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BETTING ON THE HOUSE:  
SUBJECTIVE EXPECTATIONS AND MARKET CHOICES

Nicolas L. Bottan  
Ricardo Perez-Truglia

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### **ABSTRACT**

Home price expectations play a central role in macroeconomics and finance. However, there is little direct evidence on how these expectations affect market choices. We provide the first causal evidence based on a large-scale, high-stakes, naturally occurring field experiment in the United States. We mailed letters with information on trends in home prices to 57,910 homeowners who recently listed their homes on the market. Collectively, these homes were worth \$34 billion. We randomized the information contained in the mailing to create non-deceptive, exogenous variation in the subjects' home price expectations. We then used rich administrative data to measure the effects of these information shocks on the subject's market choices. We find that, consistent with economic theory, higher home price expectations caused a reduction in the probability of selling the home. These effects are highly statistically significant, economically large in magnitude, and robust to a number of sharp checks. Our results indicate that market choices are highly elastic to expectations: a 1 percentage point increase in home price expectations causes a 2.63 percentage point reduction in the probability of selling the property within 12 weeks. Also, our evidence indicates that information frictions play a significant role in the housing market.

Nicolas L. Bottan  
Policy Analysis and Management  
Cornell University  
MVR Hall 3220  
Ithaca, NY 14853  
nicolas.bottan@cornell.edu

Ricardo Perez-Truglia  
Haas School of Business  
University of California, Berkeley  
545 Student Services Building #1900  
Berkeley, CA 94720-1900  
and NBER  
ricardotruglia@berkeley.edu

A randomized controlled trials registry entry is available at <https://www.socialscienceregistry.org/trials/3663>

A data appendix is available at <https://data.nber.org/data-appendix/w27412/>

# 1 Introduction

Consumer expectations play a central role in modern macroeconomics and finance, and they are of special interest to policymakers (Bernanke, 2007). In the context of the housing market, there is growing interest in home price expectations, particularly the homeowners' expectations about future growth in home prices (Bailey, Cao, Kuchler, and Stroebel, 2018; Bailey, Davila, Kuchler, and Stroebel, 2018). Because homes account for a large fraction of households' assets, home price expectations can have major welfare and policy implications. Indeed, home price expectations are believed to have played a key role in the 2008 U.S. housing crisis (Shiller, 2005). Despite their central role, there is little *direct* evidence on whether home price expectations have a causal effect on the decision to sell a property. In this paper, we fill this gap in the literature using a large-scale, high-stakes, pre-registered natural field experiment.

Estimating the relationship between home price expectations and market choices is plagued with challenges to causal identification. Consider the following correlation as an example: in markets with more optimistic homes price expectations, homeowners are less willing to sell their homes. This correlation does not imply that home price expectations have a causal effect on sellers' market behavior. Causality could run in the opposite direction: homeowners' reluctance to sell may drive up home price expectations, rather than the other way around. The correlation also may be spuriously driven by an omitted variable. For example, an improvement in fundamentals may simultaneously generate a rise in home price expectations and homeowners' unwillingness to sell. Without exogenous variation in expectations, it is challenging to identify the causal effect of home price expectations on market outcomes.

To better illustrate the link between subjective expectations and market choices, consider a sample of homeowners who have listed a home for sale. Those sellers receive a sequence of offers. For each offer, they must decide whether to accept it or wait for a better offer to come along. According to economic theory, subjective home price expectations could be a key input in sellers' decision-making (Glaeser and Nathanson, 2017; Anenberg, 2016). More precisely, when homeowners become more optimistic about future home prices, they should be less excited about selling their properties (i.e., they should increase their "reservation price"). As a result, the property should take longer to sell. Conversely, homeowners who become more pessimistic about future home values are expected to sell the properties faster.

In the ideal experiment, we would take this group of sellers and flip a coin to randomize their expectations. For example, if the coin lands on heads, a homeowner would be persuaded that median home prices will appreciate by 1% over the next year. If the coin lands on tails, the homeowner would be persuaded that median home prices will appreciate by 10% over

the next year. Then, we would check which owners sold their homes and which ones did not sell their homes in the months following the randomization. Our main hypothesis is that, relative to the homeowners who are randomly assigned to the 1% home price expectation, the homeowners assigned to the 10% home price expectation would take longer to sell their properties. Moreover, the magnitude of those effects can shed light on how elastic homeowners are to changes in their home price expectations.

We designed a field experiment that closely resembles the ideal experiment. We mailed letters to a sample of U.S. homeowners who had recently listed a home on the market. These letters included information on the current median home price for comparable homes (i.e., homes in the same ZIP Code and with the same number of bedrooms). We randomized additional factual information in the letters to create non-deceptive, exogenous shocks to the subjects' home price expectations. By chance, some owners received optimistic shocks to their expectations, while other owners received pessimistic shocks. We then used publicly available administrative records to check if and when homeowners sold the listed property. We measure whether the exogenous shocks to home price expectations induced by our letters affected the subjects' subsequent market choices.

Information-provision experiments typically require data on prior beliefs. For example, imagine that an individual is given information that home prices will grow by 5% per year. Whether that individual should update expectations up, down, or not at all depends on the individual's prior belief. Individuals who believe that home prices will grow by less than 5% should adjust their expectations downward, whereas individuals who believe that homes will appreciate by more than 5% should update their expectations upward. We devise an experimental design to leverage exogenous shocks to beliefs even in the absence of data on prior beliefs. This design creates two distinct sources of exogenous shocks to the subjects' expectations, *source-randomization* and *disclosure-randomization*.

Source-randomization consists of randomizing the *source* used for the additional information on the evolution of home prices. We use three sources that prior survey experiments (Armona et al., 2019; Fuster et al., 2020) have shown to significantly affect the formation of home price expectations: the average price change over the past year, the average price change over the past two years, and one of three forecasts about the price change in the next year based on different statistical models.

To illustrate the intuition of the source-randomization design, consider a group of subjects selling a 2-bedroom home in ZIP Code 33308. Each homeowner could be randomly allocated to one of the following five signals of home price growth: an annual growth rate of 1.2% over the past one year; an annual growth rate of 3.6% over the past two years; an annual growth forecast of 2.6% (according to statistical model 1); an annual growth forecast of 4.1%

(model 2); or an annual growth forecast of 3.5% (model 3). For each subject, we randomized which one of the five signals is shown to him or her. This source of variation constitutes the exogenous information shock. By chance, some subjects in this group are shown a more optimistic signal and others a more pessimistic signal. Relative to receiving a signal from the first source (1.2%), receiving a signal from the second source (3.6%) constitutes a positive information shock, which should result in more optimistic home price expectations. More precisely, relative to being assigned to the first signal, being assigned to the second signal amounts to an information shock of +2.4 pp ( $= 3.6 - 1.2$ ). Likewise, receiving the third, fourth, or fifth signal should amount to an information shock of +1.4 pp, +2.9 pp, or +2.3 pp, respectively.

The disclosure-randomization variation creates additional exogenous variation in beliefs. We cross-randomize whether the additional information on the evolution of home values is included in the letter or not. The disclosure-randomization follows a similar logic as the source-randomization variation, but instead of leveraging variation in signals across sources, it exploits variation in signals across different markets. These two sources of variation, disclosure-randomization and source-randomization, can be analyzed separately or combined in a single estimator to maximize power. Indeed, comparing the results across the two identification strategies provides a unique and valuable robustness check.

We implement the field experiment using a sample of individuals who had recently listed their homes for sale. We identify this subject pool using publicly available information from a major online listing website. Using unique identifiers for the property, we merge those records with rich administrative data from the assessor's office of their respective counties. These public records include detailed information about the property and its owners, such as their full names and mailing addresses. We use that contact information to mail a letter to the owners of the listed properties. We then use public records to track whether and when each property was sold during the six months following the mailing intervention. In June 2019, we mailed the letters to 57,910 unique homeowners in 36 counties across seven U.S. states. The homes were collectively valued at \$34 billion dollars.

The results from the field experiment confirm that the information shocks affect actual, high-stakes market choices and in the direction predicted by economic theory. A larger information shock (i.e., making expectations more optimistic) reduces the speed at which properties are sold. This effect is highly statistically significant and large in magnitude: a 1 pp higher information shock causes a 0.350 pp drop in the probability that the property sells within 12 weeks (p-value=0.001), implying a behavioral elasticity of -0.35.

The results from the field experiment are robust to a number of checks. We use an event-study analysis to exploit variation in the timing of when subjects receive and read our letters.

First, we estimate the effects on the outcomes right before the letters are delivered, at which point they should have no effect on the sales outcome. As expected, the effects of the information shocks are precisely estimated around zero in the pre-treatment period. Moreover, we leverage the fact that not all letters are delivered and read at the same time but instead are gradually opened over a period of seven weeks. We show that, as expected, the effects of the letters intensify during that period and stabilize thereafter. Moreover, the event-study analysis shows that the effects of our information shocks are highly persistent. For instance, the behavioral elasticity estimated at 28 weeks post-treatment (-0.295, p-value=0.006) is close to the corresponding elasticity estimated at 12 weeks post-treatment (-0.350, p-value=0.001).

For an additional falsification test, we estimate placebo regressions that are identical to the baseline specification, except that they use pre-treatment characteristics as dependent variables. For example, some of those placebo outcomes are the number of days the property had been listed prior to our experiment or the original listing price. Because those outcomes are determined prior to our mailing intervention, the randomized information shocks contained in the letters should not affect them. As expected, we find that the placebo effects are close to zero, statistically insignificant, and precisely estimated.

We provide several additional robustness checks. In our baseline specification, we combine the disclosure-randomization and source-randomization variation to maximize statistical power. However, the results are almost identical if we use those two sources of variation separately. The results are almost identical in alternative specifications, such as including a rich set of control variables. For a less parametric look at the data, we use a binned scatterplot version of the baseline specification. The results confirm that the findings are not driven by non-linearities or outliers.

To provide complementary evidence on the effects of information shocks, we designed a supplemental survey experiment. This supplemental experiment was included in the same randomized control trial pre-registration as the field experiment, and it was conducted around the same dates as the main field experiment too. The survey experiment exposes a separate sample of 1,404 subjects to the exact same information treatments used in the field experiment. However, instead of measuring the effects of the information shocks on their market choices, the survey experiment measures the effects on their subsequent home price expectations. The results from the supplementary experiment confirm that our information shocks have significant effects on home price expectations and in the expected direction. A 1 pp information shock increases the one-year-ahead home price expectations by 0.205 pp (p-value=0.001). Indeed, this degree of pass-through from information shocks to posterior beliefs is consistent in magnitude with the results from other survey experiments about home price expectations (Armona et al., 2019; Fuster et al., 2020) and other macroeconomic ex-

pectations (Cavallo et al., 2017; Roth and Wohlfart, 2019).

Next, we discuss the magnitude of the effects of home price expectations on sellers' behavior. Due to the presence of two forms of non-compliance, the behavioral elasticity of -0.350 reported above constitutes an intention-to-treat effect. First, some subjects may not have received or read the letter in time. Second, conditional on reading the information provided in the letter, some recipients may ignore the information or may update their expectations only partially. We provide estimates of the treatment-on-the-treated effect by correcting for both forms of non-compliance. We use statistics from the U.S. Postal Office to correct for the first form of non-compliance and estimates from the supplemental survey experiment to correct for the second. These calculations suggest that subjects are highly elastic to their home price expectations: increasing home price expectations by 1 pp would cause a reduction of 2.63 pp in the probability of selling the home within the next 12 weeks.

Given the large variation in home price expectations observed across households and over time, our findings imply that subjective expectations can be a major factor driving decisions in the housing market. For example, in a given cross-section of households, some households tend to be more optimistic than other households. Our treatment-on-the-treated estimates imply that a one-standard-deviation increase in home price expectations would cause a reduction of 14.18 pp in the probability of selling the property within the next 12 months.

Last, we measure heterogeneity in the effects of information shocks on home price expectations using characteristics of the owner, property, and local housing market. We find similar effects across the board. For example, the point estimates are similar and statistically indistinguishable between male and female homeowners and between more and less expensive homes. Two notable exceptions include stronger effects when the owner is a non-occupant and when the owner is aged 59 and above. These two differences must be taken with a grain of salt, however, as they are large in magnitude but statistically insignificant. Our preferred interpretation for these sources of heterogeneity is based on optimization frictions: some owners may want to react to the information on home price expectations but face constraints on when to sell due to changes in job, school, or family composition.

This study relates and contributes to various strands of literature. Most important, it relates to literature on the role of subjective expectations in the housing market. Some non-experimental studies measure the relationship between home price expectations and market choices, such as whether to buy or rent or the mortgage leverage choice (Bailey, Cao, Kuchler, and Stroebel, 2018; Bailey, Davila, Kuchler, and Stroebel, 2018). For example, Bailey et al. (2018) presents evidence that individuals are more likely to transition from renting to owning after geographically distant friends experience large recent home price increases. To the best

of our knowledge, nobody conducted a field experiment to study the effects of home price expectations, likely because of the challenges associated with field experiments in such high-stakes contexts. There are a few experimental exercises in the contexts of surveys (Fuster, Perez-Truglia, Wiederholt, and Zafar, 2020) and laboratory games (Armona, Fuster, and Zafar, 2019), but it is difficult to extrapolate from those stylized contexts to high-stakes choices in the real world.

We thus contribute to this literature by being the first to measure the effects of home price expectations on market behavior using what is arguably a near ideal context. In terms of causal identification, our study uses the gold standard in economic research: a randomized controlled trial. Our study exploits a naturally occurring context in that our subjects (like millions of homeowners in the country each year) had already decided to put their homes on the market. We employ a high-stakes context wherein the decision accounts for a large fraction of the net worth of the decision maker. Indeed, buying or selling a home is arguably one of the biggest decisions that Americans make, financially and otherwise (Brooks, 2017). Our study involves a large-scale experiment with 57,910 subjects, which allows us to provide precise estimates and sharp falsification tests (e.g., event-study analysis). Finally, rather than using survey data to measure behavior, which is subject to many criticisms such as measurement error and experimenter demand effects, we measure the *actual* market outcomes using administrative records.<sup>1</sup>

This study also contributes to the broader literature on deviations from rational expectations in the housing market. These irrational expectations are frequently discussed as major factors in the housing market (Case and Shiller, 1989; Shiller, 2005; Glaeser and Nathanson, 2015; Anenberg, 2016; Glaeser and Nathanson, 2017; Gennaioli and Shleifer, 2018; Bailey et al., 2018; Kaplan et al., 2019; Bailey et al., 2018). However, some observers have voiced skepticism about widespread non-rationality in the housing market (Guren, 2018). According to their view, the stakes of housing transactions are so large that households could not possibly have irrational expectations. We provide experimental evidence that providing households with publicly available information on the evolution of home prices has substantial effects on their market choices. This direct evidence shows that despite the high-stakes, information frictions do play a significant role in the housing market.

Last, we intend to make a methodological contribution by using a field experiment to pro-

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<sup>1</sup> Moreover, our contribution extends to a broader literature on subjective macroeconomic expectations, including other topics such as inflation (Armantier et al., 2016; Cavallo et al., 2017; Coibion et al., 2018) and GDP growth (Roth and Wohlfart, 2019). These studies typically provide a random subset of respondents with a piece of information and measure the corresponding effects on their subsequent survey responses, including their posterior beliefs, attitudes, or even small-stakes laboratory choices (Armantier et al., 2015; Armona et al., 2019). We contribute to this literature by assessing the effects of macroeconomic expectations on *actual* behavior in a high-stakes and naturally occurring context.



vide causal evidence on why individuals choose to sell their homes. Home price expectations are one factor that individuals take into account in this decision. Our experimental framework can be adapted to study preferences over a host of other factors such as perceptions about the value of local amenities, property taxes, or the quality of government services. Other researchers can use this framework to test their own hypotheses from diverse fields such as urban economics, real estate, finance, and behavioral economics. As discussed above, this approach is nearly ideal for revealed-preference evidence because it uses administrative data in a large-scale, naturally occurring context and high-stakes choices. Additionally, our framework has practical advantages. It relies on data sources that are publicly available and easily accessible and thus can be conducted by any researcher. The experiments can be implemented in a matter of a few weeks and scaled up to hundreds of thousands of subjects. Moreover, the cost per subject (around \$0.25) is quite low, even relative to online surveys.

The rest of the paper proceeds as follows. Section 2 presents the conceptual design and econometric model. Sections 3 and 4 discuss the details of the design and implementation of the field experiment. Section 5 presents the main results from the field experiment. Section 6 presents the results from the supplemental survey experiment. Section 7 discusses the magnitude of the estimates, while while Section 8 presents the heterogeneity analysis. The last section concludes.

## 2 Research Design and Econometric Model

Information-provision experiments typically rely on data about prior beliefs. Consider randomizing subjects to receive, or not receive, a signal that the annual growth rate of home prices in the next 12 months will be 5%. The effect of the signal on the subsequent home price expectations (and market choices) depends on the prior beliefs of the individual. For subjects whose prior home price expectations were below 5%, we expect the signal to cause them to update their home price expectations upward (and subsequently take longer to sell their homes). On the contrary, subjects whose prior home price expectations exceeded 5% should update their home price expectations downward (and subsequently sell their homes faster). If the prior expectations were exactly 5%, then recipients should not update their home price expectations (and their behavior should not change either).

The challenge in our high-stakes, large-scale context is that it is not feasible to measure the prior beliefs of tens of thousands of homeowners. Thus, we designed an information-provision experiment that does not rely on information about prior beliefs (see e.g. [Bergolo et al., 2017](#); [Perez-Truglia and Troiano, 2018](#); [Bottan and Perez-Truglia, 2020](#)). In fact, our experimental design creates not only one, but two sources of exogenous shocks to the subjects' expectations,

that we call *source-randomization* and *disclosure-randomization* and are described in detail below. These two sources of variation can be combined in a single estimator to maximize power, but they can be analyzed separately too – indeed, this comparison provides a unique robustness check of the identification strategy.

## 2.1 Source-Randomization

Let subscript  $i$  index subjects. Each subject belongs to a specific market  $m$ , denoted by the combination of property location (in the field experiment: the 5-digit ZIP Code) and property type (the number of bedrooms).

Let  $E_m^j$  be a signal about the future growth of home prices, where the subscript  $m$  notes that this signal pertains to market  $m$  (e.g., 3-bedroom homes in ZIP Code 33308) and the superscript  $j = 1, \dots, J$  corresponds to the information source. For example,  $j = 1$  could be the annual price change over the past one year, and  $j = 2$  could be the annual price change over the past two years. For this design, it is important that the signals produced by different information sources cannot be exactly equal: e.g., even if two sources are equally optimistic on average, for some markets one source must provide a more optimistic signal while for other markets another source must provide a more pessimistic signal. In this design, all subjects are provided with a signal, only that from a randomly-chosen source. We use  $j_i^*$  to denote the source that was chosen for individual  $i$ .

Let  $Y_{i,m}^{post}$  be the outcome of interest. In the field experiment,  $Y_{i,m}^{post}$  is an indicator variable for whether the homeowner  $i$  who has a property in market  $m$  sold the property within a given number of months after the experiment. We use superscript *post* to make it salient that the outcome was measured in the post-treatment period (for falsification tests we will use pre-treatment outcomes, which will be denoted by superscript *pre* instead). The regression of interest is as follows:

$$Y_{i,m}^{post} = \nu_0 + \nu_1 \cdot E_m^{j_i^*} + \phi_m + \varepsilon_{i,m} \quad (1)$$

Where  $\phi_m$  are market-specific fixed effects, to make sure that we are always comparing between pairs of individuals from the same market (i.e., comparing between individuals who could have been assigned to the exact same set of signals). One intuitive way of thinking about this experiment is that for individuals in a given market  $m$ , they could be shown one of several signals ( $E_m^j$  for  $j = 1, \dots, J$ ), some of them more optimistic and other more pessimistic. So we flip a coin to decide which individuals in market  $m$  get the more optimistic signal and which individuals get the more pessimistic signals, and then we measure if the those who were randomly assigned to more optimistic signals behave differently than those

assigned to more pessimistic signals.

The coefficient of interest,  $\nu_1$ , measures the effects of the information shock. To gain more intuition about this coefficient, and without loss of generality, we can normalize  $E_m^{j_i^*}$  by taking the difference with respect to the first information source ( $j = 1$ ) and re-express equation (1) as follows:

$$Y_{i,m}^{post} = \nu_0 + \nu_1 \cdot [E_m^{j_i^*} - E_m^1] + \phi'_m + \varepsilon_{i,m} \quad (2)$$

Note that since  $E_m^1$  is the same for every individual within market  $m$ , this transformation amounts to a simple translation of the market fixed effects (from  $\phi_m$  to  $\phi'_m$ ). Consider a subject selling a 2-bedroom home in ZIP Code 33308. That homeowner could be randomly presented with one of the following five signals: an annual growth rate of 1.2% over the past one year ( $j = 1$ ); an annual growth rate of 3.6% over the past two years ( $j = 2$ ); an annual growth forecast of 2.6% according to statistical model 1 ( $j = 3$ ); an annual growth forecast of 4.1% according to model 2 ( $j = 4$ ); or an annual growth forecast of 3.5% according to model 3 ( $j = 5$ ). The variable  $[E_m^{j_i^*} - E_m^1]$  equals zero if  $j_i^* = 1$  and 2.4 if  $j_i^* = 2$ . That is, relative to receiving the first signal, receiving the second signal means an information shock of 2.4 pp ( $= 3.6 - 1.2$ ). The information shock equals 1.4 pp, 2.9 pp, and 2.3 pp when  $j_i^* = 3$ ,  $j_i^* = 4$ , and  $j_i^* = 5$ , respectively. Thus, the coefficient  $\nu_1$  measures the effect of a 1 pp increase in the information shock.

## 2.2 Disclosure-Randomization

The disclosure-randomization creates additional exogenous shocks to expectations. We randomly assigned one information source to every subject, but then cross-randomized whether the chosen signal would be actually disclosed to the subject (i.e., included in the letter sent to the subject or not included).

To explain this research design, it is useful to start with a stylized example. To keep things simple, assume that we have a single information source. According to this source, some markets are expected to grow strongly in the future, but other markets should expect weak price growth. Again, for the sake of simplicity, let's consider the extreme case where subjects do not have access to the information source. As a result, there will be no relationship between the value of the signal and the home price expectations: subjects who could have been shown the more optimistic signal will be equally optimistic as those subjects who could have been shown the more pessimistic signal, because they never got to actually see the signal. For subjects who *are* shown the signal, however, there should be a significant correlation between the value of the signal and their home price expectations: individuals who find out that the

signal is optimistic in their market should react by forming more optimistic expectations; while individuals who find out that the signal is pessimistic in their market should form less optimistic expectations. Our research design seeks to exploit that prediction: i.e., that the correlation between expectations and the signal should become stronger when the signal is disclosed vs. when it is not disclosed.

Let  $D_i$  be an indicator variable that equals 1 if the chosen signal is disclosed to subject  $i$ , and 0 otherwise. The regression of interest is as follows:

$$Y_{i,m}^{post} = \mu_0 + \mu_1 \cdot E_m^{j_i^*} \cdot D_i + \mu_2 \cdot E_m^{j_i^*} + \mu_3 \cdot D_i + \varepsilon_{i,m} \quad (3)$$

The parameter  $\mu_2$  measures the relationship between the signal ( $E_m^{j_i^*}$ ) and the outcome ( $Y_{i,m}^{post}$ ) for individuals who are not shown the signal (i.e., when  $D_i = 0$ ). The key parameter is  $\mu_1$ : i.e., how much stronger that relationship is for individuals who were shown the signal ( $D_i = 1$ ), relative to individuals who were not shown the signal ( $D_i = 0$ ). The coefficient of interest,  $\mu_1$ , measures the effect of a 1 pp increase in the information shock.

The exogenous shocks induced by disclosure-randomization operate similarly to the source-randomization, except that they exploit heterogeneity in signals across markets rather than across information sources. Note that the disclosure-randomization requires that there is heterogeneity in signals across subjects. If, for example, all subjects had properties in the same market (i.e., same ZIP Code and property type), there would be no variation to identify  $\mu_1$ .

## 2.3 Pooled Specification

As stated in the pre-registration, our baseline specification pools the two sources of random variation to maximize statistical power. This pooled specification is given by the following equation:

$$Y_{i,m}^{post} = \pi_0 + \pi_1 \cdot E_m^{j_i^*} \cdot D_i + \pi_2 \cdot D_i + \phi_m + \varepsilon_{i,m} \quad (4)$$

The coefficient interest is  $\pi_1$ , that measures the effect of a 1 pp increase in the information shock. To show that equation (4) pools the two sources of variation, we can show that equation (1), for source-randomization, and equation (3), for disclosure-randomization, are just special cases of equation (4). First, consider what happens if we disclosed information to every subject: i.e.,  $D_i = 1$  for every  $i$ . In that case, by construction, the only exogenous variation left would be the source-randomization. To represent this case, we can replace  $D_i = 1$  in equation (4). As a result, equation (4) turns into equation (1). Second, consider what happens if we used a single information source: i.e.,  $J = 1$ . By construction, the only

exogenous variation left would be the disclosure-randomization. To represent this case, we can replace  $E_m^{j_i^*}$  by  $E_m^1$  in equation (4). As a result, equation (4) turns into equation (3), with the only difference that we do not need to control for  $E_m^1$  (as in equation (3)) because that control would be absorbed by the market fixed effects (note that  $E_m^1$  cannot take different values within a given market  $m$ ).

Last, it is important to note that, in practice, different individuals may react differently to changes in their home price expectations, amounting to heterogeneity in the key parameter of interest (e.g.,  $\pi_1$ ). In that case, our estimates would identify the Local Average Treatment Effect (LATE) of information shocks (Imbens and Angrist, 1994) – that is, a weighted average of  $\pi_1$ ’s with a higher weight given to homeowners whose expectations are more responsive to the information shocks. This is not a specific caveat to this study, but a common occurrence in information-provision experiments (e.g. see the discussion in Cullen and Perez-Truglia, 2018). For these reasons, in the empirical analysis we present the effects of information shocks on expectations and we also conduct heterogeneity analysis.

## 3 Design of the Field Experiment

### 3.1 Design of the Mailings

The field experiment consists of sending letters to a sample of homeowners who had listed a property for sale. Figure 1 shows a sample of the envelope. We took a number of measures to communicate that the letter, though unsolicited, came from a legitimate source. The top-left corner of the envelope included the logo for the University of California at Los Angeles (UCLA) and a note about the research study. The top-right corner of the envelope included non-profit organization postage. Figure 2 shows a sample letter, for a fictitious subject: panel (a) corresponds to the front page of the letter, while panel (b) corresponds to the back page (Figure 2.b). All letters included an introduction, the official UCLA logo in the header, contact information in the footer, a physical correspondence address, and a URL of the study’s website with additional information.<sup>2</sup>

One letter was mailed to every individual in the subject pool. All letters were identical, except for some of the information that was personalized (e.g., the recipient’s name) and the some of the information that was randomized. Figure 2 shows the placeholders (marked as «*Information*» and «*Information Details*») for the two pieces of information that differed across treatment groups. The «*Information*» portion included a table with information about

<sup>2</sup> This website contained general information such as contact information for the researchers and institutional review board, but did not specify the hypotheses being tested. A copy of this website was hosted on the UCLA’s website. For a screenshot of the website, see Appendix C.

home prices. Figure 3 shows a sample table for each of the six treatment groups, which are discussed in detail below. The «*Information Details*» portion included methodological notes for the table, such as data sources and statistical models used.<sup>3</sup>

All letters contained information on the current median home value of similar properties. For example, subjects who listed a 3-bedroom home in ZIP Code 90210 received a letter indicating Zillow’s estimated median home value for 3-bedroom homes in ZIP Code 90210. We used the same property types as those used by online real estate market platforms: 1-bedroom, 2-bedroom, 3-bedroom, 4-bedroom, and 5+ bedroom.<sup>4</sup> In addition to information about the current median home values, the table could include information on the evolution of median home values, which individuals could use in forming their home price expectations. Homeowners were randomized into one of six treatment groups. These treatment groups differ in whether the table includes additional information on the price evolution and the source of said information:

- **Baseline:** no additional information on price evolution
- **Past-1:** price change over the past year
- **Past-2:** price change over the past two years
- **Forecast-1:** price change forecast over the next year using statistical model 1
- **Forecast-2:** price change forecast over the next year using statistical model 2
- **Forecast-3:** price change forecast over the next year using statistical model 3

We choose these information sources because they were shown to have significant effects on home price expectations in previous studies. For example, Fuster et al. (2020) show that, upon being shown one of these types of information, subjects update their expectations in the expected direction. Fuster et al. (2020) also show that most households are willing to pay to have access to these information sources, which suggests that they find them useful. Beyond survey experiments, there are theoretical arguments for why subjects may be interested in learning about these information sources. According to the backward-looking expectations model, individuals form beliefs by looking at recent price changes (Case and

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<sup>3</sup> Appendix Figure A.1 shows the six corresponding methodological notes. For a sample letter with the final product, see Appendix B.

<sup>4</sup> For a small minority of properties, the number of bedrooms was not available in the tax rolls and thus we assigned them to a broader category called “all homes”. Zillow does not produce estimates of median home values for some combinations of ZIP codes and number of bedrooms. For those subjects, we used the category “all homes” as well.

Shiller, 1989; Shiller, 2005). And according to models of rational expectation, households may form expectations based on professional forecasts (Carroll, 2003).

The price changes over the past one year (Past-1) and past two years (Past-2) corresponded to the raw Zillow data. The three statistical forecasts (Forecast-1, Forecast-2, and Forecast-3) were based on the same Zillow data. All three forecasts were estimated using year/ZIP Code-level data on the Zillow Home Value Index for 1997–2019. All three models are autoregressive, but they differed in the set of explanatory variables chosen. These differences in specification yielded slightly different forecasts. Forecast Model 1 used five lags of the dependent variable. Forecast Model 2 used five lags of the dependent variable plus five lags of the state-level average of the dependent variable. Forecast Model 3 used three lags of the dependent variable, three lags of the city-level average of the dependent variable, and three lags of the city-level employment rate.<sup>5</sup> All five information sources were informative to a reasonably similar degree: all had similar predictive power and were comparable to the predictive power of Zillow’s official forecasts.<sup>6</sup> It is important to note that this design is non-deceptive: all letters, regardless of the source, were based on real data that homeowners could access from publicly available sources.

Each panel of Figure 3 corresponds to the hypothetical table that a subject would receive, depending on the assigned treatment group. It shows real examples based on an individual selling a 2-bedroom home in ZIP Code 33308. Panel (a) shows the baseline letter, which includes the current median price level only. The following five panels add information on the price evolution: panel (b) shows an average annual growth rate of 1.2% over the past year (Past-1 treatment); panel (c) shows an annual growth rate of 3.6% over the past two years (Past-2 treatment); panel (d) shows the annual growth of rate 2.6% projected by Forecast Model 1 (Forecast-1 treatment); panel (e) shows the annual growth rate of 4.1% projected by Forecast Model 2 (Forecast-2 treatment); and panel (f) shows the annual growth rate of 3.5% projected by Forecast Model 3 (Forecast-3 treatment). As discussed in Section 2, our identification strategy requires heterogeneity in signals across individuals and across information sources, which we show in Section 4 below.

As shown inside the blue box at the bottom of Figure 2.a, the letter includes a URL to an online survey and a unique survey code to verify that the response came from a legitimate subject.<sup>7</sup> The main goal for including the survey link was to provide a proxy for the dates when recipients opened the letters, as in Perez-Truglia and Cruces (2017) and Nathan et al.

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<sup>5</sup> For more details about the three forecasting models, see Appendix A.3.

<sup>6</sup> Details presented in Appendix A.5.

<sup>7</sup> To verify that the respondents were legitimate subjects and to link survey responses at the individual level, subjects had to enter a unique survey code in the first screen of the survey before they could answer any questions. Each code consisted of a unique combination of six characters and was displayed prominently in the letter next to the URL of the survey.

(2020).

## 4 Data Sources and Implementation of the Field Experiment

### 4.1 Data Sources

To implement the mailing experiment, we combined two publicly available sources of data: data on active real estate listings and data for the property tax rolls from the county assessor. We scraped data on real estate listings from a major listing website. These data included rich information about the listed properties, such as address, listing price, property characteristics (number of bedrooms, bathrooms, size, days on market), and the assessor’s unique parcel number (APN) for the property. We used the APN to match each listing to its corresponding record in the county assessor’s tax rolls. Tax rolls contain rich information on the properties and owners. Most important for our experiment, the tax rolls include the names of the owners and their mailing addresses (for more details on the data sources, see Appendix A.1).

U.S. counties typically make their property tax rolls publicly available. However, how accessible those tax rolls are can vary widely, even within a state. Some counties post the data online. For example, raw data from all counties in Florida can be easily downloaded at any time using a file transfer protocol (FTP) address. Other counties, such as Alameda County in California, provide this information only in person and on a case-by-case basis. Many others, such as Los Angeles County, require filling out a short form and paying a fee to obtain a Compact Disc with the raw data. For the field experiment, we selected a set of 36 counties to obtain a large enough subject pool and for which all the required information from the tax rolls (e.g., owner’s name and mailing address) was easily accessible. These counties are distributed across seven states and include 30 counties in Florida, Los Angeles County in California, Maricopa County in Arizona, Clark County in Nevada, Cuyahoga County in Ohio, King County in Washington, and Harris County in Texas. In practice, many other U.S. counties likely would be feasible to include in this type of experiment.

### 4.2 Mailing Campaign

On May 28, 2019, we obtained the information on the active real estate listings and the latest available version of the secured tax rolls. Of the 173,708 active listings scraped, around 164,298 included the APN. For these listings, we merged nearly all listings (164,176 out of 164,298) with the county assessor’s data. As the number of individuals in this initial sample



was substantially higher than the number of subjects needed for our experiment, we adopted a conservative approach and excluded properties or individuals who were not ideal for the experiment. For example, we excluded non-residential properties and residential properties owned by businesses, because it was unclear whether our letter would be delivered to the person choosing whether to sell or not (e.g., the mailing address may correspond to the firm’s lawyer). Similarly, we excluded individuals who recently moved, according to the latest mail-forwarding data from the U.S. Postal Services, and individuals who owned multiple properties in the same county (for more implementation details, see Appendix A.2).

After applying these filters, our pool of potential subjects consisted of 61,176 individuals. From those, we selected a random sample of 60,000 individuals to receive a letter. After processing the data through the U.S. Postal Service, we excluded a minority of subjects (2%) whose mailing addresses were flagged as undeliverable or vacant (1,198). An additional 892 subjects were excluded, mainly because their tax rolls were outdated and thus we mailed the letters to the previous owners.<sup>8</sup> The final subject pool comprised 57,910 individuals to receive letters. These individuals were randomly assigned to the following treatments: 20% to Baseline, 15% each to Past-1 and Past-2, and 16.6% each to Forecast-1, Forecast-2, and Forecast-3. All letters were mailed on June 10, 2019.

### 4.3 Descriptive Statistics and Randomization Balance

We present descriptive statistics for the subject pool in column (1) of Table 1. The average property was listed for \$575,000, had 3.3 bedrooms, 2.6 bathrooms, 2,300 sq. ft. of living space, and a lot size of 13,000 sq. ft. Table 1 provides a balance test as well. Columns (2) through (7) of Table 1 break down the average property characteristics by treatment groups. The last column reports p-values for the null hypothesis that the average characteristics were equal across all six treatment groups. The results are consistent with successful random assignment: the observable characteristics are similar across all treatment groups in magnitude and not statistically distinguishable from each other.

In Appendix A.4 we present additional details about the sample. For example, even though the tax rolls do not include owner characteristics such as gender, we were able to obtain complementary data from a private vendor. The average owner in our sample was 58.7 years old, 31.9% were female, 62.9% were white, 3% were African-American, 13% were Hispanic, 40% were College graduates and their average annual household income was \$128,000.

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<sup>8</sup> The tax rolls are updated with a lag. As a result, when we received the most up-to-date rolls we identified 845 letters that were sent to a previous owner rather than the most current owner (i.e., the one who listed the home on the market). Another 37 subjects were dropped because could not be matched to the administrative data. Last, 10 subjects were dropped because they were “fake” subjects included on purpose for quality control of the mailing campaign.

Additionally, Appendix A.4 compares our subject pool to a representative sample of homeowners from the American Community Survey. We find that our experimental sample is fairly representative of homeowners in the same counties where the experiment was conducted. Moreover, the subject pool is representative of homeowners in the country as a whole in many dimensions such as the numbers of bedrooms, bathrooms and living space. However, there is one notable difference: homes in our subject pool (average listing price of \$575,000) are more expensive than in the country as a whole (average price of \$222,000, according to Zillow’s Consumer Housing Trends Report). However, this difference arises mechanically, because the subject pool includes counties that are more urban and more expensive than the U.S. average.

Besides the characteristics of the properties and the owners, the subject pool is fairly representative in terms of the characteristics of the housing markets. The time to sell for properties in our subject pool (on average, 158 days) is somewhat higher, but still in the same order of magnitude, than the corresponding average for homes in the same metropolitan areas (105 days).<sup>9</sup> This difference is also mechanical, due to the way in which we selected the subject pool.<sup>10</sup> The owner occupancy rate is quite close to the country average: around 67% of the listings are owner-occupied in our subject pool, which is similar to the 64% of the housing units that were owner-occupied in the country as a whole.<sup>11</sup>

## 4.4 Variation in Signals

As explained in Section 2, the identification strategy relies on variation in signals across information sources and across markets. In this section, we show that there is significant variation in both of these dimensions.

Figure 4.a presents the variation in signals across information sources, which is the relevant variation for the source-randomization design. This scatterplot shows the relationship between the signals that the subjects would have received had they been assigned to the Past-1 treatment (i.e., annual growth rate over the past one year) versus the Past-2 treatment (i.e., annual growth rate over the past two years). For example, for 2-bedroom homes in ZIP Code 33308, the recipient would have been shown a price change of 1.2% if randomly assigned to

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<sup>9</sup> To calculate this benchmark, we start with Zillow’s Housing Data for June 2019 for the same metropolitan areas where the experiment was conducted. On average, it takes 60 days for a property to go from the initial listing to sale pending status. And we then add 45 days to account for the average closing time (according to Ellie Mae’s Origination Insight Report).

<sup>10</sup> When we selected the subject pool, we did not focus on the properties that had been listed on that same day but we also chose to include properties that had been listed weeks or even months before, to increase the sample size.

<sup>11</sup> The U.S. average statistics are from the 5-year estimates from the American Community Survey corresponding to the period 2015–2019.

the Past-1 treatment and a price change of 3.5% if randomly assigned to the Past-2 treatment. The two signals are highly correlated: on average, an extra 1% increase in the annual price change over the past one year is associated with an extra 0.659% increase in the annual price change over the past two years. This relationship is partly mechanical (Past-2 is the average between Past-1 and another number) and partly due to the well-known momentum in home prices. In any case, the most important fact is that the relationship between these two potential signals is far from perfect: the  $R^2 = 0.659$  is high but substantially below one. Moreover, we document significant variation occurs across other pairs of information sources too (see Appendix A.5 for more details).

Figure 4.b shows the variation in signals across markets, which is relevant for the disclosure-randomization design. This figure shows a histogram of the signal that subjects would have received if they had they been assigned to the Past-1 treatment. The figure shows plenty of variation. Subjects in the 10th percentile lived in areas where median home values declined by -0.7% in the previous 12 months, and subjects in the 90th percentile lived in areas where property values increased 8.6%. There is quite a bit of variation in the other four information sources too (see Appendix A.5 for more details).

## 4.5 Letter Delivery

The letters were mailed on June 10, 2019. To make the experiment more affordable, we used non-profit postage. According to the U.S. Monitor Non-Profit Standard Mail Delivery Study, it takes non-profit mailings about 10 days to be delivered, with some letters arriving as much as a month after mailing (U.S. Monitor, 2014).<sup>12</sup> As such, some subjects received the letter a few days after June 10, whereas other subjects received it weeks later. Even after the envelope is delivered, it may take days or even weeks for the subjects to open the envelope and read the letter. Some subjects may have been out of town when the letter was delivered to their homes. Other subjects may have received the letter right away but put it away and did not open it until weeks later.

Following Perez-Truglia and Cruces (2017) and Nathan et al. (2020), we used the distribution of dates when the individuals entered the survey codes included in the letter, as a proxy for when the letters were actually read. Hereafter, we refer to these dates as the “read-receipt.”<sup>13</sup> Figure 5.a presents the results. The first survey response was recorded on June 15, thus marking the start of letter delivery. Indeed, this date coincided with the best

<sup>12</sup> This delivery time is more than twice that of first-class mail, which is handled first, followed by presort standard and finally non-profit mail.

<sup>13</sup> Our proxy probably had some upward bias, because some people may have read the letter and waited a few days to respond to the survey. Another potential source of bias, which may be upwards or downwards, is that survey respondents could open the letters more or less slowly than survey non-respondents.

guess provided by the mailing company.<sup>14</sup> This figure suggests that the letters were opened gradually from the start of the letter delivery until eight weeks later. The median time from the start of the letter delivery until the read-receipt was approximately three weeks.

## 4.6 Main Outcome: Home Sale

To measure the behavioral outcomes, we scraped the administrative data from the real estate listing website on a weekly basis, from two weeks before the start of letter delivery until 28 weeks after the start of letter delivery.<sup>15</sup>

Whether the property was sold by a given date is the main outcome of interest, as listed in the American Economic Association randomized controlled trial pre-registry. Administrative records indicate whether the property was sold and on what date. Confirmation of property sale came from either the Multiple Listing Service or the county assessor records. If we had confirmation from both sources, we used the earliest date for which we had confirmation. Our records usually included both sources, and the two dates were typically close. The Multiple Listing Service date is often earlier, allowing us to detect a sale as soon as possible (which is helpful for the interpretation of the event-study analysis).

Figure 5.b shows the evolution of the sales outcome, corresponding to the cumulative fraction of properties in the subject pool. Note that the fraction of homes sold increases smoothly over time. By 12 weeks after the start of letter delivery, 36.99% of homes had been sold. By 20 weeks after the start of letter delivery, 50.6% of homes had been sold. By 28 weeks, the end of our panel data, 57.5% of the properties had been sold.

# 5 Main Results

## 5.1 Effects on Market Choices

We first present the main results from the field experiment: the reduced form effects of information shocks on market choices of homeowners. Our main outcome of interest is whether the property was sold a given number of weeks after the start of the mail delivery. When interpreting these estimates, it is crucial to keep in mind that the effects of the information shocks on the sales outcome are not expected to materialize instantaneously. First, while some letters were read shortly after the start of the letter delivery, most letters were not read until a few weeks after that date. Second, even after the subject has read the letter, some

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<sup>14</sup> We asked the mailing company to provide a guess for when the first letters would be delivered based on the location of the shipping facility (Lombard, Illinois) and the location of the letter recipients.

<sup>15</sup> While we have not processed or looked at the data yet, our algorithm continued to automatically scrape the data on a weekly basis. As a result, if needed, we could look at longer time horizons too.

time needs to pass so that the owner has the opportunity to act on their updated expectations: it may take one or more weeks for the seller to receive an offer; and after the offer is accepted, it may take one or more weeks for the property to appear as sold in the records of the Multiple Listing Service or the county assessor’s office.

The main regression results are presented in Table 2. Each column corresponds to a different regression. All regressions in this table are based on data from the field experiment and using the same dependent variable: an indicator variable ( $S_{+12w}$ ) that takes the value 100 if the property was sold at 12 weeks after the start of the letter delivery and 0 otherwise. 36.99% of the properties were sold within this time horizon. We use this time horizon for the baseline results just because it provides enough time so that all subjects could have been affected by the information shocks.<sup>16</sup> In any case, later we reproduce the estimates for every possible time horizon and show that the results are robust.

We first examine the effects of information shocks by separately exploiting our two distinct sources of experimental variation (source-randomization and disclosure randomization). Column (1) of Table 2 corresponds to the specification that uses the source-randomization only: i.e., equation (1) from Section 2, which is restricted to the sub-sample of subjects who received signals (i.e.,  $D_i = 1$ ). *Information Shock* in Table 2 always corresponds to the coefficient on the key independent variable (in this specification,  $E_m^{j_i^*}$ ). The results from column (1) indicate that the information shocks significantly affected homeowners’ market choices: the information shock has a large negative effect on the probability that the property is sold (-0.318) and highly statistically significant (p-value=0.009). This negative sign is consistent with the prediction from economic theory: i.e., a positive shock to expectations should decrease the probability that the property is sold. This coefficient is also economically large: an increase in the information shock of just 1 pp causes a 0.318 pp drop in the probability that the property is sold within the following 12 weeks. Since the right-hand-side and left-hand-side variables are measured in percentage points, this coefficient can be interpreted as a behavioral elasticity of -0.318. While this effect is already economically significant, note that it reflects an intention-to-treat effect, because the information shocks should not be expected to fully materialize in changes to expectations. We defer a more careful discussion on economic magnitude to Section 7 below, where we estimate a treatment-on-the-treated effect by correcting for various sources of non-compliance.

Using the variation induced solely by disclosure-randomization yields similar estimates. Results are presented in column (2) of Table 2 (corresponding to equation (3), that uses the

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<sup>16</sup> According to the read-receipt proxy from Section 4.5, virtually all subjects read our letter within eight weeks after the start of the letter delivery. As a result, when looking at the sales outcome at 12 weeks after the start of the letter delivery, most subjects had been “exposed” to the information for 4–11 weeks, thus allowing sufficient of time for the information to affect the sales outcomes.

entire subject pool). As in column (1), the results from column (2) indicate that the information shock has an effect on market choices that is negative, large (-0.419) and statistically significant (p-value=0.014). Most notably, the coefficients are similar in the specification that uses solely the source-randomization (-0.318, from column (1)) as in the specification that uses solely the disclosure-randomization (-0.419, from column (2)) – in fact, we cannot reject the null hypothesis that these two estimates (-0.318 and -0.419) are equal (p-value=0.577). The fact that the results are highly consistent across two very different experimental designs is quite re-assuring about the validity of the identification strategies.

To maximize statistical power, in column (3) of Table 2 we use the specification from equation (4) that combines the shocks induced by both the source-randomization and the disclosure-randomization. Again, we find that the information shock has an effect on market choices that is negative, large (-0.350) and highly statistically significant (p-value=0.001). Given that it combines both sources of variation, it should not be surprising that the pooled specification yields an estimated value (-0.350, from column (3)) in between the corresponding estimates from the specification that uses the source-randomization (-0.318, from column (1)) and the disclosure-randomization (-0.419, from column (2)). As anticipated in the pre-registration, we use this pooled specification in the remainder of the paper due to its superior statistical power.

The baseline specification from equation (4) assumes that the effects of information shocks on market choices are linear. This functional form assumption is natural for two reasons. First, in theory, this simple linear specification is consistent with a simple model of Bayesian learning (Cavallo et al., 2017). Most importantly, in practice, several information-provision experiments found this linear specification to fit the data quite neatly in a variety of contexts such as home price expectations and inflation expectations (Armantier et al., 2016; Cavallo et al., 2017; Bottan and Perez-Truglia, 2020; Fuster et al., 2020; Cullen and Perez-Truglia, 2018). In theory, however, this functional form specification may miss some important features. For example, this specification may miss asymmetries: e.g., individuals may find it easier to react to good news than to bad news. This specification may also miss non-linearities: e.g., some individuals may only consider signals that are not too extreme. To take a less parametric look at the data, Figure 6.a presents the binned scatterplot version of the baseline results from column (3) of Table 2. The results indicate that the baseline specification fits the data well. Moreover, this binned scatterplot confirms that the results are not driven by non-linearities or by outliers.

The next robustness test exploits the timing of when subjects read the letters. The letters were being received and read progressively over a specific period of time. Thus, we can verify if the timing of the estimated effects is consistent with the timing of when subjects read

the letters. Figure 7.a presents an event-study analysis of the effects of our information intervention. Each coefficient in Figure 7.a corresponds to a different regression using the same baseline specification from column (3) of Table 2 but with different dependent variables. For example, the leftmost coefficient uses a binary dependent variable that takes the value 100 if the property was sold 2 weeks before the first letter was delivered.<sup>17</sup> The next coefficients correspond to a horizon of 1 week prior to the start of the letter delivery, then 1 week after the letter delivery, and so on and so forth until the farthest horizon for which we have data (28 weeks after the start of letter delivery). To facilitate the comparison of timing of read-receipts and the effects of the information, the bottom half of Figure 7.a shows the evolution of the read-receipt proxy.

The evidence indicates that the timing of the experimental effects is largely consistent with the timing of letter delivery. First, the information contained in the letter should not have any effects prior to the start of the letter delivery, because the subjects had not received the letters yet. As expected, Figure 7.a shows estimated coefficients for the two pre-treatment horizons (i.e., the two leftmost coefficients) that are close to zero, precisely estimated and statistically insignificant.

Next, we test two predictions about the timing of the effects in the post-treatment period. First, we expect the effects of the information shocks not to materialize immediately, but to build up gradually over the course of weeks as more and more letters are being read. Second, due to the nature of the sales process in the real estate market, some of the effects of the information shocks may lag a few weeks behind the read-receipt of the letters. More precisely, after reading our letter, some sellers may have been sitting on an offer already, but for other sellers it may take one or two weeks to receive their first offer and thus have the opportunity to sell. And even after a seller chooses to accept an offer, due to the standard real estate closing process, it may take one or more weeks for that property to appear as sold in the records of the Multiple Listing Service or the county assessor’s office.

Figure 7.a shows that these two predictions about the post-treatment coefficients are borne out by the data. The effects of the information shocks build up over time at a similar rate as the read-receipt of the letters, and with a lag of a couple of weeks. Figure 7.b provides a complementary view of the same results from Figure 7.a. For each possible weekly horizon, Figure 7.b plots our proxy for the share of letters read (in the x-axis) against the estimated effect of the information shocks (in the y-axis). Again, we see that the effects of the information shocks are close to zero prior to the start of the letter delivery, and then they become negative and grow stronger as more and more subjects read the letters.

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<sup>17</sup> This is the earliest horizon we can use, since this is the date when the administrative data was first downloaded to create the letters.



Figure 7.a also provides some useful information about the persistence of the effects. If the information made some sellers delay their decision by a matter of just a few weeks, we would expect the post-treatment coefficients in Figure 7.a to quickly revert back to zero. On the contrary, we find that the effects of the information shocks were highly persistent, remaining as strong at six months after the start of letter delivery as they were at three months after the start of the letter delivery. More precisely, Figure 7.a shows a coefficient for 28 weeks later that is negative (-0.295), precisely estimated and statistically highly significant (p-value=0.006). Moreover, this coefficient at the 28-week horizon (-0.295) is similar in magnitude, and statistically indistinguishable from, the corresponding coefficient for the 12-week horizon (-0.350).

In Section 4.3, we show that the pre-treatment characteristics were balanced across the six treatment groups. However, given that our econometric model goes beyond a simple comparison of means, this balance test is useful but not sufficient. In the spirit of Chetty et al. (2014), for a more direct falsification test we reproduce the same regression from the baseline specification (column (3) of Table 2) but using pre-treatment characteristics as dependent variables. One challenge with this type of falsification analysis is that the dependent variables have different distributions and thus the coefficients are not directly comparable across different regressions. As in Chetty et al. (2014), we standardize the coefficients by dividing them by the standard deviation of the corresponding dependent variable. Then, we multiply the standardized coefficients by 100, for readability.

The results from these falsification tests are presented in Figure 8. The two standardized coefficients on the left hand side of the figure correspond to two of the post-treatment outcomes shown in the Figure 7.a: whether the property was sold at 12 or 28 weeks after the start of letter delivery. The six standardized coefficients on the right hand side of the figure are based on the exact same regression specification, but using “placebo” dependent variables based on pre-treatment characteristics: the number of days the property was listed, the initial listing price, the number of bedrooms, the number of bathrooms, the square footage of the building and the lot size. Because each of these six placebo outcomes were determined before the letters were mailed, the information shocks should have no effect on them. As expected, the coefficients on *Information Shock* for these falsification outcomes are close to zero, statistically insignificant and precisely estimated. Moreover, these six placebo coefficients are statistically different from the corresponding effects on the post-treatment outcomes. For example, the standardized coefficients (times 100) are -0.725 (p-value=0.001) for the sales probability at 12 weeks post-treatment and -0.037 (p-value=0.868) for the pre-treatment number of days listed. Furthermore, the difference between these two coefficients



(-0.725 and -0.037) is statistically significant (p-value=0.021).<sup>18</sup>

## 5.2 Additional Robustness Checks

Columns (4)–(11) of Table 2 present some additional robustness checks. Columns (4)–(6) show the results under some alternative regression specifications. The specification from column (4) is identical to that of column (3), only that it includes a host of additional control variables: the number of the days the property was on the market prior to the experiment, the initial listing price, a set of four indicator variables for the number of bedrooms, four indicator variables for the number of bathrooms, and the square footage of the building and the lot. Because the treatment is randomized, controlling for additional variables should not significantly affect the coefficient on *Information Shock*. As expected, the point estimate with the additional controls (-0.341, from column (4)) is almost identical to, and statistically indistinguishable from, the baseline coefficient (-0.350, from column (3)).

In the baseline specification (column (3) of Table 2) we control for one dummy variable that indicates if the information was disclosed to the subject. Because letters disclosed information from different sources, one might worry that the source being presented may have an effect on its own, above and beyond the effect of its signal. For example, when sharing information sources like Past-1 and Past-2, the reader may be prompted to think about the past and that may have an effect of its own. To address that concern, the specification from column (5) of Table 2 is identical to the baseline specification of column (3), except that instead of controlling for one disclosure indicator, it controls for a set of five disclosure indicators (i.e., one for each information source). The results are virtually identical in this alternative specification as in the baseline specification. For instance, the coefficient from the alternative specification (-0.349, from column (5)) is almost identical to the coefficient from the baseline specification (-0.350, from column (3)), and their difference is statistically insignificant (p-value=0.999).

The baseline specification (column (4) of Table 2) controls for market fixed effects. Those fixed effects are meant to isolate the exogenous variation of the source-randomization, by controlling flexibly for the set of signals that a given subject could have been assigned to. While this baseline specification is feasible in the context of our large-scale field experiment, where there are multiple subjects per market, it has the limitation that it would not be feasible in smaller datasets. For example, this specification cannot be estimated with our

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<sup>18</sup> This equality test between two coefficients is based on the same data but different regressions. To allow for a non-zero covariance between these two coefficients, we estimate a system of seemingly unrelated regressions. In the remainder of the paper, when comparing coefficients from the same data but different regressions, we always use this method.

auxiliary survey experiment, for which there is typically one subject per market. Column (6) of Table 2 is identical to that of column (3), except that instead of the market fixed effects, it controls for the potential feedback in a less data-intensive way: it includes five linear terms, one for each of the five potential signals that the subject could have been assigned to (i.e., one per information source), as well as five sets of dummies breaking those variables in deciles to deal with any potential non-linearities. The results from this less-demanding specification are almost identical to the results from the baseline specification: the coefficient from the less-demanding specification (-0.330, from column (6)) is almost identical in magnitude to, and statistically indistinguishable from, the corresponding coefficient from the baseline specification (-0.350, from column (3)).

In columns (7)–(11) of Table 2, we explore whether the results are sensitive to dropping observations for each of the five information sources, on a one-by-one basis. Relative to the baseline specification, the resulting coefficients are a bit less precisely estimated, because dropping each treatment group entails throwing away between 15% and 20% of the observations. However, the results are always similar in magnitude, precisely estimated and statistically significant. The six coefficients from columns (7)–(11) (-0.353, -0.307, -0.364, -0.411, and -0.331) are similar in magnitude to the corresponding coefficient from the baseline coefficient (-0.350, from column (3)), thus confirming that the results are not driven by any single information source.<sup>19</sup>

Due to space constraints, some additional robustness checks and results are presented in the Appendix. In the previous analysis, the outcome variable is the probability that property is sold at a given point in time. An alternative outcome variable would be the number of days elapsed from the start of letter delivery to the sale of the property. As we anticipated in the pre-registration, a problem with this outcome is that it is truncated: for 42% of properties in the sample that were not sold by the end of the sample window, we do not know when they will ultimately be sold. By looking at the probability of selling at a given point in time, we avoid having to deal with any sort of truncation bias. In any case, there is a host of duration models that can deal with this type of truncation. We report those results in Appendix A.7, which are qualitatively and quantitatively similar to the results from the baseline specification presented above.

The data that we scraped to construct the main outcome of interest contained other

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<sup>19</sup> A related question is whether the information about the past (Past-1 and Past-2 treatments) was more or less compelling than the information about the forecasts (Forecast-1, Forecast-2, and Forecast-3 treatments). For example, if most subjects have backward-looking expectations, they may be more elastic to information about the past than to the forecasts (Case and Shiller, 1989; Shiller, 2005). In Appendix A.8, we provide some suggestive evidence that the backward-looking information may have been more effective than the forecasts – however, that result must be taken with a grain of salt due to lack of sufficient statistical power.

information that can be used to construct secondary outcomes. Appendix A.9 presents the effects on these secondary outcomes, which shed light on the causal mechanisms at play. We show that there were some significant effects on listing prices: more optimistic home price expectations resulted in a lower probability of dropping the listing price. We also show that the sellers with more optimistic expectations kept their properties on the listing website, suggesting that they were still waiting for better offers to come along.

## 6 The Supplemental Survey Experiment

We designed a supplemental survey experiment that randomized the same information included in the field experiment but was designed to be conducted on an auxiliary sample of respondents recruited from an online platform. The goal of this supplemental survey experiment was twofold. The first goal was to provide direct evidence that the information shocks had the intended effects on expectations. The second goal is to quantify the strength of the effects of information shocks on expectations, which we use to scale-up the intention-to-treat estimates.

### 6.1 Survey Design

The survey instrument was part of the same RCT pre-registration used for the field experiment. Moreover, the supplemental survey was also conducted around the same dates when the main field experiment was conducted.<sup>20</sup> Appendix E includes the full survey instrument, which is summarized below:

- **Step 1 (Elicit Property Details):** To provide randomized information relevant to the respondent, we asked respondents about their current residency, such as the number of bedrooms and 5-digit ZIP code.
- **Step 2 (Elicit Prior Belief):** Respondents were shown the current median home value (in May 2019) for a similar home (same number of bedrooms and ZIP code) and were asked to report their expectations for that median value one year later (in May 2020).

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<sup>20</sup> The online survey that was included in the letter, which we use to construct the read-receipt proxy, included some survey questions about home price expectations (see Appendix D for the full survey instrument). However, based on previous studies, and as stated in the RCT pre-registration, we anticipated that this additional survey data would be inadequate for the analysis. For example, [Perez-Truglia and Troiano \(2018\)](#) conducted a mailing intervention in the context of tax compliance that included a link to an online survey similar to ours, but they received a response rate of just 0.2%. In line with that study, the response rate was to the survey link included in the letter was extremely low: 0.79% (i.e., 455 out of the 57,910 subjects completed the survey).

- **Step 3 (Information-Provision Experiment):** All respondents were told that some survey participants will be randomly chosen to receive information about home prices. On the following screen, respondents find out the information selected for them. Respondents were assigned to one of the same six treatment groups from the field experiment described in Section 3.1.
- **Step 4 (Elicit Posterior Belief):** On the following screen, subjects were told that all participants can reassess their guess about future home prices, regardless of what they guessed initially or the information that they received from us. We re-elicited the same question about their expectations of the median price one year later, as well as an additional question on their expectations five years later. As a placebo outcome, respondents were asked about their stock market expectations: i.e., they were told the closing price of the Dow Jones on May 31st and asked what they expected the price to be 12 months later.<sup>21</sup>

We recruited subjects from Amazon’s Mechanical Turk (AMT) online marketplace. We followed several best practices for recruiting participants and ensuring high-quality responses via AMT (Crump et al., 2013). We restricted participants to those who resided in the United States. We offered a \$0.75 participation reward for a 5-minute survey and collected responses from June 21 to June 24. The final AMT sample included 1,404 respondents. Appendix A.6 presents more details about the AMT sample. For example, we show that relative to the field experiment sample and the universe of U.S. homeowners, the online sample is younger, less wealthy, and lives in smaller and cheaper homes. And consistent with successful random assignment, the observable characteristics were balanced across treatment groups.

## 6.2 Effects on Survey Expectations

Unlike the field experiment, in the auxiliary survey we *do* observe subjects’ the prior beliefs. As a result, we can use that information to estimate the updating of expectations as in more traditional information-provision experiments.

Learning models, such as the Bayesian model, predict that respondents who overestimated relative to the signal would revise their beliefs downward when shown the signal, while those who underestimated with respect to the signal would revise their beliefs upward. Figure 9 provides a simple test of this prediction. This figure provides a binned scatterplot between the prior gaps (i.e., the difference between the signal chosen for the subject and the subject’s prior

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<sup>21</sup> The survey included some additional questions that could be used for disentangling mechanisms and for heterogeneity analysis (see Appendix A.6 for more details). The survey also included a short follow-up module (see Appendix A.6 for more details).

belief, in the x-axis) and the belief revisions (i.e., the difference between posterior and prior beliefs, in the y-axis). Intuitively, the x-axis shows the maximum revision we would expect if the respondent were to fully react to the information, and the y-axis shows the observed revision. The red diamonds from Figure 9 correspond to individuals in the treatment group (i.e., those who were shown the signal). As expected, there is a strong relationship between the belief revisions and prior gaps for individuals in the treatment group: an additional 1 pp in perception gap is associated with a 0.545 pp higher revision (p-value<0.001). In contrast, the blue circles show a much weaker relationship among individuals in the control group: an additional 1 pp in the prior gap is associated with a revision of 0.10 pp (p-value<0.001). This finding suggests a statistically significant but economically small degree of spurious revision, consistent with the results from other information-provision experiments (Armantier et al., 2016; Cavallo et al., 2017; Bottan and Perez-Truglia, 2020; Fuster et al., 2020; Cullen and Perez-Truglia, 2018).

The results from Figure 9 indicate that when receiving a signal, the average subject is updating posterior belief, but not fully. There are many reasons why subjects, even if fully rational, may update partially, or may even not update at all. For example, the subject may have already seen the information, or may feel more confident in their prior beliefs than in the signal. Indeed, the degree to which individuals incorporated the information provided in the experiment is comparable in magnitude with the findings from other survey experiments. The most direct comparison is with respect to Fuster et al. (2020), who conducted a survey experiment in the 2017 Federal Reserve Bank of New York’s Survey of Consumer Expectations. They randomly provided subjects with signals from similar information sources as those used in this paper (e.g., price changes over recent years and professional forecasts for the next year) and then measured the effects of the information provision on the subsequent home price expectations. The results from our paper are not expected to be identical to those in Fuster et al. (2020), because of differences in implementation and survey design. For example, Fuster et al. (2020) concerns expectations about national home prices while our supplemental survey revolves around local home prices. There are even more significant differences in how the respondents were recruited: while our supplemental survey relies on a convenience sample from AMT, Fuster et al. (2020) is based on a nationally representative survey conducted by the Federal Reserve Bank of New York.

Despite these significant differences in methods and the sample, the results from our supplementary survey experiment are quantitatively similar to the results from Fuster et al. (2020). According to a simple Bayesian learning model, Fuster et al. (2020) estimates that their average subject forms posterior beliefs by assigning a weight of 0.380 to the signal and

the remaining weight of 0.620 to the prior belief.<sup>22</sup> In comparison, when applied to our own supplementary survey, the same model indicates that subject forms posterior beliefs by assigning a weight of 0.445 to the signal.<sup>23</sup> These two learning rates (0.380 and 0.445) are in the same ballpark. Moreover, the rate of learning we estimated in our survey experiment is also in the same order of magnitude as the corresponding estimates in studies that involve different types of macroeconomic expectations. For instance, Cavallo et al. (2017) reports that, when forming inflation expectations, the average Argentine respondent assigns a weight of 0.432 to the signal about inflation.<sup>24</sup>

Figure 9 presents a detailed analysis of belief updating, which relies heavily on the data on prior beliefs. For a more direct comparison to the results of the main field experiment, Table 3 presents the results of the supplemental survey experiment using the exact same regression specification used in the main field experiment, which does not require any data on prior beliefs. More precisely, all regressions in Table 3 use the exact same econometric specification as in column (6) of Table 2, but with different dependent variables.<sup>25</sup> In column (1), the dependent variable is the posterior belief about the home price expectation one year ahead ( $H_{1y}^{post}$ ). The coefficient on *Information Shock* from column (1) is positive (0.205) and statistically significant (p-value=0.001). A 1 pp increase in the information shock causes an increase in home price expectations of 0.205 pp. In other words, there is a 20.5% “pass-through” from the information shocks to the expectations. The fact that the coefficient on *Information Shock* is significantly greater than zero implies that the subjects found the information provided in the experiment relevant to form their home price expectations. The fact that the coefficient on *Information Shock* is significantly less than one is due to a variety of factors, such as the fact that some subjects felt confident about their prior beliefs, already knew the information, or were just not paying enough attention to the survey.

Columns (2) through (4) of Table 3 present the effects on other survey outcomes. The dependent variable in column (2) is identical to the dependent variable in column (1), except that it corresponds to the expectation for five-years-ahead instead of one-year-ahead. The coefficient on *Information Shock* is positive (0.167) and statistically significant (p-value=0.017). This result implies that the information shocks affected not only the more immediate expectations (one-year-ahead) but also the more distant expectations (five-years-ahead). The point

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<sup>22</sup> This estimate corresponds to the slope reported in Figure 3.a of Fuster et al. (2020).

<sup>23</sup> This estimate is based on the difference in slopes between the treatment and control groups from Figure 9 – for more details about this methodology, see Fuster et al. (2020).

<sup>24</sup> This estimate corresponds to coefficient  $\alpha$ -statistics reported in Panel B, column (1) of Table 1 from Cavallo et al. (2017).

<sup>25</sup> As explained in the above section, we cannot use the specification from column (3) of Table 2 because the supplemental survey does not have multiple subjects with properties of the same type and in the same ZIP Code.

estimate from column (2) is smaller in magnitude than the point estimate from column (1), suggesting that the information shocks affected the one-year-ahead expectations more strongly than the five-years-ahead expectations; however, that comparison must be taken with a grain of salt because the difference between the coefficients from column (1) and (2) is statistically insignificant (p-value=0.560).

Columns (3) and (4) of Table 3 present some falsification tests. The dependent variable from columns (3) is identical to that in column (1), except that while column (1) corresponds to the posterior belief, column (3) refers to the corresponding prior belief (i.e., the home price expectations elicited *before* the information-provision experiment). Since information shock had not been administered to the subject yet, it should have no effect on prior beliefs. As expected, the coefficient on *Information Shock* from column (3) is close to zero (-0.014), statistically insignificant (p-value=0.837), and precisely estimated. Moreover, we can reject the null hypothesis that the effect on posterior beliefs (0.205, from column (1)) is equal to the falsification coefficient (-0.014, from column (3)), with a p-value<0.001.

Column (4) of Table 2 presents another falsification test. The dependent variables in columns (1) and (4) are both posterior beliefs (i.e., elicited after the information-provision experiment). However, whereas the dependent variable from column (1) corresponds to the home price expectations, the dependent variable from column (4) corresponds to stock market expectations. Because the information shock is specific to local home prices, we do not expect individuals to extrapolate this information to the stock market expectations.<sup>26</sup> As expected, the coefficient on *Information Shock* from column (4) is close to zero (0.017), statistically insignificant (p-value=0.899), and precisely estimated.

As a final robustness check, Figure 6.b presents a binned scatterplot version of the results from column (1) of Table 3. As discussed in Section 2 above, the baseline specification assumes that the effects of the information shocks are linear and symmetric around zero. The results from Figure 6.b indicate that the linear specification fits the data quite well, and also confirms that the results are not driven by non-linearities or by outliers.

## 7 Magnitude of the Effects

The results from the main field experiment presented in Section 5 indicate an elasticity between the information shocks and the probability of selling the property within 12 weeks of -0.350 (column (3) of Table 2). However, the magnitude of this elasticity must be taken

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<sup>26</sup> If, on the contrary, subjects did extrapolate to stock market expectations, that would suggest spurious motives behind the belief updating such as numerical anchoring or experimenter-demand (Cavallo et al., 2017).



with a grain of salt, because it corresponds to an intention-to-treat effect. In this section, to better assess the true magnitude of the effects, we use complementary data to scale up from intention-to-treat effects to treatment-on-the-treated effects.

## 7.1 First Form of Non-Compliance

The most basic form of non-compliance is that some subjects may not read the letter, and thus whatever information was included in the letter will have no chance of changing their expectations. To correct for this form of non-compliance, following [Perez-Truglia and Cruces \(2017\)](#) and others, we need to estimate the *reading rate*: i.e., the share of recipients who *actually* read the letter and on time. For that, we need to figure out the share of letters that were not delivered, were delivered but not read, or were read but after it was too late (i.e., after the owner had already accepted an offer). We estimate each of these shares using various sources of data. The main estimates are summarized below, while the details are discussed in [Appendix A.10](#). According to the U.S. Monitor Non-Profit Standard Mail Delivery Study, around 5% of standard non-profit mailers fail to be delivered ([U.S. Monitor, 2014](#)). Based on data from the U.S. Postal Service Household Diary Survey ([Mazzone and Rehman, 2019](#)), conditional on delivery, around 26% of the letters were ignored. And based on the timing of survey responses and home sales in the baseline group, we estimate that roughly 7.7% of the letters were not read until after the property was sold. Combining these three estimates leads to a reading rate of 64.9% ( $= 0.95 \cdot 0.74 \cdot 0.923$ ). Following [Perez-Truglia and Cruces \(2017\)](#), to account for the first source of non-compliance we can divide the intention-to-treat elasticity (-0.350, from column (3) of [Table 2](#)) by the the reading rate. This adjustment results in a scaled-up elasticity of -0.539 ( $\frac{-0.350}{0.649}$ ).

## 7.2 Second Form of Non-Compliance

The second source of non-compliance is that, even if they read the letter, the information shocks introduced in the letter may not change the home price expectations of the recipients. To address this second form of non-compliance, we can combine the estimates from the main field experiment and the supplemental survey experiment.

An important caveat, however, is that this exercise relies on the assumption that the reaction to the information in the supplemental survey experiment is a reasonable approximation to the reaction to the information in the main field experiment. Indeed, there are multiple reasons for why, in theory, the reaction to the information may have been somewhat stronger or weaker in one context than in the other.<sup>27</sup> As discussed in [Section 6](#) above, how-

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<sup>27</sup> On the one hand, maybe the subjects in the supplemental survey paid less attention to the information



ever, the degree to which subjects updated their expectations in our supplemental survey is remarkably similar to the corresponding degrees in related studies concerning not only home price expectations but also other types such as expectations about the inflation rate or the GDP growth. Indeed, even within a given sample, it has been documented that subjects update their expectations to a similar degree regardless of their characteristics such as gender, income or education (Fuster et al., 2020). For these reasons, it is not unreasonable to extrapolate the findings from the supplementary survey to the main field experiment.

With these caveats in mind, we proceed with combining the two sets of results. The results from the field experiment indicate that a 1 pp higher information shock decreased the probability of selling the property within 12 weeks by 0.350 pp (column (3) of Table 2). The survey experiment indicates that a 1 pp higher information shock increased home price expectations by 0.205 pp (column (1) of Table 3). Jointly, these two results imply that a 1 pp increase in home price expectations causes a decline of 1.70 pp ( $= \frac{0.350}{0.205}$ ) in the probability of selling the property within 12 weeks: i.e., a behavioral elasticity between expectations and sales probability of -1.70.<sup>28</sup> Moreover, if we adjust for the two forms of non-compliance simultaneously, we arrive to a scaled-up behavioral elasticity of -2.63 ( $\frac{-0.350}{0.205 \cdot 0.649}$ ).

This elasticity of -2.63 between market choices and home price expectations suggests that sellers are highly elastic. Moreover, this estimate implies that changes in home price expectations across individuals and over time may be a powerful driver of choices in the housing market. For example, according to the prior belief measures in our supplemental survey, the standard deviation in home price expectations across individuals is 5.39 pp.<sup>29</sup> According to the elasticity of -2.63, an increase in home price expectations of this magnitude (5.39 pp) would reduce the probability that one of our subjects sells the property within 12 weeks by 14.18 pp ( $= 2.63 \cdot 5.39$ ). In other words, a one standard deviation increase in home price expectations causes a decrease in the sales probability of 0.29 standard deviations.<sup>30</sup>

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than the subjects in the field experiment, because they do not have strong financial incentives to stay informed on local home prices. On the other hand, perhaps the subjects in the supplemental survey paid more attention to the information because it is shown on the computer screen seconds before the elicitation of the posterior expectations.

<sup>28</sup> In practice, the decision to sell probably depends jointly on expectations at many horizons (e.g., 1 month, 12 months, 5 years). This back-of-the-envelope calculation focuses exclusively on the 1-year horizon, because we do not have sufficient data or exogenous variation to disentangle the contribution of the different horizons.

<sup>29</sup> This measure corresponds to the raw standard deviation. The results are similar (5.51 instead of 5.39) if we instead use the standard deviation within a given property type (i.e., same ZIP code and number of bedrooms).

<sup>30</sup> To arrive at this estimate, we use the standard deviation in the probability of selling the property within 12 weeks: 48.28 pp, as reported in column (3) of Table 2.

### 7.3 Other Forms of Non-Compliance

There are a couple of additional sources of attenuation bias that are worth mentioning but that we do not adjust for. To the extent that it does not account for these additional sources of attenuation bias, the elasticity of -2.63 reported above constitutes a conservative estimate.

One form of attenuation bias is that the home price expectations may be irrelevant for the decisions of some sellers. More precisely, if after selling the current property the seller plans to buy another property in the same location, then the seller will be exposed to the same home price appreciation regardless of when the current property is sold or whether it is sold at all. Thus, for these sellers, their home price expectations should not be relevant for their selling decision. However, we present some data indicating that the scope for this form of attenuation is probably minor because only a small minority of subjects would be subject to it. For instance, using the administrative data from the county assessor’s records, we find that after selling the properties, only 8% of subjects bought another property in the same county.<sup>31</sup> We reach estimates of the same order of magnitude using an alternative data source: mail forwarding data from the United States Postal Service. Moreover, our estimates are also in the same ballpark as the estimates reported by other analysts using independent sources of data (Zillow, 2019). Under the assumption that the above 8% of subjects do not care about home price expectations, we would need to further scale-up our estimates by a factor of just 1.087 ( $= \frac{1}{0.92}$ ).

Another potential source of attenuation bias relates to spillovers in the information treatments. For example, one subject who received a signal from the source Past-1 could share that signal with a subject who received information from the source Forecast-1. In our baseline regression model, this type of information spillovers would lead to an attenuation bias. There are some contexts in which the room for information spillovers can be significant, such as distributing information among classmates or coworkers (Cullen and Perez-Truglia, 2018). In the context of our housing experiment, however, the scope for spillovers is negligible. We distributed information to a tiny share of all the households who live in the counties where the experiment was conducted. As a result, the subjects who did receive information were geographically distant from each other. These subjects are highly unlikely to know each other, much less to have opportunities to share the information included in our letters.<sup>32</sup> Moreover, the type of information that we included in the letter was tailored to each subject (i.e., to their specific ZIP Code and property type), which limits the scope of spillovers even further. There are standard methods for measuring and accounting for spillovers in an experiment like ours (Cullen and Perez-Truglia, 2018; Nathan et al., 2020). However, given how negligible

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<sup>31</sup> For details, see Appendix A.11.

<sup>32</sup> For more details, see Appendix A.13.

the spillovers are in our context, we did not bother making these adjustments.

## 8 Heterogeneity Analysis

The evidence presented above indicates that, when deciding whether to sell a property, the average subject cares quite a lot about home price expectations. Next, we explore whether that average effect masks some meaningful heterogeneity across subjects. The heterogeneity analysis is presented in Figure 10. The coefficient at the very top of the figure corresponds to the effect for the average subject (-0.350, from column (3) of Table 2). The rest of the coefficients are divided in pairs, corresponding to the same regression specification but estimated in split samples.

We split the sample by a series of observable characteristics of the property, the seller or the market, which are summarized below. The first form of heterogeneity presented in Figure 10 corresponds to the owner occupancy status. We pre-registered this form of heterogeneity based on the widespread conjecture that non-owner-occupied properties have a disproportionate influence on speculation in the housing market (Gao et al., 2020). We split the sample by whether the owner was living in the property at the time of letter delivery (66.99% of subjects) or not (33.01% of subjects).<sup>33</sup> It is possible that home price expectations are more relevant for homes that are more expensive. To shed light on this question, we split the properties by whether the listing price is above or below the median listing price in the sample (\$359,900). Our letters provided information about the median home values. Recipients may trust the information more when their listing price is close to that median home value, because they may infer that the information is more applicable to their own property. Thus, we split the subjects by whether the initial listing price of the listed property is closer or farther away from the median home value in their market. It is possible that owners react differently depending on whether they started with a more aggressive or conservative listing price. We split the sample by whether the initial listing price is above or below the estimated market value for that same property according to the website Redfin.com. For subjects for which the demographic data was available via a data vendor, we split them by gender, age and ethnicity.<sup>34</sup> Last, we consider a number of characteristics of the local market. We split the sample by counties from Florida (68% of the subjects) versus the rest of the

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<sup>33</sup> We can split subjects in this way by comparing the mailing address to the property address in the county assessor’s office records. Since some mailing address may take some time to be updated in the official records, we complemented those records with the mail forwarding data from the United States Postal Service.

<sup>34</sup> The vendor was able to provide data for most, but not all, the homeowners in the subject pool. For more details about this complementary data see Appendix A.4.

counties. To differentiate between more and less expensive areas, we split the ZIP Codes by whether their median home values are above or below the median ZIP Code. And we also split ZIP Codes by a measure of seller market power (Zillow’s Seller Power Index) as well as the market size (Zillow’s Size Rank).

Figure 10 shows that the effects of home price expectations are similar across the board. None of the pairwise differences in Figure 10 are statistically significant at conventional levels. And almost all of the pairwise differences are close to zero.<sup>35</sup> The most notable difference are by owner occupancy status and age. We find that the effects of the information shocks are almost twice as strong for the non-occupant owners (-0.439, p-value=0.025) as for the occupant owners (-0.247, p-value=0.060). The difference between those two estimates (-0.439 and -0.247) must be taken with a grain of salt, however, as it is statistically insignificant (p-value=0.409).<sup>36</sup> We also find that the point estimates were larger (in absolute value) for individuals aged 59 or older (-0.404, p-value=0.032) than for individuals below the age of 59 (-0.058, p-value=0.781). This result must be taken with a grain of salt, however, as the difference in coefficients (-0.404 and -0.058) is statistically insignificant (p-value=0.220). Indeed, the data on the seller’s age is missing for a third of the sample, and as a result the heterogeneity by age is less precisely estimated.

The differences by occupancy status and age are statistically insignificant, so we should not draw any strong conclusions. Moreover, even if those differences were statistically significant, we should still be cautious with our interpretation because we did not randomize the owner occupancy status or the age of the subjects. With those caveats in mind, we believe that the above evidence may be suggestive of optimization frictions. Owners who are non-occupants and owners who are age 59 or older probably face less frictions when deciding whether to sell their home or when to sell it, and thus are better able to react when their expectations change. Occupant owners need to move out of the property after selling it, while non-occupant owners do not need to move out.<sup>37</sup> And relative to those aged 59 or above, owners aged below 59 years are more likely to experience changes in jobs, school or marital status that may put constraints on when the property can be sold.<sup>38</sup>

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<sup>35</sup> These results must be taken with a grain of salt, however: given the statistical precision, in some cases we cannot rule out moderate or even large differences.

<sup>36</sup> In Appendix A.12 we reproduce this heterogeneity analysis for other time horizons, and the results are broadly consistent.

<sup>37</sup> In principle, the large heterogeneity by occupancy status could be due to other factors. In Appendix A.12 we discuss alternative explanations and present some evidence suggesting they are less plausible.

<sup>38</sup> Our interpretation of optimization frictions in the housing market is related to evidence of optimization frictions on another important asset market: the market for stocks and bonds (Giglio et al., 2019).

## 9 Conclusions

We provide the first experimental evidence on the causal effects of home price expectations on market choices by conducting a large-scale, high-stakes, natural field experiment involving 57,910 U.S. individuals who recently listed homes on the market. To generate exogenous shocks to their home price expectations, we send each of these homeowners a letter containing randomized information about their home prices. We then use administrative records to measure the effects of those information shocks on the recipients' subsequent sales outcomes. Consistent with economic theory, we find that higher home price expectations cause the homes to take longer to sell. Moreover, the magnitude of this reaction indicates that market choices are highly elastic to expectations: a 1 pp increase in home price expectations reduces by 2.63 pp the probability that the property will sell in the following 12 weeks.

Our estimates address an open question that is central to the literature on macroeconomics and finance: what is the causal effect of expectations on market choices? We believe our experimental framework can be adapted to study other questions in a range of fields (e.g., macroeconomics, urban economics, finance, real estate economics, and behavioral economics) because the mailing can include any type of information hypothesized to be relevant in the decision to sell a property. Our experimental framework also has several advantages that could warrant its widespread adoption. For example, it provides causal inferences based on experimental variation. Also, the effects on behavior are measured using objective data from administrative records in a naturally occurring context and based on high-stakes choices. Our framework has practical advantages, too. Following our detailed instructions, the experiment can be implemented relatively quickly after a few weeks of preparation, and the data can be ready for analysis within just a few months. The experiment is relatively cheap, costing less than \$0.25 per subject. Last, it can be implemented at massive scales of up to 1 million subjects at a time.<sup>39</sup>

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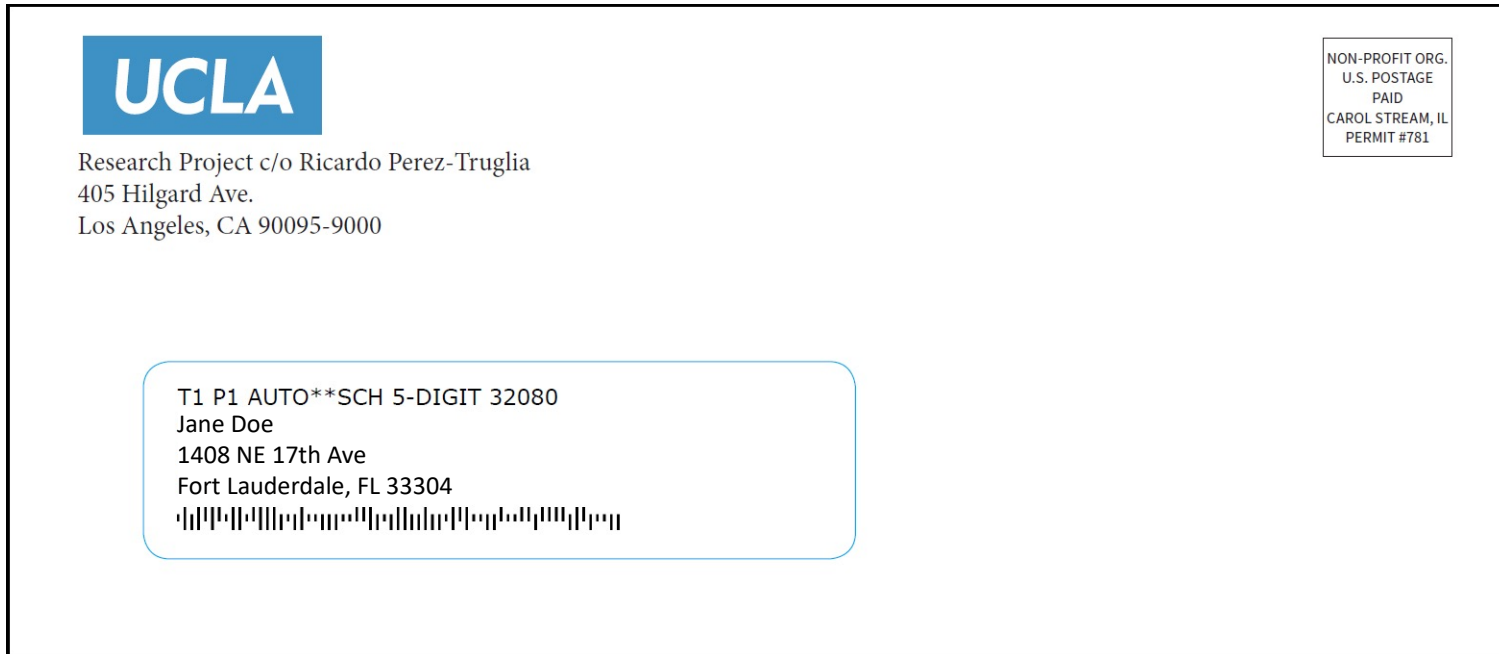
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<sup>39</sup> For example, Zillow Research (<https://www.zillow.com/research/data/>) shows 1,705,251 unique active listings in May 2019 (and county assessor records likely include data for most of these listings). Also, according to data from the National Association of Realtors, 5.34 million homes were sold in 2019.

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Figure 1: Sample Envelope



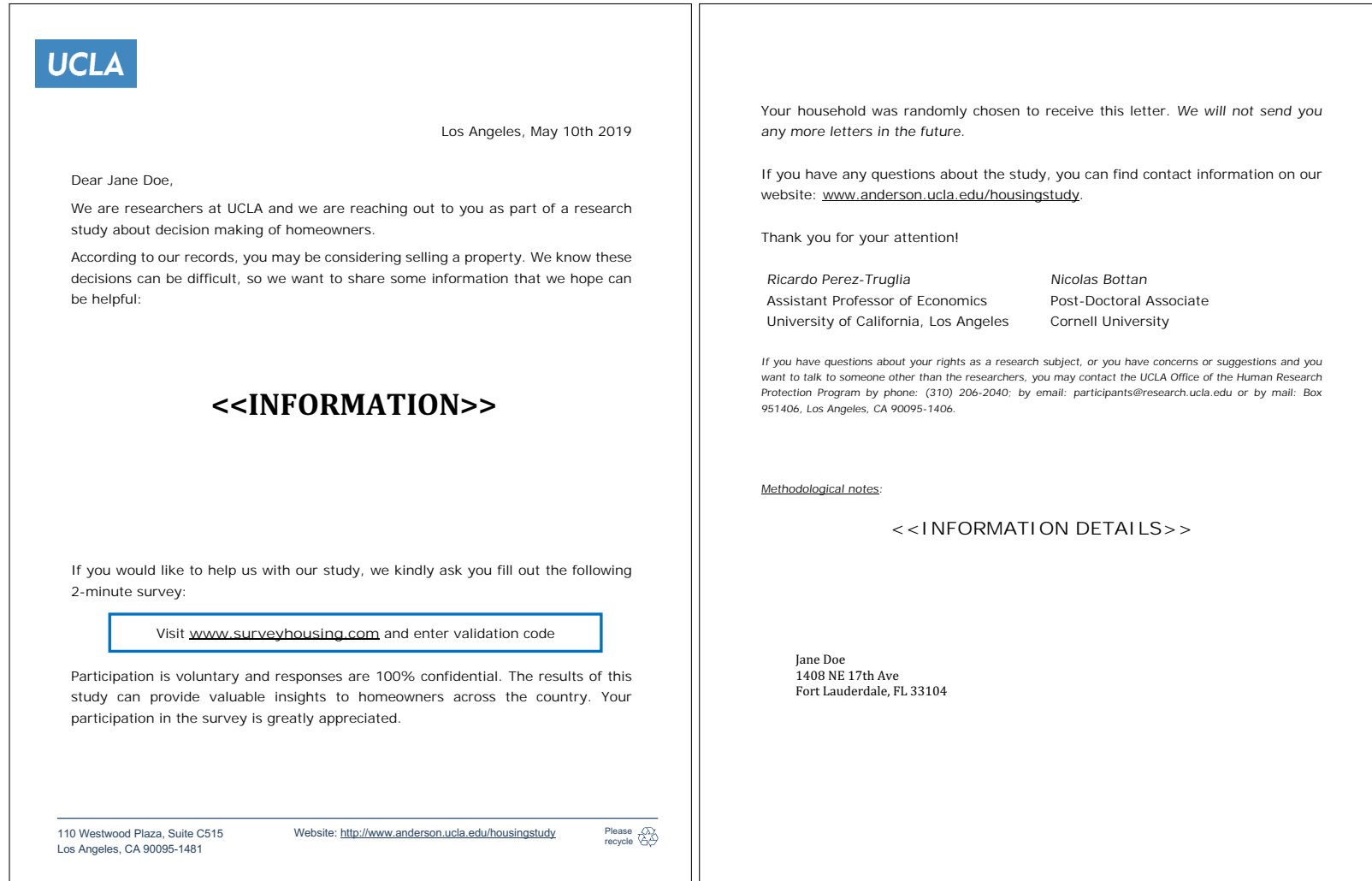
Notes: Screenshot of the outside of the envelope used in the field experiment.



Figure 2: Sample Letter

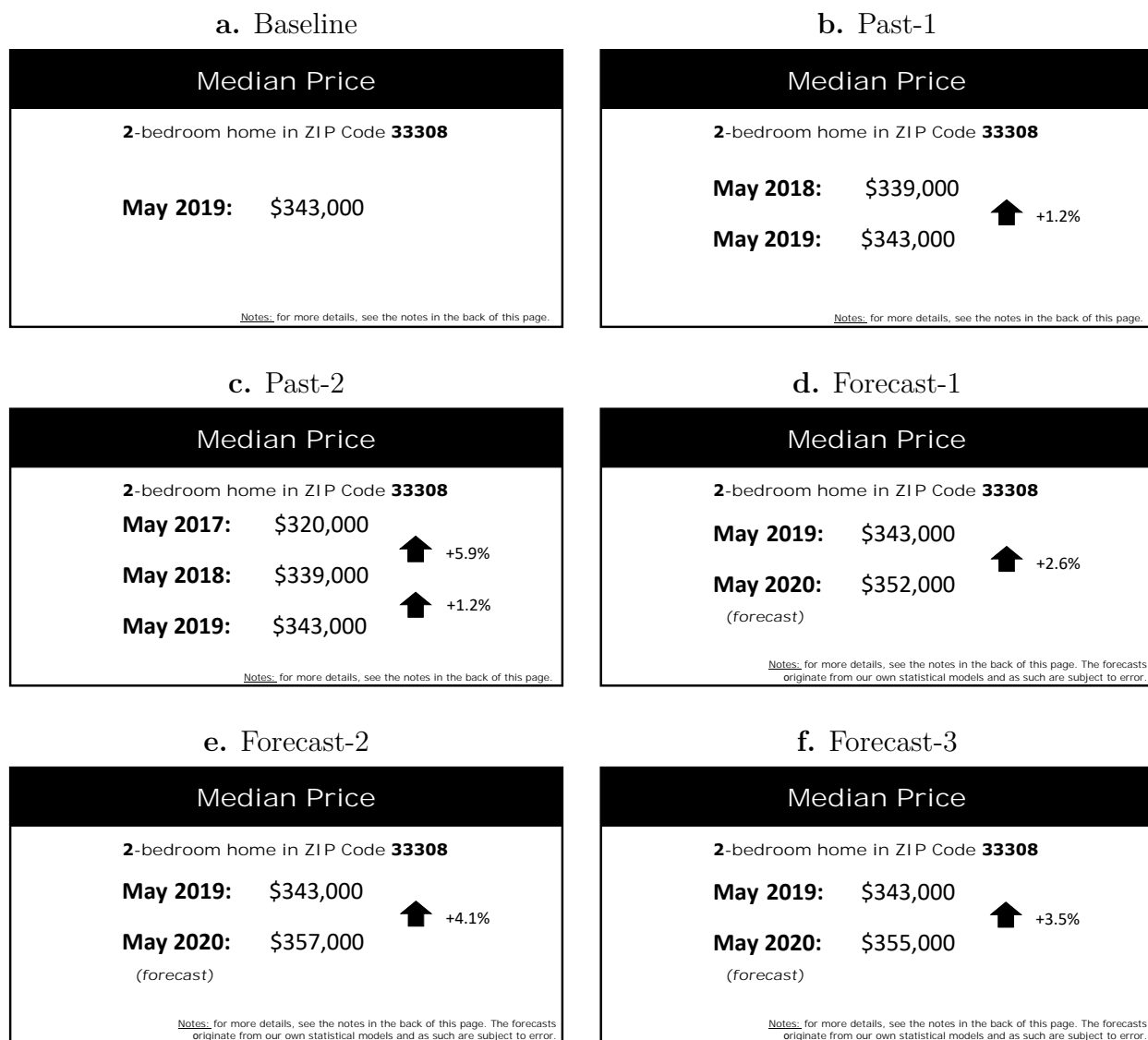
a. First Page

b. Second Page



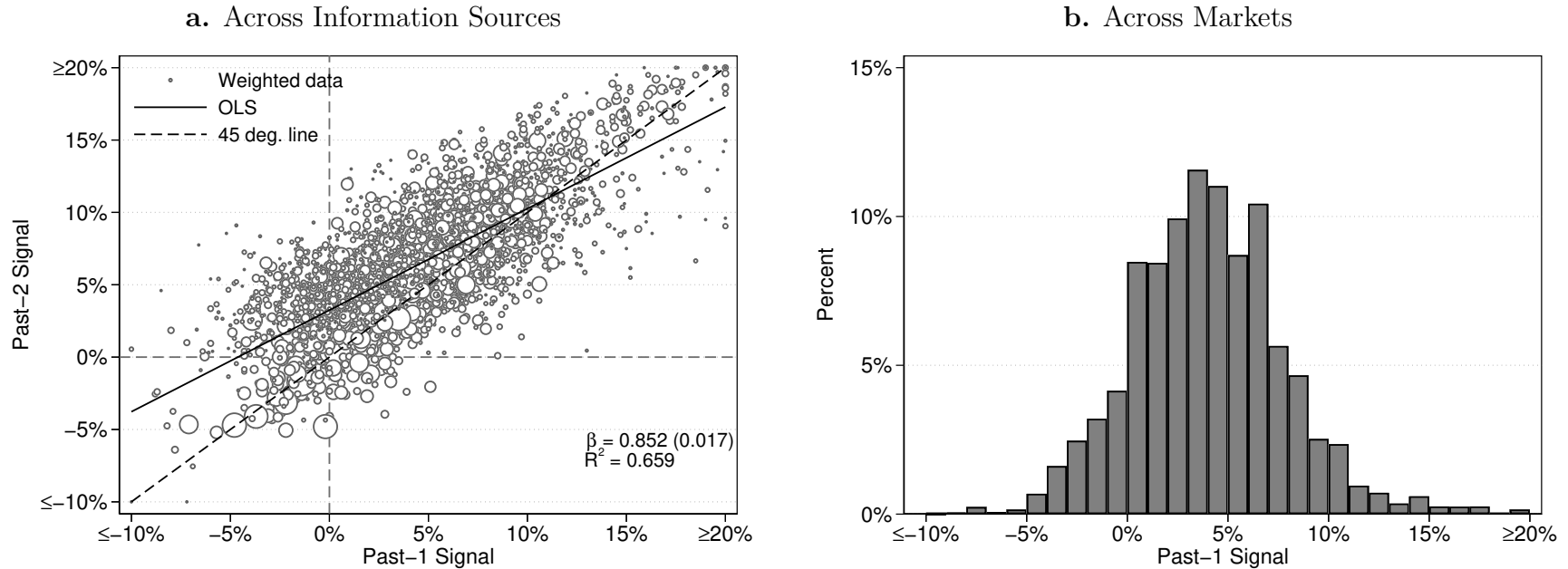
Notes: Screenshot of the letter used in the field experiment. The two placeholders (marked as «*Information*» and «*Information Details*») indicate the placement of the two components that were randomly allocated. Their samples, by treatment group, are presented in Figures 3 and A.1 respectively. Appendix B shows a sample of the final product.

Figure 3: Sample Information Tables



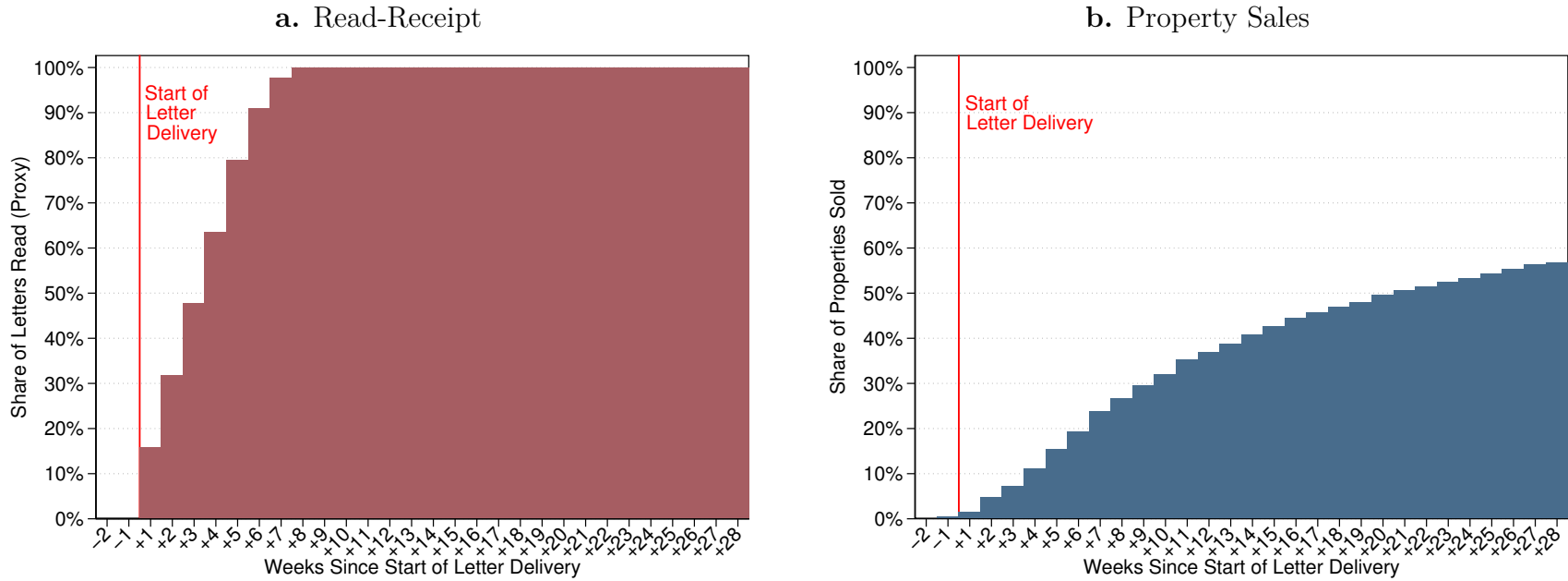
Notes: Each panel corresponds to the hypothetical table that a given individual would have received under the different treatment groups. The table is then placed in the middle of the first page of the letter, in the location of the placeholder «*Information*» from Figure 2. See Figure A.1 for the methodological notes accompanying each table.

Figure 4: Variation in Signals Across Information Sources and Across Markets



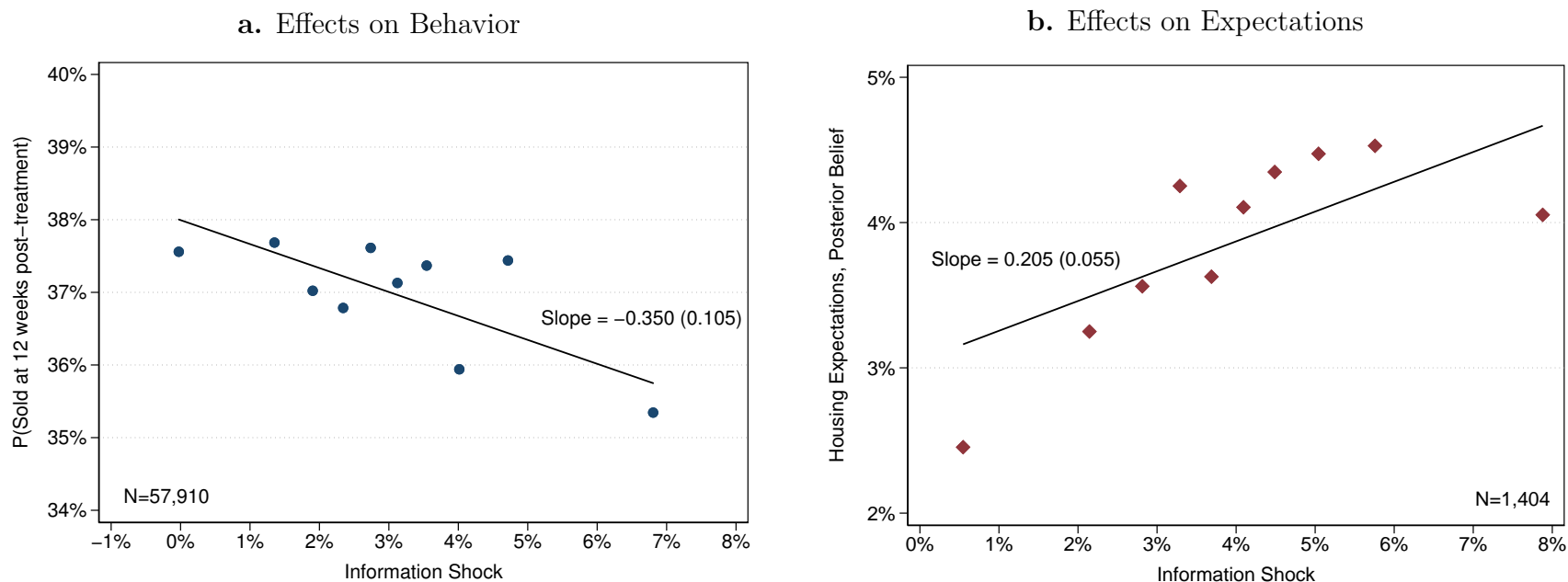
Notes: Panel (a) is a scatterplot showing the relationship between the signals that the 57,910 subjects would have received if they had been assigned to the Past-1 treatment versus the Past-2 treatment. The size of the circles are proportional to the number of observations, and the signals are truncated at -10% and +20%. Panel (a) shows the distribution of the signals that the 57,910 subjects would have received if they had been assigned to the Past-1 treatment (i.e., the annual growth rate over the past year). The bins have a width of 1 pp and are truncated at -10% and +20%.

Figure 5: Timing of Read-Receipt and Property Sales



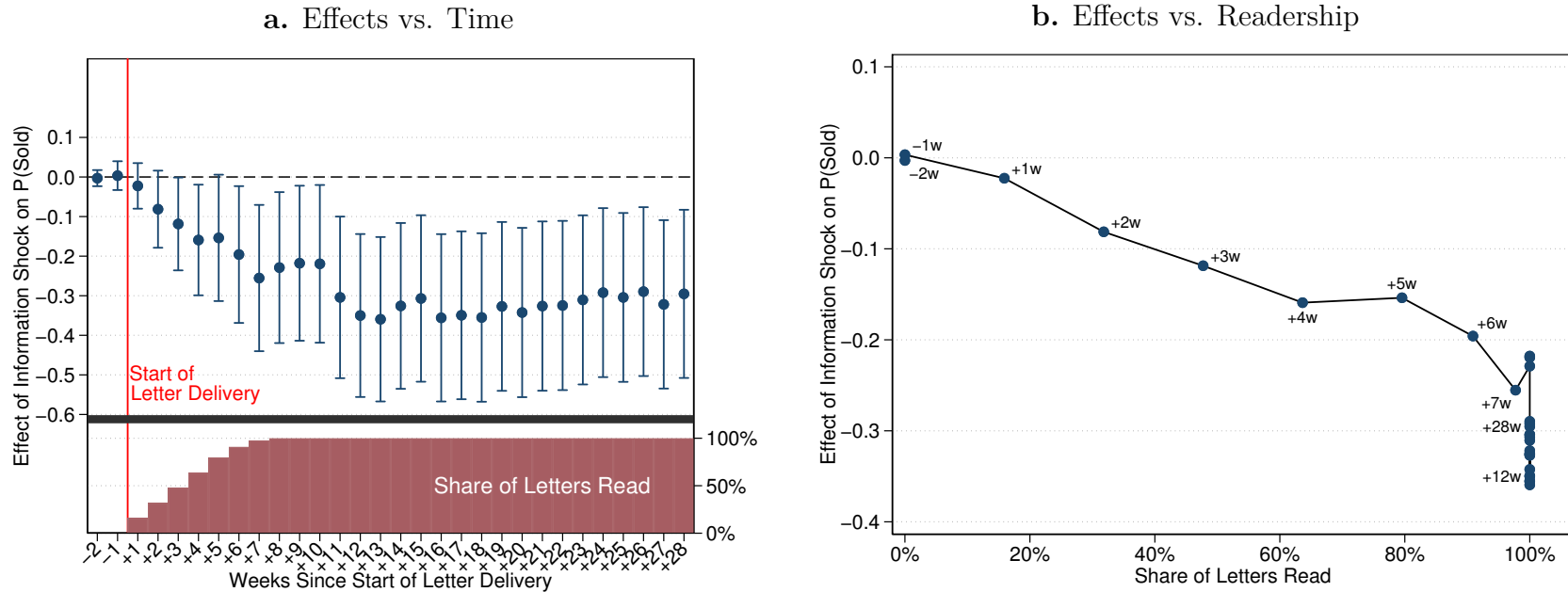
Notes: The vertical red line indicates the estimated date when the first letter was delivered (June 15 2019). Panel (a) shows the evolution of the responses to the online survey included in the letter. These dates constitute our read-receipt: i.e., our proxy for the dates when the letters were actually read. Panel (b) shows the fraction of the properties in the subject pool that were sold at each point in time, according to the administrative records.

Figure 6: Effects of Information Shocks on Expectations and Behavior: Binned Scatterplots



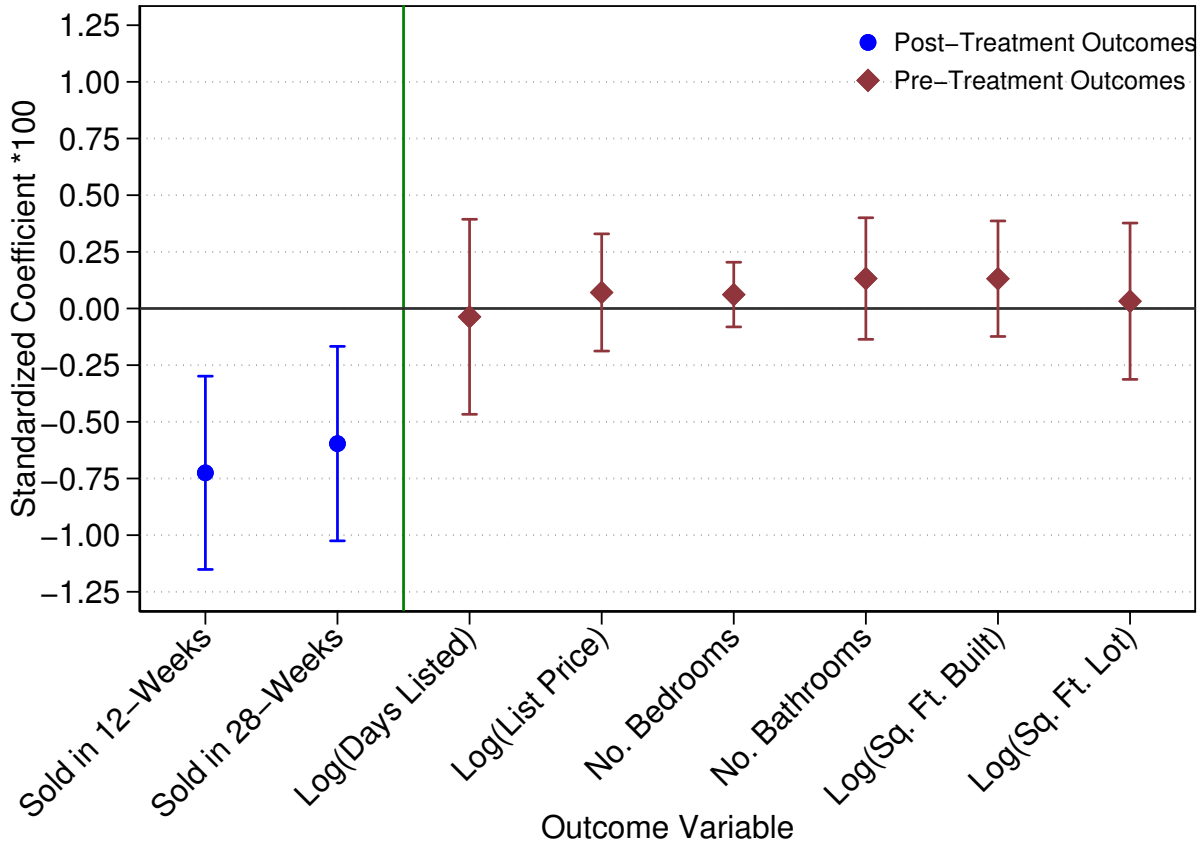
Notes: Panel (a) corresponds to a binned scatterplot representation of the results presented in column (3) of Table 2. This binned scatterplot focuses on the key independent variable, *Information Shock* ( $E_m^{j_i^*} \cdot D_i$ ). Results are based on 57,910 subjects from the field experiment. The dependent variable is an indicator variable taking the value 100 if the property was sold at 12 weeks after the start of the letter delivery and 0 otherwise. Panel (b) is the binned scatterplot representation of the results presented in column (1) of Table 3. This binned scatterplot focuses on the key independent variable, *Information Shock* ( $E_m^{j_i^*} \cdot D_i$ ). Results are based on 1,404 subjects from the AMT supplemental survey. The dependent variable is the posterior belief (i.e., elicited after the information-provision experiment) about the expected growth rate of the median home value over the following year. Each panel reports the slope with heteroskedasticity-robust standard errors in parentheses.

Figure 7: Effects of Information Shocks on Behavior: Event-Study Analysis



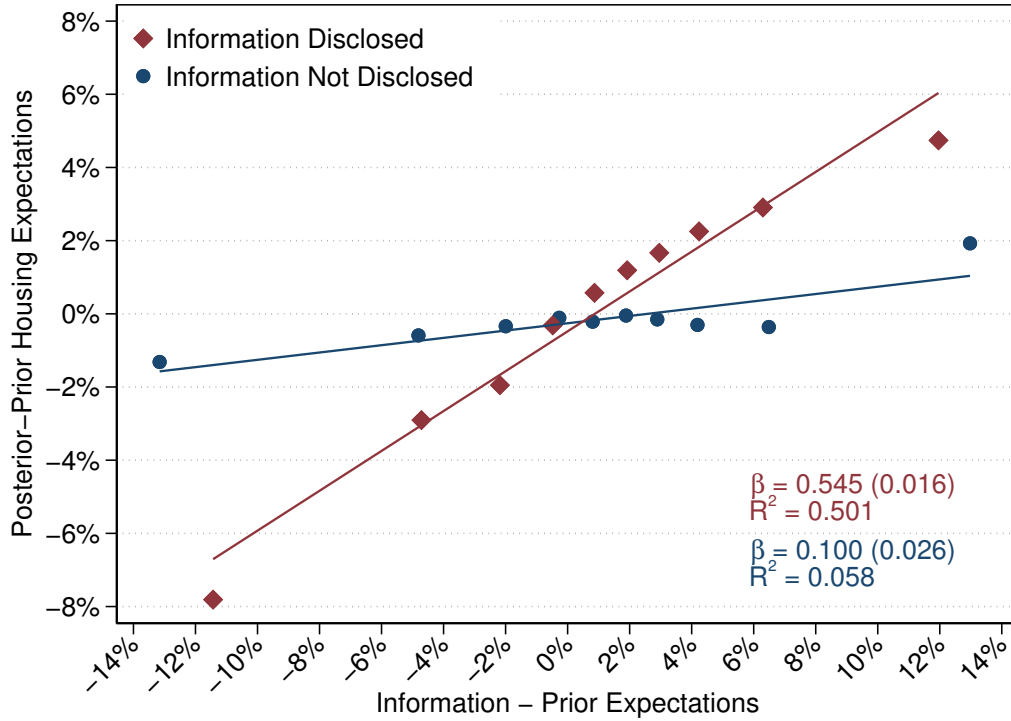
Notes: Panel (a): each coefficient in the top half of the panel corresponds to a separate regression based on 57,910 subjects from the field experiment. Every regression corresponds to equation (4) from Section 2, and the coefficient being graphed corresponds to the coefficient on the key independent variable,  $Information\ Shock (E_m^{j_i^*} \cdot D_i)$ . All regressions are identical except for the dependent variable. The x-axis indicates the dependent variable used, which is always an indicator variable that takes the value 100 if the property has been sold at a number of weeks after the start of the letter delivery and 0 otherwise. For example, the coefficient on  $+12\ weeks$  is based on a dependent variable that takes the value 100 if the property was sold at 12 weeks after the start of the letter delivery. The vertical red line indicates the estimated date when the first letter was delivered (June 15 2019). The bottom half of the panel shows the proportion of the letters from the field experiment that had been read at every point in time according to our proxy (the responses to the online survey included in the letter). The 90% confidence intervals are based on heteroskedasticity-robust standard errors. Panel (b): this figure combines the two series shown in panel (a), by plotting the treatment effects against the proportion of the letters from the field experiment that had been read at every point in time. The labels at each marker mark the number of weeks after the start of the letter delivery that the effect of the information shock and the share of letters read were recorded at.

Figure 8: Effects of Information Shocks on Behavior: Placebo Outcomes



Notes: All the regressions shown in this table correspond to equation (4) from Section 2, based on data on the 57,910 subjects in the field experiment. Each coefficient corresponds to a separate but identical regressions, with the only difference being the dependent variables. We report the coefficient on the key independent variable, *Information Shock* ( $E_m^{j_i^*} \cdot D_i$ ). All coefficients have been standardized by dividing by the standard deviations of the corresponding dependent variable, and then multiplying by 100 for readability. Each dependent variable is listed in the x-axis. We use blue circles to denote the post-treatment outcomes (i.e., that were determined after the start of letter delivery): *Sold in 12-Weeks* is an indicator variable that takes the value 100 if the property was sold at 12 weeks after the start of the letter delivery; and *Sold in 28-Weeks* is an indicator variable that takes the value 100 if the property was sold at 28 weeks after the start of the letter delivery. We use red circles to denote the pre-treatment outcomes (i.e., that were determined before the start of letter delivery): *Log(Days Listed)* is the logarithm of the number of days that the property had been listed for before our experiment; *Log(Listing Price)* is the logarithm of the original listing price of the property; *No. Bedrooms* is the property's number of bedrooms; *No. Bathrooms* is the property's number of bathrooms; *Log(Sq. Ft. Built)* is the logarithm of the property's built area in square feet; *Log(Sq. Ft. Lot)* is logarithm of the property's lot size in square feet. The 90% confidence intervals are based on heteroskedasticity-robust standard errors.

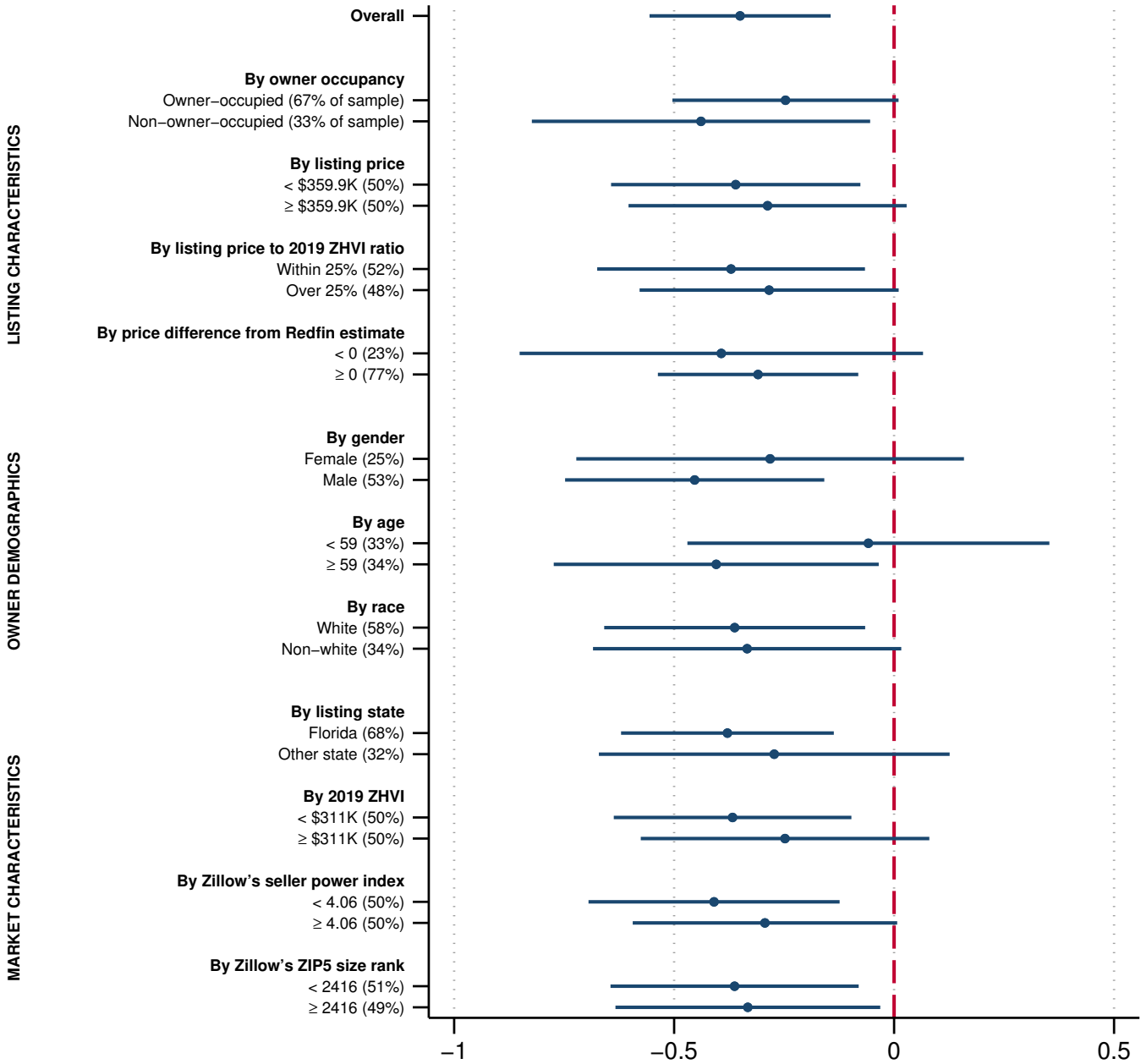
Figure 9: AMT Supplemental Survey Results: Belief Updating



Notes: The binned scatterplot provides a finer description of the belief updating for the 1,404 subjects from the AMT supplemental survey. The dependent variable is the belief update about the expected growth rate of the median home value over the following year (i.e., the gap between the posterior belief that was elicited after the information-provision experiment, and the prior belief that was elicited before the information-provision experiment). The independent variable is the gap between the signal (the information about home prices provided to the participant) and the prior belief about the expected home price growth rate. The blue circles represent the subjects that received no additional information about home price growth. The red diamonds represent the subjects that received additional information about home price growth.  $\beta$  is the slope of each line, estimated with a simple linear regression with heteroskedasticity-robust standard errors reported in parentheses.



Figure 10: Heterogeneity Analysis



Notes: Each coefficient corresponds to a separate regression based on 57,910 subjects corresponding to equation (4) from Section 2. The coefficients correspond to the one on the key independent variable, *Information Shock* ( $E_m^{J_i^*} \cdot D_i$ ). The dependent variable used is an indicator variable that takes the value 100 if the property has been sold at 12 weeks after the start of the letter delivery. All regressions are identical except for the sample restrictions, which are presented in the y-axis labels. For example, the coefficient for *Owner-occupied* represents the coefficient on *Information shock* when the regression is run for occupant owners only. The percentage totals in parentheses represent the proportion of the 57,910 subjects that each sample represents. The percentages do not sum to 100% for the demographic breakdowns due to missing values. The 90% confidence intervals are based on heteroskedasticity-robust standard errors.

Table 1: Randomization Balance Test

|                        | By Treatment Group |                    |                     |                    |                    |                     |                    | (8)<br>P-value |
|------------------------|--------------------|--------------------|---------------------|--------------------|--------------------|---------------------|--------------------|----------------|
|                        | (1)<br>All         | (2)<br>Baseline    | (3)<br>Past-1       | (4)<br>Past-2      | (5)<br>Forecast-1  | (6)<br>Forecast-2   | (7)<br>Forecast-3  |                |
| Days Listed            | 86.654<br>(0.477)  | 85.997<br>(1.017)  | 87.212<br>(1.298)   | 87.029<br>(1.253)  | 85.936<br>(1.093)  | 86.208<br>(1.203)   | 87.778<br>(1.191)  | 0.829          |
| List Price (\$1,000s)  | 574.756<br>(3.914) | 575.774<br>(8.517) | 586.787<br>(11.445) | 559.192<br>(9.516) | 574.306<br>(8.147) | 586.835<br>(10.937) | 565.092<br>(9.081) | 0.303          |
| No. Bedrooms           | 3.256<br>(0.005)   | 3.249<br>(0.010)   | 3.259<br>(0.012)    | 3.243<br>(0.012)   | 3.254<br>(0.011)   | 3.269<br>(0.011)    | 3.264<br>(0.011)   | 0.582          |
| No. Bathrooms          | 2.608<br>(0.004)   | 2.607<br>(0.010)   | 2.619<br>(0.011)    | 2.599<br>(0.011)   | 2.608<br>(0.010)   | 2.617<br>(0.011)    | 2.600<br>(0.010)   | 0.678          |
| Sq. Ft. Built (1,000s) | 2.295<br>(0.005)   | 2.292<br>(0.012)   | 2.304<br>(0.014)    | 2.292<br>(0.013)   | 2.287<br>(0.012)   | 2.308<br>(0.013)    | 2.288<br>(0.013)   | 0.820          |
| Sq. Ft. Lot (1,000s)   | 12.958<br>(0.089)  | 12.992<br>(0.199)  | 12.666<br>(0.222)   | 12.934<br>(0.230)  | 13.223<br>(0.219)  | 13.163<br>(0.221)   | 12.730<br>(0.213)  | 0.389          |
| Observations           | 57,910             | 11,487             | 8,672               | 8,669              | 9,818              | 9,635               | 9,629              |                |

Notes: Average characteristics of the 57,910 properties in the field experiment, with standard errors reported in parentheses. Column (1) corresponds to the entire sample. Columns (2) through (7) correspond to each of the six treatment groups. Column (8) reports the p-value of the test of equal means across all six treatment groups. All the variables correspond to pre-treatment characteristics (i.e., that were determined before the start of letter delivery). *Days Listed* is the number of days that the property had been listed for before our experiment. *List Price* is the original listing price of the property. *No. Bedrooms* is the property's number of bedrooms. *No. Bathrooms* is the property's number of bathrooms. *Sq. Ft. Built* is the property's built area in square feet. *Sq. Ft. Lot* is the property's lot size in square feet.

Table 2: Main Regression Results

|                           | Dep. Var.: $S_{+12w}$ |                     |                      |                      |                      |                      |                      |                     |                      |                      |                      |
|---------------------------|-----------------------|---------------------|----------------------|----------------------|----------------------|----------------------|----------------------|---------------------|----------------------|----------------------|----------------------|
|                           | (1)                   | (2)                 | (3)                  | (4)                  | (5)                  | (6)                  | (7)                  | (8)                 | (9)                  | (10)                 | (11)                 |
| Information Shock         | -0.318***<br>(0.122)  | -0.419**<br>(0.170) | -0.350***<br>(0.105) | -0.341***<br>(0.103) | -0.349***<br>(0.113) | -0.330***<br>(0.103) | -0.353***<br>(0.121) | -0.307**<br>(0.139) | -0.364***<br>(0.113) | -0.411***<br>(0.114) | -0.331***<br>(0.109) |
| Experimental Variation    | Source                | Disclosure          | Pooled               |                      |                      |                      |                      |                     |                      |                      |                      |
| Additional Controls       |                       |                     |                      | Y                    |                      |                      |                      |                     |                      |                      |                      |
| Extended Dummies          |                       |                     |                      |                      | Y                    |                      |                      |                     |                      |                      |                      |
| Alternative Specification |                       |                     |                      |                      |                      | Y                    |                      |                     |                      |                      |                      |
| Group Left Out            |                       |                     |                      |                      |                      |                      | Past-1               | Past-2              | Forecast-1           | Forecast-2           | Forecast-3           |
| Mean Outcome              | 36.93                 | 36.99               | 36.99                | 36.99                | 36.99                | 36.99                | 37.10                | 37.10               | 36.95                | 36.91                | 36.92                |
| SD Outcome                | 48.26                 | 48.28               | 48.28                | 48.28                | 48.28                | 48.28                | 48.31                | 48.31               | 48.27                | 48.26                | 48.26                |
| $R^2$                     | 0.167                 | 0.008               | 0.155                | 0.193                | 0.155                | 0.034                | 0.164                | 0.164               | 0.165                | 0.165                | 0.165                |
| Observations              | 46,423                | 57,910              | 57,910               | 57,910               | 57,910               | 57,910               | 49,238               | 49,241              | 48,092               | 48,275               | 48,281               |

Notes: Significant at \*10%, \*\*5%, \*\*\*1%. Heteroskedasticity-robust standard errors in parentheses. Each column corresponds to a different regression. All regressions are based on data from the field experiment and using the same dependent variable: an indicator variable ( $S_{+12w}$ ) that takes the value 100 if the property was sold at 12 weeks after the start of the letter delivery and 0 otherwise. Column (1) corresponds to the source-randomization specification given by equation (1) from Section 2, which is restricted to the sample with  $D_i = 1$  and with *Information Shock* corresponding to the key independent variable:  $E_m^{j_i^*}$ . Column (2) corresponds to the disclosure-randomization specification given by equation (3), with *Information Shock* referring to the key independent variable:  $E_m^{j_i^*} \cdot D_i$ . Column (3)–(11) corresponds to the pooled specification given by equation (4), with *Information Shock* referring to the key independent variable:  $E_m^{j_i^*} \cdot D_i$ . Column (4) is identical to column (3) except that it includes some additional control variables: the logarithm of the days the property was on the market prior to the experiment, the logarithm of the initial listing price, four dummies for number of bedrooms, four dummies for number of bedrooms, the logarithm of square footage built, the logarithm of lot size, and six state dummies. Column (5) is identical to column (3) except that instead of controlling for one treatment indicator, it controls for a set of five treatment indicators (i.e., one for each of the five treatments that are not Baseline). Column (6) is identical to column (3) except that, rather than controlling non-parametrically for market fixed effects, it controls for the potential signals in a parametric way. Column (7)–(11) are identical to column (3) except that they exclude subjects for one treatment group at a time.

Table 3: AMT Supplemental Survey Results

|                   | (1)                 | (2)                | (3)               | (4)              |
|-------------------|---------------------|--------------------|-------------------|------------------|
|                   | $H_{1y}^{post}$     | $H_{5y}^{post}$    | $H_{1y}^{prior}$  | $M_{1y}^{post}$  |
| Information Shock | 0.205***<br>(0.064) | 0.167**<br>(0.070) | -0.014<br>(0.066) | 0.017<br>(0.134) |
| Mean Outcome      | 3.86                | 2.31               | 3.88              | 3.58             |
| SD Outcome        | 4.42                | 4.36               | 5.39              | 9.05             |
| $R^2$             | 0.140               | 0.044              | 0.028             | 0.043            |
| Observations      | 1,404               | 1,404              | 1,404             | 1,404            |

Notes: Significant at \*10%, \*\*5%, \*\*\*1%. Heteroskedasticity-robust standard errors in parentheses. Each column corresponds to a different regression based on data from the AMT supplemental survey using a different dependent variable:  $H_{1y}^{post}$  is the posterior belief (i.e., elicited after the information-provision experiment) about the expected growth rate of the median home value over the following year.  $H_{5y}^{post}$  is the posterior belief about the annualized expected growth rate of the median home value over the following five years.  $H_{1y}^{prior}$  the prior belief (i.e., elicited before the information-provision experiment) about the expected growth rate of the median home value over the following year. And  $M_{1y}^{post}$  is the posterior belief about the annualized expected growth rate of the stock market index over the following year.