years).	26.65	22.27	18.30	2.08	98.6
Time seller had lived in the home at the date of initial listing (in years)	14.31	9.21	12.82	0.605	60.13
Whether the home has a study.	0.364	0	0.482	0	1
Number of bedrooms.	3.18	3	1.16	1	7
Number of bathrooms.	2.48	2	0.7002	1	5.5
Whether the home has a pool.	0.555	1	0.497	0	1
Square footage of the home.	2002	1931	797.0	638	6168
Size of the lot (in acres).	0.203	0.24	0.237	0	1.41
Whether the home was sold through an estate.	0.056	0	0.2294	0	1
Buyer’s age	43.5	41	11.2	24	89
Seller’s age	58.3	56	17.9	28	105

Table 2: OLS Regressions for Log Selling Price (2008 dollars)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Home fixed effects	No	No	No	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood effects	No	Yes	Yes	N/A	N/A	N/A
Year-Propensity interactions	No	No	No	No	Yes	Yes
Broker dummy	0.357 (0.0621)	0.0009 (0.0232)	0.0203 (0.0238)	-0.0603 (0.0361)	-0.0782 (0.0375)	-0.0797 (0.0378)
Home age	-	-0.0144 (0.0024)	-0.0113 (0.0027)	0.0145 (0.0093)	0.0153 (0.0086)	0.0203 (0.0101)
Home age squared	-	1.56×10^{-4} (2.78×10^{-5})	1.21×10^{-4} (2.86×10^{-5})	3.17×10^{-4} (5.85×10^{-5})	1.21×10^{-4} (8.16×10^{-5})	-1.15×10^{-4} (1.23×10^{-4})
Time in home	-	-	-0.0066 (0.0021)	-	-	-0.0014 (0.0043)
Time in home squared	-	-	9.74×10^{-5} (4.30×10^{-4})	-	-	-5.63×10^{-5} (1.44×10^{-4})
Study	-	0.0381 (0.0160)	0.0376 (0.0167)	-	-	-
Bedrooms	-	0.0207 (0.0140)	0.0286 (0.0164)	-	-	-
Baths	-	0.0446 (0.0177)	0.0300 (0.0191)	-	-	-
Pool	-	0.0607 (0.0278)	0.0481 (0.0279)	-	-	-
Square feet	-	4.72×10^{-4} (7.80×10^{-5})	5.55×10^{-4} (8.09×10^{-5})	-	-	-
Square feet squared	-	-5.72×10^{-8} (1.18×10^{-8})	-6.69×10^{-8} (1.23×10^{-8})	-	-	-
Estate	-	-0.0823 (0.0309)	-0.0510 (0.0349)	-0.1505 (0.0485)	-0.0813 (0.0682)	0.0097 (0.0649)
Acreage	-	0.532 (0.297)	0.463 (0.331)	-	-	-
Acreage squared	-	-0.360 (0.202)	-0.283 (0.207)	-	-	-
Observations	755	755	691	755	755	691
R^2	0.255	0.928	0.937	0.818	0.888	0.907

Note: Robust standard errors clustered at the home level are reported. Standard errors for specifications (4) and (5) are bootstrapped with 2000 repetitions to account for the estimated propensity score. Specifications (4) and (5) include 277 observations that are dummied out; specification (6) includes 270 such observations. The R^2 for the fixed-effects regression pertains to "within" variation.

Table 3: OLS Regressions for Log Asking Price (2008 dollars)

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Home fixed effects	No	No	No	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood effects	No	Yes	Yes	N/A	N/A	N/A
Year-Propensity interactions	No	No	No	No	Yes	Yes
Broker dummy	0.286 (0.0626)	-0.0265 (0.0219)	-0.0076 (0.0238)	-0.0614 (0.0325)	-0.0638 (0.0338)	-0.0401 (0.0320)
Home age	-	-0.01519 (0.002968)	-0.01449 (0.003229)	-0.02402 (0.008655)	-0.01681 (0.00913)	-0.01014 (0.00866)
Home age squared	-	1.46×10^{-4} (2.86×10^{-5})	1.26×10^{-4} (3.09×10^{-5})	2.78×10^{-4} (6.21×10^{-5})	1.75×10^{-5} (1.13×10^{-4})	-1.81×10^{-4} (1.09×10^{-4})
Time in home	-	-	-0.0050 (0.0024)	-	-	0.0023 (0.0037)
Time in home squared	-	-	7.86×10^{-5} (4.76×10^{-5})	-	-	-1.52×10^{-4} (1.18×10^{-4})
Study	-	0.0547 (0.0189)	0.0486 (0.0173)	-	-	-
Bedrooms	-	0.0328 (0.0172)	0.0292 (0.0183)	-	-	-
Baths	-	0.0380 (0.0189)	0.0258 (0.0209)	-	-	-
Pool	-	0.0814 (0.0304)	0.0765 (0.0312)	-	-	-
Square feet	-	3.54×10^{-4} (1.05×10^{-4})	4.85×10^{-4} (1.02×10^{-4})	-	-	-
Square feet squared	-	-3.76×10^{-8} (1.66×10^{-8})	-5.49×10^{-8} (1.67×10^{-8})	-	-	-
Estate	-	-0.0367 (0.0266)	-0.0168 (0.0295)	-0.103 (0.0413)	-0.0420 (0.0515)	0.0114 (0.0542)
Acreage	-	0.285 (0.326)	0.385 (0.326)	-	-	-
Acreage squared	-	-0.0356 (0.221)	-0.0395 (0.213)	-	-	-
Observations	755	755	691	755	755	691
R^2	0.216	0.914	0.927	0.777	0.869	0.897

Note: Robust standard errors clustered at the home level are reported. Standard errors for specifications (4) and (5) are bootstrapped with 2000 repetitions to account for the estimated propensity score. Specifications (4) and (5) include 277 observations that are dummied out; specification (6) includes 270 such observations. The R^2 for the fixed-effects regression pertains to "within" variation.

Table 4: OLS Regressions for Log Time on Market

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Home fixed effects	No	No	No	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Neighborhood effects	No	Yes	Yes	N/A	N/A	N/A
Year-Propensity interactions	No	No	No	No	Yes	Yes
Broker dummy	-0.192 (0.0815)	-0.168 (0.0884)	-0.211 (0.0928)	-0.065 (0.112)	-0.045 (0.134)	-0.191 (0.142)
Home age	-	-0.0462 (0.0125)	-0.0631 (0.0143)	-0.604 (0.0931)	-0.583 (0.105)	-0.552 (0.104)
Home age squared	-	2.36×10^{-4} (1.02×10^{-4})	3.98×10^{-4} (1.16×10^{-4})	-1.81×10^{-5} (1.44×10^{-4})	-1.31×10^{-4} (2.46×10^{-4})	1.22×10^{-4} (4.77×10^{-4})
Time in home	-	-	0.05483 (0.009159)	-	-	0.02634 (0.0168)
Time in home-squared	-	-	-1.11×10^{-3} (1.87×10^{-4})	-	-	-5.72×10^{-4} (4.60×10^{-4})
Study	-	0.122 (0.0667)	0.0548 (0.0716)	-	-	-
Bedrooms	-	0.0721 (0.0465)	0.0262 (0.0504)	-	-	-
Baths	-	-0.0107 (0.0712)	-0.0201 (0.0775)	-	-	-
Pool	-	-0.0011 (0.109)	0.0455 (0.119)	-	-	-
Square feet	-	-9.35×10^{-4} (2.93×10^{-4})	-8.03×10^{-4} (3.14×10^{-4})	-	-	-
Square feet squared	-	1.50×10^{-7} (4.45×10^{-8})	1.34×10^{-7} (4.67×10^{-8})	-	-	-
Estate	-	0.209 (0.113)	0.167 (0.124)	0.432 (0.158)	0.446 (0.202)	0.479 (0.225)
Acreage	-	-0.0020 (1.012)	0.3469 (1.207)	-	-	-
Acreage squared	-	0.635 (0.712)	0.516 (0.896)	-	-	-
Observations	755	755	691	755	755	691
R^2	0.300	0.379	0.422	0.723	0.744	0.763

Note: Robust standard errors clustered at the home level are reported. Standard errors for specifications (4) and (5) are bootstrapped with 2000 repetitions to account for the estimated propensity score. Specifications (4) and (5) include 277 observations that are dummied out; specification (6) includes 270 such observations. The R^2 for the fixed-effects regression pertains to "within" variation.

Table 5: Probit Models for Probability of Sale

Month After Listing	Number of Observations	(1) Estimated Impact of Broker on Probability of Sale, Given No Previous Sale	(2) Fitted Cumulative Probability of Sale if No Broker Used	(3) Fitted Cumulative Probability of Sale if Broker Used
First month	374	0.0253 (0.0160)	0.0533 (0.0493)	0.123 (0.0982)
Second month	513	0.175 (0.0564)	0.240 (0.151)	0.498 (0.196)
Third month	601	0.0665 (0.0569)	0.476 (0.236)	0.701 (0.188)
Fourth month	470	-0.114 (0.0275)	0.646 (0.211)	0.721 (0.189)
Fifth month	385	-0.107 (0.0286)	0.764 (0.213)	0.773 (0.192)
Sixth month	281	0.0817 (0.0806)	0.776 (0.184)	0.811 (0.165)

The left hand side variable is a dummy for selling in the *t*th month conditional on not having sold up to that point. Other right hand side variables include home characteristics, neighborhood effects, and year effects. Robust standard errors clustered at the home level are reported in parentheses. Marginal probability effects evaluated at the means of the explanatory variables are reported in column (1).