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History Dependence in the Housing Market†

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Using data on the universe of housing transactions in England and Wales over a 20-year period, we document that sale prices and selling propensities are affected by house prices prevailing in the period in which properties were previously bought. Using administrative data on mortgages, we show that cognitive frictions explain most of the history dependence in sale prices, whereas credit frictions are more relevant for selling propensities. We corroborate our analysis with data on online house listings, and we estimate the impact of history dependence on the collapse and slow recovery of housing market activity in the postcrisis period. (*JEL* E32, R21, R31)

This paper documents a pattern of history dependence in house prices and transactions by studying the universe of housing sales in England and Wales over a 20-year period. Specifically, house prices in the year a house was previously bought influence the individual price at which the house sells next, as well as the owner's propensity to sell. The effects of history dependence on house prices and the probability of sale can be material and have the potential to contribute to the persistence in prices and the pronounced volatility in sales volumes that we observe in housing markets. Consider two identical houses in the same location in 2014, one previously acquired in 2007, when aggregate prices peaked, and the other in 2001.[1] Our results show that, all else equal, the house bought in 2007 will carry a price premium of 5 percent over the one bought in 2001. Moreover, the house bought in 2007 will have, on average, 50 percent less chance of selling. (We control for tenure duration so that the results are not driven by shorter durations in the more recent period.)

History dependence is clearly at odds with a frictionless model in which the value of a house and its liquidity depend exclusively on the future stream of dividends (rental value) that the property delivers. As we define it, history dependence describes

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[1] On average, the owner of the first house will expect a 2 percent loss in nominal terms, whereas the owner of the second property will be facing a potential capital gain of 89 percent.

two frictions whose effects have been studied theoretically and empirically by the literature.

The first is credit frictions, among which a leading explanation is the so-called down payment effect, a mechanism proposed by Stein (1995). For repeat buyers, a large percentage of their down payment comes from the sale of their previous homes, and, importantly, a majority of home sales are to repeat buyers. Hence, owners who bought at high prices will have, all else equal, limited home equity; they will then have higher reservation prices and be less likely to sell than owners of comparable houses bought at lower prices, as they have less money left after their property sale.

The second type is cognitive frictions and includes mechanisms such as anchoring and learning. The notion of anchoring, or reference dependence, goes back to Tversky and Kahneman (1982) and builds on a well-established result from laboratory experiments: in estimating the value of an asset, agents tend to show a bias that overweights possibly irrelevant initial cues. In the context of the housing market, sellers may give excessive weight to the price they paid (vis-à-vis the market evolution of prices) when posting new prices; if they bought at high prices, this will lead to higher advertised prices and more time in the market. A particular kind of reference dependence is loss aversion, whereby losses have greater impact on preferences than gains (Tversky and Kahneman 1991). Genesove and Mayer (2001) provide the seminal paper for the study of loss aversion in the housing market, and we refer to the authors' work throughout this paper. With learning, reservation prices are updated slowly following specific rules, as in Anenberg (2016) and Davis and Quintin (2017). In this framework, history dependence arises because the previous purchase price of a property is an important prior in evaluating its current value.

We build on previous work by using a large new dataset with 20 million housing sales in the United Kingdom and extending the analysis in two directions. First, we analyze the effect of previous price on whether the house is sold in any given year, allowing us to investigate the quantitative implications of history dependence for aggregate housing market activity. This approach is related to the literature that has studied the relation between house prices and mobility in the United States (Engelhardt 2003; Ferreira, Gyourko, and Tracy 2012), focusing on its labor market consequences.

Second, we provide novel evidence on the relative importance of the two mechanisms behind history dependence. To disentangle cognitive frictions from credit frictions, we study a sample of properties previously bought exclusively with cash, for which the down payment effect should be muted. In this all-cash sample, we find evidence of history dependence on prices, consistent with a relevant role for cognitive frictions and, in line with Genesove and Mayer (2001), a relatively more limited role for down payment constraints. By contrast, when we turn our attention to selling propensities, we find relatively more limited evidence of history dependence for houses bought with cash, which suggests that whether a property is sold in any given year is mostly driven by credit frictions. One caveat for this analysis is that all-cash buyers are a specific subset of the population for whom cognitive frictions could have a different, and possibly less important, role in decisions, either because these buyers are wealthier or because they trade other properties—second homes or investment properties—in addition to their main residence.

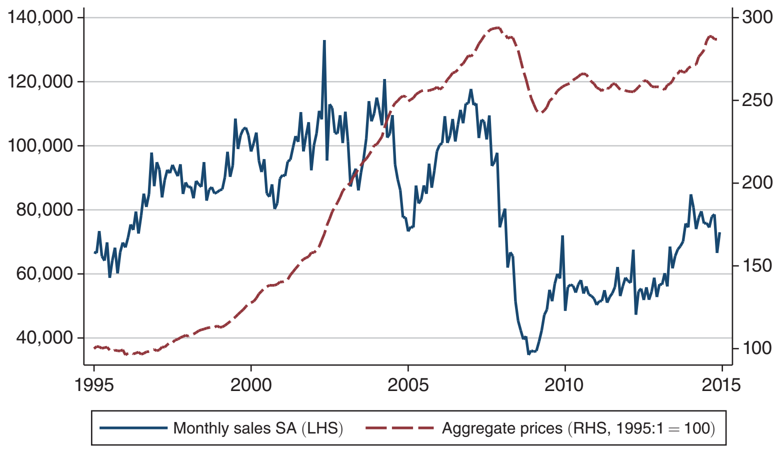

Figure 1. Monthly House Prices and Sales, England and Wales

Notes: The figure shows the monthly quality-adjusted average price and the monthly total number of transactions in England and Wales over 1995–2014. Data are taken from the England and Wales Land Registry and are quality adjusted through an hedonic regression, as described in Section II.

Because we analyze history dependence on the whole range of gains and losses (rather than focusing on loss aversion only), we can complement the evidence from the all-cash sample with an analysis of whether prices and selling propensities display a kink around zero gains, consistent with prospect theory. Both house prices and selling propensities show statistically significant concavity, but the change in slope is larger for prices, in line with the results from the all-cash sample and supportive of the importance of cognitive frictions for price determination.

Understanding history dependence is a first step to informing the design of policies aimed at preventing or reacting to future crises. In the context of the UK economy, the postcrisis period led to a collapse in the volume of transactions, illustrated in Figure 1. Transactions reached their peak in 2007 and then declined sharply. Prices reached their peak slightly afterwards, subsequently fell, and only after 2009 experienced a recovery. In the paper, we investigate the quantitative implications of history dependence for the postcrisis recovery of the housing market for different regions in England and Wales and measure the relative strengths of the mechanisms at play.

Because the mechanisms that we highlight should all be reflected in the listing process, in the last part of the paper we extend our analysis to asking prices and time on the market for a subset of properties for which we have listing information. We find significant effects of expected gains and losses on these variables.

On empirical grounds, our results are consistent with Genesove and Mayer (2001), who find strong evidence of loss aversion in the context of the Boston condominium market between 1990 and 1997. The authors report significant effects of loss aversion on list prices and time on the market and slightly less sharp effects on transacted prices. Relatedly, Anenberg (2011) analyzes the San Francisco Bay Area housing market and reports significant effects of loss aversion and leverage on transacted prices. We reinforce these findings and expand them to include the effects on the annual selling propensity of properties.[2]

On the theoretical side, our results speak in favor models for housing activity that incorporate Stein's (1995) insights on the role of credit frictions for movers but also model the decision to move, such as Ortalo-Magné and Rady (2006) and Head, Sun, and Zhou (2018).

Despite the differences in scope and markets studied, our paper finds strong evidence of cognitive frictions in line with Beggs and Graddy (2009), who study price anchoring in art auctions of modern, impressionist, and contemporary paintings in London and New York (the authors do not study selling propensities). In focusing on the role played by leverage in explaining economic activity, we join a vast literature that has documented the adverse effects of financial frictions during the crisis and postcrisis recovery. (See, for example, Mian and Sufi 2009 and the references therein.)

The rest of paper is organized as follows. Section I describes the methodology. Section II presents the data and documents the patterns of history dependence. Section III studies the potential channels underlying history dependence, and Section IV discusses its quantitative relevance. Section V contains a similar analysis on house listings from a major UK online property portal matched to the database on actual property sales, where we can examine history dependence in list prices and time on market. Section VI presents concluding remarks. The online Appendix contains additional material to complement the information in the text, as well as a disaggregated analysis of the England and Wales' regional housing markets.

I. Identifying History Dependence

The (log) house price is usually modeled as

$$p_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + w_{ijt}, \tag{1}$$

where p_{ijt} is the transaction price of house i sold at time t in local area j, v_{ij} is a property-specific fixed effect capturing time-invariant features, X_{ijt} is a vector of (time-varying) housing characteristics, δ_{jt} is the aggregate house price level at time t in local area j where i sits, and w_{ijt} is an idiosyncratic error component that contains both unobserved (time-varying) property characteristics and idiosyncratic price effects due to the features of specific transactions. In our empirical analysis, we can use property-level fixed effects because our dataset spans 20 years of transactions; such a specification is valuable since unobserved quality

[2] In a recent contribution, Guren (2018) examines the relation between local house price appreciation and list price, and uses it as an instrument to study the relation between list price and time on the market. Andersen et al. (2019) build a structural model of listing decisions based on Danish data and use it to estimate household preferences, including loss aversion. In another recent paper, Hong, Loh, and Warachka (2021) find some suggestive evidence in the Singaporean condominium market of a kink in the selling propensities at zero gains consistent with realization utility (Barberis and Xiong 2012).

is a potential threat to any analysis based on price gains—see, for instance, the discussion in Guren (2018).

To study history dependence, we augment the standard hedonic regression above with a function of the difference between today's expected sale price, $\hat{p}_{ijt}$, and the house's previous transaction price p_{ijs}:

$$p_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + f(\hat{p}_{ijt} - p_{ijs}) + w_{ijt}, \tag{2}$$

where s denotes the period when the house was previously purchased. The practical implementation of (2) hinges on the definition of $\hat{p}_{it}$—in other words, on how owners assess the expected value of their property. A simple approach is to assume that owners apply to the purchase price they paid at time s the appreciation of the local price index between s and t: $\hat{p}_{ijt} = p_{ijs} + (\hat{\delta}_{jt} - \hat{\delta}_{js})$. Equation (2) becomes

$$p_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + f\left(\widehat{GAIN}_{jst}\right) + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}, \tag{3}$$

where $\widehat{GAIN}_{jst} = \hat{\delta}_{jt} - \hat{\delta}_{js}$ is the (log) difference in aggregate house prices between time t and when the property was bought (s).[3] Notice that these are expected, rather than realized, gains. To estimate the effect of gains and losses in a nonlinear, nonparametric way, we split $\widehat{GAIN}_{jst}$ into equally sized bins for the different magnitudes of expected gains/losses (i.e., losses between -0.25 and -0.15 percent, between -0.15 and -0.05 percent, and so on). Because the literature has highlighted the seasonality of the housing market (Ngai and Tenreyro 2014), we add quarterly dummies to our main specification. We include both quarter of sale (η_t) and quarter of purchase (η_s), and we interact them.

The term $\lambda_t \otimes \lambda_s$ in equation (3) controls for all possible combinations of current year (denoted with dummy variables λ_t) and year of purchase (denoted with λ_s). Because England and Wales house prices have been trending upwards for most of the sample period, there is strong correlation between property tenure and $\widehat{GAIN}_{jst}$. Our control allows us to take this into account and is robust to potential change in tenure effects over the 20 years covered by our sample.[4] An additional advantage of this interaction term is the ability to control for time-varying unobserved property characteristics associated to a given pair of years that are homogeneous across England and Wales.

However, our coefficient of interests on $\widehat{GAIN}_{jst}$ could still be biased by other time-varying property characteristics not captured by $\lambda_t \otimes \lambda_s$. For instance, a possible correlation between home improvements and house prices (as in

[3] In Genesove and Mayer (2001), $\widehat{GAIN}_{jst} = \hat{\delta}_{jt} - \hat{\delta}_{js} - w_{ijs}$, where w_{ijs} is the error coming from estimating (1) on the previous purchase of the property. Because their specification does not use repeat sales, this term includes time-invariant property characteristics not captured by their hedonic model. In our methodology, w_{ijs} only includes time-varying characteristics of the property or noise specific to its previous transaction. In online Appendix C, we run the analysis using their measure and confirm the presence of history dependence in the data.

[4] This additional control could end up absorbing a substantial amount of variation in $\widehat{GAIN}_{jst}$ by controlling for nationwide price effects. In practice, we show in the online Appendix that results are similar whether we include $\lambda_t \otimes \lambda_s$ in the regression or not. We also show the output from a specification with simple tenure dummies (for each possible value of $t - s$); results are similarly unaffected.

Choi, Hong, and Scheinkman 2014) would affect the analysis, to the extent that the rate at which properties are renovated or extended differs across postcode districts. To address this remaining threat, we run a robustness check on the subsample of properties that have not been subject to major renovations.[5]

To measure the effect of history dependence on selling propensities, we start from an equation similar to (3) but with a $0/1$ indicator as dependent variable. This indicator takes the value one when the property was sold in a given year and zero otherwise. Using this approach, a property appears in the dataset each year after its first registered sale (we cannot compute the ongoing $\widehat{GAIN}_{jst}$ before this first sale):

$$q_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + f\left(\widehat{GAIN}_{jst}\right) + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}, \tag{4}$$

where q_{ijt}, is an indicator variable for whether property i in postcode district j was sold in year t.

Since a generated regressor ($\widehat{GAIN}_{jst}$) is the main variable of interest in both the transaction price regression (3) and the selling-probability equation (4), we bootstrap the entire two-step procedure (the estimation of the local indices that allows us to compute $\widehat{GAIN}_{jst}$ and the estimation of the main regressions (3) and (4)) to estimate standard errors. Because the process requires significant computing time (we block-bootstrap at the postcode district level the entire England and Wales Land Registry), we show bootstrapped confidence intervals only for the main results (Figures 2, 3, and 4) and leave nonbootstrapped standard errors in Section V (analysis of list prices) and the robustness checks in the online Appendix.[6]

II. History Dependence in Transaction Prices and Selling Propensities

The first part of this section describes our main data source, the England and Wales Land Registry (LR), which contains 20 years of residential transactions from January 1995 to December 2014 (HM Land Registry 1995–2014b). We explain how we compute our measure of local aggregate house prices and how we construct our two estimation datasets—one to analyze transaction prices and one to analyze selling propensities. We then show the results for history dependence.

A. *Data and Summary Statistics*

The LR records all residential property transactions, with few exceptions.[7] The dataset contains close to 20 million sales for 20 years of data—that is, approximately 1 million sales per year. For each sale, the LR contains the precise postcode, the

[5] England and Wales Energy Performance Certificate data for residential properties (Ministry of Housing, Communities, and Local Government 2018) record every instance in which a property's energy consumption and efficiency have changed. This includes any renovation where the floor area of the house is altered or the roof, walls, or windows are modified.

[6] The online Appendix also contains a nonbootstrapped version of Figures 2, 3, and 4 for comparison.

[7] The exceptions are listed at http://www.landregistry.gov.uk/market-trend-data/public-data/price-paid-data, where a public version of the dataset is available. Most of the excluded transactions refer to sales that were not for full market value, for example a transfer between parties on divorce.

street name, the street number, and the apartment number if the property belongs to a multiunit building. The LR records three attributes of the property, which we use as our X_{ijt} in our empirical analysis modeled on regression (3): its type (flat, terraced, semidetached, detached), whether the property is new, and the tenure type of the property (freehold or leasehold).[8] The type of the property is time invariant and does not enter most specifications of this paper, where we use property fixed effects. Whether a house is new and whether it is sold as a leasehold are time-varying characteristics. Date of Transfer in LR is the day written on the transfer deed—that is, the date of completion, when keys and funds change hands.

Before analyzing history dependence, we use the LR to compute the aggregate level of house prices needed to create the $\widehat{GAIN}_{jst}$ variable. We do so at the postcode district level, by running regression (1) at an annual frequency for each postcode district in England and Wales. Our procedure has therefore two steps: we first estimate equation (1) to compute expected gains and losses and then estimate (4) to compute the effect of interest.[9] Our dataset includes 2,345 postcode districts; the average postcode districts contains around 10,000 individual addresses. We keep our analysis at the annual frequency because this allows a more straightforward analysis of selling propensities, for which we need to expand our dataset in a property-by-year format.

Analysis of Transaction Prices.—Our empirical analysis relies on the identification of repeat sales. We consider two sales as happening on the same property when they share the same postcode, street name, street number, apartment number (if any), and property type (flat, terraced, semi, detached). Transaction prices from repeat sales allow us to create both a measure of realized gains ($GAIN_{jst}$) and a measure of expected gains for the regression analysis ($\widehat{GAIN}_{jst}$). Figure A1 in the online Appendix shows the two similar distributions of realized and expected gains.

Table 1 shows descriptive statistics for the analysis of transaction prices, and it distinguishes between "sales" and "properties" to highlight the presence of repeat sales. The first column displays statistics for the entire LR. while the other columns only include properties that appear at least twice in the LR. Since 2002, the LR dataset can be augmented with an additional variable ("charge") that indicates the use of a mortgage to purchase the property (HM Land Registry 2002–2014a).[10] Since 2005, the UK Financial Conduct Authority (FCA) has been recording information on all owner-occupier mortgages into the product sales data (PSD) (Financial Conduct Authority 2005–2014).[11] These more restricted samples contain more flats

[8] A leasehold is a tenancy arrangement by which someone buys a property for a limited number of years, usually 99, 125, or 999. It is typically associated with flats and is a time-varying characteristic of the property, because leaseholds can be converted to freeholds (see Giglio, Maggiori, and Stroebel 2015; Bracke, Pinchbeck, and Wyatt 2018). As such, we use a dummy indicator for leasehold tenancy as part of our control variables.

[9] In our baseline estimates, the first step does not include any measure of gains and losses, as these are not taken into account by market participants when estimating house price indices at the local level. In the online Appendix, we show results from an alternative setup that incorporates history dependence in the first step—we run an iterating procedure to make the $\hat{\delta}$ and $\widehat{GAIN}$ terms consistent across the two steps. This alternative setup yields similar results to our baseline methodology.

[10] This variable is not available in the public dataset but can be purchased from the Land Registry.

[11] The PSD include regulated mortgage contracts only and therefore exclude other regulated home finance products, such as home purchase plans and home reversions, and unregulated products, such as second charge lending and buy-to-let mortgages.

TABLE 1—SUMMARY STATISTICS, ANALYSIS OF TRANSACTION PRICES

	Land registry All sales 1995–2014	Sales with previous purchase in 1995–2014	Sales with previous purchase in 2002–2014 (funding data)	Sales with previous purchase in 2005–2014 (mortgage data)
Sales (Observations)	19,628,516	7,527,731	3,199,389	1,385,653
Properties	12,089,086	5,038,658	2,570,092	1,234,381
Current sale price (p_{ijt})				
Mean	161,266	184,100	211,919	231,694
p1	18,500	25,250	40,000	50,000
p25	70,500	93,000	119,000	125,000
p50	124,500	145,000	165,000	176,500
p75	195,000	220,000	243,000	250,000
p99	755,000	825,000	925,000	1,095,000
Property type (*proportion*)				
Flat	0.18	0.19	0.22	0.24
Terraced	0.31	0.34	0.34	0.32
Semi	0.28	0.27	0.26	0.25
Detached	0.23	0.20	0.19	0.19
Lease	0.23	0.24	0.27	0.28
New	0.10	0.00	0.00	0.00
Expected log capital gains ($\widehat{GAIN}_{jst}$)				
Mean		0.41	0.18	0.04
p1		−0.13	−0.16	−0.19
p25		0.11	0.02	−0.03
p50		0.33	0.13	0.03
p75		0.67	0.29	0.10
p99		1.24	0.75	0.43
Years between previous purchase and current sale (DUR_t)				
Mean		4.42	3.57	3.21
p1		0	0	0
p25		2	1	1
p50		4	3	3
p75		6	5	5
p99		16	11	8
Matched-in variables (*mean*)				
Bought with mortgage			0.72	0.71
Bought with LTV $>$ 80 percent				0.25

Notes: The analysis of transaction prices is based on microdata from the England and Wales Land Registry (LR) for the years 1995–2014. The first column contains information on all the sales included in the LR. The second column describes the sample used in the analysis: it is made of all properties that have at least two sales in the dataset and excludes for each property the first of such sales. (The first sale provides us with the previous price or the previous aggregate price index to include in the regression that checks for history dependence.) The third column is similar to the second but only refers to properties whose first sale took place after 2001. For this sample, we can tell whether the property was purchased with a mortgage and investigate the mechanisms behind history dependence. Finally, the fourth column describes properties whose first sale took place after March 2005 and can potentially be matched to the product sales data (PSD), a dataset of residential mortgages where we can identify the initial LTV with which a house was bought.

and more leasehold properties. There are no new properties in these samples, since transactions are part of repeat-sale pairs and the first purchase (which could potentially refer to a new house) is not part of the analyzed data (it is used to construct the $\widehat{GAIN}_{jst}$ variable). The use of property fixed effects further reduces the sample to houses with three transactions (two of which have nonmissing $\widehat{GAIN}_{jst}$) during the 20 years covered by the data. In the online Appendix, we run a robustness check without property fixed effects, which ensures that history dependence holds for a more general sample of properties. The sales described in the third and fourth column of Table 1 allow us to investigate the mechanisms behind history dependence thanks to the additional information on how housing purchases were financed.

Given the aggregate movement in house prices shown in Figure 1, for most households in England and Wales, homeownership has produced gains rather than losses—as shown by the descriptive statistics on $\widehat{GAIN}_{jst}$ in Table 1. Additional calculations, not reported in the table, reveal that the second column of the table contains 0.5 million sales with an expected loss (a negative $\widehat{GAIN}_{jst}$) out of 7.5 million transactions.

Analysis of Selling Propensities.—To estimate the impact of history dependence on a property's selling propensity (and, in aggregate, on the number of transactions), we reshape and expand the dataset so that each house has an observation in each year since its first appearance in the LR (its first sale after 1995). With 12 million properties and 20 years, the final extended dataset has over 120 million rows. (The average property appears for the first time in the middle of the sample, and we can follow it for ten years.) To keep the empirical analysis computationally manageable, we extract a 10 percent random sample of the properties. We create a variable, q_{ijt}, which equals one if property i sells in year t and zero otherwise. We treat the first sale as missing because we do not observe $\widehat{GAIN}_{jst}$ before that observation. This approach is similar to the analysis of transaction prices, in which the first sale is not part of the estimating sample. However, while the use of property fixed effects in the price analysis implies that we need three sales for each property, the house-by-year structure of the selling probability analysis does not require us to rely on multiple-sale houses.

B. *History Dependence*

The left-hand-side part of Figure 2 shows the effect of gains and losses on transaction prices. The analysis is based on regression (4).[12] All regressions control for individual property fixed effects and time-varying characteristics (whether the property was purchased new or secondhand, whether it was sold as leasehold or freehold), as well as all combinations of purchase and sale year. The regressions include year-by-postcode district fixed effects to control for average local prices.

In the charts, negative bin values indicate losses. A loss between 25 and 15 percent is associated to a 3 percent increase in the transaction price; a loss between 15 and 5 percent is associated to a 1.5 percent increase. By contrast, gains are associated to lower transaction prices (as compared to the baseline category of properties that expect to break even). The most populated bin (gains between 35 and 45 percent) is associated with a 4 percent decrease in the transaction price.

[12] Table A1 and Table A2 in the online Appendix show the regression coefficients.

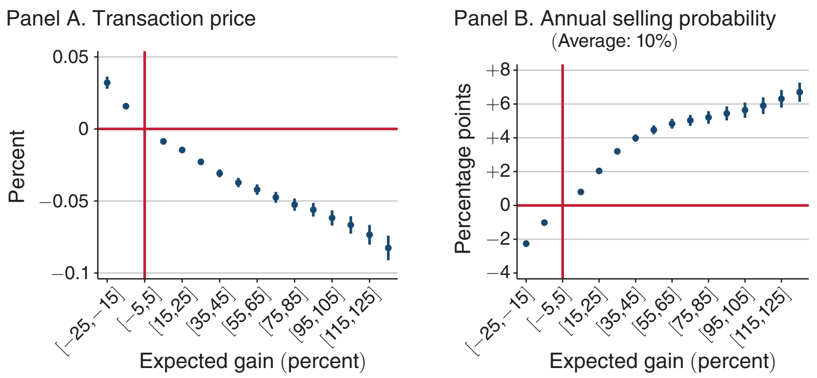

Figure 2. Effects of Gains and Losses

Notes: The charts show the coefficients and corresponding bootstrapped 95 percent confidence bands for the k dummy variables associated with different expected gains/losses ($\widehat{GAIN}_{jkst}$) in the regression $y_{ijt} = \nu_{ij} + X_{ijt}\beta + \delta_{jt} + \sum_k \gamma_k \widehat{GAIN}_{jkst} + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}$, where y_{ijt} is the transaction price (p_{ijt}) in panel A and an binary indicator of sale (q_{ijt}) in panel B. The precise values of the coefficients are reported in Tables A1 and A2 in the online Appendix. All regressions control for individual-property fixed effects and time-varying characteristics (whether the property was purchased new or secondhand, whether it was sold as leasehold or freehold), as well as all combinations of purchase and sale year. Regressions have year-by-postcode district fixed effects (δ_{jt} in the regression formula).

Standard errors get bigger for larger gains because there are fewer properties with long holding periods. Moreover, for long tenures, the collinearity between year of purchase-sale and $\widehat{GAIN}_{jst}$ increases substantially. (Only properties with a long holding period experience capital gains of more than 100 percent.)

The right-hand-side part of the figure shows the effect of expected gains and losses on selling propensities. We aim at investigating whether the purchase price of a property affects the likelihood that a house is traded in any subsequent period. As explained in the methodology section, we use a linear model—equation (4)—with a binary dependent variable indicating whether the property was sold in any given year. We get a similar picture to the one for transaction prices, albeit with the reversed sign. Losses induce lower selling propensities, and gains induce higher selling propensities. While the unconditional annual transaction probability of a house in the sample is 5 percent, as indicated by Table 2, properties with repeat sales are traded more often by construction. For those properties, the unconditional transaction probability is 10 percent, and we should compare the magnitude of the effects against this number. Properties expecting a capital loss between 25 and 15 percent have a selling propensity that is 2 percentage points lower in any given year. From there, the effect on selling propensities is gradually increasing with expected gains, reaching a positive 4 percentage points for gains between 35 and 45 percent. Once again, the effect flattens out slightly for large—above 35 percent—expected gains.

Figures A3 and A4 in the online Appendix replicate Figure 2 for each region in England and Wales.[13] The pattern of transaction prices and selling propensities appears to be similar across the different regions.

[13] To assign our property postcodes to the correct UK regions and local authorities, we use the National Statistics Postcode Lookup (Office for National Statistics 2014).

Table 2—Summary Statistics, Analysis of Selling Propensities

	Properties bought in 1995–2014	Properties bought in 2002–2014 (funding data)	Properties bought in 2005–2014 (mortgage data)
Property $\times$ year observations (N)	13,788,911	6,766,378	3,629,414
Sales	721,385	300,962	128,121
Properties	1,167,571	860,635	634,300
Sell probability (Sales/N)	0.05	0.04	0.04
Purchase price (p_{ijs})			
Mean	122,400	172,495	204,461
p1	16,000	23,500	45,000
p25	55,000	96,000	123,239
p50	90,000	144,000	168,500
p75	154,000	207,000	237,000
p99	535,000	675,000	785,000
Expected log capital gains ($GAIN_{jst}$)			
Mean	0.43	0.15	0.02
p1	−0.17	−0.20	−0.23
p25	0.08	0.01	−0.05
p50	0.30	0.10	0.02
p75	0.76	0.26	0.08
p99	1.33	0.78	0.41
Years since purchase (DUR_t)			
Mean	5.82	4.48	3.67
p1	1	1	1
p25	2	2	2
p50	5	4	3
p75	8	6	5
p99	17	12	9
Property type (proportion)			
Flat	0.15	0.19	0.20
Terraced	0.30	0.31	0.31
Semi	0.29	0.28	0.28
Detached	0.25	0.23	0.21
Lease	0.21	0.24	0.25
New	0.10	0.10	0.10
Matched-in variables (averages)			
Bought with mortgage		0.73	0.74
Bought with LTV $>$ 80 percent			0.48

Notes: The table shows descriptive statistics of the dataset used to analyze the selling propensity of properties in any given year. The dataset is created by taking the LR samples (whose descriptive statistics are shown in Table 1) and expanding them so that each house has an observation in each year since its first appearance in the LR. (For the empirical analysis, we create a variable that equals one if property i sells in year t and zero otherwise.) To keep the computational burden manageable, for the analysis of selling propensities, we extract a 10 percent random sample of the data.

Alternative Specifications.—Regression (4), on which we base our main analysis, is designed to control for as many confounding factors as possible. It is instructive to check whether the patterns identified here are also found with less stringent specifications. The results of this analysis are presented in the online Appendix, Figures A5–A8.

In our first alternative specification, we do not include purchase-year and sale-year combinations ($\lambda_s \otimes \lambda_t$). This is equivalent to not controlling for holding period and other time-varying factors that are homogeneous across England and Wales. The results on transaction prices are similar to before, although with larger standard errors, indicating that including tenure increases the precision of estimates. The results on selling propensities display the same increasing pattern, but magnitudes and standard errors are much larger, implying—as expected—that it is necessary to control for holding period when analyzing house selling propensities.

In another specification we do not include individual property fixed effects. The overall pattern of history dependence appears to be the same, but effects tend to revert back to zero for larger gains. One possible explanation for this result is that neglecting granular fixed effects makes estimates less precise, especially when focusing on large expected gains. A related, alternative regression uses full-postcode rather than individual property fixed effects. Since, in the United Kingdom, a full postcode corresponds to 15 properties on average, this specification allows us to still control for granular effects (albeit not property-specific ones) while avoiding the reduction in sample that comes with the use of repeat sales. Results are similar to the baseline case but a little smaller quantitatively.

Finally, we run a separate regression for properties that have been flagged as extended to check that our results are not driven by time-varying property improvements. The England and Wales Energy Performance of Buildings Data list for each individual property whether an extension was added. The data are drawn from Energy Performance Certificates issued for domestic and nondomestic buildings constructed, sold, or let since 2008. Figure A9 in the online Appendix shows that there is no significant difference in the history dependence displayed by these two groups of properties.

III. Mechanisms: The Role of Credit and Cognitive Frictions

To measure the relative importance of credit and cognitive frictions in explaining history dependence in the housing market, we look for sellers in the data that are likely to be credit constrained in their next house purchase, using the presence of a mortgage and of, in particular, a high loan-to-value ratio (LTV) as a proxy. While history dependence for this group would combine both cognitive and credit frictions, any history dependence in the nonconstrained group—which we identify with all-cash buyers—can only be explained by cognitive frictions.

However, the *absence* of history dependence for the nonconstrained group would not automatically prove that cognitive frictions do not matter, because all-cash owners are not necessarily representative of the universe of sellers.[14] Therefore, the second part of this mechanism section analyzes nonlinearities in history dependence. The presence of a kink in prices and selling probabilities responses around zero gains constitutes another indication of cognitive frictions, orthogonal to the distinction between constrained and unconstrained sellers.

[14] The online Appendix Table B1 compares the observed characteristics of properties bought with a mortgage and properties bought with cash. Properties bought with cash are usually less expensive, except at the top of the price distribution (above the ninety-ninth percentile).

A. *Constrained versus Unconstrained Sellers*

We identify constrained borrowers as those who were financed by a mortgage, and among them, we define highly constrained borrowers as those with a current estimated LTV greater than 80, which corresponds to the median LTV in the PSD and is a common threshold used in the literature to identify equity-constrained borrowers (Genesove and Mayer 2001, Anenberg 2011).

Because data availability varies depending on the analyzed subperiod, we run two regressions.[15] In the first regression, we focus on the fact that information on whether the property has been bought with a mortgage or with cash is available since 2002. We augment our baseline regression (3) as follows:

$$(5) \quad y_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + \sum_k \gamma_{0k}\Big(\widehat{GAIN}_{jkst} \times upto2001\Big) + \phi_0\, upto2001$$
$$+ \underbrace{\sum_k \gamma_{1k}\widehat{GAIN}_{jkst}}_{\text{cognitive frictions}} + \underbrace{\sum_k \gamma_{2k}\Big(\widehat{GAIN}_{jkst} \times mortgage\Big)}_{\text{credit frictions}} + \phi_2\, mortgage$$
$$+ \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}.$$

By adding the interaction between the gain variable and the indicator of mortgage usage in the regression, we can separate the effects of gains that are independent of mortgage finance (cognitive frictions) from the ones that depend on being credit constrained. (We also use an indicator for pre-2002 transactions, *upto*2001, so that we control for the effects on properties that have been bought early in the sample and for which we do not know the source of funding. In practice, *upto*2001 is absorbed by the origination-by-sale-year dummies.)

In the second regression, we focus on properties bought after 2005:I, for which we have information on the characteristics of the mortgage used. For this group of properties, we can use an additional interaction with the gain variable on top of those used in equation (5):

$$(6) \quad y_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt}$$
$$+ \sum_k \gamma_{0k}\Big(\widehat{GAIN}_{jkst} \times upto2005{:}\mathrm{I}\Big) + \phi_0\, upto2005{:}\mathrm{I}$$
$$+ \underbrace{\sum_k \gamma_{1k}\widehat{GAIN}_{jkst}}_{\text{cognitive frictions}} + \underbrace{\sum_k \gamma_{2k}\Big(\widehat{GAIN}_{jkst} \times mortgage\Big)}_{\text{credit frictions: low LTV}} + \phi_2 mortgage$$
$$+ \underbrace{\sum_k \gamma_{3k}\Big(\widehat{GAIN}_{jkst} \times ltv80\Big)}_{\text{credit frictions: high LTV}} + \phi_3\, ltv80 + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}.$$

[15] Figure A14 in the online Appendix overlaps the coefficients shown in Figure 2 with those obtained from restricting the history dependence analysis only to the subsamples used to investigate the mechanisms. The same pattern of history dependence holds.

The data at our disposal only contain information about the LTV at origination, when the house was purchased. We estimate current LTV by projecting forward both the outstanding loan amount and the property value. For the former, we use a standard amortization formula; for the latter, we use postcode-district-level house price appreciation, as captured by our price indices.

We show results graphically in Figures 3 and 4 and in tabular form in Tables A1 and A2 in the online Appendix. The regressions are run on the same sample as before to preserve the property fixed effects. However, the figures show the coefficients on the interaction between $\widehat{GAIN}_{jst}$ and the relevant period (post-2001 or post-2005:I transactions) so that we can focus on the additional information available in those subsamples.

The analysis brings forward potentially different mechanisms for history dependence in transaction prices and selling propensities. In Figure 3, the baseline effect on transaction prices (top-left chart) is reminiscent of the result on the entire sample (Figure 2): transaction prices decline with higher gains. The results are, however, noisier, with larger standard errors. The top-right chart shows that the additional effect of $\widehat{GAIN}_{jst}$ for properties bought with a mortgage are limited, except for large gains.

In the regression on selling propensities, both the baseline category and the properties bought with a mortgage display a pattern similar to the main result, with selling propensity increasing with expected gains. However, effects are sharper for properties bought with a mortgage. The coefficients for the cash sample are statistically significant only for large gains. Given that the coefficients on properties bought with a mortgage represent the additional effect of leverage on top of the baseline effect showed in the bottom-left chart, results suggest that credit frictions play an important role in this type of history dependence.

We run the same analysis focusing on post-2005:I transactions, where we can distinguish the effect of properties bought with a high leverage (i.e., with an LTV higher than 80 percent) from the effect on other mortgage-funded properties. Similar to the analysis of post-2001 transactions, the top row of the figure shows that most of the effect of expected gains on prices is present for properties bought with cash, but standard errors are large. The bottom row of the figure shows that most of the effect of history dependence on selling propensities comes from properties bought with a mortgage. Within this group, there is both an effect on properties bought with a low leverage and an additional effect on properties bought with an LTV greater than 80 percent.

In Figure A15 in the online Appendix, we overlap the coefficients on properties bought with cash from Figures 3 and 4, to better compare their magnitude. The effects are consistent across samples; while the point estimates for selling propensities appear slightly weaker in the latest data, the difference is not statistically significant.

Online Appendix Figure A16 shows that the results of the analysis are extremely similar if we use historical rather than current LTV. Historical LTV does not depend on $\widehat{GAIN}_{jkst}$, which enters our estimation of the updated value of the house, used to compute the current LTV.

The results using constrained and unconstrained sellers indicate that cognitive frictions are more important for transaction prices and that credit frictions are more

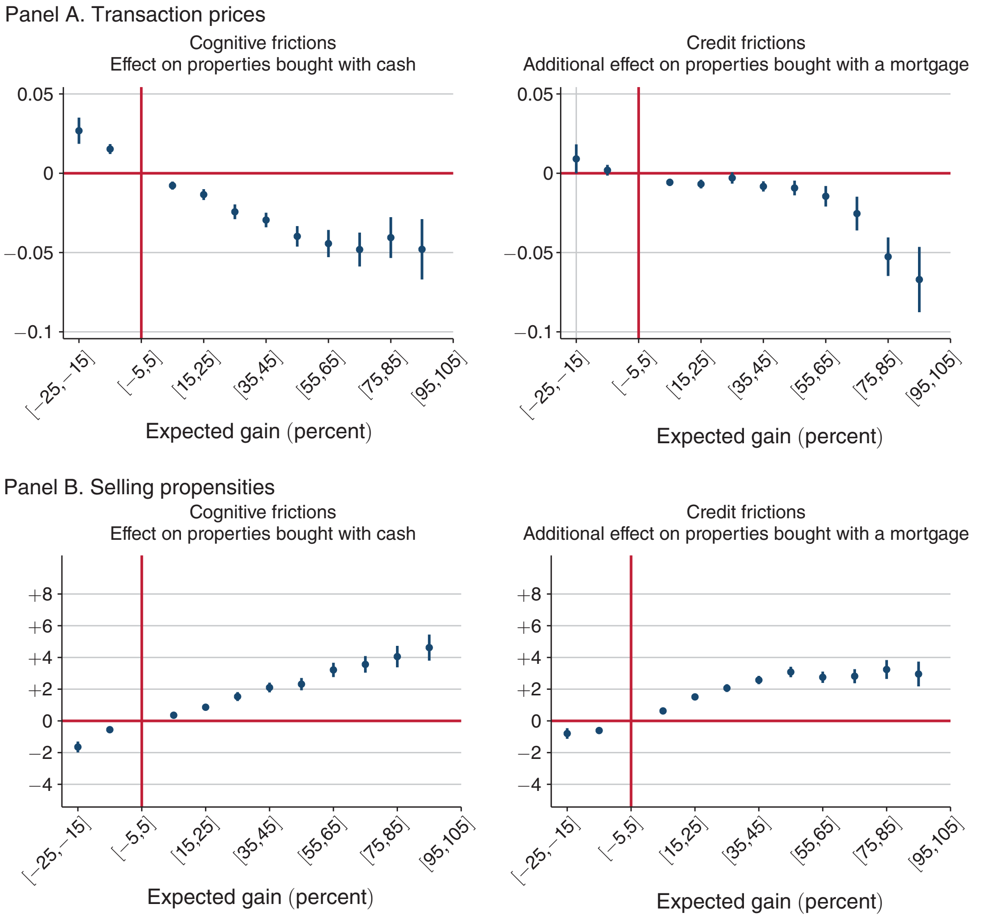

FIGURE 3. CREDIT VERSUS COGNITIVE FRICTIONS (2002–2014)

Notes: The charts replicate the analysis of Figure 2 but focus on the results for properties purchased after 2001, for which information is available on whether the transaction was financed with cash or with a mortgage. The regression is $y_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + \sum_k \gamma_{0k}(\widehat{GAIN}_{jkst} \times upto2001) + \sum_k \gamma_{1k}(\widehat{GAIN}_{jkst} \times post2001) + \sum_k \gamma_{2k}(\widehat{GAIN}_{jkst} \times mortgage) + \lambda_t \otimes \lambda_s + w_{ijt}$, and the charts report the $\hat{\gamma}_{1k}$ and $\hat{\gamma}_{2k}$ values. The indicator variable *post*2001 singles out properties for which a purchase is available after 2001; the indicator variable *mortgage* tags properties bought with a mortgage. The regression coefficients are reported in Table A1 and A2 in the online Appendix.

important for selling propensities. In the second part of this section, we check these results using a different empirical approach: checking for the presence of kinks in the effects of gains and losses.

B. *Linearity and Prospect Theory*

Prospect theory predicts a value function that is steeper for losses than for gains (Tversky and Kahneman 1991, Genesove and Mayer 2001). Therefore, searching for a change in the slope of history dependence effects can provide further evidence on the relative importance of cognitive and credit frictions.

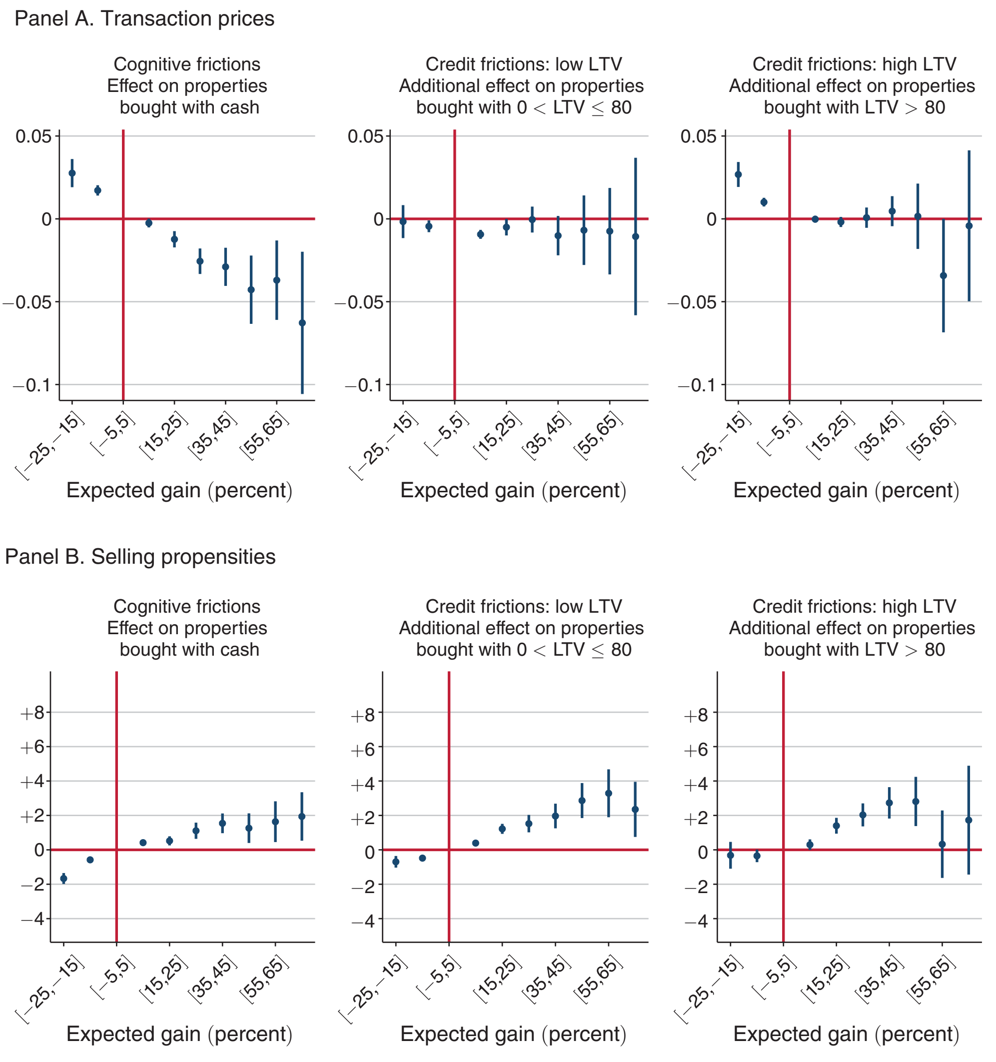

Figure 4. Effects of Credit and Cognitive Frictions after 2005:I

Notes: The charts replicate the analysis of Figure 2 but focus on the results for properties purchased after March 2005, for which information is available on the characteristics of the mortgage used to finance the transaction. The regression is $y_{ijt} = X_{ijt}\beta + \delta_{jt} + \sum_k \gamma_{0k}(\widehat{GAIN}_{jkst} \times upto2001) + \sum_k \gamma_{1k}(\widehat{GAIN}_{jkst} \times post2005{:}\mathrm{I}) + \sum_k \gamma_{2k}(\widehat{GAIN}_{jkst} \times mortgage) + \sum_k \gamma_{3k}(\widehat{GAIN}_{jkst} \times ltv80) + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}$, and the charts report the $\hat{\gamma}_{1k}$, $\hat{\gamma}_{2k}$, and $\hat{\gamma}_{3k}$ values. The indicator variable *post*2005:I singles out properties for which a purchase is available after March 2005; the indicator variable *mortgage* tags properties bought with a mortgage; *ltv*80 indicates properties that were bought with a loan-to-value (LTV) ratio greater than 80. Information on the characteristics of mortgages is available from the Product Sales Data (PSD) since March 2005. The match between Land Registry (LR) and PSD, described in online Appendix B.2, generates four subsets of *post*2015:I transactions: matched properties bought with a high LTV, matched properties bought with a low LTV, properties that were bought with cash according to the LR, and properties that were bought with a mortgage according to the LR but do not match with the PSD. All these groups are included in the regression; the latter group is controlled for through a group-specific dummy. The precise values of the coefficients are reported in Tables A1 and A2 in the online Appendix.

Table 3—Linearity and Loss Aversion

Dependent variable	No break (1)	$x = 0$ (2)	$x = 0.2$ (3)	$x = 0.4$ (4)	$x = 0.6$ (5)
Panel A. Transaction price (p_t)					
$\widehat{GAIN}_{jst}$	−0.073	−0.068	−0.073	−0.074	−0.075
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
$\widehat{GAIN}_{jst} \times \mathbf{1}(\widehat{GAIN}_{jst} < x)$		−0.144	−0.014	−0.007	−0.009
		(0.031)	(0.007)	(0.003)	(0.002)
Observations	4,285,851	4,285,851	4,285,851	4,285,851	4,285,851
Panel B. Selling propensity (q_t)					
$\widehat{GAIN}_{jst}$	0.081	0.074	0.081	0.083	0.084
	(0.009)	(0.010)	(0.009)	(0.009)	(0.008)
$\widehat{GAIN}_{jst} \times \mathbf{1}(\widehat{GAIN}_{jst} < x)$		0.094	0.004	0.022	0.021
		(0.027)	(0.012)	(0.005)	(0.003)
Observations	13,745,918	13,745,918	13,745,918	13,745,918	13,745,918

Notes: The table shows selected coefficients from regressions of the form $y_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + \gamma_0 \widehat{GAIN}_{jst} + \gamma_1 \widehat{GAIN}_{jst}\, \mathbb{I}\{\widehat{GAIN}_{jst} < x\} + \lambda_t \otimes \lambda_s + w_{ijt}$, where $\widehat{GAIN}_{jst}$ enters linearly and interacted with a dummy variable indicating that $\widehat{GAIN}_{jst}$ is below a threshold x. This specification is used to test whether the reaction of prices and selling propensities is steeper for negative gains, indicating loss aversion. The other variables and the sample are as in Figure 2.

To do so, we run a restricted version of our baseline regression (4):

$$y_{ijt} = v_{ij} + X_{ijt}\beta + \delta_{jt} + \gamma_0 \widehat{GAIN}_{jst} + \gamma_1 \widehat{GAIN}_{jst}\, \mathbf{1}\left\{\widehat{GAIN}_{jst} < x\right\} + \eta_t \otimes \eta_s + \lambda_t \otimes \lambda_s + w_{ijt}. \tag{7}$$

In this specification, capital gains enter in a linear fashion, augmented by an additional effect for gains lower than x, which is meant to capture loss aversion. The regression coefficients for different levels of x are reported in Table 3; the first column contains the estimated γ_0 coefficient when no loss aversion term is added to the regression. Compared to this benchmark, the specification with $x = 0$ yields the most notable change in γ_0 while at the same time producing the most statistically significant γ_1, consistent with loss aversion. Moreover, the additional contribution to the slope of the $\widehat{GAIN}_{jst}$ effect becomes smaller, but mostly still significant, for expected gains above 0.2, 0.4, or 0.6, revealing a declining marginal utility of price consistent with prospect theory. The estimated difference in coefficients between the two regions (negative and positive expected gains) sits well with previous findings of the literature, as summarized, for example, in Andersen et al. (2019).

Comparing the upper and bottom panel of Table 3 can give some insight on the role of cognitive and credit frictions in determining house prices and transaction probabilities. The additional slope for losses is 50 percent larger for prices, which is consistent with cognitive frictions being relatively less important for selling probabilities.

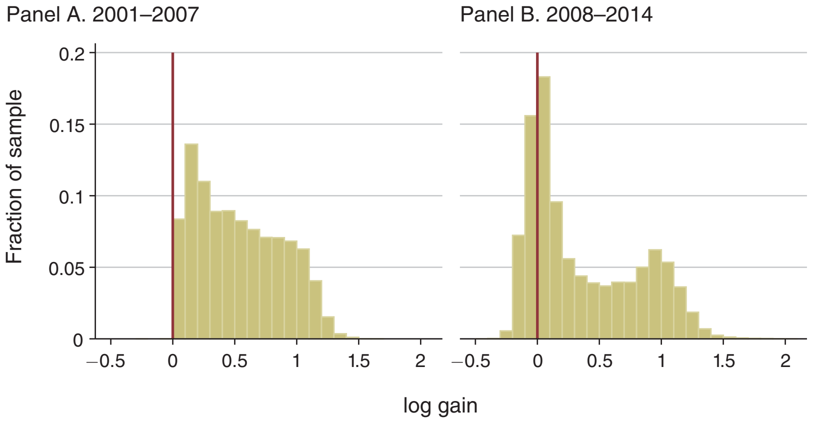

Figure 5. Distribution of Ongoing Capital Gains, Precrisis and Postcrisis

Notes: The charts show the distribution of the $\widehat{GAIN}_{jst}$ variable in two subperiods: 2001–2007 and 2008–2014. The bin width replicates the allocation of dummy variables used to split $\widehat{GAIN}_{jst}$ and compute the coefficients shown in Figure 2, 3, and 4. For each property, $\widehat{GAIN}_{jst}$ is computed as the difference between the current estimated log house price index and the log index when the house was purchased. The indices are calculated at the local authority level. The distributions are estimated for the analysis of selling propensities, and hence $\widehat{GAIN}_{jst}$ is computed for each property in each year since it first appeared in the Land Registry—these are current expected gains rather than realized gains.

IV. The Post-2007 Fall in Transactions

As shown in Figure 1, after 2007 the aggregate number of housing transactions in England and Wales did not return to its precrisis level even after seven years, in 2014. Can the results on history dependence be related to this fall in housing market activity?

We first compare the distribution of ongoing expected capital gains in the two periods, 2001–2007 and 2008–2014, using all the properties (not just those that were eventually sold) in our 10 percent random sample. Figure 5 shows that there were practically no losses in the 2001–2007 period and that the bulk of properties was in the 0–100 percent capital gain interval. By contrast, in 2008–2014, a few properties were experiencing potential losses, and many other properties had expected gains close to zero.

In 2001–2007, the average annual selling propensity for a property was 7.7 percent; this propensity fell to 3.3 percent in the 2008–2014 period. To compute the contribution of history dependence to this fall, we first calculate the change in each of the bins of the expected gain distribution between the two periods, then multiply these differences by the coefficients obtained from the regression on selling propensities and shown in the lower half of Figure 2. By summing all these numbers, we get the total contribution, in percentage points, of history dependence to the fall in transactions: -1.4. Since the total fall in transactions between the two periods was 4.4 percentage points, history dependence explains around one-third of the fall.

The fall in transactions in the postcrisis period happened in conjunction with house price resilience: without history dependence house prices in England and

Wales would have experienced a larger fall. To estimate the size of this counterfactual drop, we employ the same method as above: we multiply the changes in the bins that make up the distribution of expected gains by the coefficients shown in the upper half of Figure 2. The overall effect on prices is more modest: we find that England and Wales house prices would have been 1 percent lower in the absence of history dependence.

Given the reduced-form analysis of this paper, this exercise must be taken as suggestive of the *potential* impact of history dependence rather than a precise quantitative estimation of general equilibrium effects.

V. Extension: List Prices and Time on the Market

In this section, we study history dependence in the selling decision. The analysis is based on data from WhenFresh, a company that collects all daily listings from Zoopla, a major UK property portal (WhenFresh 2008–2014). Using this source allows us to study list prices and time on the market for properties that were advertised for sale in England and Wales after 2008. Many of these properties can be matched back to a previous purchase on the LR. Some of these properties were later sold and recorded again on the LR.

A. *Data and Summary Statistics*

Zoopla is the second UK property portal in terms of traffic. Its dataset starts in November 2008. In this paper, we restrict our attention to sale listings where an address can be precisely identified. The dataset contains information on the address of properties, list prices, and property attributes (such as property type and number of bedrooms).

Zoopla collects data only from estate agents, not individual sellers. In the United Kingdom, most transactions occur via estate agents. In 2010, only 11 percent of homes were sold privately (see Office of Fair Trading 2010).

Table 4 shows the descriptive statistics for the WhenFresh/Zoopla dataset. The table contains information on both the dataset used to analyze list prices (the first two columns) and the dataset used to study the monthly probability of sale once advertised (the last two columns). In both cases, the table shows separate statistics for the entire sample of advertised properties and the sample of properties that were actually sold (as indicated by a match between the listing data in WhenFresh/Zoopla and the transaction data in the Land Registry). Because of the way history dependence is measured, all samples are restricted to those properties for which a *previous* sale was identified in the Land Registry.

The annual selling propensities that are analyzed in the previous sections of this paper can be split in two components: (i) whether an owner decides to list a property and (ii) conditional on listing, whether the property sells. This section mainly focuses on (ii). Online Appendix Figure A17 refers to (i): we run the same regression as (4) but use "being listed on Zoopla" as our dependent variable. In our data, there does not appear to be a significant effect of gains and losses, except in cases with larger losses (above 15 percent), for which the likelihood of being listed is lower.

Table 4—WhenFresh/Zoopla Summary Statistics

	Prices		Probabilities of sale	
	All (previous LR record)	Sold (matched with LR record after listing)	All (previous LR record)	Sold (matched with LR record after listing)
Listings (Observations)	2,601,406	1,127,866	2,601,406	1,127,866
Properties	2,040,936	1,079,646	2,040,936	1,079,646
Monthly observations			13,800,249	5,261,150
List price (l_t)				
Mean	232,658	236,199	228,792	236,315
p1	59,950	64,950	60,000	64,950
p25	130,000	139,950	129,950	139,950
p50	185,000	189,995	180,000	189,995
p75	275,000	275,000	270,000	275,000
p99	925,000	900,000	899,950	899,950
Property type (*proportion*)				
Flat	0.16	0.15	0.16	0.15
Terraced	0.32	0.33	0.31	0.32
Semi	0.29	0.31	0.29	0.31
Detached	0.23	0.21	0.24	0.22
Bedrooms	2.84	2.81	2.85	2.82
Lease	0.21	0.19	0.22	0.20
New	0.10	0.10	0.11	0.10
Capital gains ($GAIN_t$)				
Mean	0.28	0.31	0.28	0.30
p1	−0.19	−0.17	−0.20	−0.18
p25	−0.00	0.01	−0.01	0.00
p50	0.11	0.13	0.10	0.13
p75	0.54	0.59	0.56	0.59
p99	1.27	1.29	1.26	1.27
Years since last purchase (DUR_t)				
Mean	6.68	6.97	6.73	6.94
p1	0	0	0	0
p25	3	4	4	4
p50	6	6	6	6
p75	9	10	9	10
p99	17	17	17	17
Months since listing (TOM_t)				
Mean			4.40	3.57
p1			1	1
p25			2	2
p50			4	3
p75			6	5
p99			12	10

Notes: The table contains statistics for the subset of WhenFresh/Zoopla listings for which it was possible to retrieve a *previous* purchase in the Land Registry (LR) (the matching procedure is described in online Appendix B.3). “All” refers to this entire sample, whereas “Sold” contains listings that match a *subsequent* sale in the LR. The first two columns report statistics for the analysis of list prices; the third and the fourth columns describe the dataset used to analyze the time on market of listed properties. The latter dataset is created by expanding the original sample for list price analysis so that each advertised property has an observation in each month since its appearance on Zoopla until its sale or withdrawal. (We truncate the number of months at 12 when there is no sale.)

Similar to the analysis of unconditional selling (or listing) propensities, the analysis of conditional probabilities of sale is performed on an expanded dataset where each row corresponds to a property-time observation. In this case, the time dimension is monthly; we allow for properties to stay on the market for up to 12 months, as in Anenberg (2016)—in this way we avoid cases in which property listings are simply "forgotten" on the website.

B. *History Dependence in List Prices and Time on the Market*

The nonparametric results on the effect of $\widehat{GAIN}_{jst}$ are displayed in Figure 6. Because the WhenFresh/Zoopla data start in 2008, using individual property fixed effects, as in the first part of this paper, would restrict the sample to properties that transacted multiple times in a time window of only a few years. For this analysis, we use full-postcode fixed effects instead.

The top-left chart of Figure 6 is derived from the sample of all listings; the chart shows that sellers who expect a loss tend to post higher list prices, whereas properties that are experiencing a gain tend to post a lower price. This is consistent with the analysis on actual prices in the previous section, although the effect appears smaller when compared to Figure 2. Figure 6, on the left-hand side of the middle row, shows the results for the sample of properties that were eventually sold. The effects, especially the discounts on properties that enjoy substantial expected gains, are larger and comparable to Figure 2.[16] This intriguing difference seems to suggest that discounts associated with large expected gains help the selling process.

The results on the rate at which a house sells once it has been advertised on the property portal (top-right and middle-right charts) are consistent with this interpretation. When analyzing the sample of all listings, for which price effects are muted, monthly probabilities of sale vary significantly between properties with different expected gains. By contrast, when analyzing the sample of sold properties, probabilities of sale are relatively more homogeneous.

The bottom-left chart in Figure 6 reports the effect on transaction prices for properties advertised on Zoopla that were actually sold. The effects of expected gains are similar to the ones on list prices and reminiscent of the results for the entire LR sample in Figure 2. The effects on implied discounts, defined as the difference between list and transaction price, are relatively small, reaching around 1 percent for properties with large expected gains, but consistent with the idea that sellers expecting large gains are more willing to accept lower offers. The similarity between effects on listing and transaction prices seems to indicate substantial seller bargaining power.

VI. Conclusion

This paper investigates history dependence in the housing market using the universe of housing transactions in England and Wales in the last 20 years. We find that house prices in the year a house was previously bought influence the price at which

[16] Online Appendix Figure A18 replicates the selling-propensity chart of Figure 2 for the properties that appear in the Zoopla listing. The pattern of history dependence in annual selling propensities is the same as in the main sample.

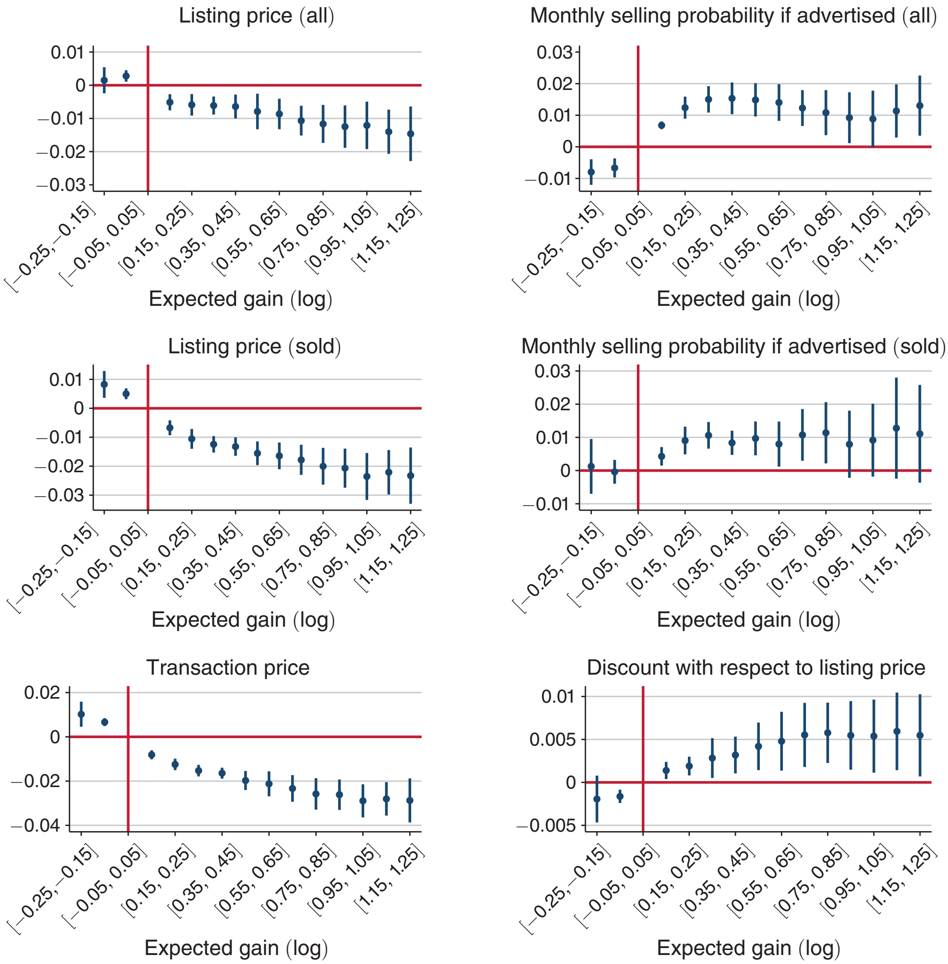

FIGURE 6. EFFECTS OF GAINS AND LOSSES ON LIST PRICES AND TIME ON THE MARKET

Notes: The charts report the coefficients and associated 95 percent confidence bands on the $\widehat{GAIN}_{jkst}$ dummy variables in the regression $y_{ijt} = \phi_h + X_{ijt}\beta + \delta_{jt} + \sum_k \gamma_k \widehat{GAIN}_{jkst} + \lambda_s \otimes \lambda_t + w_{ijt}$, where ϕ_h represents full-postcode fixed effects (in contrast to the individual property fixed effects in the first part of the paper). The confidence bands in the chart are computed through standard errors double clustered by year and local authority. The two charts in the upper row refer to the entire sample, made of all listings that have appeared on the Zoopla property portal since 2009, provided that a previous sale of the same property can be retrieved from the Land Registry (LR). The dependent variables are the property list price (l_t) in the first chart and a monthly selling indicator (h_t) in the second chart. The middle row replicates the analysis of the upper row on the *Sold* subsample, made of the subset of listings that can be matched with a subsequent sale in the LR, provided that the sale occurs within 12 months of the listing. Also, the bottom row shows results estimated from the *Sold* subsample. The bottom-left chart is based on a regression where the dependent variable is the final transaction price (p) of properties, whereas the bottom-right chart reports results of a regression on the discount between listing and transaction price ($l - p$).

the house sells next, as well as the likelihood that a transaction takes place. Our data allow us to separate properties that were bought with a mortgage and properties that were bought with cash. For a subsample of the data, we can also separate out properties with a high-LTV mortgage.

We provide novel evidence on the relative importance of the two mechanisms behind history dependence, cognitive and credit frictions. In the sample of properties bought with cash, we find evidence of history dependence on prices, consistent with a relevant role for cognitive frictions. When we turn our attention to selling propensities, we find relatively more limited evidence of history dependence in the same sample, suggesting an important role for credit frictions. Consistent with this insight, the kink in history dependence effects around zero gains—one of the signs of loss aversion—is larger for prices than for selling propensities.

In the last part of the paper, we find evidence of history dependence for advertised prices; sellers appear to have enough bargaining power to pass through a significant part of their history premia to transaction prices.

Our findings raise interesting trade-offs in an environment in which housing market activity is history dependent. In particular, while higher house price growth could spur more housing market activity today, it raises the need to sustain this growth in the future, feeding in the unsettling need for potentially spiraling house prices.

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