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Essays in Behavioural Science

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Dedicated to my late parents. Mum wanted me to become a doctor and Dad to get into computing.

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Declaration

I declare that..

The acknowledgements should be followed under a separate heading by a declaration in which the author indicates any material contained in the thesis which s/he has used before or which the author has had published. The declaration shall state that the thesis is the candidate's own work except where it contains work based on collaborative research, in which case the nature and extent of the author's individual contribution shall be indicated.

Authors 2 and 5 proposed the initial concept. All authors contributed to the design of the analysis and the interpretation of the results. Authors 1 and 2 constructed variables and prepared all figures and tables. Author 3 wrote the initial draft, Author 1 conducted a review of the existing literature, and Author 2 wrote up the results. All authors contributed to the revision. Author 4 established collaboration with LBG. Authors 3, 4, and 5 secured funding for the research.

The author shall also confirm in the declaration that the thesis has not been submitted for a degree at another university.

Abstract

Chapter 1 explains how this thesis presents four independent research projects in behavioural science centred on the high stakes setting of residential property transactions. The aim is to provide a better understanding of individual decision making and to illustrate some of the field's contemporary overarching debates.

Chapter 2. The disposition effect is the reluctance to sell assets at a loss relative to a salient point of reference. Typically, that referent has been assumed to be the purchase price, but other values can also assume prominence as reference points. We test the idea that the *peak price* achieved by an asset during the owner's period of holding constitutes an additional salient reference point for asset owners that overlaps, and interacts, with the purchase price reference point. We demonstrate this using data documenting price changes and selling decisions for both housing and stocks.

In *Chapter 3* we estimate the effect of rank position of online adverts on the clicks received using a novel approach to ameliorate selection issues found in related research. We reconstruct over 9 million search results from real-world online property listings data. There is random variation in each target advert's ranking because its position moves unpredictably day-by-day as new properties list or existing properties change price or delist. We find an exponential decline in clicks as rank decreases but our results are limited by the assumptions and data exclusions made.

Chapter 4. In many settings, nudges are thought to be powerful, low-cost tools for changing economic outcomes while preserving free choice. We evaluate the economic effects of a common form of nudge that makes one item more salient in the choice set by moving the item to the first-ranked position. Our setting is the online property listings market, whereby vendors can pay a premium to nudge their property listing to being first ranked in the choice set. Using a randomised control trial, we show that this nudge drastically increases property viewings on the website, by over 30%, but has no economically significant effect on the sale price achieved. Further evidence suggests this nudge generates economically inefficient congestion, slowing time taken to achieve the sale. Our study suggests that behavioural research needs to look beyond the immediate impact of a nudge (on, for example, attention) to the downstream economic impact, where effects may be different to those intended or observed when looking only at proximate, near-term outcomes.

In *Chapter 5* we attempt to replicate the results from a study by Thomas et al. (2010) which found that precise list prices led to a higher sales prices. Using their data and a larger novel dataset, we could broadly replicate their effect when list prices are greater than sales prices. However when list price is less than sales price we found a larger reverse effect which the original study does not report.

Chapter 6 summarises our findings.

Abbreviations

eCDF .... empirical Cumulative Distribution Function
CT .... Click Through
LI .... Long Island
OLS .... Ordinary Least Squares
ONS .... Office for National Statistics
PEA .... Participating Real Estate Agent
PH .... Proportional Hazard (survival model)
RCT .... Randomized Control Trial
SCM .... Synthetic Control Methods
SERP .... Search Engine Results Pages
SFL .... South Florida
(S)STC...... (Sale) Subject to Contract

1 Introduction

1.1 Context

People's homes may be filled with meaning (Smith, 2005), but for the majority of owners their property is also their most valuable asset (Campbell, 2006) and its sale, the highest stake transaction that they will enter into. According to a survey by the consumer issues magazine *Which* only divorce beats house buying and selling as life's most stressful event (Blake, 2016). The transactions require buyers and sellers to make a number of important choices: buyers select from a large choice set; sellers decide how to market their property and both parties make decisions about timing and will want to achieve the best price. Although housing transactions are common in aggregate, on an individual level they are rare so people have few learning opportunities.

Empirical evidence from the behavioural sciences shows that people do not make decisions by following standard economic models. Instead their choices are subject to cognitive biases such as reference dependency, commonly associated with prospect theory (Kahneman and Tversky, 1979), where people evaluate outcomes relative to salient reference points rather than make absolute judgements. They also take mental short-cuts or heuristics (Gigerenzer et al., 1999) e.g., tending to anchor on the first available piece of information they encounter (Tversky and Kahneman, 1974) rather than optimising their choices by using all relevant data (Gigerenzer and Selten, 2002).

This thesis presents four independent research projects in behavioural science (hereafter referred to by their chapter numbers 2 to 5) that use field data and are centred on property transactions. Its aim is to improve our understanding of individual decision making and to contribute to three of the discipline's broader debates: the use of novel methods, the effectiveness of nudges and the replication crisis.

Each research project stands alone but share one or both of our main data sources. Listings data (used by Chapters 3, 4, and 5) was provided by a property portal who also collaborated in our field study. Sales transactions data (used by Chapters 2, 4, and 5) came from an administrative dataset. These data enable us to investigate specific behavioural influences on different aspects of the transaction process from the owner's decision to sell (Chapter 2), consumers viewing online adverts (Chapters 3 and 4), making an agreed offer (Chapter 4) and deciding on price (Chapters 4 and 5).

1.2 Engaging with Debates

Novel Methods

The research methods used in this thesis benefit from the transformation of the social sciences in the information age with the availability of large-scale granular behavioural data and the mass ownership of digital devices. We carry out secondary data analysis on mass transaction datasets, incorporating quasi-experimental designs to establish causality and more classically conduct a randomised control trial (RCT) in the field (DellaVigna, 2009) – albeit online. These contemporary methods present opportunities and challenges.

The data used in the research was created by individuals as a by-product of them carrying on their everyday lives. It was collected unobtrusively so should lack reactivity, unlike other data collection methods, such as laboratory experiments, where the knowledge that they are research participants may affect people's responses (Lee, 2000). However since the data was not specifically created for social research purposes it is not necessarily optimal for the required analysis. In Chapter 3 our findings are limited by the data, whereas in Chapters 2 and 5 we augment our data with datasets from other public bodies and other countries respectively.

To generalise their findings, quantitative researchers may strive to collect a representative sample that accurately reflects the population from which it is drawn. In Chapter 2 our administrative dataset has all the English & Welsh housing transactions over 24 years so providing external validity. Very large data sets can also enable researchers to find relationships within subpopulations (Fan et al., 2014). In Chapters 4 and 5 we are apply strict selection rules to our data and still have sufficient observations to carry out robust statistical analysis.

Conversely, with Big Data it is easier to find statistically significant relationships which in practice are meaningless or spurious (Jeffreys, 1939; Fan et al., 2014). However in Chapter 2 we find the same effect in two different markets and using different analytical techniques; in Chapter 3 our findings are consistent with other studies; in Chapter 4 the results for a series of outcomes give a consistent picture and in Chapter 5 the findings are consistent using data from three countries.

Synthetic control methods (SCMs) (Abadie and Gardeazabal, 2003; Abadie et al., 2010) are a quasi-experimental method which we use in Chapter 3. They estimate causal effects by the manufacture of a simulated control group as the counterfactual to the treatment group using observational data. The synthetic control group is constructed to have similar characteristics to the treatment group; with matching methods used so that they have a similar distribution of observable covariates.

SCMs have become a widely adopted empirical research tool e.g., to investigate the impacts of German reunifcation (Abadie et al., 2015), economic liberalization on national income in a large sample of countries (Billmeier and Nannicini, 2013) and minimum wages (Allegretto et al., 2017). There use is not restricted to economics, they have been employed in numerous fields including health (Pieters et al., 2016), and marketing (Fisher et al., 2019; Gill et al., 2017). Indeed Athey and Imbens (2017, p9) claim that SCMs are "the most important innovation in the policy evaluation literature in the last 15 years."

However can these observational methods imitate the identification quality of RCTs? A comparison of the results from 15 large scale field trials conducted on a leading online social networking website to the results from the most widely used SCMs, found the observational methods often showed different effects (Gordon et al., 2019). We also compare results from a RCT in the field to those from a SCM and draw a similar conclusion.

RCTs by contrast, are a very well established research method for establishing whether an intervention is effective. Simply, researchers randomly allocate a sample of individuals between two arms. All individuals in the experimental arm receive a particular intervention whereas those in the control arm, which acts the counterfactual, do not. Because of the randomisation, on average the individuals in the two arms are alike so any difference in outcome can be attributed to the intervention. Without randomization alternative explanations cannot be ruled out (Friedman et al., 2015)

In clinical trials RCTs are regarded as the gold standard (HMT, 2020), however their use in social analysis is more disputed. Partly this arises from the conflicting research paradigms of positivism and interpretivism. The positivist view is the social world is one external reality (Hudson and Ozanne, 1988) which is best perceived objectively with scientific research methods (Reiss et al., 2014). RCTs are an obvious candidate for this type of research since they establish causation – if you do A than B will happens; thus it is claimed RCTs provide depoliticised. scientifically neutral evidence on 'what works' (Stoker and Evans, 2016).

In comparison the interpretivist view is the social world comprises multiple realities (Hudson and Ozanne, 1988), and favour research methods such as learning from narratives to investigate the meanings people place on policy issues (Stoker and Evans, 2016). Interpretivists argue that it is overly simplistic to create a policy based on the reduction of an issue into cause and effect because reality is much more complex; moreover RCTs rather than producing evidence-based policy instead produce "authority-based" policy with debate limited to experts whilst the experiences of non-experts, particularly those from disadvantaged groups, dismissed as anecdotal (Haskins and Margolis, 2014).

The use of RCTs in public policy grew partly as a result of the increasing interest in 'nudges'. These are interventions which steer people to better choices while preserving free choice and leaving economic incentives unchanged (Thaler and Sunstein, 2009).

They are particularly suited for testing by RCT arguably to the exclusion of other interventionalist approaches which cannot be tested in this way (Ball and Head, 2021). The limits to nudges, which are contextualised below, is the focus of Chapter 3.

Limits of Nudges

In many settings, nudges are regarded as powerful tools for changing human behaviour. These innovations in 'choice architecture' have been claimed to deliver economically important outcomes with low financial cost (Thaler and Sunstein, 2009). In 2010 a 'nudge unit' to apply these insights to public policy was set up in the U.K. by the Cabinet Office and since then specialised behavioural science teams have emerged in governments across the world. However from the beginning the effectiveness of nudges in public policy has been questioned, such as in the 2011 report by the House of Lords Science and Technology Select Committee.

Meta-studies of specific choice architecture intervention techniques show that they usually have a small to medium effect and even this may be overstated due to publication bias (Jachimowicz et al., 2019; Beshears and Kosowsky, 2020; Mertens et al., 2022). The effect size depends on the type of intervention and the context. Nudges are most effective where the behavioural costs and perceived long-term consequences to the decision-maker are low, such as in the food domain, and are least effective where their impact is high, such as the finance domain (Mertens et al., 2022). Despite their generally modest effect sizes, market participants and policymakers may promote nudge-type policies where the onus is on individual behaviour change and so avoid politically unpopular and more expensive interventions which require system change (Loewenstein and Chater, 2017; Chater and Loewenstein, 2022).

There is now a growing concern that (some) nudges may only cause short-term, or proximate changes in behaviour. In many contexts, policymakers may only consider immediate outcomes when the forming the evidence base for policy changes, due to limited observable outcomes and political urgency. Although evidence of short-term, narrow effects may be sufficient to build political case for policy change,medium-term, downstream outcomes may only be detectable with a significant time lag, involve joining disparate sources of data, and tracking subjects over time (Laibson, 2020; Chater and Loewenstein, 2022).

Recent research in fields including household finance, environment science and health therefore seeks to measure short-term versus long-term, proximate versus downstream effects of common nudges. These studies reveal subsequent off-setting effects that call into question the efficacy of nudges in achieving their desired outcomes. For example changing the default so that employees are automatically enrolled into their company pension schemes did accelerate the rate of enrolment in the UK, but not hugely (Choi et al., 2004). Moreover the low minimum auto-enrolment contribution rate dragged down the prior average contribution rate (Madrian and Shea, 2001).

In Chapter 4 we build on this research in a high stakes setting in which the effects of nudging could be economically large. Unlike buying consumer goods or even equities online, for property there is a long involved process between viewing an advert online and completing the purchase. This enables us to examine both the immediate effect of a nudge on advert views and the medium term downstream outcome of completing the purchase.

Replication Crisis

The "replication crisis" arose from a series of methodological controversies which raised concerns about the reproducibility of published psychological studies (Pashler and Wagenmakers, 2012). These included the Stapel fraud affair where a distinguished scholar from Tilburg University was suspended for academic misconduct after accusations of faking data (Stroebe et al., 2012) and the public ridicule of a study by Bem (2011) which claimed evidence of paranormal abilities (Pashler and Wagenmakers, 2012). The Open Science Collaboration (2015) reran 100 studies that had been published in a top three psychology journals. They found only 36% of the replications were statistically significant compared to 97% of the original published experiments and the average effect size was less than half that previously claimed. This lack of reproducibility has been found across social sciences (Camerer et al., 2018).

Researchers have much freedom in how they carry out their work including their choices in data, hypotheses and methodology. Orben and Przybylski (2019) found, algorithmically, over 600 million ways of analyzing their 3 datasets of 300,000 observations in their study on teenage wellbeing. Similarly, Silberzahn and Uhlmann (2015) asked 29 separate research teams to investigate whether there was a relationship between the number of red cards a footballer received and their skin colour. Each team was given the same data but there were large differences in their analytical approaches and conclusions.

The careers of academic researchers depend on publishing their research in the top academic journals. However these journals are biased towards publishing statistically significant results. Simmons et al. (2011) argue researchers have too much flexibility over their choices without a requirement to disclose their rationale; that it is accepted practice for researchers to explore different alternatives until they find one that works best (i.e. gives statistically significant results) which they will then self-servingly justify as the most appropriate method. Gelman and Loken (2013) give a more generous interpretation suggesting researchers follow their data along the different routes, "a garden of forking paths" (p 1), and so unsurprisingly come to different conclusions.

Good practice is evolving with e.g., the advance public pre-registration of research protocols (Amrhein et al., 2019). Other recommendations put forward include researchers publishing two articles from the same experiment: first a theory-based exploratory paper and then a replication with pre-registered protocols (Gelman and Loken, 2013) or reviewers prioritising transparency, tolerating strange results and generally enforcing the rules (Simmons et al., 2011). In the spirit of open science and taking advantage of the large data sets that are now available Chapters 2 and 5 each include a replication of part of a published study.

1.3 The Research Projects

Chapter 2

The disposition effect is a reference dependent bias where asset owners are reluctant to sell their assets at a loss relative to a salient point of reference but are willing to sell assets that have gained value (Shefrin and Statman, 1985). Typically, that referent has been assumed to be the purchase price, but other values can also assume prominence as reference points. In Chapter 2 we test the idea that the *peak price* achieved by an asset during the owner's period of holding constitutes an additional salient reference point for asset owners that overlaps, and interacts, with the purchase price reference point. We investigate this in the two largest asset markets in which individuals participate – housing and stocks.

These two markets are distinct. Whereas individuals buy every-day products for consumption and stocks for investment purposes, their homes serve both investment and consumption motives. Traditionally, the consumption motive has received greater attention (Henderson et al., 1987; Ioannides and Rosenthal, 1994); nevertheless US householders regard their homes as investments, and are much more willing to sell them for a profit than a loss (Case and Shiller, 1988), and nearly half of Britons surveyed regarded owning a home a risky investment (Taylor, 2011). Unlike stocks where markets set the prices for these frequently traded fungible assets, properties are heterogenous assets, their markets are transactional and illiquid. So our analysis includes additional checks to address particular confounds which might apply to housing showing that our results are not due to market liquidity or individual leverage, which might restrict selling opportunities (Ortalo-Magné and Rady, 2004; Chan, 2001; Stein, 1995).

Our findings contribute to the growing literature studying the consequences of salience and attention for individual behaviours. More generally, our study expands a new line of research on how multiple reference points interact to determine individual choices. It also complements analysis by Genesove and Mayer (2001), who analyse 3,400 condominium sales from a listings agent in Boston during the 1990s, and find owners are less likely to sell if they are in loss. Here we confirm this disposition effect on purchase price in a dataset covering 25 million transactions from 1995 to 2019.

Chapter 3

Spending on digital advertising first overtook traditional advertising in 2020 (Barker, 2020) and continues to rise, so advertisers have an increasing requirement to measure online advertising efficacy efficiently. It is critical for them to understand the relationship between an advert's position and its effectiveness, since online sellers pay more for adverts positioned at the top of the search results on the popular e-commerce sites, such as Alibaba, Ebay and Google. However current methods have limitations (Lavrakas, 2010). Across two projects (Chapters 3 and 4) we measure the effectiveness of highly ranked property adverts using three different methods.

In Chapter 3 we estimate the effect of rank position of web page adverts on the clicks received using a novel approach to ameliorate selection issues found in related research. First, we reconstruct over 9 million search results from real-world online property listings data. Then we conduct a quasi-experiment where random variation in each target advert's ranking arises from its position moving unpredictably day-by-day as new properties list or existing properties change price or delist. We find an exponential decline in clicks as rank decreases.

However due to data limitations and our objective to avoid selection issues it is necessary to make a number of assumptions about the data and to exclude some major categories of data. This calls into question the robustness and generalizability of our findings.

Chapter 4

Our focus in Chapter 4 is investigating the limits of nudges using a randomized control trial (RCT) in the field. We use a common nudge type: making one item in a consideration set more salient that the remainder of items in the set via its position and prominence. This nudge is frequently seen on e-commerce sites where sellers pay for their adverts to be promoted above their natural rankings. In our study 'nudged' adverts on the property portal are elevated to the top-ranked position.

We demonstrate that the nudge has a strong positive strong short-term effect on a property's page views increasing them by over 30%. However it delivers no positive economically important medium-term, downstream outcomes related to the property's purchase. Our estimates for the nudge's effect on a consumer emailing the agent, making an accepted offer or paying the sales price are small and defined around zero. Its effects on the probability of sale and time to sale are negative – the nudge is creating economically inefficient congestion. Our results suggests that the nudge influences the consumer's attention but this does not correlate with persuading the consumer to take action and buy the property.

Extending our analysis, descriptive results suggest that the nudge affects the page views of properties in thicker markets (lower-price, higher-volume) more strongly than those in thinner markets (higher-price, lower-volume properties). However we see estate agents tend to apply the nudge to properties in thinner markets despite the properties already being more distinctive and desirable.

Finally using historic data from the portal, we attempt to reproduce the results from our field study using synthetic control methods. Taking our RCT as the 'gold standard' we show that the exact marching SCM (Stuart, 2010) did not consistently measure the nudge's effectiveness accurately. These results support the findings of Gordon et al. (2019).

Chapter 5

Lastly, in Chapter 5 we seek to replicate the results from a study by Thomas et al. (2010) which found that precise property list prices led to a higher sales prices. We use the authors' data and our larger novel dataset.

Our results are broadly consistent to those of Thomas et al. (2010), where a property's list price is greater than its sales price. However when list price is less than sales price we found the reverse effect, more precise list prices are associated with lower sales prices. This effect was up to 45 times larger than the original.

Janiszewski and Uy (2008)'s theory that the the precision of an anchor (here the list price) influences the amount of adjustment (to the sales price) partially explains the original price precision effect and the reverse effect.

2 At the Top of the Mind

Peak Prices and the Disposition Effect

2.1 Introduction

In part because absolute judgments are difficult, people often evaluate outcomes relative to salient reference points. When we refer to a “small” elephant and a “large” mouse, for example, few people would mistake the ordering of their weights, because we automatically ’norm’ each descriptor to animals of the same species. Applied to decision making, we don’t evaluate outcomes in terms of the final levels of wealth they confer (as the expected utility model assumes), but evaluate outcomes as gains or losses depending on whether they exceed or fall short of salient points of comparison.

The most widely documented case of reference dependence in economics and finance is the *disposition effect*, which refers to the reluctance of purchasers of an asset to sell it at a loss (Shefrin and Statman, 1985). The disposition effect is a feature of individual financial behaviour observed in the sale of both stocks (e.g., Barber and Odean, 2000; Shapira and Venezia, 2001; Feng and Seasholes, 2005; Chang et al., 2016) and housing (Genesove and Mayer, 2001; Andersen et al., 2021; Bracke and Tenreyro, 2021).[1] and Most prior research on the disposition effect has assumed that the relevant reference point for selling decisions is the purchase price of the owned asset.[2] Both field research focusing on housing sales, and field and experimental research involving financial assets, have supported the disposition effect prediction that people will be reluctant to sell assets at prices below the nominal price at which they were purchased.[3]

[1] Using data from a sample of 5,800 condominium property listings from downtown Boston in the 1990s, Genesove and Mayer (2001) show that owners who experience nominal losses on the original purchase price set higher asking prices to compensate (and attain higher selling prices). Recent studies also show that prevailing market conditions at the time of house purchase influence future selling prices, on which see Andersen et al. (2021) and Bracke and Tenreyro (2021).

[2] With the exception of our own series of contributions on this topic, starting with (Quispe-Torreblanca et al., 2021). In that paper, we examined the impact of the price on an asset *on the last occasion on which the investor logged in* on selling behavior. Also, a few studies have examined the impact of reference points not related to the price history. Hartzmark (2015) finds that investors selling decisions are influenced by their stocks’ rank of returns within their portfolio. Frydman et al. (2018) find that there is a disposition effect when using both a stock’s purchase price and the purchase price of a recently sold stock. An et al. (2019) show that the portfolio gain/loss moderates the disposition effect.

[3] Genesove and Mayer (2001), for example, show that real prices – nominal prices adjusted for inflation – do not predict reluctance to sell.

However, perhaps as a result of some kind of motivated bias (Bénabou and Tirole, 2016), or simply the salience of extreme values (Gagne and Dayan, 2021), asset owners often have a distorted view of true value. Casual observation of both home-owners and stock-holders suggests that owners often seem to believe that the 'true' value of their asset is the highest price it achieved while they have held it, the *peak price*. This might lead to a reluctance to sell at what is perceived to be an unfair price (Isoni, 2011; Weaver and Frederick, 2012), and can also contribute to the belief that the asset's value is likely to regress toward the higher price corresponding to its perceived true value.

In this paper we examine whether a consideration of the peak historic price an asset achieved while the owner has held it does, indeed, help to predict selling behavior. To do so we estimate the disposition effect on returns since purchase and returns since peak price in both housing and stock data.[4] The housing and stock markets represent the two largest, and distinct, asset markets in which individuals participate. Our empirical analysis confirms the importance of peak prices in individual trading decisions for both housing and stocks despite their differences—very liquid stocks purchased for investment and very illiquid housing purchased both as an investment and for consumption.

Our estimates reveal that the selling probability for stocks and homes more than doubles when returns since a past peak price turn positive. Whereas a stock (home) in gain since purchase but in loss since a past peak price is approximately 50% (40%) more likely to be sold, a stock (home) in gain since purchase and in gain since a past peak price is 130% (96%) more likely to be sold. These discontinuities in the sale probability are found regardless of the magnitudes of gains or losses—with respect to either the peak price or the purchase price. Importantly, we show that these reference point effects only occur if the peak price events took place during the time when the individual owns the asset. Peak price events that happened before the investor bought the assets do not affect current sale decisions. This result helps us evaluate classical explanations of the disposition effect for which peaks price events occurring prior to the holding period should matter (e.g., beliefs on peak reversion versus regret avoidance). The existence of the peak price reference point is important because it serves as evidence that individuals update their reference points over time, and shows that the evolution of housing prices and stock prices lead to changing evaluations of gains and losses.

Our main estimates are robust to a variety of econometric specifications and a wide range of controls, including the duration of holding, which has been shown to be important for understanding the disposition effect for stocks (Ben-David and

[4] Our housing data provides population-level coverage of house price sales, providing a substantially larger data set in which to examine the housing disposition effect compared with previous studies. Our large sample of stock data, in addition to records of trades, provides records of login events allowing us to restrict our sample to days on which investors pay attention to their trading accounts.

Hirshleifer, 2012). In a series of tests, we condition the model to include individual fixed effects; flexible controls for returns since purchase and returns since the peak price event; a range of controls for housing and stock characteristics, plus other controls for portfolio and investor characteristics. We also conduct additional checks to address particular confounds which might apply to the analysis of housing and stocks.[5]

We interpret our results in light of a recent framework of the disposition effect in which asset prices experienced by the individual during their period of ownership can generate reference points for future decisions (Quispe-Torreblanca et al., 2021). The key assumption of the framework, motivated by diverse research in psychology, is that asset owners tend to focus on *the highest* of the different reference points they could pay attention to. Adopting an asset's highest price as one's reference point is self-serving in the sense that it assumes that the 'true' value of an asset one purchased is the highest price that asset achieved, thus giving oneself maximum credit for making a smart purchase decision. Yet, at the level of utility maximization it may not be self-serving since it means that current market valuations will generally fall below one's reference value. The same pattern is evident in many other domains in life in which people tend to make upward social comparisons (Festinger, 1954), which could be motivating, but entails hedonic costs (Salovey and Rodin, 1984). This tendency is probably largely responsible for the hedonic treadmill phenomenon (Lykken and Tellegen, 1996), whereby people continually struggle to achieve aspirations but then raise them further the moment they are attained. In general, people do not seem to adopt the reference points that would make them hedonically best off (Thaler, 1985).

The framework generates two main predictions, both of which are confirmed in our empirical analysis.[6] First, the framework implies the existence of a disposition effect defined over returns since purchase, but also a disposition effect defined over returns since the peak price. Second, the framework implies that an individual may not sell even when the asset is in the domain of gains since purchase if the peak price is higher than the purchase price. When this is the case, then the reference point is at the peak price. In such cases, the positive effect on utility of the return since purchase is nullified by the loss since the peak. Hence, the individual chooses not to sell.[7]

Our study builds upon a large experimental literature and smaller field literature on the role of peak prices in individual decisions. An experimental literature beginning

[5] In the analysis of housing sales, we show that our results are not due to market liquidity or individual leverage, which might restrict selling opportunities (Ortalo-Magné and Rady, 2004; Chan, 2001; Stein, 1995). In the analysis of stocks, further tests confirm that our results are not due to portfolio rebalancing; our results also remain consistent after taking into account overall market movements since the purchase of the stock and since the past peak price; our results are very similar when using hazard models; and our results are consistent across samples of sell-days and login-days.

[6] An illustration of the framework in a basic four-period setting is presented in the Online Appendix.

[7] Further details of the framework and a simulation exercise using sensible parameters in a prospect theory value function are provided in the Online Appendix, see Table A.1.

with Kahneman et al. (1993) presents evidence in support of a "peak-end rule," which states that agents judge an experience or event by how they felt at its peak and end rather than by the sum total, or the average, of every moment of the event (for a review of this literature, see Fredrickson, 2000). Peaks play an important role in the subjective evaluation of magnitudes, including economic magnitudes. For example, in range-frequency theory (Parducci, 1965, 1995) the range of values encountered – and top of the range is the peak – provide the context against which magnitudes are evaluated, such that a higher peak makes any given magnitude seem smaller.[8]

Perhaps most relevant to the current research, previous studies in finance show how 52-week high act as reference points in momentum trading strategies (George and Hwang, 2004) and merger and acquisition decisions (Baker et al., 2012). Furthermore, in the popular news media, house price indices are among the most commonly reported economic data, and turning points in indices are newsworthy events. Price anchors (such as the 52-week high) are also commonly published alongside current prices. Hence, home owners and investors in stocks are exposed to information on peak prices on a regular basis.

Unlike typical price anchors, we study here the effect of the highest price experienced by asset holders during the period of asset ownership. Importantly, we show that the peak price acts as a reference point only when it is a price experienced by the individual during the holding period. Consistent with other research showing that events that happen to oneself are much more salient to people than those that happen to others, or at other times (Kaustia and Knüpfer, 2008; Simonsohn et al., 2008; Herz and Taubinsky, 2018; Hartzmark et al., 2020; Malmendier et al., 2020), we show that peak prices that pre-date the individual's purchase of the stock have no bearing on future trading decisions. Hence, it is the individual's experience of the peak price that forms the enduring reference point.

Our results also contribute to the literature around the disposition effect outside of stock market trading. The disposition effect against purchase price is well documented for stocks, but for housing the effect is not well established. We complement the dataset from Genesove and Mayer, 2001, who analyse 3,400 condominium sales from a listings agent in Boston during the 1990s, and find owners are less likely to sell if they are in loss. Here we confirm this disposition effect on purchase price in a dataset covering 25 million transactions from 1995 to 2019.

The disposition effect is a widely documented bias. It has been observed in

[8] In laboratory choice experiments, increases in the peak gain on offer across an experiment cause large reductions in the propensity to accept the chance to play gambles offering a mixture of gains and losses, as if a higher peak gain makes other gains seem smaller (Walasek and Stewart, 2015; Walasek et al., 2020). In personnel economics, Heath et al. (1999) show that employees are more likely to exercise stock options when the stock price exceeds the maximum price attained during the previous year. In strategic games, Anderson and Green (2018) show that chess players stop playing once they pass their previous maximum rating.

brokerage data across multiple countries and time periods (Grinblatt and Keloharju, 2001; Brown et al., 2006; Barber et al., 2007; Calvet et al., 2009), as well as in experimental settings, such as in Weber and Camerer (1998). Models of the disposition effect have centred on the importance of realization utility and loss aversion (Barberis and Xiong, 2009; Frydman et al., 2014).[9] Odean (1998) shows that the disposition effect cannot be explained by portfolio rebalancing, transaction costs, a preference for realizing gains more frequently than losses, or due to different beliefs about expected future returns. In a laboratory experiment, Frydman and Rangel (2014) study the role of the salience of prices in the disposition effect, demonstrating that the disposition effect diminishes with reduced salience.[10]

Our findings also contribute to the growing literature studying the consequences of salience and attention for individual behaviours. Arkes et al. (2007) study the shift in each subject's reference point following prior gains or losses. Their experimental evidence suggest that reference point adaptation is asymmetric: adaptation following a gain is greater than following a loss. Earlier work suggests that the first and last prices act as reference points. Baucells et al. (2011) explore the determinants of investors' reference points by exposing participants to hypothetical sequences of stock prices in a laboratory experiment. They find that a stock's starting and ending prices are the two most important inputs into an investor's reference point. More generally, research in the psychology literature documents that participants exposed to a series of stimuli tend to remember better the first and the most recent values (Ebbinghaus, 1913; Murdock, 1962; Ward, 2002). We extend this line of work by empirically testing the effects of another salient reference point, the peak price.

More generally, our study expands a new line of research on how multiple reference points interact to determine individual choices. Very few empirical papers have examined the consequences for decision making in situations in which specific outcomes are evaluated against multiple reference points despite evidence of reference-dependent behavior in a variety of settings.[11] Studies documenting the important of different reference points, have tended to examine each in isolation.[12] Prior research

[9] However, mixed evidence on whether these aspects of Prospect Theory preferences determine the disposition effect have been observed in other studies (Kaustia, 2010; Hens and Vlcek, 2011; Henderson, 2012).

[10] There is also evidence that the disposition effect tends to be stronger in retail investors, as compared with institutional investors (Shapira and Venezia, 2001), less-experienced investors (Feng and Seasholes, 2005), as well as with investors who are lower in wealth (Dhar and Zhu, 2006). In more recent research, the disposition effect has, however, been shown to not arise for mutual funds (Chang et al., 2016). The focus of our analysis of stock market investors is, however, on sales of individual stocks rather than index funds or mutual funds.

[11] Studies of reference-dependent behaviour in the context of multiple reference points include consumer products marketing (Hardie et al., 1993), tax compliance (Yaniv, 1999), food choices (Van Herpen et al., 2014), and effort in sports (Allen et al., 2016)

[12] For example, people evaluate the salary they receive from work relative to what they received in the past (Bewley, 2009; DellaVigna et al., 2017), but also relative to what they expected to receive (Kőszegi

has, moreover, typically involved hypothetical choices (see. e.g., Sullivan and Kida, 1995; Ordóñez et al., 2000) or stylized laboratory experiments (Koop and Johnson, 2012), although one recent empirical study found that purchase price and neighborhood price influenced the length of ownership in Singapore's private housing market (Huang et al., 2021). A limited number of studies explore how multiple reference points affect choices on separate dimensions, such as income and work hours (Crawford and Meng, 2011) or goals and experience (Markle et al., 2018). Another strand of the literature seeks to understand how reference points relate to Regret Theory in dynamic decisions (Brettschneider et al., 2020; Strack and Viefers, 2021).

The remainder of the paper proceeds as follows. Section 2.2 describes the home sales data and stockbroking and measurement of returns since purchase and returns since peak. Section 2.3 presents the econometric specification used in the analysis and describes the sample selection restrictions. Section 2.4 and Section 2.5 present the main results and additional robustness and sensitivity tests for housing and stocks, respectively. Section 2.6 describes potential mechanisms and Section 2.7 concludes.

2.2 Data

2.2.1 Home Sales Data

We use data on home sales provided by the UK's principal registry of home sale data[13], Her Majesty's Land Registry (HMLR) Price Paid Dataset (PPD).[14] The price paid data contain entries for the universe of residential property sales in the UK beginning on 1 January 1995. We draw upon records up to and including 31 December 2019. In total, the dataset covers 25.1 million transactions relating to 14.6 million properties. The data record the date and price of each property purchase, the property's address and property characteristics, including whether the property is a new-build home or a resale and the type of dwelling.[15] With these data, we can identify property resales,

and Rabin, 2006; Mas, 2006; Crawford and Meng, 2011),what others receive (Brown et al., 2008; Card et al., 2012; Bracha et al., 2015), and what they would like to receive (aspirations) (March and Shapira, 1992; Heath et al., 1999). Neale and Bazerman, in a book describing research on negotiation (cited in Kahneman, 1992) identify five possible reference points that might influence worker responses to a wage offer from management: last year's wage; management's initial offer; the union's estimate of management's reservation point; the union's reservation point; and the union's publicly announced bargaining position.

[13] The UK consists of the four nations of England, Northern Ireland, Scotland and Wales. Northern Ireland and Scotland, which comprise 7.5% of the UK population, have separate land registries.

[14] HM Land Registry data © Crown copyright and database right 2018. This data is licensed under the Open Government Licence v3.0. HMLR is a UK government agency with legal responsibility for registering the ownership of land and property in England and Wales. The dataset provides no details about property owners.

[15] Dwelling types are flat, or apartment; detached house, one with no walls shared with neighbouring property; semi-detached house, shares a common wall on one side; and terraced house, shares common walls on both sides

where an individual purchases a property and then sells at a later date.

We combine the price paid data with a local-level house price index to create property $\times$ quarter observations of property values, which is the unit of observation for our analysis of the disposition effect for housing. We draw upon the UK House Price Index (HPI), using records for England and Wales (EW). The index is constructed by the UK's national institute of statistics, the Office of National Statistics (ONS) using the price paid data as well as local government property tax and demographic datasets to determine property and location characteristics.[16] We also use the difference between the price paid for a property (as recorded in the price data set) and average value of properties of the same type in the locality as a measure of "quality", following Genesove and Mayer (2001). In addition, we merge in a set of local-level covariates, including the size of the housing stock, volume of property transactions, and average loan-to-value (LTV) ratio.[17]

Sample Selection

We apply a number of steps in sample selection. First, as our interest is in the selling behaviour of owner's of standard residential properties, we drop non-standard sales (as defined by HMLR). These include commercial transactions, properties purchased specifically to be rented out (where identifiable), gifts, and sales below full market value or under court or compulsory purchase order. At this step we also drop a very small number of properties sold for token values (less than £250). Second, we drop properties with incomplete address details. A property's address is used to match its purchase with any subsequent sale and to link the property with the other datasets. Properties without full addresses, or that for other reasons cannot be matched to the HPI dataset, are necessarily excluded. Third, we drop a small number of observations with apparent data errors, such as where properties are owned for less than a week before resale. Together, these restrictions result in 3.1% of properties being dropped from the data set. We then take a 15% random sample of the total data to match compute capacity. We use this baseline (or "Full") sample of approximately 3.6 million

[16] The ONS applies a hedonic regression model to the data sources to produce estimates of property price changes for each period (ONS, 2016). We use the indices for each calendar quarter between 1 January 1995 until 31 December 2019 by dwelling type and at the most granular level of geographic area provided, which are the 340 EW local government districts.

[17] The annual estimated housing stock for England and Wales districts is published by the Ministry of Housing, Communities & Local Government (for English districts) and the Welsh Government (for Welsh districts). Data are available from 31 March 2001 to 31 March 2018. Quarterly housing stock is estimated by linear interpolation of the annual figures. The number of property transactions by district and calendar quarter is derived from the PPD dataset. The PPD, HPI and housing stock datasets are publicly available, online and free. Additionally, the Financial Conduct Authority (FCA), the conduct regulator for the UK financial services sector provided to us a private dataset of quarterly LTV ratios for re-mortgagors by the 10 EW regions between the second quarter 2005 and the fourth quarter of 2019. This is an extension of a table that the FCA makes public as part of their quarterly Mortgage Lenders & Administrators Return. (The 10 EW regions are shown in Table A.2).

home purchases and 128.4 million observations for the estimates of the disposition effect based upon returns since purchase. Additionally, observations where there is no peak price are necessarily dropped for the estimates of the disposition effect based upon returns since purchase and since peak ("Peak sub-sample"), with approximately 2.4 million property purchases and 72.1 million observations retained. The effect of these restrictions on our sample is set out in Table 2.1.

Summary Statistics

Table 2.2 provides summary statistics for the baseline sample of property × quarter observations. The proportion of properties by dwelling type ranges from 16.2% for flats to 29.7% for terraced houses, with new-build properties representing 10.0% of all transactions. The median purchase price is £250,000 and the corresponding mean is £333,000 (prices are adjusted to December 2019 prices). The median time to resale (i.e., length of ownership) is 8.8 years and the corresponding mean is 9.6 years.[18]

[18] North East England has the lowest proportion of property transactions at (4.5% of all transactions) whereas London and South East England have the highest proportions (13.4% and 17.0% of all transactions, see Table A.2).

Table 2.1: Housing Data Sample Selection

Description	Purchases	Purchases (%)	Properties	Properties (%)	Property × Quarters
Full dataset up to 31 December 2019	25,120,172	100	14,623,011	100	–
Standard price paid entry	24,353,878	96.9	14,185,549	97	–
Price at least £250	24,353,521	96.9	14,185,384	97	–
Complete address	24,326,978	96.8	14,171,258	96.9	–
Sample: 15%	3,646,940	14.5	2,125,688	14.5	–
At least 1/52 year between successive purchases	3,637,530	14.5	2,125,688	14.5	–
Quarterly valuation	3,631,906	14.5	2,125,618	14.5	128,444,588
Both peak and purchase price present	2,403,974	9.6	1,893,273	12.9	72,113,609

Note: The table details the steps in sample selection. Purchases (Column 2) are the number of transactions; Properties (Column 4) are the number of unique properties to which these transactions relate, and Property × Quarters (Column 6) are the number of quarterly valuations calculated thereon. We drop non-standard sales retaining “Standard price paid entries” (as defined by HMLR), which are sales of individual properties at full market price to private individuals and so we exclude, for example, commercial transactions and gifts. We also drop properties sold for a nominal value below £250 and those resold within a month. Properties without full addresses or otherwise cannot be matched to the HPI dataset are necessarily dropped. A 15% random sample is taken of the data and this “Full Sample” of 128.4 million observations is used in in the regression estimates of our baseline specification. Observations without a peak price are necessarily dropped for our “Peak sub-sample” used in the regression estimates of our modified baseline specification.

Table 2.2: Housing Sample Summary Statistics (Full Sample)

Panel (A): Full Sample

	Mean	SD	Percentiles				
			10th	25th	50th	75th	90th
Purchase Price (£)	332,859	387,392	108,655	162,637	249,714	391,181	603,215
Time to Sale (Years)	9.55	5.22	3.24	5.32	8.83	13.12	17.14
Property type							
Flat (%)	16.19						
Detached (%)	25.10						
Semi-Detached (%)	29.02						
Terraced (%)	29.69						
New-Build (%)	10.00						
N Property X Quarter	128,444,588						

Panel (B): Peak Sub-Sample

	Mean	SD	Percentiles				
			10th	25th	50th	75th	90th
Purchase Price (£)	338,839	419,202	106,548	161,031	250,396	396,806	615,501
Time to Sale (Years)	11.83	4.75	5.76	8.24	11.49	15.09	18.53
Property type							
Flat (%)	15.78						
Detached (%)	25.91						
Semi-Detached (%)	29.19						
Terraced (%)	29.12						
New-Build (%)	9.73						
N Property X Quarter	72,113,609						

Note: The table presents summary statistics for the housing samples. Panel (A) uses the “Full sample” used in the regression estimates of our baseline specification. Panel (B) uses the “Peak sub-sample” used in the regression estimates of our modified baseline specification. Observations are at the property × quarter level. In our regression analysis, the purchase prices used are actual prices paid, but for the purposes of this table the purchase prices are adjusted by inflation to December 2019 prices to facilitate their aggregation. Time to sale represents duration of ownership before resale. Property types are: flat (apartment); detached (house with no shared walls); semi-detached (house which shares a common wall with a neighbouring property) and terraced (house which shares common walls with neighbouring properties on both sides). New-build are newly developed properties.

2.2.2 Stockbroking Data

We use brokerage data provided by Barclays Stockbroking, an execution-only online brokerage service operating in the United Kingdom. The data cover the period April 2012 to March 2016 and include daily-level records of all trades and quarterly-level records of all positions in the portfolio. Barclays provide brokerage services for both common stocks and mutual funds, although the former are much more common in the portfolios of investors using Barclays Stockbroking. By combining the account-level data with daily stock price data, we can calculate the value of each stock in an investor's portfolio on each day of the sample period. This data construction allows us to track prices at daily frequency, which is the time unit we use in analysis. The unit of observation used in the analysis is an investor $\times$ stock $\times$ day.

We focus on new accounts that open after the beginning of April 2012, as this sample restriction allows to calculate returns since purchase on all stocks held within the account, which is required for the estimation of the disposition effect. This provides a starting sample of approximately 13,600 accounts and approximately 123,000 observations in which an investor makes a sale.

Sample Selection

We apply two stages of sample selection. At the first stage, we drop observations due to data cleaning restrictions. We drop observations for which we cannot match stocks to price data. We also drop observations for which there is an unknown purchase price because the position was transferred into the account after opening (e.g., from a different brokerage service provider). Together, these restrictions result in 1,196 accounts and 29,700 sells being dropped from the sample.

At the second stage, we apply a series of restrictions necessary for the topic of analysis. First, following Odean (1998), we keep only observations for which we observe at least two stocks in the portfolio on the day of the observation. This restriction allows us to analyse selling decisions when investors have the possibility to choose which stock they prefer to sell among the set of stocks they hold in their portfolio. We also drop accounts for which there are missing demographic data. In a third step, we excluded the days in which the stocks were purchased (days when stocks started a positive position) since we are interested in analysing trades for stocks that have been held at least one day in the portfolio—also, note that day trading is usually performed by professional investors, whose trading strategies differ from those used by retail investors. In a fourth step, we also drop accounts for which there are no sales in the sample period, which results in a drop of 1,244 accounts, but no sales. Finally, we drop observations for which the returns since purchase and/or returns since peak price (defined below) are above the 99th percentile or below the 1st percentile, to remove outliers from the data which might skew the analysis.

The resulting baseline sample retains approximately 8,800 accounts and 58,863 observations of sales. A breakdown of the steps in sample selection is shown in Table 2.3.

Table 2.3: Stockbroking Data Sample Selection

	Accounts	Login-Days	Sells
Starting Sample	13635	12420193	123119
Drop due to:			
Data cleaning			
Unmatched Prices	21	2276860	13210
Unknown Purchase Price (transfers-in)	1175	2465752	16490
Analytical restrictions			
At Least Two Stocks in Portfolio	2232	356532	13250
Missing Demographic Data	2	4	4
Starting Position Days	10	97332	7
Accounts With No Remaining Selling Days	1244	339766	0
Outliers in Returns	196	624903	21295
Baseline sample	8755	6259044	58863

Note: The table details the steps in sample selection. Logins-Days in Column 2 reflect the number of observations at the account × stock × day level for the set of days in which the investors made at least one login to their account. *Sells* in Column 4 include all the stocks' liquidations or partial sells in the data, again at the account × stock × day level. Outliers in returns since purchase and since peak price (week-peak) are excluded (observations below percentile 1 and above percentile 99 of returns). This step also exclude apparent instances in which an stock is in gain since a past peak but in loss since purchase (0.018% of login days). We define peak prices as the highest prices observed (since purchase) that remained as the highest for an interval of time (a week or a month). Because under this definition, peak prices can only be updated after that interval, we excluded the apparent instances of an stock in gain since a past peak but in loss since purchase that occurred when an investor top-up an stock at an expensive price, much higher than a pass peak, but when the peak price has not been updated yet.

Summary Statistics

Summary statistics for the baseline sample are shown in Table 2.4. Approximately 85% of the the account holders are male and the average age of an account holder is 48 years. A similar profile for account holders is observed in the Large Brokerage Dataset used by Barber and Odean (for example, see Barber and Odean, 2000).[19] The average tenure of an account is approximately two years. The average portfolio value

[19] In the LBD used in Barber and Odean (2000), 79% of account holders are male, with an average age of 50 years, see Table 1 in Barber and Odean (2000).

is approximately £43,000, with a right-skew distribution with a median portfolio value of approximately £10,000.

The large majority of holdings in the sample are common stocks. By value, investors hold only 5.6% of their position in mutual funds (a small minority of investors hold only mutual funds in their portfolio). Investors also overwhelming hold positions in a few common stocks. On average, investors hold only four stocks, with the median number of stocks held of three.[20] In the last two rows of Table 2.4, we summarize the degree of attention investors pay to their portfolio and their trading frequency. We show first the percentage of all days (including non-market days) on which investors login to their account (which we define as a "login day"). On average, investors login once every four days (median once ever six days). Second, we show the percentage of all market open days on which investors make a transaction of at least one stock in their portfolio. On average, investors make a trade (either a buy or a sell) every 20 market open days.

Table 2.4: Stockbroking Accounts Sample Summary Statistics

	Mean	Min	p25	p50	p75	Max
Account Holder Characteristics						
Female	0.146					
Age (years)	48.326	17.000	37.000	47.000	57.000	87.000
Account Tenure (years)	2.259	0.348	1.507	2.219	3.027	3.995
Account Characteristics						
Portfolio Value (£10000)	4.346	0.000	0.366	0.963	2.205	5077.266
Investment in Mutual Funds (£10000)	0.169	0.000	0.000	0.000	0.000	82.147
Investment in Mutual Funds (%)	5.644	0.000	0.000	0.000	0.000	100.000
Number of Stocks	4.376	2.000	2.167	3.125	5.091	55.444
Login days (% all days)	23.346	0.307	7.580	17.229	35.443	76.471
Transaction days (% all market open days)	5.030	0.195	1.649	3.030	5.923	73.913
N Accounts	8755					

Note: The table presents summary statistics of new accounts. Age is measured at 2017. Account tenure is measured on the final day of the data period. Portfolio value is the value of all securities within the portfolio at market prices. Portfolio value, number of stocks, and investment in mutual funds are measured as within-account averages of values at the first day of each calendar month in the data period. Login days is the percentage of days the account is open in the data period and the account holder made at least one login. Transaction days is the percentage of market open days the account is open in the data period and the account holder made at least one trade.

[20] Note that in sample selection we dropped observations in which investors held only one stock in their position. Hence, the unconditional mean of the number of stocks held is lower than four in the starting sample.

2.2.3 Calculation of Returns

In this section, we describe how we calculate returns for housing and stocks. Our main analysis uses two calculations of returns: return since purchase and return since "peak" price. For housing, quarterly returns are calculated via application of the house price index at the locality. Returns since purchase are the difference between valuation and purchase price. For stocks, returns are calculated using daily prices. Returns since purchase represent gross returns on the asset prices over the holding period and in cases of multiple purchases of a stock, returns are calculated as a weighted average.[21] For housing and stocks, in addition to returns, we also define a dummy variable by whether the value of the asset is in gain or loss since purchase (i.e., whether the return since purchase is positive or negative).

An innovation of our study is to introduce the concept of return since peak. Return since peak price measures the return since the asset's most recent peak price. "Peak price" requires definition. We define the peak price as the highest price achieved by an asset in the individual's period of ownership, that remains the highest price for a persistent period of time. Central to this definition is the idea that a peak price is a price that has persisted as the highest price for some period. For example, an asset that monotonically increases in value each period could not be said to have formed a "peak" at any period; but an asset that hits a high price from which it then falls for a number of periods could be said to have achieved a peak.[22] Analogous to a series of peaks in a mountain range, peaks are salient price-points in the history of the individual's ownership of the asset.[23] This definition captures the idea that a "peak" occurs when an asset reaches a high value that stands out in its recent history. In our main analysis, we define a peak house price as the highest price achieved by the house in the home owners holding period that remains the highest price for at least three calendar quarters. We define a peak stock price as the highest price achieved by the stock in the investor's holding period that remains the highest price for at least one week.[24]

[21] For housing and stocks, gross return is calculated before fees (such as real estate fees, or brokerage fees). Gross returns for housing are calculated as levels rather than as percentages to be consistent with the way homeowners commonly describe the change in value of their properties, unlike stocks where investors' personal brokerage accounts display the percentage changes.

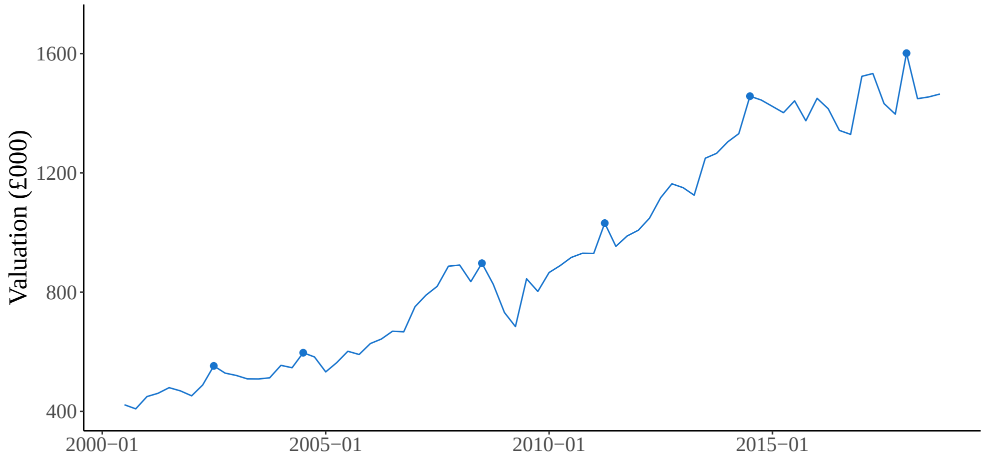
[22] An extreme case would result if *every* new high was considered a peak. Under that implementation, in a monotonically increasing price series, every day would see a new peak price.

[23] By constraining the search for peaks to the individual's ownership period, we avoid extreme cases which would result if only the all-time high was considered to be a peak, such as a stock that has a historical peak many years earlier, but has recent price spikes, which would not constitute peaks using the all-time-high definition.

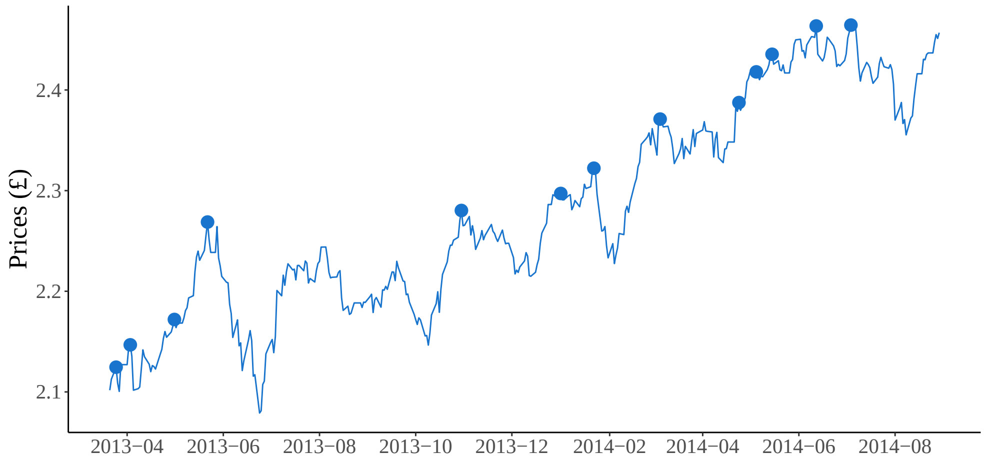
[24] This definition of peak price is different from the concept of an all-time high or low price. All-time high or low prices are defined by the day's price breaching the maximum or minimum of the complete to-date series. A peak price, by contrast, is a salient peak within the individual's own personal experience of the asset price. The salience of the peak may arise due to its persistence, whereby the peak remains the highest point in the price series for some period of time.

Figure 2.1: Examples of Peak Prices

(A) Example of Housing Price Peaks



(B) Example of Stock Price Peaks



Note: The figure illustrates the sequence of peak prices for a single example home (Panel A) and a single example stock (Panel B). For the case of houses in Panel A, a peak price is defined as the highest price (since purchase) that lasts for at least three quarters; while for the case of stocks in Panel B, the peak price must last for at a week. In both cases, a new price can only be a peak if it is higher than the purchase price and all previous peaks.

We illustrate the concept of a peak price using examples for housing and stocks. Panel A of Figure 2.1 illustrates a quarterly price series for a property in the data. The price series shown, for the full data period since the property was first purchased, shows the house price drifting upwards (with a notable drop in 2009 during the Great Recession). A peak price is defined using the three-quarter time horizon. Each blue dot therefore shows a price which is the highest price since the start of the period and which remains the highest price for at least three quarters. In sensitivity analysis, we widen this to two-quarter and four-quarter time horizons. Each blue dot in Figure A.6 shows a price which is the highest price since the start of the period and which remains the highest price for at least two quarters (Panel A) or four quarters (Panel B). Panel B of Figure 2.1 illustrates a daily price series of a commonly purchased stock in the data. The price series shown runs from April 2013 to September 2014, and shows the stock price drifting upwards. A peak price is defined using the 1-week time horizon. Each blue dot shows a price which is the highest price since the start of the period and which remains the highest price for at least one week. In sensitivity analysis, we widen this to a 1-month time horizon Figure A.10.

Summary Statistics for Returns Since Purchase and Returns Since Peak

The distribution of returns since purchase and returns since peak for observations of housing and stocks and are shown in the Online Appendix. For housing, the distributions of returns since purchase and since past peak price are both positively skewed. This is a natural result of common long-term positive trends in house prices, that induce long periods of positive returns. The distribution is shown in Figure A.2 - Figure A.5 and Table A.3 - Table A.5.[25] Additional summary data for the two-quarter time horizon definition of a peak are shown in Figure A.7 and Table A.7 and for the four-quarter definition in Figure A.8 and Table A.8. For stocks, the distribution of returns since purchase has a strong central tendency, whereas the distribution of returns since peak is negatively skewed (the latter arises because stocks' prices are more volatile, as observed in Panel B of Figure 2.1, which lead to a higher frequency of peak prices, in comparison to houses, and therefore to much shorter periods in gain since a past peak price).[26] These are shown in Figure A.9 and Table A.6, with additional summary data for the 1-month time horizon definition of a peak shown in Figure A.10 and Figure A.11 and Table A.9.

[25] As illustrated in Figure A.4, peak house price events are clustered in time at the start of the Great Recession and, to a lesser extent, at the date of the UK Brexit vote. In subsequent analyses, we include quarter fixed effects to capture the effects of these macroeconomic events in the time series.

[26] Because peak prices are updated to higher values over time, note that the distribution since past peak for both datasets, stocks and houses, is expected to be asymmetric. That is a consequence of higher returns since an earlier peak being lower returns since the subsequent peak.

2.3 Econometric Model

Linear Probability Models

We estimate the disposition effect using the standard model in the empirical literature based upon Chang et al. (2016), which we apply to housing and stocks. The standard model takes the form:

$$Sale_{ijt} = b_0 + b_1 GainSincePurchase_{ijt} + \epsilon_{ijt}, \tag{2.1}$$

in which the unit of observation for housing is at the property *(j)* and date *(t)* level, with quarterly measures of returns since purchase. *Sale* is a dummy equal to 1 if the home owner sells their property *(j)* on date *(t)*. *GainSincePurchase* is a dummy variable indicating whether, the property *(j)* had made a gain on date *(t)* compared to the purchase price. For stocks the unit of observation is at the account *(i)*, stock *(j)* and date *(t)* level. Note that, given the detailed stock data, we can construct daily measures of returns since purchase. *Sale* is a dummy equal to 1 if the investor holding account *(i)* sells the stock *(j)* on date *(t)*. *GainSincePurchase* is a dummy variable indicating whether, for the investor holding account *(i)*, stock *(j)* had made a gain on date *(t)* compared to the purchase price.

We modify the baseline specification in Equation 2.1 by adding a dummy variable indicating whether the asset is in gain compared to the peak price. We call this dummy variable *GainSincePeak*. The modified econometric specification is now:

$$Sale_{ijt} = b_0 + b_1 GainSincePurchase_{ijt} + b_2 GainSincePeak_{ijt} + \epsilon_{ijt} \tag{2.2}$$

in which *GainSincePeak* is a dummy indicating whether property *(j)* was in gain at date *(t)* compared to the peak price (housing), or for the investor *(i)*, stock *(j)* was in gain at date *(t)* compared to the peak price (stocks).

The variable *GainSincePeak* therefore adds a new element to the estimation of the disposition effect. Note that in the modified econometric specification in Equation 2.2 the dummy variable indicating whether a property × date (housing) or an account × stock × date (stocks) is in gain since peak constitutes an interaction with gain since purchase since a gain since the past peak must also be a gain since purchase.

We estimate both Equation 2.1 and Equation 2.2, allowing us first to replicate the standard estimation of the disposition effect from Equation 2.1 before introducing results from the revised specification in Equation 2.2. In subsequent robustness analyses in Section 2.4.2 and Section 2.5.2, we also estimate models that add i) individual fixed effects to control for individual-specific time invariant heterogeneity in selling behavior, ii) continuous measures of returns since purchase above and below the zero threshold, iii) a more extensive battery of control variables, including controls for property characteristics and several investment portfolio and account characteristics, and iv) we show that our results remain consistent when using different time horizons in the definition of a peak price. In the stock holding analysis, we also perform a placebo test using peak prices that occurred within the past year, instead of peak prices strictly taking place during the holding period.[27] We show that the disposition effect around peak prices is only found when the investor has experienced the peak price (i.e., when the peak occurred during the holding period).

Hazard Models

In addition to linear probability models, we show that our results remain consistent when estimating a stratified Cox proportional hazard model with time-varying covariates, following the approach suggested by Seru et al. (2010).[28] The Cox model exploits the duration dimension of the data, which is relevant for asset-selling decisions (Ben-David and Hirshleifer, 2012). The hazard model allows us to estimate the time-varying probability of a sell event without imposing any structure on the baseline hazard (i.e., without specifying the exact form of the distribution of the sell event times).

Specifically, for housing we estimate the owner selling their property j at time t (conditional on not selling the property until time t, h_{jt}). In our specification, we count every purchase of a property as the beginning of a new period of ownership which ends on the date of sale. For stocks we estimate the investor i's probability of selling position j at time t (conditional on not selling the position until time t, h_{ijt}). As in Seru et al. (2010), in our specification, we count every purchase of an stock as the beginning of a new position, and we assume that the position ends on the date the investor first sells part or all of his holdings. Our estimates are stratified by property (housing) or account (stocks), in a similar fashion to the fixed effect analysis described in the paper. That is, although coefficients are equal across accounts, baseline hazard functions are unique to each account, ϕ_i.

$$h_{ijt} = \phi_i \exp\{b_1 GainSincePurchase_{ijt} + b_2 GainSincePeak_{ijt}\} \tag{A.1}$$

We incorporate time-varying covariates into the Cox regression model, like the gain since purchase and gain since peak price variables, by dividing the follow-up time of each property (housing) or each account (stocks) into shorter time intervals. Specifically, we split the data at each calendar quarter (housing) or the observed selling days (stocks).

[27] This exercise is possible because stock prices show a higher frequency of peak price dates than peak house prices, which cluster around relatively few dates (Figure A.4).

[28] For the stockholding analysis, results also remain consistent when restricting the analysis to liquidations of entire positions from the portfolio (i.e., excluding partial sells that could occur as a result of portfolio rebalancing strategies).

2.4 Results I: Peak Price Effects in Home Sales

2.4.1 Returns Since Purchase and the Disposition Effect

We first present estimates of the disposition effect based upon returns since purchase in the home sales data using the baseline sample of observations of property × quarters.[29] The unconditional relationship between return since purchase and probability of sale is illustrated in Panel A of Figure 2.2. The figure is a binscatter plot, with each point representing a 0.5% bin of observations. Panel A illustrates the housing disposition effect result for returns since purchase. The probability of sale increases sharply when returns since purchase turn positive, in the unconditional plot the increase in the probability of sale is approximately 0.5%, against a baseline probability of approximately 0.7% (an increase in the probability of sale of over 70%).

This result is confirmed by the OLS regression estimate for Equation 2.1 shown in Table 2.5. Column 1 shows the unconditional relationship between the sale of the property and the dummy for gain since purchase. The coefficient on Gain Since Purchase of 0.0053 is positive and implies that a house which is in gain since purchase is 0.53 percentage points more likely to sell than a house in loss. Against the base probability of selling a house from the constant in the regression of 0.7% (0.0070), this represents an increase of 75%.

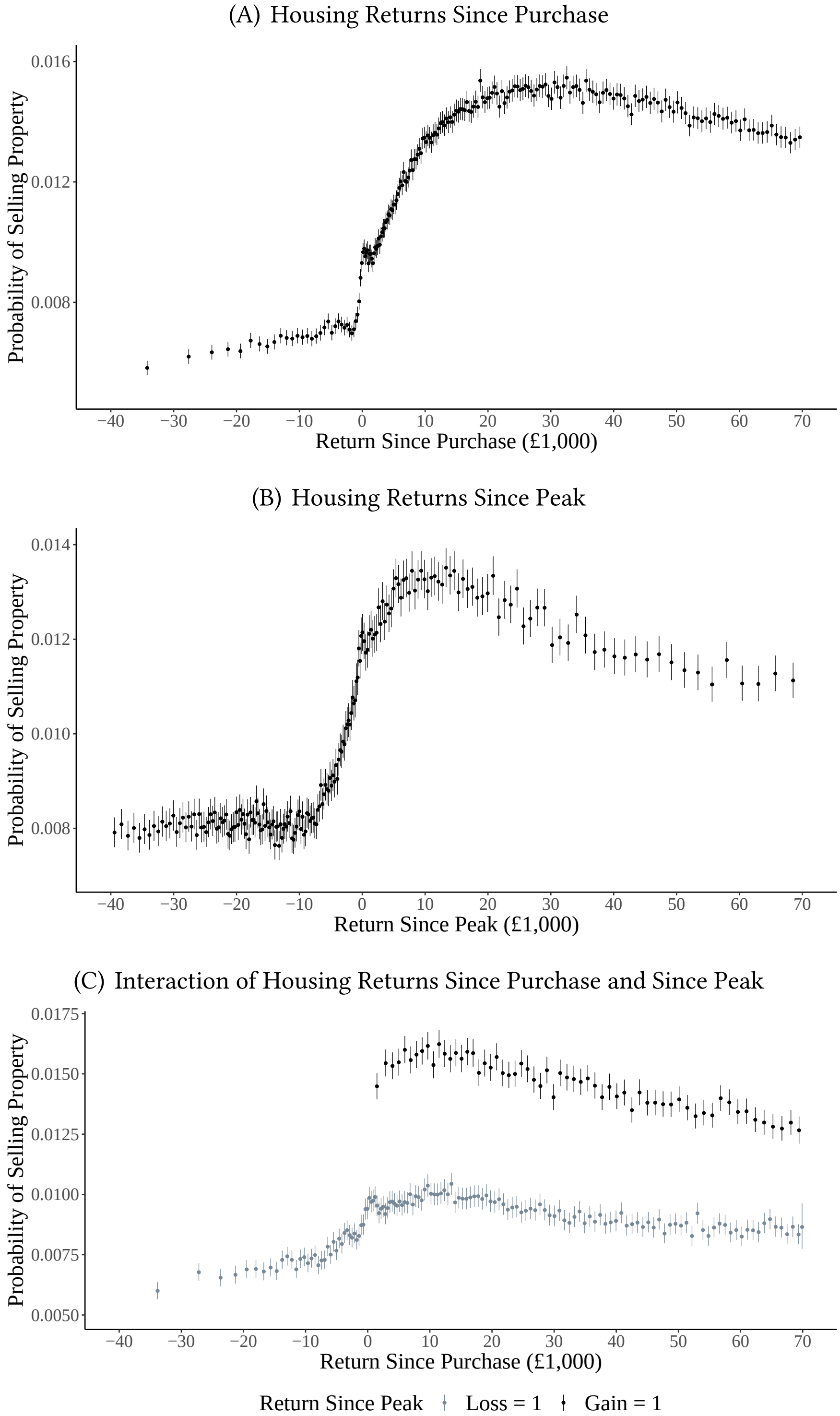
Column 2 adds a set of non-linear controls for the number of years between purchase and valuation, (included as quintics) and controls for property characteristics. Property characteristics are the property's type; whether it is new-build; its regional location and its quality.[30] The coefficient of 0.74% (0.0074) in Column 2 implies that a property in gain since purchase is twice as likely to be sold than a property in loss (the latter value defined by the unconditional probability of selling a house at a loss, given by the constant in Column 1, of 0.7%).

We examine the robustness of our results with a series of additional tests. Previous studies have shown that particular covariates may be important for explaining the disposition effect, including the length of ownership and the magnitude of returns. We therefore include these controls in additional robustness tests. We also estimate fixed effects regressions. Finally we test that the gain since purchase effect is not accounted for by market liquidity, market size or leverage.

[29] The baseline sample which provides over 128 million property × quarter observations pools together properties and calendar quarters, hence we cluster standard errors at the property and date level.

[30] Quality is proxied by the difference between the purchase price and the price at purchase predicted by the house price index, a measure of the quality suggested by Genesove and Mayer, 2001

Figure 2.2: Probability of Housing Sale, Returns Since Purchase and Returns Since Peak



Note: The figure shows the probability of house sales against return since purchase and return since peak price. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -£40,000 and + £70,000, otherwise observations include all property × quarters in the baseline sample. Vertical lines represent 95% confidence intervals.

Table 2.5: Purchase Price Disposition Effect for Housing: OLS Estimates

	$Sale_{it}$	
	(1)	(2)
Gain Since Purchase = 1	0.0053***	0.0074***
	(0.0005)	(0.0006)
Years From Purchase		0.0074***
		(0.0005)
Detached = 1		-0.0055***
		(0.0004)
Semi-detached = 1		-0.0040***
		(0.0003)
Terraced = 1		-0.0017***
		(0.0002)
New-build = 1		0.0016***
		(0.0002)
Quality (£100,000)		-0.0005***
		(0.0000)
Constant	0.0070***	0.0030***
	(0.0003)	(0.0004)
Years From Purchase Quintics	NO	YES
Region	NO	YES
Observations	128,444,588	128,444,588
R^2	0.0002	0.0017

Note: The table presents ordinary least squares regression estimates. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Column 1 displays the baseline specification. Column 2 adds controls for year since purchase and property characteristics. Years from Purchase constitute the length of property's ownership at valuation and are evaluated as quintics. Property characteristics include: detached, semi-detached, and terraced, which take the value of 1 if the property is of that type, otherwise zero (for the case of apartments). New-build takes a value of 1 if the property was acquired as a new development, otherwise zero if acquired as a resale. Quality is a proxy measure of the calibre of the property (computed following Genesove and Mayer, 2001). Region is the official EW region where the property is located. Observations includes all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

2.4.2 Robustness Tests

Controls for Continuous Returns and Property Fixed Effects

Table 2.6: Purchase Price Disposition Effect for Housing: Property Fixed Effects Estimates

	$Sale_{it}$	
	(1)	(2)
Gain Since Purchase = 1	0.0054***	0.0034***
	(0.0005)	(0.0011)
Return Since Purchase > 0 (£100,000)	0.0004**	0.0004
	(0.0002)	(0.0006)
Return Since Purchase < 0 (£100,000)	0.0194***	0.0277*
	(0.0022)	(0.0164)
Years From Purchase	0.0076***	0.0111***
	(0.0005)	(0.0007)
Constant	0.0049***	
	(0.0003)	
Years From Purchase Quintics	YES	YES
Property Characteristics	YES	NO
Property FE	NO	YES
Quarter FE	NO	YES
Observations	128,444,588	128,444,588
R^2	0.0018	0.0263

Note: The table presents ordinary least squares (Column 1) and fixed effect (Column 2) regression estimates for our baseline specification with the addition of continuous control variables for the return since purchase when the return since purchase is negative and, in a separate variable, when the return since purchase is positive. Both regression estimates include the controls used in Table 2.5, Column 2. Property characteristics are time invariant and therefore dropped in Column 2. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Fixed effects are at the property and year-quarter level. Observations include all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

As first robustness tests, we add continuous measures of returns and property and year-quarter fixed effects. Results are shown in Table 2.6. The model specification in Column 1 includes the controls from Column 2 of Table 2.5 plus continuous measures of returns since purchase. Separate linear controls are added for returns on either side of zero, for returns since purchase price. The model specification in Column 2 further adds property fixed effects and also year-quarter fixed effects (with time-invariant controls for property characteristics dropped). These fixed effects account for unobserved differences in the probability of sale across properties, or over different time periods in the data. With the inclusion of these controls, the coefficient remains positive and precisely defined in both specifications, and slightly smaller once property and year-quarter fixed effects are included in the model.

Liquidity, Market Size and Leverage

We also present a series of robustness tests to account for market liquidity, market size and leverage. Housing markets are less liquid than stock markets. Also, unlike most stock purchases, the majority of housing purchases are mortgaged. Across a series of tests, we rule out the possibility that the gain since purchase effect observed might arise due to liquidity, market size or leverage. We implement these tests as follows. We first distinguish housing market liquidity using the number of housing transactions per quarter; housing market size, using housing stock volumes per quarter; and home owner's leverage, using quarterly LTV percentages. Then, for each case, we split the dataset at the median value of the variable of interest (housing transactions, housing stock, or LTV percentage) and estimate the OLS regression models specified by Equation 2.1 for the "high" (above-median) and "low" (below-median) subsets.

Table 2.7: Purchase Price Disposition Effect for Housing: Liquidity, Market Size and Leverage Tests

	Gain Since Purchase		Constant	
Liquidity (Sales)				
Above Median	0.0051***	(0.0005)	0.0063***	(0.0004)
Below Median	0.0043***	(0.0004)	0.0047***	(0.0003)
Size (Stock)				
Above Median	0.0064***	(0.0007)	0.0043***	(0.0004)
Below Median	0.0059***	(0.0006)	0.0039***	(0.0004)
Leverage (LTV%)				
Above Median	0.0024***	(0.0005)	0.0043***	(0.0004)
Below Median	0.0052***	(0.0008)	0.0024***	(0.0007)

Note: The table summarises ordinary least squares regression estimates for our baseline specification for separate samples divided by liquidity, market size and leverage. The full estimates are presented in Table A.10, Table A.11 and Table A.12 respectively. Each row reports coefficients and standard errors from a single regression in which the dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. There are covariates for continuous returns since purchase; years from purchase and property characteristics equivalent to Table 2.6, Column 1. Column 1 displays the main coefficient of interest for the gain since purchase dummy. Column 2 shows the intercept to enable a better interpretation of the effect size of the gain since purchase dummy. Observations include all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Results are summarised in Table 2.7. The table displays the coefficients on the gain since purchase dummy for regression models estimated on the sub-samples, split at the median. The regression models include covariates for continuous returns since purchase; years from purchase and property characteristics described earlier. The pattern of positive coefficients demonstrates that the disposition effect for gain since purchase exists in above- and below- median samples in each case.

Full model results for housing market liquidity are displayed in Table A.10. Panel A shows the regression estimates for quarters where there is high market liquidity (above-median sales transactions) and Panel B shows estimates for quarters where there is low liquidity (below-median sales transactions). We observe that for the low liquidity subsample, where a property owner is less likely to be able to sell, a property in gain is approximately 0.39 percentage points more likely to be sold compared with a property in loss. Against the base probability of selling a property from the constant in the regression of of 0.67%, this represents an increase of 58% according to the first specification, without controls, and approximately 64% more likely to be sold according to the final specification, which includes the full vector of controls (relative to the unconditional probability given by the intercept in Column 1). For the high liquidity subsample, a property in gain since purchase is approximately 69% more likely to be sold than one in loss, according to the final specification and relative to the unconditional probability given by the intercept in Column 1.

The results for housing market size splits are displayed in Table A.11. Panel A shows the regression estimates for quarters where there is a larger market (above-median stock volumes) and Panel B shows estimates for quarters where there is a smaller market (below-median stock volumes). For both large and small market sizes, the positive disposition effect persists across each specification.

Finally, the results for homeowner's leverage are shown in Table A.12. Panel A displays the regression estimates for quarters where there is high property owner leverage (above-median LTV percentages) and Panel B shows estimates for quarters where there is low property owner leverage (below-median LTV percentages). In keeping with the previous results, for both high and low leverage samples, the positive disposition effect remains consistent, although we observe slightly larger effects in the low leverage sample.

2.4.3 Main Result: Peak Prices and the Disposition Effect

Our main result is the existence of a disposition effect arising from a past peak price. We use the baseline sample of observations of property × quarters with a peak price.[31] Figure 2.2 Panel B is a binscatterplot, illustrating the relationship between the probability of sale and returns since peak price. It reveals the existence of a housing disposition effect for returns since the peak price. Similar to Panel A, here we also observe a sharp increase in the probability of sale when returns since peak price turn positive (i.e., when the property price crosses its previous peak). By definition, a property that is in gain since peak must be in gain since purchase—as the peak price must be higher than the purchase price—and so the gain since peak should be understood as showing the effect of a gain since peak over and above a gain since purchase. Panel C shows the interaction of returns since purchase and returns since a past peak. There is a strong interaction effect: the disposition effect for returns since purchase doubles its magnitude when the house price is in gain since a past peak.

Table 2.8 shows the results from OLS regression models. Columns 1 - 3 show unconditional estimates for coefficients on the $GainSincePeak$ and $GainSincePurchase$, independently in Columns 1 and 2, and combined in Column 3. The coefficients in Column 3 are positive for both gain since purchase and gain since peak. Column 4 introduces control variables including years from purchase, property type controls, and the quality measure used previously. The model also includes non-linear controls (quintics) for the length of period since purchase, and region fixed effects. Coefficients for gain since peak and gain since purchase are positive and precise.

Unconditional estimates in Column 3 suggest that a home in gain since purchase but in loss since peak price is approximately 0.10 percentage points more likely to be sold (representing a 13% increase), whereas a home in gain since purchase and in gain since peak price is 0.41 percentage points more likely to be sold (representing a 55% increase, both effects evaluated against a baseline probability of 0.75%, given by the intercept), an effect size consistent with the patterns observed in Figure 2.2 Panel C. Conditional estimates in Column 4 suggest even larger effects with a home in gain since purchase but in loss since peak price approximately 0.36 percentage points more likely to be sold (representing a 48% increase), whereas a home in gain since purchase and in gain since peak price approximately 0.72 percentage points more likely to be sold (representing a 96% increase, both effects evaluated against the same baseline probability of 0.75%, given by the intercept of Column 3).

We examine the robustness of this main result with the series of additional tests described earlier; estimate a modified econometric specification that employs a Cox proportional hazard model and also show that our results remain consistent when

[31] This baseline sub-sample provides over 72 million property × quarter observations.

using different time horizons in the definition of a peak price.

Table 2.8: Purchase and Peak Price Disposition Effects for Housing: OLS Estimates

	$Sale_{it}$			
	(1)	(2)	(3)	(4)
Gain Since Purchase = 1	0.0024***		0.0010***	0.0036***
	(0.0004)		(0.0003)	(0.0004)
Gain Since Peak = 1		0.0033***	0.0031***	0.0036***
		(0.0005)	(0.0005)	(0.0005)
Years From Purchase				0.0053***
				(0.0007)
Detached = 1				-0.0039***
				(0.0003)
Semi-detached = 1				-0.0034***
				(0.0003)
Terraced = 1				-0.0020***
				(0.0002)
New-build = 1				0.0013***
				(0.0001)
Quality (£100,000)				-0.0002***
				(0.0000)
Constant	0.0075***	0.0084***	0.0075***	0.0021**
	(0.0003)	(0.0003)	(0.0003)	(0.0009)
Years From Purchase Quintics	NO	NO	NO	YES
Region	NO	NO	NO	YES
Observations	72,113,609	72,113,609	72,113,609	72,113,609
R^2	0.0001	0.0003	0.0003	0.0011

Note: The table presents ordinary least squares regression estimates for our modified baseline specification that incorporates gain since peak price. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Years from purchase constitute the length of property's ownership at valuation and are evaluated as quintics. Property characteristics include: detached, semi-detached, and terraced, which take the value of 1 if the property is of that type, otherwise zero (for the case of apartments). New-build takes a value of 1 if the property was acquired as a new development, otherwise zero if acquired as a resale. Quality is a proxy measure of the calibre of the property (computed following Genesove and Mayer, 2001). Region is the official EW region where the property is located. Observations includes all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

2.4.4 Robustness Tests

Controls for Continuous Returns and Individual Fixed Effects

Table 2.9: Purchase and Peak Price Disposition Effects for Housing: Including Continuous Returns Since Purchase and Since Peak

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0040***		0.0026***
	(0.0005)		(0.0004)
Return Since Purchase > 0 (£100,000)	0.0001		0.0001
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0112***		0.0068***
	(0.0015)		(0.0020)
Gain Since Peak = 1		0.0039***	0.0036***
		(0.0005)	(0.0005)
Return Since Peak > 0 (£100,000)		-0.0018***	-0.0017***
		(0.0003)	(0.0004)
Return Since Peak < 0 (£100,000)		0.0050***	0.0039**
		(0.0015)	(0.0016)
Years From Purchase	0.0058***	0.0055***	0.0053***
	(0.0008)	(0.0007)	(0.0007)
Constant	0.0025**	0.0037***	0.0032***
	(0.0010)	(0.0011)	(0.0009)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	72,113,609	72,113,609	72,113,609
R^2	0.0008	0.0010	0.0011

Note: This table presents ordinary least squares regression estimates for our modified baseline specification with the addition of continuous control variables for the return since purchase (and the return since peak) when the return since purchase (peak) is negative and, in a separate variable, when the return since purchase (peak) is positive. The regression estimates also include all controls from Table 2.8, Column 4, (covariates for years from purchase quintics and property characteristics). The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Observations include all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

In Table 2.9 and Table 2.10, we add a further series of covariates to the main specification. In both tables we show estimates for coefficients on the *GainSincePeak* and *GainSincePurchase*, independently in Columns 1 and 2, and combined in Column 3. Table 2.9 includes all the controls from Column 4 of Table 2.8 plus continuous measures of returns since purchase. Separate linear controls are added for returns on either side of zero, for both returns since purchase and returns since peak price. Estimates show positive coefficients on both gain since purchase and gain since peak.

In Table 2.10, we further add property and year-quarter fixed effects to the specification to account for unobserved (time invariant) heterogeneity across properties and over different time periods. Once again, the estimates show positive and precisely defined coefficients on gain since purchase and gain since peak.

Table 2.10: Purchase and Peak Price Disposition Effects for Housing: Individual Fixed Effects Estimates

		$Sale_{it}$	
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0032***		0.0030***
	(0.0011)		(0.0011)
Return Since Purchase > 0 (£100,000)	-0.0012***		-0.0012**
	(0.0004)		(0.0006)
Return Since Purchase < 0 (£100,000)	0.0055		0.0064
	(0.0096)		(0.0102)
Gain Since Peak = 1		0.0014***	0.0011**
		(0.0004)	(0.0004)
Return Since Peak > 0 (£100,000)		-0.0014***	-0.0003
		(0.0004)	(0.0005)
Return Since Peak < 0 (£100,000)		0.0007	-0.0003
		(0.0011)	(0.0013)
Years From Purchase	0.0107***	0.0103***	0.0104***
	(0.0019)	(0.0018)	(0.0019)
Years From Purchase Quintics	YES	YES	YES
Property FE	YES	YES	YES
Quarter FE	YES	YES	YES
Observations	72,113,609	72,113,609	72,113,609
R^2	0.0597	0.0596	0.0597

Note: The table presents fixed effect regression estimates for our modified baseline specification with the addition of time varying controls (return since purchase; return since peak and years from purchase quintics) from Table 2.9, Column 3. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Fixed effects are at the property and year-quarter level. Observations includes all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Liquidity, Market Size and Leverage

We replicate the robustness tests for market liquidity, market size and leverage described earlier. Results, which are summarised in Table 2.11, again show that the disposition effect for gain since purchase and also for gain since peak exists in above- and below-median samples in each case. We observe a slightly larger effect of a gain since peak for below-median leverage quarters. We again report the full set of model estimates in the supplementary Online Appendix, see Table A.13 – Table A.15.

Table 2.11: Purchase and Peak Price Disposition Effects for Housing: Liquidity, Market Size and Leverage Sensitivity Tests

	Gain Since Purchase		Gain Since Peak		Constant	
Liquidity (Sales)						
Above Median	0.0031***	(0.0004)	0.0036***	(0.0005)	0.0021*	(0.0012)
Below Median	0.0020***	(0.0003)	0.0031***	(0.0004)	0.0039***	(0.0008)
Size (Stock)						
Above Median	0.0027***	(0.0004)	0.0041***	(0.0006)	0.0008	(0.0010)
Below Median	0.0025***	(0.0004)	0.0036***	(0.0007)	0.0042***	(0.0012)
Leverage (LTV%)						
Above Median	0.0015***	(0.0004)	0.0020***	(0.0004)	0.0040***	(0.0008)
Below Median	0.0027***	(0.0005)	0.0036***	(0.0006)	0.0034***	(0.0010)

Note: The table summarises ordinary least squares regression estimates for our modified baseline specification for separate samples divided by liquidity, market size and leverage. The full estimates are presented in Table A.13, Table A.14 and Table A.15 respectively. Each row reports coefficients and standard errors from a single regression in which the dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. There are covariates for return since purchase; return since peak; years from purchase quintics and property characteristics equivalent to Table 2.9, Column 3. Columns 1 and 2 display the main coefficient of interest for the gain since purchase and gain since peak price dummies. Column 3 shows the intercept to enable a better interpretation of the effect size of the gain dummies. Observations include all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Additional Robustness Tests

Additional robustness and sensitivity tests are presented in this section. To better account for the effects of the holding period, we estimate a Cox proportional hazard model that exploits the duration dimension of the data, which is an important determinant of asset-selling decisions (Ben-David and Hirshleifer, 2012). The hazard model allows us to estimate the time-varying probability of a sell event without imposing any structure on the baseline hazard (i.e., without specifying the exact form of the distribution of the sell event times).

Table 2.12 shows estimates of Equation A.1 for housing sales. The coefficient of the gain since purchase dummy in Column 3 is 0.840, which indicates that when the property is in loss since the past peak price day, homeowners are $\exp(0.840) \approx 2.316$ times more likely to sell a property in gain since purchase compared to a property in loss. The coefficient of the gain since peak price dummy is 0.173, and indicates that when there is a gain since peak price, homeowners are $\exp(0.840 + 0.173) \approx 2.753$ times more likely to sell a property in gain compared to one in loss. This estimates are qualitatively similar to those obtained under the linear probability analysis described in the main body of the paper.

Table 2.12: Cox Proportional Hazard Model Estimates of the Housing Disposition Effect (Quarter-Peak)

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.9485***		0.8404***
	(0.0078)		(0.0085)
Gain Since Peak = 1		0.4171***	0.1731***
		(0.0050)	(0.0055)
Observations	72,113,609	72,113,609	72,113,609
R^2	0.0002	0.0001	0.0002

Note: The table presents Cox Proportional Hazard regression estimates of our baseline model with time varying covariates. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. Coefficients show stratified estimates by property. That is, coefficients are equal across properties but baseline hazard functions are unique to each property. In the model, we count every purchase of a property as the beginning of a new period of ownership which ends on the date of sale. Observations include all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property. *p<0.1; **p<0.05; ***p<0.01.

Finally, our main estimates use the three-quarter time horizon to define a peak price event: a peak price is the highest price achieved by a property in the homeowner's holding period that remains the highest price for at least three quarters. This time horizon was selected as a pragmatic lower bound. The quarter following a high price is required to establish that this price is indeed a peak, rather than part of a monotonically increasing series, and the homeowner needs some time to first become aware of the peak.

As a sensitivity test, we estimate using two- and four-quarter definitions of peak price and find consistent results in each of these alternative definitions.We test the sensitivity of our main estimates by replacing the three-quarter time horizon with two quarters and four quarters. Figure A.6 illustrates the implementation of the two-quarter time horizon (Panel A) and four-quarter time horizon (Panel B) using an example property. Using the two-quarter time horizon, a given property has more peak price events in its history due to the shorter period over which any high price event must remain the highest to be classed as a peak price. Conversely using the four-quarter time horizon, a given property has fewer peak price events.

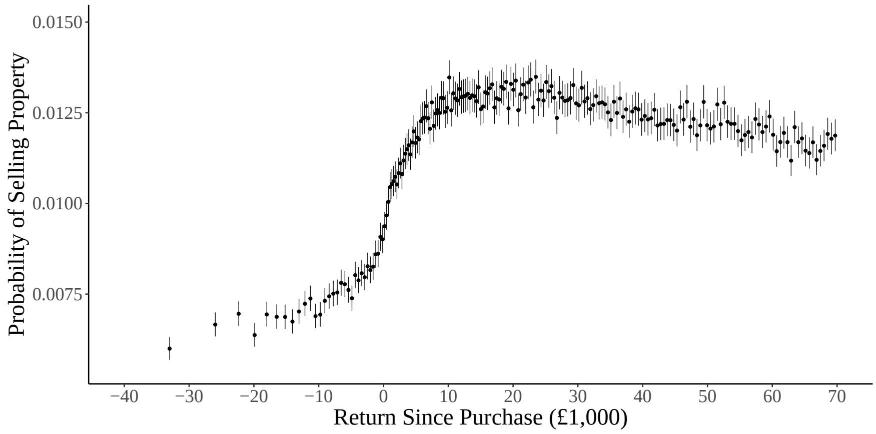
Figure A.7 and Figure A.8 show the distributions of returns since purchase and returns since peak using the two-quarter and four-quarter definitions respectively. Table A.7 and Table A.8 summarise the returns since purchase and returns since peak using the two-quarter and four-quarter definitions respectively.

Figure 2.3 illustrates the unconditional results for the two-quarter definition and Figure 2.4 for the four-quarter definition. Both mirror those shown in Figure 2.2. The plots reveal a large increase in the probability of sale when returns turn from negative to positive for both returns since purchase (Panel A) and returns since the peak price (Panel B). The plots also reveal a large interaction effect whereby the increase in probability of sale when returns turn from negative to positive is much greater when the stock is in gain since peak price compared with when the stock is in loss since peak price (Panel C).

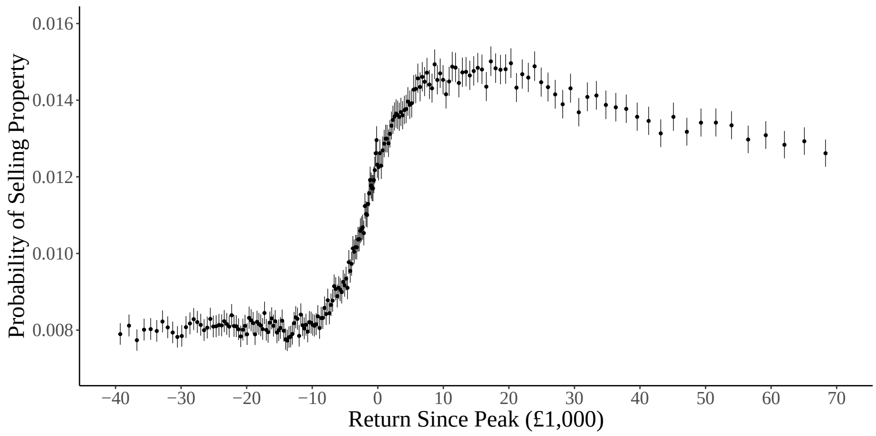
Ordinary least squares estimates of the peak and purchase disposition effects for the two-quarter and four-quarter definitions are displayed in Table A.16 and Table A.17 respectively. The regression models include the same covariates for continuous returns since purchase; years from purchase and property characteristics as the main results reported in Table 2.9. Comparing column 3, which has the fullest set of controls, in each table the coefficient values of 0.0032, 0.0026 and 0.0019 on the gain since purchase dummies for the 2-quarter, 3-quarter and 4-quarter peak definitions, respectively, and the coefficient values of 0.0040, 0.0036 and 0.0031 on the gain since peak dummies for the 2-quarter, 3-quarter and 4-quarter peak definitions, respectively, demonstrate a consistency in results despite the variation in the definition of a peak price event.

Figure 2.3: Probability of Housing Sale, Returns Since Purchase and Returns Since Peak (Two Quarters Definition)

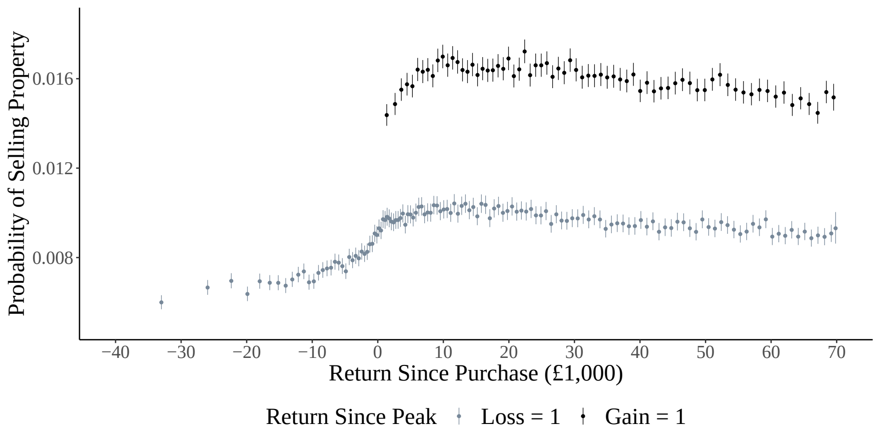
(A) Housing Returns Since Purchase



(B) Housing Returns Since Peak



(C) Interaction of Housing Returns Since Purchase and Since Peak



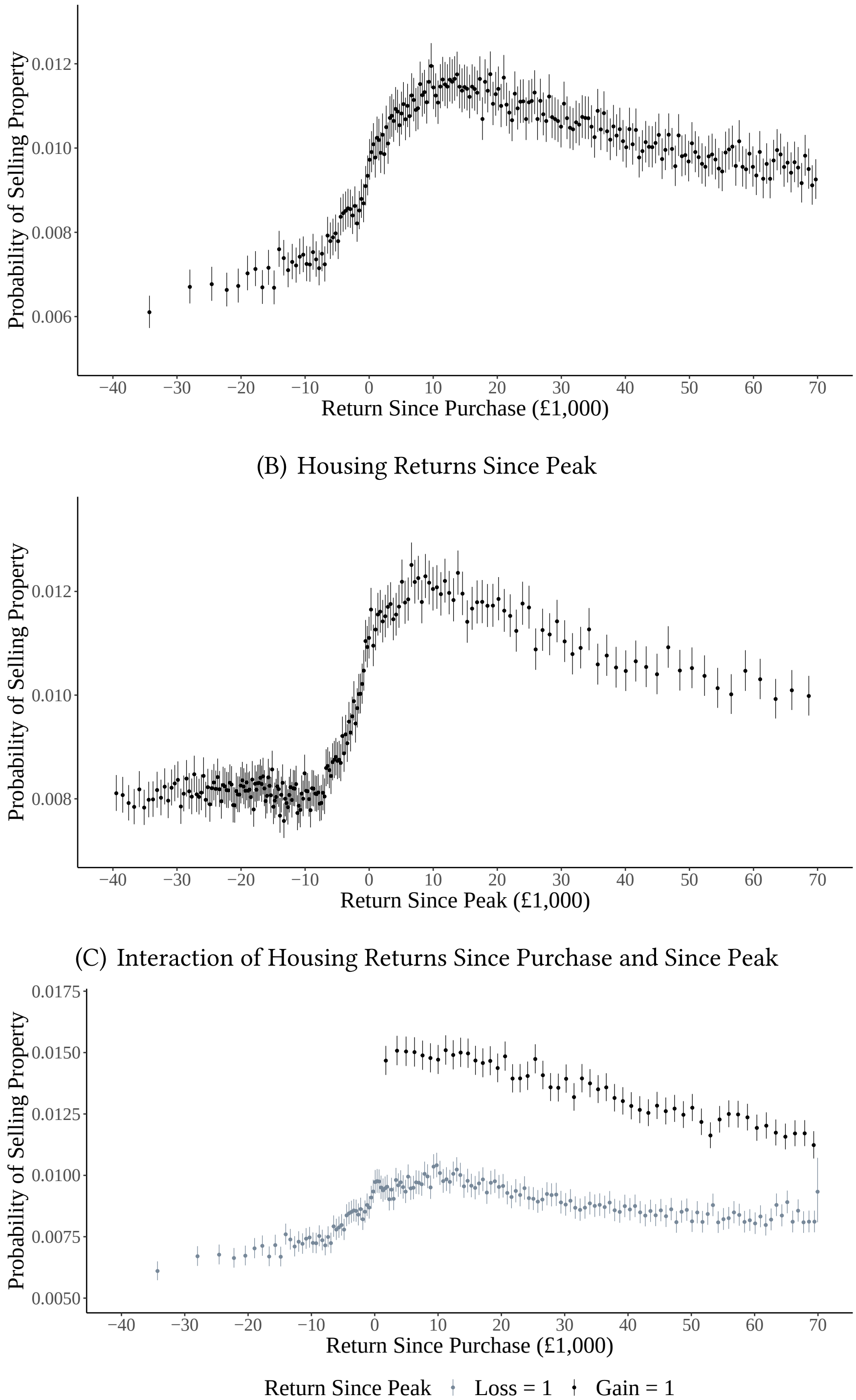
Note: The figure shows the probability of housing sales against return since purchase and return since peak price. Peak prices use an alternative definition: a peak price remains as the highest price (since purchase) for at least two quarters, instead of three quarters. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -£40,000 and + £70,000, otherwise observations include all property × quarters in the baseline sample. Vertical lines represent 95% confidence intervals.

Figure 2.4: Probability of Housing Sale, Returns Since Purchase and Returns Since Peak (Four Quarters Definition)

(A) Housing Returns Since Purchase

(B) Housing Returns Since Peak

(C) Interaction of Housing Returns Since Purchase and Since Peak



Note: The figure shows the probability of housing sales against return since purchase and return since peak price. Peak prices use an alternative definition: a peak price remains as the highest price (since purchase) for at least four quarters, instead of three quarters. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -£40,000 and + £70,000, otherwise observations include all property × quarters in the baseline sample. Vertical lines represent 95% confidence intervals.

2.5 Results II: Peak Price Effects in Stock Sales

2.5.1 Main Results

Following the previous literature on stockholding and the disposition effect, we present main estimates from models using the subsample of observations of account × stock × days on which the investor made a sale of at least one stock.[32] Our main result for the existence of a disposition effect around the peak price is illustrated in Figure 2.5. The figure is a binscatter plot, illustrating the relationship between the probability of sale and returns since purchase price (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase price and peak price (Panel C).

Figure 2.5 Panel A illustrates the standard disposition effect results for returns since purchase. The probability of sale increases sharply when returns since purchase turn positive, in the unconditional plot the increase in the probability of sale is approximately eight percentage points against a baseline probability of twelve percent (an increase in the probability of sale of two-thirds).

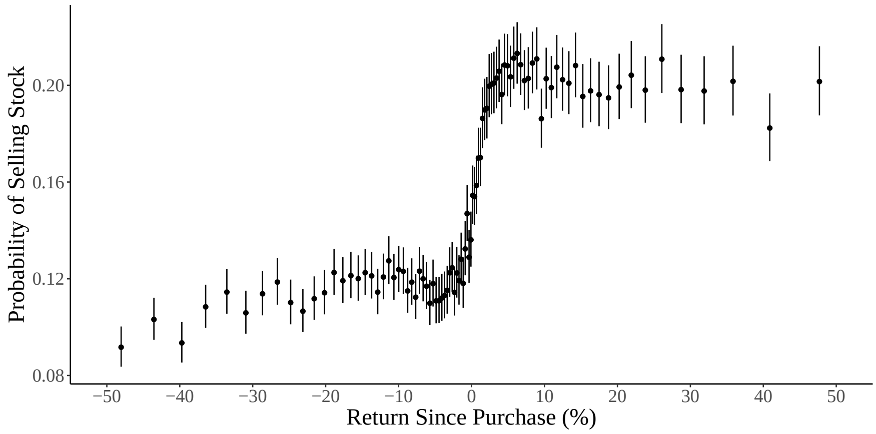
Figure 2.5 Panel B reveals the existence of a disposition effect for returns since the peak price. Again, here we observe a sharp increase in the probability of sale when returns since peak price turn positive (i.e., when the stock price crosses its previous peak). In the unconditional plot, the increase in the probability of sale is large, with the probability more than doubling when returns since peak turn positive. The plot has fewer observations to the right in consistency with the underlying distribution of returns observed in Figure A.9 and Table A.6 (where returns since peak are on average -16.2% (median -9.6%), while returns since purchase, -4.8% (median -2.2%)).[33]

[32] As discussed in (Chang et al., 2016), on days with no sales, we cannot tell whether the absence of a sale is a deliberate choice on the part of the investor, or whether it is due to inattention. Consequently, previous studies (beginning with Odean, 1998), restrict the sample to account × stock × time units on which the investor sold at least *one* stock in their portfolio. This sample restriction ensures that the investor was paying attention to the portfolio at those points in time and there was some risk that the investor would sell *any* stock. We also show results from a login-day sample: the subsample of observations of × stock × days on which the investor made a login to their account. The rationale for this sample is that on login days investors pay attention to their accounts, and hence have some non-zero likelihood of making a sale. The Sell-Day sample provides approximately 396,000 account × stock × days for by investors who sold at least one stock on the day, whereas the login sample is much larger (because login days are much more common than sale days). The Login-Day sample provides approximately 6,259,000 account × stock × days for investors who made at least one login on the day. Both data samples pool together investors and days, hence we cluster standard errors at the account and date level.

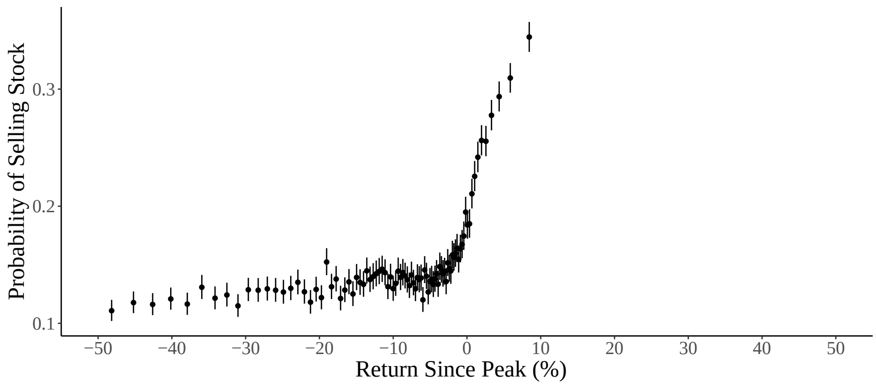
[33] Also note that because of the higher volatility in stock prices, compared to house prices, peak prices in the stock data are updated frequently, as observed in Panel A of Figure 2.1, which implies that gains since peaks last a shorter time and are therefore of small magnitude. In Figure A.12, we replicate the plots using peak prices that should be the highest prices for at least a month, instead of a week, and we observe in Panel B a larger frequency of gains since peak.

Figure 2.5: Probability of Stock Sale, Returns Since Purchase and Returns Since Peak

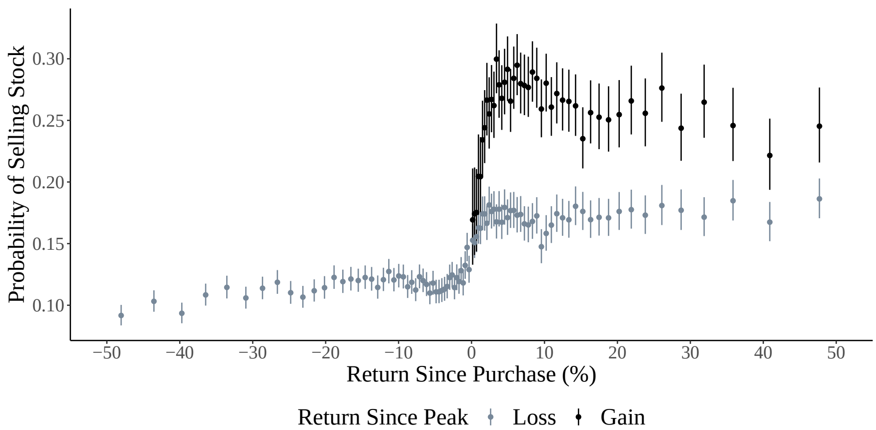
(A) Stock Returns Since Purchase



(B) Stock Returns Since Peak



(C) Interaction of Stock Returns Since Purchase and Since Peak



Note: The figure shows the probability of stocks sales against return since purchase and return since peak price. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -50% and 50%. The sample includes all investor × stock × days on which the investor sold at least one stock in his portfolio. Vertical lines represent 95% confidence intervals.

These patterns are confirmed by OLS regression estimates of Equations 1 and 2, which are shown in Table 2.13. Column 1 shows the estimate of Equation 2.1. The coefficient of the Gain Since Purchase dummy is positive and implies that a stock which is in gain since purchase is approximately 8.3 percentage points more likely to be sold compared with a stock in loss. Against the base probability of selling a stock from the constant in the regression of 11.4%, this represents an increase of 73%.

Table 2.13: Purchase and Peak Price Disposition Effect for Stocks: OLS Estimates

	$Sale_{ijt}$		
	(1)	(2)	(3)
Gain Since Purchase=1	0.0828***		0.0572***
	(0.0045)		(0.0039)
Gain Since Peak=1		0.1281***	0.0906***
		(0.0063)	(0.0050)
Constant	0.1135***	0.1332***	0.1135***
	(0.0042)	(0.0040)	(0.0042)
Observations	396,186	396,186	396,186
R^2	0.0132	0.0137	0.0188

Note: The table presents ordinary least squares regression estimates. The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. The sample includes of all investor × stock × days on which the investor sold at least one stock in the portfolio. Standard errors are clustered by account and day. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

The model in Column 2 replaces the gain since purchase dummy from Equation 2.1 with the gain since peak price. The coefficient of this dummy variable is again positive and precisely defined. The coefficient of the gain since peak price dummy in Column 2 implies that a stock which is in gain since peak price is approximately 12.8 percentage points more likely to be sold compared to a stock in loss. Against the base probability of selling a stock of 13.3%, this represents a 96% increase in the likelihood of a sale.

Figure 2.5 Panel C shows the interaction of returns since purchase and returns since peak. Similar to houses, the disposition effect since purchase is stronger when the price is above a past peak. Estimates of Equation 2.2, the corresponding regression, are shown in Column 3. Results show a positive coefficient on both the gain since purchase and the gain since peak price dummies, which are both precisely estimated. A stock in gain since purchase but in loss since peak price is 5.7 percentage points more likely to be sold. When the stock is also in gain since the peak price, this probability increases further by 9.1 percentage points. Hence, evaluated against a baseline probability of

11.4%, a stock in gain since purchase but in loss since peak price is approximately 50% more likely to be sold, whereas a stock in gain since purchase and gain since peak price is 130% more likely to be sold.

We examine the robustness of this main result with a series of additional tests. Previous studies have shown that particular covariates may be important for explaining the disposition effect, including the length of the holding period and the magnitude of returns. We therefore include these controls in additional robustness tests, together with other control variables, such as the time elapsed since a past peak, investor demographics, and several portfolio characteristics. We also estimate fixed effects regressions, and a modified econometric specification that employs a Cox proportional hazard model.

2.5.2 Robustness Tests

Individual Fixed Effects

The first robustness test adds individual fixed effects to control for individual-specific time-invariant heterogeneity in selling behavior. Results are shown in Table 2.14. The table reports results for the same four specifications as those shown in Table 2.13. With the inclusion of individual fixed effects, the coefficient values are very similar to those in Table 2.13.

Table 2.14: Purchase and Peak Price Disposition Effects for Stocks: Individual Fixed Effects Estimates

	$Sale_{ijt}$		
	(1)	(2)	(3)
Gain Since Purchase=1	0.0860***		0.0647***
	(0.0041)		(0.0033)
Gain Since Peak=1		0.1132***	0.0740***
		(0.0059)	(0.0047)
Observations	396,186	396,186	396,186
R^2	0.1466	0.1445	0.1502

Note: The table presents fixed effects regression estimates. The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. Fixed effects are at the account level. The sample includes all investor × stock × days on which the investor sold at least one stock in the portfolio. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Controls for Continuous Returns

Table 2.15: Purchase and Peak Price Disposition Effects for Stocks: Including Continuous Returns Since Purchase and Since Peak Price

	$Sale_{ijt}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Gain Since Purchase=1	0.0680***		0.0453***	0.0704***		0.0522***
	(0.0047)		(0.0039)	(0.0044)		(0.0036)
Return Since Purchase > 0 (%)	0.0002		-0.0001	0.0005***		0.0002
	(0.0002)		(0.0002)	(0.0001)		(0.0001)
Return Since Purchase < 0 (%)	0.0007***		0.0016***	0.0006***		0.0018***
	(0.0001)		(0.0002)	(0.0001)		(0.0002)
Gain Since Peak=1		0.0564***	0.0413***		0.0516***	0.0332***
		(0.0060)	(0.0055)		(0.0057)	(0.0051)
Returns Since Peak > 0 (%)		0.0177***	0.0179***		0.0157***	0.0156***
		(0.0013)	(0.0013)		(0.0012)	(0.0012)
Returns Since Peak < 0 (%)		0.0009***	-0.0008***		0.0008***	-0.0012***
		(0.0001)	(0.0002)		(0.0001)	(0.0002)
Constant	0.1264***	0.1496***	0.1207***			
	(0.0045)	(0.0041)	(0.0045)			
Account FE	NO	NO	NO	YES	YES	YES
Observations	396,186	396,186	396,186	396,186	396,186	396,186
R^2	0.0140	0.0176	0.0220	0.1473	0.1473	0.1528

Note: The table presents ordinary least squares regression estimates of our main specification with the addition of continuous control variables for the return since purchase (and return since peak price) when the return since purchase (peak) is negative and, in a separate variable, when the return since purchase (peak) is positive. The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. The sample includes all investor × stock × days on which the investor sold at least one stock in the portfolio. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

A second robustness test adds linear controls for returns to the econometric models in Equation 2.1 and Equation 2.2. Separate linear controls are added for returns on either side of zero, for both returns since purchase and returns since peak price. Results are shown in Table 2.15, which reports estimates both without individual fixed effects (shown in Columns 1-3) and with the addition of individual fixed effects (shown in Columns 4-6). The coefficient values are again very similar to those in the baseline OLS models, slightly attenuated by the linear controls for returns, which are precisely defined in the majority of models and imply that investors have a higher probability of sale when experiencing greater returns since purchase and greater returns since peak price.

Additional Controls

Table 2.16 shows estimates from models with additional controls. Previous studies suggest important control variables in econometric specifications of the disposition effect, including the stock holding period (see Ben-David and Hirshleifer, 2012) and investor experience (see Da Costa Jr et al., 2013). In a series of econometric models presented in the table, we control for the holding period (days since purchase), days since peak, portfolio and account characteristics (portfolio value, number of stocks in the portfolio, and account tenure) plus individual controls for account holder gender and account holder age. We also show specifications that include account and stock fixed effects. Coefficient estimates for gain since purchase and gain since peak are stable across a wide range of specifications incorporating these additional controls.[34]

[34] The fullest pooled model (Column 8 of Table 2.16) returns coefficient values of 0.0637 and 0.0779 on the gain since purchase and gain since peak dummies, respectively, which show little change in a model that includes account and stock fixed effects (Column 10) in which the values are 0.0724 and 0.0769. These estimates are again very similar to the OLS estimates reported in Table 2.13: the increased probability of sale for a stock in gain since purchase and gain since peak is 14.9 percentage points in Column 10 of Table 2.16, compared with 14.8 percentage points in Column 3 of Table 2.13.

Table 2.16: Purchase and Peak Price Disposition Effects for Stocks:
Including Portfolio and Demographic Controls

	$Sale_{ijt}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Gain Since Purchase=1	0.0566***	0.0590***	0.0636***	0.0652***	0.0627***	0.0634***	0.0633***	0.0637***	0.0678***	0.0724***
	(0.0038)	(0.0037)	(0.0040)	(0.0040)	(0.0038)	(0.0038)	(0.0038)	(0.0037)	(0.0036)	(0.0036)
Gain Since Peak=1	0.0903***	0.0833***	0.0824***	0.0814***	0.0773***	0.0776***	0.0777***	0.0779***	0.0746***	0.0769***
	(0.0049)	(0.0048)	(0.0048)	(0.0048)	(0.0047)	(0.0047)	(0.0047)	(0.0047)	(0.0047)	(0.0046)
Days Since Purchase (100 days)		-0.0060***	-0.0088***	-0.0079***	-0.0082***	-0.0074***	-0.0073***	-0.0072***	-0.0021**	0.0021**
		(0.0007)	(0.0011)	(0.0011)	(0.0008)	(0.0009)	(0.0009)	(0.0009)	(0.0008)	(0.0009)
Days Since Peak (100 days)			0.0049***	0.0044***	0.0058***	0.0057***	0.0057***	0.0056***	0.0039***	0.0018
			(0.0013)	(0.0013)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0011)	(0.0011)
Portfolio Value (£10000)				-0.0015***	-0.0006**	-0.0006**	-0.0006**	-0.0006***	-0.0013***	-0.0013***
				(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)
Number of Stocks (10 stocks)					-0.0368***	-0.0374***	-0.0373***	-0.0363***	-0.0109	-0.0108
					(0.0094)	(0.0096)	(0.0095)	(0.0091)	(0.0083)	(0.0085)
Account Tenure (years)						-0.0060**	-0.0059**	-0.0040		
						(0.0026)	(0.0027)	(0.0025)		
Female=1							-0.0084	-0.0021		
							(0.0056)	(0.0051)		
Age (10 years)								-0.0134***		
								(0.0016)		
Constant	0.1079***	0.1198***	0.1175***	0.1283***	0.1739***	0.1874***	0.1882***	0.2520***		
	(0.0039)	(0.0047)	(0.0046)	(0.0044)	(0.0100)	(0.0136)	(0.0133)	(0.0135)		
Account FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Stock FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	388,297	388,297	388,297	388,297	388,297	388,297	388,297	388,297	388,297	388,297
R^2	0.0192	0.0203	0.0205	0.0271	0.0479	0.0481	0.0482	0.0509	0.1302	0.1518

Note: The table presents ordinary least squares regression estimates of the baseline model with the addition of demographic controls and (daily level) portfolio controls. The sample includes all investor × stock × days on which the investor sold at least one stock. Outliers (investor × stock × days) below the 1st and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

2.5.3 Additional Robustness Tests

Additional robustness and sensitivity tests are presented in Appendix A.2. We find consistent results when using (i) a hazard model specification instead of a linear probability model (following Seru et al., 2010), (ii) excluding partial sales to rule out confounds arising from portfolio rebalancing strategies, and (iii) using month periods to define peak prices instead of week periods. We also replicate our results using (iv) the sample of login-days (which encompasses the sample of sell-days used in the main analyses, since investors have to log in to trade). These additional checks also include sensitivity analyses that include estimates of interaction effects with (v) market movements (daily market movements as well as market movements since the purchase of the stock and since past peak prices), (vi) the time elapsed since purchase and since the past peak price event, and (vii) investor and account characteristics (e.g., gender, age, account tenure, portfolio value, and portfolio size). The Online Appendix also reproduces the analysis on the triple interaction between gains since purchase, gains since the past peak price, and gains since the most recent login to the account, we document in Quispe-Torreblanca et al. (2021).[35] Losses on any of these margins reduce the probability that the investor will sell, even when other margins show gains. This further shows the importance of other reference points in creating a reluctance to realise losses.

2.5.4 Pre-Purchase Peaks and Investor Experience

As a final robustness test of the peak price disposition effect, we conduct a placebo test using peak price events that occurred *before* the investor purchased the stock. If investor trading behaviour is sensitive to the returns experienced by the investor against reference points defined by the holding period – here date of purchase and date of peak price since purchase – then price events which occur before the investor's holding period should not create reference points. A peak price event that occurs before the investor bought the stock can then be used as a placebo event: an event which occurred as a price event in the data but did not endow the investor with a reference point as the investor was not holding the stock at the time.

To implement this, for each investor × stock × day observation, we first identify the peak price for that stock within the past year (defined as the highest price achieved over the past year). We then divide the sample into observations for which the investor held the stock on the peak price day, and observations for which the investor did not hold the stock on the peak price day. Figure A.16 provides examples of scenarios in

[35] In Quispe-Torreblanca et al. (2021), we show that the price observed on the last login day constitutes another important reference point that influences investors selling decisions.

Table 2.17: Estimates of the Stocks Disposition Effect, Placebo Analysis

Panel (A): No Holding Stock

	$Sale_{ijt}$		
	(1)	(2)	(3)
Gain Since Purchase=1	0.1582***	0.1431***	0.1502***
	(0.0073)	(0.0071)	(0.0064)
Gain Since Peak=1	-0.0161	-0.0249**	-0.0015
	(0.0102)	(0.0102)	(0.0081)
Days from Purchase Day (100 days)		-0.0450***	-0.0053***
		(0.0025)	(0.0017)
Constant	0.1268***	0.1723***	
	(0.0052)	(0.0066)	
Account FE	NO	NO	YES
Stock FE	NO	NO	YES
Observations	242,536	242,536	242,536
R^2	0.0383	0.0480	0.2397

Panel (B): Holding Stock

	$Sale_{ijt}$		
	(1)	(2)	(3)
Gain Since Purchase=1	0.0361***	0.0350***	0.0322***
	(0.0043)	(0.0043)	(0.0040)
Gain Since Peak=1	0.0320***	0.0290***	0.0298***
	(0.0054)	(0.0055)	(0.0050)
Days from Purchase Day (100 days)		-0.0033***	0.0054***
		(0.0009)	(0.0007)
Constant	0.1172***	0.1289***	
	(0.0047)	(0.0062)	
Account FE	NO	NO	YES
Stock FE	NO	NO	YES
Observations	169,800	169,800	169,800
R^2	0.0045	0.0050	0.1998

Note: The table presents ordinary least squares regression estimates for the placebo test that compares the effect of peak prices that took place before the holding period of the stock with those that ocurred during the holding period. Peak prices are defined over the past year. The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. The sample includes all investor × stock × days on which the investor sold at least one stock in the portfolio. However, Panel A includes the set of days in which past peak prices occurred before the purchase of the stock; and Panel, the set of days in which peak prices occurred after the purchase of the stock. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

which investors did and did not hold a stock on the peak price day. In both scenarios, the day of observation is the 25th June 2015, a day on which the investor holds the stock shown in each panel. In Panel A, the investor purchased the stock prior to the peak day event, and in this case has experienced a gain since purchase and a gain since the peak price day. In Panel B, the investor purchased the stock after the peak price day event (which occurred approximately six months prior to the purchase day), and has also experienced a gain since purchase and a gain since peak price day. The earlier peak price therefore acts as a placebo event not experienced by the investor.

Using this approach, we find no evidence for a disposition effect around peak price events which pre-date the investor holding the stock. We estimate Equation 2.2 on both samples, with results shown in Table 2.17. Panel A reports results for the sample of observations in which the investor did not hold the stock on the peak price day. Estimates show a positive and precisely defined coefficient of the gain since purchase dummy. The coefficient of the gain since peak dummy is imprecisely defined in Column 1, and negative in Column 2 (statistically significant at the 5% level). The coefficient remains negative in Column 3 after adding account and stock fixed effects to account for heterogeneity across investors and assets. Hence, in this sample, there is no evidence for a disposition effect arising from the peak price. Panel B reports results for the sample of observations in which the investor did hold the stock on the peak price day. In this sample, estimates show positive and precisely defined coefficients for both the gain since purchase and gain since peak price dummies. The coefficient values are smaller than in the main analysis, as expected—given that a peak price in the past year is likely less salient than a peak price since purchase—but show the existence of both forms of the disposition effect as expected. Hence, this test adds evidence that peak prices affect trading behaviours through returns experienced by the investor.

2.6 Potential Mechanisms

Although not the main focus of the paper, our data do provide some evidence in support of, or against, different possible mechanisms that may be driving the peak price effects we documented here in house and stock sales. We describe them next considering that they are not necessarily mutually exclusive and different sets of evidence might provide support for more than one potential mechanism.

One possible mechanism could be a belief in 'peak-reversion'; owners of assets might infer from the fact that an asset at one point achieved a particular price that its 'true' value corresponds to that peak price, and hence expect the market price to evolve toward the peak price. Anticipation of such an increase in price would lead to a reluctance to sell the asset at its current, lower, price.[36]

Note, however, that this explanation is not supported by the placebo test presented in Table 2.17, which showed that individual selling decisions are only affected by peaks occurring during the time when an individual owns the asset. To be consistent with a peak-reversion account, one would have to assume that asset owners believe that true values correspond to peak prices *during the period when they owned the asset.*

In contrast, an analysis of ‘top-ups’ – further purchases of an owned asset – does provide some support for the idea that a belief in peak-reversion may be contributing to the observed results. If investors believe that stocks with weak performance since the past peak price event will subsequently do well, they will buy additional shares the larger the loss since the past peak event.

Figure A.17 shows binned scatterplots of the relationships between losses since purchase, losses since peak and the propensity of investors to top-up current positions with new purchases of the same stock. The proportion of observations with top-ups is shown in the y-axis, with loss since purchase (Panel A) and since peak (Panel B) shown on the x-axis. Results in Panel A show the probability of top-up reducing as losses since purchase increase. However, in Panel B we observe the opposite relationship for losses since peak price: observations with higher losses since peak price display a higher probability of top-up, consistent with the idea of belief in reversion to the past peak price. These relationships are confirmed by regression analysis in Table A.26 and Table A.27, with the former showing a positive coefficient of the loss since purchase variable, and the latter showing a negative coefficient of the loss since peak variable.[37]

A second possible mechanism involve regret-avoidance. The peak price can be a source of regret for owners who ponder selling an asset, because they would have made more money if they had sold at the peak. Asset owners might resist selling, therefore, to avoid experiencing the regret of selling ‘too late’. Such an account would be consistent with research on “inaction inertia” (Tykocinski and Pittman, 1998), which finds that “when an attractive action opportunity has been forgone, individuals tend to decline a substantially less attractive current opportunity in the same action domain, even though, in an absolute sense, it still has positive value” (page 206). Regret-aversion could help to make sense of the placebo analysis finding that only peaks occurring when the individual owned the asset matter; such peaks are the ones that would play

[36] This explanation is related to an explanation for the disposition effect proposed by Shefrin and Statman (1985) and Odean (1998) – that investors hold on to losing stocks because they expect higher future returns from losing stocks compared with winning stocks, i.e., they expect losing stocks to outperform in the future as they rebound in price.

[37] In an extension, in Table A.28, we also test whether the negative slope found in Panel B of Figure A.17 is different for the cases in which the investor has not held the stock during the past peak price, following the same spirit of our earlier placebo test, but this time analysing topping-up decisions. We, however, find that the slopes are quantitatively similar for peaks occurring before the purchase of the stocks and peaks occurring after the purchase. These findings provide additional support to the notion of beliefs in reversion to the peak.

into regret, since the individual could not have sold an asset they didn't own. The regret account could also be consistent with our analysis showing heightened top-ups of stocks that are below their peak. Top-ups, according to one possible interpretation are analogous to gamblers or investors who 'chase losses' by doubling down on gambles or investments in an attempt to recoup losses.

The final, and most mundane, possible explanation for the peak effect is simply that peaks, like purchase prices, constitute salient reference points, and that investors dislike selling below the peak, just as they dislike selling at a loss relative to the purchase price. The peak is likely to be a salient value for a number of reasons. Research has shown that people pay special attention to information when the news is good (Karlsson et al., 2009), and for asset prices, the peak price is the best news that the owner will experience. Also, exactly because they own the asset (and likely made the decision to acquire it), asset-owners might be motivated to perceive their asset as valuable. Research on motivated thinking shows that people who are motivated to reach a particular conclusion, collect, pay attention to, and analyze data in a selective, confirmatory, fashion (Gilovich, 2008; Epley and Gilovich, 2016). Note, however, that the simple reluctance-to-sell-at-a-loss account would not predict heightened top-ups for assets that are below the peak.

2.7 Conclusion

Using data on buying and selling behaviour of individual asset-owners – both housing and stocks – we show a new disposition effect for returns based upon the highest price reached by the asset during the individual's period of ownership. This disposition effect arising from the peak price experienced by the asset owner exists over and above the disposition effect arising from the purchase price. We show that this effect is robust to a range of econometric tests, and also sensitivity analyses. For the stock data, whose prices are more volatile and therefore contain a larger frequency of peak prices, we were also able to perform a placebo test that exploits peak price events that occur before investors purchase stocks.

Our study contributes to the expanding literature on how multiple reference points affect individual decisions. Research has documented the operation, and consequences for economic behavior, of diverse reference points – for example, past wages (Bewley, 2009; DellaVigna et al., 2017), other people's wages (Brown et al., 2008; Card et al., 2012; Bracha et al., 2015), and what people expect to receive (Kőszegi and Rabin, 2006; Mas, 2006; Crawford and Meng, 2011) in settings as varied as consumer products marketing (Hardie et al., 1993), tax compliance (Yaniv, 1999), food choices (Van Herpen et al., 2014), sports (Allen et al., 2016; Pope and Schweitzer, 2011), and rental choices Bordalo et al. (2019). Very few of these papers, however, have examined the interplay between different reference points in situations in which multiple natural points of comparison are operative.

The current research also contributes to the growing literature showing that an individual's personal history can affect their economic behavior (see, for examples, Malmendier and Nagel, 2011; Malmendier et al., 2011; Malmendier and Nagel, 2016; Andersen et al., 2019), and that, more specifically, the history of an individual's ownership of an object – e.g., how they came to acquire the object – can affect valuations (Loewenstein and Issacharoff, 1994; Strahilevitz and Loewenstein, 1998). One dimension of understanding the consequences of individual experience for future behavior, the current research suggests, is to understand the reference points that experience makes salient.

3 Uptown Top Ranking

Estimating Position Effects with Online Property Listings Data

3.1 Introduction

Imagine a consumer is interested in buying a home. They enter their requirements as search terms in a property portal's search engine which returns, as the Search Engine Results Pages (SERP), a choice set of adverts for properties that meet their criteria. In Scenario A, a target advert is ranked at position 3 in the choice set whereas in Scenario B, its rank position is 15. Do their different rankings affect the probability that a consumer will click on the advert to read the full property description?

Spending on digital advertising continues to rise, first overtaking traditional advertising in 2020 (Barker, 2020) and so advertisers have an increasing requirement to measure online advertising effectiveness proficiently. Specifically, online advertisers pay for a premium ranking on websites like for example Google (e.g. Baye et al., 2016) so it is critical for them to understand the relationship between rank position and outcome. However prior attempts to measure the size of the effect of different rank positions are potentially biased with the magnitude of the estimates varying between studies. The rank position of an item in a Google SERP depends on its relevance to the search terms provided by the consumer and for a paid advert the bidding behaviour of the advertiser and its competitors at auction. This can lead to significant selection issues when measuring the effect of position so biasing a simple comparison of means. Previous studies, described later, have either ignored this endogeneity or attempted different approaches to overcome it, each with its own limitations (Agarwal et al., 2011; Ghose and Yang, 2009; Yang and Ghose, 2010; Rutz and Trusov, 2011; Narayanan and Kalyanam, 2015; De los Santos and Koulayev, 2017; Ursu, 2018).

We estimate the effect of rank position of web page adverts on the clicks they receive and find an exponential decline in clicks as rank decreases. Our study is a novel approach to ameliorate common selection issues found in related research. Using real world data provided by a world-leading property portal we reconstruct for 700,000 target adverts 9.4 million SERPs, one for each day listed. By construction all adverts in each SERP are equally relevant to the specified search terms and their rank positions are only defined by their list price. Random variation in a target advert's ranking in its SERP choice set occurs because its position moves unpredictably day-by-day as new properties join the choice set or as other properties in the set change price and/ or leave. The property seller cannot change the position of their target advert once listed.

To our knowledge this is the first study to look at the economically important domain of property purchases. Additionally our novel approach of estimating the effect of rank position by reconstructing SERPs without the common selection issues and utilising the exogenous variation in our target adverts' ranking adds to the literatures on the influence of rank as a context effect in decision making and specifically the influence of rank on internet advertising outcomes. However due to data limitations and our objective to avoid selection issues it is necessary to make a number of assumptions about the data and to exclude some major categories of data. This calls into question the robustness and generalizability of the results.

The SERP from a search engine like Google typically consists of a small number of sponsored (paid) links at the top of the web page followed by a much larger number of organic (unpaid) links. The rank order of the sponsored links (i.e., adverts) depends on their relevance to the search terms provided by the by the user and on the advertisers' bids. Advertisers bid for specific positions through a keyword auction where they pay for their advert to be associated with specific search terms. Higher positions are more expensive than lower positions. The order of the organic links only depends on their relevance, however retailers may also employ Search Engine Optimisation techniques to increase the relevance of their links so that they achieve higher rankings (Baye et al., 2016).

In this study our outcome measure is Click Throughs where a potential buyer clicks through a property advert following its hyperlink to access the underlying property description. Click Throughs are a key metric used by search engines such as Google; they use current Click Throughs to charge advertisers (Hollis, 2005; Feng et al., 2007) and prior Click Throughs as a factor to determine the rank position of items in their SERP by assessing relevance to user specified search terms (Baye et al., 2016).[1] Click Throughs are also used by online advertisers as a proxy for the effectiveness of their online campaigns (e.g. Drèze and Hussherr, 2003; Hollis, 2005; Varian, 2007).

SERPs are an instance of where different available alternatives are presented together. In such circumstances, individuals tend to make joint evaluations and make a selection in the context of its alternatives (Hsee, 1996; Hsee et al., 1999). Studies show that the ranked position of alternatives on a particular dimension is such a contextual influence. According to Range Frequency Theory (Parducci, 1965; **?**) the attractiveness

[1] Here the property portal does not use Click Throughs to either charge clients or to determine the ranking of the advert.

of an in a list of item will depend on a compromise between its range position of the item (based on a dimension such as price) and its relative ranked position. Similarly decision by sampling theory (Stewart et al., 2006) holds that an item's subjective value is assumed to depend on rank. This rank effect phenomenon was originally detected in psychophysics when individuals compared physical qualities such as weight or volume but is also seen in the economic and social world with e.g. people making judgements about their wage or life satisfaction in comparison to others in their cohort or deciding which assets to trade in the context of their stock portfolios (Quiggin, 1993; Diecidue and Wakker, 2001; Brown et al., 2008; Boyce et al., 2010; Hartzmark, 2015).

Work based on both traditional media and webpages suggest multiple mechanisms acting individually or in tandem could explain the influence of ranking on consumers' clicking behaviour. Generally consumers incur a time and cognitive effort cost in scrolling down the screen looking for alternatives and each time they click through and evaluate an alternative so there is an incentive to choose a satisfactory earlier option (Card et al., 1983; Krosnick, 1991; Lohse, 1993; Olson and Olson, 1995).

Different mechanisms can explain this behaviour and may be operate together. They include, first signalling (Nelson, 1974; Kihlstrom and Riordan, 1984; Baye et al., 2016). In signalling consumers perceive the advertising spend on a product as a signal of its quality and knowing that it costs more to advertise in a higher ranked position infer those top ranked products are of higher quality.

Second relevance (Varian, 2007), consumers learn by experience that search engine results decline in relevance down the page and so infer a top ranked result is more relevant to their requirements.

Third sequential search (Weitzman, 1979), consumers start at the highest position but as they move down the list their attention decays and they stop when they find an item that satisfices their needs (Hoque and Lohse, 1999).

Fourth salience (Tversky and Kahneman, 1992), the extreme ranks are the most salient and attention grabbing. Eye tracking experiments with the Google website show consumers pay the most attention to 3 adverts at the top of the main body of results and the top right advert (Hotchkiss et al., 2005).

Finally mere exposure (Zajonc, 1968), where consumers prefer a product just because it is more familiar to them (Winkielman and Cacioppo, 2001; Fang et al., 2007). The effect is present at very short time scales and can bias preference e.g. people shown a display of two alternating faces are more likely to prefer the face exposed longer even if the difference is only hundreds of milliseconds (Shimojo et al., 2003; Bird et al., 2012). The common explanation for mere exposure effect is that when all items are favourable the longer people pay attention to a specific item the more positive evidence is accumulated increasing the probability it is chosen (Shimojo et al., 2003; Simion and Shimojo, 2006; Armel et al., 2008; Kim et al., 2012; Krajbich et al., 2010;

Mitsuda and Glaholt, 2014; Noguchi and Stewart, 2014; Mullett and Stewart, 2016).

Estimating rank position effects in a SERP without bias requires the resolution of significant endogeneity issues. The order of the links depends on their relevance to the search terms provided by the consumer. Since the SERP results are specifically targeted at a consumer the causal effect of position on viewing an advert may be overstated. It is also difficult to generate random positions for the target advert by experiment as attempted by Agarwal et al. (2011) because position generally depends on bids by both the target advertiser and its competitors e.g. a competitor may bid for higher positions on the days it is running sales promotions so a decline in sales for the target advertiser on those days may relate to the sales promotion rather than its lower position.

Other approaches used by previous studies to control for the selection issues include estimating position and Click Throughs with different parametric models either simultaneous equations (Ghose and Yang, 2009; Yang and Ghose, 2010) or control function methods (De los Santos and Koulayev, 2017). However the estimates depend on the particular model specification which may be biased unpredictably because position depends on a complex interaction of factors including the target advertiser's and its competitors' strategic bidding at auction which is difficult to capture parametrically. Rutz and Trusov (2011) attempted to instrument the position but this still required the adoption of aggressive assumptions including that selection effects do not vary with position. Narayanan and Kalyanam (2015) adopted a regression discontinuity approach, however this required significant volumes of particular data which constrained what could be estimated and also limited the results' generalizability. Finally Ursu (2018) used evidence from two different approaches, first a field experiment where online travel agency consumers were randomised into either seeing links ranked by relevance or ranked randomly. However data limitations meant the causal effect size could not be quantified so second she created a sequential search model to do this.

Despite the underlying issues there is some consistency between previous studies' conclusions on at least the trend found as rank increases e.g. per Feng et al. (2007) an exponential decline in clicks with the top few positions receiving the most; per Agarwal et al. (2011) a monotonic decline in clicks and per Narayanan and Kalyanam (2015) higher positions achieving more clicks but with the effects strongly localised so that only certain positions show significant effects. However the magnitudes of the estimates vary e.g., in two recent studies both using online travel agent data the position effects estimated by De los Santos and Koulayev (2017) ranged from \$7.76 to \$35.15, whereas Ursu (2018) estimated the average range effect at \$1.92.

We ask does considering a property advert's rank position in a property portal's search results help to predict consumer click through behaviour. We determine rankings by reconstructing SERPs for 9.4 million target adverts assembled to overcome selection issues. The data set comprises real world data from adverts for residential properties located in a major European capital city listed for sale on a property portal's website. The data is updated daily over a nine-year period.

Our empirical analysis shows an exponential decline where lower ranked adverts on the portal's web page are associated with lower clicks to access the property description. This supports prior studies which investigated the question using different methods. We condition our model to include a range of controls reflecting property and choice set characteristics. Our results are robust to the inclusion of advert, month and year fixed effects. However our simulation required excluding certain common types of advert, including those that generate SERPs with fewer than 8 members. It also required making some simplifying assumptions, including modelling only one type of search query. Our exclusions and assumptions will limit the generalizability and robustness of our findings.

The remainder of the chapter proceeds as follows: Section 3.2 dscribes the experimental detail; Section 3.3 presents the econometric specification used in the analysis: Section 3.4 presents the main results and Section 3.5 concludes.

3.2 Experiment and Data

Does a property advert's rank position in the search results from a property portal affect the probability that a consumer will click it to read the underlying full description? To investigate this question we measure the effect of rank on Click Throughs by reconstructing 9.4 million search results using actual data spanning a 9-year period and take advantage of our targets' rank positions randomly changing day-to-day. This section describes the property portal, assumptions made, experimental details, sample selection restrictions and summary statistics.

3.2.1 Portal Details

Our data set (Full Set) is provided by one of the world's largest property portals. It comprises daily data relating to all adverts for residential properties located in a particular major European Capital, listed for sale between 1 January 2010 to 31 December 2018. There are over 2.3 million unique adverts in the Full Set each of which may be listed for days, weeks, months or even years.

Germane operational details of the portal are as follows: On behalf of sellers, estate agents market their homes for sale on the portal. Their listing is in 2 parts an advert, summarising the property details, which appears on the SERP and a full property description which consumers can read by clicking the advert. To view relevant adverts, consumers (potential buyers) create a search query in the portal's search engine by selecting the property characteristics of their desired home. These characteristics are type,[2] minimum and maximum number of bedrooms, minimum and maximum price and location. The different characteristics are selected using drop down menus so all alternatives are predefined. Prices are defined as a series of round sum *price boundaries*, the increments of which increase with magnitude e.g. from ¤50,000 to ¤300,000 the increment is ¤10,000 and from ¤300,000 to ¤500,000 the increment is ¤25,000.[3]

Consumers have the flexibility to search for very similar properties (e.g., one type, exact number of bedrooms, small locality, narrow price range) or a more varied selection (multiple types, range in number of bedrooms, large locality, wide price range). They do not need to specify all characteristics. The portal responds to each search query with a SERP showing all the adverts for properties that are currently available to buy which meet all the consumer's search query criteria. The default ranking of the adverts is high to low price but consumers can choose one of 3 other options for ranking (low to high price; newest to oldest listed and oldest to newest listed).

3.2.2 Assumptions and Experimental Design

For each advert in our Full Set we have data on the number of Click Throughs it received for each day listed as well as the characteristics of the property it is marketing. However the portal only retains the total daily Click Throughs for each advert. These aggregate the clicks from all the different search queries consumers used to arrive at the advert including the four possible ways the search results can be ranked. As discussed below we reconstruct one search query where all criteria are specified narrowly and the search results are ranked from high to low price. We assume actual total daily Click Throughs are a good proxy for the Click Throughs our reconstructed search query would have received.

We apply a set of exclusion rules, described in Section 3.2.3, to the Full Set of adverts to generate our set of target adverts which are for routine sales of ordinary residential properties by private individuals. For each target, for each day it is listed we generate a Choice Set by creating a reconstructed search result that contains the target advert and adverts for similar properties which were also listed for sale on that day. Since we wish all members of each Choice Set to be equally relevant to the hypothetical consumer our reconstructed search query includes all the property characteristics a consumer can specify, defined as narrowly as practicable so that all the Choice Set properties are very similar to one another. Specifically, a target advert's fellow Choice Set members on a given day are defined as all the other adverts for

[2] Property types are: flat (apartment); detached (house with no shared walls); semi-detached (house which shares a common wall with a neighbouring property), terraced (house which shares common walls with neighbouring properties on both sides) and unspecified house.

[3] We use the generic currency symbol '¤' to obfuscate the originating country.

properties listed for sale on the portal on that day, of the same type, with the same number of bedrooms, located within a 1 mile radius of the target, whose list price falls within the portal's narrowest price band that includes the target (so if the target's list price is ¤285,000, the price range is between ¤280,000 and ¤290,000). All adverts that meet all these criteria are extracted from the Full Set to form the target's Choice Set and so each Choice Set contains the full universe of that day's adverts that match the search query equally. They have between 2 and 250 members, but those with fewer than 8 members are excluded (see Section 3.2.3).

The members of the remaining Choice Sets are ranked, by the portal's default for sorting, from high to low list price with the mid-rank position used to break any ties in ranking.[4] The rank position of a target in its Choice Set only depends on its list price compared to the other members, with random movement in its day-to-day ranking induced by new members joining the Choice Set; existing members leaving the Choice Set or changing price within the Choice Set. Once listed a target's rank position is exogenous to any decision made by the seller until it is delisted.

The relative rank of each target is computed according to the formula:

Relative Rank = (Rank Position − 1) / (Total Number Members of the Choice Set − 1)

This gives a value of between 0 (the first advert) and 1 (the last advert). The relative rank is effectively the proportion of properties in the Choice Set with list prices less than the target's list price or, equivalently, the probability that a randomly selected list price will be less than the target's list price.

The relative rank positions are cut into 9 quantiles to facilitate the aggregation of Choice Sets with different sizes and to avoid assuming any particular functional form with the independent variable. Quantile 1 are the highest ranking adverts (which would appear at the top of the device display) and Quantile 9 are the lowest ranking adverts (which would appear at the bottom of the device display). Relative ranks are cut into an odd number of quantiles to give an obvious central location (Quantile 5). The relative rank of each target and the number of Click Throughs it received is extracted from each Choice Set to give the analysis dataset used (see Section 3.4).

3.2.3 Sample Selection

As reported above to attain our targets prior to reconstructing their search results we apply a series of exclusions to the Full Set of adverts. (These exclusions are only applied to determine the targets, the Full Set is used to reconstruct the targets' Choice Sets.) We are interested in adverts for routine sales of ordinary residential properties

[4] In real life when list prices are tied the portal randomises their order. In our regression analysis we control for the number of adverts in the Choice Set listed with same price as the target.

by private individuals between 1 January 2010 and 31 December 2018, so our target adverts exclude those for special sales, unusual properties and commercial owners. Properties must be available for purchase and not put up for auction. Properties must be categorised as flats or houses and have no more than 5 bedrooms. Houses must have at least 1 bedroom. Part-businesses, retirement properties and properties sold by developers are excluded. We exclude any advert first listed before 1 January 2010 to ensure we have its full history and also exclude any advert that does not pass the portal's data quality checks. To be able to create the Choice Sets we exclude adverts that do not specify their longitude and latitude coordinates[5] or specify an asking price. These exclusions reduce the number of unique adverts from 2,300,000 in the Full Set to 700,000 targets.

To make the results easier to interpret firstly, we exclude targets where the list price falls on a portal price boundary, such as exactly ¤300,000 or exactly ¤500,000. In such cases the target has two Choice Sets on the same day, one where it is at the top of the rankings with the highest price and one where it is at the bottom with the lowest price. Secondly, we exclude Choice Sets with fewer than 8 members otherwise there is insufficient variation to be able to split relative rank into the 9 quantiles required for our analysis approach.[6]

To address endogeneity concerns a target's rank should not be influenced by the actions of its owner after its initial listing. Accordingly we exclude any target where the owner changes its price. Also, the portal sells an "Enhancing" product which boosts an advert to first place in any SERP where it meets the search query criteria so we exclude any advert that ever uses this product.[7] However these further exclusions reduce the number of targets from 700,000 to 214,000 biasing the sample towards relatively common properties see Section 3.2.4 where a few types of property are shown to dominate our data.

3.2.4 Summary Statistics

Table 3.1 presents summary statistics for the 214,000 individual target adverts for which we create our baseline sample of 9,242,000 observations (one for each day it is listed) at the advert × day level. The majority of the properties advertised are either flats (60%) or terraced houses (27%) and have either 2 (40%) or 3 (33%) bedrooms. The

[5] Needed to estimate the distance between properties using the Haversine formula.

[6] As illustrated in Figure B.1, where there are fewer than 9 quantiles the location of each quantile on a device screen becomes inconsistent. With a Choice Set of 8 or more members Quantile 5 adverts are always in the centre and Quantile 9 adverts are always the lowest ranked whereas with a Choice Set of 4 members, for example, the variation in relative rank can only be split into 5 quantiles with Quantile 3 adverts now in the centre and Quantile 5 adverts the lowest ranked.

[7] Where another member of the Choice Set has used the Enhancing product all rankings are adjusted accordingly.

median list price is ¤385,000 and the median time an advert is listed is 29 days, both distributions are skewed right with their respective means of ¤544,000 and 44 days.

Table 3.1: Summary Statistics: Baseline Sample

	Mean	SD	Percentiles				
			10th	25th	50th	75th	90th
List Price (¤)	544058	573121	209950	275000	385000	610000	950000
Days Listed (N)	49	60	7	13	29	62	112
	%						
Property Type							
Flat	59.9						
Detached	1.4						
Semi-Detached	11.2						
Terraced	26.9						
Bungalow	0.1						
Unspecified	0.4						
Bedrooms							
0	0.4						
1	17.1						
2	40.3						
3	32.8						
4	7.7						
5	1.6						
	N						
Adverts	214233						
Advert × Day	9418685						

Note: The table presents summary statistics for the individual adverts for which we create our baseline sample at the advert × day level. Property types are explained in Footnote 2

3.3 Econometric Model

We estimate the effect of rank position on Click Throughs using the below model in which the unit of observation is at the individual property listing i and day t level:

$$LogClickThroughs_{it} = b_0 + b_1 RelativeRankQuantile_{it} + \epsilon_{it}, \tag{3.1}$$

LogClickThroughs is equal to the natural logarithm of Click Throughs received by advert *(i)* on day *(t)*. *RelativeRankQuantile* are dummy variables indicating in which of 9 quantiles the relative rank of advert*(j)* falls within its reconstructed SERP choice set on day*(t)*, where Quantile 1 is the lowest rank (adverts appear at the top of the device screen); Quantile 9 is the highest rank (adverts appear at the bottom of the device screen) and Quantile 5 is located in the centre and acts as the comparison. b_0 is a constant, b_1 is a coefficient and ϵ is the error term. The sample pools together property adverts, months and years listed and so we cluster standard errors at the property advert and date level.

We also estimate models that successively add an extensive battery of timing, choice set and property characteristic control variables. Timing controls are the number of days that the target property is listed and also when it is listed. The latter is accounted for by year and month fixed effects since the property market is seasonal over the course of the year and cyclical over a number of years (Ortalo-Magné and Rady, 2004). Choice Set controls are the natural logarithm of the number of adverts in the Choice Set and the natural logarithm of the number of adverts in the Choice Set which are listed at the same price as the target. Property characteristics are the number of bedrooms, dummies for property type and dummies for location (administrative district).

Finally in robustness analysis we estimate an advert fixed effect model to control for advert-specific time invariant heterogeneity in Click Through behaviour.

3.4 Results

In this section we provide ordinary least squares (OLS) and advert fixed effect (FE) estimates of the rank effect on the Click Throughs received by property adverts using the baseline sample. Controls are described in Section 3.3.

Table 3.2 Columns 1-4 shows the OLS regression estimates. Column 1 shows the unconditional relationship, specified by Equation 3.1, between log Click Throughs received and dummies for the advert's relative rank quantile (the comparison is Quantile 5 the most centrally positioned adverts). The coefficients for Relative Rank Q1 to Q4 are not statistically significant at the 5% level and there is no distinct pattern. However all the coefficients for Relative Rank Q6 to Q9 are statistically significant at the 5% level, they are all negative with the magnitude of the coefficient increasing from Q6 to Q9.

Column 2 adds controls for timing. The estimates for log Click Throughs at all relative rank quantiles are statistically significant at the 5% level. From Relative Rank Q1 to Q4 coefficients are positive and decline in magnitude and from Q6 to Q9, like column 1, the coefficients are negative and increase in magnitude. This qualitative pattern of positive coefficients, declining in magnitude from Relative Rank Q1 to Q4 and negative coefficients, increasing in magnitude from Q6 to Q9 repeats in column 3 which adds controls for choice set characteristics and in column 4 which adds controls for property characteristics. All relative rank quantiles coefficients are also statistically significant at the 5% level in Columns 3 and 4.

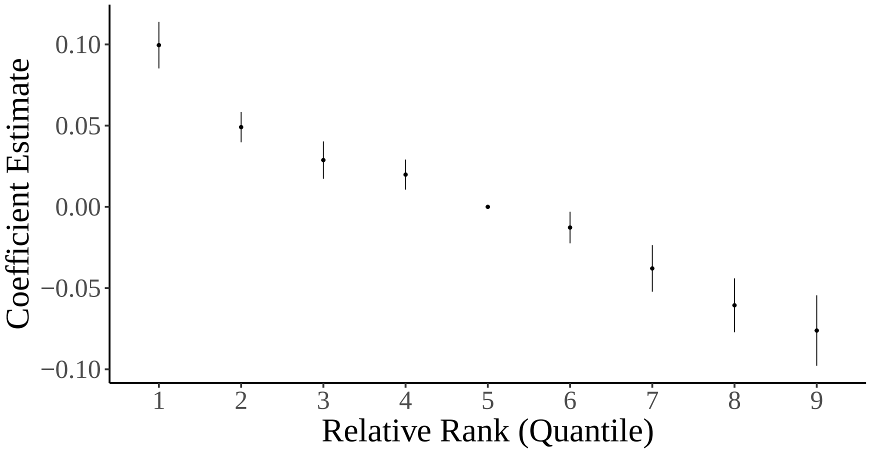
Table 3.2: Rank Effect Estimates: Ordinary Least Squares and Advert Fixed Effects

	$LogClickThroughs_i$				
	(1)	(2)	(3)	(4)	(5)
Relative Rank Q1	0.0060	0.1307***	0.0839***	0.0996***	0.0254***
	(0.0245)	(0.0168)	(0.0173)	(0.0073)	(0.0065)
Relative Rank Q2	-0.0293	0.0598***	0.0447***	0.0491***	0.0169**
	(0.0203)	(0.0112)	(0.0115)	(0.0048)	(0.0054)
Relative Rank Q3	-0.0160	0.0300***	0.0236***	0.0288***	0.0124***
	(0.0160)	(0.0069)	(0.0060)	(0.0059)	(0.0036)
Relative Rank Q4	0.0052	0.0187***	0.0175***	0.0199***	0.0054*
	(0.0071)	(0.0042)	(0.0040)	(0.0047)	(0.0025)
Relative Rank Q6	-0.0123***	-0.0148***	-0.0149***	-0.0127**	-0.0060***
	(0.0044)	(0.0043)	(0.0047)	(0.0050)	(0.0016)
Relative Rank Q7	-0.0512***	-0.0365***	-0.0400***	-0.0379***	-0.0106***
	(0.0112)	(0.0074)	(0.0066)	(0.0073)	(0.0024)
Relative Rank Q8	-0.1064***	-0.0479***	-0.0659***	-0.0606***	-0.0143***
	(0.0144)	(0.0087)	(0.0077)	(0.0085)	(0.0042)
Relative Rank Q9	-0.1814***	-0.0368**	-0.0930***	-0.0761***	-0.0162**
	(0.0368)	(0.0185)	(0.0189)	(0.0111)	(0.0063)
Log Days Listed		-0.3054***	-0.3006***	-0.2840***	-0.3608***
		(0.0181)	(0.0175)	(0.0147)	(0.0292)
Constant	2.4874***	2.3984***	3.1064***	5.0376***	
	(0.2213)	(0.0695)	(0.1444)	(0.7138)	
Advert FEs	NO	NO	NO	NO	YES
Month x Year FEs	NO	YES	YES	YES	YES
Choice Set Characteristics	NO	NO	YES	YES	YES
Property Characteristics	NO	NO	NO	YES	NO
Observations	9,418,685	9,418,685	9,418,685	9,418,685	9,418,685
R^2	0.0024	0.3800	0.3994	0.4606	0.7185

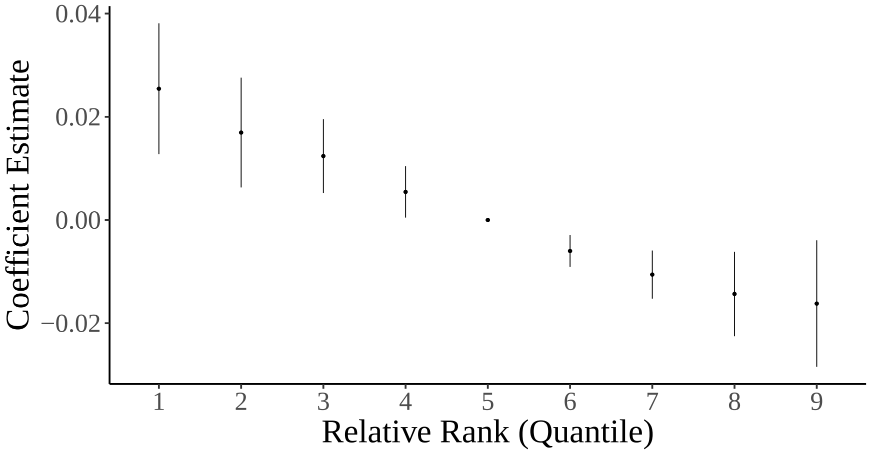
Note: The table presents ordinary least squares (columns 1-4) and advert fixed effects (column 5) regression estimates. The dependent variable takes the value of the natural logarithm of Click Throughs received by an advert and the independent variable is a dummy for the advert's relative rank quantile. Column 1 displays the baseline specification. Controls for timing (column 2), choice set characteristics (column 3) and property characteristics (column 4) are successively added. Column 5 adds advert fixed effects with the time-invariant controls for property characteristics dropped. The controls are described in Section 3.3. Observations include all advert × days in the baseline sample. Standard errors are clustered by advert, year and month. *p<0.1; **p<0.05; ***p<0.01.

Figure 3.1: Rank Effect Coefficient Estimates

(A) OLS Model



(B) Advert Fixed Effects Models



Note: The figure illustrates estimates of rank effect coefficients by quantile from Table 3.2. The Panel (A) estimates are from the OLS regression specification given in column 4. The Panel (B) estimates are from the advert fixed effects regression specification given in column 5. Vertical lines represent 95% confidence intervals. Observations include all advert × days in the sample

Figure 3.1 Panel A illustrates the OLS estimate for log Click Throughs by relative rank quantile provided by Table 3.2 column 4 where the full battery of controls are added. Considering this estimate and the figure together, we see a linear relationship between log Click Throughs and rank quantile or equivalently an exponential decline in Click Throughs as rank quantile increases i.e., people are less likely to click through a property advert to read its full description when the advert's position is lower down the web page. Adverts in the first quantile receive an estimated 47% ($e^{0.0996-^{-}0.0761}$) more Click Throughs than those in the last quantile.

In Table 3.2 column 5 we examine the robustness of our results by adding advert fixed effects. These fixed effects account for unobserved differences in Click Throughs across different adverts. This specification otherwise includes the controls of specification 3. (The controls for property characteristics added in column 4 are time-invariant controls so dropped). The qualitative pattern of positive coefficients, declining in magnitude from Relative Rank Q1 to Q4 and negative coefficients, increasing in magnitude from Q6 to Q9 which was seen in the OLS estimates of columns 2 - 4 is also seen here in column 5. All relative rank quantiles coefficients are statistically significant at the 5% level except Relative Rank Q4.

Figure 3.1 Panel B illustrates the advert FE estimate for log Click Throughs by relative rank quantile provided by Table 3.2 column 5. Again considering this estimate and the figure together, we see an exponential decline in Click Throughs as rank quantile increases. However the FE estimates are lower, with adverts in the first quantile only receiving an estimated 4% ($e^{0.0254-^{-}0.0162}$) more Click Throughs than those in the last quantile.

3.5 Conclusion

We demonstrate that a rank effect is seen with respect to clicks made on property adverts listed on a property portal. Click Throughs decline exponentially as adverts are positioned further down the web page. Our study which reconstructed over 9 million search results using real-world data is designed to reduce selection issues relating to an advert's rank position generally depending on the relevance to the consumer and the price paid by the advertiser. Our novel approach takes advantage of the random day-to-day movement in rank position of our target adverts, movement which is not caused by the actions of the target's advertiser.

Our results are consistent with other studies that used different research methods to address the endogeneity issues e.g. simultaneous equations (Ghose and Yang, 2009; Yang and Ghose, 2010), control function methods (De los Santos and Koulayev, 2017), instrument variables (Rutz and Trusov, 2011), regression discontinuity approach (Narayanan and Kalyanam, 2015) and randomised control trials (Ursu, 2018). Their limitations were discussed in Section 3.1. As well as its commercial relevance to the understanding and measurement of internet advertising effectiveness, this study more generally adds to the literature of rank effect in the social and economic world (e.g. Quiggin, 1993; Diecidue and Wakker, 2001; Brown et al., 2008; Boyce et al., 2010; Hartzmark, 2015).

However our study suffers from limitations due to the assumptions and data exclusions made. For example our dataset provides the total Click Throughs for each advert for each day it is listed. It is the aggregate of all searches. We must assume this total is a valid proxy for the results of the search query we model. We only model one type of search query and assumes the results are ranked in a specific way (i.e. high to low price) although many other options re available to the portal's users. We do not know the actual searches people made and resultant SERPs constituents and rankings because the data is not available. To avoid selection issues we exclude the most prominent list prices because they lie on two price bands and we exclude SERPs with fewer than 8 members to overcome the challenge of aggregating different sized SERPs meaningfully. These limitations effect the robustness and generalizability of our results. Many of the rectifiable limitations could be overcome with different datasets which specified a consumer's search query, the order and ranking of the results they saw and their clicking behaviour and/or by simulating a representative sample of different search queries.

The domain we investigate is property transactions which for many people are the most significant transactions they undertake. However the sales process for property is a long one, people do not just click through a link and buy. Since advert rankings change over time, we cannot associate an advert's rank on any given day with the purchase decision, so we cannot directly investigate the influence of rank on sales. Some prior studies which considered consumables bought with a series of clicks do not suffer from our limitation and were able to investigate the influence of rank on sales directly. Instead we use Click Throughs as our outcome measure. These are a key internet metric in their own right because search engines like Google and Yahoo! use them for charging advertisers and they are also assumed to equate to sales leads. However the evidence that they are a valid measure of advertising effectiveness is conflicted. For example studies by Lewis and Reiley (2014) and Manchanda et al. (2006) find an association between clicks and purchases but studies by Blake et al. (2015) and Bagwell (2007) do not.

Finally this study examines a quasi-experiment in observational data. The evidence that advertising effectiveness can be accurately measured using observational data is also conflicted. A comparison of the results from the most common observational methods to those from 15 large field studies at Facebook found that the observational methods were often inaccurate (Gordon et al., 2019). Conversely, a study investigating the influence of rank on search and purchase decisions at a leading online travel agent which separately conducted a field study and used observational data found the results from the 2 methods supported one another (Ursu, 2018). This suggests a need to better understand the circumstances where quasi-empirical approaches can be successfully employed. To this end we compare the results from a RCT carried out in the field and a quasi-experiment which applies synthetic control methods to observational data in Chapter 4.

Prior studies that investigated the effect of position on an advert's effectiveness have had limitations. This study also has its own idiosyncratic shortcomings but overall the results from our novel approach support the consensus view from the other research methods that advert clicks fall exponentially as the adverts appear further down the search results.

4 The Limits of Nudge

Evidence from the Online Property Listings Market

4.1 Introduction

In many settings, nudges are regarded as powerful tools for changing human behaviour while preserving free choice and leaving economic incentives unchanged. These innovations in 'choice architecture' have been claimed to deliver economically important outcomes with low financial cost (Thaler and Sunstein, 2009). Yet, there is growing concern that (some) nudges may only cause short-term, or proximate changes in behaviour, see for examples Laibson (2020); Chater and Loewenstein (2022). Recent research in fields including household finance, environment science and health therefore seeks to measure short-term versus long-term, proximate versus downstream effects of common nudges. These studies, described later, reveal subsequent off-setting effects that call into question the efficacy of nudges in achieving their desired outcomes (Choi et al., 2004; Argento et al., 2015; Beshears et al., 2022; Madrian and Shea, 2001; Choukhmane, 2019; Guttman-Kenney et al., 2021; Allcott and Kessler, 2019; Hagmann et al., 2019; Werfel, 2017; VanEpps et al., 2016; John et al., 2012).

In this paper, Study 1 uses a randomised control trial (RCT) in the field to test the effects of a common nudge type: making one item in a consideration set more salient that the remainder of items in the set via its position and prominence. This nudge is frequently implemented in online markets where, for a fee, an advert link is promoted above its natural ranking, usually to the top. Examples include Alibaba, Amazon and Ebay 'sponsored' product listings, booking.com 'genius' and 'preferred partner' hotel listings and Google 'Ads'. We examine this nudge in the context of property listings on an online residential real estate comparison website (property portal). As in the examples above, the 'nudged listing' is elevated to first place in the results of any search where it meets the specified search criteria.

We chose a high stakes setting in which the effects of nudging could be economically large, so if the nudge is effective it could deliver large transfers to vendors, at very low marginal cost. We focus on testing the proximate effect of this nudge on capturing the potential buyer's attention through viewing listings versus the longer-term economically important decision to purchase the property. Our measure of the proximate effect is Click Throughs (CTs) where the consumer (potential buyer) clicks on a property advert to follow its hyperlink to the property's full description. We measure downstream effects by outcomes related to the property's purchase including time to sale STC,[1] time to completed sale or final sales price achieved.

We find the nudge delivers a strong short-term, proximate outcome of a 33% increase in CTs, but it delivers no positive economically important medium-term, downstream outcomes. Our estimates for the nudge's effect on emailing the agent, making an accepted offer and the sales price paid are small and defined around zero. Indeed our evidence suggests this nudge generates economically inefficient congestion, slowing time taken to achieve the sale, as our estimates on its effects on the probability of sale and time to sale are negative. This suggests that the nudge has an influence on the consumer's attention but that does not correlate with the consumer actually taking the action of buying the property.

Our results provide proof of the need to look at the medium-term downstream outcomes of an intervention. In many contexts, policymakers may only consider immediate outcomes when the forming the evidence base for policy changes, due to limited observable outcomes and political urgency. Evidence of short-term, narrow effects may be sufficient to build political case for policy change. However medium-term, downstream outcomes may only be detectable with a significant time lag, involve joining disparate sources of data, and tracking subjects over time.

We extend the analysis in two ways. First, we show descriptive results that suggest a stronger effect on viewings in thicker markets (lower-price, higher-volume property types). Compared to nudging higher-priced property adverts, the additional improvements in viewings for nudging lower and middle-priced property adverts are 42% and 29% respectively. In these thick markets, the nudge has largest marginal effect on listing position. Second, descriptively, we see real estate agents tend to apply the nudge in thinner markets (higher-price, lower-volume property types). This suggests that, in practice, the selection into using the nudge is in opposition to evidence on where the nudge works best.

Our nudge elevates a property advert on an online real estate portal from its natural rank position to the top ranked position. Online advertisers using popular e-commerce sites, such as Google, are prepared to pay a premium to attain 'first place' (e.g. Baye et al., 2016) which according to Chitika.com (2022) receives more clicks than either positions 2 to 5 or positions 5 to 20 combined. This is because individuals expend time and cognitive effort searching and evaluating options so are incentivized

[1] A sale subject to contract (sale STC or SSTC) is an offer to buy a property that is accepted by the seller but the documentation and process needed to make it legally binding has not yet been completed.

to choose an early option that satisfices their needs (Krosnick, 1991).

The influence of message ordering on persuasion has been found in traditional and digital media (Card et al., 1983; Lohse, 1993; Olson and Olson, 1995). In a recent study by Fedyk (2018) the economic implications of position were demonstrated using a natural experiment to show company news positioned on the front page of a Bloomberg terminal caused 240% larger trading volumes and 180% bigger price changes within 10 minutes of publication than equally important articles not located on the front page.

Such primacy biases in digital advertising can be explained by multiple mechanisms including salience (Tversky and Kahneman, 1992), signalling (Nelson, 1974; Kihlstrom and Riordan, 1984; Baye et al., 2016), relevance (Varian, 2007), sequential search (Weitzman, 1979), and mere exposure (Zajonc, 1968). More than one mechanism, which are described further in Section 3.1, could be operating simultaneously. Primacy effects are seen in other contexts as diverse as answer selection in multiple choice testing (Matthews, 1927; Cronbach, 1950), food and drink choice (Coney, 1977; Dean, 1980), voting (Miller and Krosnick, 1998; Ho and Imai, 2008), selection of students for universities (Jurajda and Münich, 2010), stock trading (Jacobs and Hillert, 2016) and citation of academic papers (Feenberg et al., 2017).

Our study does not dispute the existence of primacy effects but questions whether nudges can take advantage of these primacy effects. Accordingly, we distinguish between the proximate effectiveness of the nudge on a consumer's attention measured by CTs and its downstream effectiveness at causing the prospective buyer to purchase the property. Similarly, in online advertising CTs are the leading measure of advertising effectiveness and are underpinned by the assumption that they correlate with subsequent purchases (e.g. Drèze and Hussherr, 2003; Hollis, 2005; Varian, 2007). Prior literature has examined this relationship between CTs and subsequent purchases on variety of platforms. It shows that there is mixed evidence on the relationship between attention and action arising from online viewing behaviours. Large scale field experiments on Yahoo! (Lewis and Reiley, 2014) and with a health and beauty care website (Manchanda et al., 2006) do demonstrate such a relationship. However regression discontinuity analysis of adverts on Google for consumer durables sold by a specific retailer showed that the adverts' position significantly affected CTs but not sales (Narayanan and Kalyanam, 2015). Additionally a field study with eBay using non branded goods showed that new and infrequent users are positively influenced by adverts, but that the purchasing behaviour of more frequent users, who account for most of the advertising expenses are not influenced by adverts, resulting in average returns that are negative (Blake et al., 2015). Using terminology from Bagwell (2007), Blake et al. (2015) posit that such adverts are "informative" of product characteristics but are not "persuasive" and so do not influence buyers' preferences. Our main results are consistent with the studies by Narayanan and Kalyanam (2015) and Blake et al. (2015) which found no relationship between CTs and downstream behaviours but contrast with those by Manchanda et al. (2006) and Lewis and Reiley (2014) which do demonstrate such a relationship.

Outside digital advertising, other studies compare short-term, proximate effects of nudges against their medium-term, downstream effects. In household finance, changing the default so that employees automatically enrolled into their company pension schemes did accelerate the rate of enrolment, but not hugely (Choi et al., 2004). Furthermore in the US, retirement savings accounts suffered leakage with employees making withdrawals or using them as security for borrowing at favourable interest rates (Argento et al., 2015) and there is some evidence of employees borrowing to save with an increase in first mortgage balances in foreclosure following their employer bringing in auto-enrolment (Beshears et al., 2022). Whilst in the UK, the low minimum auto-enrolment contribution rate dragged down the prior average contribution rate (Madrian and Shea, 2001) and automatically enrolled employees were found to contribute less to their subsequent employer's pension plan than their non-automatically enrolled peers if automatic enrolment was not provided (Choukhmane, 2019).

Studying credit card debt repayment, Guttman-Kenney et al. (2021) conducted a field experiment on 41,000 credit cards from large UK lender. They found shrouding the automatic minimum payment option when consumers apply for the card increases the likelihood that consumers choose above-minimum automatic payments. However this has no effect on overall debt as the automatic fixed payments that consumers choose are still very low and are coupled with a reduction in (infrequent) manual payments.

In environmental science, there is evidence that welfare gains of home energy social comparison reports were overstated because they do not take account of implementation costs borne privately by households (Allcott and Kessler, 2019). Additionally promoting green energy nudges was found to crowd out support for more substantive climate change policy interventions (Hagmann et al., 2019; Werfel, 2017). Finally, in health proximate interventions on the food ordering process generally produce only small effects (VanEpps et al., 2016) with people regaining weight as soon as weight loss incentives are removed (John et al., 2012).

Although some nudges do show large proximate effects (e.g. Bettinger et al., 2012; Chetty et al., 2014); others show no effect at all (e.g. Milkman et al., 2021; Adams et al., 2022) or even the opposite effect to what was intended (e.g. Cialdini et al., 2006; Beshears et al., 2015; John, 2018). Meta-studies of specific choice architecture intervention techniques show that the effect size, measured by Cohen's *d*, is usually small to medium and even this may be overstated due to publication bias (Jachimowicz et al., 2019; Beshears and Kosowsky, 2020; Mertens et al., 2022). Meta-studies of field experiments where an intervention may incorporate multiple choice architecture techniques (Cadario and Chandon, 2020; DellaVigna and Linos, 2022) show a lower effect size —DellaVigna and Linos (2022) whose analysis was additionally based on both published and unpublished studies claim accurate effect size measurement can be hindered by low statistical power. Effect size is found to depend on the type of intervention, defaults typically generate the largest, and the context. Interventions in the food domain, where behavioural costs and perceived long-term consequences to the decision-maker are low, are the most effective whereas interventions in the finance domain, where their impact is high, are least effective (Mertens et al., 2022).

In Study 2 we examine the measurement of the effectiveness of nudges by attempting to replicate our results from the RCT in the field with a quasi-experiment where we apply synthetic control methods (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015). Such methods use observational data, to estimate causal effects by constructing a simulated control group as the counterfactual to the treatment group. Our observational data is comparable sales listings data provided by the same property portal.

Synthetic control methods (SCMs) have become a widely adopted empirical research tool used across numerous fields including crime (Pinotti, 2015), economics (Bohn et al., 2014; Allegretto et al., 2017), health (Pieters et al., 2016), household finance (Sakaguchi et al., 2022), law (Cunningham and Shah, 2018), marketing (Fisher et al., 2019; Gill et al., 2017) and taxation (Kleven et al., 2013). Firms tend to rely on observational methods to evaluate advertising effectiveness rather than RCTs (Lavrakas, 2010) and RCTs examining nudges in finance are similarly rare (Heim and Huber, 2021).

But can observational methods imitate the identification quality of RCTs? Two studies which investigate the effectiveness of digital advertising make conflicting claims. The first examined how the rank position of adverts on a major online travel agent's website influenced consumers' search and purchase decisions by conducting a field study and then separately a complementary observational method to quantify the effect size (Ursu, 2018). However a comparison of the results from 15 large scale field trials conducted with Facebook to the results from the most widely used SCMs, including exact matching and propensity score matching, found the observational methods often showed different effects, despite conditioning upon an extensive battery of demographic and behavioural controls (Gordon et al., 2019).

We compare the results from our field study to those from observational methods to test whether they are consistent. We find the exact marching SCM did not accurately measure the effectiveness of the nudge consistently which supports the findings of Gordon et al. (2019).

Approaches

Our RCT in the field was conducted with one of the world's largest property portals. The study was pre-registered including experiment design, observation period, and main econometric analysis (see Appendix C.1). The study design involved 2,276 property adverts constituting all residential sales listings by 5 major real estate agencies over an 8 week period between 12 October and 6 December 2020 in one European country (Country). They were randomly assigned to either the treatment or control arms. Allocation of the new listings took place weekly using a stratified randomisation approach whereby the 2 arms were balanced each week by real estate agency.

Treated (nudged) adverts were automatically returned at the top of search results (known as SERPs or Search Engine Results Pages) conditional upon them matching consumers' search criteria. The treatment did not affect the probability of a listing being returned by a search, only the listing's position, conditional on being returned by a search. Listings in the control group remained at their natural positions, in order of the properties' asking price or date first marketed at each consumer's discretion.[2] The treatment applied to the property listing for a 1 week period, after which it reverted to an untreated status, (i.e., identical to the control group).

For the main study, the proximate outcome of CTs and downstream outcomes relating to the sale of the property including time to sale STC, time to completed sale and sales price were measured until their specified end-points. Ordinary least squares and survival analysis regression techniques were used to estimate the effect of the nudge. As robustness checks we conditioned on a battery of controls to account for balancing, property and advert characteristics.

As extensions, we firstly investigated whether a first-placed advert is more effective for specific categories of property by considering the interaction of our treatment with list price, number of bedrooms and property type on the proximate outcome of CTs and the downstream outcomes of time to completed sale and sales price achieved. This used data from the RCT. We secondly examined whether real estate agents purchased the first place advertising spot for those properties where it would have most effect. This used a private dataset provided by the same property portal of sales listings of residential properties located in the Country's capital city (Capital) between January 2009 to December 2018.

In Study 2 we compare results from our RCT in the field to results from two synthetic control simulations to investigate whether SCMs can return equivalent results to our RCT. The observational data is taken from the above Capital dataset. The treatment set are listings which for 1 week only property vendors had applied

[2] The portal's default setting is listing adverts from highest to lowest price. The consumer can select other options: lowest to highest price; latest to earliest marketed or earliest to latest marketed but in all cases the nudged advert is displayed first.

the nudge by purchasing enhanced adverts so they appeared first in any relevant search but otherwise had purchased standard adverts which appeared in their natural positions.[3] The control set are listings which always had standard adverts and so always appeared in their natural positions in the search results. Using two sets of matching criteria, we match our treatment set of listings to the control set to construct two samples to carry out the simulations. For our first simulation the matching of treatment and control sets is analogous to how the RCT is balanced (matched by real estate agent and week listed) and for our second simulation we match on the full battery of controls on which we condition our RCT.

We carry out equivalent ordinary least squares and survival analysis regression analysis as the main study to estimate the effect size of the nudge on each proximate and downstream outcome, conditioning on the same battery of controls which account for balancing, property characteristics and advert characteristics. We compare the effect sizes and their statistical significance to assess whether a researcher would draw similar conclusions regardless of whether they had carried out the field study or one of the simulations.

Summary

Our study contributes to the broader debate around the measurement, and effectiveness, of nudges (Science and Technology Select Committee, 2011; Laibson, 2020). Despite their generally modest effect sizes, market participants and policymakers may promote nudge-type policies where the onus is on individual behaviour change and so avoid politically unpopular and more expensive interventions which require system change (Loewenstein and Chater, 2017; Chater and Loewenstein, 2022). Additionally our study contributes to the debate on the efficacy of using observational methods in lieu of randomised control trials.

The remainder of the chapter proceeds as follows: for Study 1, including its extensions, Section 4.2 introduces the experimental details; Section 4.3 presents the econometric specification used in the analysis and Section 4.4 presents the main results. Study 2 including method and results is described in Section 4.5. Section 4.6 concludes.

[3] The portal sells three types of advert — enhanced, augmented and standard. Only enhanced adverts boost a listing's position to first place in any SERP where it matched the required search criteria. (Otherwise each advert type has its own designated format comprising of a box containing a very short property description with a set number of photographs. Enhanced and augmented adverts are highlighted with different coloured surrounds, whereas standard adverts are not highlighted.)

4.2 Study 1: Experiment and Data

We ask what effect does nudging a property advert so that for one week it is the first to be seen in the SERP of similar adverts before returning to its natural position have on the proximate outcome of CTs generated and broader outcomes including time to sale and sales price achieved? Our main study investigates this question with a RCT carried out in the field. We extend our analysis to determine the categories of property where a nudged advert is more effective and then examine whether nudged adverts are actually used in real life where they work best.

4.2.1 Experimental Design

Our study design drew upon five of the property portal's major real estate agency clients (PEAs) which did not routinely use enhanced adverts for their customers' listings, to participate in the experiment. The agencies specialise in different types of residential real estate in terms of the properties' location and value. One agency is online only, whereas the others have multiple physical branches as well as an online presence. Our sample comprises all new property listings marketed by the PEAs for routine sales of ordinary residential properties by private individuals from 12 October 2020 (Monday, Week 1) of the trial up to and including 6 December 2020 (Sunday, Week 8) the week where the pre-specified sample size of at least 2,000 listings was reached.[4]

During the trial's allocation stage, on Monday of Week 2 and each successive Monday thereafter to Week 9, the portal team selected all new property listings from the previous week that met our sample selection criteria see Section 4.2.2. They notified the experimenter of each listing's numeric identifier and PEA. The experimenter randomly allocated the listings to the control and treatment arms and provided this allocation to the portal team. The portal team nudged the treatment arm adverts straightaway (i.e., upgraded them from standard to enhanced adverts), but made no change to the control arm adverts. The experimenter checked that a random sample of 5 treated and 5 control adverts were correctly allocated.[5] The treated adverts automatically reverted to standard adverts a week later at the end of the treatment period.[6] The portal's IT system automatically collects daily data for each listing on the number of CTs and email requests received.[7] Listings' sale STC statuses are updated by the PEA as part

[4] The sample size was determined by a statistical power calculation.

[5] No misallocations were identified during the allocation stage.

[6] A property advert treated anytime on Monday is changed back to a standard advert, by computer batch process, around 2 a.m. on Tuesday of the following week. The portal's internal reporting records this as 8 days of enhancement.

[7] 'Email requests' occur where the consumer clicks a virtual button on either the property's advert or its full description to email the property's real estate agent for further particulars.

of their usual workflow. Finally on a monthly basis the portal updates its records for completed sales using data published by the Country's land registries (the government agencies responsible for collecting this information).[8] The portal team periodically sent updated data for the sample to the experimenter for analysis.

We used a stratified randomisation approach to allocate the new listings to the treatment and control arms balancing the two arms by PEA each week. The sample selection took place over many weeks, so we balanced by week to reduce the risk of temporal effects correlating with treatment. This is a multi-centre trial using different PEAs each which target different customer segments, so we also stratified by PEA to control for such differences in the trial population. Figure 4.1 Panel A illustrates the weekly build up of the sample and Panel B illustrates the split by PEA by the two arms. A computer algorithm based on a stratified urn approach (Wei, 1978) randomly allocated the listings to the two arms.

4.2.2 Sample Selection

We selected the listings pool for the trial using a series of criteria. Listings are excluded if they do not meet all of our entry criteria. We are interested in new routine sales, so properties were excluded if they had been previously listed in the previous 14 weeks; their adverts were previously enhanced; had received an offer accepted by the seller or were due to be auctioned. We are interested in ordinary residential properties so only properties that were categorised as apartments or houses (terraced, semi-detached, detached, bungalows or unspecified) were included.[9] Properties needed at least 1 bedroom unless a studio apartment (0 bedrooms) but no more than 5 bedrooms. Businesses (including farms) and properties that can only be purchased by retirees were excluded. We are interested in sales by private individuals so development properties were excluded.[10] Finally we excluded listings that did not have a full address (and so cannot be uniquely identified); did not specify an asking price or did not pass the portal's data quality checks.

The portal team screened listings as potential participants for the trial, according to these criteria, using data from the day before the random allocation took place. Since excluded listings were not randomised, their removal from the trial does not bias any treatment-control arm comparison. Our sample selection criteria returned 2,276 eligible properties, however 12 properties delisted between allocation and the collection of data on the first day of the treatment period and so we could not collect

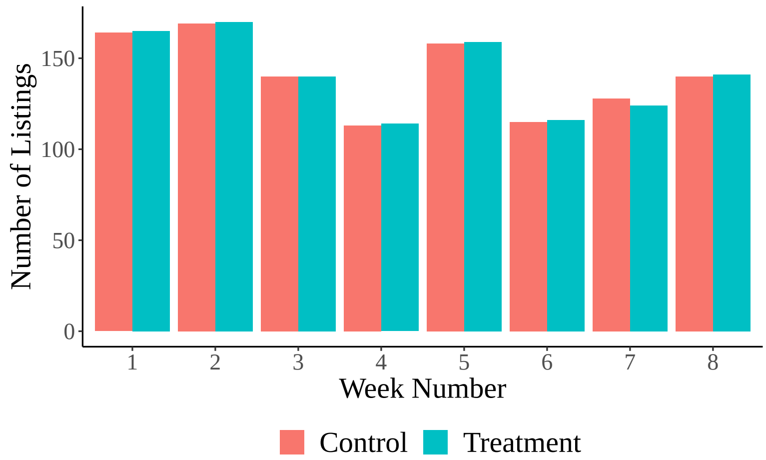
[8] The experimenter also matched land registry sales data to the listings manually.

[9] We use the following terminology apartments or synonymously flats; terraced houses share a common wall with a neighbouring house on each side; semi-detached houses share a common wall with a single neighbouring house and detached houses do not share walls. Bungalows are single storey houses.

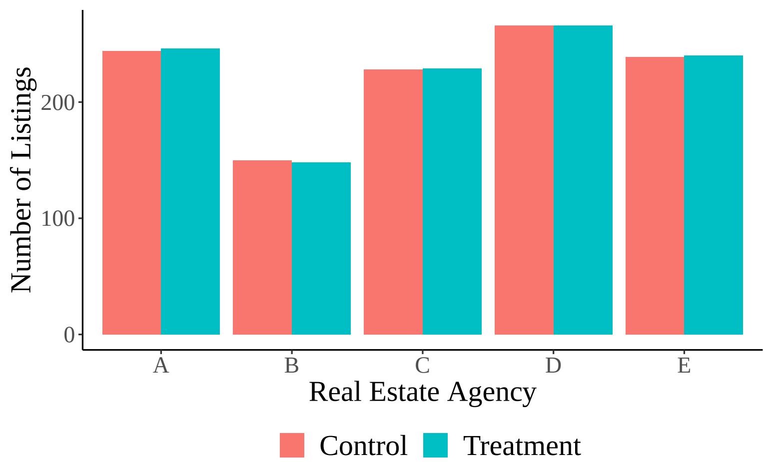
[10] Property developers are able to list their new developments in a variety of different ways e.g. one advert could relate to an individual property; all properties of the same type or the whole development.

Figure 4.1: Frequency Distribution of Listings by Week Selected and Real Estate Agency

(A) Frequency Distribution of Listings by Week Selected



(B) Frequency Distribution of Listings by Real Estate Agency



Note: New listings were balanced between the treatment arm and control arm conditions across the 5 participating real estate agencies each week of sample selection. The figure shows the distribution of listings split between treatment and control arms by week of sample selection (Panel A) and by the PEA marketing the listing (Panel B).

any data. Additionally 8 properties were found to be simultaneously listed twice, once as a full sale and once as a partial sale (with the buyer renting the remainder). This gave a baseline sample of 2,256, see Table 4.1.

Additionally Region 6's land registry does not publish sales data so its 7 listings are necessarily excluded from regression analyses for the 4 outcome variables which requires this data (Early Sale, Sale, Time to Sale and Sales Price) but are included in all other analyses. Finally for 1 listing the sales date but not sale price was registered so it must be excluded from the regression analysis for the 1 outcome variable which requires this data (Sales Price) but is included in all other analyses.

Table 4.1: Baseline Sample Selection

	Number of Listings		
Description	Total	Control	Treated
Original Allocation	2276	1138	1138
Properties Delisted Before Trial Start	-12	-7	-5
Properties Simultaneously Listed for Full and Partial Sale	-8	-4	-4
Baseline Sample	2256	1127	1129

Note: The table shows the steps in baseline sample selection by total and treatment arm. Original Allocation is the property listings that met our inclusion criteria described and were randomly allocated to the control and treatment arms. We necessarily drop 12 properties that delisted between allocation and the collection of data on the first day of the treatment period as no data is available. We further drop 8 properties that were found to be simultaneously listed for both full and partial sale after the trial started.

This RCT uses an intention to treat strategy and so our analysis compares participating listings in the arms to which they were originally randomly assigned, regardless of whether they are subsequently found not to have satisfied the study entry criteria; received the wrong treatment or suffered any other deviation from the protocol (Hollis and Campbell, 1999). At the end of the final treatment period, for good practice, all listings were retested to confirm that they were still in compliance with all study protocols (e.g. PEAs may have subsequently corrected property data). The results which are provided in Table C.1 show a very small number of discrepancies and we make no adjustments to the baseline sample or our analysis.

4.2.3 Summary Statistics

Our sample was balanced by week of first listing and by PEA. Table 4.2 provides the summary statistics and results of the chi-squared tests between treatment and control arms for week listed and for PEA. The results confirm that the sample is balanced for these criteria.

Table 4.2: Summary Statistics: Balancing Controls

Characteristics	Control	Treated	p
	N = 1127	N = 1129	
Real Estate Agency; N (%)			1.00
Agency A	244 (21.65)	246 (21.79)	
Agency B	150 (13.31)	148 (13.11)	
Agency C	228 (20.23)	229 (20.28)	
Agency D	266 (23.60)	266 (23.56)	
Agency E	239 (21.21)	240 (21.26)	
Allocation Week Number; N (%)			1.00
1	164 (14.55)	165 (14.61)	
2	169 (15.00)	170 (15.06)	
3	140 (12.42)	140 (12.40)	
4	113 (10.03)	114 (10.10)	
5	158 (14.02)	159 (14.08)	
6	115 (10.20)	116 (10.27)	
7	128 (11.36)	124 (10.98)	
8	140 (12.42)	141 (12.49)	

Note: The table displays the summary statistics for the balancing controls in our baseline sample. These two variables are *Real Estate Agency* and *Allocation Week Number* and we show the frequency and in brackets the proportion of their levels by control and treatment arm. The column p reports the *p*-value of a test for each variable that the proportion of each level does not differ between the 2 arms.

Table 4.3 shows summary statistics of the characteristics of the properties in our baseline sample by treatment and control arm. The characteristics are property type, number of bedrooms, regional location and initial asking price. For both arms the most popular property type is flat, then terraced house with unspecified house least popular; the most common number of bedroom is 2 or 3, with 0 bedrooms least common and the most popular regions are Region 8 and Region 9 with Region 6 least popular. The results from chi-squared tests show that there is no significant difference between the 2 arms with respect to these 3 criteria. The mean initial asking price for properties in the treatment arm is ¤467,000 compared to ¤437,000 for properties in the control arm and a t-test demonstrates there is not a significant difference in asking price between the 2 arms.[11]

[11] We use the generic currency symbol '¤' to obfuscate the originating country.

Table 4.3: Summary Statistics: Property Characteristics

Characteristics	Control	Treatment	p
List Price (¤); Mean (SD)	436,987 (439,935)	467,004 (596,681)	0.17
Property Type; N (%)			0.75
Flat / Apartment	361 (32.03)	358 (31.71)	
Bungalow	57 (5.06)	50 (4.43)	
House - Detached	225 (19.96)	208 (18.42)	
House - Semi-Detached	201 (17.83)	205 (18.16)	
House - Terraced	274 (24.31)	295 (26.13)	
House - Unspecified	9 (0.80)	13 (1.15)	
Bedrooms (Number); N (%)			0.72
0	4 (0.35)	6 (0.53)	
1	118 (10.47)	124 (10.98)	
2	371 (32.92)	348 (30.82)	
3	352 (31.23)	380 (33.66)	
4	212 (18.81)	198 (17.54)	
5	70 (6.21)	73 (6.47)	
Region; N (%)			0.15
Region 1	23 (2.04)	33 (2.92)	
Region 2	45 (3.99)	43 (3.81)	
Region 3	137 (12.16)	176 (15.59)	
Region 4	30 (2.66)	15 (1.33)	
Region 5	46 (4.08)	48 (4.25)	
Region 6	4 (0.35)	3 (0.27)	
Region 7	18 (1.60)	20 (1.77)	
Region 8	522 (46.32)	533 (47.21)	
Region 9	211 (18.72)	180 (15.94)	
Region 10	18 (1.60)	17 (1.51)	
Region 11	55 (4.88)	45 (3.99)	
Region 12	18 (1.60)	16 (1.42)	

Note: The table displays the summary statistics for property characteristics of the listings in our baseline sample. For the variable *List Price* we show the mean and in brackets the standard deviation by control and treatment arm. P reports the *p*-value of a test that the mean does not differ between arms. For the variables *Property Type, Bedooms* and *Region* we show the frequency and in brackets the proportion of their levels by Control and Treatment arm. The column p reports the *p*-value of a test for each variable that the proportion of each level does not differ between arms.

Table 4.4: Summary Statistics: Advert Characteristics

Characteristics	Control	Treated	p
Time Between Listing and Allocation (Days); Mean (SD)	4.72 (1.60)	4.75 (1.54)	0.63
Floor Plan Present (Proportion); Mean (SD)	0.75 (0.44)	0.76 (0.43)	0.48
Images (Number); N (%)			0.11
0 - 2 images	9 (0.80)	2 (0.18)	
3 - 7 images	73 (6.48)	74 (6.55)	
8 - 9 images	150 (13.31)	140 (12.40)	
10 - 12 images	333 (29.55)	374 (33.13)	
13 - 18 images	399 (35.40)	400 (35.43)	
> 18 images	163 (14.46)	139 (12.31)	
Augmented Advert (Proportion); Mean (SD)	0.21 (0.41)	0.21 (0.41)	0.74

Note: The table displays the summary statistics for advert characteristics of the listings in our baseline sample. For the variables *Time Between Listing and Allocation*, *Floor Plan Present* and *Augmented Advert* we show the mean and in brackets the standard deviation by control and treatment arm. Column p reports the *p*-value of a test that the mean does not differ between arms. For the variable *Images* we show the frequency and in brackets the proportion of its levels by Control and Treatment arm. The column P reports the *p*-value of a test that the proportion of each level does not differ between arms.

Table 4.4 shows summary statistics of the characteristics of the adverts by arm. The characteristics are time between first listing and allocation day; whether the advert was augmented; whether the property description included a floor plan and how many photographs were included in the description. The mean time to allocation was 4.7 days and 21% of adverts were augmented for both arms. 76% of treated arm compared to 75% of control arm listings had a floor plan. The results from t-tests demonstrate no significant difference between the 2 arms with respect to these criteria. The most common range of photographs present was 13-18 and least common 0-2 for both arms. A chi-squared test demonstrates there is not a significant difference in number of images between arms.

4.2.4 Extensions

We extend the main study with additional exploratory analysis of the interaction between treatment and property categories. Property categories are not randomly assigned in this analysis. First, we investigate whether having an advert first in the SERP is more effective for specific categories of property by considering the interaction of the treatment with list price, number of bedrooms and type on the proximate outcome of CTs and the downstream outcomes of time to sale and sales price achieved. We use the data and baseline sample from the field study, but discretize list price by splitting into 3 approximately equally sized quantiles "Low", "Medium" and "High"; reclassify number of bedrooms into 3 groups "Two or Fewer Bedrooms", "Three Bedrooms" and "Four or More Bedrooms" and property type into 3 groups "Flat/Terraced", "Bungalow/Unspecified" and "Semi/Detached".

Second, we then examine whether real estate agents actually purchase enhanced adverts for the property categories where they are most effective. To do so we use the Capital sample which is a private dataset of residential property listings in the Capital over a 9 year period and apply to it the same sample selection criteria as in the main study. This dataset is also used in Study 2 to construct the samples for the synthetic control analysis and is described in Section 4.5.1. Summary statistics for the Capital sample by property category and trial arm are given in Table 4.11.

We compare by property category the proportion of listings in the treatment arm which had an enhanced advert for 1 week so ranking first in any applicable SERP, to those in the Control arm where the advert was always non-enhanced and so always ranked in its natural position.

4.3 Study 1: Econometric Model

The econometric models used for estimation in the main study and extensions are described in Section 4.3.1 and Section 4.3.2 respectively.

4.3.1 Main Study

OLS Analysis

OLS regression analysis estimates the average treatment effect, the difference in mean outcomes between listings assigned to the treatment and the control arms. We estimate the baseline effect of treating the listing using the econometric specification given in Equation 4.1 in which the unit of observation is an individual property listing *i*.

$$DepVar_i = b_0 + b_1 Treated_i + \epsilon_i, \tag{4.1}$$

Treated is a dummy variable with a value of 1 if the advert for listing *i* is treated, so that for one week it is displayed first in the SERP of similar adverts, but is otherwise ranked at its natural position. The dependent variable *DepVar* is determined by the outcome being estimated. We have 1 ‘proximate’ outcome, CTs generated, measuring whether a consumer pays more attention to the treated adverts and 6 ‘downstream’ outcomes relating to whether the consumer takes action, which are Email Requests, Early Sale STC, Sale STC, Early Sale, Sale and Sales Price.[12] Table 4.5 describes each different dependent variable including its measurement period.

We balanced the two arms by allocation week and PEA. Where variables are used in the randomisation process there is strong evidence that adjustments must be made

[12] We include OLS regression estimates for the effect of treating an advert for a week has on sale STC and sale for completeness although, as noted in the main text, they do not take censoring into account.

Table 4.5: Description of Dependent Variables

Outcome	Model	Description	Measurement Period
Click Throughs	OLS	Natural logarithm of the sum of CTs an advert i receives.	Treatment period.
Email Requests	OLS	Dummy variable with a value of 1 if advert i receives at least 1 email request.	Treatment period.
Early SSTC	OLS	Dummy variable with a value of 1 if an advertised property i is sold STC.	45 days from first listing.
SSTC	OLS	Dummy variable with a value of 1 if an advertised property i is sold STC.	170 days from first listing.
Early Sale	OLS	Dummy variable with a value of 1 if an advertised property i is sold.	190 days from first listing.
Sale	OLS	Dummy variable with a value of 1 if an advertised property i is sold.	280 days from first listing.
Sales Price	OLS	Sales price of advertised property i as a proportion of its initial list price.	At sale.
SSTC	Cox	Time to SSTC	170 days from the first listing of the final property to be listed.
Sale	Cox	Time to Sale	280 days from the first listing of the final property to be listed.

Note: The table presents the descriptions of the different dependent variables measured. The rationale for the endpoints of the Sale, Sales Price and SSTC dependent variables is based on a sample of 800,000 Capital listings on the portal over a 9 year period from 1 January 2010 to 31 Dec 2018: Of the listings that were sold STC, 25% were sold SSTC within 45 days and 90% within 170 days of first listing. Of the listings that were sold 25% were sold within 190 days and 75% within 280 days of first listing.

in the econometric analysis to avoid the upwards bias of standard errors (Kahan and Morris, 2012). Accordingly we modify our baseline OLS specification to include the balancing controls —allocation week number and PEA as covariates (Committee for Proprietary Medicinal Products et al., 2004). In subsequent robustness analyses we also estimate specifications that add controls for property characteristics (list price, property type, number of bedrooms, region) and advert characteristics (number of days between first listing and allocation day; whether the advert was augmented on allocation day; whether the property description included a floor plan and the number of photographs in the property description). Their descriptions and the summary statistics are given in Section 4.2.3.

Survival Analysis

This RCT has staggered entry for participating listings with follow-up periods differing by a number of weeks. Survival analysis methods, unlike OLS regression, allow the entire survival experience during follow-up to be compared (Friedman et al., 2015) and can account for censoring where for example properties either delist or the event does not occur (e.g. the property does not sell) before the study's end point. Merely excluding censored properties may introduce bias (Altman and Bland, 1998; Spruance et al., 2004). The Kaplan–Meier survival model, a nonparametric approach, estimates the survival functions in treatment and control arms, from which the absolute change in the probability of an event occurring within a set follow-up time can be computed. The Cox proportional hazards (Cox PH) survival model, a semi-parametric approach, estimates the treatment's effect on the relative change in the hazard of the event taking place.

With the Cox PH model, we estimate 2 downstream action outcomes: time-to-sale STC and time-to-completed sale, specifically h_{it} the size of the effect that treating an advert has on the owner selling (STC) their property i at time t, conditional on not selling their property (STC) until time t, using the econometric specification given in Equation 4.2.

$$h_{it} = \phi_i \exp\{b_1 Treated_{it}\} \tag{4.2}$$

The endpoints for sale STC and for completed sale are 170 and 280 days after the final property to be listed is first listed respectively (see Table 4.5). We modify our baseline Cox PH specification by adding as covariates the controls for balancing, property characteristics and advert characteristics described above for the OLS regression estimates.[13]

4.3.2 Extensions

To investigate whether having an advert first in the SERP is more effective for specific property categories we use the proximate attention outcome of CTs generated and two downstream action outcomes of time to completed sale and sales price achieved as the dependent variables. List price, number of bedrooms and type are used to differentiate the property categories. For the CTs and sales price analysis we extend our OLS baseline specification to include property category and the interaction of category and treating an advert to give Equation 4.3.

$$DepVar_i = b_0 + b_1 Treated_i + b_2 Category_i + b_3 Treated_i \times Category_i + \epsilon_i, \tag{4.3}$$

For the time to sale analysis we similarly extend our Cox PH baseline specification to give Equation 4.4.

$$h_{it} = \phi_i \exp\{b_1 Treated_{it} + b_2 Category_{it} + b_3 Treated_{it} \times Category_{it}\} \tag{4.4}$$

The meanings of terms which are also part of Equation 4.1 and/or Equation 4.2

[13] We test the proportional hazards assumption, that the relative hazard stays constant over time with different covariate levels, using a correlation test which is based on Schoenfeld residuals. The results, which are given in Table C.2, demonstrate that for SSTC as the dependent variable PEA and number of bedrooms violate the proportional hazard assumption and so we incorporate them in the Cox PH model as stratification factors rather than covariates and that for Sales as the the dependent variable PEA and allocation week violate the proportional hazard assumption and so similarly we incorporate them in the Cox PH model as stratification factors rather than covariates.

are unchanged. Category is a dummy with value 1 if advert i is for a property that falls within the relevant category class.[14] $Treated \times Category$ is the interaction term, a dummy with value 1 if advert i is treated and the property it advertises falls within the relevant category class.

We modify this specification by adding as covariates the controls for balancing, property characteristics and advert characteristics described above (excluding from property characteristics the category already included in the specification). Additionally, Figure C.1 Panel A demonstrates the distribution of list prices is positively skewed. Therefore, in this exploratory analysis we transform list prices by taking their natural log and standardising by deducting the mean of the log list prices and dividing by the standard deviation of the log list prices. Panel B illustrates the resultant symmetrical distribution.

4.4 Study 1: Results

4.4.1 Main Study

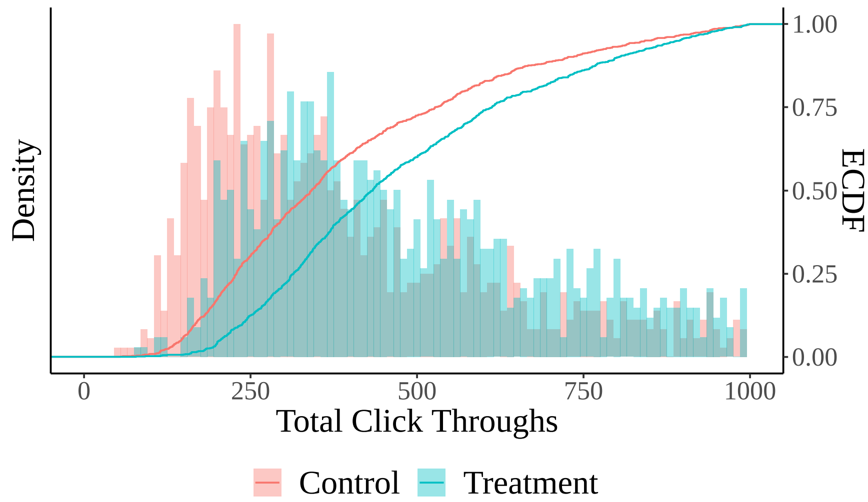
The unconditional relationship between treating an advert and the sum of the CTs for that listing during the treatment period is illustrated in Figure 4.2. The figure shows both the distribution and the empirical Cumulative Distribution Function (eCDF) of CTs generated for the treatment and control groups during the treatment period. The distribution function and eCDF for the treatment group are consistently to the right of the control group showing that across the whole distribution more CTs are generated when the advert is first in the SERP. Hence the treatment effect shifts the whole distribution of CTs.

Proximate Outcome

The mean result is confirmed by the OLS regression estimate which uses Equation 4.1 and is shown in Table 4.6. Column 1 shows the unconditional relationship between the natural logarithm of the number of CTs generated during the treatment period and the dummy for whether the advert is treated for a week so that for one week it is displayed first in the SERP of similar adverts, but is otherwise ranked at its natural position. The coefficient on the treatment dummy of 0.2807 is positive and significant at the 1% level. It implies that a treated advert generates 32.4% ($e^{0.2807}$) more CTs than an untreated advert on the baseline number of CTs, given by the constant of 377 ($e^{5.9321}$).

[14] For list price the classes are "Low" and "Medium" otherwise both coded 0 if "High". For number of bedrooms the classes are "Two or Fewer Bedrooms" and "Three Bedrooms", otherwise both coded 0 if "Four or More Bedrooms". For type the classes are "Flat/Terraced" and "Bungalow/Unspecified", otherwise both coded 0 if "Semi/Detached".

Figure 4.2: Comparison of Click Through Distributions



Note: The figure shows the distributions and empirical cumulative distribution functions of click throughs during the treatment period for the treatment and control arms. For a better visualisation the x-axis is truncated at 1,000, otherwise observations include all listings in the baseline sample.

Table 4.6: Treatment Effect on Click Throughs: OLS Estimates

	$LogClickThroughs_i$			
	(1)	(2)	(3)	(4)
Treated = 1	0.2807***	0.2815***	0.2946***	0.2997***
	(0.0255)	(0.0237)	(0.0195)	(0.0190)
Constant	5.9321***	5.8781***	5.5950***	6.2846***
	(0.0180)	(0.0387)	(0.0784)	(0.1821)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256
R^2	0.0510	0.1868	0.4583	0.4960

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4). The controls are described in Section 4.2.3. The dependent variable takes the value of the natural logarithm of the total number of the listing's click throughs during the treatment period. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm and zero if allocated to the control arm. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

Column 2 adds fixed effects for the allocation week and PEA which are the variables used in the randomisation process. The coefficient on the treatment dummy barely changes to 0.2815 implying that a treated advert generates 32.5% ($e^{0.2815}$) more CTs than an untreated advert. We examine the robustness of our results by adding controls for property characteristics in Column 3 and advert characteristics in Column 4, The coefficients on the treatment dummy remain positive, consistent and significant at the 1% level at 0.2946 (column 3) and 0.2997 (column 4) corresponding to a 32.4% and 34.9% increase in CTs respectively.

Downstream Outcomes

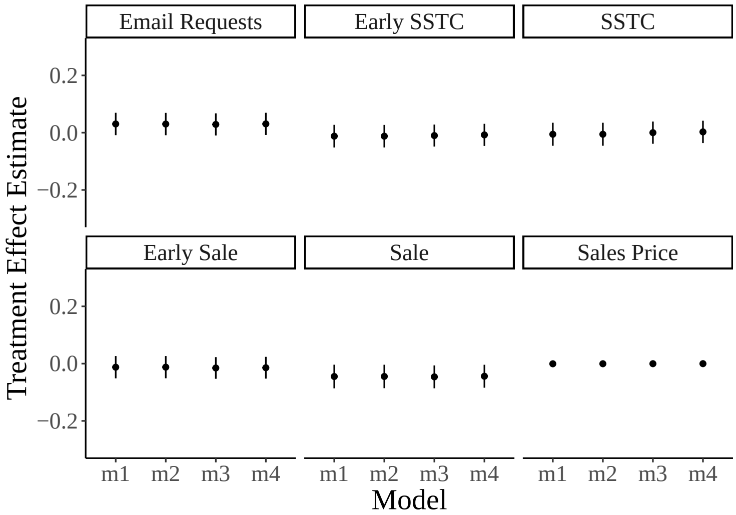
Figure 4.3 illustrates the estimates of the treatment effect and their confidence intervals by each OLS regression model specification for the downstream outcomes. The dependent variables are described in Table 4.5. Model m1 shows the unconditional relationship between the dependent variable and the treatment dummy, m2 adds fixed effects for the allocation week and PEA which are the variables used in the randomisation process, m3 adds controls for property characteristics and m4 adds advert characteristics. The estimates are derived from the regression estimates presented in Table C.3 to Table C.10. For each dependent variable, estimates are consistent across the 4 specifications and so we discuss in detail only their estimates for the final specification with the full set of controls added, which are presented in Table 4.7. Most estimates are small and defined around zero or in some cases they are negative.

Email Requests. Table 4.7 summarises the OLS regression estimate for the relationship between the probability that an advert generates at least one email request during the treatment period and the dummy for whether the advert is treated. Observations include all listings in our RCT baseline sample. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.3). The coefficient of the treatment dummy is 0.0307. It implies that a treated advert is only 3.1% more likely to generate at least 1 email request [95% CI: 0.8% less likely, 6.9% more likely] than an untreated advert. The mean probability that an advert generates at least 1 email request during the treatment period is 34%.

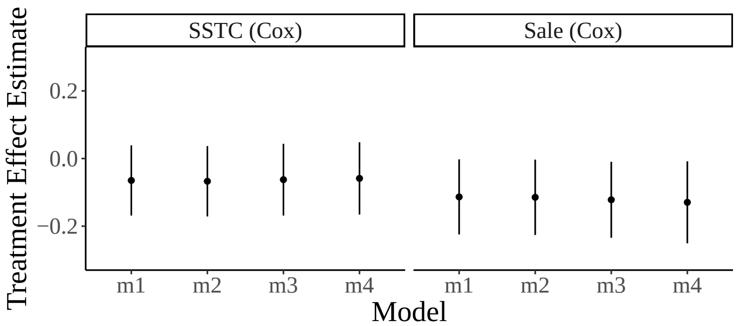
Early SSTC. Table 4.7 summarises the OLS regression estimate for the relationship between the probability that the property an advert is marketing reaches SSTC within 45 days from its first listing and the dummy for whether the advert is treated. Observations include all listings in our RCT baseline sample. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.4).

Figure 4.3: Treatment Effect Estimates for Downstream Outcomes from RCT

(A) Ordinary Least Squares



(B) Cox Proportional Hazard



Note: The figure shows the treatment effect point estimates and 95% confidence intervals (vertical lines) for the RCT. Model m1 shows the unconditional relationship between the dependent variable and the treatment dummy, m2, m3 and m4 add controls for balancing, property characteristics and advert characteristics respectively. The dependent variables and controls are described in Table 4.5 and Section 4.2.3 respectively. The estimates are presented in Table C.3 to Table C.10. Observations include all listings in their baseline samples except the Early Sale STC, Sale STC, Sale STC (Cox) Early Sale, Sale, Sale (Cox) and Sales Price estimates do not include the 7 RCT listings located in Region 6 and the Sales Price estimate also does not include the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2)

Table 4.7: Summary of Treatment Effect Estimates

	OLS			Cox PH	
	Treated = 1		Mean	Treated = 1	
Email Requests	0.0307	(0.0198)	0.3373		
Early SSTC	-0.0077	(0.0197)	0.3551		
SSTC	0.0027	(0.0199)	0.6148		
SSTC (Cox)				-0.0587	(0.0545)
Early Sale	-0.0143	(0.0194)	0.3295		
Sale	-0.0441**	(0.0205)	0.5296		
Sale (Cox)				-0.1295**	(0.0619)
Sales Price	-0.0001	(0.0027)	0.9628		

Note: The table summarises the OLS or if stated the Cox Proportional Hazard regression estimates for our baseline specification with Balancing, Property Characteristics and Advert Characteristics added. These controls are described in Section 4.2.3. Dependent variables are described in Table 4.5. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm or otherwise 0. Observations include all listings in the baseline sample except the Early Sale STC, Sale STC, Sale STC (Cox) Early Sale, Sale, Sale (Cox) and Sales Price estimates do not include the 7 RCT listings located in Region 6 and the Sales Price estimate also does not include the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2). $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

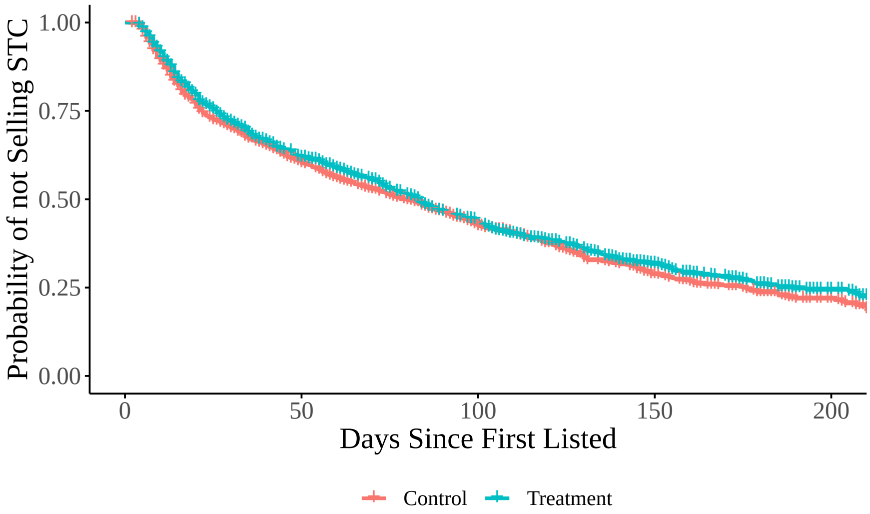
The coefficient of the treatment dummy is -0.0077. It implies that a property with a treated advert is 0.8% less likely to sell STC early [95% CI: 4.6% less likely, 3.1% more likely] than a property with an untreated advert. The mean probability that a property sells STC early is 36%. Hence the treatment effect is near zero.

SSTC. Table 4.7 summarises the OLS regression estimate for the relationship between the probability that the property an advert is marketing reaches SSTC within 170 days from its first listing and the dummy for whether the advert is treated. Observations include all listings in our RCT baseline sample. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.5).

The coefficient on the treatment dummy is 0.0027. It implies that a property with a treated advert is 0.3% less likely to sell STC [95% CI: 3.6% less likely, 4.2% more likely] than a property with an untreated advert. The mean probability that an a property sells STC is 61%. Hence, again, the treatment effect is extremely small compared to the mean probability.

SSTC (Cox). Figure 4.4 illustrates the Kaplan-Meier Survival Curves for sales STC for the treatment arm and control arm adverts. Table 4.7 summarises the estimate for a Cox PH model where we estimate the size of the effect that treating an advert has on the owner selling their property STC at time t conditional on not selling their property

Figure 4.4: Kaplan-Meier Survival Curves for Sales Subject to Contract



Note: The figure shows the Kaplan-Meier Survival Curves for Sales Subject to Contract for the treatment arm and control arm adverts. The end point is 170 days after the final property to be listed is first listed (see Table 4.5). Observations include all listings in our RCT baseline sample.

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STC until time *t*. The endpoint is 170 days of the final property to be listed is first listed. Observations include all listings in our RCT baseline sample. The model uses the specification given in Equation 4.2 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.6).

The coefficient on the treatment dummy is -0.0587. It implies that a property with a treated advert is 6.0% ($e^{-0.0587}$) less likely to sell STC [95% CI: 15.3% less likely, 4.9% more likely] than a property with an untreated advert.

Early Sale. Table 4.7 summarises the OLS regression estimate for the relationship between the probability that the property an advert is marketing sells up to 190 days from its first listing and the dummy for whether the advert is treated.[15] The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.7).

The coefficient on the treatment dummy is -0.0143. It implies that a property with a treated advert is 1.4% less likely to sell early [95% CI: 5.2% less likely, 2.4% more likely] than a property with an untreated advert. The mean probability that an a property sells early is 33%. Again, the treatment effect is extremely small compared to

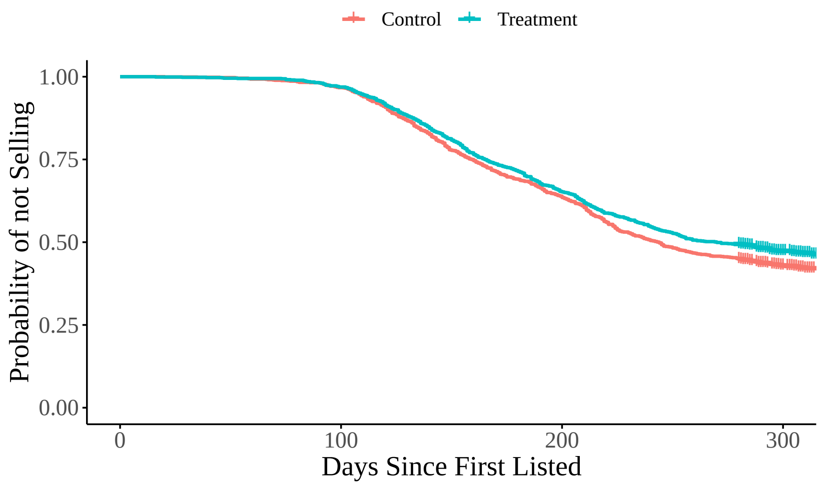
[15] Observations include all listings in our RCT baseline sample except for the 7 RCT listings located in Region 6 (see Section 4.2.2).

the mean probability.

Sale. Table 4.7 summarises the OLS regression estimate for the relationship between the probability that the property an advert is marketing sells STC up to 280 days from its first listing and the dummy for whether the advert is treated. For sample see Footnote 15. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.8).

The coefficient on the treatment dummy is -0.0441. It implies that a property with a treated advert is 4.4% less likely to sell [95% CI: 8.4% less likely, 0.4% less likely] than a property with an untreated advert. The mean probability that an a property sells is 53%.

Figure 4.5: Kaplan-Meier Survival Curves for Sales

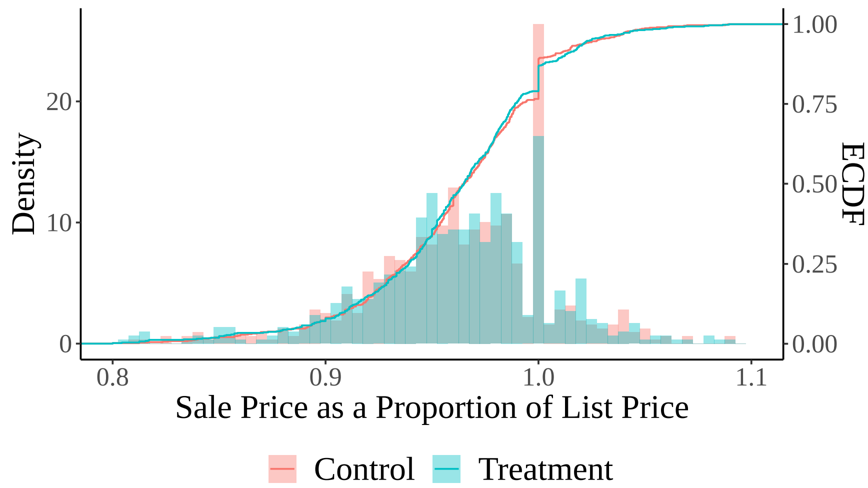


Note: The figure shows the Kaplan-Meier Survival Curves for Sales for the treatment and control arms. The end point is 280 days after the final property to be listed is first listed. See Table 4.5. Observations include all listings in our RCT baseline samples except for the 7 RCT listings located in Region 6 (see Section 4.2.2).

Sale (Cox). Figure 4.5 illustrates the Kaplan-Meier Survival Curves for sales for the treatment arm and control arm adverts. Table 4.7 provides the estimate for a Cox PH model where we estimate the size of the effect that treating an advert has on the owner selling their property at time t conditional on not selling their property until time t. The endpoint is 280 days of the final property to be listed is first listed. For sample see Footnote 15. The model uses the specification given in Equation 4.2 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.9).

The coefficient on the treatment dummy is -0.1295. It implies that a property with a treated advert is 12.1% ($e^{-0.1295}$) less likely to sell [95% CI: 28.5% less likely, 0.8% less likely] than a property with an untreated advert.

Figure 4.6: Comparision of Sale Price Distributions



Note: The figure shows the distributions and empirical cumulative distribution functions of sale price as a proportion of list price for the adverts in the treatment and control arms. Observations include all listings sold up to 280 days from the first list date of the final property to be listed (see Table 4.5), except for any sold property located in Region 6 and the one property where the sales date but not sales price was registered (see Section 4.2.2).

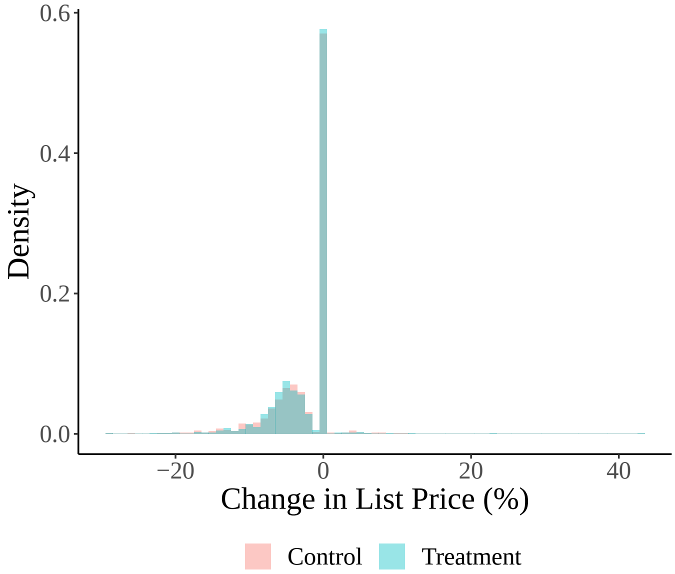
Sales Price. Figure 4.6 shows the distributions and empirical cumulative distribution functions of sale price as a proportion of list price for the adverts in the treatment and control arms. The figure illustrates the similarity between the 2 arms. This contention is supported by the OLS regression estimates for the relationship between the sales price of an advertised property as a proportion of its original list price and the dummy for whether its advert is treated provided in Table 4.14.[16] The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. (Estimates for the other specifications are provided in Table C.10).

The coefficient on the treatment dummy of -0.0001 is extremely close to zero. It implies that that the sales price as a proportion of original list price of a property with a treated advert is 0.0% lower [95% CI: 0.6% lower, 0.5% higher] than a property with an untreated advert. The mean sales price as a proportion of original list price is 96%.

Potentially a seller's willingness to change their property's list price may be influenced by whether their advert was treated. Figure 4.7 illustrates the distribution of change in list prices comparing the treatment and control arms. The change in

[16] Observations include all listings in our RCT baseline sample where the advertised property sold except for the 7 RCT listings located in Region 6 and the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2.)

Figure 4.7: Distribution of Change in List Price



Note: Distribution of change in list prices comparing the treatment and control arms. The change in price is taken as the difference between the original list price and the last recorded list price during the study's full measurement period (280 days from the first listing of the final property to be listed). Observations include all listings in our RCT baseline samples except for the 7 RCT listings located in Region 6 (see Section 4.2.2).

price is taken as the difference between the original list price and the last recorded list price during the study's full measurement period (280 days from the first listing of the final property to be listed). There is no significant effect for treatment, $t(2247) = -0.85$, $p = 0.39$, despite the list price of properties with treated adverts ($M = -2.32\%$, $SD = 4.06$) reducing less than those with untreated adverts ($M = -2.47\%$, $SD = 4.06$).

Summary. In summary we see consumers pay more attention to treated adverts since there is a large effect on CTs. However the treatment has no positive effects on their downstream behaviours of actually buying the property. Estimates for the effect on email requests, SSTC and sales price are small and defined around zero, whereas the effects on the probability of sale and the time to sale are negative.

4.4.2 Extensions

We ask whether there are categories of property listing where a first placed advert in the SERP has the greatest impact. Specifically we investigate whether treating the advert for a property that falls in a thick market of more common, similar real estate has a different effect then one in a thin market of less common, more distinctive real estate. We use the property categories of list price, number of bedrooms and property type as indicators of the thickness of the market e.g. moderately priced, 2 bedrooms flats are more common than highly priced, 5 bedroom detached houses. We examine the interactions between treated adverts and property categories on the proximate attention outcome of CTs generated and 2 downstream outcomes of time to sale and sales price achieved. We then investigate whether real estate agents actually purchase enhanced adverts, remember these are equivalent to our advert treatment, for the property categories where they are most effective.

Proximate Outcome

Table 4.8: Treatment Effect on Click Throughs with List Price Interaction: OLS Estimates

	$LogClickThroughs_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	0.2692***	0.1236***	0.1372***	0.1963***	0.2013***
	(0.0237)	(0.0418)	(0.0407)	(0.0345)	(0.0335)
Low List Price	-0.5499***	-0.6869***	-0.5102***	-0.2093***	-0.1781***
	(0.0293)	(0.0415)	(0.0443)	(0.0461)	(0.0453)
Medium List Price	-0.3553***	-0.4376***	-0.3155***	-0.1349***	-0.1203***
	(0.0290)	(0.0412)	(0.0430)	(0.0396)	(0.0386)
Treated = 1 x Low List Price		0.2711***	0.2533***	0.1865***	0.1814***
		(0.0584)	(0.0570)	(0.0483)	(0.0468)
Treated = 1 x Medium List Price		0.1590***	0.1480***	0.1032**	0.1083**
		(0.0579)	(0.0564)	(0.0477)	(0.0463)
Constant	6.2446***	6.3203***	6.1423***	5.7728***	6.4342***
	(0.0243)	(0.0301)	(0.0458)	(0.0892)	(0.1862)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256	2,256
R^2	0.1819	0.1897	0.2379	0.4628	0.4999

Note: The table presents ordinary least squares regression estimates for our baseline specification and list price with list price split into 3 approximately equal quantiles (high, medium and low) (column 1), with interaction terms for treating an advert and list price quantile (column 2), Balancing Controls (column 3), controls for Property Characteristics other than list price (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes the value of the natural logarithm of the total number of the listing's click throughs during the treatment period. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

We test the proximate effect that the consumer pays more attention to the treated advert using Click Throughs as the output variable. Recall the advert is treated so that for one week it is displayed first in the SERP of similar adverts but is otherwise ranked at its natural position. We first consider the interaction of the treatment with list price, splitting the observations into three approximately equally sized quantiles for high, medium and low list price. The OLS regression estimates are shown in Table 4.8. In column 1 the natural logarithm of the sum of CTs generated during the treatment period is the dependent variable; a dummy for whether the advert is treated and a dummy for list price quantile are the independent variables. The coefficients on list price quantile dummies are 0.5499 for low list price and 0.3553 for medium list price. The coefficients are negative and statistically significant at the 5% level implying that lower priced properties are associated with 58% ($e^{-0.5499}$) of the CTs and medium priced properties 70% ($e^{-0.3553}$) of the CTs attained by high priced properties from a baseline given by the constant of 515 ($e^{6.2446}$) CTs.

Column 2 adds terms for the interaction of treating an advert and the low and medium list price dummies, the coefficients of 0.2711 and 0.1590 respectively are positive and statistically significant at the 5% level. Adding these interaction term coefficients to those for the corresponding list price dummies which remain negative and statistically significant implies that treated adverts for lower priced properties are associated with 66% ($e^{-0.6869+0.2711}$) and treated adverts for medium priced properties are associated with 76% ($e^{-0.4349+0.1590}$) of the CTs attained by treated adverts for high priced properties, increases of 8% and 6% from column 1. This indicates that the treatment has a greater impact on CTs for cheaper properties than for more expensive properties.

Columns 3, 4 and 5 successively add controls for the balancing variables, other property characteristics and advert characteristics. The coefficients of the list price price dummies and the coefficients for the interactions of treating an advert and the list price dummies remain negative and positive respectively and statistically significant at the 5% level. For column 5, with the full set of controls the interaction term coefficients for low priced and medium priced properties are 0.1814 and 0.1083 respectively. Adding these interaction term coefficients to those for the corresponding list price dummies implies that after accounting for balancing, property and advertising characteristics treated adverts for lower priced properties are associated with 100% ($e^{-0.1781+0.1814}$) and treated adverts for medium priced properties are associated with 99% ($e^{-0.1203+0.1083}$) of the CTs attained by treated adverts for high priced properties These are increases of 42% and 29% from column 1 and again indicate that treating adverts for less expensive properties has a greater impact on CTs than treating adverts for higher priced properties.

We next consider the interaction between treating an advert and the property's number of bedrooms. The OLS regression estimates are shown in Table 4.9. In column 1 the natural logarithm of the sum of CTs generated during the treatment period is the dependent variable; and dummies for whether the advert is treated and number of bedrooms (2 or fewer or 3, the comparison is 4 or 5) are the independent variables. The coefficients for number of bedrooms dummies of 0.6920 for 2 or fewer and 0.2668 for 3 bedrooms are negative and statistically significant at the 5% level. They say that properties with 2 or fewer bedrooms are associated with 50% ($e^{-0.6920}$) and 3 bedroom

Table 4.9: Treatment Effect on Click Throughs with Number of Bedrooms Interaction: OLS Estimates

	$LogClickThroughs_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	0.2775***	0.2129***	0.2158***	0.2368***	0.2411***
	(0.0225)	(0.0454)	(0.0431)	(0.0395)	(0.0382)
Two or Fewer Bedrooms	-0.6920***	-0.7519***	-0.6704***	-0.1836***	-0.1667***
	(0.0285)	(0.0399)	(0.0393)	(0.0466)	(0.0457)
Three Bedrooms	-0.2668***	-0.2843***	-0.2234***	-0.0673*	-0.0525
	(0.0301)	(0.0427)	(0.0415)	(0.0408)	(0.0398)
Treated = 1 x Two or Fewer Bedrooms		0.1219**	0.1165**	0.1057**	0.1000**
		(0.0569)	(0.0540)	(0.0494)	(0.0480)
Treated = 1 x Three Bedrooms		0.0374	0.0354	0.0391	0.0489
		(0.0602)	(0.0571)	(0.0524)	(0.0508)
Constant	6.3181***	6.3498***	6.2606***	5.8227***	6.4728***
	(0.0253)	(0.0318)	(0.0446)	(0.0879)	(0.1875)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256	2,256
R^2	0.2619	0.2637	0.3422	0.4576	0.4946

Note: The table presents ordinary least squares regression estimates for our baseline specification and controls for number of bedrooms where the comparison is 4 or 5 bedrooms (column 1), with interaction terms for treating an advert and number of bedrooms (column 2), Balancing Controls (column 3), controls for Property Characteristics other than number of bedrooms (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes the value of the natural logarithm of the total number of the listing's click throughs during the treatment period. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

properties are associated with 77% ($e^{-0.2668}$) of the CTs attained by properties with 4 or 5 bedrooms from a baseline given by the constant of 554 ($e^{6.3181}$) CTs. [17]

Column 2 adds terms for the interaction of treating an advert and the number of bedrooms dummies, the coefficient of 0.1219 for the 2 or fewer bedrooms interaction term is positive and statistically significant at the 5% level. Adding this interaction term coefficient to the coefficient for the 2 or fewer bedrooms dummy, which remains negative and statistically significant, says that treated adverts for 2 or fewer bedroomed properties are associated with 53% ($e^{-0.7519+0.1219}$) of the CTs attained by treated adverts for 4 or 5 bedroomed properties an increase of 3% from column 1. This indicates that treating adverts for properties with 2 or fewer has a greater impact on CTs than treating adverts for properties with 4 or 5 bedrooms.

Columns 3, 4 and 5 successively add controls for the balancing variables, other property characteristics and advert characteristics. The coefficient for the 2 or fewer

[17] Across the specifications 2 to 5, the 3 bedroom coefficients and associated interaction terms are consistently negative and positive respectively but not statistically significant and are not described further.

bedrooms dummy and the interaction of treating an advert and the 2 or fewer bedrooms dummy remain negative and positive respectively and statistically significant at the 5% level. For column 5 with the full set of controls the interaction term coefficient for 2 or fewer bedrooms is 0.1000. Adding this interaction term coefficient to the coefficient for the 2 or fewer bedrooms dummy implies that after accounting for balancing, property and advertising characteristics treated adverts for 2 or fewer bedroomed properties are associated with 94% ($e^{-0.1667+0.1000}$) of the CTs attained by treated adverts for properties with 4 or more bedrooms an increase of 44% from column 1. Again this indicates that treating adverts for properties with 2 or fewer bedrooms has a greater impact on CTs than treating adverts for properties with 4 or 5 bedrooms.

Table 4.10: Treatment Effect on Click Throughs with Property Type Interactions: OLS Estimates

	$LogClickThroughs_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	0.2889***	0.1981***	0.2108***	0.2241***	0.2336***
	(0.0225)	(0.0369)	(0.0345)	(0.0325)	(0.0315)
Flat/Terraced	-0.5944***	-0.6691***	-0.6132***	-0.3557***	-0.3313***
	(0.0238)	(0.0335)	(0.0318)	(0.0342)	(0.0333)
Bungalow/Unspecified	-0.2403***	-0.2858***	-0.2258***	-0.1495**	-0.1551**
	(0.0506)	(0.0707)	(0.0664)	(0.0630)	(0.0611)
Treated = 1 x Flat/Terraced		0.1499***	0.1286***	0.1165***	0.1100***
		(0.0474)	(0.0444)	(0.0417)	(0.0405)
Treated = 1 x Bungalow/Unspecified		0.0925	0.0818	0.0976	0.0884
		(0.1011)	(0.0946)	(0.0892)	(0.0866)
Constant	6.2811***	6.3258***	6.2270***	6.1477***	6.7913***
	(0.0216)	(0.0259)	(0.0393)	(0.0823)	(0.1868)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256	2,256
R^2	0.2591	0.2623	0.3590	0.4402	0.4778

Note: The table presents ordinary least squares regression estimates for our baseline specification and controls for property type where the comparison is detached and semi detached houses (column 1), with interaction terms for treating an advert and property type (column 2), Balancing Controls (column 3), controls for Property Characteristics other than property type (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes the value of the natural logarithm of the total number of the listing's click throughs during the treatment period. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

We finally consider the interaction between treating an advert and the property's type. The OLS regression estimates are shown in Table 4.10. In column 1 the natural logarithm of the sum of CTs generated during the treatment period is the dependent variable; and dummies for whether the advert is treated and property type (whether a flat/ terraced house or a bungalow/ unspecified house – the comparison is semi-detached/ detached houses) are the independent variables. The coefficients for property type dummies of 0.5944 for flat or terraced house and 0.2403 for bungalow or unspecified house are negative and statistically significant at the 5% level. They say that flats/ terraced houses are associated with 55% ($e^{-0.5944}$) and bungalow/ unspecified houses are associated with 79% ($e^{-0.2403}$) of the CTs attained by detached/ semi-detached properties from a baseline given by the constant of 534 ($e^{6.2811}$) CTs. [18]

Column 2 adds terms for the interaction of treating an advert and the property type dummies, the coefficient of 0.1497 for the flat/ terraced house interaction term is positive and statistically significant at the 5% level. Adding this interaction term coefficient to the coefficient for flats/ terraced houses, which remains negative and statistically significant, implies that treated adverts for flats or terraced houses are associated with 59% ($e^{-0.6691+0.1499}$) of the CTs attained by treated adverts for detached/ semi-detached houses an increase of 4% from column 1. This indicates that treating adverts for flats or terraced houses has a greater impact on CTs than treating adverts for detached or semi-detached properties.

Columns 3, 4 and 5 successively add controls for the balancing variables, other property characteristics and advert characteristics. The coefficient for the flats/ terraced houses dummy and the interaction of treating an advert and the flats/ terraced houses dummy remain negative and positive respectively and statistically significant at the 5% level. For column 5 with the full set of controls the interaction term coefficient for the flats/ terraced houses is 0.1100. Adding this interaction term coefficient to the coefficient for flat/ terraced houses implies that after accounting for balancing, property and advertising characteristics treated adverts for flats / terraced houses are associated with 80% ($e^{-0.3313+0.1100}$) of the CTs attained by treated adverts for detached/ semi-detached houses an increase of 25% from column 1. Again this indicates that treating adverts for flats or terraced houses has a greater impact on CTs than treating adverts for detached or semi-detached properties.

Summary. In summary we see treating adverts for properties in thick markets, so less expensive properties, properties with 2 or fewer bedrooms, flats and terraced houses, is more effective at improving the CTs they receive than properties in thin markets where consumers already pay more attention to the more desirable outcomes.

Downstream Outcomes

We examine the interaction between different property categories and treated adverts on 2 downstream outcomes which require the consumer to take action —time to sale and sales price achieved. Recall the advert is treated so that for one week it is displayed

[18] The Bungalow/ Unspecified property type is essentially a miscellaneous catch-all for the remaining properties, representing 5% of the sample. All its interaction terms across specifications 2 to 4 are consistently positive but not statistically significant and so are not discussed further.

first in the SERP of similar adverts but is otherwise ranked at its natural position. Regression estimates are presented as part of additional analysis in Appendix C.

Sale: Interaction of list price and treated adverts. Table C.11 presents Cox Proportional Hazard regression estimates. In column 1 the dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Dummies for whether the advert is treated and for list price quantile (Low or Medium – with High as the comparison) are the independent variables. The list price dummies have negative coefficients which are statistically significant at the 5% level implying that any point in time low and medium list price properties are 34% and 22% less likely to sell than high list price properties. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the list price dummies and controls for the balancing variables, other property characteristics and advert characteristics. The interaction term coefficients are consistently positive across these 4 specifications but for sales there was not a consistently statistically significant interaction at the 5% level between the effects of treating the advert and the property list price.

Sale: Interaction of number of bedrooms and treated adverts. Table C.12 presents Cox PH regression estimates. In column 1 the dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Dummies for whether the advert is treated and for number of bedrooms (2 or fewer or 3 – with 4 or 5 as the comparison) are the independent variables. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the property type dummies and controls for the balancing variables, other property characteristics and advert characteristics. The coefficients for the dummies 2 or fewer bedrooms and its interaction term are consistently negative and positive respectively and significant at the 5% level across the specifications until column 5 when neither are significant. The coefficient for the dummy 3 bedrooms is positive in specification 1, negative in specifications 2 to 5 and significant at the 5% level in specifications 3 to 5. Its value is -0.3018 at specification 5. The coefficient for the interaction term of the 3 bedroom dummy and treated advert is consistently positive and significant at the 5% level across all specifications. Its value is 0.4893 at specification 5. This indicates that after accounting for balancing, property and advertising characteristics treated adverts for 3 bedroom bedroom properties are 20% ($e^{-0.3018+0.4893}$) more likely to sell than 4 or 5 bedroom properties.

Sale: Interaction of property type and treated adverts. Table C.13 presents Cox Proportional Hazard regression estimates. In column 1 the dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Dummies for whether the advert is treated and for property type (Flat/Terraced or Bungalow/Unspecified – with Semi/Detached as the comparison) are the independent variables. The property type dummies have negative coefficients which are statistically significant at the 5% level implying that at any point in time flats/ terraced house and bungalows/ unspecified houses are 49% and 26% respectively less likely to sell than semi/ detached houses. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the property type dummies and controls for the balancing variables, other property characteristics and advert characteristics. The interaction term coefficients for treated advert and Flat/Terraced are consistently negative and for treated advert and Bungalow/Unspecified consistently positive across these 4 specifications. However for sales there were no statistically significant interactions at the 5% level between the effects of treating the advert and the property type.

Sales Price: Interaction of list price and treated adverts. Table C.14 presents OLS regression estimates. In column 1 the dependent variable is sales price as a proportion of list price. Dummies for whether the advert is treated and for list price quantile (Medium or Low, with High as the comparison) are the independent variables. The list price dummy coefficients are close to zero. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the list price dummies and controls for the balancing variables, other property characteristics and advert characteristics. The interaction term coefficients are also consistently close to zero across these 4 specifications and not statistically significant interaction at the 5% level.

Sales Price: Interaction of number of bedrooms and treated adverts. Table C.15 presents OLS regression estimates. In column 1 the dependent variable is sales price as a proportion of list price. Dummies for whether the advert is treated and for number of bedrooms (2 or fewer, or 3, with 4 or 5 as the comparison) are the independent variables. The number of bedrooms dummy coefficients are close to zero. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the number of bedrooms dummies and controls for the balancing variables, other property characteristics and advert characteristics. The interaction term coefficients are consistently close to zero across these 4 specifications and not statistically significant interaction at the 5% level.

Sales Price: Interaction of property type and treated adverts. Table C.16 presents OLS regression estimates. In column 1 the dependent variable is sales price as a proportion of list price. Dummies for whether the advert is treated and for property type (Flat/Terraced or Bungalow/Unspecified, with Semi/Detached as the comparison) are the independent variables. The property type dummy coefficients are close to zero. Columns 2 to 5 successively adds terms for the interaction of treating an advert and the property type dummies and controls for the balancing variables, other property characteristics and advert characteristics. The interaction term coefficients are consistently close to zero across these 4 specifications and not statistically significant interaction at the 5% level.

Summary

The results from the analysis of the interaction of treating an advert with its list price, number of bedrooms or property type on the proximate effect of CTs generated suggest that treating adverts for smaller, lower priced, less desirable properties has a greater positive impact on CTs generated than treating adverts for bigger, higher priced, more aspirational properties. This is likely to arise because enhancing the advert of a property that is situated in a thick market of more homogeneous real estate helps the listing stand out more than one positioned in a thin market of more distinctive, desirable real estate.

In the main study treating an advert is shown to have no positive effect on other downstream outcomes. We similarly find here that, with one exception, the interaction of treating an advert with its list price, number of bedrooms or property type is not associated with any differences in time to sale or in sales price achieved that are statistically significant at the 5% level, so these results are consistent with our main results.

Real Estate Agents' Behaviour

In this section we compare our evidence of the property categories enhancing an advert is most effective against the property categories real estate agents choose to enhance in practice. An enhanced advert exhibits the behaviour of our treatment so that for one week it is displayed first in the SERP of similar adverts but is otherwise ranked at its natural position.

For the Capital sample, Table 4.11 compares the proportion of adverts by list price, number of bedrooms and property type that were never enhanced with those that were enhanced for a week. Results from chi-squared tests demonstrate that there are significant differences between the two arms. Real estate agents are more likely to buy enhanced adverts for pricier (high price: enhanced 48%, not enhanced 31%), bigger (4 or 5 bedrooms: enhanced 26%, not enhanced 21%), more aspirational properties (detached/semi: enhanced 25%, not enhanced 23%),[19] i.e. real estate agents are more likely to buy enhanced adverts for those properties where they have a smaller effect on click throughs. We will discuss this in more detail in Section 4.6.

[19] Compared to the rest of the country the Capital has large numbers of "luxury" apartments and well-appointed terraced houses located in aspirational areas.

Table 4.11: Comparison of Property Characteristics Between Enhanced and Standard Adverts for Capital Sample

Characteristics	Never Enhanced	Enhanced for a Week	P
	N = 210011	N = 27097	
Price Band (Number); N (%)			0.00
High	65316 (31.10)	12995 (47.96)	
Medium	69456 (33.07)	9454 (34.89)	
Low	75239 (35.83)	4648 (17.15)	
Bedrooms; N (%)			0.00
2 or Fewer Bedrooms	93027 (44.30)	10917 (40.29)	
3 Bedrooms	73889 (35.18)	9089 (33.54)	
4 or More Bedrooms	43095 (20.52)	7091 (26.17)	
Property Type; N (%)			0.00
Detached/Semi	49051 (23.36)	6833 (25.22)	
Flat/Terraced	149028 (70.96)	18879 (69.67)	
Bungalow/Unspecified	11932 (5.68)	1385 (5.11)	

Note: The table displays selected property characteristic summary statistics for the Capital dataset. For *Price Band*, *Bedrooms* and *Property Type* we show the frequency and in brackets the proportion of their levels for adverts never enhanced and adverts enhanced for a week. The column P reports the *p*-value of a test for each variable that the proportion of each level does not differ between arms.

4.5 Study 2

We ask are the results from synthetic control methods that use observational data to simulate randomised control trials consistent with the results from an actual randomised control trial in the field?

4.5.1 Experiment, Data and Econometric Model

The property portal team provided data on all their adverts for residential properties located in the Capital listed for sale between 1 January 2010 and 31 December 2018. They supplied the same static and dynamic data for each advert as in our RCT. This includes when the advert was first listed, its characteristics, the property's characteristics, its real estate agent, daily click throughs received, email requests made and SSTC status.

To ensure comparability, we update this observational dataset by applying to it the same selection criteria as we applied when forming our RCT baseline sample e.g. excluding unusual sales, uncommon properties or commercial owners (see Section 4.2.2). Additionally we exclude any advert first listed before 1 January 2010, so that we have its full history. Adverts are matched to sales data from the land registry based on the portal's location identifier and then a series of tests are applied to ensure the matching is accurate.[20]

In the field study, our RCT treatment group comprises listings which for 1 week had an enhanced advert so ranking first in any applicable SERP, otherwise they had a non-enhanced advert which ranked in their natural positions. This enhancing treatment started within the first week of listing, for consistency we generate the synthetic treatment set by extracting from our updated observational dataset all listings that have an enhanced advert for 1 week only with their enhancing starting within the first week of listing. (The portal sells enhanced adverts as a 1-week block). Similarly, our RCT control group comprises listings where the advert was always non-enhanced and so always ranked in its natural position. Analogously we extract from our updated observational dataset all listings where the advert was always non-enhanced to form the synthetic control set. Combining the synthetic treatment and control sets creates the Capital sample of 237,000 adverts (27,000 treatment, 210,000 control). Summary statistics for the Capital sample, which is also used in Study 1 to examine whether real estate agents buy enhanced adverts for the properties where they are most effective, are given in Table 4.11.

[20] The rules applied are:- the sale should take place no more than 1 year after the property is first listed; if two sales transactions are matched to the listing then select the one whose sales price is closest to the list price; if two listings are matched to the same sales transaction then select the one whose last listed date is closest to the sales transaction date and finally the sales price should be no more than 20% lower or 10% higher than list price (which is consistent with the winsorised RCT results).

A synthetic control group is constructed to have similar characteristics to the treatment group to mimic a randomised control trial as closely as practical and thereby reduce bias. This can be achieved by using matching methods to create a control group and treatment group with a similar distribution of observable covariates. Matching on observable covariates also controls for those unobserved covariates that correlate with them so that the only unobservables of concern are those unrelated to the observables (Stuart, 2010). To generate the synthetic control samples we use the exact, or 1:1 matching, technique where we pair each advert from the synthetic treatment group with a counterpart from the synthetic control group if available. We create 2 samples to carry out 2 simulations by using 2 different sets of matching criteria. The sample for the first simulation (Sim 1, basic matching) is constructed to be similar to how the adverts were balanced between arms in our RCT so the matching criteria used are both adverts must have the same real estate agent and be first listed in the same month and year (in the RCT the balancing criteria are agent and week of first listing). For the second simulation sample (Sim 2, full matching) the adverts are matched by all available observable criteria relating to balancing, property characteristics and advert characteristics.[21]

Adverts are only paired once per sample and where an advert has more than one legitimate partner its pair is randomly selected from the set of partners. We exclude adverts without partners, which particularly for the Sim 1, are generally from the much larger control group. The reduction is statistical power from these exclusions is ameliorated because the size of the smaller group largely drives precision in a two-sample comparison of means (Cohen, 2013; Ho et al., 2007) and because power increases with increasing similarity of groups due to reduced extrapolation (Snedecor and Cochran, 1980). This matching approach creates a Sim 1 sample of 19,028 adverts and a Sim 2 sample of 2,878 adverts with adverts split equally between treatment and control in both samples.

Table 4.12 compares property characteristic summary statistics for the Sim 1, Sim 2 and RCT baseline samples. Properties in the Sim 1 and Sim 2 samples are located wholly in the Captial with 51% (Sim 1) and 49% (Sim 2) located in the South and West districts whereas 86% of RCT properties are located outside the Capital and only 9% located in the Capital's South and West districts. The mean list price for Sim 1 and Sim 2 properties are ¤695,000 and ¤718,000 respectively but only ¤452,000 for RCT properties reflecting lower prices outside the Capital. Across all the three samples the

[21] The Sim 2 sample matching criteria are an advert and its pair must have the same values on all the following criteria: balancing - real estate agent, month-year of first listing; property characteristics - type, number of bedrooms, price band (High, Medium, Low), location (North, East, West, South, Central) and advert characteristics - whether floor plan present, whether premium listing and number of images (High, Medium, Low).

Table 4.12: Summary Statistics: Comparing the SCM and RCT Samples

Characteristics	Sim 1	Sim 2	RCT
List Price (¤); Mean (SD)	694,903 (675,855)	717,961 (615,807)	452,009 (524,367)
Property Type; N (%)			
Bungalow	333 (1.75)	12 (0.42)	107 (4.74)
Flat / Apartment	7674 (40.33)	1282 (44.54)	719 (31.87)
House - Detached	1308 (6.87)	186 (6.46)	433 (19.19)
House - Semi-Detached	3409 (17.92)	428 (14.87)	406 (18.00)
House - Terraced	5593 (29.39)	916 (31.83)	569 (25.22)
House - Unspecified	711 (3.74)	54 (1.88)	22 (0.98)
Bedrooms (Number); N (%)			
0	227 (1.19)	8 (0.28)	10 (0.44)
1	2107 (11.07)	268 (9.31)	242 (10.73)
2	5592 (29.39)	1000 (34.75)	719 (31.87)
3	6555 (34.45)	1020 (35.44)	732 (32.45)
4	3289 (17.29)	464 (16.12)	410 (18.17)
5	1258 (6.61)	118 (4.10)	143 (6.34)
District; N (%)			
Capital: Central	3206 (16.85)	590 (20.50)	72 (3.19)
Capital: East	4419 (23.22)	714 (24.81)	26 (1.15)
Capital: North	1702 (8.94)	158 (5.49)	11 (0.49)
Capital: South	4921 (25.86)	704 (24.46)	148 (6.56)
Capital: West	4780 (25.12)	712 (24.74)	53 (2.35)
Rest of Country	0 (0.00)	0 (0.00)	1946 (86.26)

Note: The table compares property characteristic summary statistics for the Sim 1 (basic matching), Sim 2 (full matching) and RCT baseline samples. For the variable *List Price* we show the mean and in brackets the standard deviation. For the variables *Property Type*, *Bedrooms* and *District* we show the frequency and in brackets the proportion of their levels.

two most common property types are flat and terraced house representing 70% of all property types for Sim 1 and Sim 2 and 57% for the RCT and the two most common numbers of bedrooms are 2 and 3 representing 64%, 67% and 64% for Sim 1, Sim 2 and RCT respectively.

The OLS and survival econometric models we use for estimation are based on those used for the main study (see Section 4.3.1) except we now condition on month-year of the advert's first listing rather than week. It is necessary to control for the covariates used in the matching process (Athey and Imbens, 2017) so we use the specifications that include the full suite of controls for balancing, property characteristics and advert characteristics as Sim 2 matches on these. We can then compare results from Sim 1, Sim 2 and our RCT (Ho et al., 2007).

4.5.2 Results

Using samples of homes advertised for sale on a property portal we compare estimates from 2 synthetic control experiments, Sim 1 (basic matching) and Sim 2 (full matching), and a RCT in the field (RCT) of the effect of treating an advert, so that for one week it is displayed first in the SERP of similar adverts, but is otherwise ranked at its natural position. We examine this treatment effect on the proximate attention outcome of CTs generated and on a set of downstream outcomes including time to sale and sales price achieved. We employ OLS and survival analysis regression techniques.

Proximate Outcome

Table 4.13: Treatment Effect on
Click Throughs:
Comparison between SCM and RCT

	$LogClickThroughs_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.5897***	0.6626***	0.2997***
	(0.0228)	(0.0632)	(0.0190)
Constant	5.9847***	4.8882***	6.9448***
	(0.9891)	(1.6098)	(0.1980)
Observations	16,859	2,496	2,256
R^2	0.5290	0.5908	0.4960

Note: The table presents ordinary least squares regression estimates for our Sim 1 (basic matching), Sim 2 (full matching) and RCT baseline specifications with controls for Balancing, Property Characteristics and Advert Characteristics added. The dependent variable takes the value of the natural logarithm of the total number of the listing's click throughs during the treatment period. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm and zero if allocated to the control arm. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. *p<0.1; **p<0.05; ***p<0.01.

Click Throughs. Table 4.13 shows the OLS regression estimates for the relationship between the natural logarithm of the sum CTs generated during the treatment period and a dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1, Sim 2 and RCT estimates are all positive and significant at the 5% level but the values for Sim 1, Sim 2 are much higher than RCT. Potentially properties outside the Capital have less competition and garner more views.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.5897 implies that a treated advert generates 80.4% ($e^{0.5897}$) more CTs [95% CI:72.5% more, 88.6% more] than an untreated advert, the mean CTs generated during the treatment period for this sample's adverts is 184; for Sim 2 the treatment dummy coefficient of 0.6626 implies that a treated advert generates 94.0% ($e^{0.6626}$) more CTs [95% CI: 71.4% more, 119.6% more] than an untreated advert, here the mean CTs generated is 157 and finally for RCT the treatment dummy coefficient of 0.2997 implies that a treated advert generates only 34.9% ($e^{0.2997}$) more CTs [95% CI: 30.0% more, 40.1% more] than an untreated advert but the mean CTs generated during the treatment period for this sample's adverts is much higher at 433.

Downstream Outcomes

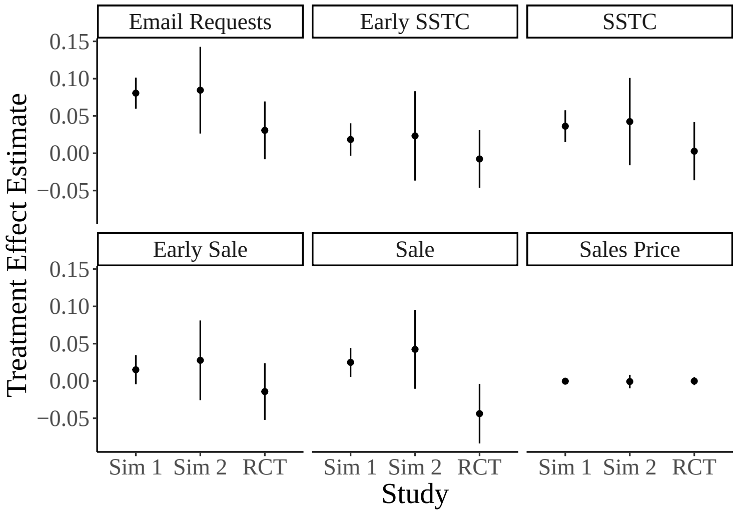
For the downstream dependent variables their estimates of the effect of the treatment after accounting for balancing, property characteristics and advertising characteristics lie between +1 and -1 (albeit the meaning of the treatment coefficient differs between OLS and Cox models and so they are not directly comparable). The estimates with their confidence intervals are illustrated in Figure 4.8 and summarised in Table 4.14. The full tables for these estimates are provided as separate panels in Table C.17.

Email Requests. Table 4.14 provides the OLS regression estimates for the relationship between the probability that an advert generates at least one email request during the treatment period and the dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1, Sim 2 and RCT estimates are all positive and for Sim 1 and Sim 2 but not RCT significant at the 5% level.

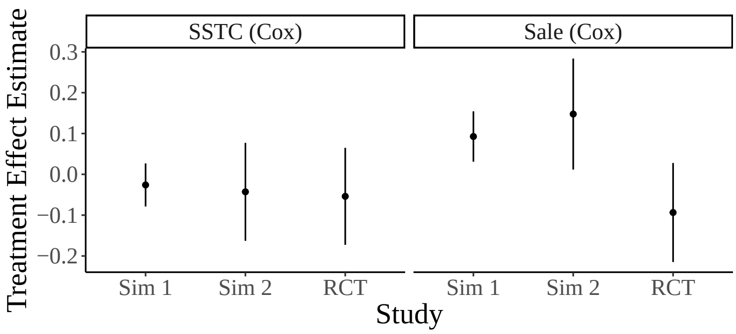
In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0806 implies that a treated advert is 8.1% more likely to generate at least 1 email request [95% CI: 6.0% more likely, 10.1% more likely] than an untreated advert, the mean probability for this sample's adverts is 33%; for Sim 2 the treatment dummy coefficient of 0.0845 implies that a treated advert is 8.5% more likely to generate at least 1 email request [95% CI: 2.6% more likely, 14.3% more likely] than an untreated advert, here the mean probability is 31% and finally for RCT the treatment dummy coefficient of 0.0307 implies that a treated advert is only 3.1% more likely to generate at least 1 email request [95% CI: 0.8% less likely, 6.9%

Figure 4.8: Treatment Effect Estimates for Downstream Outcomes Comparing SCM and the RCT

(A) Ordinary Least Squares



(B) Cox Proportional Hazard



Note: The figure shows the treatment effect point estimates and 95% confidence intervals (vertical lines) for our Sim 1, Sim 2 and RCT baseline specifications with controls for Balancing, Property Characteristics and Advert Characteristics added. The estimates are summarised in Table 4.14. Regression estimates are Panel A) Ordinary least squares and Panel B) Cox Proportional Hazard. Dependent variables are described in Table 4.5. For Sim 1 and Sim 2 observations include all listings in their baseline samples. For RCT observations include all listings in their baseline samples except the Early Sale STC, Sale STC, Sale STC (Cox) Early Sale, Sale, Sale (Cox) and Sales Price estimates do not include the 7 RCT listings located in Region 6 and the Sales Price estimate also does not include the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2)

Table 4.14: Summary of Treatment Effect Estimates Comparing SCM and the RCT for Downstream Outcomes

	Expt	OLS			Cox PH	
		Treated = 1		Mean	Treated = 1	
Email Requests	Sim 1	0.0806***	(0.0106)	0.3299		
Email Requests	Sim 2	0.0845***	(0.0296)	0.3093		
Email Requests	RCT	0.0307	(0.0198)	0.3373		
Early SSTC	Sim 1	0.0184*	(0.0111)	0.4666		
Early SSTC	Sim 2	0.0233	(0.0305)	0.4687		
Early SSTC	RCT	-0.0077	(0.0197)	0.3551		
SSTC	Sim 1	0.0362***	(0.0109)	0.5978		
SSTC	Sim 2	0.0424	(0.0298)	0.5976		
SSTC	RCT	0.0027	(0.0199)	0.6148		
SSTC (Cox)	Sim 1				-0.0260	(0.0269)
SSTC (Cox)	Sim 2				-0.0429	(0.0612)
SSTC (Cox)	RCT				-0.0539	(0.0606)
Early Sale	Sim 1	0.0150	(0.0099)	0.3231		
Early Sale	Sim 2	0.0277	(0.0272)	0.3277		
Early Sale	RCT	-0.0142	(0.0193)	0.3285		
Sale	Sim 1	0.0249**	(0.0099)	0.3746		
Sale	Sim 2	0.0424	(0.0269)	0.376		
Sale	RCT	-0.0438**	(0.0204)	0.5279		
Sale (Cox)	Sim 1				0.0927***	(0.0315)
Sale (Cox)	Sim 2				0.1477**	(0.0694)
Sale (Cox)	RCT				-0.0935	(0.0619)
Sales Price	Sim 1	-0.0003	(0.0015)	0.9803		
Sales Price	Sim 2	-0.0008	(0.0046)	0.9814		
Sales Price	RCT	-0.0001	(0.0027)	0.9628		

Note: The table summarises the regression estimates for Sim 1, Sim 2 and RCT for our baseline specifications with Balancing, Property Characteristics and Advert Characteristics added which are presented in Table C.17. Regression estimates are OLS with the mean for the dependent variable also provided, unless stated as Cox (in which case they are Cox Proportional Hazard regression estimates of our baseline survival mode). Dependent variables are described in Table 4.5. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm or otherwise 0. For Sim 1 and Sim 2 observations include all listings in their baseline samples. For RCT observations include all listings in their baseline samples except the Early Sale STC, Sale STC, Sale STC (Cox) Early Sale, Sale, Sale (Cox) and Sales Price estimates do not include the 7 RCT listings located in Region 6 and the Sales Price estimate also does not include the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2) *p<0.1; **p<0.05; ***p<0.01.

more likely] than an untreated advert and here the mean probability that an advert generates at least 1 email request during the treatment period is 34%.

Early SSTC. Table 4.14 provides the OLS regression estimates for the relationship between the probability that the property an advert is marketing sells STC up to 45 days from its first listing and the dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1 and Sim 2 estimates are positive but the RCT coefficient estimate is negative. No coefficient is significant at the 5% level.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0184 implies that a property with a treated advert is 1.8% more likely to sell STC early [95% CI: 0.3% less likely, 4.0% more likely] than a property with an untreated advert, the mean probability of early sale STC for this sample's adverts is 47%; for Sim 2 the treatment dummy coefficient of 0.0233 implies that a property with a treated advert is 2.3% more likely to sell STC early [95% CI: 3.7% less likely, 8.3% more likely] than an untreated advert, here the mean probability is also 47% and finally for RCT the treatment dummy coefficient of -0.0077 implies that a property with a treated advert is 0.8% less likely to sell STC early [95% CI: 4.6% less likely, 3.1% more likely] than a property with an untreated advert and here the mean probability that an a property sells STC early is only 36%.

SSTC. Table 4.14 provides the OLS regression estimates for the relationship between the probability that the property an advert is marketing sells STC up to 170 days from its first listing and the dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1, Sim 2 and RCT estimates are all positive. Only the Sim 1 treatment dummy coefficient is significant at the 5% level.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0362 implies that a property with a treated advert is 3.6% more likely to sell STC [95% CI: 1.5% more likely, 5.8% more likely] than a property with an untreated advert, the mean probability of sale STC for this sample's adverts is 60%; for Sim 2 the treatment dummy coefficient of 0.0424 implies that a property with a treated advert is 4.2% more likely to sell STC [95% CI: 1.6% less likely, 10.1% more likely] than an untreated advert, here the mean probability is also 60% and finally for RCT the treatment dummy coefficient of 0.0027 implies that a property with a treated advert is 0.3% less likely to sell STC [95% CI: 3.6% less likely, 4.2% more likely] than a property with an untreated advert and here the mean probability that an a property sells STC is similarly 61%.

SSTC (Cox). Table 4.14 provides the estimate for a Cox PH model where we estimate the size of the effect that treating an advert has on the owner selling their property STC at time t conditional on not selling their property STC until time t for Sim 1, Sim 2 and RCT. The endpoint is 170 days of the final property to be listed is first listed. Observations include all listings in our Sim 1, Sim 2 and RCT baseline samples. The model uses the specification given in Equation 4.2 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1, Sim 2 and RCT estimates are all negative and not significant at the 5% level.

In more detail, at a given instance in time and after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of -0.0260 implies that a property with a treated advert is 2.6% ($e^{-0.0260}$) less likely to sell STC [95% CI: 7.6% less likely, 2.7% more likely] than a property with an untreated advert; for Sim 2 the treatment dummy coefficient of -0.0429 implies that a property with a treated advert is 4.2% ($e^{-0.0429}$) less likely to sell STC [95% CI: 15% less likely, 8% more likely] than a property with an untreated advert and finally for RCT the treatment dummy coefficient of -0.0539 implies that a property with a treated advert is 5.2% ($e^{-0.0539}$) less likely to sell STC [95% CI: 15.9% less likely, 6.7% more likely] than a property with an untreated advert.

Early Sale. Table 4.14 provides the OLS regression estimates for the relationship between the probability that the property an advert is marketing sells up to 190 days from its first listing and the dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1 and Sim 2 baseline samples. For the RCT sample see Footnote 15. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1 and Sim 2 estimates are positive but the RCT coefficient estimate is negative. No coefficient is significant at the 5% level.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0150 implies that a property with a treated advert is 1.5% more likely to sell early [95% CI: 0.4% less likely, 3.4% more likely] than a property with an untreated advert, the mean probability of early sale for this sample's adverts is 32%; for Sim 2 the treatment dummy coefficient of 0.0277 implies that a property with a treated advert is 2.8% more likely to sell early [95% CI: 2.6% less likely, 8.1% more likely] than an untreated advert, here the mean probability is similarly 33% and finally for RCT the treatment dummy coefficient of -0.0142 implies that a property with a treated advert is 1.4% less likely to sell early [95% CI: 5.2% less likely, 2.4% more likely] than a property with an untreated advert and here the mean probability that an a property sells early is also 33%.

Sale. Table 4.14 provides the OLS regression estimates for the relationship between the probability that the property an advert is marketing sells STC up to 280 days from its first listing and the dummy for whether the advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1 and Sim 2 baseline samples. For the RCT sample see Footnote 15. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1 and Sim 2 estimates are positive but the RCT coefficient estimate is negative. Both the Sim 1 and RCT, but not Sim 2, treatment dummy coefficients are significant at the 5% level.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0249 implies that a property with a treated advert is 2.5% more likely to sell [95% CI: 0.6% more likely, 4.4% more likely] than a property with an untreated advert, the mean probability of sale for this sample's adverts is 37%; for Sim 2 the treatment dummy coefficient of 0.0424 implies that a property with a treated advert is 4.2% more likely to sell [95% CI: 1% less likely, 9.5% more likely] than an untreated advert, here the mean probability is 38% and finally for RCT the treatment dummy coefficient of -0.0438 implies that a property with a treated advert is 4.4% less likely sell [95% CI: 8.4% less likely, 0.4% less likely] than a property with an untreated advert and here the mean probability that an a property sells is similarly 53%.

Sale (Cox). Table 4.14 provides the estimate for a Cox PH model where we estimate the size of the effect that treating an advert has on the owner selling their property at time t conditional on not selling their property until time t for Sim 1, Sim 2 and RCT. The endpoint is 280 days of the final property to be listed is first listed. Observations include all listings in our Sim 1 and Sim 2 baseline samples. For the RCT sample see Footnote 15. The model uses the specification given in Equation 4.2 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1 and Sim 2 estimates are positive but the RCT coefficient estimate is negative. Both the Sim 1 and Sim 2, but not RCT, treatment dummy coefficients are significant at the 5% level.

In more detail, at a given instance in time and after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of 0.0927 implies that a property with a treated advert is 9.7% ($e^{0.0927}$) more likely to sell [95% CI: 3.1% more likely, 16.7% more likely] than a property with an untreated advert; for Sim 2 the treatment dummy coefficient of 0.1477 implies that a property with a treated advert is 15.9% ($e^{0.1477}$) more likely to sell [95% CI: 1.2% more likely, 32.8% more likely] than a property with an untreated advert and finally for RCT the treatment dummy coefficient of-0.0935 implies that a property with a treated advert is 8.9% ($e^{-0.0935}$) less likely to sell [95% CI: 19.3% less likely, 2.8% more likely] than a property with an untreated advert.

Sales Price. Table 4.14 provides the OLS regression estimates for the relationship between the sales price of an advertised property as a proportion of its original list price and the dummy for whether its advert is treated for Sim 1, Sim 2 and RCT. Observations include all listings in our Sim 1 and Sim 2 baseline samples. For the RCT sample see Footnote 16. The relationship is specified by Equation 4.1 with the addition of controls for balancing, property characteristics and advert characteristics. The coefficient on the treatment dummies for the Sim 1, Sim 2 and RCT estimates are all negative and very close to zero. No treatment dummy coefficients are significant at the 5% level.

In more detail, after accounting for balancing, property and advertising characteristics: for Sim 1 the treatment dummy coefficient of -0.0003 implies that the sales price as a proportion of original list price of a property with a treated advert is 0.0% lower [95% CI: 0.3% lower, 0.3% higher] than a property with an untreated advert, the mean sales price as a proportion of original list price is 98%; for Sim 2 the treatment dummy coefficient of -0.0008 implies that the sales price as a proportion of original list price of a property with a treated advert is -0.1% lower [95% CI: 1% lower, 0.8% higher] than an untreated advert, here the mean sales price proportion is also 98% and finally for RCT the treatment dummy coefficient of -0.0001 implies that that the sales price as a proportion of original list price of a property with a treated advert is 0.0% lower [95% CI: -0.5% lower, 0.5% higher] than a property with an untreated advert and here the mean sales price as a proportion of original list price is 96%.

Summary

Table 4.15 recaps the consistency in estimates of the treatment for the pairs Sim 1 & Sim 2; Sim 1 & RCT and Sim 2 & RCT. It summarises whether the higher powered but less well-matched Sim 1 sample gives the same results as Sim 2 and more importantly whether the same conclusions would have been drawn regardless of whether the main study had been conducted by a SCM experiment or the field study. In each pair and for each outcome we consider whether there is an overlap in the range in the treatment dummy coefficient estimates (i.e. the confidence intervals) and whether both estimates are either statistically significant or not statistically significant at the 5% level.

First, comparing the consistency of the Sim 1 & Sim 2 pair of results, although their range in estimates always overlap on two occasions Sim 1 sample are statistically significant at the 5% level but those for Sim 2 are not. Next, comparing the consistency of the SCM experiments and the field study, out of the ten different outcomes the Sim 1 & RCT pair of results are consistent with each other on both measures four times

Table 4.15: Summary of Consistency of SCM and RCT Estimates

	Sim 1—Sim 2		Sim 1—RCT		Sim 2—RCT	
	Est. Range	Sig. Level	Est. Range	Sig. Level	Est. Range	Sig. Level
Click Throughs	Yes	Yes	No	Yes	No	Yes
Email Requests	Yes	Yes	Yes	No	Yes	No
Early SSTC	Yes	Yes	Yes	Yes	Yes	Yes
SSTC	Yes	No	Yes	No	Yes	Yes
SSTC (Cox)	Yes	Yes	Yes	Yes	Yes	Yes
Early Sale	Yes	Yes	Yes	Yes	Yes	Yes
Sale	Yes	No	No	Yes	Yes	No
Sale (Cox)	Yes	Yes	Yes	No	Yes	No
Sales Price	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table summarises the consistency in estimates for treatment dummy coefficients in the pairs Sim 1 & RCT; Sim 2 & RCT and Sim 1 & Sim 2. Est. Range is Yes if the range in estimates for the pair's members overlap. Sig. Level is Yes if both estimates are statistically significant or both not statistically significant at the 5% level.

and the Sim 2 & RCT pair consistent five times. This indicates that the SCM and field study methods do not demonstrate convergent validity or equivalently, taking the RCT results as the gold standard, the synthetic control methods do not give consistently accurate results.

4.6 Discussion and Conclusion

Our main study tests the immediate, proximate effects versus the medium-term, broad effects of a nudge commonly used by advertisers in e-commerce sites such as Alibaba and Google. We apply this in the context of property listings on an online real estate comparison website. The nudge elevates the listing's ranking from its natural position to top ranked position By carrying out a RCT in the field, we find a large effect on the proximate outcome of attracting Sig the consumers' attention, with the CTs of nudged adverts by over 30%, but it has no economically significant effect on the sale price achieved. Further evidence suggests this nudge generates economically inefficient congestion, slowing time taken to achieve the sale. This suggests that the nudge has an influence on the consumer's attention but that does not correlate with the consumer actually taking the action of buying the property.

Descriptive results from our extensions first suggest that for the same proximate outcome, measuring gaining consumers' attention, our nudge has a stronger effect in thicker markets where the properties are lower-priced and higher-volume than thinner markets with higher-priced but lower-volume property types. Potentially being first helps the less distinctive thicker market properties stand-out in a crowded market but has a lower impact with the less common and therefore already more distinctive thinner market properties. This stronger effect on attention is still not however associated with people paying more for the thicker market properties.

Second, descriptively, we see real estate agents tend to apply the nudge, by buying enhanced adverts, to market properties in thinner markets. This suggests that they are using the nudge in a manner contrary to the evidence on where it works best. Enhanced adverts are more expensive than standard adverts and so a real estate agent may be more prepared to bear this additional marketing cost to attract or retain more valuable clients who will not know the nudge is not actually economically efficacious.

We can speculate why the nudge with its positive immediate effect does not have a positive medium-term effect. Context matters. Nudges are found to have the smallest effect in the financial domain and it is suggested people may be less influenced by biases and heuristics and therefore nudges in choices which have a substantial impact on their lives. Nudges are also found to more effective in domains where people form habits where potentially they can cause highly automatised behavioural responses (Mertens et al., 2022). For many individuals buying a property is both among the most important and uncommon financial transactions that they undertake.

It is claimed digital adverts inform consumers of product features but do not persuade them to change preferences (Blake et al., 2015) and it is further argued that informational nudges such as calorie menu labelling may operate by evoking negative emotions such as anxiety or guilt and acting as 'emotional taxes' (Thunström, 2019). With a property portal the provision of information by clicking through to the full property description is certainly not to intended to evoke negative emotions about the listing. Agarwal et al. (2011) finds that information seekers as opposed to buying consumers were less likely to visit lower ranked listings. Potentially non-serious property buyers are clicking what stands out for entertainment whereas the serious buyers are thoroughly checking everything that meets their requirements.

The nudge could be having other effects which differ according to context; so for one property the nudge helps to locate the one interested buyer and so the sale happens more quickly but for another property it facilitates competition between potential buyers so the sales process takes longer. One seller may secure a higher price for their property but another one becomes overconfident and does not sell at all. The nudge's negative influence on sales may result from increased competition slowing down the sales process or causing buyers to become overconfident and not accept reasonable offers.

Further development of this research would explore why the nudge is effective for proximate outcomes but not for downstream outcomes. The former would be informed by an understanding of the mechanism by which the first placed advert causes a consumer to click through. For the latter more data on the customer journey e.g. the number of viewings that are set-up, the number and size of offers to buy made but not accepted would provide greater clarity on the point where the nudge stops being effective and insight into the cause.

Our second study found that using the exact marching synthetic control method did not give consistently accurate results when compared to a RCT. A researcher could draw different conclusions depending on the method chosen. Our findings support those by Gordon et al. (2019) that estimates based on observed data are not necessarily accurate. Exact matching controls for the matched covariates and for the unobservable covariates which correlate with them but not the unobservable covariates which do not. Accordingly, the differences between results may arise from omitted variable bias in the synthetic control method estimates.

However, we only test one synthetic control method, potentially a different method would lead to more accurate results e.g. with propensity score matching rather than the requirement for close matches for each participant on all individual variables, matches are pooled so that the control and treatment arms are created with similar overall distributions of the covariates leading to larger samples and greater statistical power (Ho et al., 2007; Stuart, 2010).

Although our data came from the same third party, the samples used by the SCMs and RCT are not the same which could also account for the conflicting results. The time periods over which the data was collected are different and whilst properties in the RCT sample are located across the entire country the properties in the SCM samples are only located in the capital. Property prices are higher in the capital which also has a greater proportion of flats and terraced houses. Additionally the requirement to match properties could skew the SCM samples to the most common property types. However our estimates did control for when listed and property characteristics and our findings were similar regardless of whether the matching was basic (2 matching covariates) or full (9 matching covariates).

As synthetic methods were seen as an evolution in difference-in-differences estimation, we would encourage researchers who carry out a RCT, to where possible, also carry out a replication using synthetic control methods to improve understanding of the limitations of this growing method and motivate its further improvement.

Our study suggests that behavioural research needs to look beyond the immediate impact of a nudge (on, for example, attention) to the downstream economic impact, where effects may be different to those intended or observed when looking only at proximate, near-term outcomes. Moreover it serves as a corrective to the philosophy that low-cost, easy interventions will always be sufficient to effect the the desired behaviour change. More specifically it supports the marketing literature that claims a highly placed digital advert does not necessarily increase sales. Our study also supports the contention that estimates based on observed data are not necessarily accurate and provides an example of the need for researchers when applying novel methods to question and substantiate their applicability in different contexts.

5 A Re-evaluation of a Price Precision Effect Study

5.1 Introduction

Buying and selling a property is, for most homeowners, the biggest negotiation that they lead. Agreeing the price is critical. Research by Thomas et al. (2010) posits the *price precision effect*, in which buyers pay more for goods when sellers market them with precise prices. Their article comprises five studies; four are randomised control trials and the fifth, to test external validity, examines whether this precision effect is observed in actual real estate transactions. We attempt to replicate the findings of their fifth study using their two original U.S. datasets and a private European dataset ('Capital') which incorporates twenty times more transactions.

In this fifth study, Thomas et al. (2010) examine transactions where a property's list price is greater than its ultimate sales price. Here our findings are broadly consistent, that a property with a precise list price is associated with a higher selling price (i.e., a sale price closer to, though still below the list price) than a property with a rounded list price. We find smaller but consistent effect sizes in the Capital dataset e.g a property listed at ¤500,000 would achieve a sales price almost ¤1,000 lower than one listed at ¤499,995.[1]

More striking is our finding that the precision effect reverses if transactions where the property's list price is less than its sale price are examined instead. (Thomas et al. (2010) do not separately analyse this case). Now, more precise list prices are associated with lower sales prices. This result is consistent across the three datasets, and for the Capital dataset, the effect is at least eight times larger than the original effect, with a property listed at ¤500,000 associated with a sales price ¤8,250 higher than one listed at ¤499,995.

The precision or roundedness of a price is found to influence behaviour. For example, people neglect a price's right-hand digits and instead focus on the left-hand side. Thus they perceive a 9-ending price to be much smaller than a price 1 cent higher (Thomas and Morwitz, 2005). Studies show how these 'charm prices' (prices set just below a round number and so more precise) increase sales revenue on different types

[1] We use the generic currency symbol '¤' to obfuscate the originating country.

of consumer goods (Schindler and Kibarian, 1996; Gendall et al., 1997). In real estate, homes marketed in Fort Lauderdale, Florida at certain charm prices were found to exhibit significantly smaller gaps between sales and list price and so sell for much more than those marketed at round number prices (Allen and Dare, 2004a,b). Conversely, contradicting Thomas et al. (2010) an analysis of sales transactions for homes in Houston, Texas found houses marketed at charm prices sold more slowly and at a lower price than those at rounded prices (Palmon et al., 2004). An analysis of the distribution of the sale prices for U.S. homes demonstrated round numbers can act as reference points in high-stakes negotiations (Pope et al., 2015), with the authors suggesting that individuals do not value homes precisely and are indifferent to a range of possible values so frequently choose round numbers because of their salience. Generally people find rounded numbers are easier to process and create a positive subjective experience of "feeling right" (Wadhwa and Zhang, 2015).

For our study, a world leading online real estate comparison website (property portal) provided to us a private dataset of all residential properties located in a major European capital listed for sale on their website between 2010 and 2018. This listings data was matched to publicly available sales transaction data; an administrative dataset issued by the Country's land registry (the government agency with legal responsibility to collect and publish this information). This gave 235,000 sales, a sample size twenty-five times larger than the largest single sample used in the Thomas et al. (2010) study. Additionally, Thomas et al. generously provided the two real estate transaction datasets that they had used in their study. These were properties listed for sale in South Florida (SFL) and in Long Island, New York (LI). Their data came from the local Multiple Listing Service, which act as clearing houses for real estate agents listing properties for sale. Using different measures of precision, we carry out separate ordinary least squares (OLS) regression analysis to estimate the precision effect for the three samples. Our econometric specification includes a set of controls to account for property, agent, time, and local market. We compare our results to those of Thomas et al. (2010).

The remainder of the paper proceeds as follows: Section 5.2 describes the data, methodology, econometric specification and the original study's results; Section 5.3 presents our results and Section 5.4 discusses the results and concludes.

5.2 Data and Analysis

Research by Thomas et al. (2010) found the precision effect. Do we replicate the same results using a novel European real estate transaction dataset, as well as their two original datasets? Section 5.2.1 describes the data and how the sample is selected. Section 5.2.2 describes the measures of precision, Section 5.2.3 describes the econometric specification used for analysis and Section 5.2.4 summarises the relevant results from the original study, ahead of our analysis in Section 5.3.

5.2.1 Data and Sample Selection

Our novel listings data are provided by one of the world's largest property portals. It comprises daily data relating to all adverts for residential properties located in the Capital, listed for sale on their website between 1 January 2010 to 31 December 2018.

Since we are interested in adverts for routine sales of ordinary residential properties by private individuals between we exclude adverts for special sales, unusual properties and commercial owners. Properties must be available for purchase and not put up for auction. Properties must be categorised as flats or houses and have no more than 5 bedrooms. Houses must have at least 1 bedroom, but flats with 0 bedrooms are permitted. Part-businesses, retirement properties and properties sold by developers are excluded. We exclude any advert first listed before 1 January 2010 to ensure we have its full history, any advert that does not pass the portal's data quality checks or specify an asking price. To be able to match listings to sales data we exclude listings without the portal's bespoke address identifier. Finally we are interested in cases where sales price is either greater or less than sales price so we exclude cases where sales price equals list price.

To create the Capital sample, we match our listings data to land registry sales data by the address identifier. We apply a series of tests to ensure the matching is accurate. A property's sales price is no more than 20% lower or 10% higher than its list price (the percentages are consistent with a separate dataset of matched list and sales prices from the same sources see Section 4.2.1); the sale can take place up to 1 year after the property is first listed; if two sales transactions are matched to the listing then the sales price closest in value to the list price is selected; if two listings are matched to the same sales transaction then the one whose last list date is closest to the sales transaction date is selected.

Summary statistics for the Capital dataset are provided in Table 5.1. Both list and sales price are skewed right with means of ¤498,000 and ¤480,000 medians of ¤380,000 and ¤370,000 respectively. 67% of the properties are either flats or terraced houses and 70% with either 2 or 3 bedrooms.

Table 5.1: Summary Statistics: Capital Sample

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
List Price (¤)	498054	460614	210000	275000	380000	560000	850000
Sales Price (¤)	479657	435208	203000	263998	370000	542500	825682
.							
	%						
Property Type							
Flat	32.0						
Detached	6.0						
Semi-Detached	20.8						
Terraced	34.8						
Bungalow	2.7						
Unspecified	3.7						
Bedrooms							
0	0.7						
1	9.4						
2	30.0						
3	40.4						
4	14.6						
5	4.9						
.							
	N						
Properties	235447						

Note: The table presents summary statistics for the properties for which we create our Capital baseline sample at the property level.

Additionally, the authors of the Thomas et al. (2010) article provided to us the SFL and LI real estate transactions data used in their study. Their article only presents results where list price is greater than sales price. It argues that there are probably multiple bidders in transactions where the sales price is greater than the list price, so buyers will be more influenced by their rivals' bids than the precision of the list price. The authors provided their raw datasets but no information, in the article or separately, about the exclusion criteria applied prior to analysis. We exclude properties with missing information, data errors or where sales price is more than 20% lower or 10% higher than its list price which leaves 15,595 SFL and 13,450 LI properties where list price is greater than sales price. These numbers of properties are both higher than the numbers regressed in Thomas et al.'s analysis of 7,342 and 9,026 properties respectively. We have been unable to resolve this discrepancy.

As well as sales and list price the three samples all include additional information about each property. The Capital sample includes information on when the property is listed, property type, number of bedrooms, location and real estate agent. The SFL and LI samples both contain more comprehensive information about their listings. The LI dataset is the most detailed; including information on lot size, local property tax as well as granular details on each property's amenities such as the number of dishwashers and stoves. Thomas et al. (2010) do not provide any information on how this additional property data was wrangled to achieve consistency prior to analysis (e.g. for a criteria such as 'Garages' the real estate agent can provide either a numeric or a yes/no response); we describe how we used this data to condition the model in Section 5.2.3

5.2.2 Precision

Thomas et al. (2010) measure price precision in their SFL and LI samples using two approaches which followed Dehaene and Mehler (1992): *Precise000* (hereafter T1) and *Number of Ending Zeroes* (hereafter T2). These approaches are based on the highest positive integer power of 10 that the list price is a multiple of (i.e. 10, 100, 1000 etc.) so that 499550, a multiple of 10, has 1 ending zero and 499000, a multiple of 1000, has 3 ending zeroes. A survey-based approach is also used to measure price precision for the SFL sample. Its delineation of precision is strongly correlated with the other measures. In this study we use methods T1 and T2 and for the Capital dataset a third new approach (hereafter N) which modifies T2:

T1: treats list prices with 2 or fewer ending zeroes as precise and 3 or more ending zeroes as rounded. By this measure, for example 89,900 with 2 ending zeroes is precise and 89,000 with 3 ending zeroes is rounded.

T2: counts how many ending zeroes (Z) the list price has, e.g. Z equals 0 for 89,999.

N: also counts the list price's number of ending zeroes but rather than treating Z as a continuous variable, and thereby assuming a linear relationship, it instead codes the number of ending zeroes in a vector of dummies (No 0s, Ends0, Ends00, Ends000, etc.).

Table 5.2: Comparison of List Price Precision Between Samples

Measure	Capital	South Florida	Long Island
T1: Precise000 (Proportion)	0.27	0.33	0.16
T2: Ending zeroes (Mean)	2.9	2.8	2.8
New: Ending zeroes (Proportion):-			
No 0s	0.08	0.03	0.06
Ends0	0.18	0.01	0.04
Ends00	0.01	0.28	0.05
Ends000	0.35	0.48	0.75
Ends0000	0.28	0.19	0.08
Ends00000	0.09	0.01	0.01

Note: The table compares the precision of list prices by each measure for the Capital, South Florida and Long Island samples.

Table 5.2 compares the precision of list prices by these three measures for the three samples. The proportion of the list prices that are precise under measure T1 ranges from 0.16 for LI to more than double at 0.33 for SFL. The mean number of ending zeroes at just under 3 is similar across the three samples, however there are differences in their distribution. Three ending zeroes is the mode for all samples but at 0.75 the proportion for LI is more than twice as high as Capital. For SFL, two and four ending zeroes are also common whereas for Capital two ending zeroes are rare but one and four ending zeroes are common.

5.2.3 Econometric Model

We measure the effect of each of our three measures of list price precision on the sale price paid by the buyer using the below model. Following Thomas et al. (2010) we estimate using samples where sales price is less than list price. Additionally, and separately, we estimate using samples where sales price is more than list price. The unit of observation is the individual property *i*:

$$DepVar_i = b_0 + b_1 PrecisionMeasure_i + \sum_{j=1}^{n} c_j Controls_i + \epsilon_i, \tag{5.1}$$

DepVar is the dependent variable ln(sale price). *PrecisionMeasure* is one of the three precision measures T1, T2 or N described in Section 5.2.2.

Controls are a battery of controls that we add to account for other factors that may correlate with list price and precision. Thomas et al. (2010) discuss the correlation between the list price and its precision which arises because first, buyers often anchor their bids to list price (Northcraft and Neale, 1987) and second, large precise numbers are uncommon with sellers tending to round off higher prices. They account for this correlation by adding a term for log list price to the right-hand side of their specification equation and so we follow them.

Thomas et al. (2010) also discuss adding controls that fall into four categories property, agent, time, and (local) market but do not itemise the controls that they include. To fit with their broad outline for the Capital sample we include controls for numbers of bedrooms and log days listed and dummies for property type, real estate agent, month and year of listing and administrative district. As noted above the SFL and LI samples, which the authors analyse separately, contain different information about their listings. For SFL we control for year built, property type, district, numbers of bedrooms, bathrooms and garages and whether a pool is present. For LI we additionally control for year sold, lot size, property tax rate, whether direct negotiation and presence of amenities including den, dishwasher, dryer, stove, washer, refrigerator and carpets.

b and c denote coefficients. ϵ_i is the error term.

5.2.4 Original Study Results

Thomas et al. (2010)'s analysis, where list price is greater than sales price, is shown in Figure 5.1. The estimates show that in SFL for precision measure T1 (Precise000) list prices with fewer than three ending zeroes increase the sales price by approximately 0.2% and for T2 each extra ending zero reduces the sale price by approximately 0.09%. Both estimates are significant at the 5% level. Similar results are seen in LI, with larger effect sizes although only the T2 precision measure estimate is significant at the 5% level.

For both datasets the equivalent analysis is carried out on the full sample of observations (i.e. also including list prices less than sales price), but these results are not presented. Instead Thomas et al. (2010) comment that "the precision of the list price continues to have the predicted, although smaller, effect on the final sale price." (p 187).

5.3 Results

We present OLS estimates of the precision effect, using the three different measures of precision, which we compare to those in a study by Thomas et al. (2010).

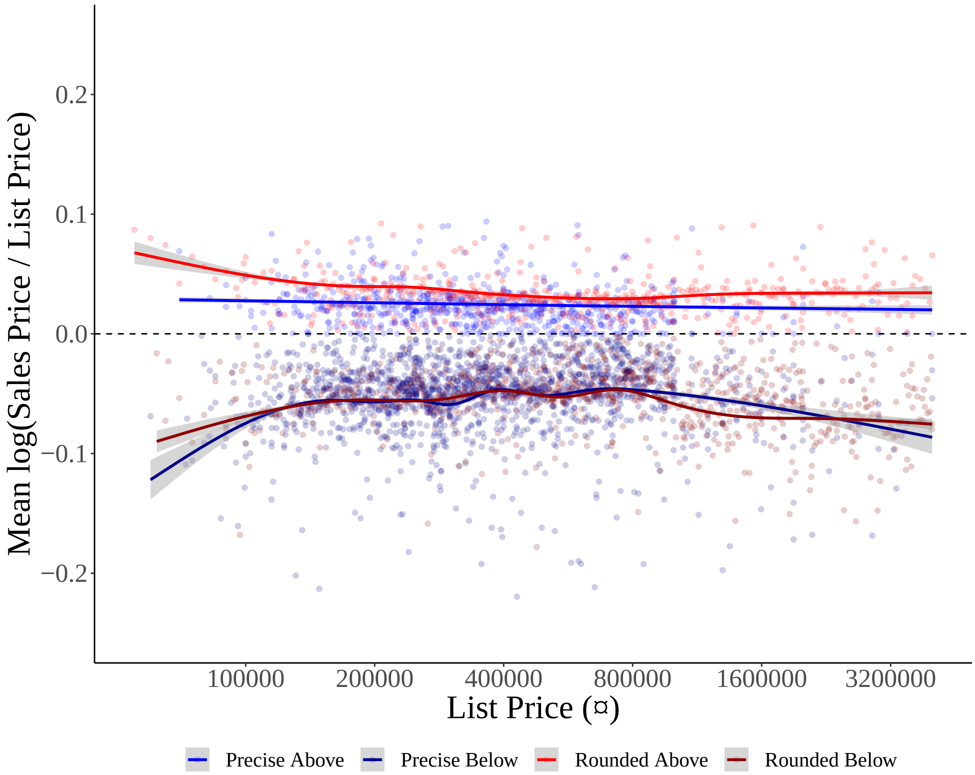
Figure 5.2 is a bin scatter plot showing for the Capital sample the mean of ln(sale price/ list price) for each list price (so dividing Equation 5.1 by log list price). Its four trend lines are smoothed conditional means. They denote whether list price is precise or rounded according to definition T1 (a price is precise if two or fewer ending

Figure 5.1: "Effect of List Price Precision on Final Sale Price: Controlling for Other Factors" (Thomas et al., 2010, p. 187)

Dependent variable: ln(sale price)	South Florida	South Florida	South Florida	Long Island	Long Island
Precise000 (fewer than three ending zeroes)	0.0023** (0.0005)			0.0028+ (0.0015)	
Number of ending zeroes		−0.0009** (0.0002)			−0.0017* (0.0007)
Precise (survey-based measure)			0.0058** (0.0010)		
ln(list price)	0.96** (0.00)	0.96** (0.00)	0.96** (0.00)	0.95** (0.01)	0.95** (0.01)
Within *R*-squared	0.99	0.99	0.99	0.98	0.98
N	7,342	7,342	7,341	9,026	9,026

Notes. All models include control variables described in §7.2 (house characteristics, selling agent commission, agent, zip code, and year fixed effects, etc.). Standard errors, clustered by agent (i.e., we allow for arbitrary correlations in error structure for each agent), are reported in parentheses.
+Significant at $p < 0.10$; *significant at $p < 0.05$; **significant at $p < 0.01$.

Figure 5.2: Bin Scatter Plot of Sales Price and List Price With T1 Precision Measure



Note: The figure shows a scatter plot of the mean log sales price / list price by list price. The trend lines are smoothed conditional means denoting whether list price is precise or rounded according to definition T1 and whether sale price is less than ('Below') or greater than ('Above') list price. The x-axis is truncated at ¤4,000,000.

zeroes) and whether the sales price is less than ('Below') or greater than ('Above') its list price. Where the sales price is less than the list price the precise and rounded trend lines overlap and cross each other suggesting no strong effect. However when the sales price is greater than the list price the rounded trend line is consistently higher than the precise trend line, suggesting rounded list prices are associated with higher sales prices than precise ones.

This result is confirmed by the OLS regression estimates for Equation 5.1 shown in Table 5.3. Considering first where sales price is less than list price (the specifications labelled 'Less'), for the T1 precision definition the coefficient of the precision measure of 0.0002 is positive suggesting a precise list price is associated with a higher sales price than a rounded list price but the effect size is near zero and not significant at the 5% level, after controlling for property, agent, time, and market. In the original study, the estimate of the equivalent precision measure for SFL was more than ten times bigger, although the LI estimate was similarly not significant at the 5% level.

Table 5.3: Precision Effect Estimates for Capital Sample: OLS Estimates

	$LnSalesPrice_i$					
	T1 Less	T2 Less	N Less	T1 Greater	T2 Greater	N Greater
Precise000	0.0002			-0.0093***		
	(0.0004)			(0.0006)		
No. of ending zeroes		-0.0004***			0.0033***	
		(0.0001)			(0.0001)	
Ends 0			0.0009			0.0046***
			(0.0006)			(0.0009)
Ends 00			0.0064***			0.0032***
			(0.0009)			(0.0012)
Ends 000			0.0015***			0.0103***
			(0.0005)			(0.0006)
Ends 0000			0.0003			0.0133***
			(0.0006)			(0.0007)
Ends 00000			-0.0042***			0.0173***
			(0.0007)			(0.0008)
Ends 000000			0.0015			0.0304***
			(0.0020)			(0.0020)
Ln(List Price)	0.9953***	0.9955***	0.9957***	0.9912***	0.9909***	0.9907***
	(0.0008)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0009)
Constant	0.0289***	0.0277***	0.0226**	0.1480***	0.1404***	0.1424***
	(0.0100)	(0.0100)	(0.0098)	(0.0106)	(0.0102)	(0.0099)
Observations	184,821	184,821	184,821	50,626	50,626	50,626
R^2	0.9956	0.9956	0.9956	0.9982	0.9982	0.9982

Note: The table presents ordinary least squares regression estimates of the precision effect for our baseline specification which includes controls for property, agent, time, and market. The dependent variable takes the value of log Sales Price. The T1, T2 and N specifications incorporate different precision measures, described in Section 5.2.2, as dependent variables. For specifications labelled 'Less' the sales price is less than the list price and for specifications labelled 'Greater' the sales price is greater than the list price. *p<0.1; **p<0.05; ***p<0.01.

For the T2 precision definition, the coefficient of the precision measure of 0.0004 is negative and significant at the 5% level, after controlling for property, agent, time, and market. It says that for each ending zero on the list price than sales price is approximately 0.04% ($e^{-0.0004}$) less. So a property listed at ¤500,000 would achieve a sales price almost ¤1,000 lower (0.04% of 500,000 x 5) than one listed at ¤499,995. In the original study, the estimate for SFL was more than twice as large.

However with the N precision definition, the number of ending zeroes are coded as individual dummies rather than summarised by a single term and are tested against the reference level of 'No 0s'. After controlling for property, agent, time, and market the coefficients for 'Ends 00', 'Ends 000', 'Ends 00000' are statistically significant at 5% level but the coefficients for 'Ends 0', 'Ends 0000', 'Ends 000000' are not. The effect size of 'Ends 00' is higher than for the two terms it sits in-between - 'Ends 0' and 'Ends 000' - and so as illustrated in Panel A of Figure 5.3 the relationship between precision effect and number of ending zeroes is not monotonic.

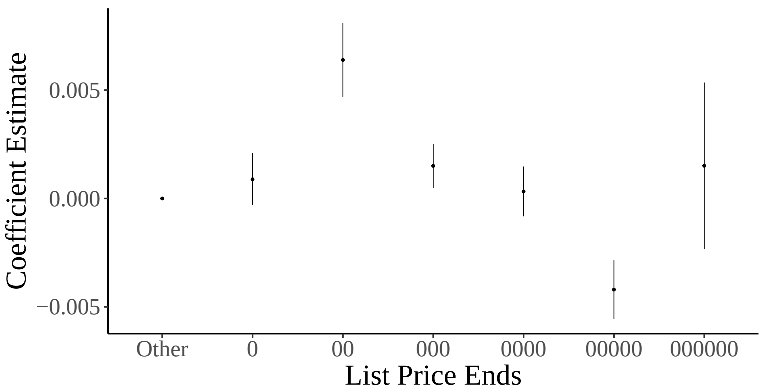
Considering now where sales price is greater than list price (specifications labelled 'Greater'), for the T1 precision definition the coefficient of the precision measure of 0.0093 is negative and significant at the 5% level. Its says that precise list prices with two or fewer ending zeroes are associated with sales prices approximately 0.93% ($e^{-0.0093}$) less than rounded list prices with three or more ending zeroes, after controlling for property, agent, time, and market. So a property listed at 499,995 would attain a sales price almost ¤4,650 lower (0.93% of ¤499,995) than one listed at 500,000. For the Capital sample this reverse precision effect is 45 times larger than its equivalent precision effect.

For the T2 precision definition, the coefficient of 0.0033 is positive and significant at the 5% level. It says that for each ending zero on the list price than the associated sales price is approximately 0.33% ($e^{0.0033}$) more, after controlling for property, agent, time, and market. So a property listed at ¤500,000 is associated with a sales price ¤8,250 higher (0.33% of 500,000 x 5) than one listed at ¤499,995. Here the reverse precision effect is 8 times larger than its equivalent precision effect.

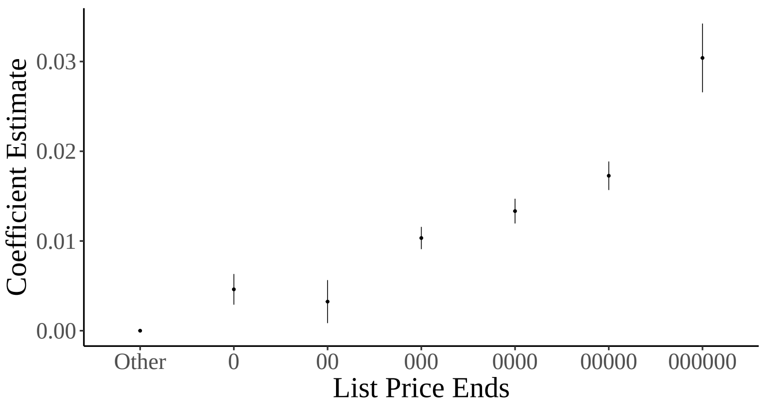
Finally the N precision definition, after controlling for property, agent, time and market the coefficients for each precision effect dummy are statistically significant at the 5% level. The coefficients are positive and generally increase monotonically as illustrated by Panel B of Figure 5.3, ('Ends' 00 is an exception with a lower coefficient than 'Ends 0'). The coefficient of 0.0173 for 'Ends 00000' implies that the associated sales price of a list price with five ending zeroes is approximately 1.75% ($e^{0.0173}$) more than a list price with no ending zeroes. So a property listed at 500,000 is associated with a sales price approximately ¤8,725 higher (1.75% of ¤500,000) than one listed at 499,995.

Figure 5.3: Precision Effect Estimates for the ‘New’ Precision Measure

(A) Sales Price Less Than List Price



(B) Sales Price Greater Than List Price



Note: The figure shows the ordinary least squares precision effect point estimates and 95% confidence intervals (vertical lines) for our baseline specification where the precision measure is ’New’ described in Section 5.2.2. Panel (A) Sales Price is less than List Price and Panel (B) Sales Price is less than List Price. The estimates are summarised in Table 5.3. Observations include all properties in the Capital sample.

Table 5.4: Precision Effect Estimates for South Florida and Long Island Samples: OLS Estimates

Panel (A): South Florida

	$LnSalesPrice_i$					
	T1 Less	T2 Less	T1 Full	T2 Full	T1 Greater	T2 Greater
Precise000	0.0056***		0.0036***		-0.0046***	
	(0.0006)		(0.0006)		(0.0012)	
No. of ending zeroes		-0.0028***		-0.0014***		0.0014**
		(0.0003)		(0.0003)		(0.0006)
Ln(List Price)	1.0029***	1.0024***	0.9984***	0.9981***	0.9935***	0.9936***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0020)	(0.0020)
Constant	-0.0684	-0.0543	-0.0189	-0.0116	0.0579	0.0472
	(0.0498)	(0.0498)	(0.0548)	(0.0549)	(0.0364)	(0.0365)
Observations	15,595	15,595	20,249	20,249	1,416	1,416
R^2	0.9960	0.9960	0.9951	0.9951	0.9991	0.9991

Panel (B): Long Island

	$LnSalesPrice_i$					
	T1 Less	T2 Less	T1 Full	T2 Full	T1 Greater	T2 Greater
Precise000	0.0038***		0.0023***		-0.0079***	
	(0.0006)		(0.0004)		(0.0010)	
No. of ending zeroes		-0.0020***		-0.0010***		0.0033***
		(0.0003)		(0.0001)		(0.0002)
Ln(List Price)	0.9687***	0.9693***	0.9649***	0.9652***	0.9889***	0.9890***
	(0.0054)	(0.0053)	(0.0049)	(0.0049)	(0.0058)	(0.0057)
Constant	0.3048***	0.3000***	0.3556***	0.3542***	0.1616**	0.1545**
	(0.0682)	(0.0675)	(0.0653)	(0.0644)	(0.0738)	(0.0705)
Observations	13,450	13,450	16,010	16,010	1,432	1,432
R^2	0.9934	0.9934	0.9916	0.9916	0.9975	0.9975

Note: The table presents ordinary least squares regression estimates of the precision effect for Panel (A) the South Florida sample and for Panel (B) the Long Island sample. Estimates use our baseline specification which includes controls for property, agent, time, and market. The dependent variable takes the value of log Sales Price. The T1 and T2 specifications incorporate different dependent variables which are different precision measures described in Section 5.2.2. For specifications labelled 'Less', sales price is less than list price; for specifications labelled 'Full', all listings in each sample are included and for specifications labelled 'Greater', sales price is greater than list price. *p<0.1; **p<0.05; ***p<0.01.*p<0.1.

Table 5.4 presents the OLS regression estimates for the SFL (Panel A) and LI (Panel B) samples. The baseline specification is given by Equation 5.1. First, considering where sales price is less than list price (the specifications labelled 'Less'), for the T1 precision definition the coefficients of 0.0056 (SFL) and 0.0038 (LI) are positive and significant at the 5% level, after controlling for property, agent, time, and market. They say that precise list prices with two or fewer ending zeroes are associated with sales prices approximately 0.56% ($e^{0.0056}$) higher and 0.38% ($e^{0.0038}$) higher than rounded list prices with three or more ending zeroes for SFL and LI respectively. So a property listed at 499,995 would attain a sales price almost ¤2,800 higher (0.56% of ¤499,995) for SFL and almost ¤1,900 higher (0.38% of ¤499,995) for LI than one listed at 500,000. The effect sizes are larger than in the original studies.

For the T2 precision definition, the coefficients of 0.0028 (SFL) and 0.0020 are negative and significant at the 5% level, after controlling for property, agent, time, and market. They say that for each ending zero on the list price than the associated sales price is approximately 0.28% ($e^{-0.0028}$) less and 0.20% ($e^{-0.0020}$) less for SFL and LI respectively. So a property listed at ¤500,000 is associated with a sales price ¤7,000 lower (0.28% of 500,000 x 5) for SFL and ¤5,000 lower (0.20% of 500,000 x 5) for LI than one listed at ¤499,995. Again, the effect sizes are larger than in the original studies.

Second, considering where all listings are included in the samples regardless of the relationship between sales price and list price (the specifications labelled 'Full'). In both SFL and LI the coefficients of the T1 precision measure are positive and significant at the 5% level but show a lower effect size than for their equivalent 'Less' samples. Similarly in both SFL and LI the coefficients of the T2 precision measure are negative and significant at the 5% level but show a lower effect size than for their equivalent 'Less' samples. This is consistent with Thomas et al. (2010), who do not publish the results for their full samples but instead say the effects are the same but smaller. Transactions where sales price is more than list price represent approximately 10% of the Full samples, their reversal of the original precision price effects (described below) reduces but does not dominate the estimates.

Finally considering where sales price is more than list price (the specifications labelled 'Greater'), for the T1 precision definition the coefficients of 0.0046 (SFL) and 0.0079 (LI) are negative and significant at the 5% level, after controlling for property, agent, time, and market. They say that precise list prices with two or fewer ending zeroes are associated with sales prices approximately 0.46% ($e^{-0.0046}$) lower and 0.79% ($e^{-0.0079}$) lower than rounded list prices with three or more ending zeroes for SFL and LI respectively. So a property listed at 499,995 would attain a sales price almost ¤2,300 lower (0.46% of ¤499,995) for SFL and almost ¤3,950 lower (0.79% of ¤499,995) for LI than one listed at 500,000.

For the T2 precision definition, the coefficients of 0.0014 (SFL) and 0.0033 (LI) are positive and significant at the 5% level, after controlling for property, agent, time, and market. They say that for each ending zero on the list price than the associated sales price is approximately 0.14% ($e^{0.0014}$) more and 0.33% ($e^{0.0033}$) more for SFL and LI respectively. So a property listed at ¤500,000 is associated with a sales price almost ¤3,500 higher (0.14% of 500,000 x 5) for SFL and ¤8,250 higher (0.33% of 500,000 x 5) for LI than one listed at ¤499,995.

5.4 Discussion and Conclusion

For the majority of homeowners buying and selling their property is the highest stake negotiation in which they participate. Arriving at the best the price is of paramount importance. Research by Thomas et al. (2010) found that a precise list price led to a higher sales price. We attempt to replicate the results from one study with their data and with a larger novel dataset.

Thomas et al. (2010) analysis focused on transactions where the list prices of the properties were greater than their sales prices. Considering this case first, our results for their SFL and LI datasets are qualitatively similar to their original study despite using different sample selection criteria and wrangling the data differently. Indeed our results show stronger effects sizes. Estimates are significant at the 5% level.

The results for our new Capital dataset are also broadly consistent although effect sizes are much smaller. Defining a precise price as having 2 or fewer ending zeroes, the effect size of 0.02% is 28 times smaller than its SFL equivalent. Similarly, defining precision by the number of ending zeroes the effect size of 0.04% is less than 7 times smaller than its equivalent

Considering now, transactions where the list prices of properties are less than sales price. Thomas et. al had excluded these. We find for all three samples that their results are qualitatively consistent and demonstrate a reverse price precision effect — a more precise list price is associated with a lower sales price. For Capital there is a much larger effect size than the original price precision effect. The decrease in sales price associated with a precise list price with 2 or fewer ending zeroes is 0.93% (45 times larger) or 0.33% for each fewer ending zero (8 times larger).

A possible single explanation for the original effect and its reversal may be contained in a paper published just two years before the original study by Janiszewski and Uy (2008) which examines the influence of precision on anchoring and adjustment. The anchoring and adjustment heuristic is a cognitive bias whereby individuals making estimates use an initial (often irrelevant) value (or anchor) as their starting point and adjust it insufficiently so that the initial value biases their final estimate (Tversky and Kahneman, 1974). Examples include judging future effort and performance (Switzer III and Sniezek, 1991) and assessing risk and uncertainty (Wright and Anderson, 1989).

In negotiation, the first offer is found to act as an anchor influencing the final price (Galinsky and Mussweiler, 2001).

Multiple studies posit the influence of anchoring on the pricing in the housing market. Asking prices, the first offer in a property sale negotiation, are a commonly found anchor. In a study by Northcraft and Neale (1987), students used as a proxy for first-time buyers, and professional estate agents were provided with detailed information packs, visited, and evaluated residential properties. Only the list price was manipulated but it had a significant impact on both the students' and professionals' evaluations. An analysis of 14,000 property sales in three U.S. States, which took account of differences in the properties' features, location and timing of the sale, found that higher initial listing prices led to higher sales prices contravening typical estate agency advice to under-price and encourage rival bids between multiple potential buyers; in fact no evidence of herding behaviour was found (Bucchianeri and Minson, 2013). This anchoring effect is also found in commercial real estate (Bokhari and Geltner, 2011). Examples of different anchors influencing housing market negotiations include buyers moving to a new area with different property prices tending not to alter their benchmark, making bids at a significant variance to those of existing residents (Lambson et al., 2004; Simonsohn and Loewenstein, 2006) and sellers anchoring on their purchase price unwilling to settle for a nominal loss (Genesove and Mayer, 2001).

Research has investigated the influence of the precision of the anchor on the adjustment it causes. Janiszewski and Uy (2008) carried out four laboratory experiments and as here a quasi-experiment analysing residential real estate sales data. Like the Thomas et al. (2010) study, properties were located in Florida and transactions restricted to where list price was more than sales price. Janiszewski and Uy (2008) showed that the more precise the numerical anchor, the smaller the adjustment and so the greater the anchoring effect. This effect has been explained by two distinct mechanisms. First, the *scale granularity* mechanism which propounds a precise anchor creates a finely grained mental scale causing people to make adjustments in small increments whereas a rounded anchor creates a coarsely grained mental scale with adjustments made in large increments (Janiszewski and Uy, 2008). Second, the *attribution of competence* mechanism propounds precision influences social perceptions. A precise anchor conveys competence so people makes smaller adjustments from it (Loschelder et al., 2014, 2016).

Potentially here the list price is the anchor which is adjusted to the sales price (Northcraft and Neale, 1987). Where the list price is greater than sales price then the adjustment is necessarily downwards, a precise list price is associated with less adjustment than a rounded list price so this smaller adjustment attains a higher sales price. Conversely where list price is less than sales price than the adjustment is necessarily upwards. If a precise list price leads to less upwards adjustment than a rounded one then a lower sales price is attained. To fully account for our results a more nuanced theory is required to explain the asymmetry in effect sizes i.e. for the Capital dataset the reverse precision effect is much larger than the precision effect.

Thomas et al. (2010) and Janiszewski and Uy (2008) assumed transactions where sales price is greater than list price arise from multiple offers from rival bidders, with buyers more influenced by their rivals' offers than any price precision considerations. However our analysis suggests price precision may still have an impact. Future research could investigate in the laboratory the influence of price precision on participants' willingness to pay in (hypothetical) competitive bidding situations. Another topic for further investigation is explaining the asymmetry in effect sizes that we found.

We were broadly able to replicate the price precision effect when list price is greater than sales price, a more precise list price is associated with a higher sales price as Thomas et al. (2010). However we also found a larger reverse effect which the original study does not report. When list price is less than sales price a more precise list price is associated with a lower sales price. Both effects can be explained, in part, by the theory that the precision of an anchor influences the amount of adjustment.

6 Conclusion

This thesis presents four independent behavioural science research projects centred on the high-stakes domain of residential property transactions. They use field data to investigate individual decision making and have the secondary aim of illustrating and engaging with three of the discipline's broader debates: the use of novel methods, the effectiveness of nudges and the replication crisis. Our research methods benefit from the twenty-first century's huge expansion in the digitisation of behavioural information. We apply econometric techniques to mass transaction datasets and conduct an online field study. Our main findings are briefly summarised below.

Chapter 2 Casual observation of both home-owners and stock-holders suggests that owners often seem to believe that the 'true' value of their asset is the highest price it achieved while they have held it, the *peak price*. We show a new disposition effect arising from the peak price experienced by the asset owner, which exists over and above the disposition effect arising from the purchase price. We demonstrate this using large data sets documenting price changes and selling decisions for both housing and stocks.

Our estimates reveal that the selling probability for stocks and homes more than doubles when returns since a past peak price turn positive. Whereas a home (stock) in gain since purchase but in loss since a past peak price is approximately 40% (50%) more likely to be sold, a home (stock) in gain since purchase and in gain since a past peak price is 130% (96%) more likely to be sold. These discontinuities in the sale probability are found regardless of the magnitudes of gains or losses—with respect to either the peak price or the purchase price. For stocks, we show that these reference point effects only occur if the peak price events took place during the time when the individual owns the asset.

The main estimates are robust to a variety of econometric specifications and a wide range of controls. In a series of tests, we condition the model to include individual fixed effects; flexible controls for returns since purchase and returns since the peak price event; a range of controls for housing and stock characteristics. We also conduct additional checks to address particular confounds which might apply to the analysis of housing and stocks; for housing we show our results are not due to market liquidity or individual leverage, which might restrict selling opportunities.

Our findings also complement analysis by Genesove and Mayer (2001), who analyse 3,400 condominium sales from a listings agent in Boston during the 1990s, and find owners are less likely to sell if they are in loss. Here we confirm and precisely measure this purchase price disposition effect on purchase price in a dataset covering 25 million transactions from 1995 to 2019.

This study contributes to the growing literature examining the consequences of salience and attention for individual behaviours. More generally, our study expands a new line of research on how different reference points interact to determine individual choices in situations in which multiple natural points of comparison operate.

In Chapter 3 we estimate the effect of the rank position of online property adverts on the clicks they receive. Online sellers need to understand how an advert's position influences its effectiveness because adverts at the top of the search results on websites such as Google cost more. Typically an advert's position depends on its relevance to the search terms provided by the consumer and the bidding behaviour of the advertiser and its competitors at auction. This can lead to significant selection issues when measuring rank effects.

Previous studies either ignored this endogeneity or attempted different approaches to overcome it, each with its own limitations. We use a new approach. First, we reconstruct over 9 million search results from real-world online property listings data where all adverts in each search result are equally relevant to the specified search terms. Then we conduct a quasi-experiment where random variation in each target advert's ranking arises from its position moving unpredictably day-by-day as new properties list or existing properties change price or delist. Rank positions are only defined by list price and the property seller cannot change the position of their target advert once listed. We find an exponential decline in clicks as rank decreases, which despite our novel method's shortcomings supports findings from prior research.

Our findings are limited by the assumptions and data exclusions made e.g. we reconstruct only one search query type and we exclude the most prominent list prices because they lie on two price bands. These limitations effect the robustness and generalizability of our results. Different datasets could help to overcome many of these rectifiable limitations. Additionally since the property purchase process is long and advert rankings change over time, we cannot associate an advert's rank on any given day with the subsequent property purchase. Thus we cannot investigate the influence of advert position on purchase unlike the studies which investigated the online purchase of consumables or as we do with a RCT in Chapter 4.

In Chapter 4 we investigate the limits to nudges. In many settings, nudges are thought to be powerful, low-cost tools for changing economic outcomes while preserving free choice. We evaluate the economic effects of a common form of nudge that makes one item more salient in the choice set by moving the item to the first-ranked position. Our setting is the online property listings market, whereby vendors can pay a premium to nudge their property listing to being first ranked in the choice set. Using a randomised control trial, we show that this nudge dramatically increases property viewings on the website, by over 30%, but has no significant effect on the economically important downstream outcome of sales price paid. Further evidence suggests this nudge generates economically inefficient congestion, slowing time taken to achieve the sale.

We can speculate why the nudge does not have a positive downstream economic impact. Nudges are found to have the smallest effect in the financial domain where they could have a substantial life impact on people's lives. Consumers who actually buy are more likely to visit lower ranked listings than information seekers (Agarwal et al., 2011). Potentially non-serious property buyers are clicking adverts that stand out for entertainment whereas serious buyers are thoroughly checking everything that meets their requirements. The nudge's negative influence on time to sale may result from additional buyers (serious or otherwise) slowing down the sales process or causing buyers to become overconfident and not accept reasonable offers. Additional data on the customer journey e.g. the number of viewings, the size of offers made but not accepted would provide insight on where the nudge stops being effective and the cause.

Our findings suggest that behavioural research needs to look beyond the immediate impact of a nudge (on, e.g., attention) to the downstream economic impact, where effects may be different to those intended or observed when looking only at proximate, near-term outcomes. Moreover they serve as a corrective to the philosophy that low-cost, easy, interventions will always be sufficient to effect the the desired behaviour change. More specifically our results support the marketing literature that claims a highly placed digital advert does not necessarily increase sales (Narayanan and Kalyanam, 2015; Blake et al., 2015).

Extending our main analysis, descriptive results suggest that the nudge affects the page views of properties in thicker markets (lower-price, higher-volume) more strongly than those in thinner markets (higher-price, lower-volume properties). Potentially being first helps the less distinctive thicker market properties stand-out in a crowded market but has a lower impact with the less common and already more distinctive thinner market properties. This stronger effect on attention is still not associated with people paying more for the thicker market properties.

Moreover we find real estate agents tend to apply the nudge to thinner market properties. Enhanced adverts are more expensive than standard adverts and so a real estate agent may be more prepared to bear this additional marketing cost to attract or retain higher-fee paying clients who will not know the nudge does not have any positive economic effect.

Finally using historic data, we attempt to reproduce the results from our field study using SCMs. Taking our RCT as the 'gold standard' we show that the exact marching SCM did not consistently measure the nudge's effectiveness accurately. However, although our data comes from the same third party, the SCMs and RCT use different samples: SCM properties are only located in the capital whereas the RCT properties are spread across the country and the data is collected over different time periods. This may account for the conflicting results. We also only test one synthetic control method, possibly a different method would lead to more consistency between the two research methods.

Nevertheless our findings support the conclusions of Gordon et al. (2019) that estimates based on observed data are not necessarily accurate. They provide an example of the need for researchers when applying novel methods to question and substantiate their applicability in different contexts.

In Chapter 5 we sought to replicate the results from a study by Thomas et al. (2010) which found that precise property list prices led to a higher sales prices. We use the authors' data and our larger novel dataset and were broadly able to replicate their results when list price is greater than sales price. However we also found a reverse effect when list price is less than sales price – a more precise list price is associated with a lower sales price. Thomas et al. (2010) do not report this reverse effect which from our results was up to 45 times larger than their original precision effect.

Both effects can be partially explained, by the theory that the the precision of an anchor influences the amount of adjustment (Janiszewski and Uy, 2008). The list price is an anchor which is adjusted to the sales price (Northcraft and Neale, 1987) and a more precise anchor has a greater anchoring effect leading to a smaller adjustment. So here the precise list price is adjusted less downwards when sales price is less than list price and adjusted less upwards when sales price is greater than list price. However this explanation requires further refinement and investigation to account for the asymmetry in effect sizes.

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A Appendix: At the Top of the Mind

Peak Prices and the Disposition Effect

A.1 An Illustration of the Framework Presented in Quispe-Torreblanca et al. (2021) for the Case of Peak Prices and the Disposition Effect

We derive predictions about the interactive impact of purchase price and peak price on selling decisions drawing upon a recent framework of the disposition effect proposed by Quispe-Torreblanca et al. (2021). In that paper, the authors develop a framework of realization utility which incorporates prospect theory preferences, but with the innovation of multiple reference points (e.g., the purchase price, the price at the last login day, the peak price, and the current price). A key assumption in the framework is that an individual who is exposed to more than one salient reference point focuses on the most aspirational price – here meaning the highest price – when deciding whether or not to sell an asset at a particular point in time. This price represents the best price achieved to date and hence it is actually the least favourable for a comparison of the individual's current position.

In Figure A.1, we augment the basic four-period model of Quispe-Torreblanca et al. (2021), introducing peak prices in to the model.

Period 0. The individual purchases an asset at a price p_0. This purchase price constitutes a salient reference point. Between Period 0, and Period 1, the price then either rises or falls with equal likelihood to a price p_1.

Period 1. In Period 1, the individual observes the new price p_1, which becomes a salient reference point. Between Period 1, and Period 2, the price then either rises or falls with equal likelihood to a price p_2.

Period 2. In Period 2, the individual observes the new price, then chooses whether or not to sell the asset. Between Period 2, and Period 3, the price then either rises or falls with equal likelihood to a price p_3.

Period 3. In this final period, the individual liquidates any remaining position in the asset.

The model incorporates two simplifying assumptions, first, that at the start of Period 0 the individual purchases an asset which takes the form of a single unit and that prior to each period the price rises or falls with equal likelihood (independent of the price history) by a fixed amount (for simplicity, normalized to 1), and second, that once having sold the asset, the receipts are held in a risk-free asset, as is most commonly the case with proceeds from housing sales or modern brokerage accounts.[1] With the assumption of realization utility, the individual is only concerned with the utility experienced from selling the asset, either in Period 2 or 3.

The version of the framework presented here differs from that in Quispe-Torreblanca et al. (2021) by assuming that at Period 1 the individual always observes the price of the asset. In the formulation of the model in Quispe-Torreblanca et al. (2021), which focuses on the role of attention in creating reference points, whether the individual observed the price depended on whether she looked (e.g., made a login to the account on the investment platform, or consulted a financial news database).

Figure A.1 Panel A illustrates the events in the framework. Beginning from p_0 at time $t = 0$, the price of the asset rises or falls through Periods $t = 1, 2, 3$, resulting in the individual arriving at a node in each time period, dependent on the evolution of the price of the asset. Panel B describes the individual's selling decision under prospect theory preferences at each node in the period $t = 2$.

At $t = 2$, the individual maximises a prospect theory value function given by

$$\begin{aligned} |p - r|^{\delta} \ \text{ if } \ p - r \geq 0, \\ -\lambda|p - r|^{\delta} \ \text{ if } \ p - r < 0, \end{aligned} \tag{A.1}$$

where δ, $(0 < \delta < 1)$ and λ respectively, determine the curvature of the value function and the degree of loss aversion. The reference point r, is determined by the price in period $t = 1$. The reference point is given by:

$$r = \gamma p_1 + (1 - \gamma) p_o \tag{A.2}$$

where γ takes a value of 1 if $p_1 > p_0$ and 0 otherwise.

Node +2 and Node −2 are two degenerate cases in the model. These result from

[1] Proceeds from housing sales, net of any mortgage redemption, are transferred to checking accounts. In the Barclays Stockbroking data used in this study, proceeds from sales are automatically transferred to a liquid account paying nominal money market returns.

the price either falling prior to both $t = 1$ and $t = 2$, or rising prior to both $t = 1$ and $t = 2$. At Node -2 the individual is in the domain of losses. Because of the convexity of the value function, the individual is risk-seeking in this situation, which means holding the asset and risking the possibility of an increase prior to $t = 3$. At Node +2 the individual is in the domain of gains. Because of the concavity of the value function, the individual is risk-averse and hence sells the asset, shifting receipts to the safe asset.

Node 0 constitutes the most interesting situation. At this node, whether the individual is in loss or gain rest on the price history of the asset. If the asset price rose, then the reference point is the peak price $p_o + 1 > p_0$, and the individual is in the risk-seeking domain of losses and doesn't sell (incurs the risk of holding the asset for an additional period). If the asset price fell, then the reference point is equal to the purchase price, which is equal to the sell price and the individual sells (due to the concavity of the value function).

The model generates two main predictions. First, it implies the existence of a disposition effect defined over returns since purchase, but also a disposition effect defined over returns since the peak price. Second, the model implies that an individual may not sell even when the asset is in the domain of gains since purchase if the peak price is higher than the purchase price. When this is the case, then the reference point is at a higher price (this is the case at Node 0 when the price of the stock rose, then fell at period $t = 2$). In such cases, the positive effect on utility of the return since purchase is nullified by the loss since the peak. Hence, the individual chooses not to sell. A simulation exercise using sensible parameters in a prospect theory value function are provided in Table A.1.

A.2 Additional Stock Sales Results

Peak Price Effects in Stock Sales: Additional Robustness Test

Hazard Models

We estimate a Cox proportional hazard model that exploits the duration dimension of the data, which is an important determinant of asset-selling decisions (Ben-David and Hirshleifer, 2012). The hazard model allows us to estimate the time-varying probability of a sell event without imposing any structure on the baseline hazard (i.e., without specifying the exact form of the distribution of the sell event times).

Table A.20 shows estimates of Equation A.1 for stock sales. The coefficient of the gain since purchase dummy in Column 3 is 0.323, which indicates that when the stock is in loss since the past peak price day, investors are $\exp(0.323) \approx 1.38$ times more likely to sell a winning stock (since purchase) compared to a losing stock. The coefficient of the gain since peak price dummy is 0.6702, and indicates that when there is a gain since peak price, investors are $\exp(0.323 + 0.670) \approx 2.699$ times more likely to sell a winning stock compared to a losing stock. This estimates are also qualitatively similar to those obtained under the linear probability analysis described in the main body of the paper[2]

Peak Price Effects in Stock Sales: Sensitivity Analysis

Excluding Partial Sells

One potential explanation for the disposition effect might be portfolio rebalancing, whereby investors decrease their positions in winning stocks in order to rebalance their portfolio. To test for this, we restrict the dependent variable to indicate complete sales only (i.e., liquidation of positions), thereby excluding partial sales which might reflect a desire to rebalance portfolios. This test of whether selling activity is driven by a desire to rebalance when prices increase (increasing the likelihood of a gain since purchase of gain since peak price) is used by Odean (1998). Results are shown in Table A.21, which reports estimates from models with and without the inclusion of individual fixed effects. The coefficient estimates from these models are in line with those in the main results, with the coefficients of gain since purchase and gain since peak price both precisely defined and returning similar magnitudes to those in the earlier analysis. This result suggests that the increased likelihood of selling a stock

[2] The magnitude of the effect of a gain since purchase, conditional on a gain since past peak, in Column 3 is also qualitatively similar to results obtained by Seru et al. (2010). Seru et al. (2010) estimated a Cox model using data from 11,000 individual investors in Finland. In their analyses, they computed the hazard ratio for selling a winning stock (since purchase) for each investor and year in the data, finding that the median investor has a hazard ratio of about 2.8, a value close to the hazard ratio we observe in the data of 2.699

when the investor experiences a gain since purchase and/or gain since peak price does not arise due to a desire to rebalance the overall portfolio.

Peak Price Definition

Our main estimates use the one-week time horizon to define a peak price event: a peak price is the highest price achieved by a stock in the the investor's holding period that remains the highest price for at least one week. In this section, we test the sensitivity of our main estimates by replacing the one-week time horizon with one month. Figure A.10 illustrates the implementation of the one-month time horizon using an example stock. Using the one-month time horizon, a given stock has fewer peak price events in its history due to the longer period over which any high price event has remained the highest so far in the history of the stock for it to be classed as a peak price.

Using the same baseline sample, we implement the one-month time horizon definition of a peak price. Figure A.11 shows the distributions of returns since purchase and returns since peak using the month-peak definition. Table A.9 summarises the returns since purchase and returns since peak using the month-peak definition.

We see the same quantitative patterns in the unconditional analysis and econometric model estimates using the month-peak definition as for the week-peak definition. Figure A.12 illustrates the unconditional results, which mirror those shown in Figure 2.5. We see a large increase in the probability of sale when returns turn from negative to positive for both returns since purchase (Panel A) and returns since the peak price (Panel B). We also see a large interaction effect whereby the increase in probability of sale when returns turn from negative to positive is much greater when the stock is in gain since peak price compared with when the stock is in loss since peak price (Panel C).

Table A.18 reports estimates of Equation 2.2 (Column 1) plus a rich set of additional specifications including a range of controls as reported in the main analysis in Table 2.16. The fullest pooled model (Column 8 of Table A.18) returns coefficient values of 0.0699 and 0.0511 on the gain since purchase and gain since peak dummies respectively, which show little change in the model that includes account and stock fixed effects (Column 10) in which the values are 0.0759 and 0.0586. These estimates are again very similar to the OLS estimates reported in Table 2.13: the increased probability of sale for a stock in gain since purchase and gain since peak is 13.4 percentage points in Column 10 of Table A.18, compared with 14.8 percentage points in Column 3 of Table 2.13.

Login Days

We also test the sensitivity of our estimates by using the sample of login days. Our main estimates use sell-days, following previous studies (beginning with Odean, 1998), which restrict the sample to account × stock × time units on which the investor sold at least *one* stock in their portfolio. The rationale for this sample restriction is that the investor was paying attention to the portfolio at those points in time and there was some risk that the investor would sell *any* stock. Given the availability of login data in the Barclays Stockbroking data set, we also show results from a login-day sample: the subsample of observations of × stock × days on which the investor made a login to their account. The rationale for this sample is that on login days investors pay attention to their accounts, and hence have some non-zero likelihood of making a sale. Figure A.13 shows distributions of returns since purchase and returns since peak in the login-day sample.

Again, we see the same quantitative patterns in the unconditional analysis and econometric model estimates using the login-day sample as for the sell-day sample. Figure A.14 illustrates the unconditional results, which mirror those shown in Figure 2.5. We see a large increase in the probability of sale when returns turn from negative to positive for both returns since purchase (Panel A) and returns since the peak price (Panel B). We also see a large interaction effect whereby the increase in probability of sale when returns turn from negative to positive is much greater when the stock is in gain since peak price compared with when the stock is in loss since peak price (Panel C).

In Table A.19, we report estimates of Equation 2.2 (Column 1) plus a rich set of additional specifications including a range of controls as reported in the main analysis in Table 2.16. The probability of sale in the login-day sample is one tenth of that the sell-day sample (for comparison, in Table A.18 the intercept value is approximately 0.1, whereas in Table A.19, the intercept value is approximately 0.01).

The coefficient values in Table A.19 are very similar to those in the main analysis (adjusting for the 1:10 ratio of sales in the login-day and sell-day samples). In a model that includes account and stock fixed effects (Column 10), the coefficient values of the gain since purchase and gain since peak price variables are 0.0058 and 0.0068, respectively. These estimates are again very similar to the OLS estimates reported in Table 2.13, once allowing for the 1:10 ratio of sales: the increased probability of sale for a stock in gain since purchase and gain since peak is 12.6 percentage points in Column 10 of Table A.19, compared with 14.8 percentage points in Column 3 of Table 2.13.

Interactions

We test the sensitivity of our main results to market, account and investor characteristics. This subsection analysis is motivated by previous studies showing that investor behaviours vary by a range of characteristics such as gender and age (Barber and Odean, 2001; Agnew et al., 2003; Dorn and Huberman, 2005; Mitchell, Mottola, Utkus, and Yamaguchi, Mitchell et al.) and trading experience (Feng and Seasholes, 2005; Seru et al., 2010).

To begin, Table A.22 shows estimates by subsamples split by market index movements and days elapsed since purchase and days elapsed since the peak price event. In this analysis, we report the coefficients for the gain since purchase and gain since peak from a separate regression on each row of the table. First, we find the coefficients for gain since purchase and gain since peak are very similar in rising and falling markets. Second, we find some evidence that the strength of the disposition effect varies with time since purchase and the peak price event. Notably, the coefficient of gain since purchase is the weakest when the peak price event was more recent, suggesting that recent peak price events may diminish the power of the purchase price as a reference point.

Table A.23 shows estimates by subsamples of individual characteristics. Results show the disposition effect on gain since purchase and gain since peak price exists across both samples of females and males, younger and older, with smaller coefficient estimates for the subsample of older investments. The coefficient estimates are also slightly smaller among investors who have held their accounts for longer, these two pieces of evidence suggest a weaker disposition effect among more experience investors. Estimates also show smaller coefficients for those with larger portfolios and those with a larger number of stocks. These estimates might suggest that the propensity to exhibit a disposition effect over purchase price or peak price falls with the financial stakes of the decision (as proxied by the total value of the portfolio and the number of stocks). However, because intercept terms are also smaller in these subsamples, the *relative* effect of the gain dummies (with respect to the baselines given by the intercepts) doesn't diminish in large portfolios.

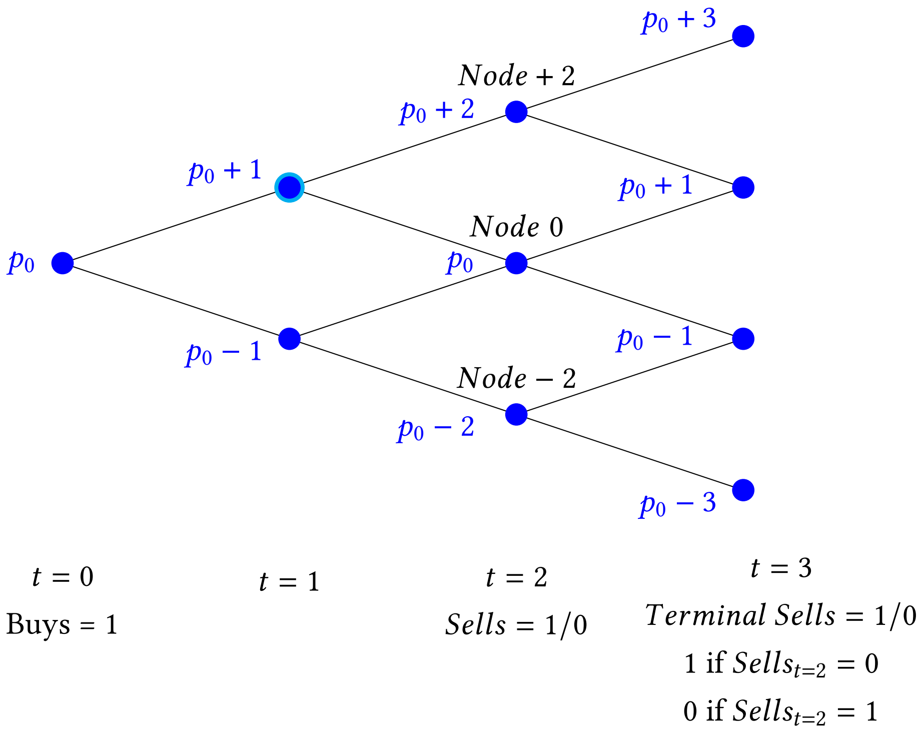
In a final analysis in this subsection, Table A.24 and Table A.25 present estimates from subsamples of observations defined by market gains / losses since purchase and since the day of the peak price event. This analysis splits the sample by whether the market (FTSE100) was in loss or gain since the peak price event (results shown in Columns 1-3 and 4-6 respectively). Coefficient estimates show the existence of quantitatively similar disposition effects on both purchase price and peak price, with some evidence that the effect of being in gain since peak is weaker when the market is in gain since peak (Column 6 compared with Column 3 in both tables).

Other Reference Points

In Quispe-Torreblanca et al. (2021), we show that the price observed on the last login day constitutes another important reference point that influences investors selling decisions. Using our one-week time horizon definition of peak events, stocks could be in gain since the peak price day but at the same time in loss since the most recent login day to their account. Figure A.15 reproduces the analysis on the triple interaction between gains since purchase, gains since the past peak price, and gains since the most recent login, we document in Quispe-Torreblanca et al. (2021) (Figure A6). We observe that losses on any of these margins reduce the probability that the investor will sell, even when other margins show gains. These results are consistent with the framework presented in Quispe-Torreblanca et al. (2021), in which people focus on the most aspirational reference point (i.e., the highest reference point).

Figure A.1: Illustration of the Model of Multiple Reference Points

(A) Model Structure



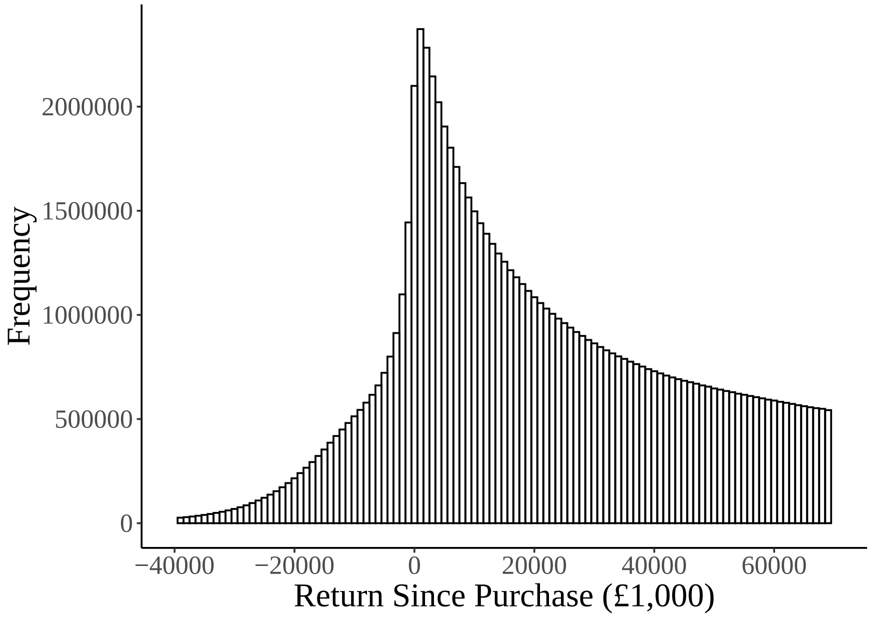
(B) Sell Decisions for Different Reference Points

Price at $t = 1$	Reference point at $t = 2$	Price at $t = 2$		
		Node -2	Node 0	Node +2
$P_0 + 1$	$P_0 + 1$	Don't Sell	Don't sell	Sell
$P_0 - 1$	P_0	Don't Sell	Sell	Sell

Note: The figure illustrates the four-period model of multiple reference points. In Panel A, at $t = 0$ the individual purchases an asset at a price p_0, which constitutes a first reference point. At $t = 1$, he observes his portfolio and the price observed becomes a new reference point. At $t = 2$, he chooses whether or not to sell the asset, and at $t = 3$ he liquidates any remaining position in the asset. Panel B displays the predictions of the model under which an individual with prospect theory preferences based his selling decisions using the highest reference point. An example of a peak price is highlighted in Panel A when the price reaches $p_0 + 1$. An individual experiencing that peak price will use it as the main reference point for his selling decision at $t = 2$.

A.3 Supplementary Figures

Figure A.2: Housing Return Since Purchase (Full Sample)

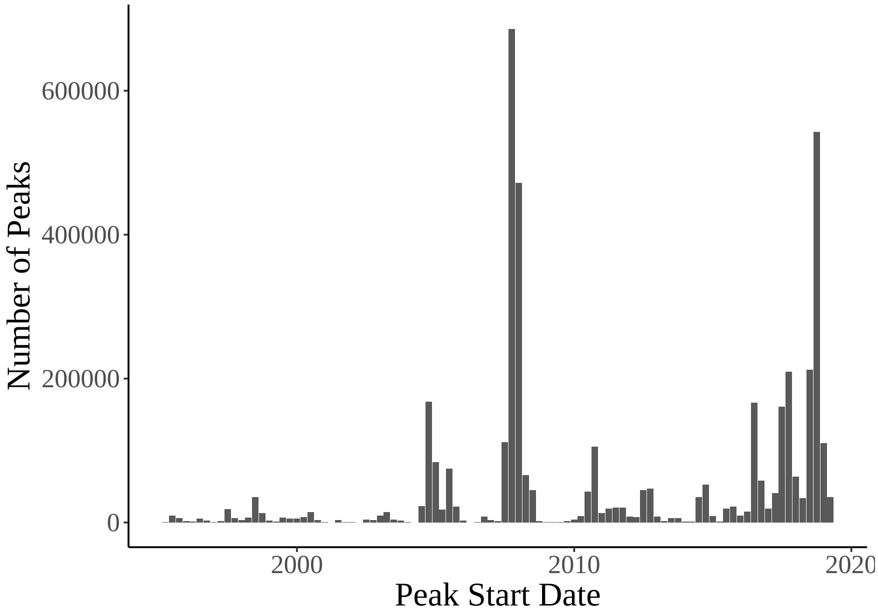


Note: The figure shows the distribution of returns since purchase. Returns are plotted as levels and for a better visualisation are in the range -£40,000 and +£60,000. Observations include all property × quarters in the baseline sample.

Figure A.3: Housing Return Since Purchase (Peak Price Sub-Sample)

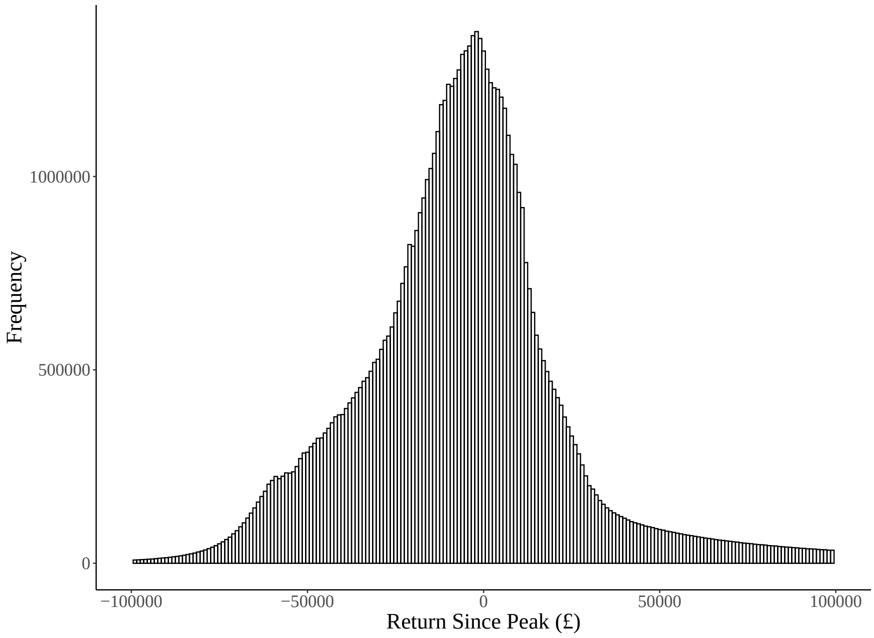
Note: The figure shows the distribution of returns since purchase. Returns are plotted as levels and for a better visualisation are in the range -£40,000 and +£60,000. Observations include all property × quarters in the baseline sample with a past peak price.

Figure A.4: Frequency Distribution of Peak Price Dates



Note: The figure shows the distribution of the peak dates for house prices. Peak prices are clustered at the start of the Great Recession and, to a lesser extent, at the date of the UK Brexit vote.

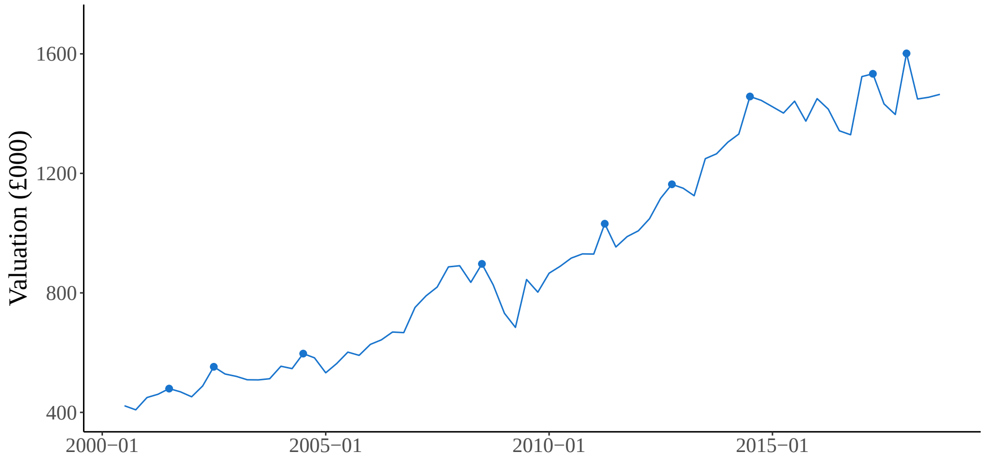
Figure A.5: Housing Returns Since Peak (Peak Price Sub-Sample)



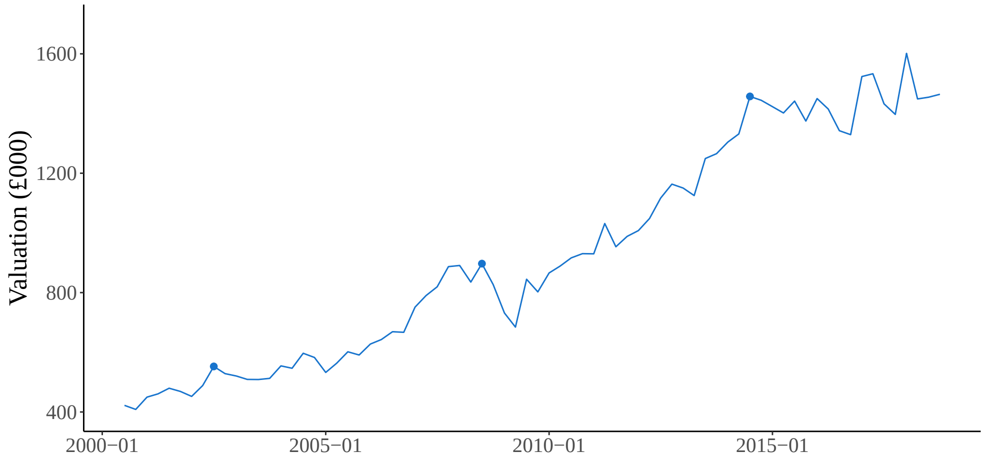
Note: The figure shows the distribution of residualized returns since peak price. Residualized returns are plotted as levels and to remove the effect of returns clustered on a few dates (see Figure A.4), the panels display residuals from linear specifications that regress return since peak against year-quarter dummies, controlling for time since peak and omitting the intercepts. For a better visualisation, levels are in the range -£100,000 and £100,000. Observations includes all property × quarters in the baseline sample with a past peak price.

Figure A.6: Example of Peak Prices, Housing Data, Alternative Definitions

(A) Two Quarters Definition



(B) Four Quarters Definition

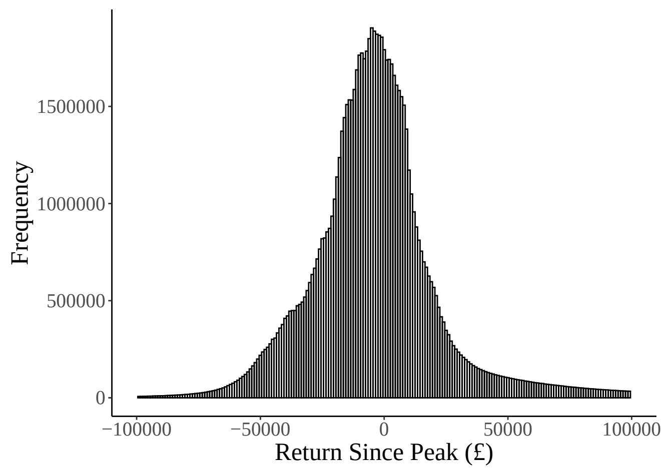


Note: The figure shows the sequence of peak prices for housing data under two alternative definitions of peaks. In the figure, a peak price remains as the highest price (since purchase) for at least two quarters (Panel A) and for at least four quarters (Panel B), instead of three quarters (as in the main results of the paper, see Figure 2.1 Panel A).

Figure A.7: Housing Returns Since Purchase and Returns Since Peak (Two Quarters Definition)

(A) Housing Returns Since Purchase

(B) Housing Returns Since Peak

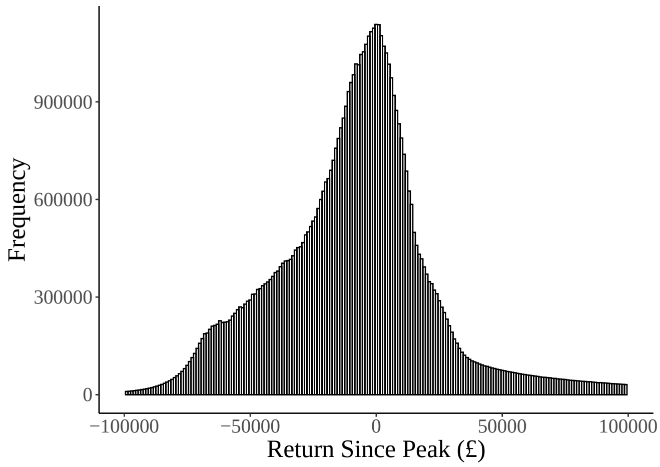


Note: The figure shows the distribution of returns since purchase (Panel A) and residualized returns since peak (Panel B) under an alternative definition of peaks. In the figure, a peak price remains as the highest price (since purchase) for at least two quarters, instead of three quarters (main results). Returns are plotted as levels. To remove the effect of returns clustered on a few dates Panel B displays residuals from linear specifications that regress return since peak against year-quarter dummies, controlling for time since peak and omitting the intercepts. For a better visualisation levels are in the range -£40,000 and +£60,000 (Panel A) and -£100,000 and +£100,000 (Panel B). Observations include all property × quarters in the baseline sample with a past peak price.

Figure A.8: Housing Returns Since Purchase and Returns Since Peak (Four Quarters Definition)

(A) Housing Returns Since Purchase

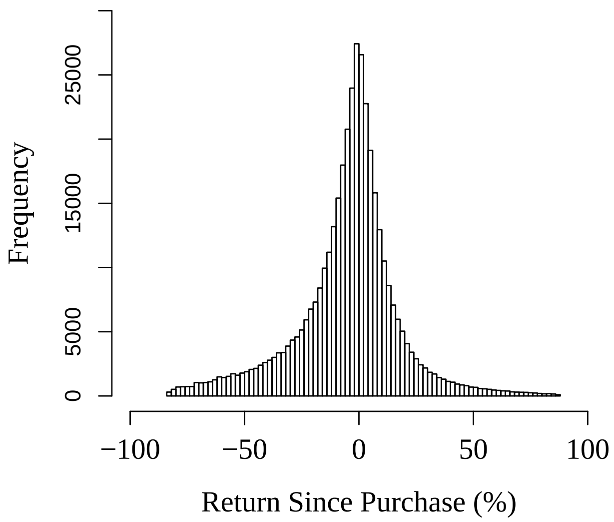
(B) Housing Returns Since Peak



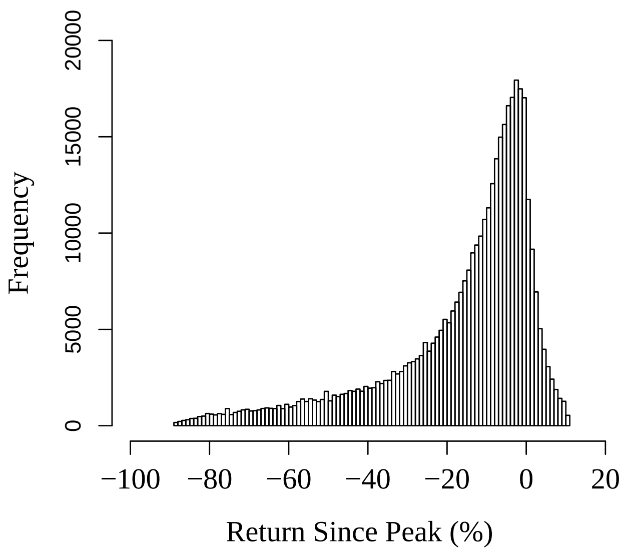
Note: The figure shows the distribution of returns since purchase (Panel A) and residualized returns since peak (Panel B) under an alternative definition of peaks. In the figure, a peak price remains as the highest price (since purchase) for at least four quarters, instead of three quarters (main results). Returns are plotted as levels. To remove the effect of returns clustered on a few dates Panel B displays residuals from linear specifications that regress return since peak against year-quarter dummies, controlling for time since peak and omitting the intercepts. For a better visualisation levels are in the range -£40,000 and +£60,000 (Panel A) and -£100,000 and +£100,000 (Panel B). Observations include all property × quarters in the baseline sample with a past peak price.

Figure A.9: Stock Returns Since Purchase and Returns Since Peak (Week Definition)

(A) Stock Returns Since Purchase

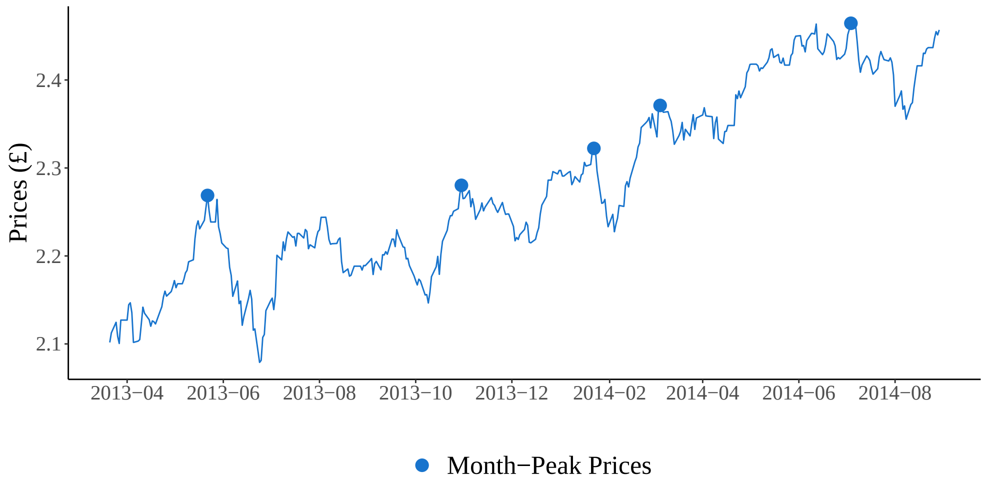


(B) Stock Returns Since Peak



Note: The figure shows the distribution of returns since purchase (top panel) and returns since peak (bottom panel). Observations include all investor × stock × days on which the investor sold at least one stock in his portfolio. Outliers below the first and above the 99th percentiles of returns are excluded.

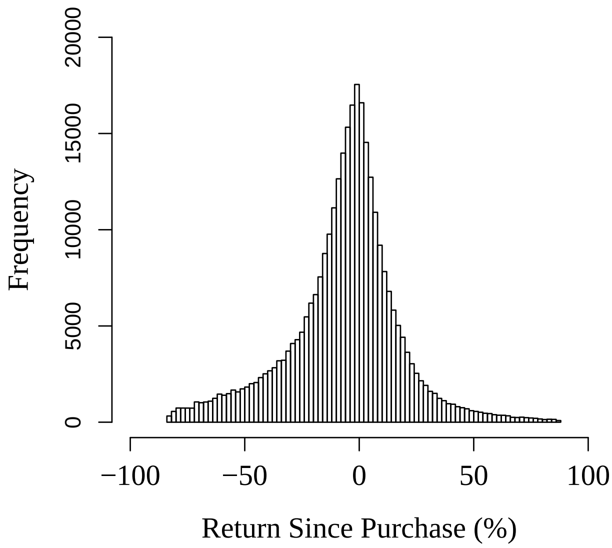
Figure A.10: Example of Peak Prices, Stock Data, Month Definition



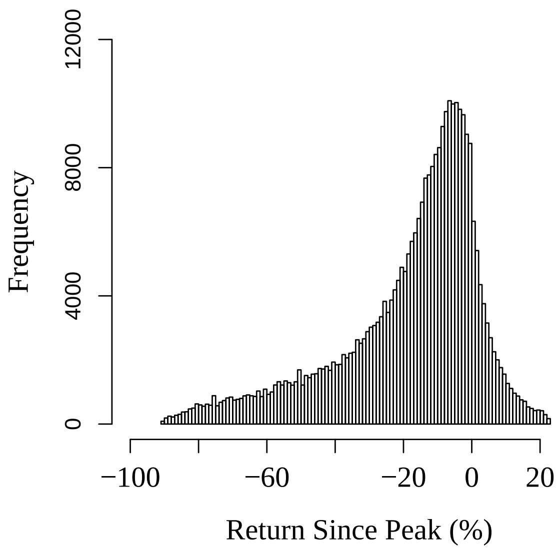
Note: The figure shows the sequence of peak prices under an alternative definition of peaks. In the figure, a peak price remains as the highest price (since purchase) for at least a month, instead of a week (as in the main results of the paper, see Figure 2.1 Panel B).

Figure A.11: Stock Returns Since Purchase and Returns Since Peak (Month Definition)

(A) Stock Returns Since Purchase



(B) Stock Returns Since Peak

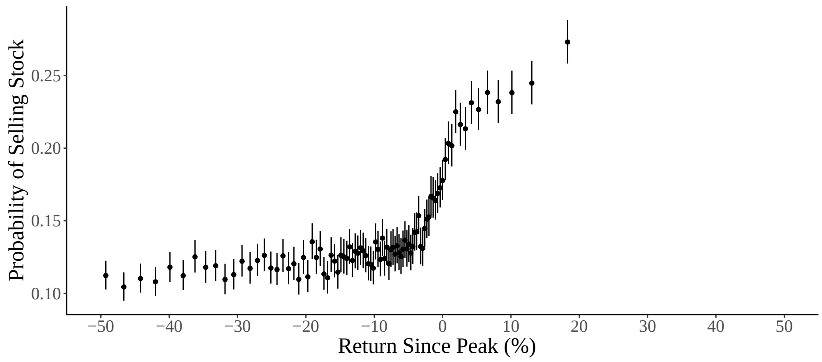


Note: The figure shows the distribution of returns since purchase (top panel) and returns since peak (bottom panel) under an alternative definition of peaks. In the figure, a peak price remains as the highest price (since purchase) for at least a month, instead of a week. Observations include all investor × stock × days on which the investor sold at least one stock in his portfolio. Outliers below the first and above the 99th percentiles of returns are excluded.

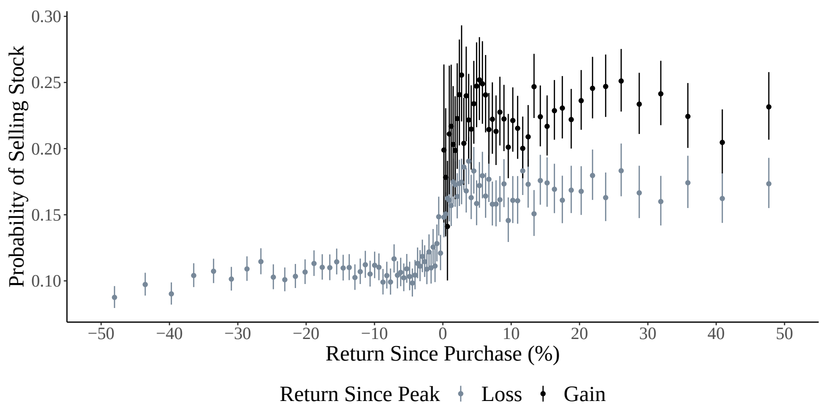
Figure A.12: Probability of Stock Sale, Returns Since Purchase and Returns Since Peak (Month Definition)

(A) Stock Returns Since Purchase

(B) Stock Returns Since Peak



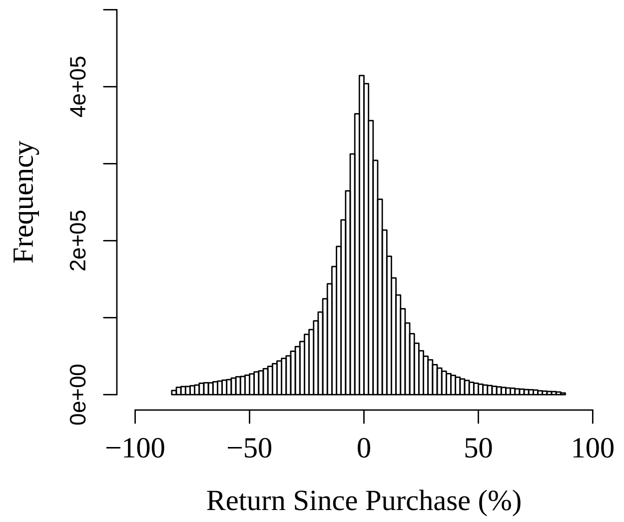
(C) Interaction of Stock Returns Since Purchase and Since Peak



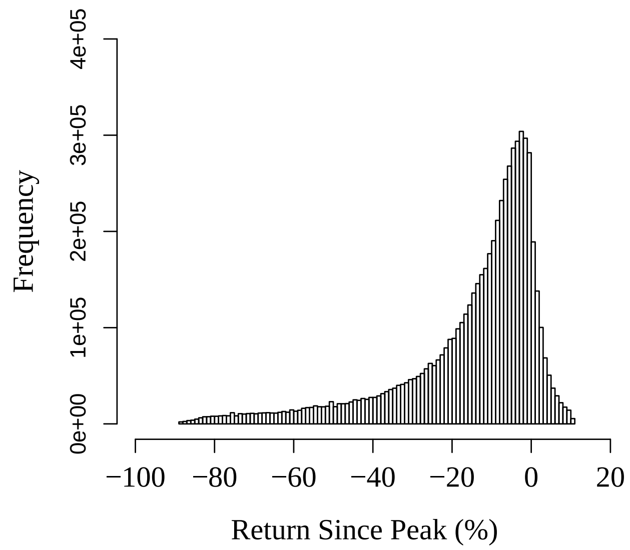
Note: The figure shows the probability of stocks sales against return since purchase and return since peak price. Peak prices use an alternative definition: a peak price remains as the highest price (since purchase) for at least a month, instead of a week. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -50% and 50%. The sample includes all investor × stock × days on which the investor sold at least one stock in his portfolio. Vertical lines represent 95% confidence intervals.

Figure A.13: Stock Returns Since Purchase and Returns Since Peak, Login-Day Sample

(A) Stock Returns Since Purchase Date



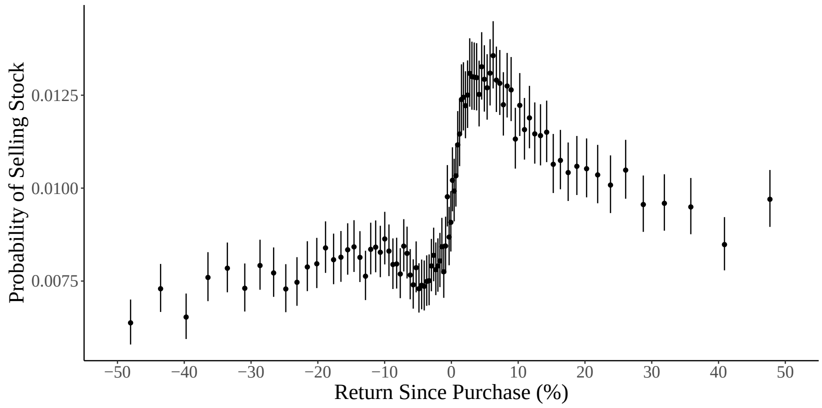
(B) Stock Returns Since Peak



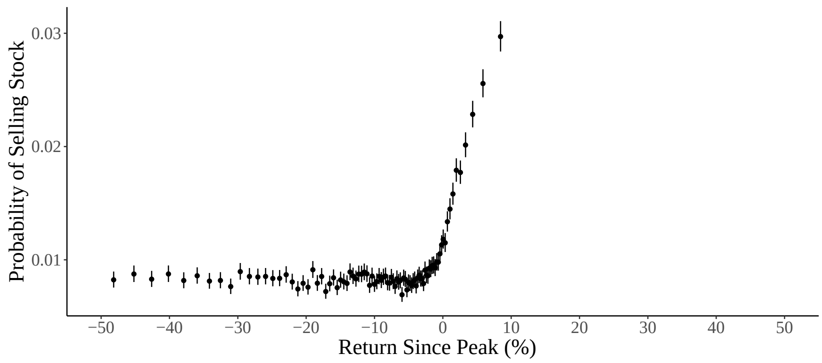
Note: The figure shows the distribution of returns since purchase (top panel) and returns since peak (bottom panel) for the Login-Day sample. Observations include all investor × stock × days for the set of days in which the investor made at least one login to his account. Outliers below the first and above the 99th percentiles of returns are excluded.

Figure A.14: Probability of Stock Sale, Returns Since Purchase and Returns Since Peak in the Login-Day Sample

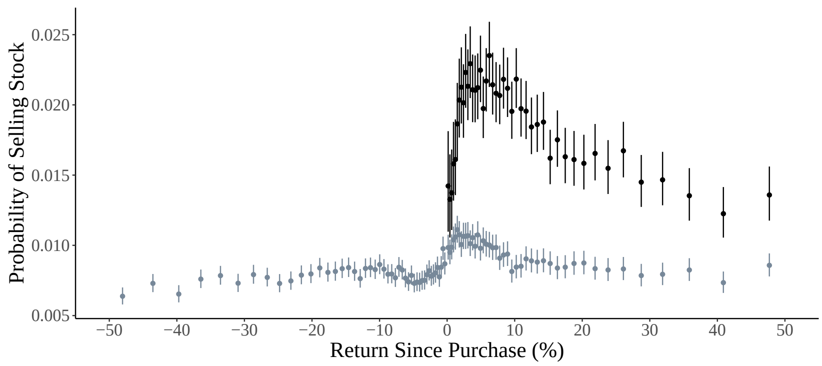
(A) Stock Returns Since Purchase



(B) Stock Returns Since Peak

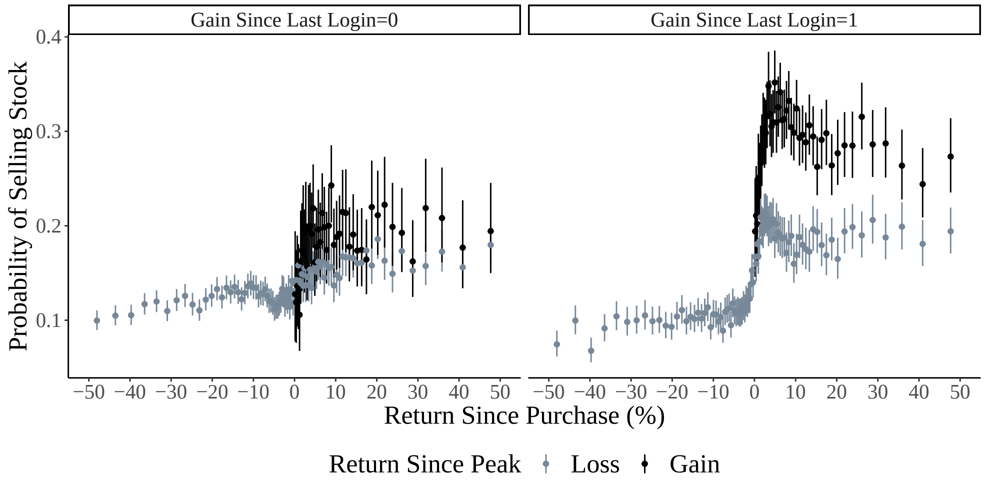


(C) Interaction of Stock Returns Since Purchase and Since Peak



Note: The figure shows the probability of stocks sales against return since purchase and return since peak price for the Login-Day sample. Panels display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), returns since peak price (Panel B) and the interaction between returns since purchase and returns since peak price (Panel C). For visual purposes, returns are restricted to the range of -50% and 50%. The sample includes all investor × stock × days for the set of day in which the investor made at least one login to his account. Vertical lines represent 95% confidence intervals.

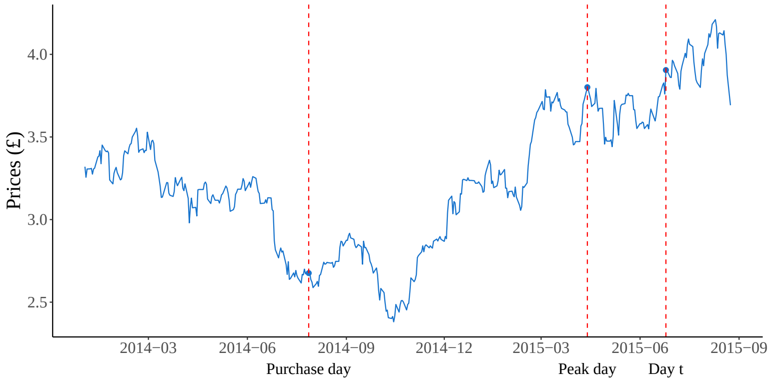
Figure A.15: Probability of Stock Sale, Returns Since Purchase, Returns Since Peak, and Returns Since Last Login Day



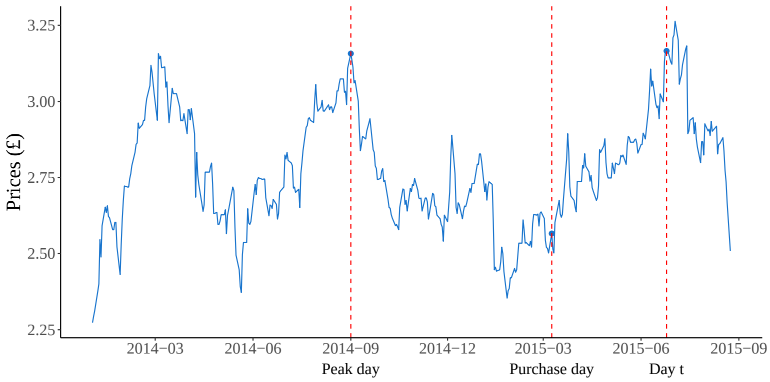
Note: The figure shows the probability of stocks sales against return since purchase, return since peak price, and return since last login day. This figure reproduces Figure A6 in Quispe-Torreblanca et al. (2021). Panels display binscatter plots. The left panel includes stocks in loss since the last login day, whereas the right panel, stocks in gain. For visual purposes, returns are restricted to the range of -50% and 50%. The sample includes all investor × stock × days for the set of day in which the investor sold at least one stock in the portfolio. Vertical lines represent 95% confidence intervals.

Figure A.16: Examples of Stocks Price Trajectories for the Placebo Analysis

(A) Holding Stock on Peak Day - Gain Since Purchase - Gain Since Peak



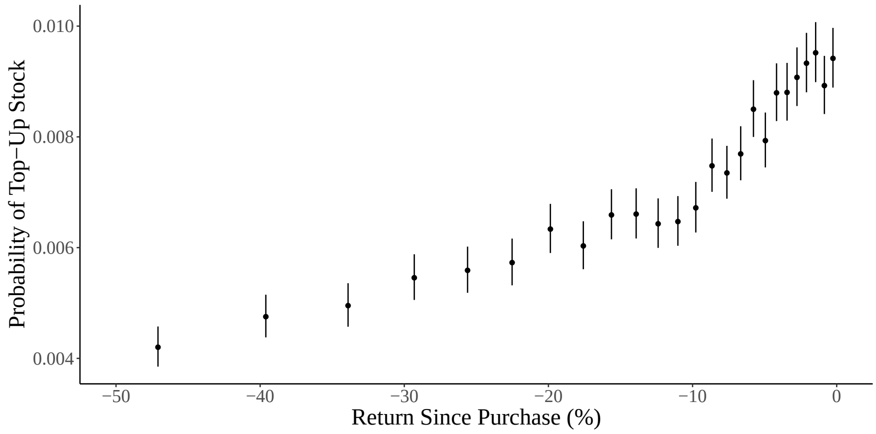
(B) Not Holding Stock on Peak Day - Gain Since Purchase - Gain Since Peak



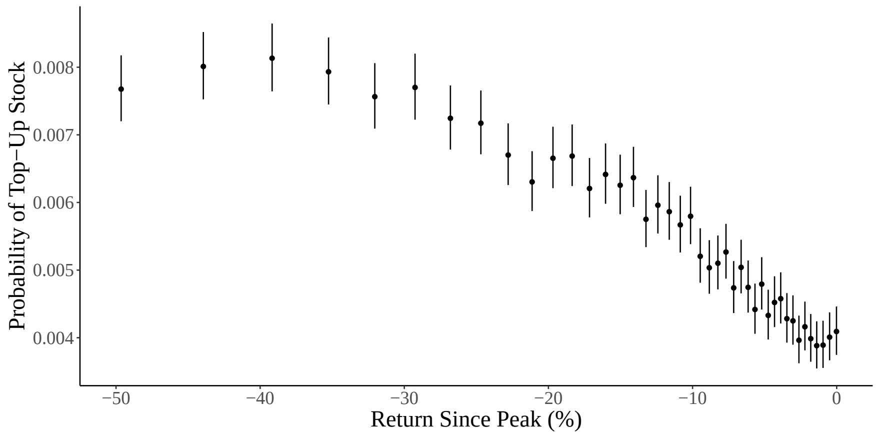
Note: The figure illustrate the placebo test that contrast the effect of peak prices that occurred before the purchase of the stock with those that occurred after the purchase of the stock. Across panels, peak prices are defined over the past year. The panels display examples of days with a gain since the past peak day and a gain since purchase (the day of evaluation is *Day t*, 2015-06-25) but distinguishing the case when the investor has held the stock during the past peak (Panel A) or hasn't held it (Panel B).

Figure A.17: Stock Top-Ups, Returns Since Purchase and Return Since Peak in the Login-Day Sample

(A) Stock Disposition Effect Since Purchase for Top-Ups



(B) Stock Disposition Effect Since Peak for Top-Ups



Note: The figure display binscatter plots to illustrate the relationship between the probability of sale and returns since purchase (Panel A), and returns since peak price (Panel B). Plots intend to test potential beliefs in reversion to the purchase price and peak price, respectively. As such, returns are restricted to the range of -50% and 0%. The sample includes all investor × stock × days on which the investor made at least one login to his account. Vertical lines represent 95% confidence intervals.

A.4 Supplementary Tables

Table A.1: Example of Trading Strategies for Different Reference Points With Prospect Theory Preferences

		Price at $t = 2$								
		Node -2			Node 0			Node +2		
		PT Value			PT Value			PT Value		
Price at $t = 1$	Reference point at $t = 2$	If sell at $t = 2$	If sell at $t = 3$	Decision at $t = 2$	If sell at $t = 2$	If sell at $t = 3$	Decision at $t = 2$	If sell at $t = 2$	If sell at $t = 3$	Decision at $t = 2$
$P_0 + 1$	$P_0 + 1$	-3.46	-3.41	Don't sell	-2	-1.41	Don't sell	1	0.71	Sell
$P_0 - 1$	P_0	-2.83	-2.73	Don't sell	0	-0.50	Sell	1.41	1.37	Sell

Note: The table illustrates selling strategies for different reference points in the model (illustrated in Figure A.1). In the simulation, the investor solves a value function $|p - r|^{\delta}$ for cases where $p - r > 0$ and a value function $-\lambda|p - r|^{\delta}$ for cases where $p - r < 0$. We conservatively choose parameters for risk aversion and loss aversion of $\delta = 0.5$ and $\lambda = 2$. In the model, the reference point is given by $r = \gamma p_1 + (1 - \gamma)p_o$, where γ takes a value of 1 if $p_1 > p_0$ and 0 otherwise.

Table A.2: Geographic Distribution of House Purchases

Panel (A): Full Sample

Region	%
East Midlands	8.25
East of England	11.28
London	13.37
North East	4.48
North West	12.28
South East	16.92
South West	10.64
Wales	4.83
West Midlands	8.98
Yorkshire and the Humber	8.97

Panel (B): Peak Sample

Region	%
East Midlands	8.18
East of England	10.71
London	13.62
North East	4.95
North West	12.61
South East	16.65
South West	10.37
Wales	5.17
West Midlands	8.69
Yorkshire and the Humber	9.05

Note: The table presents the distribution of sampled property purchases by location. Panel (A) includes the "Full sample" used in the regression estimates of our baseline specification. Panel (B) includes the "Peak sub-sample" used in the regression estimates of our modified baseline specification. England and Wales are nations within the United Kingdom, England is further sub-divided into 9 official regions.

Table A.3: Summary Statistics for Housing Return Since Purchase (Full Sample)

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Purchase (£)	77,919	163,400	-752	9,059	40,624	100,126	184,644
Return Since Purchase (%)	66.68	82.83	-0.61	7.36	31.79	105.73	187.46
Return Since Purchase >= 0 (%)	88.93						

Note: The table presents summary statistics for returns since purchase in the "Full sample". The sample includes all property × calendar quarters in the baseline sample. Returns are calculated at the calendar quarter level. Returns in the percentiles 1 and 99 are winsorized.

Table A.4: Summary Statistics for Housing Return Since Purchase (Peak Sub-Sample)

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Purchase (£)	105,272	204,608	-66	18,071	64,338	132,152	235,927
Return Since Purchase (%)	87.64	94.29	-0.05	13.22	53.29	147.99	216.55
Return Since Purchase >= 0 (%)	89.95						

Note: The table presents summary statistics for returns since purchase in the "Peak sub sample". The sample includes all property × calendar quarters with a peak price. Returns are calculated at the calendar quarter level. Returns in the percentiles 1 and 99 are winsorized.

Table A.5: Summary Statistics for Housing Return Since Peak Price

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Peak (£)	8,520	72,575	-31,458	-18,781	-5,008	14,783	64,216
Return Since Peak (%)	3.77	26.88	-15.5	-10.56	-2.46	8.06	28.95
Price Peaks Per Property	1.73	0.94	1	1	1	2	3
Return Since Peak >= 0 (%)	40.04						

Note: The table presents summary statistics for returns since peak in the "Peak sub sample". The sample includes all property × calendar quarters with a peak price. Returns are calculated at the calendar quarter level. Returns in the percentiles 1 and 99 are winsorized.

Table A.6: Summary Statistics for Stock Returns Since Purchase and Returns Since Peak Price

	Mean	SD	Median
Return Since Purchase			
Return Since Purchase (%)	−4.843	22.642	−2.243
Gain Since Purchase=1	0.423	0.494	0
Return Since Peak			
Returns Since Peak (%)	−16.197	19.197	−9.647
Gain Since Peak=1	0.120	0.325	0
N Investor × Stock × Day	396186		

Note: The table presents summary statistics for returns since purchase and returns since peak price in the sell-day sample and login-day samples. The sell-day sample includes all investor × stock × days on which the investor sold at least one position in the portfolio. Returns are calculated at the daily level. Returns below the percentile 1 and above the percentile 99 are excluded.

Table A.7: Summary Statistics for Housing Returns Since Purchase and Returns Since Peak (Two Quarters Definition)

Panel (A): Housing Returns Since Purchase

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Purchase (£)	99,763	179,616	1,268	17,975	61,144	125,821	222,860
Return Since Purchase (%)	83.71	90.72	1.09	13.37	49.21	140.68	209.49
Return Since Purchase >= 0 (%)	91.22						

Panel (B): Housing Returns Since Peak

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Peak (£)	5,210	55,555	-29,307	-16,491	-2,749	11,892	49,346
Return Since Peak (%)	3.96	26.39	-14.8	-9.01	-1.39	6.43	23.96
Price Peaks Per Property	2.54	1.59	1	1	2	3	5
Return Since Peak >= 0 (%)	42.06						

Note: The table presents summary statistics for housing returns since purchase (Panel A) and returns since peak (Panel B) under an alternative definition of peaks. A peak price remains the highest price (since purchase) for at least two quarters instead of three quarters. The sample includes all property × calendar quarters with a peak price. Returns in the percentiles 1 and 99 are winsorized.

Table A.8: Summary Statistics for Housing Returns Since Purchase and Returns Since Peak (Four Quarters Definition)

Panel (A): Housing Returns Since Purchase

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Purchase (£)	109,243	213,218	-1,258	18,707	67,081	136,807	244,977
Return Since Purchase (%)	90.85	96.76	-0.92	13.3	57.74	152.33	221.55
Return Since Purchase >= 0 (%)	89						

Panel (B): Housing Returns Since Peak

			Percentiles				
	Mean	SD	10th	25th	50th	75th	90th
Return Since Peak (£)	10,983	89,798	-32,740	-20,122	-7,052	17,394	73,448
Return Since Peak (%)	2.76	23.92	-16	-11.46	-3.64	9.2	30.95
Price Peaks Per Property	1.4	0.66	1	1	1	2	2
Return Since Peak >= 0 (%)	38.57						

Note: The table presents summary statistics for housing returns since purchase (Panel A) and returns since peak (Panel B) under an alternative definition of peaks. A peak price remains the highest price (since purchase) for at least four quarters instead of three quarters. The sample includes all property × calendar quarters with a peak price. Returns in the percentiles 1 and 99 are winsorized.

Table A.9: Summary Statistics for Stock Returns Since Purchase and Returns Since Peak Price (Month-Peak)

	Mean	SD	Median
Return Since Purchase			
Return Since Purchase (%)	−6.452	24.348	−3.901
Gain Since Purchase=1	0.393	0.489	0
Return Since Peak			
Returns Since Peak (%)	−18.300	21.001	−12.371
Gain Since Peak=1	0.134	0.340	0
N Investor × Stock × Day	312233		

Note: The table presents summary statistics for returns since purchase and returns since peak price in the sell-day sample and login-day samples. The sell-day sample includes all investor × stock × days on which the investor sold at least one position in the portfolio. Returns below the percentile 1 and above the percentile 99 are excluded.

Table A.10: OLS Estimates of the Housing Disposition Effect by Market Liquidity

Panel (A): Above Median Sales Volume

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0064***	0.0078***	0.0051***
	(0.0007)	(0.0007)	(0.0005)
Years From Purchase		0.0083***	0.0085***
		(0.0005)	(0.0005)
Return Since Purchase > 0 (£100,000)			0.0005**
			(0.0002)
Return Since Purchase < 0 (£100,000)			0.0324***
			(0.0033)
Constant	0.0074***	0.0037***	0.0063***
	(0.0003)	(0.0005)	(0.0004)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	64,165,601	64,165,601	64,165,601
R^2	0.0002	0.0021	0.0021

Panel (B): Below Median Sales Volume

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0039***	0.0057***	0.0043***
	(0.0004)	(0.0005)	(0.0004)
Years From Purchase		0.0064***	0.0066***
		(0.0005)	(0.0005)
Return Since Purchase > 0 (£100,000)			0.0004***
			(0.0001)
Return Since Purchase < 0 (£100,000)			0.0125***
			(0.0016)
Constant	0.0067***	0.0033***	0.0047***
	(0.0003)	(0.0003)	(0.0003)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	64,135,150	64,135,150	64,135,150
R^2	0.0002	0.0013	0.0014

Note: The table presents ordinary least squares regression estimates for our baseline specification for separate samples split by liquidity. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added in Columns 2 and 3 correspond to Table 2.5, Column 2, and Table 2.6, Column 1, respectively. Observations includes all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.11: OLS Estimates of the Housing Disposition Effect by Market Size

Panel (A): Above Median Stock

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0055***	0.0081***	0.0064***
	(0.0006)	(0.0008)	(0.0007)
Years From Purchase		0.0066***	0.0068***
		(0.0006)	(0.0005)
Return Since Purchase > 0 (£100,000)			0.0004**
			(0.0002)
Return Since Purchase < 0 (£100,000)			0.0153***
			(0.0018)
Constant	0.0067***	0.0026***	0.0043***
	(0.0003)	(0.0005)	(0.0004)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	51,045,213	51,045,213	51,045,213
R^2	0.0003	0.0017	0.0017

Panel (B): Below Median Stock

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0055***	0.0077***	0.0059***
	(0.0006)	(0.0008)	(0.0006)
Years From Purchase		0.0088***	0.0089***
		(0.0007)	(0.0006)
Return Since Purchase > 0 (£100,000)			0.0010***
			(0.0003)
Return Since Purchase < 0 (£100,000)			0.0143***
			(0.0018)
Constant	0.0074***	0.0018***	0.0039***
	(0.0003)	(0.0005)	(0.0004)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	51,042,566	51,042,566	51,042,566
R^2	0.0003	0.0018	0.0019

Note: The table presents ordinary least squares regression estimates for our baseline specification for separate samples split by market size. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added in Columns 2 and 3 correspond to Table 2.5, Column 2, and Table 2.6, Column 1, respectively. Observations includes all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.12: OLS Estimates of the Housing Disposition Effect by Leverage

Panel (A): Above Median LTV%

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0031***	0.0039***	0.0024***
	(0.0006)	(0.0007)	(0.0005)
Years From Purchase		0.0058***	0.0059***
		(0.0007)	(0.0007)
Return Since Purchase > 0 (£100,000)			0.0005
			(0.0004)
Return Since Purchase < 0 (£100,000)			0.0157***
			(0.0017)
Constant	0.0073***	0.0028***	0.0043***
	(0.0005)	(0.0006)	(0.0004)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	50,495,217	50,495,217	50,495,217
R^2	0.0001	0.0012	0.0012

Panel (B): Below Median LTV%

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0039***	0.0062***	0.0052***
	(0.0006)	(0.0009)	(0.0008)
Years From Purchase		0.0060***	0.0061***
		(0.0005)	(0.0005)
Return Since Purchase > 0 (£100,000)			0.0005***
			(0.0002)
Return Since Purchase < 0 (£100,000)			0.0076***
			(0.0016)
Constant	0.0065***	0.0012*	0.0024***
	(0.0003)	(0.0007)	(0.0007)
Years From Purchase Quintics	NO	YES	YES
Property Characteristics	NO	YES	YES
Observations	50,512,021	50,512,021	50,512,021
R^2	0.0002	0.0013	0.0013

Note: The table presents ordinary least squares regression estimates for our baseline specification for separate samples split by leverage. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added in Columns 2 and 3 correspond to Table 2.5, Column 2, and Table 2.6, Column 1, respectively. Observations includes all property × quarters in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.13: OLS Estimates of the Housing Peak and Purchase Disposition Effects by Market Liquidity

Panel (A): Above Median Sales Volume

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0050***		0.0031***
	(0.0006)		(0.0004)
Return Since Purchase > 0 (£100,000)	0.0001		0.0004***
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0152***		0.0100***
	(0.0021)		(0.0021)
Gain Since Peak = 1		0.0039***	0.0036***
		(0.0006)	(0.0005)
Return Since Peak > 0 (£100,000)		-0.0019***	-0.0020***
		(0.0003)	(0.0003)
Return Since Peak < 0 (£100,000)		0.0064***	0.0049***
		(0.0018)	(0.0018)
Years From Purchase	0.0075***	0.0068***	0.0066***
	(0.0010)	(0.0009)	(0.0009)
Constant	0.0012	0.0033***	0.0021*
	(0.0014)	(0.0013)	(0.0012)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	29,673,895	29,673,895	29,673,895
R^2	0.0011	0.0013	0.0014

Panel (B): Below Median Sales Volume

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0030***		0.0020***
	(0.0004)		(0.0003)
Return Since Purchase > 0 (£100,000)	-0.0000		0.0000
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0095***		0.0062***
	(0.0013)		(0.0018)
Gain Since Peak = 1		0.0033***	0.0031***
		(0.0004)	(0.0004)
Return Since Peak > 0 (£100,000)		-0.0017***	-0.0015***
		(0.0003)	(0.0004)
Return Since Peak < 0 (£100,000)		0.0038***	0.0028*
		(0.0014)	(0.0015)
Years From Purchase	0.0047***	0.0046***	0.0044***
	(0.0006)	(0.0006)	(0.0006)
Constant	0.0037***	0.0040***	0.0039***
	(0.0009)	(0.0010)	(0.0008)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	42,352,251	42,352,251	42,352,251
R^2	0.0006	0.0008	0.0008

Note: The table presents ordinary least squares regression estimate for our modified baseline specification for separate samples split by liquidity. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added correspond to Table 2.9. Observations include all property × quarters with a peak price in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.14: OLS Estimates of the Housing Peak and Purchase Disposition Effects by Market Size

Panel (A): Above Median Stock

	$Sale_{it}$ (1)	(2)	(3)
Gain Since Purchase = 1	0.0042***		0.0027***
	(0.0006)		(0.0004)
Return Since Purchase > 0 (£100,000)	-0.0001		0.0000
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0097***		0.0058***
	(0.0017)		(0.0020)
Gain Since Peak = 1		0.0045***	0.0041***
		(0.0006)	(0.0006)
Return Since Peak > 0 (£100,000)		-0.0020***	-0.0017***
		(0.0002)	(0.0003)
Return Since Peak < 0 (£100,000)		0.0045***	0.0036**
		(0.0014)	(0.0014)
Years From Purchase	0.0063***	0.0060***	0.0058***
	(0.0009)	(0.0007)	(0.0007)
Constant	0.0002	0.0011	0.0008
	(0.0012)	(0.0012)	(0.0010)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	29,453,534	29,453,534	29,453,534
R^2	0.0008	0.0011	0.0012

Panel (B): Below Median Stock

	$Sale_{it}$ (1)	(2)	(3)
Gain Since Purchase = 1	0.0040***		0.0025***
	(0.0006)		(0.0004)
Return Since Purchase > 0 (£100,000)	0.0003		0.0003
	(0.0002)		(0.0002)
Return Since Purchase < 0 (£100,000)	0.0110***		0.0059***
	(0.0015)		(0.0021)
Gain Since Peak = 1		0.0039***	0.0036***
		(0.0007)	(0.0007)
Return Since Peak > 0 (£100,000)		-0.0025***	-0.0024***
		(0.0004)	(0.0004)
Return Since Peak < 0 (£100,000)		0.0061***	0.0049***
		(0.0017)	(0.0017)
Years From Purchase	0.0058***	0.0056***	0.0054***
	(0.0009)	(0.0008)	(0.0007)
Constant	0.0036***	0.0044***	0.0042***
	(0.0013)	(0.0015)	(0.0012)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	29,608,906	29,608,906	29,608,906
R^2	0.0008	0.0010	0.0011

Note: The table presents ordinary least squares regression estimates for our modified baseline specification for separate samples split by market size. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added correspond to Table 2.9. Observations include all property × quarters with a peak price in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.15: OLS Estimates of the Housing Peak and Purchase Disposition Effects by Leverage

Panel (A): Above Median LTV%

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0024***		0.0015***
	(0.0004)		(0.0004)
Return Since Purchase > 0 (£100,000)	-0.0002		-0.0001
	(0.0002)		(0.0002)
Return Since Purchase < 0 (£100,000)	0.0139***		0.0059
	(0.0015)		(0.0043)
Gain Since Peak = 1		0.0021***	0.0020***
		(0.0004)	(0.0004)
Return Since Peak > 0 (£100,000)		-0.0019***	-0.0016***
		(0.0005)	(0.0005)
Return Since Peak < 0 (£100,000)		0.0086***	0.0073**
		(0.0028)	(0.0032)
Years From Purchase	0.0055***	0.0050***	0.0048***
	(0.0009)	(0.0008)	(0.0008)
Constant	0.0027***	0.0045***	0.0040***
	(0.0009)	(0.0009)	(0.0008)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	33,570,287	33,570,287	33,570,287
R^2	0.0007	0.0009	0.0009

Panel (B): Below Median LTV%

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0033***		0.0027***
	(0.0006)		(0.0005)
Return Since Purchase > 0 (£100,000)	0.0002*		0.0002**
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0067***		0.0054***
	(0.0012)		(0.0015)
Gain Since Peak = 1		0.0037***	0.0036***
		(0.0006)	(0.0006)
Return Since Peak > 0 (£100,000)		-0.0012***	-0.0012***
		(0.0003)	(0.0003)
Return Since Peak < 0 (£100,000)		0.0021	0.0013
		(0.0013)	(0.0013)
Years From Purchase	0.0042***	0.0045***	0.0045***
	(0.0007)	(0.0007)	(0.0006)
Constant	0.0031***	0.0035**	0.0034***
	(0.0011)	(0.0014)	(0.0010)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	35,638,330	35,638,330	35,638,330
R^2	0.0007	0.0008	0.0009

Note: The table presents ordinary least squares regression estimates for our modified baseline specification for separate samples split by leverage. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added correspond to Table 2.9. Observations include all property × quarters with a peak price in the baseline sample. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.16: OLS Estimates of the Housing Peak and Purchase Disposition Effects (Two Quarters Definition)

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0051***		0.0032***
	(0.0006)		(0.0004)
Return Since Purchase > 0 (£100,000)	0.0003**		0.0002*
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0131***		0.0078***
	(0.0016)		(0.0020)
Gain Since Peak = 1		0.0044***	0.0040***
		(0.0006)	(0.0006)
Return Since Peak > 0 (£100,000)		-0.0019***	-0.0017***
		(0.0005)	(0.0005)
Return Since Peak < 0 (£100,000)		0.0061***	0.0044***
		(0.0016)	(0.0016)
Years From Purchase	0.0077***	0.0068***	0.0068***
	(0.0008)	(0.0007)	(0.0007)
Constant	0.0013	0.0039***	0.0022***
	(0.0009)	(0.0008)	(0.0007)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	86,792,483	86,792,483	86,792,483
R^2	0.0011	0.0014	0.0015

Note: This table presents ordinary least squares regression estimates for our modified baseline specification under an alternative definition of peaks. A peak price remains the highest price (since purchase) for at least two quarters instead of three quarters. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added correspond to Table 2.9. Observations include all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.17: OLS Estimates of the Housing Peak and Purchase Disposition Effects (Four Quarters Definition)

	$Sale_{it}$		
	(1)	(2)	(3)
Gain Since Purchase = 1	0.0029***		0.0019***
	(0.0004)		(0.0003)
Return Since Purchase > 0 (£100,000)	-0.0001		0.0001
	(0.0001)		(0.0001)
Return Since Purchase < 0 (£100,000)	0.0108***		0.0072***
	(0.0014)		(0.0017)
Gain Since Peak = 1		0.0033***	0.0031***
		(0.0004)	(0.0004)
Return Since Peak > 0 (£100,000)		-0.0016***	-0.0015***
		(0.0002)	(0.0003)
Return Since Peak < 0 (£100,000)		0.0041***	0.0033**
		(0.0014)	(0.0015)
Years From Purchase	0.0040***	0.0045***	0.0039***
	(0.0007)	(0.0007)	(0.0006)
Constant	0.0043***	0.0038***	0.0046***
	(0.0011)	(0.0013)	(0.0010)
Years From Purchase Quintics	YES	YES	YES
Property Characteristics	YES	YES	YES
Observations	62,965,529	62,965,529	62,965,529
R^2	0.0006	0.0008	0.0008

Note: This table presents ordinary least squares regression estimates for our modified baseline specification under an alternative definition of peaks. A peak price remains the highest price (since purchase) for at least four quarters instead of three quarters. The dependent variable takes a value of 1 if the homeowner sold their property and zero otherwise. The controls added correspond to Table 2.9. Observations include all property × quarters in the baseline sample with a peak price. Standard errors are clustered by property and year-quarter. *p<0.1; **p<0.05; ***p<0.01.

Table A.18: Estimates of the Disposition Effect for Stocks Including Portfolio and Demographic Controls (Month-Peak)

	$Sale_{ijt}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Gain Since Purchase=1	0.0624***	0.0648***	0.0705***	0.0720***	0.0686***	0.0696***	0.0695***	0.0699***	0.0698***	0.0759***
	(0.0039)	(0.0038)	(0.0040)	(0.0040)	(0.0038)	(0.0038)	(0.0038)	(0.0037)	(0.0037)	(0.0037)
Gain Since Peak=1	0.0589***	0.0553***	0.0534***	0.0532***	0.0506***	0.0510***	0.0511***	0.0511***	0.0524***	0.0586***
	(0.0043)	(0.0043)	(0.0043)	(0.0043)	(0.0042)	(0.0042)	(0.0042)	(0.0042)	(0.0041)	(0.0041)
Days Since Purchase (100 days)		-0.0039***	-0.0074***	-0.0068***	-0.0075***	-0.0065***	-0.0065***	-0.0063***	-0.0028***	0.0013
		(0.0008)	(0.0011)	(0.0011)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
Days Since Peak (100 days)			0.0060***	0.0056***	0.0064***	0.0064***	0.0064***	0.0063***	0.0055***	0.0031***
			(0.0013)	(0.0013)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0011)	(0.0011)
Portfolio Value (£10000)				-0.0013***	-0.0006***	-0.0005**	-0.0005**	-0.0005***	-0.0014***	-0.0015***
				(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0004)
Number of Stocks (10 stocks)					-0.0323***	-0.0330***	-0.0330***	-0.0323***	-0.0075	-0.0074
					(0.0083)	(0.0085)	(0.0084)	(0.0080)	(0.0069)	(0.0069)
Account Tenure (years)						-0.0076***	-0.0074***	-0.0057**		
						(0.0026)	(0.0027)	(0.0025)		
Female=1							-0.0088	-0.0029		
							(0.0055)	(0.0050)		
Age (10 years)								-0.0123***		
								(0.0016)		
Constant	0.0991***	0.1083***	0.1050***	0.1155***	0.1590***	0.1759***	0.1768***	0.2362***		
	(0.0038)	(0.0047)	(0.0046)	(0.0044)	(0.0093)	(0.0129)	(0.0126)	(0.0138)		
Account FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Stock FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	306,015	306,015	306,015	306,015	306,015	306,015	306,015	306,015	306,015	306,015
R^2	0.0168	0.0174	0.0176	0.0238	0.0425	0.0428	0.0429	0.0454	0.1344	0.1583

Note: The table presents ordinary least squares regression estimates of the baseline model with the addition of demographic controls and (daily level) portfolio controls. Peak prices use an alternative definition: a peak price remains as the highest price (since purchase) for at least a month, instead of a week. The sample includes all investor × stock × days on which the investor sold at least one stock. Outliers (investor × stock × days) below the first and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.19: Estimates of the Disposition Effect for Stocks Including Portfolio and Demographic Controls (Login-Days)

	$Sale_{ijt}$									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Gain Since Purchase=1	0.0016***	0.0021***	0.0030***	0.0031***	0.0030***	0.0032***	0.0032***	0.0032***	0.0052***	0.0058***
	(0.0002)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Gain Since Peak=1	0.0088***	0.0073***	0.0071***	0.0071***	0.0070***	0.0071***	0.0071***	0.0071***	0.0066***	0.0068***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Days Since Purchase (100 days)		-0.0011***	-0.0015***	-0.0014***	-0.0014***	-0.0013***	-0.0013***	-0.0013***	0.0000	0.0002***
		(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Days Since Peak (100 days)			0.0007***	0.0007***	0.0007***	0.0007***	0.0007***	0.0007***	0.0005***	0.0003***
			(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Portfolio Value (£10000)				-0.0001***	-0.0001**	-0.0001**	-0.0001**	-0.0000*	-0.0006***	-0.0006***
				(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)
Number of Stocks (10 stocks)					-0.0020***	-0.0021***	-0.0021***	-0.0021***	0.0031***	0.0032***
					(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0007)	(0.0007)
Account Tenure (years)						-0.0010***	-0.0010***	-0.0010***		
						(0.0002)	(0.0002)	(0.0002)		
Female=1							-0.0003	-0.0001		
							(0.0004)	(0.0004)		
Age (10 years)								-0.0008***		
								(0.0001)		
Constant	0.0070***	0.0098***	0.0093***	0.0099***	0.0118***	0.0141***	0.0142***	0.0180***		
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0005)	(0.0007)	(0.0007)	(0.0009)		
Account FE	NO	NO	NO	NO	NO	NO	NO	NO	YES	YES
Stock FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	YES
Observations	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870	6,133,870
R^2	0.0011	0.0019	0.0019	0.0021	0.0026	0.0026	0.0026	0.0028	0.0360	0.0383

Note: The table presents ordinary least squares regression estimates of the baseline model with the addition of demographic controls and (daily level) portfolio controls. The sample includes all investor × stock × days for the set of days in which the investor made at least one login to his account. Outliers (investor × stock × days) below the first and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.20: Cox Proportional Hazard Model Estimates of the Stocks Disposition Effect (Week-Peak)

	$Sale_{ijt}$		
	(1)	(2)	(3)
Gain Since Purchase=1	0.5611***		0.3231***
	(0.0110)		(0.0125)
Gain Since Peak=1		0.8608***	0.6702***
		(0.0132)	(0.0149)
Observations	336,472	336,472	336,472
R^2	0.0078	0.0117	0.0137

Note: The table presents Cox Proportional Hazard regression estimates of our baseline model with time varying covariates. The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. Coefficients show stratified estimates by account. That is, coefficients are equal across accounts but baseline hazard functions are unique to each account. In the model, we count every purchase of a stock as the beginning of a new position, and we assume a position ends on the date the investor first sells part or all of his holdings. The sample includes all investor × stock × days on which the investor sold at least one stock in the portfolio. Standard errors are clustered by account. *p<0.1; **p<0.05; ***p<0.01.

Table A.21: Estimates of the Stocks Disposition Effect
Excluding Partial Sells

	Complete Sale$_{ijt}$					
	(1)	(2)	(3)	(4)	(5)	(6)
Gain Since Purchase=1	0.0682***		0.0435***	0.0655***		0.0444***
	(0.0040)		(0.0033)	(0.0037)		(0.0028)
Gain Since Peak=1		0.1159***	0.0866***		0.1017***	0.0747***
		(0.0061)	(0.0048)		(0.0057)	(0.0047)
Days from Purchase Day (100 days)	-0.0097***	-0.0067***	-0.0076***	-0.0039***	-0.0018***	-0.0020***
	(0.0007)	(0.0007)	(0.0007)	(0.0005)	(0.0005)	(0.0005)
Constant	0.1031***	0.1121***	0.0989***			
	(0.0043)	(0.0042)	(0.0043)			
Account FE	NO	NO	NO	YES	YES	YES
Observations	396,186	396,186	396,186	396,186	396,186	396,186
R^2	0.0149	0.0175	0.0212	0.1545	0.1556	0.1590

Note: The table presents ordinary least squares regression estimates of our main specification. The dependent variable takes a value of 1 if the investor made a complete sale of the stock and zero otherwise. The sample includes all investor × stock × days on which the investor sold at least one stock in the portfolio. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.22: The Stocks Disposition Effect:
Sub-Sample Analysis, Sell-Day Sample, Week-Peak

	Gain Since Purchase		Gain Since Peak		Constant	
FTSE 100 Index						
Return in $t-1>0$	0.0622***	(0.0041)	0.0996***	(0.0058)	0.1081***	(0.0041)
Return in $t-1<0$	0.0516***	(0.0045)	0.0780***	(0.0065)	0.1193***	(0.0047)
Days Since Peak						
Below Median	0.0306***	(0.0046)	0.0876***	(0.0051)	0.1387***	(0.0050)
Above Median	0.0740***	(0.0044)	0.1148***	(0.0082)	0.0989***	(0.0039)
Days Since Purchase						
Below Median	0.0554***	(0.0051)	0.0929***	(0.0055)	0.1288***	(0.0049)
Above Median	0.0625***	(0.0041)	0.0616***	(0.0060)	0.0983***	(0.0040)

Note: The table presents ordinary least squares regression estimates for separate samples. Each row reports coefficients and standard errors from a single regression in which the dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise, there are two covariates (returns since purchase and returns since peak) and an intercept term. The sample includes all investor × stock × days on which the investor sold at least one stock. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.23: The Stocks Disposition Effect:
Demographics Sub-Sample Analysis, Sell-Day Sample, Week-Peak

	Gain Since Purchase		Gain Since Peak		Constant	
Gender						
Female	0.0659***	(0.0101)	0.1117***	(0.0116)	0.0938***	(0.0099)
Male	0.0555***	(0.0041)	0.0867***	(0.0052)	0.1172***	(0.0046)
Age						
Below Median	0.0651***	(0.0046)	0.0953***	(0.0057)	0.1178***	(0.0052)
Above Median	0.0373***	(0.0071)	0.0788***	(0.0083)	0.1002***	(0.0060)
Account Tenure						
Below Median	0.0634***	(0.0056)	0.0914***	(0.0065)	0.1100***	(0.0062)
Above Median	0.0509***	(0.0052)	0.0895***	(0.0067)	0.1176***	(0.0051)
Portfolio Value						
Below Median	0.0880***	(0.0055)	0.1078***	(0.0064)	0.1538***	(0.0050)
Above Median	0.0377***	(0.0039)	0.0587***	(0.0057)	0.0709***	(0.0043)
Number of Stocks						
Below Median	0.0811***	(0.0044)	0.1078***	(0.0062)	0.1715***	(0.0032)
Above Median	0.0350***	(0.0036)	0.0430***	(0.0054)	0.0519***	(0.0031)

Note: The table presents ordinary least squares regression estimates for separate samples split by gender, age, trading experience and portfolio value. Each row reports coefficients and standard errors from a single regression in which the dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise, there are two covariates (returns since purchase and returns since peak) and an intercept term. The sample includes all investor × stock × days on which the investor sold at least one stock. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.24: Estimates of the Stocks Disposition Effect
Sub-samples by FTSE100 Returns Since Purchase (Week-Peak), Sell-Day Sample

	Market in Loss Since Purchase & Market in Loss Since Peak Sample			Market in Loss Since Purchase & Market in Gain Since Peak Sample		
		$Sale_{ijt}$			$Sale_{ijt}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Gain Since Purchase=1	0.0841***		0.0600***	0.0849***		0.0609***
	(0.0056)		(0.0050)	(0.0085)		(0.0087)
Gain Since Peak=1		0.1572***	0.1117***		0.1040***	0.0745***
		(0.0091)	(0.0075)		(0.0105)	(0.0108)
Days from Purchase Day (100 days)	-0.0091***	-0.0058***	-0.0071***	-0.0115***	-0.0077***	-0.0096***
	(0.0010)	(0.0010)	(0.0010)	(0.0027)	(0.0026)	(0.0027)
Constant	0.1335***	0.1427***	0.1295***	0.1299***	0.1559***	0.1271***
	(0.0055)	(0.0054)	(0.0055)	(0.0078)	(0.0077)	(0.0078)
Observations	184,423	184,423	184,423	15,203	15,203	15,203
R^2	0.0147	0.0147	0.0200	0.0138	0.0138	0.0191

Note: The table presents ordinary least squares regression estimates of our main specification for separate samples split by market movements.The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. The sample includes all investor × stock × days on which the investor sold at least one stock in the day. However, Columns 1 to 3 restrict the sample to days when the market was in loss since purchase and since past peak price; and Columns 4 to 6, to days when the market was in gain since past peak price. Standard errors are clustered by account and day. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Table A.25: Estimates of the Stocks Disposition Effect
Sub-samples by FTSE100 Returns Since Purchase (Week-Peak), Sell-Day Sample

	Market in Gain Since Purchase & Market in Loss Since Peak Sample			Market in Gain Since Purchase & Market in Gain Since Peak Sample		
		$Sale_{ijt}$			$Sale_{ijt}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Gain Since Purchase=1	0.0515***		0.0366***	0.0996***		0.0727***
	(0.0049)		(0.0046)	(0.0053)		(0.0045)
Gain Since Peak=1		0.0909***	0.0760***		0.1179***	0.0688***
		(0.0082)	(0.0079)		(0.0071)	(0.0057)
Days from Purchase Day (100 days)	-0.0055***	-0.0034***	-0.0040***	-0.0076***	-0.0049***	-0.0055***
	(0.0012)	(0.0012)	(0.0012)	(0.0011)	(0.0011)	(0.0011)
Constant	0.1347***	0.1521***	0.1313***	0.1225***	0.1414***	0.1186***
	(0.0060)	(0.0054)	(0.0060)	(0.0056)	(0.0054)	(0.0056)
Observations	57,757	57,757	57,757	138,803	138,803	138,803
R^2	0.0055	0.0078	0.0099	0.0212	0.0176	0.0250

Note: The table presents ordinary least squares regression estimates of our main specification for separate samples split by market movements.The dependent variable takes a value of 1 if the investor made a sale of the stock and zero otherwise. The sample includes all investor × stock × days on which the investor sold at least one stock in the day. However, Columns 1 to 3 restrict the sample to days when the market was in gain since purchase but in loss since past peak price; and Columns 4 to 6, to days when the market was in gain since purchase and since past peak price. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.26: Stock Top-Up Behaviour When Stocks are in Loss Since Purchase: OLS and Individual Fixed Effects Estimates

	$Top-Up_{ijt}$		
	(1)	(2)	(3)
Return Since Purchase < 0 (%)	0.0001***	0.0001***	0.0002***
	(0.0000)	(0.0000)	(0.0000)
Days Since Purchase (100 days)		-0.0007***	-0.0001**
		(0.0001)	(0.0001)
Portfolio Value (£10000)		0.0001***	0.0000
		(0.0000)	(0.0000)
Number of Stocks (10 stocks)		-0.0016***	-0.0008**
		(0.0005)	(0.0004)
Account Tenure (years)		-0.0006**	
		(0.0002)	
Female=1		-0.0017***	
		(0.0004)	
Age (10 years)		-0.0005***	
		(0.0001)	
Constant	0.0086***	0.0153***	
	(0.0003)	(0.0009)	
Account FE	NO	NO	YES
Stock FE	NO	NO	YES
Observations	3,374,734	3,306,124	3,306,124
R^2	0.0005	0.0015	0.0313

Note: The table presents ordinary least squares regression estimates for the likelihood to top-up an stock as a function of the return since purchase. Peaks are defined since the purchase of the stock. The dependent variable takes a value of 1 if the investor topped-up the stock and zero otherwise. The sample includes all investor × stock × days on which the investor made at least one login to his account and the stock had negative returns since purchase. Outliers (investor × stock × days) below the first and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.27: Top-Up Behaviour When Stocks are in Loss Since Past Peak Price: OLS and Individual Fixed Effects Estimates

	$Top - Up_{ijt}$		
	(1)	(2)	(3)
Return Since Peak < 0 (%)	-0.0001***	-0.0002***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)
Days Since Peak (100 days)		-0.0015***	-0.0011***
		(0.0001)	(0.0001)
Portfolio Value (£10000)		0.0001***	0.0000
		(0.0000)	(0.0000)
Number of Stocks (10 stocks)		-0.0014***	-0.0009**
		(0.0004)	(0.0004)
Account Tenure (years)		-0.0007***	
		(0.0002)	
Female=1		-0.0010***	
		(0.0003)	
Age (10 years)		-0.0003***	
		(0.0001)	
Constant	0.0046***	0.0104***	
	(0.0002)	(0.0007)	
Account FE	NO	NO	YES
Stock FE	NO	NO	YES
Observations	5,587,106	5,475,583	5,475,583
R^2	0.0003	0.0018	0.0248

Note: The table presents ordinary least squares regression estimates for the likelihood to top-up as a function of the return since peak price. Peaks are defined since the purchase of the stock. The dependent variable takes a value of 1 if the investor topped-up the stock and zero otherwise. The sample includes all investor × stock × days on which the investor made at least one login to the account and the stock has negative returns since past peak price. Outliers (investor × stock × days) below the first and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

Table A.28: Top-Up Behaviour When Stocks are in Loss Since Past Peak Price: Placebo Test (Peaks defined for the Past Year)

	$Top-Up_{ijt}$						
	No Holding Stock on Past Peak			Holding Stock on Past Peak			All Sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Return Since Peak < 0 (%)	-0.0001***	-0.0002***	-0.0001***	-0.0001***	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Holding Stock During Past Peak=1							-0.0007***
							(0.0002)
Days Since Peak (100 days)		-0.0005***	-0.0002		-0.0012***	-0.0008***	-0.0009***
		(0.0001)	(0.0001)		(0.0001)	(0.0001)	(0.0001)
Days Since Purchase (100 days)		-0.0042***	-0.0030***		-0.0002***	-0.0001***	-0.0004***
		(0.0002)	(0.0002)		(0.0000)	(0.0000)	(0.0000)
Portfolio Value (£10000)		0.0002***	0.0001		0.0001***	0.0001***	0.0001
		(0.0000)	(0.0001)		(0.0000)	(0.0000)	(0.0001)
Number of Stocks (10 stocks)		-0.0019***	-0.0012**		-0.0012***	-0.0007***	-0.0010**
		(0.0005)	(0.0006)		(0.0004)	(0.0003)	(0.0004)
Account Tenure (years)		-0.0006**			-0.0005***		
		(0.0003)			(0.0002)		
Female=1		-0.0016***			-0.0009***		
		(0.0005)			(0.0003)		
Age (10 years)		-0.0003**			-0.0003***		
		(0.0001)			(0.0001)		
Return Since Peak < 0 (%) × Holding Stock During Past Peak=1							-0.0000
							(0.0000)
Constant	0.0053***	0.0142***		0.0030***	0.0090***		
	(0.0003)	(0.0011)		(0.0002)	(0.0007)		
Account FE	NO	NO	YES	NO	NO	YES	YES
Stock FE	NO	NO	YES	NO	NO	YES	YES
Observations	2,788,283	2,730,549	2,730,549	2,971,126	2,913,287	2,913,287	5,643,836
R^2	0.0008	0.0034	0.0340	0.0005	0.0015	0.0301	0.0274

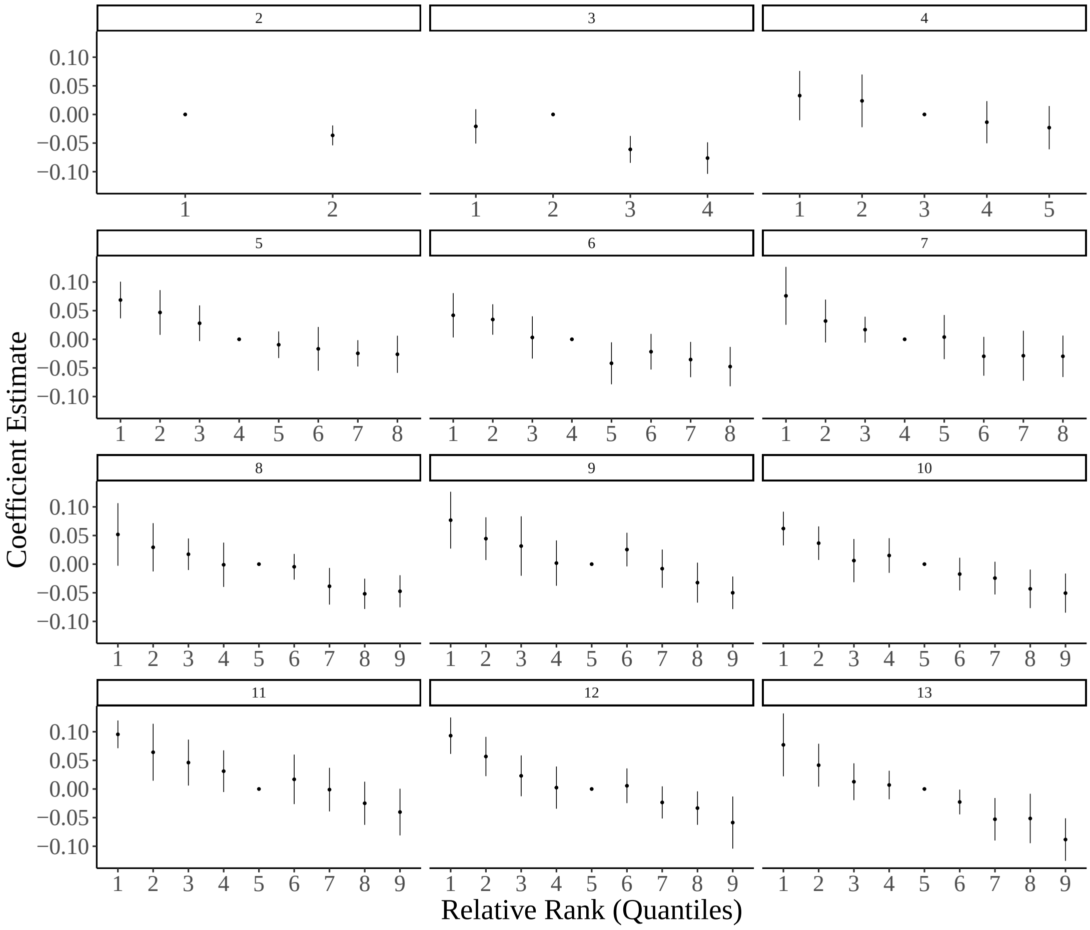
Note: This table presents ordinary least squares regression estimates for the likelihood to top-up an stock as a function of return since peak. Peaks are defined over the past year (rather than since purchase). The dependent variable takes a value of 1 if the investor topped-up the stock and zero otherwise. The sample includes all investor × stock × days on which the investor made at least one login to the account and the stock has negative returns since past peak price. Columns 1 to 3 subset the data to observation when the investor have not held the stock during the past peak. Columns 4 to 6, to observation when the investor held the stock in the past peak. Column 7 includes all observations and adds the interaction with the returns since past peak and the holding stock dummy. Outliers (investor × stock × days) below the first and above the 99th percentiles of daily portfolio values are excluded. Account tenure, gender and age (calculated from decades of birth) are within individual time invariant. Standard errors are clustered by account and day. *p<0.1; **p<0.05; ***p<0.01.

B Appendix: Uptown Top Ranking

Estimating Position Effects with Online Property Listings Data

B.1 Supplementary Figures

Figure B.1: Rank Effect Coefficient Estimates by Choice Set Size: OLS Model



Note: The figure illustrates the estimates of rank effect coefficients by quantile for choice set sizes from 2 to 13 members. For each choice set the estimates are derived from an OLS regression with a specification equivalent to column 4 of Table 3.2. Observations include all advert × days in the sample where the corresponding choice set has the applicable number of members. Vertical lines represent 95% confidence intervals.

C Appendix: The Limits of Nudge

Evidence from the Online Property Listings Market

C.1 Pre-registration

The analysis for the main study was pre-registered with AsPredicted on 17 October 2020 with reference number 49879 and title *Property listing salience WBS*. A copy of the pre-registration documentation is provided overleaf. The extensions were not pre-registered. Study 2 was not pre-registered but this quasi-experiment is a replication of the main study and the small differences in its econometric analysis are described in Section 4.5.1.

ASPREDICTED

Wharton CREDIBILITY LAB
UNIVERSITY of PENNSYLVANIA

Property listing salience WBS, October 2020 (#)

Created: 10/17/2020 11:03 AM (PT)
Public: MM/DD/YYYY HH:MM (PT)

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1) Have any data been collected for this study already?
No, no data have been collected for this study yet.

2) What's the main question being asked or hypothesis being tested in this study?
Does the salience of a property’s listing on a property portal website increase the likelihood of the property’s sale; reduce the time to sale and increase the sales price?

The study investigates the property portal's product called 'featuring’ which ensures a property listing (advert) is the first to be seen in any search matching the potential buyer's specified criteria.

3) Describe the key dependent variable(s) specifying how they will be measured.
1) Number of detailed website page views per day. Endpoints:
a) Featuring Period – 1week period starting from the date that the properties in that week's cohort are first featured
b) Early – 45 days after the property is first listed
c) Late – 170 days after the property is first listed
2) Number of email requests to estate agents per day, coded as a zero emails / one or more emails dummy. Endpoints as 3.1
3) Time to SSTC (Sale Subject to Contract). Number of days between the property's first date of listing and first SSTC received. End point 170 days after the final property to be listed is first listed.
4) Time to completed sale. Number of days between the property's first date of listing and sale is completed. End point 280 days after the final property to be listed is first listed.
5) Dummy variable for whether a property sells STC or not. Endpoints:
a) Early – 45 days after the property is first listed
b) Late – 170 days after the property is first listed
6) Dummy variable for whether a property sells or not:
a) Early – 190 days after the property is first listed
b) Late – 280 days after the property is first listed
7) Sales price as recorded in Land Registry data as fraction the of original price at first listing. End point 280 days after the property is first listed; properties that have not sold in time are missing data.

4) How many and which conditions will participants be assigned to?
Participating property listings will be randomly assigned to 1 of 2 conditions in the ratio 1:1: not featured at all (control group) or featured for 1 week.

5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.
1a, b, c) Detailed views. Log-linear OLS regression, with day and treatment dummies
2a, b, c) Email requests to agent dummy (no emails vs 1 or more email), OLS regression, with day and treatment dummies
3) Time to SSTC. Survival analysis (Kaplan Meier). Comparisons between treatment arms will be presented as Hazard Ratios using Cox regression model.
4) Time to completed sale. Survival analysis (Kaplan Meier). Comparisons between treatment arms will be presented as Hazard Ratios using a Cox regression model.
5 a, b) Whether a property sells STC. Linear probability model (OLS regression)
6 a, b) Whether a property sells. Linear probability model (OLS regression).
7) Sales price as proportion of list price. Log-linear OLS regression

For each dependent variable the following 2 regressions are carried out
A) The independent variable is a dummy variable indicating whether the listing was featured or not
B) As A) plus the below controls
Additionally, for just Time to SSTC and Time to completed sale
C) As B) with number of detailed website page views in the Feature Period as an additional covariate

Controls:
Timing: Set of dummy variables indicating which week the property was listed; number of days between the dates the property was first listed and the day

the properties in that cohort are first featured.
Estate agency: set of dummy variables indicating which estate agency is marketing the property
Property characteristics: list price prior to allocating to condition group; type of property (flat, bungalow, terraced, semi-detached, detached or unspecified house); number of bedrooms, (categorical variable 0 to 5 scale), UK region (categorical variable for the 11 official regions).
Listing characteristics: Premium listing (TRUE/FALSE), Floor plan available (TRUE/FALSE), Number of images (continuous variable)

6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.
The data for any participant who withdraws consent is excluded. Additionally:
1) Detailed views. Winsorize at the 99% percentile
2) None
3) Time to SSTC. Censor properties that delist or are not SSTC by trial endpoint (see 3.3)
4) Time to completed sale. Censor properties not sold by trial endpoint (see 3.4)
5 a, b) None
6 a, b) None
7) Sales price as proportion of list price. Winsorize at the 1% and 99% percentiles.

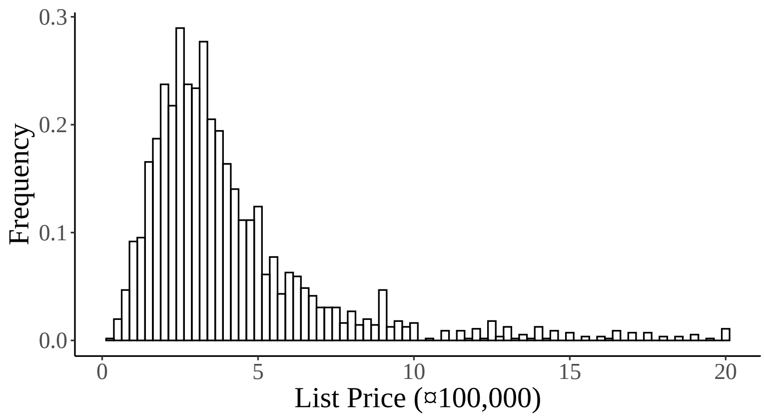
7) How many observations will be collected or what will determine sample size? No need to justify decision, but be precise about exactly how the number will be determined.
We continue to collect observations to the earlier of 1) up to and including the week that the sample size equals or exceeds 2000 or 2) our commercial partner, unilaterally decides to stop featuring properties for us, without examining the data.

8) Anything else you would like to pre-register? (e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)
Significance level 5%
The following listings are ineligible to participate in the study
* Rentals
* Properties previously delisted from the property portal within the last 14 weeks (Reloads)
* Sales status is not 'Available'
* Listing does not meet portals' data quality rules
* Business for sale listings
* Retirement properties
* Properties for auction
* Property type is not: bungalow; flat; terraced, semi-detached, detached and unspecified houses
* Property is a new development
* Selling price is not provided
* 0 bedrooms (unless flat) and no more than 5 bedrooms
* No full address
* Not already featured

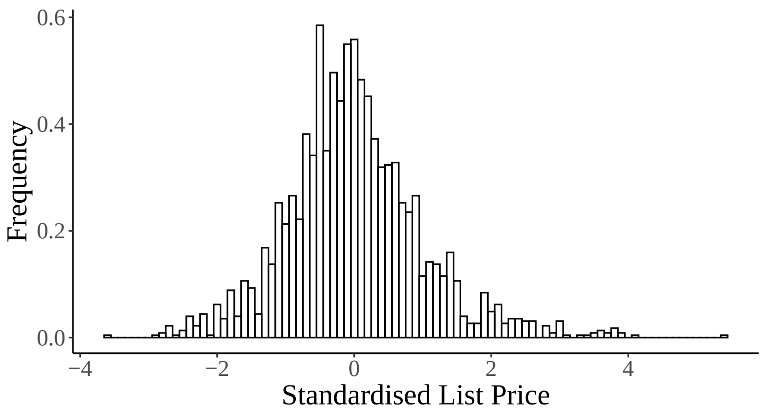
C.2 Supplementary Figures

Figure C.1: Frequency Distribution of List Prices

(A) List Price Before Standardisation



(B) List Price After Standardisation



Note: The figure shows the distribution of prices at first listing before and after standardization. Panel A shows prices before standardisation. For a better a visualisation an upper limit of £2,000,000 is applied. but no lower limit. The distribution shows a positive skew resulting from a relatively small number of highly priced properties. Panel B shows price after standardisation, where the mean of log list prices is deducted from the log of each list price and the result divided by the standard deviation of log list prices. No upper or lower limits are applied.

C.3 Supplementary Tables

Table C.1: Noncompliance With Research Protocol

	Number of Listings		
Description	Total	Control	Treated
Allocated to Incorrect Treatment Arm	0	0	0
Not Available for Sale at Allocation	0	0	0
Previously Listed in Preceding 14 Weeks	0	0	0
First Listed More Than 7 Days Before Allocation	0	0	0
New Development	1	1	0
Business for Sale	0	0	0
Retirement Property	5	3	2
Invalid Property Type	1	0	1
No Bedrooms but not a Flat	0	0	0
More Than 5 Bedrooms	1	1	0
List Price not Specified	0	0	0
Not Auction Property at Allocation	0	0	0
Data Quality Issue	2	2	0
Allocated to Control Arm but Treated	6	6	NA
Treated for More Than 8 Days	8	0	8

Note: The table shows the number of listings by total, control arm and treatment arm that were found to breach any research protocol (described in Section 4.2) following the treatment periods. In accordance with our attention to treat strategy we make no adjustments for any discrepancy.

Table C.2: Test of the Proportional Hazards Assumption

Panel (A): Sales Subject to Contract

Description	Chi-Squared	DF	P
Treatment Arm	0.20	1	0.65
Real Estate Agency	10.80	4	0.03
Allocation Week Number	8.91	7	0.26
List Price	0.13	1	0.72
Property Type	8.90	5	0.11
Bedrooms	17.81	5	0.00
Region	9.51	11	0.58
Time Between Listing and Allocation	9.12	6	0.17
Floor Plan Present	2.83	1	0.09
Images	12.07	5	0.03
Augmented Advert	1.74	1	0.19
Global Test	69.57	47	0.02

Panel (B): Sales

Description	Chi-Squared	DF	P
Treatment Arm	0.16	1	0.69
Real Estate Agency	24.24	4	0.00
Allocation Week Number	15.84	7	0.03
List Price	2.22	1	0.14
Property Type	36.97	5	0.00
Bedrooms	35.70	5	0.00
Region	19.50	10	0.03
Time Between Listing and Allocation	11.25	6	0.08
Floor Plan Present	0.00	1	0.96
Images	5.35	5	0.37
Augmented Advert	0.01	1	0.91
Global Test	106.54	46	0.00

Note: The tables present results of tests of the proportional hazards assumption that the relative hazard stays constant over time with different covariate levels and globally for A) SSTC and B) Sales. The correlation test is based on Schoenfeld residuals.

Table C.3: Treatment Effect on Email Requests for Further Information: OLS Estimates

	$EmailRequests_i$			
	(1)	(2)	(3)	(4)
Treated = 1	0.0304	0.0302	0.0289	0.0307
	(0.0199)	(0.0199)	(0.0197)	(0.0198)
Constant	0.3221***	0.3414***	0.2995***	0.7082***
	(0.0141)	(0.0325)	(0.0792)	(0.1897)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256
R^2	0.0010	0.0093	0.0460	0.0552

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if at least one email request was made to the property's real estate agent for further information during the treatment period and zero otherwise. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

Table C.4: Treatment Effect on Early Sale Subject to Contract: OLS Estimates

	$EarlySSTC_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0122	-0.0122	-0.0101	-0.0077
	(0.0202)	(0.0201)	(0.0196)	(0.0197)
Constant	0.3611***	0.3745***	0.4304***	0.6190***
	(0.0143)	(0.0328)	(0.0787)	(0.1891)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256
R^2	0.0002	0.0137	0.0789	0.0835

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold STC up to 45 days (see Table 4.5) after the property's first listing. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

Table C.5: Treatment Effect on Sale Subject to Contract: OLS Estimates

	$SSTC_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0055	-0.0055	0.0000	0.0027
	(0.0205)	(0.0205)	(0.0198)	(0.0199)
Constant	0.6176***	0.6245***	0.6676***	0.7296***
	(0.0145)	(0.0335)	(0.0795)	(0.1908)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256
R^2	0.0000	0.0094	0.0918	0.0980

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold STC up to 170 days (see Table 4.5) from the property's listing. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

Table C.6: Treatment Effect on Sale Subject to Contract: Cox Proportional Hazard Model Estimates

	$SSTC_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0648	-0.0672	-0.0625	-0.0587
	(0.0530)	(0.0531)	(0.0542)	(0.0545)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,256	2,256	2,256	2,256
R^2	0.0007	0.0049	0.0809	0.0933

Note: The table presents Cox Proportional Hazard regression estimates of our baseline survival model (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold STC up to 170 days after the final property to be listed is first listed. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample. *p<0.1; **p<0.05; ***p<0.01.

Table C.7: Treatment Effect on Early Sales: OLS Estimates

	$EarlySale_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0124	-0.0123	-0.0152	-0.0143
	(0.0198)	(0.0198)	(0.0193)	(0.0194)
Constant	0.3357***	0.3584***	0.2972***	0.2849
	(0.0140)	(0.0323)	(0.0774)	(0.1858)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249
R^2	0.0002	0.0123	0.0781	0.0831

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 190 days (see Table 4.5) from the property's first listing. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.8: Treatment Effect on Sales: OLS Estimates

	$Sale_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0450**	-0.0448**	-0.0462**	-0.0441**
	(0.0210)	(0.0209)	(0.0204)	(0.0205)
Constant	0.5521***	0.5640***	0.4637***	0.2080
	(0.0149)	(0.0342)	(0.0817)	(0.1961)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249
R^2	0.0020	0.0207	0.0880	0.0939

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 280 days (see Table 4.5) from the property's listing. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in our baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.9: Treatment Effect on Sales:
Cox Proportional Hazard Model Estimates

	$Sale_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.1136**	-0.1146**	-0.1220**	-0.1295**
	(0.0567)	(0.0568)	(0.0573)	(0.0619)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249
R^2	0.0018	0.0018	0.0804	0.0760

Note: The table presents Cox Proportional Hazard regression estimates of our baseline survival model (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings in the baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.10: Treatment Effect on Sales Price as a Proportion
of List Price: OLS Estimates

	$SalePriceProportion_i$			
	(1)	(2)	(3)	(4)
Treated = 1	-0.0006	-0.0004	-0.0002	-0.0001
	(0.0027)	(0.0027)	(0.0027)	(0.0027)
Constant	0.9631***	0.9517***	0.9502***	0.9757***
	(0.0019)	(0.0045)	(0.0107)	(0.0263)
Balancing Controls	NO	YES	YES	YES
Property Characteristics	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	YES
Observations	1,247	1,247	1,247	1,247
R^2	0.0000	0.0228	0.0755	0.0873

Note: The table presents ordinary least squares regression estimates for our baseline specification (column 1), with Balancing Controls (column 2) and controls for Property Characteristics (column 3) and Advert Characteristics (column 4) successively added. The controls are described in Section 4.2.3, list price now part of the dependent variable is excluded as a control. The dependent variable is sales price as a proportion of list price. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero if allocated to the control arm. Observations include all listings sold up to 280 days from the first list date of the final property to be listed (see Table 4.5), except for any sold property located in Region 6 and the one property where the sales date but not sales price was registered (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.11: Treatment Effect on Sales with List Price Interaction:
Cox Proportional Hazard Model Estimates

	$Sales_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.1282**	-0.3059***	-0.3324***	-0.3073***	-0.2478**
	(0.0567)	(0.0957)	(0.0969)	(0.0974)	(0.1069)
Low List Price	-0.3441***	-0.4678***	-0.4866***	-0.0946	-0.0032
	(0.0702)	(0.0969)	(0.1062)	(0.1321)	(0.1469)
Medium List Price	-0.2223***	-0.3654***	-0.4615***	-0.3060***	-0.2383*
	(0.0680)	(0.0946)	(0.1029)	(0.1141)	(0.1244)
Treated = 1 x Low List Price		0.2530*	0.2947**	0.2725*	0.2107
		(0.1403)	(0.1421)	(0.1431)	(0.1552)
Treated = 1 x Medium List Price		0.2924**	0.3206**	0.2854**	0.1452
		(0.1360)	(0.1382)	(0.1390)	(0.1519)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249	2,249
R^2	0.0127	0.0151	0.0136	0.0844	0.0796

Note: The table presents Cox Proportional Hazard regression estimates of our baseline survival model and list price dummies (list price is split into 3 approximately equal quantiles —high, medium and low) (column 1), with interaction terms for treating an advert and list price quantile (column 2), Balancing Controls (column 3), controls for Property Characteristics other than list price (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in the baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.12: Treatment Effect on Sales with Number of Bedrooms Interaction: Cox Proportional Hazard Model Estimates

	$Sales_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.1250**	-0.4064***	-0.4208***	-0.4625***	-0.4276***
	(0.0567)	(0.1111)	(0.1113)	(0.1140)	(0.1249)
Two or Fewer Bedrooms	-0.4573***	-0.5995***	-0.6803***	-0.2809**	-0.2761*
	(0.0724)	(0.0976)	(0.1021)	(0.1319)	(0.1466)
Three Bedrooms	0.0618	-0.1593	-0.2298**	-0.2925**	-0.3018**
	(0.0720)	(0.0995)	(0.1026)	(0.1138)	(0.1253)
Treated = 1 x Two or Fewer Bedrooms		0.3032**	0.3161**	0.3606**	0.3094*
		(0.1456)	(0.1458)	(0.1485)	(0.1616)
Treated = 1 x Three Bedrooms		0.4591***	0.4765***	0.5528***	0.4893***
		(0.1449)	(0.1451)	(0.1496)	(0.1642)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249	2,249
R^2	0.0337	0.0381	0.0406	0.0843	0.0798

Note: The table presents Cox Proportional Hazard regression estimates of our baseline survival model and controls for number of bedrooms where the comparison is 4 or 5 bedrooms (column 1), with interaction terms for treating an advert and number of bedrooms (column 2), Balancing Controls (column 3), controls for Property Characteristics other than number of bedrooms (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in the baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.13: Treatment Effect on Sales with Property Type Interactions: Cox Proportional Hazard Model Estimates

	$Sales_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.1087*	-0.1068	-0.1084	-0.1287	-0.0979
	(0.0567)	(0.0861)	(0.0862)	(0.0874)	(0.0962)
Flat/Terraced	-0.4915***	-0.4780***	-0.5049***	-0.3224***	-0.2867***
	(0.0586)	(0.0812)	(0.0825)	(0.0952)	(0.1055)
Bungalow/Unspecified	-0.2557**	-0.3566**	-0.2983*	-0.2489	-0.2081
	(0.1263)	(0.1770)	(0.1775)	(0.1817)	(0.1983)
Treated = 1 x Flat/Terraced		-0.0277	-0.0289	-0.0163	-0.0777
		(0.1171)	(0.1173)	(0.1188)	(0.1296)
Treated = 1 x Bungalow/Unspecified		0.2166	0.2466	0.3836	0.3552
		(0.2525)	(0.2527)	(0.2578)	(0.2803)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	2,249	2,249	2,249	2,249	2,249
R^2	0.0319	0.0323	0.0345	0.0596	0.0606

Note: The table presents Cox Proportional Hazard regression estimates of our baseline survival model and controls for property type where the comparison is detached and semi detached houses (column 1), with interaction terms for treating an advert and property type (column 2), Balancing Controls (column 3), controls for Property Characteristics other than property type (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable takes a value of 1 if the property was sold up to 280 days after the final property to be listed is first listed. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings in the baseline sample except the seven listings from Region 6 (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.14: Treatment Effect on Sales Price as a Proportion of List Price with List Price Interaction: OLS Estimates

	$SalesPriceProportion_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.0006	-0.0008	-0.0005	-0.0002	0.0002
	(0.0027)	(0.0046)	(0.0046)	(0.0046)	(0.0046)
Low List Price	0.0021	0.0025	0.0044	0.0074	0.0102
	(0.0034)	(0.0047)	(0.0051)	(0.0063)	(0.0064)
Medium List Price	0.0028	0.0021	0.0040	0.0026	0.0034
	(0.0033)	(0.0046)	(0.0049)	(0.0052)	(0.0052)
Treated = 1 x Low List Price		-0.0009	-0.0011	-0.0015	-0.0027
		(0.0068)	(0.0068)	(0.0067)	(0.0067)
Treated = 1 x Medium List Price		0.0013	0.0010	0.0011	0.0013
		(0.0066)	(0.0066)	(0.0065)	(0.0065)
Constant	0.9615***	0.9616***	0.9489***	0.9433***	0.9671***
	(0.0027)	(0.0032)	(0.0053)	(0.0123)	(0.0269)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	1,247	1,247	1,247	1,247	1,247
R^2	0.0007	0.0007	0.0242	0.0768	0.0896

Note: The table presents ordinary least squares regression estimates for our baseline specification and list price dummies (column 1), with interaction terms for treating an advert and list price quantile (column 2), Balancing Controls (column 3), controls for Property Characteristics other than list price (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable is sales price as a proportion of list price. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings sold up to 280 days from the first list date of the final property to be listed (see Table 4.5), except for any sold property located in Region 6 and the one property where the sales date but not sales price was registered (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.15: Treatment Effect on Sales Price as a Proportion of List Price with Number of Bedrooms Interaction: OLS Estimates

	$SalesPriceProportion_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.0008	-0.0030	-0.0033	-0.0047	-0.0047
	(0.0027)	(0.0054)	(0.0054)	(0.0053)	(0.0053)
Two or Fewer Bedrooms	-0.0064*	-0.0054	-0.0057	-0.0035	-0.0004
	(0.0035)	(0.0047)	(0.0049)	(0.0057)	(0.0059)
Three Bedrooms	0.0018	-0.0022	-0.0021	-0.0043	-0.0030
	(0.0035)	(0.0048)	(0.0049)	(0.0051)	(0.0052)
Treated = 1 x Two or Fewer Bedrooms		-0.0020	-0.0016	-0.0003	-0.0007
		(0.0070)	(0.0070)	(0.0069)	(0.0070)
Treated = 1 x Three Bedrooms		0.0079	0.0086	0.0116*	0.0120*
		(0.0070)	(0.0070)	(0.0069)	(0.0070)
Constant	0.9649***	0.9659***	0.9549***	0.9544***	0.9806***
	(0.0029)	(0.0035)	(0.0056)	(0.0118)	(0.0269)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	1,247	1,247	1,247	1,247	1,247
R^2	0.0057	0.0078	0.0310	0.0727	0.0853

Note: The table presents ordinary least squares regression estimates for our baseline specification and controls for number of bedrooms where the comparison is 4 or 5 bedrooms (column 1), with interaction terms for treating an advert and number of bedrooms (column 2), Balancing Controls (column 3), controls for Property Characteristics other than number of bedrooms (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable is sales price as a proportion of list price. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings sold up to 280 days from the first list date of the final property to be listed (see Table 4.5), except for any sold property located in Region 6 and the one property where the sales date but not sales price was registered (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.16: Treatment Effect on Sales Price as a Proportion of List Price with Property Type Interactions: OLS Estimates

	$SalesPriceProportion_i$				
	(1)	(2)	(3)	(4)	(5)
Treated = 1	-0.0006	0.0025	0.0030	0.0033	0.0039
	(0.0027)	(0.0042)	(0.0042)	(0.0041)	(0.0041)
Flat/Terraced	-0.0057**	-0.0025	-0.0026	0.0028	0.0052
	(0.0028)	(0.0039)	(0.0040)	(0.0043)	(0.0044)
Bungalow/Unspecified	-0.0013	-0.0034	-0.0053	-0.0057	-0.0057
	(0.0061)	(0.0086)	(0.0085)	(0.0085)	(0.0085)
Treated = 1 x Flat/Terraced		-0.0066	-0.0071	-0.0076	-0.0086
		(0.0057)	(0.0057)	(0.0056)	(0.0056)
Treated = 1 x Bungalow/Unspecified		0.0043	0.0030	0.0060	0.0064
		(0.0122)	(0.0122)	(0.0120)	(0.0120)
Constant	0.9661***	0.9646***	0.9532***	0.9513***	0.9751***
	(0.0025)	(0.0029)	(0.0050)	(0.0106)	(0.0263)
Balancing Controls	NO	NO	YES	YES	YES
Other Property Characteristics	NO	NO	NO	YES	YES
Advert Characteristics	NO	NO	NO	NO	YES
Observations	1,247	1,247	1,247	1,247	1,247
R^2	0.0034	0.0048	0.0277	0.0767	0.0893

Note: The table presents ordinary least squares regression estimates for our baseline specification and controls for property type where the comparison is detached and semi detached houses (column 1), with interaction terms for treating an advert and property type (column 2), Balancing Controls (column 3), controls for Property Characteristics other than property type (column 4) and Advert Characteristics (column 5) successively added. The controls are described in Section 4.2.3. The dependent variable is sales price as a proportion of list price. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm otherwise zero. Observations include all listings sold up to 280 days from the first list date of the final property to be listed (see Table 4.5), except for any sold property located in Region 6 and the one property where the sales date but not sales price was registered (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.

Table C.17: Comparison of Treatment Effect Estimates Between Synthetic Control Methods and the RCT

Panel (A): Click Throughs — OLS Estimates

	$LogClickThroughs_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.5897***	0.6626***	0.2997***
	(0.0228)	(0.0632)	(0.0190)
Constant	5.9847***	4.8882***	6.9448***
	(0.9891)	(1.6098)	(0.1980)
Observations	16,859	2,496	2,256
R^2	0.5290	0.5908	0.4960

Panel (B): Email Requests — OLS Estimates

	$EmailRequests_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0806***	0.0845***	0.0307
	(0.0106)	(0.0296)	(0.0198)
Constant	0.4591	-0.5316	1.0570***
	(0.4609)	(0.7548)	(0.2063)
Observations	16,859	2,496	2,256
R^2	0.1818	0.2459	0.0552

Panel (C): Early Sales Subject to Contract — OLS Estimates

	$EarlySSTC_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0184*	0.0233	-0.0077
	(0.0111)	(0.0305)	(0.0197)
Constant	1.3164***	0.2625	0.9897***
	(0.3558)	(0.6505)	(0.2056)
Observations	19,028	2,878	2,256
R^2	0.1714	0.2455	0.0835

Panel (D): Sales Subject to Contract —
OLS Estimates

	$SSTC_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0362***	0.0424	0.0027
	(0.0109)	(0.0298)	(0.0199)
Constant	1.3930***	0.3130	1.0565***
	(0.3496)	(0.6357)	(0.2075)
Observations	19,028	2,878	2,256
R^2	0.1719	0.2539	0.0980

Panel (E): Sales Subject to Contract —
Cox PH Estimates

	$SSTC_i$		
	Sim 1	Sim 2	RCT
Treated = 1	-0.0260	-0.0429	-0.0587
	(0.0269)	(0.0612)	(0.0545)
Observations	19,028	2,878	2,256
R^2	0.0157	0.0189	0.0933

Panel (F): Early Sales —
OLS Estimates

	$EarlySale_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0150	0.0277	-0.0143
	(0.0099)	(0.0272)	(0.0194)
Constant	0.2634	0.5721	0.7490***
	(0.3174)	(0.5802)	(0.2020)
Observations	19,028	2,878	2,249
R^2	0.2499	0.3215	0.0831

Panel (G): Sales —
OLS Estimates

	$Sale_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0249**	0.0424	-0.0441**
	(0.0099)	(0.0269)	(0.0205)
Constant	0.3357	0.6892	0.5753***
	(0.3170)	(0.5736)	(0.2132)
Observations	19,028	2,878	2,249
R^2	0.3014	0.3773	0.0939

Panel (H): Sales —
Cox PH Estimates

	$SSTC_i$		
	Sim 1	Sim 2	RCT
Treated = 1	0.0927***	0.1477**	-0.1295**
	(0.0315)	(0.0694)	(0.0619)
Observations	19,028	2,878	2,249
R^2	0.0281	0.0214	0.0760

Panel (I): Sales Price Proportion —
OLS Estimates

	$SalePriceProportion_i$		
	Sim 1	Sim 2	RCT
Treated = 1	-0.0003	-0.0008	-0.0001
	(0.0015)	(0.0046)	(0.0027)
Constant	1.0287***	0.9268***	0.9722***
	(0.0230)	(0.0810)	(0.0319)
Observations	7,602	1,160	1,247
R^2	0.2017	0.2982	0.0873

Note: The table presents estimates from our baseline specifications with controls for Balancing, Property Characteristics and Advert Characteristics added. These controls are described in Section 4.2.3. Regression estimates are OLS unless stated as Cox (in which case they are Cox Proportional Hazard regression estimates of our baseline survival mode). Dependent variables are described in Table 4.5. Treated takes a value of 1 if the listing was randomly allocated to the treatment arm or otherwise 0. *Sim 1* is the first synthetic trial, *Sim 2* the second synthetic trial, *RCT* the main RCT in the field see Section 4.5. For Sim 1 and Sim 2 observations include all listings in their baseline samples. For RCT observations include all listings in their baseline samples except the Early Sale STC, Sale STC, Sale STC (Cox) Early Sale, Sale, Sale (Cox) and Sales Price estimates do not include the 7 RCT listings located in Region 6 and the Sales Price estimate also does not include the 1 RCT listing where the sales date but not the sales price was registered (see Section 4.2.2). *p<0.1; **p<0.05; ***p<0.01.