# **Building Beliefs:**
# Are Danes Housing Supply Skeptics And Can Such Beliefs Be Altered? Evidence from an RCT

Thesis by

**Emil Bories Hüttel**

*In Fulfillment of the Requirements for the degree*
*of*
*MSc in Political Science*

AARHUS UNIVERSITY
Business and Social Sciences

*Department of Political Science*

Supervisor: Niels Nyholt
Character count incl. appendix: 208.685

# Contents

# List of Figures

## List of Tables

**Abstract**

This thesis investigates the cognitive foundations of public opposition to market-rate housing development in Denmark, focusing on supply skepticism; the belief that increasing housing supply does not reduce prices. Drawing on a Randomized Controlled Trial (RCT), the study examines whether Danish citizens are supply skeptics and whether they update their beliefs in a Bayesian manner when presented with credible information on the price effects of new supply. By modeling price expectations as probability distributions rather than point estimates, I develop a novel measure of supply skepticism, which reveals that while supply skepticism is common, it is mild, uncertain, and weakly held. Respondents exposed to informational treatments revised their beliefs toward the evidence, with stronger signals inducing greater belief change. Learning patterns were broadly consistent with Bayesian updating, moderated by prior uncertainty and perceived credibility. Exploratory analysis further indicated that education, ideology, and tenure status also shaped belief revision. Furthermore, reduced-form estimates from a Bayesian IV model suggest that belief updating translated only weakly into downstream changes in housing policy preferences. However, there is stronger evidence of a small compound effect of information on preferences, possibly reflecting learning through channels beyond price expectations. The study contributes to the emerging literature on housing politics by introducing a novel measure of supply skepticism that captures uncertainty in respondents' beliefs; by formally modeling belief updating within a Bayesian framework; and by offering policy-relevant insights into how information may, and may not, shift public support for housing supply.

Last edit: 01-09-2025

# 1 Introduction

Across much of the world, real house prices and rents are surging, with metropolitan areas bearing the brunt of this trend (Brouwer & Trounstine, 2024). Combined with broader inflationary pressures, these escalating housing costs have become a central driver of what is now widely referred to as the cost of living crisis (OECD, 2023). A growing consensus among economists points to a fundamental market imbalance at the heart of this crisis: a persistent and widening shortage of housing supply (Glaeser et al., 2005; Glaeser & Gyourko, 2018). As demand outpaces supply – particularly in urban centers – prices soar, placing increasing strain on households and limiting access to the economic opportunities concentrated in these metropolitan hubs (Bertaud, 2018).

What then causes this supply-shortage? The predominant explanation in economics and political science is, that the housing shortage stems from local opposition to development and up-zoning. Existing research highlights two key explanations; both of which lend themselves to the notion of NIMBYism (Not In My Backyard). The first explanation is rooted in homeowners' economic self-interest in driving house prices up (Fischel, 2001; Einstein et al., 2019; Yoder, 2020; Marble & Nall, 2021). The second is broader NIMBY opposition from homeowners and renters alike who seek to avoid *the noise, congestion, and aesthetic change that comes with new housing* which is felt more intensely in the immediate vicinity of a given project (Hankinson, 2018, p. 474).

While there is a great deal of evidence for NIMBYism and its various mechanisms as a cause of the housing shortage (Hankinson, 2018; Sahn, 2024; Brouwer & Trounstine, 2024; Larsen & Nyholt, 2024a), an emerging literature at the cutting edge of political science challenges the notion that NIMBYism and economic self-interest are the sole drivers of opposition to housing (Broockman et al., 2024). This literature focuses less on self-interest and more on the apparent complexity of the housing policy domain and the lack of elite cues within it. Such a policy domain appears to shift voters' attention toward political symbols associated with housing politics (Broockman et al., 2024) or heavily personified *folk economics* (Nall et al., 2024), with the latter seemingly driving supply skepticism: *the questioning of the premise that increasing the supply of market-rate housing will result in housing that is more affordable* (Been et al., 2019, p. 25). Consequently, while a majority of voters actually desire lower rents and prices (Nall et al., 2024), they oppose market-rate housing and instead favor ineffective policies such as price controls and subsidized affordable housing, directly contradicting expert consensus (Müller & Gsottbauer, 2022; Elmendorf et al., 2024b).

However, since this literature is still emerging, the more affective dynamics of housing politics remain underexplored, with existing studies – apart from a single poll in Australia (Garvin, 2023) – largely confined to the American context, despite the fact that the cost-of-living crisis is a broadly global phenomenon. This geographic limitation is particularly salient given that housing markets and policy frameworks vary significantly across countries, and public attitudes may not follow the same patterns outside the U.S. In this light, Denmark offers a compelling context for further inquiry: it is a wealthy, urbanized country experiencing acute housing pressures, yet with a different political and planning tradition. Combined with the seemingly self-defeating nature of supply skepticism and the strong planning and economic case for market-driven development (Bertaud, 2018), this makes it all the more urgent to investigate *to what extent supply skepticism is present in the Danish context, whether it can be alleviated, and, if so, whether it translates to changes in policy preferences.*

In addressing these questions, the thesis makes several important contributions. First, as alluded above, it extends the literature on supply skepticism to the Danish context, which is a valuable contribution in itself. Political beliefs are known to vary significantly across national contexts – particularly between American and Scandinavian electorates (Aarøe & Petersen, 2014) – and supply skepticism should not be assumed to be a universal phenomenon. Investigating it in Denmark thus helps assess the generalizability of prior findings. Moreover, as the following section will show, Denmark is, like many other countries, facing challenges related to housing supply constraints and rising prices. Understanding the public's resistance to housing development, and potential avenues for shifting these attitudes, is therefore highly policy-relevant. Second, the thesis develops a novel measure of supply skepticism by conceptualizing it as a distribution rather than a fixed point belief. This approach captures not only the direction of respondents' expectations but also the degree of uncertainty surrounding them. By doing so, it helps formally differentiate between genuinely held skeptical views and what may instead be weakly formed non-attitudes, which Nall et al. (2024) suggest they may be. Third, the thesis more formally models and identifies the relationship between belief updating and preference change. While Elmendorf et al. (2024a) have taken important initial steps by showing that supply skepticism can be reduced through information, they do not examine the mechanisms driving this change. This study addresses that gap by unpacking the cognitive process of belief revision and tracing its potential impact on policy preferences. Understanding these mechanisms is not only theoretically valuable but also crucial for designing more targeted and effective informational interventions.

The remainder of the thesis proceeds as follows. Section 2 introduces the Danish case by outlining key features of the housing market and the political institutions that shape housing policy. Section 3 presents the theoretical framework, situating the study within the broader literature on housing politics and belief formation. Section 4 details the experimental design and describes the estimation procedures used to analyze belief and preference updating. Section 5 presents the empirical results, while Section 6 discusses their their limitations and implications. Finally, Section 7 concludes.

# 2 Empirical prelude

This section provides a brief introduction to the empirical context of the study by outlining key aspects of the Danish housing market, land-use institutions, and the issue salience of housing. The purpose of this outline is twofold. First, the section on Danish housing market trends underscores the policy relevance of the study. While much of the political science literature on housing policy focuses on the United States, there is a clear need for similar studies in Denmark, given the mounting pressures on housing prices. Second, the high complexity of institutions and the low issue salience of housing are central to the core theoretical arguments presented in the theory section. The second subsection, therefore, empirically qualifies these arguments by showing that these characteristics – high complexity and low salience – accurately describe the Danish case.

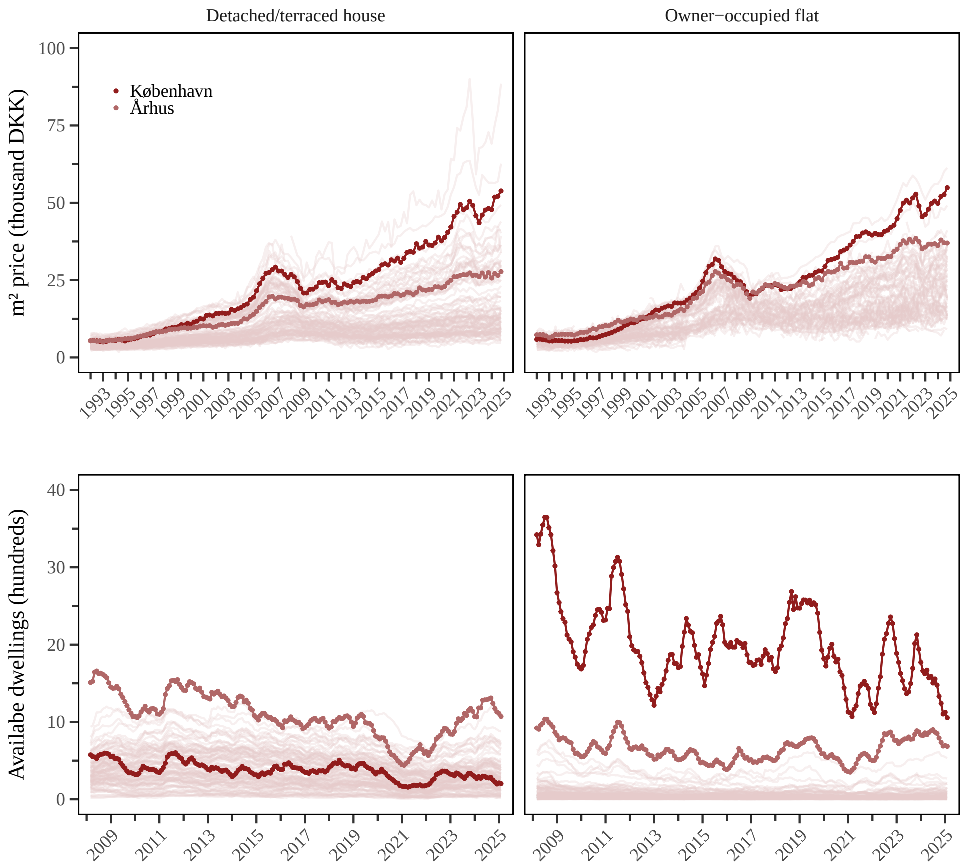
## 2.1 Brief summary of danish housing market trends

As outlined in the introduction, rents and house prices are surging across much of the world, with metropolitan areas being particularly affected. As also mentioned, much of this development appears to be driven by a lack of housing supply in these urban centers (Glaeser et al., 2005; Glaeser & Gyourko, 2018). Much of the literature on the subject originates from the United States, where zoning laws can be quite rigid, exacerbating the problem; for example, it is illegal to build apartment buildings in 76 percent of San Francisco (Telles et al., 2021). In Europe, however, zoning laws – while still present – tend to be more flexible (Hirt, 2012), generally allowing for more development than in the United States. Nonetheless, housing supply remains limited in Denmark (Heebøll & Haahr, 2018; Boliga, 2020; Københavns Kommune, 2024; Larsen, 2025), particularly in municipalities such as Copenhagen and Aarhus.

Figure 1 provides a broad overview of the trends in both supply and prices across all 98 municipalities in Denmark. As the two upper panels illustrate, nominal house prices[1] have steadily grown since 1993, with municipalities such as Copenhagen and Aarhus seeing substantially higher growth rates than others. Note that that house prices in the municipality of Aarhus conceal substantial variation, as the inclusion of more rural zip codes pulls down the overall average. The $m^2$ prices in the urban areas of Aarhus are more similar to those in Copenhagen than the graph suggests, which is also reflected in the apartment prices; where both Aarhus and Copenhagen show significantly higher growth rates but at similar price levels. The two bottom panels depict the supply trends across municipalities, clearly illustrating that, since 2008, supply has been stagnant in most municipalities, if not slightly decreasing. Of course, this is a very surface-level analysis, but it does suggest that booming house prices are at least partly due to stagnant

[1]Nominal house prices are used for this comparison because they capture differential inflation between municipalities (Mehtaj & Rumpf, 2024)

**Figure 1:** Price and supply trends in Denmark




**Note:** m$^2$ prices are nominal and based on realized transaction prices. Available dwellings (supply) is measured as dwellings for sale at the end of each month. Data-availability varies between price and supply measures, which is why the price time-series is longer. Pink transparent lines represent time-series for the 96 non-highlighted danish municipalities
**Source:** Finance Denmark - House Price Statistics

supply, as seen in much of the rest of the world.

At the national level, real Danish house prices exhibited trends similar to those of the United States from 2015 to 2018. However, while U.S. prices have surged significantly since then, Danish price growth has been more moderate – though still slightly outpacing that of the Euro area over the same period (OECD, 2025a).

Turning to rents, shown in Figure 2, a similar pattern becomes evident. Nationally, rent prices have been increasing at a steady rate of around 2 percent annually, but in municipalities like Aarhus and Copenhagen, growth rates have consistently been twice as high. Although supply-side data on rental unit availability is, to my knowledge, not publicly available, it seems reasonable to assume that similar constraints apply to the rental market. In cities where overall housing supply is tight – particularly in urban centers like Copenhagen and Aarhus – renting is often the main or only option for

**Figure 2:** Rent trends in Denmark


**Note:** $m^2$ prices are nominal and averaged across both private rental properties and social housing. Yearly growth rate is the percentage change in prices relative to the previous year. Notice that the left panel's y-axis is truncated for readability. Pink transparent lines represent time-series for the 96 non-highlighted danish municipalities
**Source:** The Danish Authority of Social Services and Housing

many residents, especially younger people, migrants, and lower-income households. It is therefore likely that the rental sector is facing significant supply pressure as well. Price signals in themselves (i.e. the rising rents in Figure 2) further indicate that demand is outpacing available supply, reflecting the same fundamental shortage seen across the broader housing market.

At the national level, rent trends in Denmark are very similar to the Euro area from 2015 and onward, but substantially lower than the those of the United States in the same period (OECD, 2025b).

Overall, the figures point to a pattern of persistently rising housing costs in Denmark's most dense areas, which appear to be at least partially driven by constrained supply. While the above analysis offers a descriptive overview rather than a causal analysis, the observed trends are consistent with the previously highlighted international patterns where limited housing availability contributes to price pressures, particularly in urban areas (Glaeser et al., 2005; Glaeser & Gyourko, 2018).

## 2.2 Danish housing polity and politics

Concerning land-use legislation, Denmark operates under a three-tiered system of government comprising the national government, five regional governments, and 98 municipal governments. Spatial and land-use planning responsibilities are distributed across these levels, following a hierarchical planning framework (OECD, 2017). National and regional authorities provide broad planning frameworks and strategic guidance, but the detailed regulation of land use and development is managed at the local level. Municipalities regulate land use through detailed local plans that specify what types of housing can be built where. These plans set parameters such as minimum lot sizes, floor area ratios, and zoning for residential or commercial development. Developers must apply for construction permits, which are granted or denied by the municipality based on these plans. Although the permitting process is typically administered by municipal staff, city councils can, under certain conditions, waive aspects of the local plan or delay compliant projects by requiring extensive and costly reviews. As a result, city councils hold considerable discretion over local land development (Larsen & Kettel, 2023). Needles to say, land-use legislation in Denmark, as in most other countries, is complex and very technical.

In Denmark, as in the United States (Broockman et al., 2024; Elmendorf et al., 2024a; Nall et al., 2024), housing appears to be a relatively low-salience issue. Direct measures of housing's issue salience in Denmark are limited, as public opinion research, to my knowledge, does not assess the perceived importance of housing. Yet this lack of attention may itself serve as negative evidence.

Prominent nationwide polls that track issue priorities tend to omit housing altogether (Altinget, 2025). Similarly, housing policy has received virtually no attention in the last four major Danish election studies (Stubager et al., 2013; Hansen & Stubager, 2017; Stubager & Hansen, 2021; Hansen & Stubager, 2024), and existing research on issue ownership in Denmark does not include housing as a category of interest (Seeberg, 2017). Some agenda-setting research does quantify the relative importance of housing—but focuses on parties rather than voters.[2] The general finding in this literature is that housing policy sits middle of the pack for the *party-system agenda* and on the lower end for government issue emphasis and opposition issue emphasis (Green-Pedersen & Mortensen, 2010, 2015). Finally, at the local level, tenure status doesn't seem to constitute a local social cleavage as one might expect (Hvidkjær & Larsen, 2025), which again reinforces the notion that housing politics is a low salience issue.

While the above paragraph might suggest that housing is entirely off the radar for Danish voters, this is not the case. Although housing as a broad, high-level policy issue generally exhibits low salience, certain localized projects can attract substantial public attention. A notable example is "Lynetteholmen", a large-scale land reclamation and urban development project, which has received enough coverage to warrant its own thematic section on Berlingske's[3] website. However, such cases of increased public attention tend to remain focused on specific developments rather than broader national housing policy.

[2] Naturally, this should reflect voter preferences to some extent, even if politicians often appear disconnected from those preferences (Walgrave et al., 2023).

[3] Major danish newspaper

As will become clear in the following sections, the (apparent) low salience of housing politics carries important theoretical implications, particularly when considered alongside the high complexity of land-use policy.

# 3 Theory

In the following section, I outline the theoretical framework of the thesis. I begin by briefly reviewing the literature on NIMBYism and identifying some blind spots in an otherwise well-supported theory. Next, I review the emerging literature on supply skepticism – a novel topic within housing politics – which I propose as a complementary explanation to NIMBYism, potentially addressing some of its blind spots. I then provide a brief overview of the more technical Bayesian learning framework, after which I extend the theoretical framework of supply skepticism by integrating insights from the dynamics of Bayesian learning. I conclude the section by deriving testable empirical implications based on this extended framework.

## 3.1 NIMBYism

As briefly touched upon in the introduction, scholarly attention within housing politics has largely focused on the various forms of self-interest that drive NIMBY sentiment (Brouwer & Trounstine, 2024). At the core of these self-interests lies citizens' desire to avoid the costs associated with local development, creating a collective action problem in which individuals may support housing development at the macro level but tend to *defect* and oppose projects in their own neighborhoods (Hankinson, 2018). Specifically, the self-interests associated with NIMBYism can be grouped into two overarching categories. The first is economic incentives for homeowners to drive up property values, as originally proposed by Fischel (2001), and the second is a desire to preserve the character – be it aesthetic, social, or otherwise – of a given neighborhood (Larsen & Nyholt, 2024a, p. 4). Broadly speaking, evidence supporting both explanations is substantial and spans a range of contexts (see Brouwer & Trounstine (2024) for a review).

Concerning the former, homeowners have been shown to be disproportionately active in local politics (Yoder, 2020; Hall & Yoder, 2022; Einstein et al., 2019; Sahn, 2024). This political engagement helps explain their tendency to oppose new housing developments when such projects are perceived to threaten the value of their own property (Dehring et al., 2008; Einstein et al., 2019; Marble & Nall, 2021; Fang et al., 2023). Concerning the latter, homeowners – and in some cases renters – have been shown to oppose new housing for a variety of reasons related to neighborhood characteristics, including aesthetic preferences (Larsen & Nyholt, 2024a,b), aversion to density (Wicki & Kaufmann, 2022; Wicki et al., 2022; Trounstine, 2023), and racial discrimination (Sahn, 2021).

Evidently, the scholarly attention toward NIMBYism is not without merit, and it clearly plays a major role in the housing shortages observed around the world. However, recent advances in the field of housing politics rightly point out that NIMBYism cannot be the sole explanation for these shortages, for several reasons.

As Larsen & Nyholt (2024a) point out, homeowners are generally more NIMBY than renters – but, at odds with the economic incentives typically associated with NIMBYism, renters are NIMBY as well. Hankinson (2018) suggests that some of this might be explained by the price anxiety of renters in cities where rents are particularly high, but other work suggests that the general gap between homeowners and renters might be smaller than expected when controlling for confounding factors, as done by Wicki et al. (2022) and Wicki & Kaufmann (2022). Concerning economic incentives, Elmendorf et al. (2024a) find that homeowners are actually more supportive of new development – in direct opposition to the home-voter hypothesis formulated by Fischel (2005). Of course, this does not mean that economic incentives in NIMBYism do not matter, but it does suggest that they are not the sole explanation. In a similar vein, Broockman et al. (2024) note that citizens tend to oppose even relatively distant projects, as also demonstrated by Larsen & Nyholt (2024a) and Sahn (2024) – although to a lesser degree than citizens living in close proximity to such projects. This empirical pattern poses a more serious challenge to the NIMBY explanation, as it casts doubt on the core premise of NIMBYism – that proximity to a project causes defection – rather than simply calling into question a specific mechanism, as was the case with the homeowner-renter juxtaposition discussed above. All of this taken together of course does not write off NIMBYism, as pointed out above the theory has earned it's stripes empirically. It does, however, suggest that NIMBYism cannot be the only explanation of popular opposition to housing.

## 3.2 Supply skepticism and it's drivers

This blind spot in the existing literature on housing politics has recently sparked interest in developing complementary explanations to NIMBYism, with a focus on general opposition to new development rather than solely local opposition.

One such complementary explanation can be found in the recently identified empirical phenomenon of *supply skepticism*, which was introduced as a term in a literature review on the effects of market-rate housing by Been et al. (2019). The primary motivation of the review was to respond to a growing number of affordable housing advocates and community members who were questioning the premise that increasing the supply of market-rate housing leads to greater affordability. The supply skepticism described by Been et al. (2019) is primarily an ideology situated within – and driven by – political actors and academics, rather than a widespread popular belief.

However, recent empirical work by Nall et al. (2024) has revealed that supply skepticism is in fact a widely held belief among the American public, with approximately

30–40 pct. believing that a surge in their region's housing supply would increase prices, and another 30 pct. believing it would have no effect.[4] Furthermore, the same authors suggest that supply skepticism appears to be closer to a *non-attitude* than a coherent ideology, given that beliefs about the impact of housing supply on prices are often inconsistent – even within individual respondents. In another paper, the same authors find that this widespread supply skepticism translates into concrete policy preferences: a broad majority of voters favor policies such as price controls, demand subsidies, *inclusionary zoning*, increased government spending on affordable housing, and restrictions on Wall Street buyers – all of which run counter to the consensus among economic experts (Elmendorf et al., 2024b). An earlier German study also found broad support for rent control – likewise considered a harmful policy by experts – suggesting that these findings are not unique to the American context (Müller & Gsottbauer, 2022).

Since the empirical evidence does not suggest that supply skepticism constitutes a coherent ideology, Nall et al. (2024) propose an explanation positing that the complexity of housing policy makes it difficult for voters to reason about, leading them to draw conclusions based more on affective cues available to them.

Generally speaking, voters find it difficult to reason about economic issues, at least insofar as their beliefs systematically differ from those of economic experts (Caplan, 2001, 2002). In the case of housing markets, economic reasoning may be particularly challenging due to the unique features of these markets. As Nall et al. (2024) point out, housing – a durable stock good – changes very slowly, unlike commodities such as gas, milk, or eggs, which fluctuate rapidly. Evidently, the marginal changes in housing supply that occur in real life are difficult for voters to observe. Even if they are aware of growing supply, they also observe rising prices despite that increase. What they do not observe, however, is the counterfactual price growth that would have occurred had housing supply remained stagnant (Been et al., 2019). Furthermore, since new development is often located in expensive areas, the new housing supply that voters observe tends to be available only to the top of the income distribution. Based on these observations, it is unsurprising that voters might confuse correlation with causation and conclude that new development drives up prices – an example of a *availability heuristic* (Tversky & Kahneman, 1973; Nall et al., 2024). Additionally, the complexities of housing policy can make it difficult for voters to identify their own self-interests on which to base their beliefs (Broockman et al., 2024). Partisan cues, which might otherwise offer guidance, are also scarce in housing politics, leaving voters little direction when forming beliefs and thus pushing them toward reliance on heuristics (Broockman et al., 2024).

All of these complexities taken together might lead voters to form their economic beliefs on the basis of what Rubin (2003) has termed *folk economics* (Nall et al., 2024).

[4]Again, while still a very novel area of research, a similar pattern has been observed in Australia (Garvin, 2023).

In these *intuitive economics of untrained people*, market interactions are both personalized and moralized, and fairness is prioritized over efficiency (Rubin, 2003). Taking into consideration that real-estate transactions are often personal in nature compared to transactions within other markets, it would not be unlikely that this sort of reasoning is especially prominent when dealing with housing markets. For example, rising house prices are often attributed to profit-seeking and price-gouging instead of supply shortage (Nall et al., 2024), and more "elite" supply skeptics often pitch similar arguments, blaming investors, developers, or other "bad actors" for driving up prices (Been et al., 2019, 2024). A similar theoretical expectation is posited by Broockman et al. (2024), who argue, on the basis of symbolic politics theory, that voters will be especially susceptible to rely on their affect towards the symbols made salient by housing politics, such as "developers" or "Wall Street investors", among others.

Synthesizing the arguments of Broockman et al. (2024) and Nall et al. (2024) in a more colloquial manner, the main premise is that voters rely on their dislike towards developers or investors when forming opinions on the complex topic of housing policy, which evidently lowers support for market-rate housing because market-rate housing is seen to benefit these actors.

Recent empirical findings indicate that such dynamics indeed shape public opinion about housing policy. Broockman et al. (2024) find that affect towards developers and new housing's residents, among other symbols, explains anti-development preferences when policies make these symbols visible. Marble & Nall (2021) find that liberal homeowners are more likely to support local apartment development when reminded that lower prices from additional housing benefit lower-income groups. In a survey experiment of residents in Los Angeles, Monkkonen & Manville (2019) estimate that highlighting developer profits has a negative effect of 20 percentage points on support for housing development. Klement et al. (2023) find a very similar result in the Czech Republic when highlighting investor or developer profits; however, the magnitude of the effect is slightly smaller at 12–14 percentage points. Finally, Müller & Gsottbauer (2022) find that approximately 70 pct. of Germans support rent control and that voters who perceive this policy as "fair" are more supportive of it.

Since voters, across tenure status, seem to desire lower prices (Nall et al., 2024), the empirical patterns outlined above seem self-destructive, as this policy stance directly contradicts their apparent self-interests in lower prices. Evidently, this begs the question of whether learning about the positive effects of market-rate housing will shift public opinion. In the next section, I outline the Bayesian learning framework to give a formal idea of how voters might learn about the effects of market-rate housing.

### 3.3 Bayesian learning

An increasingly popular way to conceptualize citizens' reasoning about the political world borrows from the statistical foundations of Bayesian learning. The growing popularity of the Bayesian framework is in part due to its properties as an *explicit* benchmark for information processing (Bullock, 2009), which in much seminal research concerning evaluation of citizen competence is only vaguely defined – if it is defined at all (Kuklinski & Quirk, 2001). In the present case, Bayesian learning serves mostly as a theoretical framework from which to derive empirical expectations about citizens' reactions to new information, but also as an analytical framework guiding modeling choices (Section 4.3.3 and 4.3.4).

Bayes' Theorem is a useful tool in conceptualizing political belief formation because many political phenomena – i.e., implications of a new housing policy – can be thought of as probability distributions. Like most probability distributions, these distributions have means and variances, and it is these parameters we learn about when we learn about political facts (Bullock, 2009). More colloquially, the implications of new housing policy may oscillate around some fixed but latent effect on housing prices (the mean of the distribution), and we may be more or less uncertain about the true effect, which is represented in the distribution's variance. When conceptualized in this way, learning about housing policy implications becomes a matter of learning about probability distributions (Bullock, 2009), which can aptly be modeled using Bayes' Theorem:

$$p(H|E) = \frac{p(E|H)p(H)}{p(E)} \tag{1}$$

In its abstract form, presented above, Bayes' Theorem is an equation that relates conditional and marginal probabilities, where $H$ and $E$ are events in a sample space and $p(\cdot)$ is a probability distribution function. Stated in this manner, the equation offers little insight into learning about housing policy – but reformulating its constituent parts less obtusely should make it more apparent how exactly the equation serves our purpose.

Instead, imagine that $H$ is a citizen's belief about a given housing policy's effectiveness and $E$ is some evidence about the effectiveness of the same housing policy. Before a given citizen is exposed to the evidence, $E$, the citizen's belief about the housing policy's effectiveness is represented by the probability distribution $p(H)$ – in Bayesian terminology this is labeled *the prior*.[5] Following this logic, $p(H|E)$ now represents the same citizen's belief about the housing policy after seeing the evidence for the policy – this is commonly denoted as *the posterior*. $p(E|H)$, denoted *the likelihood*, represents how likely the observed evidence is, given the citizen's belief about the underlying process

[5] The prior can be imagined as being a continuous distribution representing different estimates of how much a supply shock lowers housing prices, where the density of this distribution maps how likely the citizen believes a given price change (interval) to be. A similar logic can be applied to the posterior

that produced it.[6]

To sum up, the implication of Bayes' Theorem as presented above is that citizens will update their beliefs about a housing policy's effectiveness by combining the information contained in the evidence and the information they already hold about the policy.[7] This holds true for many other applications as long as the idea can be described in probabilistic terms (Bullock, 2009).

To illustrate how this works in practice, it is worth applying the example to a common model of Bayesian updating which assumes that prior knowledge is normally distributed and that the same holds true for new evidence (Bartels, 2002; Gerber & Green, 1999; Bullock, 2009). Suppose again that a citizen is learning about the effectiveness of housing policy – specifically she is learning about the effect of a supply shock on housing prices in percentage points. The citizen understands the effect of the supply shock as being a continuum – there are infinitely many effect sizes, within some undefined bounds, and they can be both positive and negative. The true effect of the supply shock is a latent point on this continuum which we denote $\mu$. Before receiving any evidence about the effect size, the citizen's belief about $\mu$ – her prior – is normally distributed and can be expressed as: $\mu \sim \mathcal{N}(\hat{\mu}_0, \sigma_0^2)$. The mean of the distribution, $\hat{\mu}_0$, is the citizen's best estimate of the supply shock's effect size at time 0. The variance parameter, $\sigma_0^2$, denotes how confident she is in her estimate – lower variance indicating higher confidence in the true effect size, $\mu$, taking a value near $\hat{\mu}_0$. Higher variance, on the contrary, indicates that she is quite unsure about her estimate and that she believes the true effect size, $\mu$, may assume a wide range of values, spanning both near and far from $\hat{\mu}_0$.

At some point after time 0, the citizen might receive a signal about the true effect size of the supply shock, again measured in percentage points. The citizen understands the signal as having value $x_1$, corresponding to some effect size, positive or negative, on housing prices. She assumes that the signal is "noisy" and that it is drawn from a normal distribution, the likelihood, with a mean of $\mu$. This implies two underlying assumptions about the signal: Firstly, for $\mu$ to be the mean of the distribution, the signal must come from an unbiased source; and second, that for the signal to be drawn from a normal distribution, the associated "noise" must conform to the central limit theorem (Bullock, 2009, p. 1111). The signal coming from the previously described distribution can then be expressed as $x_1 \sim \mathcal{N}(\mu, \sigma_x^2)$. The variance parameter, $\sigma_x^2$, again communicates how convincing the citizen finds the signal she has received. For example, if the signal about the effect size of the supply shock comes from a scientific publication featuring a robust

[6] This can seem abstract but it is essentially the citizen's (subjective) internal model of how likely different pieces of evidence are to occur if her prior is correct.

[7] $p(E)$ doesn't hold much substantial meaning. It is best thought of as a normalizing constant ensuring that the posterior sums (integrates) to one (McElreath, 2020, p. 37).

identification strategy, the signal is clear and the variance will be low. On the contrary, if it comes from a bombastic drunk economist rambling at the local pub, the signal is noisier and the variance will be high.[8]

According to a common result in Bayesian statistics (Lee, 2012, p. 40-41), a citizen updating in accordance with Bayes' Theorem will form a new belief, a posterior, about the true effect of the supply shock, $\mu$, that can be expressed as $\mu|x_1 \sim \mathcal{N}(\hat{\mu}_1, \sigma_1^2)$ where:

$$\hat{\mu}_1 = \hat{\mu}_0 \left(\frac{\tau_0}{\tau_0 + \tau_x}\right) + x_1 \left(\frac{\tau_x}{\tau_0 + \tau_x}\right), \text{ and} \tag{2a}$$

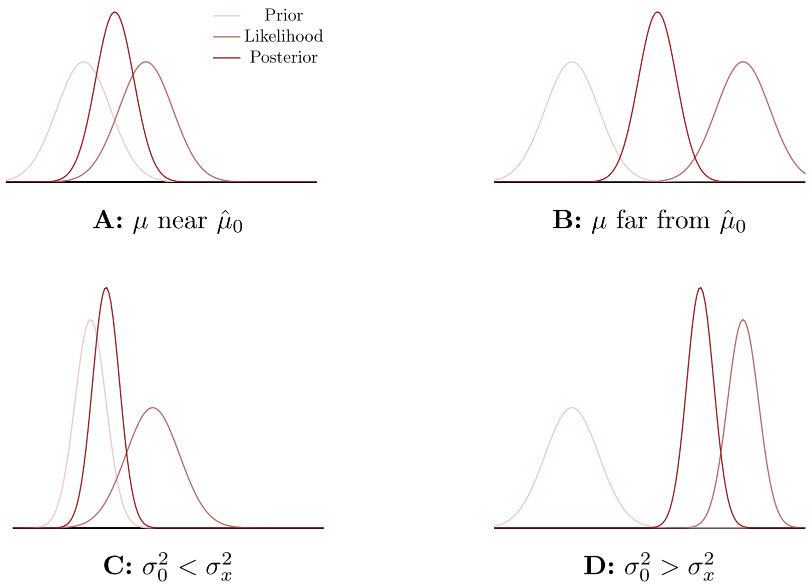
$$\sigma_1^2 = \frac{1}{\tau_0 + \tau_x} \tag{2b}$$

The expressions, $\tau_0 = 1/\sigma_0^2$ and $\tau_x = 1/\sigma_x^2$, represent the *precisions* of the prior belief and the signal, i.e. the inverse of their variance. The new belief about the effect of the supply shock, $\hat{\mu}_1$, is a weighted average of the prior belief and the signal, where the precisions serve as weights. This learning process is iterative and may be repeated multiple times. In the case that the citizen receives a new signal, $x_2$, the posterior belief, $\hat{\mu}_1$, becomes the prior, which will ultimately generate a new posterior, $\hat{\mu}_2$, following the same process as outlined in equations 2a and 2b.

To illustrate the process visually, Figure 3 depicts four different updating scenarios based on equations 2a and 2b. Panel A illustrates a scenario in which the prior and likelihood have equal variances but slightly different means. Because their variances are the same, they are weighted equally, and the posterior mean becomes a simple average of the two. The posterior's variance, however, is slightly narrower in accordance with equation 2b. Panel B essentially communicates exactly the same, but with the prior mean and the likelihood mean further apart, again resulting in the posterior mean being a simple average of the two. Panel C depicts prior and likelihood distributions with the same means as panel A, but now the prior variance is much narrower, while the likelihood variance remains unchanged. The resulting posterior mean is now much closer to the prior mean than to the likelihood mean. Staying with the example from earlier, the citizen is, in this scenario, very sure of her prior belief of the effect size of the supply shock but very unsure of the signal – resulting in only a marginal revision of her prior belief. Panel D is similar to panel C, but it features prior and likelihood distributions with the same means as panel B, and it is now the likelihood that has lower variance. Consequently, this results in the posterior being closer to the likelihood instead of the prior. In the terminology of our example, this signifies that the citizen is sure of the effect size she receives from the signal but more unsure about her prior knowledge concerning the effect size, which produces a posterior that is more dependent on the signal and less

[8] Of course these are edge cases, and one can imagine many signals in between.

**Figure 3:** Bayesian updating illustrated




**Note:** The figure illustrates four different updating scenarios following equation 2a and 2b. The x-axis represents the outcome space of a voters belief about the effect a supply shock on housing prices measured in percentage points. Distributions are based on the author's own calculations

so on the prior.

Presented in this way, the idea of Bayesian learning might seem abstract, and one could be inclined to proclaim that no human reasons using equations as described above – which is indeed a frequent critique of Bayesian learning models on political belief revision, and not without merit (Bullock, 2009, p. 1110). Even so, the main virtue of the Bayesian model is that it tells us exactly how a rational individual applying mathematical laws of probability should revise any belief after having received relevant evidence and subjectively estimated its likelihood. Thereby, Bayes' theorem serves as a useful benchmark, which, to reiterate, is often lacking in studies of citizen competence (Kuklinski & Quirk, 2001). The problem with not specifying benchmarks is that we risk confusing intelligent inferences for biased information processing (Gigerenzer, 2018). For instance, a citizen with a strong prior, updating only slightly upon receiving a weak signal, could easily be confused with motivated reasoning if one is not careful (Bullock, 2009; Hill, 2017; Little, 2021; Damgaard & James, 2024; Little, 2025).[9]

More importantly for our purposes, however, this property of Bayes' theorem makes it heuristically useful when analyzing political belief updating because it provides a sys-

[9] Not to say that motivated reasoning and Bayesian learning are one and the same, but they are *observationally equivalent* in some cases (Little, 2021).

tematic framework for understanding the balance between old beliefs and new evidence – even if people aren't perfect Bayesians in the real world (Bullock, 2009). Seen in this light, the Bayesian framework is particularly useful for deriving hypotheses about how evidence of the price-reducing effects of supply shocks might shift the beliefs of supply skeptics.

### 3.4 Learning about housing supply

While the previous section unfolded Bayesian learning in a general sense, the following section more clearly synthesizes supply skepticism and Bayesian learning and derives empirical expectations based on this synthesis; using both theory and observed empirical patterns.

First off, it is worth explicitly stating what was alluded in the previous section: empirically, citizens do not function as perfect Bayesian learners; they do, however, function as *imperfect* Bayesian learners.

To illustrate, a frequent empirical finding is that citizens are *cognitive conservatives* (Edwards, 1982; Damgaard & James, 2024), that they are *cautious Bayesians* (Hill, 2017), or that they have *sticky priors* (Stoetzer et al., 2024).[10] Substantially, this means that citizens do in fact update their beliefs, when faced with new information, but they do so at a lower rate than a strict application of Bayes' theorem would expect. What the terms above specifically allude to is, that oftentimes this occurs because citizens place excessive weight on their prior beliefs, given their uncertainty about those priors. In a unique experiment quantifying all the inputs of Bayes' theorem, Hill (2017) gauges this departure from Bayes' theorem in quantitative terms, and concludes that citizens: *update their beliefs at about 73 pct. of perfect application of Bayes' Rule* (p. 1403).

Some other patterns of deviation in empirical Bayesian updating, related to the specific characteristics of a given signal, have also been investigated. One particular concern of political scientists is that *counter-attitudinal* information might cause *backlash*, which is to say that citizens priors are reinforced when exposed to signals that run counter to their prior (Nyhan & Reifler, 2010). Empirically, this seems to be quite rare however; as shown by Wood & Porter (2019) and Guess & Coppock (2020) across a total of eight large survey experiments. A similar deviation hinges on the idea of *discounting* of evidence, where voters – due to confirmation bias – perceive signals far from their prior as less credible. Empirically, this departure from Bayes' theorem seems slightly more prevalent (Christensen, 2023), but again this is in line with the idea of citizens as imperfect Bayesians.

Based on these patterns it would not be unreasonable to expect citizens to behave, at least in part, as Bayesians, when confronted with evidence about housing policy. Fur-

[10] These terms are more or less interchangeable

thermore, the domain specific characteristics related to Danish housing politics (Section 2.2), should constitute an optimal environment for learning. As was discussed in the section on supply skepticism, as well as highlighted by Broockman et al. (2024) and Nall et al. (2024), housing politics is a domain with high complexity, low salience, low politicization, and few, if any, party-cues; and it is this low information environment – and symbols associated with market-rate housing – which generates the non-attitude of supply skepticism. More over, these characteristics should translate to most citizens, supply skeptics or not, having weakly formed beliefs about the effect of market-rate housing on prices – which in Bayesian terms translates to a prior with high uncertainty. Nall et al. (2024) find that citizens' beliefs about the price effects of housing supply shocks are considerably more volatile than their beliefs about supply shocks in other markets, suggesting a high degree of prior uncertainty in this domain.

Since supply skepticism seems prevalent in various contexts outside the US (Müller & Gsottbauer, 2022; Garvin, 2023; Klement et al., 2023), and in particular because danish housing politics share these same characteristics of high complexity, low salience, low politicization and low cue availability, I propose the two following hypotheses:

> **H1a:** *On average, danish citizens are skeptical towards market-rate housing's ability to lower prices*

> **H1b:** *On average, danish citizens have uncertain prior beliefs about market-rate housing's ability to lower prices*

A straight forward implication of the second hypothesis is, that since uncertainty in the prior belief is high, a potential signal, if perceived as credible, should have a relatively strong effect on citizens' beliefs about the effectiveness of market-rate housing. This is theoretically rooted in Bayes' theorem, where the described situation may be conceived of as similar to panel D in Figure 3 in the sense that the signal has a high degree of influence on the posterior.[11] Insofar as empirical price effects can be understood as diagnostic facts – meaning they can be verified and clearly and *fully convey central considerations relevant to a political issue* (Kuklinski et al., 2001, p. 412) – they should constitute relatively unambiguous signals.[12] Empirically there is already some preliminary evidence suggesting that the belief elasticity of supply skeptics is relatively high (Elmendorf et al., 2024a), which only corroborates the expectation. Furthermore, larger belief adjustments when priors are weak have also been identified in other areas of political science (Clifford et al., 2023; Broockman & Kalla, 2023), as well as in a multitude of other fields concerned

[11]This is a rather unique feature of housing policy. Often times (e.g. concerning tax or immigration policy) cues are plentiful and politicization is ripe leading to scenarios resembling panel C, where voters have strong priors and thus only update marginally

[12]Alluding to this, recent work by Fenger (2025) has shown that citizens' are generally willing to learn diagnostic facts, even it goes against party lines.

with learning (DellaVigna & Gentzkow, 2010, p. 654). This leads to the following hypothesis:

> **H2a:** *Exposing citizens to evidence of negative price effects of additional (market-rate) housing supply will cause citizens to update their beliefs in the direction of the evidence*

Furthermore, because housing politics are low salience and less politicized, confirmation bias should not be particularly strong within this policy domain, and consequently neither should information discounting (Christensen, 2023). It is therefore reasonable to expect that evidence of larger price effects will result in greater belief updating—an implication directly derived from Bayes' theorem, as illustrated by the contrast between Panel A and Panel B in Figure 3. This leads to the following hypothesis:

> **H2b:** *Larger negative price effects will induce larger learning effects than smaller negative price effects*

A naturally arising question on the basis of hypothesis 2a and 2b is: If evidence about housing policy updates citizens understanding of policy, does it also induce change in their preferences or behavior concerning housing policy?

Based on on similar reasoning as above, the prior uncertainty about housing policy would seem to logically imply that learning about policy would also induce changes in preferences or behavior related to said policy. However, as DellaVigna & Gentzkow (2010) point out this need not be the case, which is empirically backed by their cross-disciplinary review of persuasion experiments revealing substantial heterogeneity in the effect of persuasion on behavior across studies and within disciplines.[13] A more recent review of (economic) information provision experiments finds, that studies frequently identify null effects on behavior, even when belief updating is high (Haaland et al., 2023). Indeed, it is more so the exception to the rule, that learning changes preferences or behavior, which is consistent with the findings by Coppock & Green (2022) that suggest that citizens' opinions are *dynamically unconstrained*; meaning that a change in opinion related to a specific issue doesn't necessarily translate to a change in opinion concerning a logically connected issue. For instance, a change in perceptions about economic inequality doesn't lead to changes in economic system justification attitudes.

However, it is worth highlighting, that housing politics are not politicized in the same way economic systems – or other political topics – are, which could make these housing preferences more flexible. As Haaland et al. (2023) rightly point out, how much

[13] For instance, persuasion experiments focusing on voter turn-out report persuasion rates between 1 pct. and 20.4 pct.

belief updating affects preferences ultimately hinges on the underlying elasticity of said preferences, thus being mostly an empirical question which is hard to gauge a priori.

Nonetheless, the empirical review from section 3.2 does offer some guidance in forming a hypothesis. As highlighted by Nall et al. (2024) a majority of American voter's would prefer that future home prices and rents in their city be lower than today's prices, across tenure-status. Their policy preferences, however, do not match these socio-tropic preferences for lower prices. Instead, they favor policies such as price controls and subsidized affordable housing, which by expert consensus are considered ineffective or even destructive (Müller & Gsottbauer, 2022; Elmendorf et al., 2024b). Presumably, citizens hold these policy preferences because they perceive them as effective price reducing instruments. Following this reasoning, voters should update their preferences for market-rate housing if they learn that a policy advances their socio-tropic preferences for lower prices. Elmendorf et al. (2024a) find preliminary evidence in support of this expectation in an experiment using multiple different communication strategies to advocate for supply-side housing policies. For the most potent treatment they estimate an effect 2-3 times larger than typical informational interventions. Considering both the theoretical and empirical arguments above I posit that:

> **H3a:** *Learning about market-rate housing will make voters more supportive of market-rate housing development at the macro-level*

Note the specification: "at the macro-level", by which I intend to convey, that citizens shift their preferences towards development – but only as an abstract concept. They might be more accepting of more development in Denmark as a whole, again alluding the the socio-tropic nature of their price preferences, but this might not be the case when development becomes more local and thereby also more tangible.

As discussed in the section on NIMBYism, local opposition to housing more often than not arises from an ego-tropic desire to avoid bearing certain costs; economic, aesthetic, congestional, or otherwise. When preferences are linked to development as a diffuse concept these possible costs might be obfuscated allowing socio-tropic concerns to guide preferences. However, as soon as development project becomes local these costs become more apparent to citizens, and ego-tropic preferences become more salient. As (Elmendorf et al., 2024a, p. 12) point out, *no individual project will meaningfully affect citywide housing prices, economic agglomerations, socioeconoic mobility, greenhouse-gas emissions, or other matters of general concern*, which allows NIMBYs to be oppose a project based on their ego-tropic preferences without compromising their socio-tropic preferences. In this sense their is no trade-off between "selfish-concerns" and "big picture values", which allows for changes in preferences toward general development, without necessarily implying a change in preferences towards local development. This translates to the following hypothesis:

**H3b:** *Learning about market-rate housing will have weaker effects on preferences as proximity to a project increases*

To be entirely clear, the expectation that a near project makes ego-tropic considerations more salient, also lends itself to the notion of NIMBYism as an inherently spatial concept, and the expectation is in this sense a direct consequence of Tobler's first law of geography: *Everything is related to everything else, but near things are more related than distant things* (Tobler, 1970). NIMBY opposition will thus always be stronger when projects are near, and should like many other spatial phenomena be subject to *distance decay*,[14] which we also see empirically (Larsen & Nyholt, 2024a; Sahn, 2024). Consequently, the elasticity of development preferences should be a decreasing function of distance to a hypothetical project.

Briefly summing up the theory section, I have proposed a theoretical framework which mediates an important blind spot in the current NIMBY-focused housing literature by incorporating ideas from both supply skepticism and Bayesian learning. This framework generates several empirical implications. First, public opinion about the price effects of market-rate housing is likely to be both (supply) skeptical and uncertain, given the complexity and low politicization of housing politics. Second, given this uncertainty, well-structured informational interventions should have a measurable effect on belief updating. Finally, while learning may shift attitudes toward general housing development at the macro level, it is unlikely to erode opposition to local projects due to the continued salience of ego-tropic concerns.

# 4 Methodology

The following section describes the methodology of the thesis. I start by describing the practical implementation of the experimental design, the considerations behind it and the data it produces. In relation to the data I briefly discuss the sample composition and asses the covariate balance of the treatment arms. Hereafter I describe the thesis' estimation and identification strategy.

## 4.1 Experimental design

### 4.1.1 Survey procedure

The following subsection provides a broad overview of the experimental design and its constituent parts. Most of these elements will be discussed in greater detail in the sections that follow, but here I focus on the flow of the survey implementation.

[14] Distance decay is defined as: *Any function that implies a continuous, smooth and attenuating effect of distance on the attribute values of neighboring spatial entities* (Grekousis, 2020, p. 20)

The survey experiment employed in this thesis generally falls under the umbrella of what is known as *information provision experiments*. The core premise of experiments such as these is to *generate exogenous variation in perceptions of real-world environments* (Haaland et al., 2023, p. 4) and to study how these manipulations change beliefs about the world, and in turn, how such changes in beliefs might affect behavior or preferences. The present experiment explicitly seeks to manipulate citizens' perceptions of increased market-rate housing's ability to create affordable housing.

The general structure and flow of the survey is visualized in Figure 4. The survey begins with a brief introduction in which respondents are asked to provide consent to the terms of participation. Respondents are incentivized to complete the survey through a lottery offering a 500 DKK cash prize, accessible only upon completion. The introduction also briefly summarizes the scope of the study as an investigation into attitudes toward urban development and housing politics. To reduce the risk of demand effects, no further information is provided about the study's intentions (Berinsky et al., 2012).

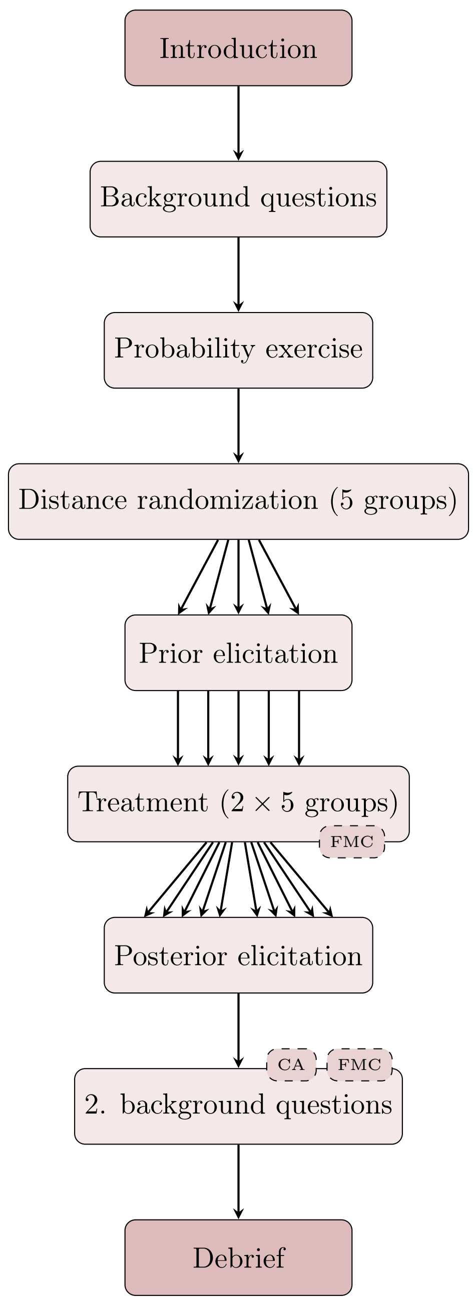
Following the introduction, respondents are presented with an initial block of demographic background questions related to gender, region, and age. GAs the primary analysis does not seek to estimate conditional effects based on demographics, these variables are primarily included to assess sample representativeness and to enhance statistical efficiency via covariate adjustment (Gerber & Green, 2012). To minimize respondent fatigue and reduce attrition prior to the experimental manipulation, this demographic section is intentionally kept brief. The selected variables were also chosen with care, as I do not expect them to introduce priming effects that might bias responses in the subsequent experimental block (Blackwell et al., 2025).

Before advancing to the prior elicitation block, respondents are asked to complete a short exercise intended to make them more comfortable thinking in terms of probabilities and outcome spaces; something most people generally struggle with (Tversky & Kahneman, 1973; Benjamin, 2019). The exercise mirrors the structure of the belief elicitation questions related to housing policy effectiveness – which will be explained in the section on outcome measures – but instead concerns weekly coffee consumption.[15] The exercise is designed to be intuitive and apolitical, in order to minimize both fatigue and priming. Furthermore, it is carried out on a scale that should be familiar to all respondents, and one that shouldn't induce anchoring in the actual belief elicitation. I section 4.1.5, I expand on how the exercise also serves as a comprehension check.

Upon completion of the exercise, respondents enter the prior elicitation block. One of the questions in this block is specifically tied to proximity to a housing project, which is why respondents are randomized into one of five groups that manipulate this distance. After eliciting prior beliefs, respondents are exposed to one of two informational

[15] Specifically, respondents are asked to estimate their most probable coffee consumption over the next seven days (in cups), the lower and upper bounds, and the probabilities of exceeding these bounds.

**Figure 4:** Survey flowchart




**Note:** CA = Credibility assessment, FMC = Factual Manipulation Check. If the dashed nodes are placed above a node it indicates the check is done entering the block. If placed below it indicates that the check is performed exiting the block

treatments, followed by a *Factual Manipulation Check* (FMC) concerning definitions presented before the treatment. Finally, their posterior beliefs are elicited. The specific measures, treatments, and checks will be discussed in greater depth in the following sections.

The survey ends with a *Credibility Assessment* (CA) of the informational treatment, another FMC, and another block of five background questions measuring education, tenure-status, ideology and urbanity of the respondents current address. Upon completion the respondents are debriefed.

### 4.1.2 Outcome measures

Both elicitation blocks follow the same structure and are designed to measure beliefs about the ability of market-rate housing to reduce rents, as well as preferences regarding both local and national development.

As recommended by Haaland et al. (2023), I elicit beliefs using both subjective[16] and quantitative measures. Beliefs about the effectiveness of market-rate housing are elicited subjectively using the following question:

> Imagine that a large number of new private homes are suddenly built in your local area. What effect do you think this will have on the rent for already existing homes in your area?

Respondents answer this question on a seven-point scale ranging from *Increase a lot* (= 1) to *Decrease a lot* (= 7). Measuring prior beliefs in this way has the advantage of being easy for respondents to understand, but it also has notable drawbacks. First, the measure is open to interpretation and thus not easily comparable across individuals, which can lead to serious identification issues (Bond & Lang, 2019). Second, the measure is relatively coarse and therefore carries limited informational value (Manski, 2018). For these reasons, this question primarily serves as a supplement to the quantitative measures discussed later in the section.

Turning to preferences, these are likewise elicited using a subjective scale. Preferences for national development are elicited through the following question:

> Are you generally for or against the construction of more private homes in Denmark?

Respondents were asked to answer using a seven-point scale ranging from *I am definitely for* (= 1) to *I am definitely against* (= 7). The final subjective measure elicited preferences concerning local development. Respondents were asked to consider the following question, where {distance} was randomized:

> Imagine that a private housing project is established within a **{distance}** radius of your home. Are you for or against the project?

[16] By subjective, I mean ordinal scales such as Likert scales, where the interpretation of the scale can vary between respondents. Haaland et al. (2023) refers to these as "qualitative"

This question was answered using the same seven-point scale as the previous question on preferences for national development. The specific distances used in the question are discussed further in the section on experimental manipulation.

Moving to quantitative belief elicitation, these questions feature both a quantitative point belief as well as probabilistic questions that measure uncertainty around this point belief. Contrary to the subjective question, the numerical point belief is easily interpersonally comparable[17] (Haaland et al., 2023), which is a major upside. Adding the probabilistic dimension has an additional advantage: since it enables computation of respondents' underlying belief distributions – similar to the distributions in the section on Bayesian learning – it renders the beliefs intrapersonally comparable[18] as well (Manski, 2018).

The specific questions, presented in Table 1, draw heavily on work by Leemann et al. (2021) and Stoetzer et al. (2024), which in turn build on the foundational work of Manski (2009). These questions are specifically designed to elicit a mode value, an upper and lower bound,[19] as well as the probabilities of exceeding those bounds. As will be described in the section on estimation, these responses enable the computation of a normal distribution representing respondents' prior and posterior beliefs about the effects of market-rate housing on prices, in probabilistic terms – analogous to the theoretical framework outlined in the section on Bayesian learning.

**Table 1:** Manski questions

| Question | Response |
|---|---|
| What do you think is the most likely percentage change in rental prices in a municipality if the number of private homes increases by 10 percent? | $y_{1_i}$ |
| What do you think is a likely range of the change in prices? Please indicate the lower bound. | $y_{2_i}$ |
| Please indicate the upper bound in percentage points. | $y_{3_i}$ |
| What do you believe the probability is that the price change is smaller than the lower limit ($y_{2_i}$) you specified? | $p_{1_i}$ |
| What do you believe the probability is that the price change exceeds the upper bound ($y_{3_i}$) you specified? | $p_{2_i}$ |

**Note:** Note that $y_{1_i}$ essentialy estimates $\hat{\mu}_0$ when measured pre-treatment and $\hat{\mu}_1$ when measured post-treatment. The other measures combined produce an estimate for $\sigma_0^2$ pre-treatment and $\sigma_1^2$ post-treatment

[17]Meaning that responses can be meaningfully compared across different individuals.

[18]Meaning that a single respondent's beliefs can be meaningfully compared across different points in time or different states.

[19]In other words, a likely range or interval

Respondents answer the questions using sliders, with some imposed restrictions. The sliders concerning price changes, $y_{k_i}$, move in 0,1 increments where $y \in [-50, 50]$. The range was designed to provide as much freedom as possible while keeping the slider easy to operate and limiting very extreme price changes. When respondents move the slider for $y_{1_i}$, the slider for $y_{2_i}$ automatically adjusts so that $y_{2_i} = y_{1_i} - 5$, and the slider for $y_{3_i}$ adjusts so that $y_{3_i} = y_{1_i} + 5$. This ensures that the logical constraint in the questions, $y_{2_i} < y_{1_i} < y_{3_i}$, holds by default, though respondents can freely adjust the secondary sliders after modifying $y_{1_i}$.

The sliders concerning probabilities $p_{k_i}$ likewise move in 0,1 increments where $p \in [0,1; 49,9]$. The answers are given in percentages rather than actual probabilities[20] for ease of interpretation. Again, the range is designed to provide freedom while ensuring that answers satisfy logical properties regarding probabilities. Specifically, they are designed to ensure that $0 < (p_{1_i} + p_{2_i}) < 1$, which must hold if the measures represent an underlying probability distribution (Moore & Siegel, 2013).

As mentioned in the previous section, these questions are answered twice throughout the survey, both pre- and post-treatment. Within-subject designs like this have the clear advantage of increasing statistical precision by measuring outcomes twice (Clifford et al., 2021). The downside, however, is that within-subject designs may potentially induce stronger *experimenter demand effects* compared to designs that measure outcomes only post-treatment. While demand effects seem like a reasonable concern intuitively, recent studies on demand effects in online experiments generally do not support this claim (Mummolo & Peterson, 2019; Roth & Wohlfart, 2020; Clifford et al., 2021). Furthermore, since housing politics are less politicized and respondents remain anonymous, the risk of demand effects is presumably marginal (Haaland et al., 2023).

As mentioned above, the elicitation method has the clear advantage of yielding a measure that is both inter- and intrapersonally comparable and can be modeled as a probability distribution. However, it also comes with some drawbacks. As previously noted, a large portion of the population finds it difficult to understand and interpret probabilities (Tversky & Kahneman, 1973; Benjamin, 2019), which may lead to noisy responses. Additionally, probabilistic questions are time-consuming and cognitively demanding for respondents, increasing the risk of attrition and missing data. That said, the use of carefully designed sliders and the short probability exercise should help make these questions somewhat easier to answer (Haaland et al., 2023). Nonetheless, respondents' answers on the subjective scale, as well as their likely intervals without the assigned probabilities, serve as valuable robustness checks, which will be discussed further in the estimation section.

[20] On the probability scale the range is $p \in [0,001; 0,499]$

### 4.1.3 Informational treatment

The main experimental manipulation takes the form of two informational vignettes that summarize recent evidence on the effects of increased market-rate housing on prices and accessibility (the full wording of the vignettes is presented in Appendix A.1). The core logic of the vignettes draws heavily on recent literature reviews in the field (Been et al., 2019; Phillips et al., 2021; Been et al., 2024), while specifically referencing findings from Mast (2023), Bratu et al. (2023), and Mense (2025).[21] The first two studies show that when new housing is constructed in high-income sub-markets, existing housing stock is freed up in lower-income sub-markets through a filtering mechanism often described as *moving chains*. The third study, by Mense (2025), focuses on price effects and finds that a 1 pct. increase in market-rate housing stock at the municipality level leads to a 0,19 pct. reduction in rental prices.

The result by Mense (2025) is an integral part of the vignette's design, because – when multiplied by ten – it conveys information on the exact same scale $y_{1_i}$ is measured on. Through the lens of a simplified Bayesian framework (Hjort et al., 2021), the result can be thought of as a noisy signal about the true effect of supply increases on rents, analogous to how the likelihood, $x_1 \sim \mathcal{N}(\mu, \sigma_x^2)$, was conceptualized in the theory section.

Building on this logic, the treatment vignettes attempt to manipulate $\mu$ by keeping all elements constant except the reported price change, which takes either the value of $-1,9$ pct. or $-7,6$ pct. While this design choice is useful for modeling purposes, it does come with a methodological downside, as it may induce numerical anchoring in responses (Haaland et al., 2023). To mitigate this risk, the vignettes are constructed to obfuscate the anchor by presenting the effect alongside other potential numerical anchors (i.e., the effect is benchmarked against general rent growth trends in Denmark), as recommended by Haaland et al. (2023).

For several reasons, the experiment is designed as an *active control* study, meaning that it does not include a *pure* control group. Instead, both groups receive an informational signal, but the magnitude of $\mu$ varies between them.[22]

The primary reason this setup is employed is that the active control design has several useful properties for identifying the effects of information on behavior. Firstly, active control ensures that the design isolates the effect of the signal from other possible effects such as priming. Providing both groups with information ensures that the effect of priming remains constant across groups (Haaland et al., 2023). In a similar vein, receiving informational treatment might have side-effects such as uncertainty reduction, varying attention, and emotional responses – especially if respondents feel corrected (Haaland et al., 2023). Emotional responses are similar to the ideas of backlash (Guess & Coppock, 2020) or information discounting (Christensen, 2022) discussed in the theory

[21] The papers are not cited directly, but their key results are paraphrased in the vignettes.

[22] Formally expressed, the groups, $\mathcal{G}$, are $\mathcal{G} \in \{\mu^{low}, \mu^{high}\}$ instead of $\mathcal{G} \in \{\text{control}, \text{treatment}\}$.

section and, as stated, these tendencies are empirically rare; but a design that addresses them is still preferable to one that does not.

Secondly, in pure control designs, variation relies on prior beliefs, with identification driven by those who held larger initial misperceptions. An active control group, by contrast, allows for variation even among individuals with accurate priors, enabling estimation of average causal effects across a broader population (Haaland et al., 2023).

Active control is also employed for more technical reasons. Firstly, as will be discussed in the estimation section, estimating changes in behavior as a function of changes in beliefs hinges on an instrumental variable (IV) approach as the identification strategy. For this approach to be valid, the *monotonicity* assumption must be satisfied.[23] (Vilfort & Zhang, 2024). Since passive control designs do not provide any information, they are less likely to satisfy this assumption; for instance, individuals might update more or less randomly in either direction because they think harder about the question, seek out information on their own, or answer inconsistently. As Vilfort & Zhang (2024) demonstrate both formally and empirically, this dynamic can lead to serious identification problems.

Secondly, a design using passive control would not allow for a test of Hypothesis 2b unless it featured three groups: a pure control group and two treatment groups receiving different signals. I opted for an active control design with fewer groups, both for the reasons stated above and because information provision experiments typically yield small effects (Haaland et al., 2023). Adding more groups would require a substantially larger sample size to estimate these effects with sufficient power.[24]

It must be noted, however, that employing an active control design also entails certain drawbacks. For the purposes of this thesis, the primary downside is that an active control design answers the question: *what is the effect of giving an agent one piece of information relative to another?*, whereas a passive control design answers the arguably more policy-relevant question: *what is the effect of giving an agent information, relative to not giving them information?* (Vilfort & Zhang, 2024, p. 15). This distinction is important, as the latter is often more directly applicable to real-world decisions about whether to provide information at all (Haaland et al., 2023; Vilfort & Zhang, 2024). The implications of this design choice – both methodological and substantive – are discussed throughout the thesis and revisited in the concluding discussion.

#### 4.1.4 Distance treatment

As Figure 4 illustrates, the design also features another treatment mechanism, which tries to create exogenous variation in the distance to a hypothetical development project.

[23] This assumption requires: *that the instrumental variable (weakly) operates in the same direction on all individual units* (Cunningham, 2021, p. 350)

[24] However, this might not be the case for information about housing politics, as Elmendorf et al. (2024a) estimate effects approximately twice the size of typical information provision effects.

This treatment condition is loosely inspired by (Larsen & Nyholt, 2024a), but is less sophisticated, both because it is less granular and less realistic. However, it is included to construct a test of Hypothesis 3b. The treatment conditions present projects at different distances, $d$, to the respondents' home, where $d \in \{80\text{m}, 500\text{m}, 1\text{km}, 3\text{km}, 5\text{km}\}$ with equal probability.

It should be noted that these distances are rather crude, and that defining a person's neighborhood (or backyard in NIMBY-terminology) is no easy task which constitutes an active research area in its own right (Wong et al., 2012; Dinesen & Sønderskov, 2015; Bisgaard et al., 2016; Munch & Albertsen, 2025). However, since the data are not geo-coded, the distances are strictly hypothetical, making it difficult to improve upon convenience-based selections. Therefore, the distances are chosen so as to represent both near and far projects while still keeping the number of groups as low as possible.

As was discussed in the section on outcome measures, the treatment is implemented by randomizing $d$ within a question concerning support for a project within $d$ radius. The question is then repeated post-treatment, where $d$ is kept constant within respondents.

#### 4.1.5 Manipulation checks and credibility assessment

As has been alluded to multiple times now, the experiment is presumably rather cognitively challenging for most respondents, for several reasons. First, as already discussed, probabilities are generally difficult for most people to understand and work with (Tversky & Kahneman, 1973; Benjamin, 2019; Haaland et al., 2023), and therefore the Manski-style questions require respondents to be especially attentive and concentrated. The same applies to the general subject matter of the survey; as established in the theory section, economic reasoning is typically challenging for most individuals (Caplan, 2001, 2002; Rubin, 2003), and this seems to be particularly true in the context of housing economics (Nall et al., 2024).

While the survey clearly instructs respondents to offer their best guess – and emphasizes that no "correct" answer is expected – even doing so is far from trivial. Although the treatment information is communicated in as colloquial a manner as possible, it still conveys a substantial amount of content on a complex topic.

This cognitive complexity likely contributes to the already existing measurement noise in responses. As is well-established in survey research, questions that tap into latent concepts often do so with considerable error (Zaller, 1992; Berinsky et al., 2014). Given the difficulty of the questions in the present experiment, such noise is likely to be relatively high – especially if respondents are inattentive (Berinsky et al., 2014).

For this particular reason – and because it is generally considered best practice in information provision experiments (Haaland et al., 2023) – it is crucial to obtain some measure of respondent attentiveness. To this end, I incorporate two survey items aimed at gauging attentiveness, both pre- and post-treatment. Multiple measures are included because attentiveness is itself also a latent variable that is subject to measurement error, and relying on a single item risks failing to adequately capture it (Berinsky et al., 2014, 2021).

However, to avoid distorting potential treatment effects, I refrain from implementing *instructional manipulation checks* (IMCs),[25] and instead implement two *factual manipulation checks* (FMCs). These are objective questions that ask respondents to recall key elements of the experimental design (Kane & Barabas, 2019; Kane, 2025).

The first FMC is unrelated to the treatment and asks respondents to correctly identify whether rent in public housing ("almene boliger") is: (1) *determined by market conditions*, (2) *determined based on costs* (correct), or (3) *negotiated between the tenants in the dwelling*. As illustrated in Figure 4, this FMC is administered immediately after the treatment and refers back to a definition provided earlier in the prior-elicitation block.

The second FMC is directly related to the treatment and asks whether the treatment stated that new market-rate housing benefits: (1) *low-income households only*, (2) *middle-income households only*, or (3) *both income groups* (correct). To avoid potential secondary treatment effects, this FMC is positioned after the posterior-elicitation block (Kane & Barabas, 2019).

It should be noted that attentiveness is measured with the intent of transparently gauging whether possible failures to retrieve intention-to-treat effects (ITT) might be due to lack of attentiveness, and by extension, treatment failure (Kane, 2025). However, since attentiveness is measured post-treatment, the attentiveness measures are not included in subsequent statistical models, nor are respondents dropped from the sample, because this risks inducing serious bias (Aronow et al., 2019).

Apart from scoping the respondents' attentiveness, assessing their comprehension of the task at hand is also crucial. Since the Manski questions are cognitively taxing and require certain logical properties to hold (Section 4.1.2), it is important that respondents understand the instructions and respond accordingly. If respondents fail to do so and answer randomly, the results may be misleading and could ultimately lead to erroneous conclusions (Banki et al., 2025).[26] The survey does not include a formal comprehension check, but since the Manski questions – and the probability exercise – must satisfy strict logical constraints, they function as implicit comprehension checks. Specifically, if a respondent fails to answer in a way that satisfies $y_{2_i} < y_{1_i} < y_{3_i}$, it suggests that they have not understood the structure of the Manski questions or the task at hand.

As with the attention checks, I refrain from excluding respondents purely based on

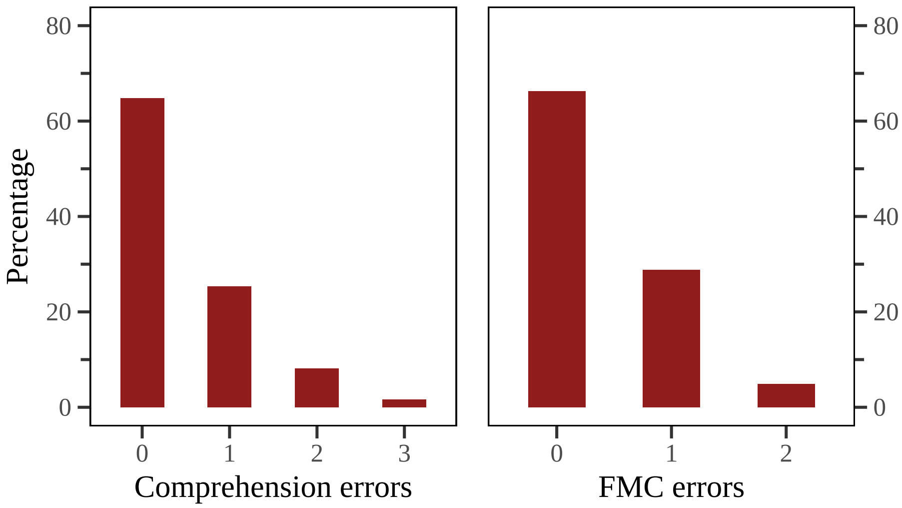
[25] IMCs embed a specific instruction – often contrary to the apparent task – within a survey item. See Berinsky et al. (2014) for examples.

[26] The cited paper illustrates this well by performing a reanalysis of a study claiming to disprove prospect theory, in which 75 pct. of the sample failed the comprehension checks.

comprehension issues in order to avoid introducing bias (Aronow et al., 2019). However, because two of the three Manski question sets are measured pre-treatment, they can be used to exclude respondents in a way that avoids post-treatment bias. Specifically, if a respondent fails both of the pre-treatment Manski question sets, they are excluded from the full analysis. If they fail either of the Manski question sets used to measure outcomes (prior or posterior), they are excluded from analyses involving that specific belief distribution, as it cannot be computed for subjects who fail the Manski questions.

As shown in Figure 5, over 60 percent of the sample made no errors on either the FMC or the comprehension checks. Considering the difficulty of the survey and and the amount of checks, this failure rate is considered relatively low. As such neither lack of comprehension or attention should be a concern in the analysis.

**Figure 5:** Comprehension and FMC errors




**Note:** Percentage of respondents making n number of errors across the full sample (n = 489)

Apart from attentiveness and comprehension, the credibility of the treatment plays a major role in information provision experiments (Haaland et al., 2023). A specific concern in the present design is that some respondents might perceive the treatment vignette as biased. As highlighted by (Elmendorf et al., 2024a), there is some evidence that individuals may be reluctant to trust economic experts when their message runs counter to partisan dogma. In this context, a plausible scenario is that particularly left-leaning respondents might perceive the informational treatment as "neo-liberal" and, as a result, may not view it as credible. To address this concern, I follow the recommendation by (Haaland et al., 2023) and measure treatment credibility directly by asking respondents how credible they found the information. Within the Bayesian framework, this measure can be interpreted as a proxy for $\sigma_x^2$, albeit measured on a non-numerical, subjective scale.

## 4.2 Sample

The following section briefly summarizes the sample the survey produced, beginning with a description of the fielding method, followed by an assessment of its size and statistical power. The section then turns to representativeness and concludes with an evaluation of covariate balance across treatment arms.

The sample was fielded from 04-04-2025 to 02-05-2025 using various social media channels, my own personal network, and a Danish news outlet specializing in urban development.[27] Although I employed non-probability sampling, I made efforts to distribute the survey across various regions of Denmark. Still, the sample is best characterized as a convenience sample. The median response time was 10 minutes (mean = 12 minutes), and the completion rate was 34,41 pct., yielding a total of 489 completed responses out of 1,421 total attempts (effectively $n = 978$, given repeated measures). After dropping respondents who failed both pre-treatment comprehension checks, the final sample includes 472 complete respondents (effectively $n = 944$). Needless to say, the attrition rate is relatively high, which is unsurprising given the survey's length and complexity. However, it is reassuring that most attrition occurs early in the survey, prior to treatment assignment, and is not systematically different across treatment groups, thereby preserving the internal validity of the causal estimates (see Figure A.9).

Turning to statistical power, it is worth noting that null findings and small effects are common in information provision experiments, and a sample size of 472 falls short of the generic power recommendations by Haaland et al. (2023). However, the same authors emphasize that effect sizes in such experiments vary considerably across contexts, and that power considerations should ultimately be made on a case-by-case basis. Considering that Elmendorf et al. (2024a) report effects roughly twice the size of those typically observed in information provision studies, this study is actually well powered to detect belief changes of similar magnitude. Assuming a comparable effect size of $d = 0, 3$, a sample size of $n = 472$ yields approximately 90 pct. power.[28] However, since second-stage effects on preferences are typically smaller, it would still have been ideal to have greater power for that part of the analysis.

While the second-stage effects may be underpowered, the results still offer valuable insights into how belief change may translate into shifts in policy preferences. Because I employ Bayesian inference, the notion of statistical power – a concept strictly rooted in frequentist null hypothesis testing – is not of major concern (McElreath, 2020; Clayton, 2021). Rather than relying on binary significance thresholds, Bayesian analysis expresses uncertainty directly through the posterior distribution. In the Bayesian framework, wide

[27] Link to advertisement

[28] See `power.R` in the replication materials for calculation details.

credible intervals that include zero do not indicate a failure to detect an effect. Instead, they reflect the degree of certainty the data and model warrant about both the sign and magnitude of a given effect (Gelman et al., 2013; McElreath, 2020). These findings should therefore be viewed as informative but provisional – highlighting plausible downstream effects while acknowledging their uncertainty. By contrast, low power in a frequentist setting may obscure such effects entirely by rejecting them or even inflate false positives, due to reliance on arbitrary significance thresholds (Gelman & Carlin, 2014). Readers less familiar with Bayesian inference can refer to Appendix A.2 for a more detailed discussion of this distinction.

**Table 2:** Sample vs. Population Comparison (pct.)

| | Population | Sample | Difference |
|---|---|---|---|
| **Region:** | | | |
| Capital | 30,3 | 26,8 | -3,5 |
| Zealand | 14,9 | 6,6 | -8,3 |
| Southern Denmark | 21,2 | 12,5 | -8,7 |
| Mid Jutland | 23,1 | 46,1 | 23,0 |
| Northern Jutland | 10,5 | 8,1 | -2,4 |
| **Sex:** | | | |
| Male | 49,3 | 54,8 | 5,5 |
| Female | 50,7 | 45,2 | -5,5 |
| **Age:** | | | |
| 18–29 | 18,6 | 40,7 | 22,1 |
| 30–39 | 13,7 | 15,8 | 2,1 |
| 40–49 | 14,7 | 9,4 | -5,3 |
| 50–59 | 17,5 | 14,5 | -3,0 |
| 60–69 | 15,2 | 11,7 | -3,5 |
| 70+ | 20,2 | 7,9 | -12,3 |
| **Education:** | | | |
| Primary school | 22,9 | 2,6 | -20,3 |
| High school | 10,5 | 4,5 | 6,0 |
| Vocational training | 26,9 | 8,7 | -18,2 |
| Short higher ed. | 4,9 | 4,9 | 0,0 |
| Medium higher ed. | 17,2 | 27,5 | 10,3 |
| Long higher ed. | 11,5 | 51,8 | 40,3 |

**Note:** 2022 population shares retrieved from Hansen & Stubager (2023). Columns may not sum to 100 due to rounding.

Turning to representativeness, Table 2 assesses this by benchmarking the sample margins against danish population margins. As expected from a convenience sample, the sample is not very representative.

Regionally, respondents from Mid Jutland are substantially overrepresented (+23 percentage points), while those from Zealand ($-8,3$), Southern Denmark ($-8,7$), and the Capital Region ($-3,5$) are underrepresented. The sample also skews younger, with 40,7 pct. aged 18–29 compared to 18,6 pct. in the population, and older age groups – especially those aged 70 or older – underrepresented (7,9 pct. vs. 20,2 pct.). In terms of education, the sample is substantially more educated than the general population: 51,8 pct. hold a long higher education degree compared to 11,5 pct. in the population, while those with only primary school or vocational training are severely underrepresented. Finally, the sample slightly overrepresents males (54,8 pct. vs. 49,3pct.).

Needless to say, the skewness of the sample limits the thesis' inferential potential. However, this limitation is largely beyond my control, as probability-based sampling is economically infeasible within the scope of a Master's thesis. As will be described shortly, some measures are taken, however, to adjust some of these imbalances in the descriptive part of the analysis. It is also worth noting that there is evidence suggesting convenience samples can be useful for identifying generalizable treatment effects (Mullinix et al., 2015; Coppock et al., 2018).

If one were to speculate on how this skewness might affect the results, the high prevalence of highly educated respondents seems most relevant, as education is positively and robustly correlated with political knowledge (Barabas et al., 2014). Consequently, the prevalence of supply skepticism may be slightly underestimated in the present sample, and learning effects might also be somewhat underestimated, since less educated individuals are more likely to hold uncertain beliefs. Of course, this is tentative, and one could imagine other forms of bias influencing the results in different ways.

Turning to covariate balance across treatment groups – a question of greater concern for the internal validity of the study – Table 3 presents a balance check across treatment arms for both categorical and continuous covariates, where T1 is the group receiving the –1,9 pct. price decrease signal and T2 is the group receiving the –7,6 pct. signal. Broadly speaking, there are no concerning signs of randomization failure.

Among categorical variables, most share differences are small. However, regional distribution and urbanity show somewhat larger imbalances. For instance, respondents from Mid Jutland are overrepresented in T2 ($+9,52$ percentage points), while Southern Denmark and Aarhus/Aalborg/Odense are underrepresented ($-7,44$ and $-8,57$ percentage points, respectively). Differences in tenure and sex are modest, with a slightly higher proportion of renters and males in T2.

For continuous variables, all standardized mean differences (Cohen's $d$) fall well below conventional thresholds for concern ($|d| < 0,3$), indicating limited imbalance. The largest observed difference pre-treatment is for age $y_1$ ($d = -0,18$), while all other measures – such as pre-treatment beliefs and preferences, the treatment's perceived credibility, and comprehension checks – show negligible differences across groups.

### 4.3 Estimation

In the following section I explain the thesis' estimation procedures and empirical strategy. Within each subsection, I clarify how the empirical procedure relates to a given hypothesis by clearly defining the empirical estimand a given estimator targets and how

**Table 3:** Covariate balance

| | T1 | T2 | Difference |
|---|---|---|---|
| **Categorical variables** | | **Group shares (pct.)** | **Share diff.** |
| Education: | | | |
| Primary school | 2,56 | 2,55 | -0,01 |
| High school | 4,70 | 4,26 | -0,45 |
| Vocational training | 10,26 | 7,23 | -3,02 |
| Short higher ed. | 5,13 | 4,68 | -0,45 |
| Medium higher ed. | 26,92 | 28,09 | 1,16 |
| Long higher ed. | 50,43 | 53,19 | 2,76 |
| Region: | | | |
| Capital | 24,88 | 26,98 | 2,10 |
| Zealand | 6,22 | 5,58 | -0,64 |
| Southern Denmark | 16,75 | 9,30 | -7,44 |
| Mid Jutland | 42,11 | 51,63 | 9,52 |
| Northern Jutland | 10,05 | 6,51 | -3,54 |
| Tenure: | | | |
| Renter | 48,07 | 54,62 | 6,55 |
| Co-operative housing | 10,30 | 8,40 | -1,90 |
| Owner | 40,77 | 34,87 | -5,90 |
| Other | 0,86 | 2,10 | 1,24 |
| Sex: | | | |
| Male | 52,84 | 56,78 | 3,94 |
| Female | 47,16 | 43,22 | -3,94 |
| Urbanity: | | | |
| Copenhagen | 24,14 | 24,89 | 0,76 |
| Aarhus/Aalborg/Odense | 33,62 | 42,19 | 8,57 |
| City $>$ 10,000 | 20,26 | 18,14 | -2,12 |
| City $<$ 10,000 | 14,66 | 11,81 | -2,84 |
| Town $<$ 200 or rural | 7,33 | 2,95 | -4,37 |
| **Continuous variables** | | **Raw group means** | **Cohen's $d$** |
| $y_{1_i}$ prior | 0,51 | -0,14 | -0,05 |
| Anti-development prior | 3,42 | 3,38 | -0,03 |
| Likert-scale prior | 3,98 | 4,00 | 0,02 |
| Credibility | 4,84 | 4,91 | 0,06 |
| FMC errors | 0,41 | 0,33 | -0,14 |
| Comprehension errors | 0,43 | 0,36 | -0,11 |
| Left-right | 4,04 | 4,00 | -0,01 |
| Age | 43,94 | 39,94 | -0,18 |
| Respondents | $n$ = 234 | $n$ = 238 | $n$ = 472 |

**Note:** NAs are dropped when performing the calculations for each variables, which is why columns might not sum to 100 for categorical variables. Notice that I intentionally refrain from calculating p-values for the imbalances, as these are non-nonsensical for assessing balance on covariates (although commonly used) (Bruhn & McKenzie, 2009; Rubin & Imbens, 2015) – instead I focus solely on the magnitude of the potential imbalances.

this relates to the hypotheses generated in the theory section (Lundberg et al., 2021).

Note that unless stated otherwise, all models are estimated using Bayesian inference. While Bayesian inference is less commonly used than frequentist methods in political science,[29] it is entirely uncontroversial. I therefore refrain from justifying its use

[29]See König et al. (2017), Clifford et al. (2023), or Stoetzer et al. (2024) for examples of recent applications.

here — many, much brighter minds have already done so (Gelman et al., 2013; McElreath, 2020; Imbens, 2021; Engsted & Schneider, 2024). That said, Appendix A.2 offers a brief introduction to the logic and interpretation of Bayesian inference.

To avoid cluttering the section with too much cumbersome notation, I omit priors from the main text. Full model specifications, including prior distributions, are presented in Appendix A.14. As a general rule, priors are centered on zero with a standard deviation equal to 1 standard deviation of the outcome variable,[30] which is typically considered a weak prior. All models follow the notational style of McElreath (2020) and are estimated using the `brms` R-package (Bürkner, 2017).

### 4.3.1 Modeling beliefs as distributions

To address Hypotheses 1a – that respondents are supply skeptics – and 1b – that their beliefs about price effects are uncertain, this thesis targets two empirical estimands: both related to citizens' underlying beliefs about the price effects of increased market-rate housing. These estimands are grounded in the theoretical framework presented in the theory section, which conceptualized beliefs as probability distributions. Specifically, the estimands correspond to the parameters of respondents' prior belief distributions regarding price effects – namely the prior mean, $\hat{\mu}_0$, and the prior variance, $\sigma_0^2$. Given these estimands, support for Hypothesis 1a would imply that the prior mean takes a positive value, meaning that respondents believe price increases are the most likely outcome (i.e. are supply skeptic). For Hypothesis 1b to be supported, the prior variance needs to take a large absolute value, which indicates that respondents are uncertain about their best guess.

As stated in section 4.1.2, I follow Leemann et al. (2021) and Stoetzer et al. (2024), and employ questions inspired by Manski (2009) to elicit measures that can be used to approximate respondents' prior distributions.[31] I elicit five measures of respondent $i$'s belief about the effect of a supply shock on housing prices. Specifically, I measure three key values: the respondent's best guess (mean), denoted $y_{1i}$, as well as a lower and an upper bound, denoted $y_{2i}$ and $y_{3i}$, respectively. In addition to these values, I elicit two probabilities: the probability that the effect is lower than $y_{2i}$, denoted $p_{1i}$, and the probability that it is higher than $y_{3i}$, denoted $p_{2i}$. The wording of these questions and their corresponding measures were presented in Table 1.

Leemann et al. (2021) and Stoetzer et al. (2024) use these questions to elicit a distribution where the outcome space is bounded between 0 and 1 and therefore fit a Beta distribution using Maximum Likelihood Estimation (MLE). Since I am measuring beliefs about an expected price change from a hypothetical supply shock, a normal

[30] i.e. $\mathcal{N}(0, 12)$ if the outcome variable's SD is 12

[31] Vis-à-vis other survey elicitation methods, the Manski method is both easy to implement and tends to yield better approximations (Leemann et al., 2021).

distribution seems more appropriate.[32] Accordingly, I assume that the latent belief, $B_i$, of respondent $i$ is characterized by a normal distribution where $\mu_i$ represents the mean, and $\sigma_i$ reflects the respondent's uncertainty:

$$B_i \sim \mathcal{N}(\mu_i, \sigma_i)$$

To model this distribution based on the Manski questions, I assume that $y_{1_i}$ is equal to $\mu_i$, allowing $\mu_i$ to be directly estimated through respondent $i$'s answer – that is: $y_{1_i} = \hat{\mu}_i$. Assuming no measurement error in $y_{2_i}$, $y_{3_i}$, $p_{1_i}$ and $p_{2_i}$, the uncertainty, $\sigma_i$, can be modeled using the following equation:

$$\hat{\sigma}_i = \frac{y_{3_i} - y_{2_i}}{\Phi^{-1}(1 - p_{1_i}) - \Phi^{-1}(p_{2_i})} \tag{3}$$

where $\Phi^{-1}(\cdot)$ is the inverse cumulative distribution function (quantile function) of the standard normal distribution (The derivation of equation 3 is presented in Appendix A.3). Equation (3) can likewise estimate the average parameter for a whole sample (or some subsample; e.g. treatment or control group) by substituting the individual Manski responses with the the average Manski responses in the formula instead, i.e. $\bar{y}_1, \bar{y}_2, \bar{y}_3, \bar{p}_1$, and $\bar{p}_2$.

It should be noted that assuming no measurement error is a strong assumption, especially given what we know about untrained individuals' understanding of probabilities (Tversky & Kahneman, 1973; Edwards, 1982; Benjamin, 2019). However, since I am modeling a normal distribution, I cannot directly implement the estimation strategy of Leemann et al. (2021), which, again, relies on fitting a Beta distribution using numerical optimization and MLE.

To keep things parsimonious – and because further mathematical innovation is beyond the scope of this thesis – I instead adopt the simplified approach described above, using equation (3), which can be derived using algebra and probability theory. While this approach is more limited than the one proposed by Leemann et al. (2021), it is sufficient for estimating the parameters of a normal distribution and avoids further complexity in the estimation procedure.

Importantly, because measurement error is assumed random, it will tend to cancel out when calculating the average parameters for the full sample (or relevant subsamples), which are the primary quantities of interest in this analysis. That said, individual-level belief distributions may differ slightly when estimated using equation (3) rather than MLE.[33] While the focus is on aggregate parameters, I still present the individual-level

[32] In theory, an expected price change represents a continuous variable that can take any real value within some unknown interval. Furthermore, the normal is practical because it is a weak – but uninformative – assumption (McElreath, 2020, p. 77).

[33] This has been verified by comparing results from equation (3) with an MLE-based approach on simulated data. These results can be reproduced by running `eq3_mle_comp.R` in the replication materials. Differences are generally limited to the second decimal point.

belief distributions to illustrate the heterogeneity in respondents' beliefs.

As an additional robustness check, I also employ an alternative modeling approach that excludes the probability questions (i.e., $p_{1i}$ and $p_{2i}$) from the estimation. Instead, this approach focuses solely on estimating the sample averages for the most likely value, the lower bound, and the upper bound. This has the advantage of circumventing the problems caused by the difficulty of the probability questions, and it explicitly models statistical uncertainty about the estimates.

However, this model only gauges what respondents believe is a "likely" interval. Because no formal probability is attached to this interval, "likely" remains a subjective notion. In other words, the interval could correspond to a 50 pct. highest density interval or a 95 pct. highest density interval; to this end, the model remains agnostic.

As alluded to above, the modeling approach consists of three distinct intercept-only models, which can be formally expressed as:

$$y_i^m \sim \mathcal{N}(\mu_i^m, \sigma^m), \quad \text{for } m = y_{1_i}, y_{2_i}, y_{3_i} \tag{4}$$
$$\mu_i^m = \alpha^m$$

where $m$ indexes the three Manski responses related to the most likely value, the lower bound, and the upper bound – meaning that equation (4) effectively specifies three distinct models. Each Manski response is assumed to be normally distributed, where $\mu_i^m$ is a direct function of an intercept term, $\alpha^m$. That is, the model estimates a separate mean and standard deviation for each of the three quantities.

Because the sample is not representative, I report both raw and weighted estimates. The weighted estimates are implemented using the raking algorithm[34] (Battaglia et al., 2009), based on the demographic variables and population margins presented in Table 2. In theory, raking can improve representativeness (Solon et al., 2013), but the results should still be interpreted with caution. The presence of many small marginal categories in the sample poses challenges for the raking algorithm, such as convergence issues and highly variable weights (Battaglia et al., 2009). These issues are indeed present in this sample,[35] and I return to these limitations – and their implications for the interpretation of the weighted estimates – in the results section.

#### 4.3.2 Average Treatment Effects

Hypothesis 2a posits that respondents update their beliefs in response to evidence, implying that the theoretical estimand is the causal effect of the informational treatment – that is, beliefs when receiving information versus receiving no information. Ideally,

[34] From the `survey` R-package.

[35] Region is excluded from the weighting scheme, as these issues are exacerbated by the region of Zealand, which constitutes a small marginal category ($n = 22$) and includes several large outliers that disproportionately influence results.

this would be estimated using a passive control group to construct a no-information counterfactual. As previously discussed, implementing such a design is not practically feasible within the scope of this thesis, and thus the ideal estimand cannot be directly recovered. However, an informative approximation is possible by leveraging the temporal variation introduced by the active control design to construct a pre-post design that estimates within-person change in beliefs, using each respondent as their own counterfactual. While this approach relies on additional assumptions – discussed below – these are likely to be met, thus providing useful insight into how individuals update their beliefs, although slightly less robust than the gold standard.

To estimate the within change in beliefs (where belief is measured by the point estimate $y_{1_i}$) about the price effects of increased market-rate housing, I employ the following model specification:

$$B_{it} \sim \mathcal{N}(\mu_{it}, \sigma) \tag{5}$$
$$\mu_{it} = \alpha_{j[it]} + \theta T_{it} + \mathbf{X}'_{it}\boldsymbol{\beta}$$

where $B_{it}$ represents the belief about price effects for observation $i$ at time $t$, assumed to follow a normal distribution with mean $\mu_{it}$ and standard deviation $\sigma$. The mean belief $\mu_{it}$ is modeled using a hierarchical structure, where $\alpha_{j[it]}$ is a random intercept for individual $j$, $\theta$ is a fixed slope capturing the average change in belief between pre ($t = 0$) and post ($t = 1$), and $\mathbf{X}'_i\boldsymbol{\beta}$ is a vector of individual level covariates. I employ this modeling strategy to appropriately reflect the hierarchical structure of the panel data – observations nested within respondents – which in turn enables estimation of how much of the variation in beliefs is driven by baseline differences across respondents rather than by belief updating (Gelman & Hill, 2006; Bell & Jones, 2015). While the random effects (RE) model serves as the primary estimator, I also fit three alternative models as robustness checks: a simple model with a fixed (pooled) intercept, a RE model without covariates, and a model with individual fixed effects (FE). In all four specifications, the central empirical estimand is $\theta$, representing the average change in beliefs between pre- and post-treatment. For Hypothesis 2a to be strongly supported, $\theta$ must be negative and have a low posterior probability of being zero or positive.

Causal interpretation of pre-post models such as this one rests on the assumption that no time-varying factors – aside from the informational treatment – affect beliefs between measurements. This includes, for instance, respondents independently seeking out additional information. It also assumes that, in the absence of new information, beliefs remain stable over short intervals. In the present case, the risk of contamination from external information is minimal, as the median survey completion time was approximately 10 minutes.[36] Moreover, it is standard in the literature to assume that

[36]While 10 minutes is relatively long, the complexity of the survey justifies the duration and does

beliefs do not shift meaningfully within such a short timeframe without new information (Hjort et al., 2021). Thus, while the causal interpretation of Equation (5) hinges on these assumptions, they are considered credible in this context.

Turning to Hypothesis 2b – which posits that a stronger signal induces greater learning – the implied estimand is the causal effect of being presented a larger price effect point estimate on the magnitude of belief updating. Unlike the estimand for Hypothesis 2a, this quantity can be directly identified within the active control design. Accordingly, it is estimated using the following model:

$$\begin{aligned} B_i^1 &\sim \mathcal{N}(\mu_i, \sigma) \\ \mu_i &= \alpha + \theta D_i + \varphi B_i^0 + \mathbf{X}_i' \boldsymbol{\beta} \end{aligned} \tag{6}$$

where the response variable $B_i^1$ represents respondent $i$'s posterior belief – that is, their belief about price effects at time $t = 1$. This is assumed to follow a normal distribution with mean $\mu_i$ and standard deviation $\sigma$. The mean $\mu_i$ is modeled as a linear function of the intercept $\alpha$, the treatment effect term $\theta D_i$, where $D_i$ is a binary indicator of treatment status, the prior belief term $\varphi B_i^0$, and a vector of individual level covariates $\mathbf{X}_i' \boldsymbol{\beta}$.

There is some debate in the statistics literature about whether to model pre-post data using a specification like the one above – where the post-treatment measure is the outcome and the pre-treatment measure is included as a covariate – or whether to use a first-difference approach, where the outcome is defined as $\Delta B_i = B_i^1 - B_i^0$, which is a less biased model when dealing with observational data (Allison, 1990). Because treatment is randomly assigned, the risk of bias is negligible and the specification in the above equation is preferable as it tends to yield more efficient estimates (Lin, 2013). Nonetheless, for robustness, I also estimate a simple model with no covariates, a model adjusting only for the pre-treatment belief, and a first-difference model. In all cases, the central empirical estimand is $\theta$, representing the average difference in posterior beliefs between treatment groups – that is a difference in *levels*. In the first-difference specification, however, $\theta$ captures the average difference in belief updating between treatment groups (i.e. a difference in *change*), as it effectively operates as a difference-in-differences (DiD) estimator. In either case, a negative $\theta$ with a low posterior probability of being zero or positive, would support Hypothesis 2b.

Additionally, the estimators provide a test of a structural implication derived from Equation (2). Because treatment is randomly assigned, prior uncertainty should be constant across groups. While signal variance is not theoretically fixed – due to possible information discounting (Christensen, 2023) – the treatment signals are perceived as similarly credible (see Table 3), suggesting comparable signal uncertainty in practice.

not raise concern.

The primary difference between groups, then, is the signal mean: –7,6 percentage points in the treatment group versus -–1,9 in the control, which, under Bayesian updating, implies that the treatment group should, on average, update their beliefs approximately four times more than the control group (see panels A and B in Figure 3). Empirically, this means that the sum of the intercept and the slope in the first-difference model should be roughly four times the size of the intercept alone, or that the mean posterior should be four times larger in T2 than in T1 – if respondents are perfectly Bayesian.

### 4.3.3 Learning rates

While the study is not explicitly designed to quantify deviations from Bayesian behavior –unlike, Hill (2017) for instance – it still allows for meaningful evaluation of whether the observed patterns are consistent with the expectations of the Bayesian framework, which the above hypotheses are built on.

As briefly mentioned in the experimental design section, the signal provided to respondents is presented on the same scale as their beliefs, allowing the data to be interpreted through a simplified Bayesian framework (Hjort et al., 2021; Bottan & Perez-Truglia, 2022). Following the logic of Equation (2), we can treat the pre-treatment Manski-response (i.e. $y_{1_i}$) as the mean in respondent $i$'s prior belief about the price effects of market rate housing $B_i^0 \sim \mathcal{N}(\mu_i, \sigma_{i,0}^2)$ and the treatment conditions as noisy signals drawn from $S_i \sim \mathcal{N}(\mu, \sigma_{i,S}^2)$. Under these assumptions, a Bayesian respondent wishing to have accurate beliefs (minimize squared error) will form a posterior $B_i^1$ in accordance with:

$$B_i^1 = (1 - \pi)B_i^0 + \pi S_i$$

where the weight $\pi$ reflects the relative precision of the signal and prior beliefs, calculated as $\pi = \tau_{i,S}/(\tau_{i,0} + \tau_{i,S})$, with $\tau_{i,0} = 1/\sigma_{i,0}^2$ and $\tau_{i,S} = 1/\sigma_{i,S}^2$. Substantively, this implies that the posterior belief is a convex combination of the prior and the signal, weighted by their perceived relative precision (Hjort et al., 2021)[37] – and consequently, that the empirical weight can be estimated using the following model:

$$\begin{aligned} B_i^1 &\sim \mathcal{N}(\mu_i, \sigma) \\ \mu_i &= \pi_b B_i^0 + \pi_s S_i \end{aligned} \tag{7}$$

where the posterior belief $B_i^1$ is assumed to follow a normal distribution with mean $\mu_i$ and standard deviation $\sigma$. The mean $\mu_i$ is modeled as a weighted combination of the prior $B_i^0$ and the signal $S_i$, where the coefficients $\pi_b$ and $\pi_s$ represent the empirical weight respondents place on the prior and the signal when forming their posterior beliefs. Accordingly, if $\pi_b + \pi_s \approx 1$, it would indicate that respondents' updating is consistent

[37]This formulation is a more compact, indexed expression of the model outlined in Equation 2.

with the Bayesian framework (Hjort et al., 2021). Moreover, I posited in the theory section that respondents would readily update their beliefs due to presumably weak priors – empirically a larger $\pi_s$ relative to $\pi_b$ would support this claim, as it indicates that more weight is placed on the signal when updating.

To reiterate, this modeling approach doesn't test a specific hypothesis, but it does provide valuable insights into the underlying reasoning processes respondents use when processing the information provided in the experiment; thus functioning more as a mechanism test.

#### 4.3.4 Heterogeneous learning rates

While the above model estimates the average weight across all respondents, it can also be extended to explore heterogeneity in learning behavior. Individuals may vary in how much they trust the signal or in the strength of their initial beliefs, leading to systematic differences in the estimated weights, which is obscured by the above model. By examining how $\pi_s$ and $\pi_b$ is moderated by observable respondent characteristics we can gain a richer understanding of who updates and why, again shedding light on the underlying mechanisms of belief updating. Specifically, individuals who perceive the informational treatment as credible should place greater weight on the signal – assuming that perceived credibility serves as a valid proxy for perceived signal variance (i.e. $\sigma_x^2$). Furthermore, given that perceived credibility appears constant across treatment arms, a systematic difference in signal weighting between treatment conditions is not expected.

Beyond these more mechanistic expectations, variation in signal weighting may also arise from sociodemographic characteristics. For instance, it is plausible that renters hold stronger priors about price effects, given their direct exposure to rent fluctuations and associated anxieties about housing affordability (Hankinson, 2018). Thus, their direct experience with the rental market may make them more confident in their beliefs, leading to less updating in response to new information.

Furthermore, Müller & Gsottbauer (2022) find that left-leaning individuals tend to hold more optimistic priors about rent control, but also update more strongly when confronted with disconfirming evidence. By analogy, it is plausible that left-leaning respondents in the present study may hold more pessimistic priors about market-rate housing and, as a result, will exhibit greater belief updating when presented with information suggesting beneficial effects – especially because the informational treatment highlight benefits for low-income households.

Finally, as hypothesized by Müller & Gsottbauer (2022), education may also influence the degree of signal weighting. More educated individuals may be more inclined to support economically efficient policies (Baranzini & Carattini, 2017), which could make them more receptive to the informational treatment. This implies that a higher level of education may be associated with greater weight placed on the informational signal.

To shed light on these mechanisms, I slightly rearrange the updating model[38] and add an interaction term:

$$\Delta B_i \sim \mathcal{T}(\nu, \mu_i, \sigma) \tag{8}$$
$$\mu_i = \pi_s G_i + \zeta C_i^j + \gamma(G_i \times C_i^j)$$

Here, the response variable, $\Delta B_i$ represents the difference between the prior and posterior belief for respondent $i$, which is assumed to follow a Student's t-distribution[39] with degrees of freedom $\nu$, mean $\mu_i$ and standard deviation $\sigma$. The mean $\mu_i$ is specified as a function of the prior gap (Signal–Prior) term $\pi_s G_i$, a dummy variable term $\zeta C_i^j$ for covariate $j$, and their interaction term $\gamma(G_i \times C_i^j)$. The interpretation of $\pi_s$ is the same as in equation (7), which means that $\gamma$ denotes the difference in signal weight between groups. As in Equation (4), this formulation essentially represents a series of separate models – one for each covariate $j$ described above.[40]

As a final exploration of mechanisms I investigate weather signal weighting increases with prior uncertainty as well as with a more granular measure of credibility, as would be expected in the Bayesian framework. This is implemented through the following model:

$$\Delta B_i \sim \mathcal{T}(\nu, \mu_i, \sigma) \tag{9}$$
$$\mu_i = \pi_{s_{j[i]}} G_i$$

This specification is conceptually similar to Equation (8), but models $\Delta B_i$ as a function of a single varying effect $\pi_{s_{j[i]}} G_i$ for covariate $j$. I adopt this approach for both the granular credibility and prior uncertainty measures because there is substantial heterogeneity in group sizes – for example, few respondents rated the treatment as highly unconvincing. Partial pooling through a hierarchical model structure enables more precise estimates for these smaller subgroups (Gelman & Hill, 2006) and facilitates flexible, non-parametric modeling of these moderators without imposing linearity assumptions.

The specific measure for prior uncertainty is the estimated prior $\hat{\sigma}_i$ (i.e. Equation 3), which I collapse into ten bins ranging from 1-10+ to reduce the previously discussed measurement error in $\hat{\sigma}_i$. The "granular" measure of credibility is the full 7-point Likert-scale, instead of the dummy used in Equation (8).

[38] $B_i^1 = (1-\pi)B_i^0 + \pi S_i \Leftrightarrow B_i^1 - B_i^0 = \pi(S_i - B_i^0)$ as shown by Bottan & Perez-Truglia (2022)

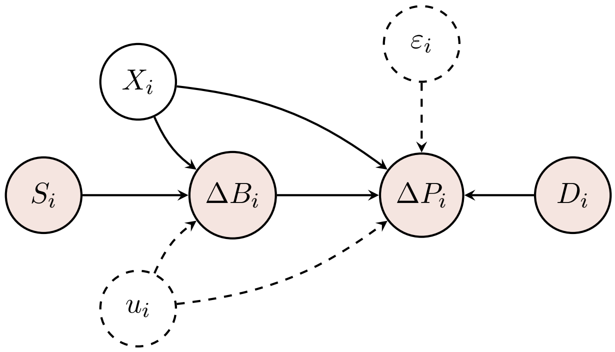
[39] The model employs this likelihood in favor of the normal likelihood of previous models, because the data include some large outliers. The Student's t-distribution provides a more robust fit without excluding these observations (McElreath, 2020, p.237).

[40] i.e. $j = \{\text{credibility, treatment, tenure, ideology, education}\}$

### 4.3.5 Effects on housing policy preferences

Turning to effects on preferences, Hypothesis 3a posits that belief updating leads to changes in macro-level housing policy preferences. This implies that the empirical estimand is the causal effect of belief updating on policy preferences. Unlike the previous estimands, however, identifying this effect presents a substantial challenge, as belief updating is not randomly assigned – a complication illustrated in Figure 6. As illustrated

**Figure 6:** Data generating process for policy preference updating




**Note:** Dashed nodes and lines indicate latent (unobserved) variables. Slight extension of the canonical IV-DAG (Cunningham, 2021, p. 325)

in the Directed Acyclical Graph (DAG), estimating changes in preferences ($\Delta P_i$) directly as a function of changes in beliefs ($\Delta B_i$) is likely to produce biased results due to endogeneity. For instance, an unobserved factor such as self-perceived knowledge of the housing market could influence both how individuals update their beliefs and how they revise their preferences. A similar issue arises with observed confounders like tenure status: being a homeowner might plausibly affect both belief updating (as discussed in Section 4.3.4) and housing policy preferences, as suggested by Fischel (2005). Furthermore, it wouldn't be improbable that housing policy preferences shape updating behavior, thus possibly creating a problem of reverse causality.

However, as the DAG illustrates, the signal ($S_i$) provided to respondents – i.e., the treatment condition – is independent of both observed and unobserved confounders ($X_i$ and $u_i$) as well as the structural error term ($\varepsilon_i$). This independence makes the signal a valid binary instrument, as it satisfies the classical exclusion restriction and, given the active control design, plausibly meets the monotonicity assumption (Angrist & Imbens, 1995; Imbens & Wooldridge, 2009; Cunningham, 2021; Vilfort & Zhang, 2024).

In addition to these classical IV assumptions, active control information provision experiments introduce a unique requirement: the neutrality condition, which holds that respondents perceive the informational treatment as "equally credible" across treatment arms (Vilfort & Zhang, 2024, p. 14). While the classical IV assumptions are not directly testable in this context, the neutrality condition is. As shown in Table 3, perceived credibility does not vary meaningfully between treatment groups, suggesting that this assumption is satisfied.

Given that the assumptions hold the identification problems can be circumvented by fitting the following IV-model:

$$\begin{pmatrix} \Delta P_i \\ \Delta B_i \end{pmatrix} \sim \text{MVN}\left( \begin{pmatrix} \mu_{\Delta P,i} \\ \mu_{\Delta B,i} \end{pmatrix}, \mathbf{S} \right) \tag{10}$$
$$\mu_{\Delta P,i} = \tau + \delta \Delta B_i$$
$$\mu_{\Delta B,i} = \alpha + \beta S_i$$

The model used is a multivariate linear model (McElreath, 2020, p. 472)[41], in which the two response variables – belief updating ($\Delta B_i$) and policy preference updating ($\Delta P_i$)[42] – are assumed to follow a multivariate normal distribution with an error covariance matrix $\mathbf{S}$.

The first mean, $\mu_{\Delta B}$, is modeled as a function of an intercept $\alpha$ and the treatment signal $\beta S_i$. The second mean, $\mu_{\Delta P}$, is a function of an intercept $\tau$ and belief updating, represented by the coefficient $\delta \Delta B_i$. This coefficient, $\delta$, is the key empirical estimand of interest and is interpreted analogously to the second stage of a 2SLS model.

Support for Hypothesis 3a requires that $\delta$ be positive and credibly different from zero – indicating that belief updating toward more positive price effects (i.e., believing that market-rate housing increases prices) is associated with a shift toward more anti-development preferences, and vice versa. Since both the predictor and outcome are expressed as change scores, direct interpretation of the coefficient can be non-intuitive. However, a positive $\delta$ implies that updating toward a price decrease (i.e., negative $\Delta B_i$) predicts a decrease in anti-development preferences, when $\tau \approx 0$, which is the expected value in a Bayesian framework where no preference change is predicted in the absence of belief updating.[43]

Given the model's complexity, the analysis focuses primarily on the described predicted changes in preferences, rather than solely on the coefficient estimate.

Hypothesis 3b posits that learning about market-rate housing has weaker effects on preferences as proximity to a project increases. This implies an estimand similar to that of Hypothesis 3a, but conditioned on respondents' distance to a specific housing project. Because distance to the project ($D_i$) is randomized, it is exogenous by design – as illustrated in the DAG in Figure 6 – allowing identification of the conditional causal

[41] This IV estimation approach models the instrumental variable structure within a joint model, rather than through a two-step procedure like two-stage least squares (2SLS), though the interpretation remains similar.

[42] Defined as $\Delta P_i = P_i^1 - P_i^0$, based on the Likert-scale item described in the outcome measure section, where superscripts denote prior ($t = 0$) and posterior ($t = 1$) responses.

[43] If $\tau \neq 0$, interpretation shifts slightly. For instance, a positive $\tau$ would imply a baseline increase in anti-development preferences, with belief updating moderating that shift.

effect. This is achieved by extending the IV model above to include an interaction between belief updating and distance.[44]

$$\begin{pmatrix} \Delta P_i \\ \Delta B_i \end{pmatrix} \sim \text{MVN}\left(\begin{pmatrix} \mu_{\Delta P,i} \\ \mu_{\Delta B,i} \end{pmatrix}, \mathbf{S}\right) \tag{11}$$

$$\mu_{\Delta P,i} = \tau + \delta \Delta B_i + \sum_{d=2}^{5} \theta_d D_{id} + \sum_{d=2}^{5} \gamma_d (D_{id} \times \Delta B_i)$$

$$\mu_{\Delta B,i} = \alpha + \beta S_i$$

The model is essentially the same as in Equation (10), but the function determining $\mu_{\Delta P}$ includes four indicator variables $D_{id}$ representing discrete distance categories from a hypothetical housing project – 500 meters ($d = 2$), 1 kilometer ($d = 3$), 3 kilometers ($d = 4$), and 5 kilometers ($d = 5$) – with the closest distance category, 80 meters ($d = 1$), serving as the reference group. The four interaction terms $\gamma_d(D_{id} \times \Delta B_i)$ allow the effect of belief updating on preference updating to vary across these distance categories. Distance is modeled as discrete categories rather than as a continuous measure to allow for greater flexibility and to avoid imposing potentially unwarranted linearity assumptions on how proximity moderates treatment effects. Here the empirical estimand is the marginal effect of belief updating on preference updating (i.e. $\delta + \gamma$ for each $d$). Support for Hypothesis 3b entails larger positive marginal effects as distance increases, where the substantial interpretation is the same as in equation (10), but conditional on distance.

# 5 Results

The following section reports the results of the analysis and mirrors the structure of the estimation section, presenting findings in the same chronological order as the estimators introduced above – with the exception of the first section, which estimates simple differences using the frequentist framework.

## 5.1 Simple (frequentist) differences

Before turning to more formal modeling, I report differences in post-treatment outcomes between treatment groups (Table 4), which are essentially simple average treatment effects (ATEs). Reporting these preliminary effects serves two purposes. First, it promotes transparency by demonstrating that the results do not rely on *suppression effects*[45] (Lenz & Sahn, 2021) or other modeling assumptions. Secondly, since my analysis is Bayesian I

[44] While the DAG does not explicitly represent an interaction term, it conveys that $\Delta P = f(\Delta B, D)$ and thus that an interaction can be estimated (McElreath, 2020, p. 244). The choice to fit an interaction is governed by the theoretical arguments related to Hypothesis 3b

[45] *Control-variable-induced increases in estimated effect sizes*

compute p-values for these effects, to reassure readers unfamiliar with Bayesian inference, that the results are not an artifact of the Bayesian approach.

**Table 4:** Frequentist ATEs

| | Raw group means | | ATE |
|---|---|---|---|
| **Treatment differences** | **T1** | **T2** | **Cohen's $d$** |
| Price effect ($y_{1_i}$) posterior | –1,71 | –4,88 | –0,29** |
| Likert-scale posterior | 3,51 | 3,29 | –0,19* |
| Posterior−Prior ($\Delta B_i$) | –2,21 | –4,75 | –0,21* |
| Anti-development posterior | 3,25 | 3,15 | –0,06 |
| Anti-dev. posterior−prior ($\Delta P_i$) | –0,2 | –0,23 | –0,04 |
| **Pre-post differences** | **Pre** | **Post** | **Cohen's $d$** |
| Price effect ($y_{1_i}$) | 0,18 | –3,31 | –0,29*** |
| Anti-development | 3,40 | 3,20 | –0,13* |
| $n =$[a] | 234 & 472 | 238 & 472 | 472 & 944 |

**Note:** *p<0,05; **p<0,01; ***p<0,001. [a] Sample sizes are listed as Treatment $n$ followed by Pre-Post $n$, in each column. T1 = -1,9 signal and T2 = -7,6 signal

The first outcome of interest is the posterior difference in $y_{1i}$, which is estimated to be $d = -0,29$ (p < 0,01), meaning that the group receiving the stronger signal, believes that increasing market rate housing has stronger negative price effects than the group receiving the weaker signal. The table also presents the difference in posterior belief about price effects measured on the Likert-scale. The difference is of slightly smaller magnitude at $d = -0,19$ but still significant at the conventional threshold (p < 0,05) and conveys the same substantial interpretation as above. Turning to updating behavior, the group receiving the stronger signal update more in the expected direction ($d = -0,21$, p < 0,05). Generally speaking, these effects align with the expectation that respondents update their beliefs upon receiving information (H2a), and that the extent of updating scales with the magnitude of the presented price effect (H2b).

Turning to downstream effects on preferences, respondents in T2 exhibit marginally lower post-treatment anti-development preferences at the macro level ($d = -0,06$), though the difference is not conventionally significant ($p = 0,487$). Similarly, T2 participants shift their preferences slightly more in a pro-development direction over time ($d = -0,04$), but again the difference is not statistically significant ($p = 0,648$). While both effects are in the expected direction, these tests fail to reject the null at conventional thresholds and thus do not provide support for the claim that belief updating drives changes in policy preferences (H3a).

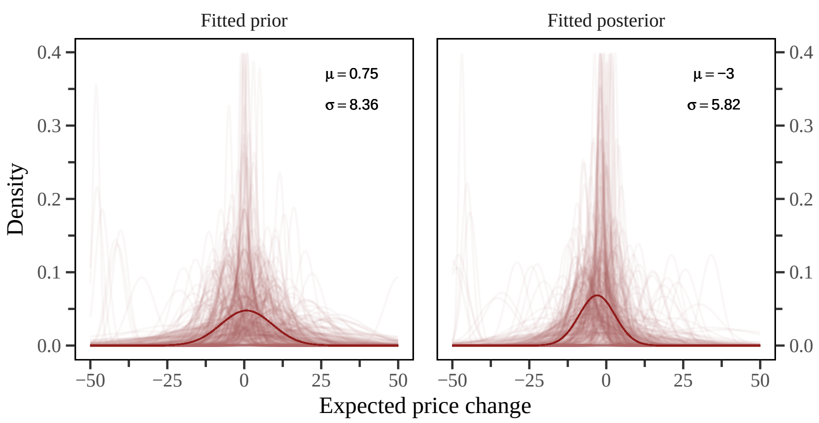
The table also reports pre-post differences in beliefs and preferences. Here, the expected price change shows a pre-post difference of $d = 0,29$, which is statistically significant ($p < 0,001$). As discussed in section 4.3.2, this difference approximates the effect of receiving the information compared to not receiving it (under the assumptions discussed in the same section), and thus supports Hypothesis 2a. While the research designs are not directly comparable, this is very similar to the effect size Elmendorf et al. (2024a) report for a similar informational treatment. Likewise, receiving the information leads to a reduction in anti-development preferences at the macro level ($d = -0,13$), which is also statistically significant (p < 0,05). This lends tentative support to Hypothesis 3a. However, as will be discussed in Section 5.6, this change in preferences likely reflects a compound treatment effect operating through additional mechanisms beyond price beliefs alone.

To reiterate, these initial tests are quite simple and do not allow for a full examination of some hypotheses, which require more careful modeling to be properly tested. However, it is reassuring that the results I present below replicate even in this basic setup. I will now turn to the more rigorously modeled tests of the hypotheses outlined in the estimation section.

## 5.2 Are respondents supply skeptics?

Concerning danish citizens beliefs about the price effects of market-rate housing, the average elicited belief distributions, and the underlying individual level distributions, are presented in Figure 7.

**Figure 7:** Prior and posterior belief distributions




**Note:** Opaque lines show the prior and posterior averaged across respondents; transparent lines depict individual-level distributions. The prior includes respondents who passed the prior Manski question (n = 415), while the posterior includes those who passed both prior and posterior questions (n = 380). Calculating the prior for the same subpopulation as the posterior yields substantially identical results; with the mean unchanged and the standard deviation reduced by 0,12.

On average, citizens hold mildly supply-skeptic priors with relatively high uncertainty. The prior distribution's mean ($\mu$) is estimated to be 0,75, indicating that respondents, on average, consider a 0,75 pct price increase to be the most likely effect when housing supply increases by 10 pct. in a given municipality. This implies that respondents generally perceive price increases or no change as more probable than price decreases $\big(\mathbb{P}(X > 0) = 0,54\big)$, which is an empirical pattern in alignment with the expectation from Hypothesis 1a. However, as this is the first study to elicit supply skepticism on a continuous numerical scale, it remains difficult to assess whether this should be interpreted as a high or low degree of skepticism. Intuitively, a 0,75 pct. increase suggests only a modest expected price increase, and supply skepticism is thus interpreted as mild.

Turning to the standard deviation ($\sigma$), the parameter is estimated to be 8,36, indicating that respondents, on average, consider price changes between –7,6 pct. and 9,11 pct. to be fairly probable.[46] Again, in the absence of established benchmarks, it is difficult to definitively assess whether this reflects high or low uncertainty. Still, the fact that respondents view both sizeable price increases and decreases as plausible suggests a substantial degree of uncertainty. This aligns with the expectation from Hypothesis 1b and reinforces the notion put forward by Nall et al. (2024) that supply skepticism is a weakly held belief, more akin to a non-attitude.

While both Hypothesis 1a and 1b are supported by the data, it is worth noting that the underlying individual-level prior distributions exhibit substantial heterogeneity across both parameters. As previously discussed, these individual-level estimates are likely to be noisy and should be interpreted with caution; nevertheless, they suggest that a non-negligible share of respondents do not hold supply-skeptical views. This suggests an important distinction: uncertain supply skepticism is the predominant belief only on average, not a uniform view across individuals.

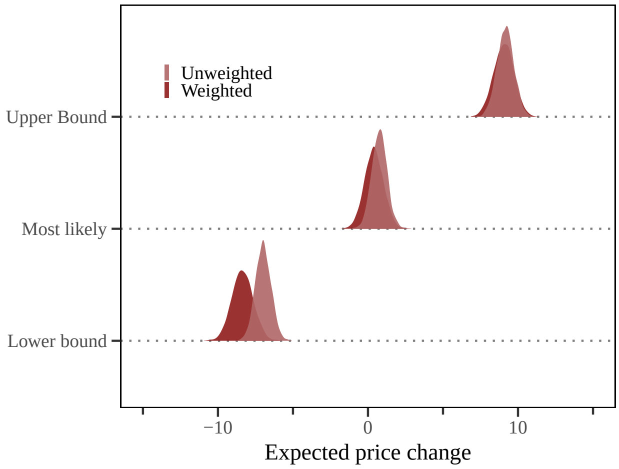
Turning to average posteriors, the informational treatments seems to drive updating in a way that is compliant with Bayesian updating rules. The average posterior mean is –3 pct. which has both reversed the sign of the average expected price effect and increased it's magnitude. Consequently, respondents consider positive price effects as rather improbable a posteriori $\big(\mathbb{P}(X > 0) = 0,30\big)$. The average posterior standard deviation ($\sigma = 5,82$) is diminished by approximately one third of the prior distributions standard deviation, which is still quite uncertain, but this uncertainty mostly spans negative values. Since I don't observe the respondent's likelihood function I cannot say whether the observed posterior is equal to the one under perfect Bayesian updating, nor can I quantify possible deviations from Bayes rule. However, the mean moving towards the signal with a reduction in uncertainty is in line with the theoretical expectations of the Bayesain framework (i.e. Figure 3) and thus also lends preliminary support to Hypothesis 2a – that individuals update their beliefs when faced with information.

As displayed in Figure 8 – which presents the likely range estimates – the findings are robust to estimation methods that don't hinge on the Manski probability questions. Overall the the likely range estimates, retrieve a very similar pattern, as above, with

[46] $\mathbb{P}(-7,6 < X < 9,11) = 0,68$

mild and uncertain supply skepticism on average.

**Figure 8:** Elicited likely range priors




**Note:** Marginal posterior distributions estimating the average response to the three likely range questions ($y3$ = upper bound, $y2$ = lower bound, $y1$ = most likely value) Weighted (n = 404) and unweighted (n = 415) posterior distributions based on Equation 4.

The raw posterior distribution for the most likely value ($y_1$) has a mean of 0,76 percentage points, with 95 pct. of the posterior probability density falling between –0,24 and 1,78 percentage points – reflecting the previously discussed heterogeneity in individual-level beliefs (see Figure A.2 for the distribution of individual-level ranges). The posterior means for the lower ($y_2$) and upper ($y_3$) bounds are –6,98 and 9,16 percentage points, respectively, with corresponding 95 pct. credible intervals of [–8,00; –5,93] and [8,04; 10,28]. This implies a slightly asymmetric likely range, with a modest skew toward upward uncertainty. As discussed in the methods section, the likely range is diffuse in the sense that no precise probability density is assigned within it, making it unclear how much probability density respondents associate with the interval. Nonetheless, the slight asymmetry in the range may suggest that respondents' beliefs depart from normality – though this interpretation should be made with caution, as it remains ambiguous whether the likely range reflects an underlying probability distribution.

Across all three measures, weighted and unweighted posterior distributions are closely aligned, indicating that the results are robust to the application of sample weights. However, under the weighted specification, the most likely value shifts slightly downward to 0,41 percentage points, with a 95 pct. credible interval of [–0,81, 1,66], suggesting somewhat less and more uncertainly estimated supply skepticism in the broader Danish population than in the unweighted sample. The lower bound also shifts slightly downward, resulting in a more symmetric likely range. As discussed in the methods section, the reliability of these weights is uncertain, and the weighted results should therefore be interpreted primarily as a robustness check rather than as definitively population-representative estimates. As before, this distributional pattern is consistent with both Hypotheses 1a and 1b.

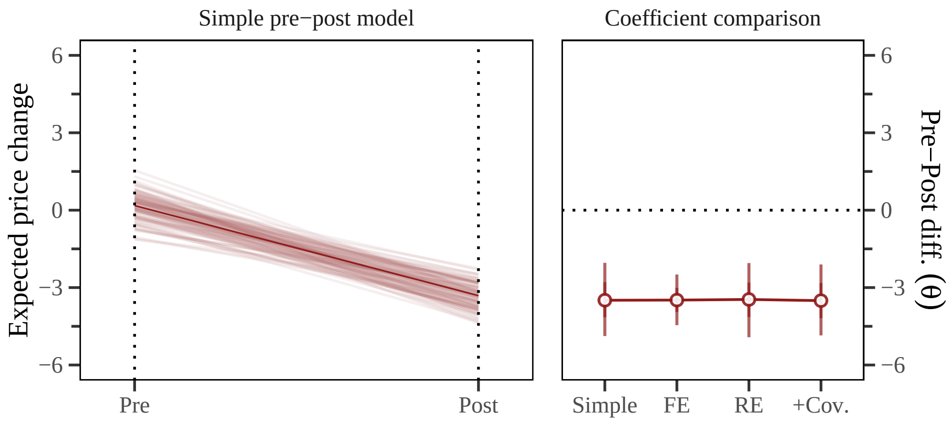
As a final robustness check related to supply skepticism, it is worth noting that the subjective Likert scale measure yields results consistent with the main analysis. Specifically, 62,1 pct. of respondents reported prior beliefs that increasing market-rate housing would either have no effect or increase prices – closely aligning with the proportion reported by Nall et al. (2024) in the American context. The corresponding posterior proportion drops to 38,1 pct. (see Figure A.1 for the marginal distribution across the Likert scale).

In sum, the results suggest that respondents are indeed slightly supply-skeptical, but that their beliefs are highly uncertain and therefore more accurately characterized as a non-attitude; a conclusion which is robust across multiple alternative measures. Furthermore, supply skepticism is the average belief, but it varies substantially at the individual level, where some individuals also hold very supply optimistic – or pessimistic – views.

### 5.3 Do respondents learn?

Regarding whether respondents learn from evidence or not (H2a), the pre-post changes in beliefs (Equation 5) are displayed in Figure 9. The left panel presents the simple model's predicted point beliefs pre and post; the right panel compares pre-post coefficients across model-specifications.

**Figure 9:** Pre-Post updating behaviour




**Note:** 100 lines ($\alpha + \theta T$) sampled from the posterior distribution (transparent lines) and posterior median line $\theta$ (opaque line). Right panel: Posterior median with 66 pct. and 95 pct. credible intervals (n varies between 944 and 882 across models). "FE" = Fixed Effects, "RE" = Random effects, "+Cov." = Model with RE and covariates

As depicted in the left panel, the simple model estimates the average prior point belief at 0,18 pct.[47] with a 95 pct. credible interval of [–0,81; 1,18] which decreases by –3,48 pct. [–4,88; –2,04] when forming the point belief posterior.[48] This decrease is reliably estimated and very stable across all model specifications, with the preferred specification (RE with covariates) yielding a point estimate of –3,5 pct. with a corresponding 95 pct. credible interval of [–4,85; –2,10]. Substantively then, respondents' beliefs about expected price changes update from being slightly supply skeptic to being rather optimistic about increased market-rate housings ability to lower prices. The magnitude of the change is very similar to the one noted in the frequentist section, and thus further corroborates support for Hypothesis 2a.

On a final note, the intraclass correlation (ICC) in the random effects model with covariates is approximately zero,[49] which indicates that nearly all variation in beliefs is attributable to pre-post updating rather than to stable individual-level differences (Figure A.3 plots the random intercepts). Substantively, this suggests that beliefs are highly dynamic. This finding aligns with expectations laid out in the theory section, which posited high belief elasticity due to the complexity of the housing policy domain and the low availability of party-cues – thereby lending support to the theoretical mechanisms underlying Hypothesis 2a.

Concerning whether a stronger signal induces more learning (H2b), the differences in updating magnitude between treatment groups (Equation 6) are displayed in Figure 10. The left panel presents the simple model's predicted posterior price change expectation for T1 and T2 respectively; the right panel compares coefficients across model-specifications.

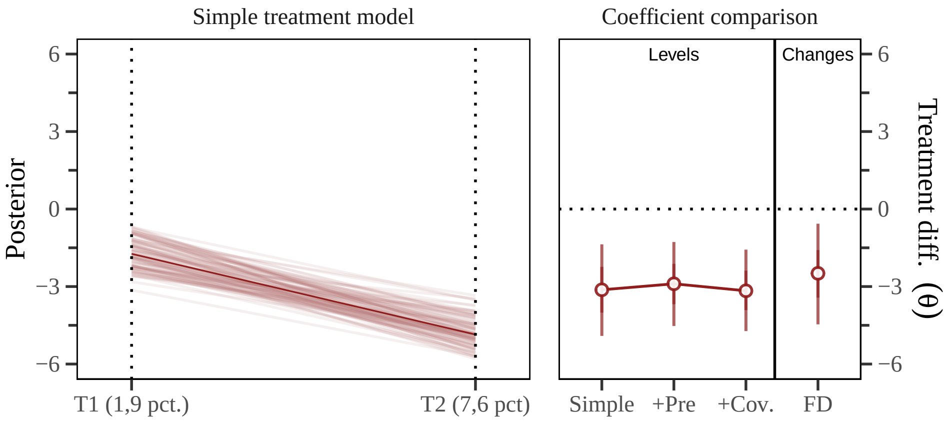
As shown in the left panel the posterior expected price change for T1 is estimated to be –1,72 pct. [–3,00; –0,47] and the posterior for T2 is estimated to be –4,84 pct. [–6,14; –3,57]. The difference in posteriors is estimated to be –3,13 pct. [–4,93; –1,37], which is likewise numerically stable across models, where the preferred specification – which controls for both the baseline belief (prior) and covariates – yields an estimated difference in posteriors of –3,16 [–4,73; –1,57]. Substantively, these results suggest that individuals exposed to the larger price effect in the informational treatment (–7,6 pct.) revised their beliefs about expected price changes downward to a greater extent than those exposed to the smaller price effect (–1,9 pct.). The difference in posterior beliefs – over 3 percentage points – reflects meaningful responsiveness to the magnitude of the informational signal. This supports the idea that individuals are not only updating

[47] Note that this deviates from the results in the previous section (0,75 pct.), as this model focuses solely on point beliefs and therefore includes a broader sample that is not trimmed based on the logical consistency constraints applied to the full set of Manski questions (i.e. section 4.1.5).

[48] Posterior point belief is thus –3.3 pct. [–4,28; –2,29]

[49] ICC $= \frac{\sigma_j^2}{\sigma_j^2 + \sigma_y^2} = \frac{0{,}09^2}{0{,}09^2 + 10{,}42^2} \approx 0$ (Gelman & Hill, 2006, p. 258)

**Figure 10:** Differential updating across treatment status




**Note:** Left panel: 100 lines ($\alpha + \theta T$) sampled from the posterior distribution (transparent lines) and posterior median line $\theta$ (opaque line). Right panel: Posterior median with 66 pct. and 95 pct. credible intervals (n varies between 472 and 441 across models)

but doing so in proportion to the strength of the evidence presented, consistent with Hypothesis 2b.

The first-differencing model produces very similar results with T1 updating –2,21 pct. [–3,57; –0,82] and T2 updating –4,71 pct. [–6,06; –3,38] yielding an estimated difference in updating of –2,51 pct. [–4,48; –0,57]. To reiterate, the levels models capture differences in posterior beliefs between treatment groups, whereas the first-difference model directly estimates the belief change within individuals, offering a more direct interpretation of belief shifts. That the results hold across both the levels-based and first-differencing specifications strengthens the evidence for a dose-response relationship between signal strength and belief updating and thus further supports Hypothesis 2b.

As briefly discussed in the methods section, a more structural expectation derived from the Bayesian framework was that respondents in T2 would update their beliefs four times as much as those in T1, assuming the signals were perceived as equally credible. Empirically, the most likely updating ratio is approximately 2,13.[50] While this does not rule out a fourfold updating ratio, the model assigns only 5,5 pct. posterior probability to an updating ratio between 3,5 and 4,5. Thus, the data do not support the structural implication that updating scales proportionally with signal strength. Theoretically, this suggests one or more of the following: 1) the credibility measure may not accurately capture perceived likelihood uncertainty; 2) updating behavior is nonlinear; or 3) citizens discount larger signals (Christensen, 2023). Some more data-driven explanations will be proposed in the next subsection.

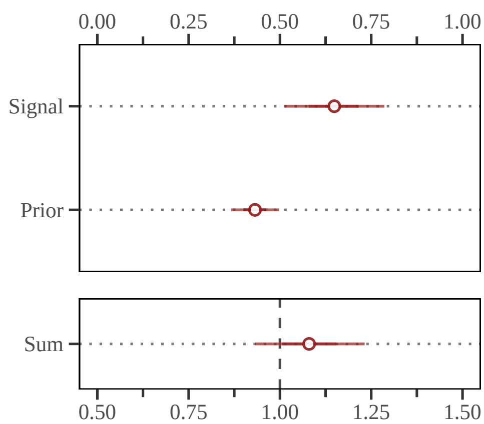
[50] $(-4,71/-2,21 = 2,13)$

In sum, the evidence clearly indicates that respondents do learn from the informational treatment. Both overall belief shifts (H2a) and differential updating by signal strength (H2b) demonstrate that individuals revise their expectations in the direction of the information they are shown. On average, respondents become substantially more optimistic about the price effects of market-rate housing after treatment, and those exposed to a stronger signal (T2) update more than those given a weaker one (T1). These patterns are robust across multiple modeling strategies and support the notion that belief formation in the housing policy domain is responsive to evidence. However, the magnitude of updating does not scale fully proportionally with signal size, which may suggest some signal discounting or nonlinearity in updating behavior. Nevertheless, the results strongly support the conclusion that respondents engage in belief updating that is directionally consistent with the information presented, consistent with the expectations of Hypotheses 2a and 2b.

### 5.4 How do respondents learn, and are they Bayesian?

To better understand the mechanisms driving the observed belief updating behavior, Figure 11 displays the average weight that respondents assign to their prior beliefs ($\pi_p$) and to the informational signal ($\pi_s$), as well as the sum of these weights (Equation 7).

**Figure 11:** Aggregate updating weights




**Note:** Posterior medians with 66 pct. and 95 pct. credible intervals (n = 472). Sum = Signal + Prior

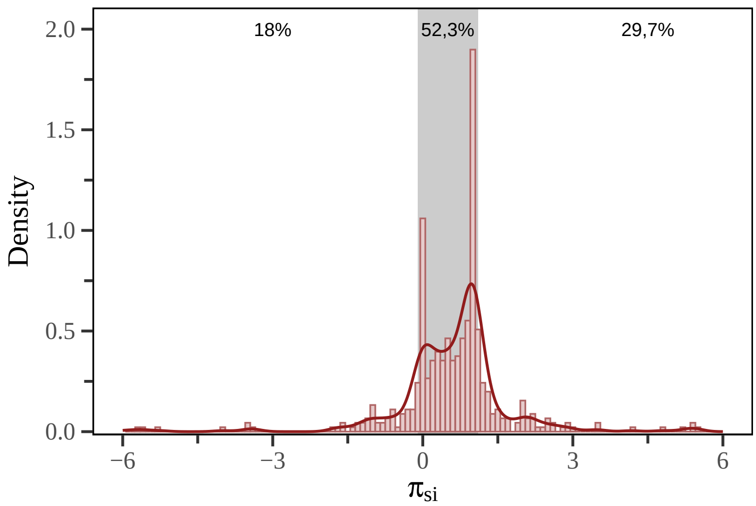
Consistent with the Bayesian framework, both $\pi_p$ and $\pi_s$ are credibly positive and non-zero, with $\pi_p$ estimated to be 0,43 [0,37; 0,50] and $\pi_s$ estimated to be 0,65 [0,51; 0,79]. Substantively, respondent's, on average, place approximately three-fifths weight on the signal and two-fifths on their prior when forming their posterior beliefs.

Substantively, this suggests that people tend to trust the informational treatment more than their initial beliefs – further reinforcing the idea that supply skepticism is a loosely held view, easily swayed by new evidence.

The sum of the two weights is estimated to be 1,08 [0,93; 1,23], which is broadly consistent with the Bayesian expectation that the weights on the prior and the signal should sum to one. However, the fact that the posterior median is slightly above one suggests a modest tendency toward over-updating on average. This may again reflect weakly held and uncertain priors – leading respondents to overweight new information relative to their priors. Alternatively, it may be driven by measurement error in the elicited priors or posteriors, which can artificially inflate the inferred signal weight. This occurs because noise in the reported prior makes the prior appear less informative than it truly is, prompting the estimation procedure to assign greater weight to the signal to reconcile the observed posterior. As will be shown shortly, however, the deviation seems to be mostly driven by a few extreme outliers.

In sum, while the credible interval includes 1, indicating that many respondents are consistent with rational Bayesian updating, the central tendency still points toward a systematic, albeit modest, deviation from purely Bayesian behavior.

**Figure 12:** Distribution of individual level signal weights




**Note:** Individual level signal weights in 0,1 bins. The shaded area denotes $0 \leq \pi \leq 1$. Extreme weights ($-6 < \pi < 6$) have been excluded from the plot for readability (n = 453)

Figure 12 disaggregates the belief updating pattern to the individual level by displaying the distribution of respondent-specific signal weights, calculated as $\pi_{s_i} = (B_i^1 - B_i^0)/(S_i - B_i^0)$ (Hjort et al., 2021). The histogram reveals substantial heterogeneity in how individuals integrate new information. A majority – 52 percent – have signal weights between 0 and 1, suggesting partial updating. Within this group, 5,5 pct. fully adopt the signal ($\pi_{s_i} = 1$), effectively discarding their prior, while 4,5 pct. place full weight on their prior ($\pi_{s_i} = 0$), entirely ignoring the signal.

Outside this range, 18 percent of respondents appear to update in the opposite direction of the signal ($\pi_{s_i} < 0$), and 29 percent exhibit over-updating ($\pi_{s_i} > 1$). These patterns underscore the heterogeneity in belief updating strategies at the individual level.

The relatively large share of respondents who discard their priors entirely may again reflect the complexity and lack of party-cues in the housing policy domain. Likewise, the relatively large proportion of over-updating could be driven by the flexibility in beliefs the housing policy domain induces. However, the similarly large proportion who ignore the signal suggests that some respondents possess strong priors or perceive the signal as very uncertain. The remaining share who update in the opposite direction may reflect inattentiveness, misinterpretation of the signal, or potential "Backlash" effects, where exposure to the signal strengthens contrary prior beliefs (Guess & Coppock, 2020). Another, more methodological, explanation is rounding in the belief measurements. Since beliefs are measured in 0,1 pct. increments, small updates may be obscured (e.g., 0,46 to 0,54 appears as no change) or inflated (e.g., 0,44 to 0,46 recorded as 0,1 shift). "This introduces noise into the individual-level weights and thus warrants caution in interpreting them; however, this noise is canceled out when calculating the aggregated weights (Figure 11) which are of primary interest.

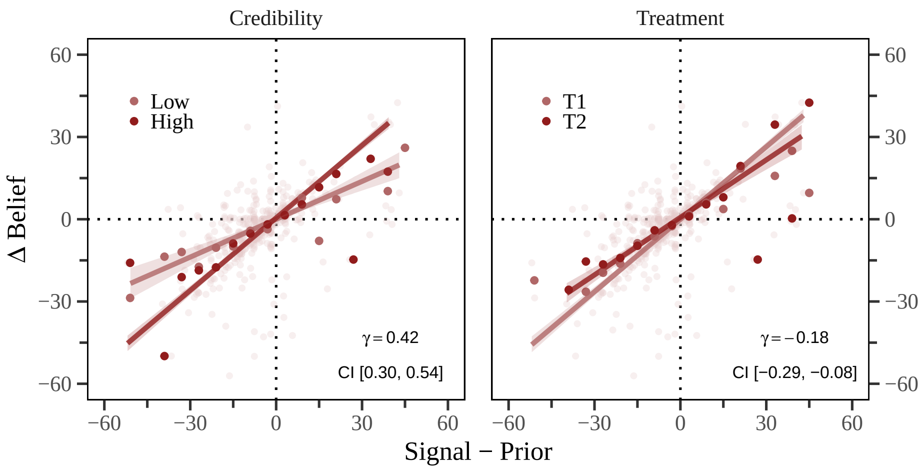
The distribution of signal weights in Figure 12 broadly resembles that reported by Vivalt & Coville (2023) for ordinary citizens but diverges notably from those observed among politicians and policy experts (Hjort et al., 2021; Vivalt & Coville, 2023), though in different policy contexts. Compared to their results – where most weights fall between ˘3 and 3 – the present distribution features a higher frequency of large weights in both directions, and includes values ranging from $-6$ to 6. Values beyond this range are omitted from the figure for visual clarity, and because such extreme outliers are rare and substantively difficult to interpret. These outliers likely reflect random measurement noise rather than systematic updating differences, and excluding them from the main analysis does not dramatically change the results. However, doing so brings the sum of the aggregate weights much closer to one, with the combined estimate of $\pi_p + \pi_s$ at 0,993 [0,86; 1,14] (see Figure A.4), suggesting that the modest over-updating observed earlier is almost entirely attributable to these extreme individual weights. In the first-difference model, the results remain substantively similar, though the intercept contracts to –1,92 [–3,16; –0,67] (see Table A.1), indicating that the extreme weights were primarily concentrated in T1. T2 continues to update by roughly twice as much as T1.

In summary, while respondents do not behave as perfect Bayesians, their updating is broadly consistent with the Bayesian framework. Most individuals place meaningful weight on both their prior beliefs and the informational signal, and aggregate patterns align with theoretical expectations. The modest deviations – such as over-updating and the presence of negative or extreme weights – can largely be explained by measurement noise, heterogeneity in prior certainty, or signal interpretation. Overall, the evidence suggests that respondents are responsive to information in a way that is directionally Bayesian, even if not normatively optimal in every instance.

### 5.5 What drives differences in learning behavior?

To investigate why some respondents trust the information more, Figure 13 displays heterogeneity in signal weights as a function of signal characteristics (i.e. credibility and signal dose). The figure displays predicted values estimated from Equation (8), alongside the raw and binned data. A steeper slope indicates that respondents place greater weight on the signal when updating their beliefs.

**Figure 13:** Signal weights by signal characteristics



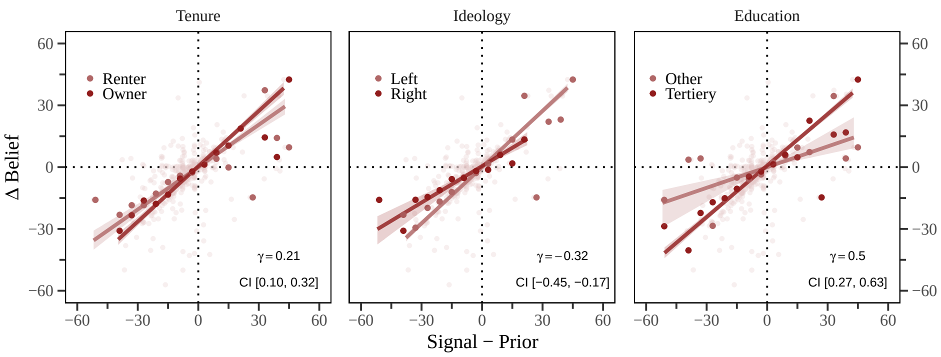

**Note:** Lines represent posterior predictions with ribbons indicating the 95 pct. credible interval. $\gamma$ denotes the difference in slopes between groups and CI denotes the 95 pct. credible interval. The light pink dots represent the raw scatter plot, and the darker dots correspond to the binned scatter plot based on twenty bins. (n varies between 472 and 454 across models)

First, the left panel reveals a substantial difference of 0,42 in signal weighting between respondents who found the signal credible and those who did not. This pattern aligns with the Bayesian updating framework and suggests that perceived credibility at least partially captures subjective signal uncertainty. Second, the relationship between the signal-prior gap ($S_i - B_i^0$) and belief updating ($B_i^1 - B_i^0$) appears approximately linear within both credibility groups, suggesting a consistent pattern of updating more when ones initial belief is further from the signal. Nonetheless, a few large outliers remain, primarily concentrated in the tails of the data. This outlier pattern also appears when plotting the aggregate slope (see Figure A.6), which shows that belief updating is mostly linear, but less so at the extremes of the informational gap. Substantively, this could suggest some degree of signal discounting, as respondents whose priors are far from the signal tend to update less. However, data is sparse in these tails, so this pattern should be interpreted with caution.

The right panel shows a more modest difference of –0,18, meaning that T1 weights the signal slightly more than T2 – again, with approximately linear updating within groups but with outliers. That T1 weights the signal more was contrary to structural expectation outlined in the methods section, and might explain why updating doesn't scale proportionally to signal size. Again, this could reflect some degree of signal discounting (Christensen, 2023); however, inspecting individual-level signal weights by treatment group (see Figure A.5) shows that respondents in T1 are slightly more prone to over-update than those in T2.

Figure 14 likewise investigates heterogeneity in signal weights, but now as a function of respondent demographics. Again, predicted values estimated from Equation (8) along with the raw and binned data are displayed.

**Figure 14:** Signal weights by demographics

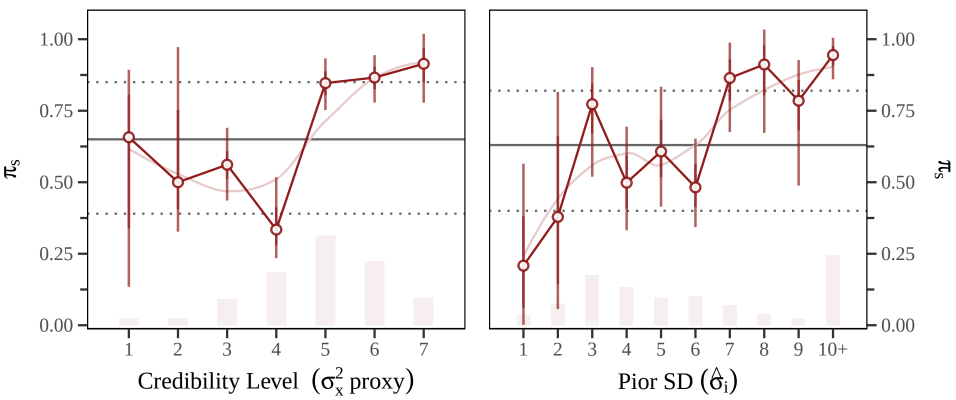



**Note:** Lines represent posterior predictions with ribbons indicating the 95 pct. credible interval. $\gamma$ denotes the difference in slopes between groups and CI denotes the 95 pct. credible interval. The light pink dots represent the raw scatter plot, and the darker dots correspond to the binned scatter plot based on twenty bins. (n varies between 469 and 384 across models).

As expected renters are slightly more receptive to the signal with an estimated signal weight difference of 0,21 [0,10; 0,32], right leaning respondents are less receptive with an estimated difference of –0,32 [–0,45; –0,17] and the highly educated are substantively more receptive to the signal with a difference of 0,5 [0,27; 0,63]. Apart from the outliers, which where also present in the Figure 13 most groups still update in a manner that is consistent with Bayesian updating. Substantively then, these results suggest, that while respondents are generally Bayesian in their behavior, their relative receptiveness to the signal is not a universal process, but one that is shaped by individuals backgrounds; either through weaker priors or more trust in the signal.[51] While these findings are interesting and in line with expectations, it is important to note, that they are exploratory and mostly suggestive, both because the demographic variables may not be exogenous, because interactions are more data hungry (Gelman et al., 2021), and because demographic groups are not balanced.

Figure 15 further examines the influence of prior uncertainty and perceived signal credibility on respondents' reliance on the informational signal. The figure displays marginal signal weights across perceived credibility and prior uncertainty.

**Figure 15:** Signal weights by prior uncertainty and signal credibility




**Note:** Posterior median with 66 pct. and 95 pct. credible intervals (left panel n = 454 and right panel n = 383). Light red line represents Lowess fits. The dark line denotes the posterior mean of the population level $\pi_s$ posterior, and the dotted grey lines denote the population level 95 pct. credible interval's upper and lower bound respectively. The light pink bars represent the marginal distribution across groups.

The left panel shows a somewhat noisy but overall positive relationship between perceived credibility and signal weighting. Among respondents who view the signal as highly credible, signal weights increase substantially. In contrast, those reporting very low credibility or indifference assign markedly lower weights to the signal. Notably, signal weights are lowest among respondents who express neutrality regarding the signal's credibility; a pattern that is both surprising and difficult to interpret. One possible explanation is that these respondents lack sufficient motivation or cognitive engagement to process the signal meaningfully. It is also important to note that few respondents rated the signal as very low in credibility, and thus estimates for these groups are imprecise, as reflected in the wide credible intervals. Consequently, the apparent U-shaped pattern should be interpreted with caution. Nonetheless, it is reassuring for the Bayesian framework that respondents who perceive the information as highly credible tend to update

[51] Most of these specifications are robust to using a normal likelihood instead of the Student's t likelihood. Across all models the Student's t likelihood provides a substantially better fit, both in sample ($R^2$) and out of sample (Expected log pointwise predictive density (Vehtari et al., 2017))

more in line with it than those who do not.

The right panel displays a clearer and less noisy positive relationship between prior uncertainty and signal weighting. Respondents with smaller prior standard deviations, indicating high confidence in their initial beliefs, tend to place relatively little weight on the signal. In contrast, those with higher prior standard deviations – reflecting greater uncertainty – exhibit significantly higher signal weights. While some subgroup estimates remain imprecise due to small sample sizes, the overall trend is consistent: individuals who are more uncertain about their priors are more receptive to new information. It is important to reiterate, however, that the measure of prior uncertainty is itself somewhat noisy, and should be interpreted accordingly. Nevertheless, the pattern aligns well with Bayesian expectations, where greater uncertainty in prior beliefs leads to greater reliance on new evidence.

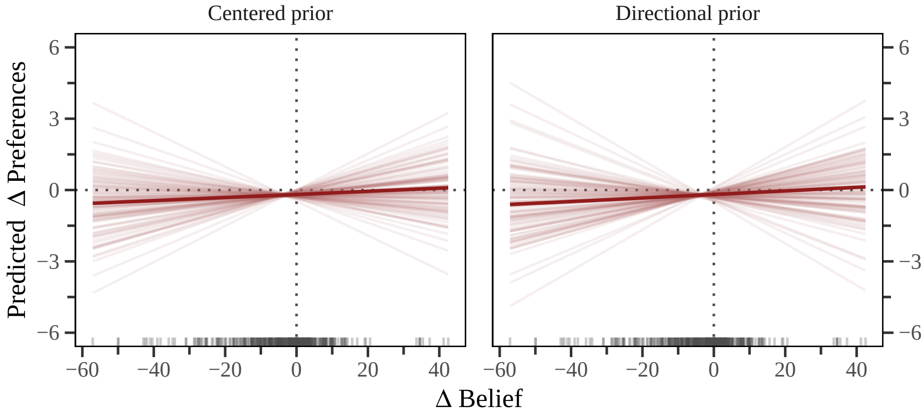
In sum, differences in learning behavior are primarily driven by variation in signal characteristics and individual-level traits. Respondents who perceive the signal as more credible assign it substantially greater weight, consistent with the Bayesian notion that individuals adjust more in response to information they deem reliable. Similarly, those with greater prior uncertainty – i.e., less confidence in their initial beliefs – demonstrate higher receptiveness to the signal, also in line with Bayesian predictions. Beyond these informational factors, demographic characteristics further explain heterogeneity in updating: renters and the highly educated are more responsive to the signal, while right leaning respondents exhibit greater skepticism. Importantly, while most respondents appear to update in a manner broadly consistent with Bayesian learning, the strength of their response varies systematically with both subjective assessments of the information and individual background, suggesting that learning is shaped not by universal cognitive rules alone, but by contextual and dispositional factors as well.

## 5.6 Does learning affect policy preferences?

At this stage, the evidence provides strong empirical support for the claim that respondents update their beliefs about the price effects of market-rate housing. However, it remains an open question whether such belief updating also translates into changes in downstream housing policy preferences at the macro-level, as posited by Hypothesis 3a. Figure 16 addresses this by illustrating predicted changes in policy preferences as a function of belief updating, based on estimates from Equation (10).

The left panel presents estimates from a model employing an uninformative prior, centered on zero. The results indicate that individuals who revise their beliefs in the direction of more negative price effects (i.e., stronger belief that market-rate housing reduces prices) tend to exhibit a slight decrease in anti-development policy preferences. This pattern is consistent with the theoretical expectation outlined in Hypothesis 3a. However, the estimated effect is small, approximately 0,01, and highly uncertain, with

**Figure 16:** Effect of belief updating on macro-level policy preferences




**Note:** 100 randomly drawn linear posterior predictions (transparent lines) and linear predictions based on posterior average (opaque line) (n=458). The x-axis takes the same range as observed belief updating and the grey bars represent the distribution of observations along the axis

a 95 pct. credible interval ranging from –0,05 to 0,06. This implies that the largest observed belief update (–57,1 pct.) is associated with a predicted shift in anti-development preferences of approximately −0,59 Likert scale points. However, the associated 95 percent credible interval [–3,38; 2,23] is wide and includes both substantial decreases and increases in anti-development preferences. This substantial uncertainty highlights the imprecision of the estimate and suggests that even large belief updates may translate into highly variable, and potentially minimal, changes in stated policy preferences. As such, while suggestive, the evidence offers only weak support for a systematic link between belief updating and shifts in policy preferences under this specification.

To help alleviate potential issues of detecting a small effect with limited data, the right panel presents estimates from a model that formally incorporates prior information from Elmendorf et al. (2024a), who report an effect size of 0,15 standard deviations for a comparable informational treatment.[52] With this informative prior, the estimated effect of belief updating on policy preferences remains virtually unchanged; however, the lower bound of the 95 percent credible interval increases to –0,04, thereby marginally reducing the uncertainty surrounding the estimate. While the substantive conclusion is largely unaffected, the inclusion of external evidence lends modest additional precision to the model's estimates but support for Hypothesis 3a is still weak.

Estimating a pre-post difference in anti-development preferences using Equation (5) (and imposing the appropriate assumptions discussed in section 4.3.2) reveals a less modest and reliably estimated decrease of –0,21 Likert-points [–0,27; –0,14] (Estimate is stable across all specifications. See Table A.2). However, this shift cannot be fully

[52]In practice, this is implemented by centering the prior on an effect size of 0,15 SD instead of zero.

attributed to changes in beliefs about price effects. The treatment also made other aspects salient (i.e. moving chains benefiting lower-income groups) introducing a degree of compound treatment or cross-learning into this effect-estimate.[53] This effect translates to Cohen's $d = -0,13$ which is very similar to the effect on preferences retrieved by Elmendorf et al. (2024a) using a similar informational treatment (and the frequentist pre-post model), which is reassuring. Contrary to the pre-post model on beliefs, this model yields an estimated ICC of 0,87, indicating that while the treatment accounts for some variation in policy preferences, the majority of the variance is attributable to stable individual-level characteristics. In less technical terms, this underscores that policy preferences appear to be substantially more stable and less responsive to new information than beliefs about price effects.

According to the data at hand, belief updating toward negative price effects leading to modest reduction anti-development preferences appears to be the most probable empirical pattern. However, the model and data also allocate substantial probability to a reversed or null relationship, meaning that Hypothesis 3a is not conclusively supported. However, there is stronger evidence that the informational treatment exerts a small compound effect on preferences, operating through multiple channels beyond price beliefs alone, which is still highly policy relevant.

From a theoretical perspective, the small and uncertain effect once again underscores a key insight from previous behavioral research: while beliefs may be relatively elastic, preferences are often more rigid (DellaVigna & Gentzkow, 2010; Haaland et al., 2023). Beyond this general rigidity, however, the present data also offer insight into why preferences may remain relatively fixed in this specific context. As has now been well documented, evidence showing that supply measures reduce prices does shift respondents' point beliefs. Yet, as illustrated in Figure 7, the uncertainty surrounding these updated beliefs remains relatively high and respondents do not fully rule out the possibility of price increases. Accordingly, respondents may still feel too uncertain about the effectiveness of supply measures to adjust their preferences accordingly. If this is the case, it suggests that additional evidence and further reductions in uncertainty could eventually lead to more pronounced changes in policy preferences.[54]

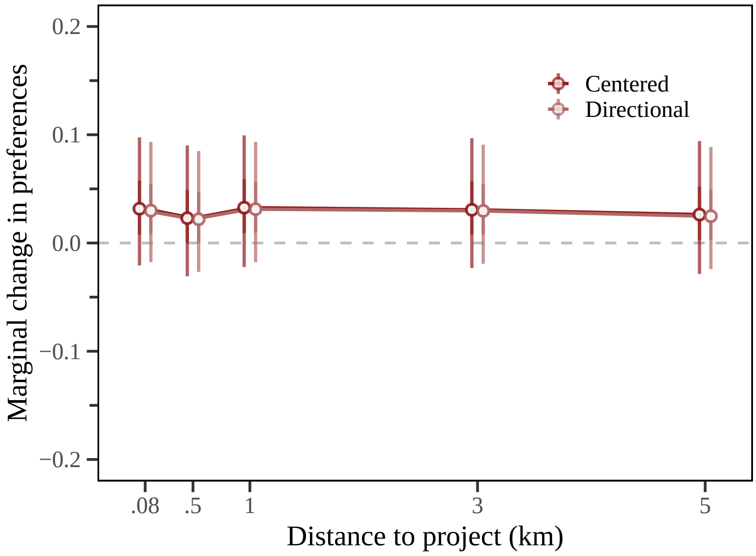
While the theory section posited that preference shifts would be most pronounced at the macro level, it also anticipated some degree of change in preferences toward local projects located further from the respondents' place of residence (H3b). Figure 17 tests this expectation by plotting the marginal effects of belief updating on support for

[53] At first glance, this may appear to violate the exclusion restriction underlying the IV estimator. However, it is important to note that the additional information is held constant across treatment groups. Thus, the instrument (treatment) does not differentially affect the outcome except through its effect on beliefs and the exclusion criterion still holds

[54] See Appendix A.11 for a descriptive test of preference updating across respondents' posterior beliefs about price increases.

hypothetical housing projects, conditional on their distance from the respondent's place of residence.

**Figure 17:** Link between beliefs updating and change in support for local project across distances




**Note:** Posterior median with 66 pct. and 95 pct. credible intervals (n = 464). Marginal effects are calculated by summing posterior draws of $\delta$ and $\gamma$ from Equation (10)

As above, the figure presents results from two models: one using an uninformative prior and one incorporating a directional prior based on findings from Elmendorf et al. (2024a). First, there is no visually discernible pattern of distance decay in the effect of belief updating on project support, indicating that the effect remains relatively stable across all distances. The estimated interaction terms are centered around 0 or –0.01, with wide credible intervals, reinforcing this visual impression. Second, this pattern holds in the model with the directional prior, but marginal effects are estimated with sligthly greater precision. Third, across all distances, the estimated effects are generally slightly larger and less likely to be null or reversed than those observed at the macro level, with the reference category (80m) effect estimated to be 0,03 [–0,02; 0,10]. This remains a modest effect, with the largest observed negative belief change translating to a predicted reduction in opposition to the project of –1,83 [–5,3; 1,08]. Nonetheless, it further strengthens the case for a possible direct effect of belief updating on policy preferences.

In substantive terms, belief updating appears to influence support for a hypothetical housing project regardless of the project's proximity to the respondent's residence. Accordingly, there is no empirical support for Hypothesis 3b. One concern with this result is that the distance manipulation may be too abstract, especially given the hypothetical and loosely defined nature of the housing project. In the absence of concrete costs related to local projects or specific contextual cues, respondents may simply be indifferent to the distance framing, leading to muted or inconsistent distance-based effects. On a similar note, it is quite puzzling that the changes in preferences are larger at the local level than the macro level. The theoretical framework expected that preferences toward abstract, nation-wide policy – being more removed from localized costs – would be more elastic. The fact that the reverse pattern emerges may suggest that respondents find local development, even when hypothetical or distant, more tangible or cognitively engaging. It may also indicate that framing housing development in terms of price reductions resonates more when linked to concrete projects, as opposed to abstract policy. These possible explanations are only tentative, however, and the observed difference between local and macro-level effects is negligible. It may well reflect random noise rather than a meaningful pattern, and should therefore not be over-interpreted either way.

It should also be noted that only the link between belief updating and preferences appears stronger at the local level. The compound (pre-post) treatment effect is estimated to be less than half the size of that at the macro level, at –0,07 [–0,14; –0,01].[55] As such, it is the belief–preference link – not the compound effect of the treatment – that is stronger locally, which does lend some support to the intuitive idea that proximity does make preferences more rigid, as suggested by H3b.

In summary, there is mixed but suggestive evidence that belief updating regarding the price effects of market-rate housing can influence housing policy preferences; both at the macro level and in relation to support for local housing projects. While the direction of effects is broadly consistent with theoretical expectations, the estimated magnitudes are modest and accompanied by substantial uncertainty. Particularly at the macro level, the evidence does not permit strong inferences about the presence or absence of an effect. The local-level findings offer somewhat stronger indications of a link between belief change and project support, but again, the size and credibility intervals suggest caution. The treatment itself appears to have influenced pre-post changes in preferences in the expected direction, and this effect is more precisely estimated. However, it may also reflect cross-learning, as the intervention likely activated considerations beyond price beliefs alone. Overall, the evidence suggests a potential link between updated beliefs and policy preferences, but more data would be needed to more confidently characterize the nature and magnitude of this relationship.

[55] This estimate can be replicated by running `appendix table pre-post.R` in the replication material

# 6 Discussion

## 6.1 Methodological limitations

Apart from limitations already mentioned in the methods and results section, the study has other possible flaws worth bearing in mind when interpreting the result.

First, while considerable effort has been devoted to the operationalization of supply-skeptical beliefs, the second outcome measure, policy preferences, is somewhat more crude. Although the measure aligns with previous research on local opposition (Larsen & Nyholt, 2024a) and clearly captures relevant dimensions of the concept, it may have been more appropriate to construct a more comprehensive index in the present study. One reason is that "housing policy preferences" plausibly encompass a broader set of attitudes than simply support for or opposition to additional market-rate housing. For example, belief updating might also reduce preferences for inefficient policies such as rent control, which are not captured by the current measure. In addition, people may differentiate between types of market-rate housing or where it is built (Elmendorf et al., 2024a) – granularity that is lost in a single-item measure. Second, a composite index would likely yield a more precise and reliable measure (Hansen et al., 2021; Haaland et al., 2023) which could help with identifying small effects and avoiding some of the identification and interpretation problems associated with Likert-scales (Bond & Lang, 2019). Again, while the single-item measure captures the core aspects of the relevant preferences, a composite index would have been a useful addition – at least for robustness.

Second, and as already discussed briefly, the use of an active control design has benefits for the causal identification of the link between beliefs and preferences, but it also has some caveats. One such caveat was noted in the methods section, where I argued on the basis of Vilfort & Zhang (2024), that active control designs may be less policy-relevant because they address how different types of information affect preferences, rather than the overall effect of receiving information itself. However, since Elmendorf et al. (2024a) have already demonstrated treatment effects using passive control designs, the logical next step is to also explore the underlying mechanisms driving these effects. This study contributes by advancing that understanding, specifically by isolating the role of belief updating in shaping policy preferences. Either way, pre-post comparisons can, under the assumptions discussed in section 4.3.2, approximate the effects observed in passive control designs, allowing the thesis to partially circumvent this limitation. Another caveat is that I had to artificially manipulate the results from (Mense, 2025) in order to construct the stronger treatment. This effectively involves misleading respondents, something that would not have been necessary in a passive control design. However, given the relatively modest nature of the manipulation and the inclusion of a debriefing that informs respondents of the true effect size, I still consider the design ethically defensible.

Third, a possible attenuating factor is the risk of floor effects. In this study, average pre-treatment preferences are centered around 3,4 on a 7-point Likert scale, where higher values reflect stronger anti-development sentiment (as per section 4.1.2). This suggests that most respondents are either mildly pro-development or indifferent, leaving more scope for increases in opposition than for decreases. As a result, floor effects may constrain the observable impact of belief updating on stated preferences, particularly among already pro-development respondents. This dynamic could attenuate estimated treatment effects and contribute to conservative estimates of preference change; especially given that policy preferences may generally be relatively inelastic (Haaland et al., 2023). That said, floor effects do not appear to be a major issue in this study; however, it may simply be more difficult to shift respondents from indifference to strong support than from opposition to neutrality.

Since some of the above concerns are presumed to attenuate the possible link between belief updating and policy preferences, it is worth returning shortly to the issue of power. While the study was adequately powered to detect belief change, detecting modest downstream effects on preferences likely requires a larger sample. Again, while Bayesian statistics does not rely on binary significance thresholds, a larger sample would have yielded more precise estimates and allowed for greater certainty in evaluating the downstream effects on preferences. Again, however, a larger sample would have required either a longer timeframe or greater monetary incentives – neither of which was feasible. An alternative would have been to reduce the complexity of the survey to improve completion rates. However, doing so would have meant sacrificing many of the novel components and insights built into the design.

A final minor methodological concern in this study relates to rounding error, which was briefly discussed in connection with the signal-weights, where rounding occurred by design due to the belief elicitation scale. A separate concern is the possibility that respondents themselves rounded their beliefs when reporting them. Recent work from Rosokha & Bland (2024) shows that such rounding behavior constitutes non-classical measurement error and may bias estimates of Bayesian updating. Two points are worth noting in this regard. First, the prevalence of rounding behavior in this sample appears comparatively modest (see Figure A.8, bottom panels, and Rosokha & Bland, p. 6) although present. Second, rounding is particularly problematic for studies attempting to precisely quantify deviations from Bayesian behavior (Rosokha & Bland, 2024); an aim this study does not pursue. Nevertheless, the potential for rounding-related bias should be acknowledged as a possible limitation, and future work might try to explicitly model this measurement-error.

## 6.2 Theoretical limitations

Other than the methodological limitations, there are also some theoretical considerations worth reflecting on in light of the empirical results.

The first concerns the interpretation of the uncertainty measure tied to supply-skeptical beliefs. Throughout the thesis, I have argued that this uncertainty reflects the complexity of the policy domain and, consequently, that it captures respondents' awareness of their own limited knowledge on the topic. A valid concern, however, is that the measure may instead – or also – capture ambiguity in the question itself. Respondents might feel confident in their beliefs under specific assumptions (e.g., short-term vs. long-term effects, with or without inflation correction, or assuming ceteris paribus conditions), but find it difficult to respond precisely because the question does not specify these conditions. In this case, reported uncertainty may not indicate a lack of belief strength, but rather the difficulty of mapping a conditional belief onto an unconditional answer. Judging by the open-ended feedback on the survey, some respondents did indeed find the question about price changes ambiguous due to a lack of clearly specified conditions. However, only a few respondents expressed this concern, and it would be somewhat surprising – even in this relatively highly educated sample – if many were so well-versed in economics that they consciously differentiated between such conditions; especially given how much ordinary citizens' reasoning deviates from economists' reasoning (Caplan, 2001, 2002). This suggests that while question ambiguity may account for some of the observed uncertainty, it is unlikely to fully explain it. Rather, the uncertainty measure likely captures a genuine lack of conviction or knowledge about the price effects of new housing supply, which supports its interpretation as a meaningful indicator of belief strength. Empirically, however, the two are observationally equivalent, and the thesis does not allow for separating them.

In addition to the above, it is worth pointing out that the measurement model based on the Manski-questions explicitly assumes that beliefs about price effects follow a normal distribution. While this assumption is practical due to its simplicity and tractability in estimation (McElreath, 2020), it lacks the flexibility of alternative approaches such as those employed by Leemann et al. (2021) and Stoetzer et al. (2024), which allow for capturing skewed distributions. The inability to capture asymmetry could be problematic if respondents systematically expect more extreme changes in one direction than the other – for instance, anticipating large price increases but relatively small decreases. This could lead to biased estimates of belief precision if the model forces a symmetric representation of what is in fact a skewed belief. While moving beyond the normality assumption was infeasible within the scope of this thesis, future work could explore more flexible elicitation and modeling techniques to better capture the distributional shape of supply skeptic beliefs.

A second concern is what the observed belief updating actually represents. While the theoretical framework of the thesis interprets this as evidence of Bayesian behavior, it is also possible that some respondents simply anchored their posterior beliefs to the signal received in the informational treatment (Haaland et al., 2023). The 5,5 pct. of respondents who appear to completely abandon their prior (illustrated in Figure 12 and further supported by the distribution of raw answers in the upper panels of Figure A.8) may indicate some anchoring. At the same time, an almost equal share (4,5 pct) of respondents do not revise their beliefs at all, negating this concern slightly. Moreover, given the observed uncertainty and weakness of priors, large belief shifts may not necessarily reflect anchoring, but rather a reasonable updating process in response to credible information in a complex policy domain. Again, however, these explanations are observationally equivalent and the thesis cannot directly separate the two.

While not a direct concern, a third blind spot of the thesis is the temporal stability of the belief and preference revisions. In general, it is recommended to conduct multiple follow-up waves to assess how durable such effects are over time (Haaland et al., 2023), particularly since treatment effects may fade, as seen in studies such as (Larsen & Olsen, 2020). Due to the time constraints of the project, this was not practically feasible. Nonetheless, the results from a single post-treatment measurement still offer meaningful insights, but understanding the longevity of belief and preference change remains highly policy-relevant and should be a priority for future research.

A final overlooked insight is that while the thesis focuses on belief updating about the price effects of market-rate housing, it does not speak directly to what other factors shape housing policy preferences. This is not a flaw per se, but it does leave open important questions for both theory and policy. For example, stable anti-development preferences may reflect broader ideological commitments – such as support for a larger welfare state and thus more government intervention in the housing market, or environmental concerns related to development. Moreover, many citizens may still favor subsidized or social housing over supply measures, and thus may not be swayed by information highlighting the effectiveness of market-rate supply alone – especially because the informational treatment does not mention the potential inefficiencies of subsidized housing or rent control, nor the fact that these policy approaches often represent trade-offs in terms of land use or public funding. As such, the treatment may have failed to induce larger shifts in preferences not because respondents reject supply measures outright, but because it did not frame them as alternatives to the more interventionist policies many tend to favor (Müller & Gsottbauer, 2022; Elmendorf et al., 2024b).[56] Understanding these additional drivers would offer a more complete picture of how policymakers can design information interventions to gather public support for supply-side

[56]There is some emerging evidence demonstrating that perceived trade-offs can affect willingness to support new housing initiatives (Kettel & Larsen, 2024).

initiatives – but would go beyond the realistic scope of the present thesis.

### 6.3 Theoretical contributions

This thesis makes several contributions to the literature on housing politics as well as belief formation and learning.

First, it extends the emerging literature on supply skepticism to a new political and institutional context. Most existing work has focused on the United States, where market-oriented welfare institutions and decentralized land-use governance shape both public attitudes and policy dynamics. By focusing on Denmark – a Scandinavian welfare regime with a more centralized planning framework – this study provides valuable comparative insights and demonstrates that supply skepticism is not exclusive to the US. In doing so, it highlights the need for a broader, more internationally oriented research agenda on housing attitudes and public opposition to development, especially because the housing crisis is not confined to the US.

Second, the thesis contributes a novel operationalization of supply skepticism by treating it as a distribution rather than a point estimate. This allows for the measurement of subjective uncertainty about price effects, offering empirical traction on whether these beliefs are firmly held or reflect non-attitudes, as proposed by Nall et al. (2024). Once again, the findings suggest that supply-skeptical beliefs in Denmark are prevalent but characterized by considerable uncertainty, reinforcing the idea that many individuals are aware of their limited understanding of housing market dynamics. This opens up space for belief updating and explains why informational interventions may have more traction in housing politics than other more entrenched policy domains (Haaland et al., 2023; Elmendorf et al., 2024a).

Third, the study corroborates and extends the findings of Elmendorf et al. (2024a), showing that citizens are willing to update their beliefs – and maybe, to a lesser extent, their preferences – particularly when presented with information that includes distributional aspects. It goes further by formally modeling belief updating and tracing its downstream effects on policy preferences, thus demonstrating that belief updating occurs because supply skepticism appears to be a relatively weakly held and uncertain belief. At the same time, the weak translation into preference change seems to reflect lingering uncertainty: while posterior beliefs move in the direction of the signal, respondents still assign non-negligible probability to supply measures increasing prices. Thus, the thesis shifts the focus from *whether* skeptics revise their beliefs to *why* and *how* they do so – and why this learning only partially maps onto preferences. In doing so, it adds theoretical granularity to our understanding of the belief–preference link, reinforces the broader argument that supply skepticism is not immutable, and advances our understanding of how to design effective informational interventions on the subject.

Finally, the findings speak to broader theories of policy learning and political persuasion. The evidence suggests that voters behave in a somewhat Bayesian manner; updating their beliefs in proportion to the strength and credibility of the information they receive. At the same time, the imperfect transmission of beliefs into preferences aligns with prior work suggesting that belief change does not always translate one-to-one into attitudinal change across domains (DellaVigna & Gentzkow, 2010; Coppock & Green, 2022; Haaland et al., 2023). This underscores the importance of continued investigation into the relationship between belief updating and preference formation. In particular, examining how posterior uncertainty mediates this transmission appears to be a promising avenue for future research.

### 6.4 Policy implications

Although Denmark's planning system is decentralized (Section 2.2), the national government retains meaningful discretion over land-use policy – unlike in the United States, where such authority is largely delegated to state and local governments (OECD, 2017). This institutional difference has important implications for how public opinion translates into policy outcomes. As Nall et al. (2024) argue, in the U.S., macro-level preference shifts are unlikely to influence housing policy unless accompanied by broader centralizing reforms. In contrast, Denmark already has a somewhat more centralized structure in place that allows national preferences to play a more direct role in shaping land-use outcomes, making macro-level opinion change more immediately policy-relevant.

While Danish municipalities still play a key role in planning, and local councils are important decision-makers (Larsen & Kettel, 2023), the broader institutional structure channels political participation through mechanisms that align more closely with higher-level preferences. For example, compared to the U.S., Danish citizens have fewer formal opportunities to block individual projects through local referendums (Hvidkjær & Larsen, 2025, p. 148). Instead, input is typically expressed through representative political processes such as municipal elections – which, while local in scope, tend to reflect broader ideological alignments, not opposition to specific projects. This institutional design should limit the capacity for localized veto points and increases the responsiveness of housing policy to shifts in public sentiment at the macro level. As such, both national and local pathways exist for public opinion to influence outcomes, but the Danish system is distinct in allowing centralized preferences to meaningfully shape the policy trajectory – without requiring the kinds of structural reforms that would be necessary in more fragmented systems like that of the United States. As such higher-level preference changes is highly policy relevant in the danish context.

The findings of this thesis speak to this by suggesting that is possible to alleviate supply skepticism through informational interventions, and that such belief change may have downstream effects on housing policy preferences – albeit modest and uncertain. While the estimated effects are small and accompanied by considerable uncertainty, they point in a theoretically consistent direction and replicate across multiple outcomes (i.e. local and national) and modeling strategies. This indicates that public beliefs about the effects of new housing supply are not fixed, and that targeted communication can shift them. Given the institutional structure of Danish housing politics, even small shifts in public preferences, both at the macro level and local level, may carry meaningful policy implications. At the same time, the limited magnitude and uncertainty of observed effects underscores the need for realistic expectations: changing price beliefs alone is unlikely to completely eliminate supply issues, and as the compound treatment effect suggests, other concerns, such as benefits to lower-income groups, also matter to citizens. However, combining insights from the compound treatment effect, the belief–preference update link, and prior findings by Elmendorf et al. (2024a) suggests that informational interventions should be viewed as a useful part of the policy toolkit for addressing supply constraints in the Danish housing market. Despite the modest and uncertain effects, the low cost and scalability of informational interventions suggest that even minimal impact may translate into considerable returns (Benartzi et al., 2017; DellaVigna & Linos, 2022).

Furthermore, the observation that the magnitude of belief updating may only translate weakly to preference updating carries important implications for the design of such informational interventions. Specifically, it suggests that simply correcting misconceptions about the effects of housing supply may not be sufficient to meaningfully shift policy preferences. Instead, interventions may need to be more comprehensive – either by combining factual information with value framing (Elmendorf et al., 2024a), by addressing multiple dimensions of concern such as not only price effects but also fairness and distributional impacts (e.g., moving chains), or by further reducing uncertainty around the price effects themselves. This insight underscores the importance of tailoring communication strategies not only to correct beliefs but also to resonate with the broader set of values and motivations that shape citizens' preferences.

# 7 Conclusion

This thesis set out to investigate the cognitive foundations of public opposition to market-rate housing development in Denmark, with a particular focus on the phenomenon of supply skepticism. Drawing on a randomized controlled trial, the study examined whether Danish citizens hold skeptical beliefs about the price effects of new housing supply, whether these beliefs are weakly held and thus possibly amenable to change, and whether such belief updating translates into changes in housing policy preferences.

The findings provide clear evidence that supply skepticism is indeed present in the Danish context, but that it is mild and characterized by high uncertainty. This supports the notion that such beliefs are more akin to non-attitudes than deeply held ideological commitments. Respondents' beliefs about the price effects of market-rate housing were, on average, slightly skeptical, but the associated uncertainty was substantial; suggesting that many citizens are aware of their limited knowledge in this complex policy domain.

When exposed to evidence about the price-lowering effects of new housing supply, respondents updated their beliefs in the direction of the evidence. This learning behavior was broadly consistent with Bayesian updating, with stronger signals inducing larger belief changes. Moreover, the weight respondents placed on the signal was moderated by prior uncertainty and perceived credibility, as well as by demographic factors such as education, ideology, and tenure status. These findings reinforce the idea that belief updating is not a uniform process across individuals, but one shaped by both informational and dispositional factors.

However, the link between belief updating and policy preferences was found to be weak. While the informational treatment did induce a modest shift in macro-level housing preferences, this effect appears to be driven by a compound treatment effect rather than belief updating alone. The estimated causal effect of belief change on preference change was small and uncertain, and no clear pattern of distance decay was observed in support for local housing projects. This suggests that while beliefs about price effects can be shifted, preferences are more stable and likely shaped by additional considerations beyond affordability – such as fairness, distributional concerns, or symbolic politics.

In sum, the thesis contributes to the emerging literature on housing politics by introducing a novel measure of supply skepticism, modeling belief updating using a Bayesian framework, and tracing the downstream effects of learning on policy preferences. The findings underscore both the promise and the limits of informational interventions: while beliefs can be shifted, translating these changes into broader attitudinal or behavioral shifts remains a significant challenge. Nonetheless, given the low cost and scalability of such interventions, they may still represent a valuable tool in the broader effort to address housing supply constraints in Denmark and beyond.

## References

Aarøe, L. and Petersen, M. B. (2014). Crowding Out Culture: Scandinavians and Americans Agree on Social Welfare in the Face of Deservingness Cues. *The Journal of Politics*, 76(3):684–697.

Allison, P. D. (1990). Change Scores as Dependent Variables in Regression Analysis. *Sociological Methodology*, 20:93–114.

Altinget (2025). Sådan står partier, politikere og vælgere: Her er tallene, der fortæller alt om dansk politik lige nu - Altinget.dk.

Angrist, J. D. and Imbens, G. W. (1995). Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity. *Journal of the American Statistical Association*, 90(430):431–442.

Aronow, P. M., Baron, J., and Pinson, L. (2019). A Note on Dropping Experimental Subjects who Fail a Manipulation Check. *Political Analysis*, 27(4):572–589.

Banki, D., Simonsohn, U., Walatka, R., and Wu, G. (2025). Decisions under Risk Are Decisions under Complexity: Comment.

Barabas, J., Jerit, J., Pollock, W., and Rainey, C. (2014). The Question(s) of Political Knowledge. *American Political Science Review*, 108(4):840–855.

Baranzini, A. and Carattini, S. (2017). Effectiveness, earmarking and labeling: testing the acceptability of carbon taxes with survey data. *Environmental Economics and Policy Studies*, 19(1):197–227.

Bartels, L. M. (2002). Beyond the Running Tally: Partisan Bias in Political Perceptions. *Political Behavior*, 24(2):117–150.

Battaglia, M. P., Hoaglin, D. C., and Frankel, M. R. (2009). Practical Considerations in Raking Survey Data. *Survey Practice*, 2(5).

Been, V., Ellen, I. G., and O'Regan, K. (2019). Supply Skepticism: Housing Supply and Affordability. *Housing Policy Debate*, 29(1):25–40.

Been, V., Ellen, I. G., and O'Regan, K. (2024). Supply Skepticism Revisited. *Housing Policy Debate*, pages 1–18.

Bell, A. and Jones, K. (2015). Explaining Fixed Effects: Random Effects Modeling of Time-Series Cross-Sectional and Panel Data. *Political Science Research and Methods*, 3(1):133–153.

Benartzi, S., Beshears, J., Milkman, K. L., Sunstein, C. R., Thaler, R. H., Shankar, M., Tucker-Ray, W., Congdon, W. J., and Galing, S. (2017). Should Governments Invest More in Nudging? *Psychological Science*, 28(8):1041–1055.

Benjamin, D. J. (2019). Errors in probabilistic reasoning and judgment biases. In *Handbook of behavioral economics - Foundations and applications 2, Vol. 2*, pages 69–186. North-Holland/Elsevier, Amsterdam, Netherlands.

Berinsky, A. J., Huber, G. A., and Lenz, G. S. (2012). Evaluating Online Labor Markets for Experimental Research: Amazon.com's Mechanical Turk. *Political Analysis*, 20(3):351–368.

Berinsky, A. J., Margolis, M. F., and Sances, M. W. (2014). Separating the Shirkers from the Workers? Making Sure Respondents Pay Attention on Self-Administered Surveys. *American Journal of Political Science*, 58(3):739–753.

Berinsky, A. J., Margolis, M. F., Sances, M. W., and Warshaw, C. (2021). Using screeners to measure respondent attention on self-administered surveys: Which items and how many? *Political Science Research and Methods*, 9(2):430–437.

Bertaud, A. (2018). *Order without Design: How Markets Shape Cities*. The MIT Press.

Bisgaard, M., Dinesen, P. T., and Sønderskov, K. M. (2016). Reconsidering the Neighborhood Effect: Does Exposure to Residential Unemployment Influence Voters' Perceptions of the National Economy? *The Journal of Politics*, 78(3):719–732.

Blackwell, M., Brown, J. R., Hill, S., Imai, K., and Yamamoto, T. (2025). Priming Bias Versus Post-Treatment Bias in Experimental Designs. *Political Analysis*, pages 1–17.

Boliga (2020). Historisk lavt boligudbud: Mæglerne mangler varer på hylderne.

Bond, T. N. and Lang, K. (2019). The Sad Truth about Happiness Scales. *Journal of Political Economy*, 127(4):1629–1640.

Bottan, N. L. and Perez-Truglia, R. (2022). Choosing Your Pond: Location Choices and Relative Income. *The Review of Economics and Statistics*, 104(5):1010–1027.

Bratu, C., Harjunen, O., and Saarimaa, T. (2023). JUE Insight: City-wide effects of new housing supply: Evidence from moving chains. *Journal of Urban Economics*, 133:103528.

Broockman, D., Elmendorf, C. S., and Kalla, J. (2024). The Symbolic Politics of Housing.

Broockman, D. E. and Kalla, J. L. (2023). When and Why Are Campaigns' Persuasive Effects Small? Evidence from the 2020 U.S. Presidential Election. *American Journal of Political Science*, 67(4):833–849.

Brouwer, N. R. and Trounstine, J. (2024). NIMBYs, YIMBYs, and the Politics of Land Use in American Cities. *Annual Review of Political Science*, 27(Volume 27, 2024):165–184.

Bruhn, M. and McKenzie, D. (2009). In Pursuit of Balance: Randomization in Practice in Development Field Experiments. *American Economic Journal: Applied Economics*, 1(4):200–232.

Bullock, J. G. (2009). Partisan Bias and the Bayesian Ideal in the Study of Public Opinion. *The Journal of Politics*, 71(3):1109–1124.

Bürkner, P.-C. (2017). brms: An R Package for Bayesian Multilevel Models Using Stan. *Journal of Statistical Software*, 80:1–28.

Caplan, B. (2001). What Makes People Think Like Economists? Evidence on Economic Cognition from the "Survey of Americans and Economists on the Economy". *The Journal of Law & Economics*, 44(2):395–426.

Caplan, B. (2002). Systematically Biased Beliefs about Economics: Robust Evidence of Judgemental Anomalies from the Survey of Americans and Economists on the Economy. *The Economic Journal*, 112(479):433–458.

Christensen, L. (2022). How Does Uncertainty Affect Voters' Preferences? *British Journal of Political Science*, 52(3):1186–1204.

Christensen, L. (2023). Optimal Persuasion under Confirmation Bias: Theory and Evidence From a Registered Report. *Journal of Experimental Political Science*, 10(1):4–20.

Clayton, A. (2021). *Bernoulli's Fallacy: Statistical Illogic and the Crisis of Modern Science*. Columbia University Press.

Clifford, S., Leeper, T. J., and Rainey, C. (2023). Generalizing Survey Experiments Using Topic Sampling: An Application to Party Cues. *Political Behavior*.

Clifford, S., Sheagley, G., and Piston, S. (2021). Increasing Precision without Altering Treatment Effects: Repeated Measures Designs in Survey Experiments. *American Political Science Review*, 115(3):1048–1065.

Coppock, A. and Green, D. P. (2022). Do Belief Systems Exhibit Dynamic Constraint? *The Journal of Politics*, 84(2):725–738.

Coppock, A., Leeper, T. J., and Mullinix, K. J. (2018). Generalizability of heterogeneous treatment effect estimates across samples. *Proceedings of the National Academy of Sciences*, 115(49):12441–12446.

Cunningham, S. (2021). *Causal Inference: The Mixtape*. Yale University Press.

Damgaard, P. R. and James, O. (2024). Public service users' responses to performance information: Bayesian learning or motivated reasoning? *Journal of Public Administration Research and Theory*, 34(4):532–546.

Dehring, C. A., Depken, C. A., and Ward, M. R. (2008). A direct test of the homevoter hypothesis. *Journal of Urban Economics*, 64(1):155–170.

DellaVigna, S. and Gentzkow, M. (2010). Persuasion: Empirical Evidence. *Annual Review of Economics*, 2(Volume 2, 2010):643–669.

DellaVigna, S. and Linos, E. (2022). RCTs to Scale: Comprehensive Evidence From Two Nudge Units. *Econometrica*, 90(1):81–116.

Dinesen, P. T. and Sønderskov, K. M. (2015). Ethnic Diversity and Social Trust: Evidence from the Micro-Context. *American Sociological Review*, 80(3):550–573.

Edwards, W. (1982). Conservatism in human information processing. In Tversky, A., Kahneman, D., and Slovic, P., editors, *Judgment under Uncertainty: Heuristics and Biases*, pages 359–369. Cambridge University Press, Cambridge.

Einstein, K. L., Palmer, M., and Glick, D. M. (2019). Who Participates in Local Government? Evidence from Meeting Minutes. *Perspectives on Politics*, 17(1):28–46.

Elmendorf, C. S., Nall, C., and Oklobdzija, S. (2024a). Do Housing Supply Skeptics Learn? Evidence from Economics and Advocacy Treatments.

Elmendorf, C. S., Nall, C., and Oklobdzija, S. (2024b). What State Housing Policies Do Voters Want? Evidence from a Platform-Choice Experiment.

Engsted, T. and Schneider, J. W. (2024). Non-Experimental Data, Hypothesis Testing, and the Likelihood Principle: A Social Science Perspective. *Foundations and Trends® in Econometrics*, 13(1):1–66.

Fang, L., Stewart, N., and Tyndall, J. (2023). Homeowner politics and housing supply. *Journal of Urban Economics*, 138:103608.

Fenger, J. (2025). *When facts matter: how diagnostic information enlightens public opinion in real-world politics*. PhD thesis, Politica, Aarhus University.

Fischel, W. A. (2001). Why Are There NIMBYs? *Land Economics*, 77(1):144–152.

Fischel, W. A. (2005). *The Homevoter Hypothesis: How Home Values Influence Local Government Taxation, School Finance, and Land-Use Policies*. Harvard University Press.

Garvin, N. (2023). The effect of housing supply on prices: Expectations versus reality – e61 INSTITUTE.

Gelman, A. and Carlin, J. (2014). Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors. *Perspectives on Psychological Science*, 9(6):641–651.

Gelman, A., Carlin, J. B., Stern, H. S., Dunson, D. B., Vehtari, A., and Rubin, D. B. (2013). *Bayesian Data Analysis*. Chapman and Hall/CRC, New York, 3 edition.

Gelman, A. and Hill, J. (2006). *Data Analysis Using Regression and Multilevel/Hierarchical Models*. Cambridge University Press, Cambridge ; New York, 1st edition edition.

Gelman, A., Hill, J., and Vehtari, A. (2021). *Regression and other stories*. Analytical Methods for Social Research Series. Cambridge University Press, Cambridge, England, first edition. edition.

Gelman, A., Hullman, J., Wlezien, C., and Morris, G. E. (2020). Information, incentives, and goals in election forecasts. *Judgment and Decision Making*, 15(5):863–880.

Gerber, A. and Green, D. (1999). MISPERCEPTIONS ABOUT PERCEPTUAL BIAS. *Annual Review of Political Science*, 2(Volume 2, 1999):189–210.

Gerber, A. S. and Green, D. P. (2012). *Field Experiments: Design, Analysis, and Interpretation*. W. W. Norton.

Gigerenzer, G. (2018). The Bias Bias in Behavioral Economics. *Review of Behavioral Economics*, 5(3-4):303–336.

Glaeser, E. and Gyourko, J. (2018). The Economic Implications of Housing Supply. *Journal of Economic Perspectives*, 32(1):3–30.

Glaeser, E. L., Gyourko, J., and Saks, R. E. (2005). Why Have Housing Prices Gone Up? *American Economic Review*, 95(2):329–333.

Green-Pedersen, C. and Mortensen, P. B. (2010). Who sets the agenda and who responds to it in the Danish parliament? A new model of issue competition and agenda-setting. *European Journal of Political Research*, 49(2):257–281.

Green-Pedersen, C. and Mortensen, P. B. (2015). Avoidance and Engagement: Issue Competition in Multiparty Systems. *Political Studies*, 63(4):747–764.

Grekousis, G. (2020). *Spatial Analysis Methods and Practice: Describe – Explore – Explain through GIS*. Cambridge University Press, Cambridge.

Guess, A. and Coppock, A. (2020). Does Counter-Attitudinal Information Cause Backlash? Results from Three Large Survey Experiments. *British Journal of Political Science*, 50(4):1497–1515.

Haaland, I., Roth, C., and Wohlfart, J. (2023). Designing Information Provision Experiments. *Journal of Economic Literature*, 61(1):3–40.

Hall, A. B. and Yoder, J. (2022). Does Homeownership Influence Political Behavior? Evidence from Administrative Data. *The Journal of Politics*, 84(1):351–366.

Hankinson, M. (2018). When Do Renters Behave Like Homeowners? High Rent, Price Anxiety, and NIMBYism. *American Political Science Review*, 112(3):473–493.

Hansen, K. and Stubager, R. (2017). *Oprør fra udkanten: folketingsvalget 2015.* Studier i dansk politik, 5. Jurist- og Økonomforbundet, Copenhagen, 1. udgave. 1. oplag. edition.

Hansen, K. M., Andersen, L. B., and Hansen, S. W. (2021). *Metoder i statskundskab.* Samfundsvidenskabernes metoder ; nr. 1. Hans Reitzel, Copenhagen, 3. edition.

Hansen, K. M. and Stubager, R. (2023). The danish national election study 2022. CVAP Working Paper Series no. 5 Copenhagen:, Department of Political Science, University of Copenhagen.

Hansen, K. M. and Stubager, R. (2024). *Partiledernes kamp om midten: folketingsvalget 2022.* Studier i dansk politik, 9. Djøf Forlag, Copenhagen, 1. edition.

Heebøll, C. and Haahr, J. H. (2018). BOLIGMARKEDSANALYSE FOR HOVEDSTADEN - Boligbehov, udviklingspotentialer og -strategier. Technical report, Copenhagen Economics & Kuben Management.

Heidemanns, M., Gelman, A., and Morris, G. E. (2020). An Updated Dynamic Bayesian Forecasting Model for the U.S. Presidential Election. *Harvard Data Science Review*, 2(4).

Hill, S. J. (2017). Learning Together Slowly: Bayesian Learning about Political Facts. *The Journal of Politics*, 79(4):1403–1418.

Hirt, S. (2012). Mixed Use by Default: How the Europeans (Don't) Zone. *Journal of Planning Literature*, 27(4):375–393.

Hjort, J., Moreira, D., Rao, G., and Santini, J. F. (2021). How Research Affects Policy: Experimental Evidence from 2,150 Brazilian Municipalities. *American Economic Review*, 111(5):1442–1480.

Hvidkjær, M. S. and Larsen, M. V. (2025). Politisk repræsentation i det danske lokaldemokrati. *Politica*, 57(2):131–151.

Imbens, G. W. (2021). Statistical Significance, -Values, and the Reporting of Uncertainty. *The Journal of Economic Perspectives*, 35(3):157–174.

Imbens, G. W. and Wooldridge, J. M. (2009). Recent Developments in the Econometrics of Program Evaluation. *Journal of Economic Literature*, 47(1):5–86.

Kane, J. V. (2025). More than meets the ITT: A guide for anticipating and investigating nonsignificant results in survey experiments. *Journal of Experimental Political Science*, 12(1):110–125.

Kane, J. V. and Barabas, J. (2019). No Harm in Checking: Using Factual Manipulation Checks to Assess Attentiveness in Experiments. *American Journal of Political Science*, 63(1):234–249.

Kettel, L. and Larsen, M. V. (2024). Limits of support. Unpublished manuscript, prepared for the Nordic Political Behavior Workshop, October 2024.

Klement, J., Kozák, J., Ševčík, M., and Pekárek, (2023). Causes of opposition to residential development: a survey-framing experiment from the Czech Republic. *European Planning Studies*, 31(2):306–327.

Kuklinski, J. H. and Quirk, P. J. (2001). Conceptual Foundations of Citizen Competence. *Political Behavior*, 23(3):285–311.

Kuklinski, J. H., Quirk, P. J., Jerit, J., and Rich, R. F. (2001). The Political Environment and Citizen Competence. *American Journal of Political Science*, 45(2):410–424.

König, T., Marbach, M., and Osnabrügge, M. (2017). Left/Right or U? Estimating the Dimensionality of National Party Competition in Europe. *The Journal of Politics*, 79(3):1101–1105.

Københavns Kommune (2024). Boligredegørelsen. Technical report.

Larsen, M. V. (2025). En lukket boligfest. In *Fra kriser til knaphed: En hvidbog om prisudvikling og boligbehov i et voksende København*, pages 4–8. By & Havn, København.

Larsen, M. V. and Kettel, L. (2023). When Smaller Local Governments Permit More Housing: Reassessing the Effect of Local Control on Land Use Policy.

Larsen, M. V. and Nyholt, N. (2024a). Understanding Nimbyism as Local Preservationism.

Larsen, M. V. and Nyholt, N. (2024b). Understanding Opposition to Apartment Buildings. *Journal of Political Institutions and Political Economy*, 5(1):29–46.

Larsen, M. V. and Olsen, A. L. (2020). Reducing Bias in Citizens' Perception of Crime Rates: Evidence from a Field Experiment on Burglary Prevalence. *The Journal of Politics*, 82(2):747–752.

Lee, P. M. (2012). *Bayesian Statistics: An Introduction.* John Wiley & Sons, Incorporated, Newark, UNITED STATES.

Leemann, L., Stoetzer, L. F., and Traunmüller, R. (2021). Eliciting Beliefs as Distributions in Online Surveys. *Political Analysis*, 29(4):541–553.

Lenz, G. S. and Sahn, A. (2021). Achieving Statistical Significance with Control Variables and Without Transparency. *Political Analysis*, 29(3):356–369.

Lin, W. (2013). Agnostic notes on regression adjustments to experimental data: Reexamining Freedman's critique. *The Annals of Applied Statistics*, 7(1):295–318.

Little, A. T. (2021). Directional motives and different priors are observationally equivalent.

Little, A. T. (2025). How to Distinguish Motivated Reasoning from Bayesian Updating. *Political Behavior*.

Lundberg, I., Johnson, R., and Stewart, B. M. (2021). What Is Your Estimand? Defining the Target Quantity Connects Statistical Evidence to Theory. *American Sociological Review*, 86(3):532–565.

Manski, C. F. (2009). *Identification for Prediction and Decision.* Harvard University Press.

Manski, C. F. (2018). Survey Measurement of Probabilistic Macroeconomic Expectations: Progress and Promise. *NBER Macroeconomics Annual*, 32:411–471.

Marble, W. and Nall, C. (2021). Where Self-Interest Trumps Ideology: Liberal Homeowners and Local Opposition to Housing Development. *The Journal of Politics*, 83(4):1747–1763.

Mast, E. (2023). JUE Insight: The effect of new market-rate housing construction on the low-income housing market. *Journal of Urban Economics*, 133:103383.

McElreath, R. (2020). *Statistical Rethinking: A Bayesian Course with Examples in R and STAN.* Chapman and Hall/CRC, New York, 2 edition.

Mehtaj, M. A. and Rumpf, M. (2024). Why inflation differs across countries | ECB Data Portal.

Mense, A. (2025). The Impact of New Housing Supply on the Distribution of Rents. *Journal of Political Economy Macroeconomics*, 3(1):1–42.

Monkkonen, P. and Manville, M. (2019). Opposition to development or opposition to developers? Experimental evidence on attitudes toward new housing. *Journal of Urban Affairs*, 41(8):1123–1141.

Moore, W. H. and Siegel, D. A. (2013). *A Mathematics Course for Political and Social Research*. Princeton University Press.

Mullinix, K. J., Leeper, T. J., Druckman, J. N., and Freese, J. (2015). The Generalizability of Survey Experiments. *Journal of Experimental Political Science*, 2(2):109–138.

Mummolo, J. and Peterson, E. (2019). Demand Effects in Survey Experiments: An Empirical Assessment. *American Political Science Review*, 113(2):517–529.

Munch, A. H. and Albertsen, A. E. (2025). At måle social kontekst: En sammenligning af subjektive kontekster, administrative enheder og mikrokontekster.

Müller, D. and Gsottbauer, E. (2022). Why Do People Demand Rent Control?

Nall, C., Elmendorf, C. S., and Oklobdzija, S. (2024). Folk Economics and the Persistence of Political Opposition to New Housing.

Nyhan, B. and Reifler, J. (2010). When Corrections Fail: The Persistence of Political Misperceptions. *Political Behavior*, 32(2):303–330.

OECD (2017). Land-use Planning Systems in the OECD. OECD Regional Development Papers, OECD Publishing, Paris.

OECD (2023). Confronting the cost-of-living and housing crisis in cities. OECD Regional Development Papers no. 49, OECD Publishing, Paris.

OECD (2025a). Real house prices (indicator). Digital link. Accessed: 17-04-2024.

OECD (2025b). Rent prices (indicator). Digital link. Accessed: 17-04-2024.

Phillips, S., Manville, M., and Lens, M. (2021). Research Roundup: The Effect of Market-Rate Development on Neighborhood Rents. UCLA Lewis Center for Regional Policy Studies Paper.

Rosokha, Y. and Bland, J. R. (2024). Rounding the (Non)Bayesian Curve: Unraveling the Effects of Rounding Errors in Belief Updating.

Roth, C. and Wohlfart, J. (2020). How Do Expectations about the Macroeconomy Affect Personal Expectations and Behavior? *The Review of Economics and Statistics*, 102(4):731–748.

Rubin, D. B. and Imbens, G. W. (2015). Assessing Overlap in Covariate Distributions. In *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*, pages 309–336. Cambridge University Press, Cambridge.

Rubin, P. H. (2003). Folk Economics. *Southern Economic Journal*, 70(1):157–171.

Sahn, A. (2021). Racial Diversity and Exclusionary Zoning: Evidence from the Great Migration – Berkeley Initiative for Young Americans.

Sahn, A. (2024). Public comment and public policy. *American Journal of Political Science.*

Seeberg, H. B. (2017). How Stable Is Political Parties' Issue Ownership? A Cross-Time, Cross-National Analysis. *Political Studies*, 65(2):475–492.

Solon, G., Haider, S. J., and Wooldridge, J. (2013). What Are We Weighting For?

Stoetzer, L. F., Leemann, L., and Traunmueller, R. (2024). Learning from Polls During Electoral Campaigns. *Political Behavior*, 46(1):543–564.

Stubager, R. and Hansen, K. (2021). *Klimavalget: folketingsvalget 2019.* Number 8 in Studier i dansk politik. Djøf, Copenhagen, 1. edition.

Stubager, R., Hansen, K. M., and Goul Andersen, J. (2013). *Krisevalg: økonomien og folketingsvalget 2011.* Studier i dansk politik, 1. Jurist- og Økonomforbundet, Copenhagen, 1. edition.

Telles, S. M., Hammond, S., and Takash, D. (2021). Cost Disease Socialism: How Subsidizing Costs While Restricting Supply Drives America's Fiscal Imbalance. Technical report, Niskanen Center.

Tobler, W. R. (1970). A Computer Movie Simulating Urban Growth in the Detroit Region. *Economic Geography*, 46:234–240.

Trounstine, J. (2023). You Won't be My Neighbor: Opposition to High Density Development. *Urban Affairs Review*, 59(1):294–308.

Tversky, A. and Kahneman, D. (1973). Availability: A heuristic for judging frequency and probability. *Cognitive Psychology*, 5(2):207–232.

Vehtari, A., Gelman, A., and Gabry, J. (2017). Practical Bayesian model evaluation using leave-one-out cross-validation and WAIC. *Statistics and Computing*, 27(5):1413–1432.

Vilfort, V. and Zhang, W. (2024). Interpreting TSLS Estimators in Information Provision Experiments. arXiv:2309.04793 [econ].

Vivalt, E. and Coville, A. (2023). How do policymakers update their beliefs? *Journal of Development Economics*, 165:103121.

Walgrave, S., Jansen, A., Sevenans, J., Soontjens, K., Pilet, J.-B., Brack, N., Varone, F., Helfer, L., Vliegenthart, R., van der Meer, T., Breunig, C., Bailer, S., Sheffer, L., and Loewen, P. J. (2023). Inaccurate Politicians: Elected Representatives' Estimations of Public Opinion in Four Countries. *The Journal of Politics*, 85(1):209–222.

Wicki, M., Hofer, K., and Kaufmann, D. (2022). Planning instruments enhance the acceptance of urban densification. *Proceedings of the National Academy of Sciences*, 119(38):e2201780119.

Wicki, M. and Kaufmann, D. (2022). Accepting and resisting densification: The importance of project-related factors and the contextualizing role of neighbourhoods. *Landscape and Urban Planning*, 220:104350.

Wong, C., Bowers, J., Williams, T., and Simmons, K. D. (2012). Bringing the Person Back In: Boundaries, Perceptions, and the Measurement of Racial Context. *The Journal of Politics*, 74(4):1153–1170.

Wood, T. and Porter, E. (2019). The Elusive Backfire Effect: Mass Attitudes' Steadfast Factual Adherence. *Political Behavior*, 41(1):135–163.

Yoder, J. (2020). Does Property Ownership Lead to Participation in Local Politics? Evidence from Property Records and Meeting Minutes. *American Political Science Review*, 114(4):1213–1229.

Zaller, J. R. (1992). *The Nature and Origins of Mass Opinion.* Cambridge Studies in Public Opinion and Political Psychology. Cambridge University Press, Cambridge.

# A Appendix

## A.1 Treatment vignettes

The exact wording of the treatment vignette is as follows, where {price change} takes either the value 1.9 pct. - which is the actual value reported by Mense (2025) (adjusted to 10 pct. supply increase instead of 1 pct.) - or 7.6 pct., which is an artificially inflated value.

> A lot of recent economic research has focused on how new private housing development affects rental prices and the availability of affordable housing in urban areas.
>
> A study from Germany shows that **a 10% increase in the number of homes in a municipality reduces rents in that municipality by `{price change}`**. This is a relatively substantial price drop, especially considering that rent across Denmark has, on average, increased by 2.26% per year over the past ten years. In very expensive municipalities, such as Aarhus and Copenhagen, rents have risen by 3–4% annually.
>
> Additionally, economic studies from both Europe and the U.S. show that the construction of new private housing in expensive neighborhoods creates so-called "moving chains," which quickly free up existing, more affordably priced homes in middle- and lower-income areas elsewhere.
>
> "Moving chains" occur when new, often upscale homes are built: some people move into them and leave behind their previous homes, which are then occupied by others—who in turn vacate their own homes. This chain reaction increases housing availability, **allowing both middle- and lower-income groups to benefit from private housing development in expensive areas—even if they themselves can't afford the new units.**

## A.2 Primer on Bayesian Inference

Bayesian inference is a statistical framework for learning from data, built on the idea that beliefs about the world (or model parameters) should be updated as new evidence becomes available. As in the theory section, Bayes' Theorem lies at the core of this statistical paradigm:

$$P(\theta \mid D) = \frac{P(D \mid \theta) \cdot P(\theta)}{P(D)}$$

However, the theorem now concerns inferences about parameters in a statistical model. Here, $\theta$ represents the *parameter* we wish to estimate – such as a vote-share for Trump. $P(\theta)$ is the *prior distribution*, capturing our initial beliefs about what the vote-share might be (based on theory, past studies, or expert knowledge) – sometimes the prior is uninformative and essentially just states that extremely large or small vote-shares are very unlikely, however this is still considered prior information. $P(D \mid \theta)$, the *likelihood*, expresses how likely the observed data $D$ are, given a particular value of $\theta$. The result, $P(\theta \mid D)$, is the *posterior distribution*: our updated belief about $\theta$ after observing the data – or in other words, the probability of a parameter-value given the data (McElreath, 2020). While the above example deals with only one parameter (vote-share), this understanding of inference can readily be extended to any other model. For example when estimating a regression model using Bayesian inference, one places priors on the intercept and the slope, which then meet data as in the above example – However, in practice, the estimation procedure is often more complex than the basic formulation above and typically relies on computational methods such as Markov Chain Monte Carlo (MCMC) to approximate the posterior distribution when analytical solutions are intractable (Gelman et al., 2013; McElreath, 2020).

Unlike traditional (frequentist) methods that provide point estimates and confidence intervals, Bayesian inference yields a full probability distribution over parameters. This allows researchers to make intuitive probability statements – such as "there's a 90 pct. chance the treatment effect is positive" – which are often more interpretable in policy contexts (Imbens, 2021). This is because Bayesian statistics treats probability as a measure of uncertainty rather than a long-run frequency (Gelman et al., 2013; Clayton, 2021). A common way to summarize this uncertainty is through the Highest Density Interval (HDI) (often referred to as credible intervals), which contains the most credible values of a parameter – for example, a 95 pct. HDI includes the most probable 95 pct. of the posterior distribution. The HDI allows us to directly state that the parameter has a 95 pct. probability of falling within that range, given the model and data. In contrast, a frequentist confidence interval tells us that if we were to resample and refit our model 100 times, 95 of those intervals (given $\alpha = 0.05$) would, on average, contain the true parameter value. However, the frequentist interval does not provide any information about the relative likelihood of different parameter values within the interval – only that, in the long run, 95 pct. of such intervals would capture the true parameter. Ironically, it is not uncommon for laypeople and trained scientists alike to misinterpret frequentist confidence intervals as Bayesian HDIs (Gelman et al., 2020; Imbens, 2021). Because Bayesian HDIs reflect the full probability distribution over a given parameter, they avoid reliance on arbitrary significance thresholds like $p < 0.05$. This makes them especially informative when the interval overlaps zero: rather than reducing inference to a binary significant/not-significant judgment, the HDI shows how probability density is distributed – allowing us to see, for instance, whether small positive effects are more credible than large negative ones, even if zero remains within the interval.

As noted above, Bayesian inference requires more subjective input – the priors – than frequentist methods. However, as the amount of data increases, the influence of the prior diminishes rapidly (McElreath, 2020). As a result, Bayesian and frequentist estimates are often – though not always – quite similar. To reiterate, the key difference is that Bayesian inference tends to be more interpretable and avoids reliance on arbitrary significance thresholds like $p < 0.05$ (McElreath, 2020; Imbens, 2021; Engsted & Schneider, 2024). To be clear, this ease of interpretation is the primary reason I prefer the Bayesian approach.

An illustrative example of how one might set priors, is the dynamic forecasting model developed by Heidemanns et al. (2020) for the 2020 U.S. presidential election. Their model combines polling data with political and economic "fundamentals" – like GDP growth and presidential approval – using a hierarchical Bayesian framework. Early in the campaign, forecasts are anchored by historical data on how fundamentals (the prior) have predicted past elections. As new polls (the likelihood) come in, the model updates its beliefs using Bayes' theorem, shifting weight from the priors to the polling-based likelihood. This produces a full posterior distribution over possible election outcomes, reflecting both prior expectations and new evidence. In many other cases, one might not have such a well developed understanding of the process one is trying to model. In this case priors will reflect this by being less informative, but one can always do better than setting no prior (McElreath, 2020). For example, in the social sciences, it is generally reasonable to assume that very large effect sizes are unlikely. This belief can serve as an (un)informative prior, helping to regularize estimates and guard against overinterpreting noisy data. As was stated in the methodology section, these are the sort of priors I use in this thesis.

## A.3 Deriving Equation (3)

As was stated in section 4.1.2 and 4.3.1, I elicit five measures of respondent $i$'s belief about a supply shock's effect on housing prices. Specifically, I measure three important values: the mean, denoted $y_{1i}$, as well as the lower and upper bound values, denoted $y_{2i}$ and $y_{3i}$ respectively. In addition to these measures, we observe two probabilities linked to observing values under the lower bound and above the upper bound. These are denoted $p_{1i}$ and $p_{2i}$. We want to use these measures to estimate the parameters $\mu_i$ and $\sigma_i$ in the following distribution:

$$B_i \sim \mathcal{N}(\mu_i, \sigma_i)$$

Because the belief, $B_i$, is assumed to be normally distributed the most likely value, $y_{1i}$, is - by construction of the Manski questionnaire - equal to $\mu_i$.

To derive the formula for $\sigma_i$, we start by stating:

$$P(B_i \leq y_{2_i}) = p_{1_i}, \text{ and}$$

$$P(B_i > y_{3_i}) = p_{2_i} \Leftrightarrow P(B_i \leq y_{3_i}) = 1 - p_{2_i}$$

Using the cumulative distribution function (CDF) of a normal distribution, we can state this as:

$$\Phi\left(\frac{y_{2_i} - \mu_i}{\sigma_i}\right) = p_{1_i}, \quad \Phi\left(\frac{y_{3_i} - \mu_i}{\sigma_i}\right) = 1 - p_{2_i}$$

Applying the inverse CDF (quantile function) yields the following:

$$\frac{y_{2_i} - \mu_i}{\sigma_i} = \Phi^{-1}(p_{1_i}), \quad \frac{y_{3_i} - \mu_i}{\sigma_i} = \Phi^{-1}(1 - p_{2_i})$$

Solving for $y_{2_i}$ and $y_{3_i}$, we get:

$$y_{2_i} = \mu_i + \sigma_i \Phi^{-1}(p_{1_i}), \quad y_{3_i} = \mu_i + \sigma_i \Phi^{-1}(1 - p_{2_i})$$

Subtracting the equations:

$$y_{3_i} - y_{2_i} = \sigma_i \left(\Phi^{-1}(1 - p_{2_i}) - \Phi^{-1}(p_{1_i})\right)$$

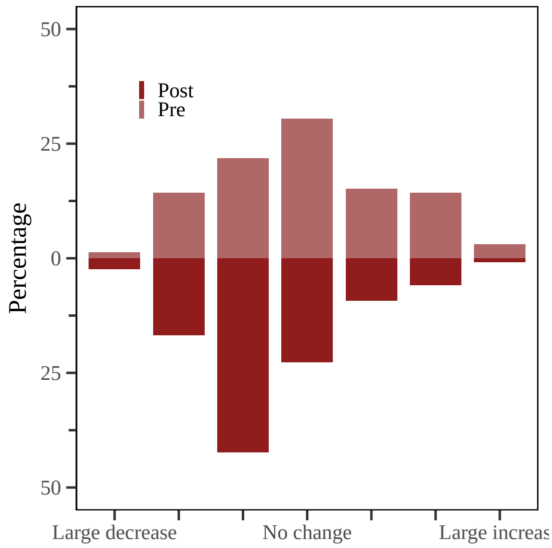
Solving for $\sigma_i$ yields Equation (3):

$$\sigma_i = \frac{y_{3_i} - y_{2_i}}{\Phi^{-1}(1 - p_{2_i}) - \Phi^{-1}(p_{1_i})}$$

## A.4 Supply skepticism on Likert scale

The figure below shows the marginal distribution of responses on the Likert-scale measure of supply skepticism, both before and after the intervention.

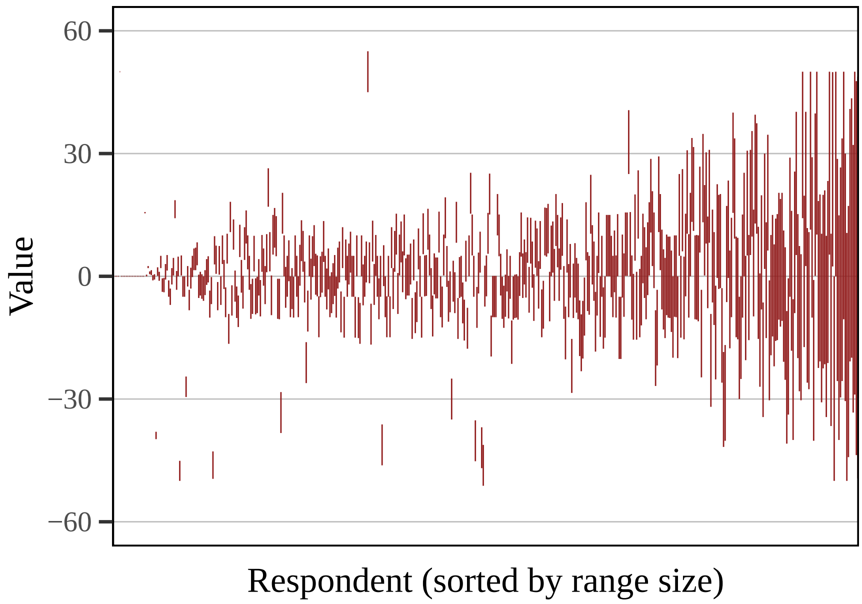
**Figure A.1:** Change in Likert skepticism



## A.5 Likely range heterogeneity

The figure below presents individuals' prior likely ranges, sorted by the size of each range $(y_{3_i} - y_{2_i})$. As with the prior probability distributions, there is considerable heterogeneity in both the coverage and size of these ranges.

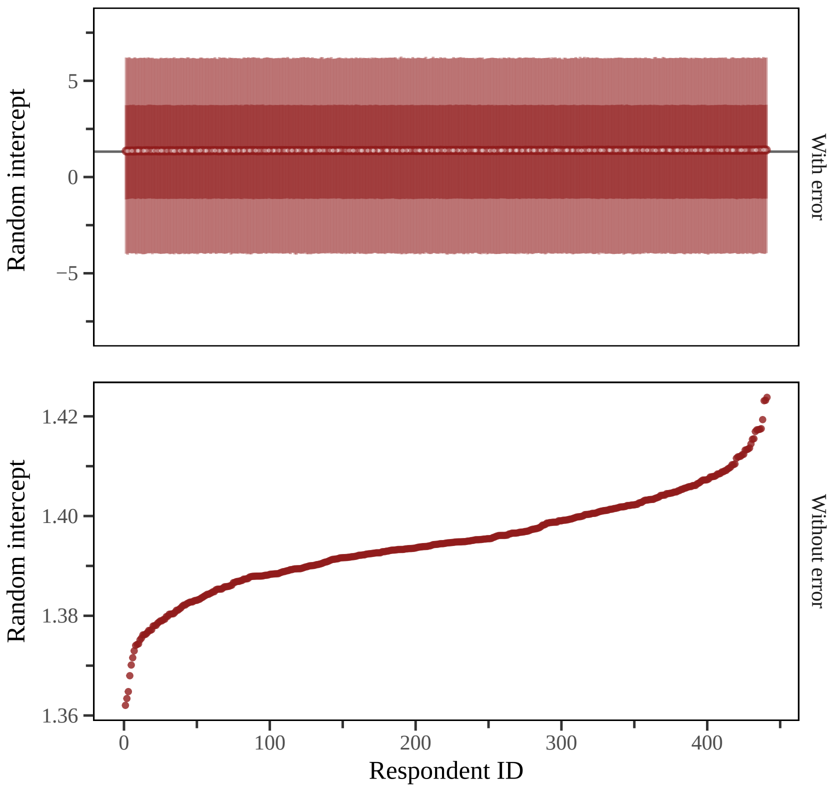
**Figure A.2:** Individual-level likely ranges



## A.6 Random intercepts in Equation (6)

The figure below plots the random effects (REs) from the main specification of Equation (6). The upper panel shows the REs with uncertainty, which appears large and uniform due to each cluster (individual) contributing only two observations (the solid line represents the global intercept). For visual clarity, the lower panel displays the REs without error bars. While some variation is observable, it remains deceptively small (notice y-axis range) – consistent with the very low intraclass correlation coefficient (ICC).

**Figure A.3:** Random intercepts in Equation (6) (RE + cov.)



## A.7 Outlier weights sensitivity analysis

As described in the main text, this section reruns Equation (7) after dropping extreme individual-level signal weights (see Fig. A.4), and re-estimates the first-difference model from Equation (6) both with and without extreme weights dropped (see Table A.1).

**Figure A.4:** Aggregate updating weights with outlier weights trimmed

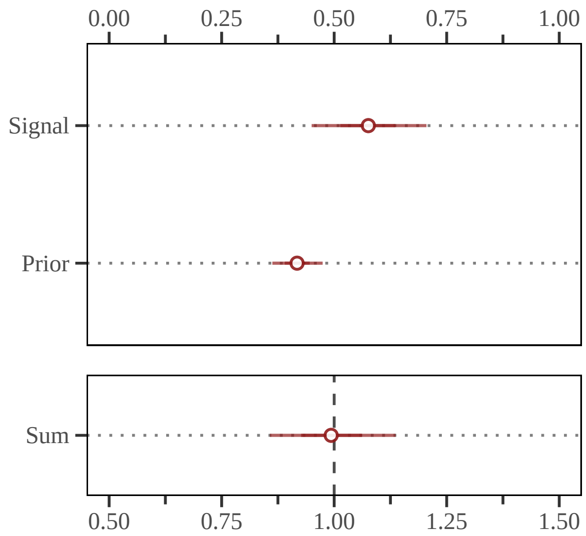



**Table A.1:** First-difference model estimates: full vs. trimmed sample

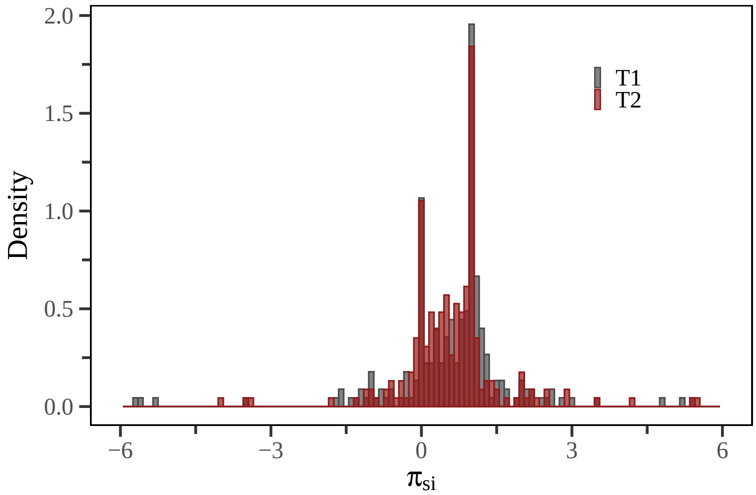
| | | | | | | | |
|---|---|---|---|---|---|---|---|
| | | | **Full Sample (n = 472)** | | | | |
| Parameter | Estimate | Error | 95% Lower | 95% Upper | $\hat{R}$ | Bulk ESS | Tail ESS |
| Intercept ($\alpha$) | -2.21 | 0.69 | -3.57 | -0.82 | 1.00 | 4746 | 3135 |
| Treatment ($\theta$) | -2.51 | 0.99 | -4.48 | -0.57 | 1.00 | 4937 | 3184 |
| Residual SD ($\sigma$) | 10.66 | 0.29 | 10.10 | 11.24 | 1.00 | 4871 | 3307 |
| | | | **Trimmed Sample (n = 453)** | | | | |
| Parameter | Estimate | Error | 95% Lower | 95% Upper | $\hat{R}$ | Bulk ESS | Tail ESS |
| Intercept ($\alpha$) | -1.92 | 0.65 | -3.16 | -0.67 | 1.00 | 3851 | 2828 |
| Treatment ($\theta$) | -2.41 | 0.93 | -4.24 | -0.62 | 1.00 | 3508 | 2789 |
| Residual SD ($\sigma$) | 9.85 | 0.27 | 9.33 | 10.41 | 1.00 | 4019 | 2753 |

**Note**: Estimates are posterior means. 95% credible intervals in brackets. Error = posterior standard deviation; Rhat = Gelman-Rubin convergence diagnostic; ESS = effective sample size (Bulk, Tail).

## A.8 Signal weights by treatment

The figure below plots individual-level signal weights as in Fig. 12, but conditional on treatment status. As shown, individuals in T1 are slightly more prone to over-updating ($\pi_{s_i} > 1$).

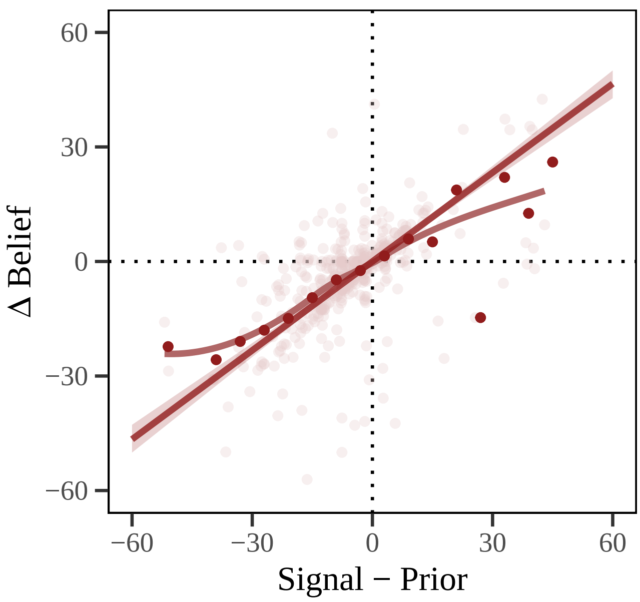
**Figure A.5:** Individual level weights by group



## A.9 Pooled Signal weights and linearity

The figure below is essentially the same as Figs. 13 and 14, but without interaction terms. As shown, updating behavior is remarkably linear overall, though some bins with a larger positive signal-prior gap deviate from this trend. However, data are sparse in this tail, so these deviations are likely due to noise.

**Figure A.6:** Pooled signal weight, linear vs. lowess fit



## A.10 Preferences pre-post

The table below presents results from estimating Equation 5 with anti-development preferences as the outcome. The "Pre-Post" row shows the treatment effect, conveying the same information as Fig. 9 but in tabular form.

**Table A.2:** Estimates for preference outcome across model specifications

| **Parameter** | **Simple** | **RE** | **RE + Cov.** | **FE** |
|---|---|---|---|---|
| Intercept[a] | 3.40 | 3.42 | 4.28 | 3.32 |
| | [3.25, 3.54] | [3.28, 3.55] | [3.42, 5.19] | [3.18, 3.47] |
| Pre-post | -0.20 | -0.21 | -0.21 | -0.21 |
| | [-0.40, -0.00] | [-0.28, -0.15] | [-0.27, -0.14] | [-0.28, -0.15] |
| Residual SD | 1.52 | 0.48 | 0.48 | 0.51 |
| | [1.46, 1.59] | [0.45, 0.52] | [0.45, 0.52] | [0.47, 0.54] |
| Random Intercept SD | – | 1.43 | 1.25 | – |
| | – | [1.34, 1.52] | [1.16, 1.35] | – |
| $n =$ | 921 | 921 | 859 | 921 |

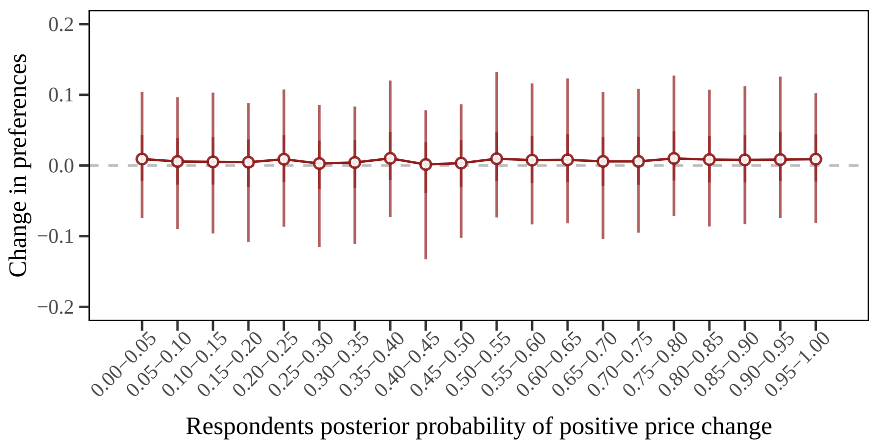
**Note:** Point estimates shown above, with 95% credible intervals in brackets below. "RE" = random intercept model; "Cov." = includes covariates; "FE" = fixed effects per individual. [a] For the fixed effects model, this is the pre-value for respondent ID = 1

## A.11 Posterior uncertainty and updating

To explore whether uncertainty about policy effectiveness influences preference updating, I conducted a simple descriptive test using the posterior probability each respondent assigned to the possibility of a price increase. I grouped respondents by their posterior probability of a price increase and compared average policy preferences across these groups. The results are from a hierarchical extension of Equation (10) where the second linear model is assigned random slopes and intercepts across with the probability intervals serving as clusters.

The results show minimal variation in preferences across levels of posterior uncertainty. This suggests that even among respondents more confident that supply measures would reduce prices, policy preferences remain largely unchanged. While this does not rule out uncertainty as a factor, it may reflect limited preference variability in the sample or the influence of more stable underlying attitudes. Furthermore, since posterior uncertainty is not randomly assigned, the causal status of these conditional effects is not obvious.

**Figure A.7:** Preference updating conditional on posterior certainty of positive price change

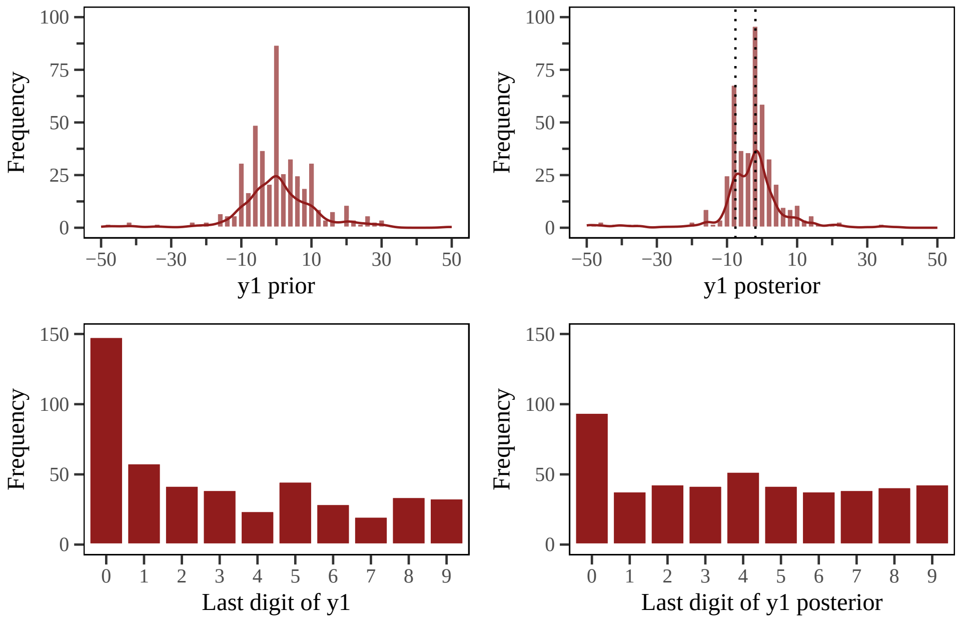


## A.12 Anchoring and rounding

The upper panels report the marginal distribution of prior and posterior $y_{1_i}$ answers in bins of size 1. As shown, there are some signs of anchoring in the post-treatment answers (dashed lines indicate –1,9 and –7,6), though, as discussed in the main text, this occurs at a relatively low rate.

The lower panels display the distribution of answers by last digit (i.e., rounding). While some rounding is evident in prior answers, it appears at a slightly lower rate than reported in other experiments (Rosokha & Bland, 2024, p. 6).

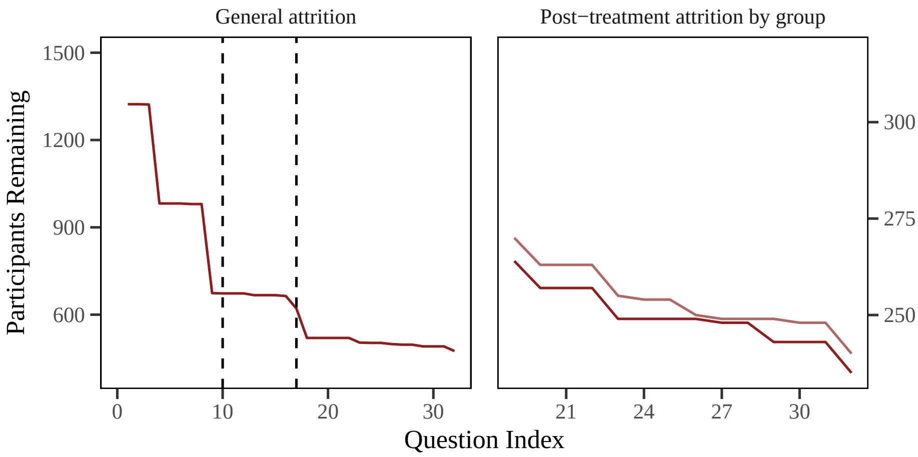
**Figure A.8:** Rounding and anchoring



## A.13 Attrition analysis

The figure below shows the number of respondents remaining in the survey by question index (1 = first question, 2 = second question, etc.). As illustrated, most attrition occurs early on, during the demographic and introductory questions. After the treatment, there is no evidence of differential dropout across treatment arms.

**Figure A.9:** Attrition curves




**Note:** Dashed lines denote start of Manski question sets. Right panel only displays post-treatment questions

### A.14 Full model specifications

**Likely range model (Equation 4)**

$$y_i^m \sim \mathcal{N}(\mu_i^m, \sigma^m), \quad \text{for } m = y_{1_i}, y_{2_i}, y_{3_i}$$
$$\mu_i^m = \alpha^m$$
$$\alpha^{y_{1_i}} \sim \mathcal{N}(0, 12.30534)$$
$$\sigma^{y_{1_i}} \sim \exp(12.30534)$$

**Pre-post model (Equation 5)**

$$B_{it} \sim \mathcal{N}(\mu_{it}, \sigma)$$
$$\mu_{it} = \alpha_{j[it]} + \theta T_{it} + \mathbf{X}'_{it}\boldsymbol{\beta}$$
$$\alpha_j \sim \mathcal{N}(\bar{\alpha}, \tau)$$
$$\bar{\alpha} \sim \mathcal{N}(0, 11.9)$$
$$\sigma, \tau \sim \text{Exponential}(11.9)$$
$$\theta, \boldsymbol{\beta} \sim \mathcal{N}(0, 11.9)$$

**Differential updating model (Equation 6)**

$$B_i^1 \sim \mathcal{N}(\mu_i, \sigma)$$
$$\mu_i = \alpha + \theta D_i + \varphi B_i^0 + \mathbf{X}'_i\boldsymbol{\beta}$$
$$\alpha, \theta, \varphi, \boldsymbol{\beta} \sim \mathcal{N}(0, 11.24)$$
$$\sigma \sim \text{Exponential}(11.24)$$

**Prior and signal weight model (Equation 7)**

$$B_i^1 \sim \mathcal{N}(\mu_i, \sigma)$$
$$\mu_i = \pi_b B_i^0 + \pi_s S_i$$
$$\pi_b, \pi_s \sim \text{Beta}(2, 2)$$
$$\sigma \sim \text{Exponential}(11.24)$$

**Heterogeneous signal weights model (Equation 8)**

$$\Delta B_i \sim \mathcal{T}(\nu, \mu_i, \sigma)$$
$$\mu_i = \pi_s G_i + \zeta C_i^j + \gamma(G_i \times C_i^j)$$
$$\pi_s, \zeta, \gamma \sim \mathcal{N}(0, 12.07)$$
$$\sigma \sim \text{Exponential}(12.07)$$
$$\nu \sim \text{Gamma}(2, 0.1)$$

**signal weights by signal characteristics model (Equation 9)**

$$\Delta B_i \sim \mathcal{T}(\nu, \mu_i, \sigma)$$

$$\mu_i = \pi^s_{j[i]} G_i$$

$$\pi_{s_j} \sim \mathcal{N}(\bar{\pi}, \tau)$$

$$\bar{\pi} \sim \text{Beta}(2, 2)$$

$$\tau \sim \text{Exponential}(0.2)$$

$$\sigma \sim \text{Exponential}(11.24)$$

$$\nu \sim \text{Gamma}(2, 0.1)$$

**Macro-level preferences model (Equation 10)**

$$\begin{pmatrix} \Delta P_i \\ \Delta B_i \end{pmatrix} \sim \text{MVN}\left(\begin{pmatrix} \mu_{\Delta P,i} \\ \mu_{\Delta B,i} \end{pmatrix}, \mathbf{S}\right)$$

$$\mu_{\Delta P,i} = \tau + \delta \Delta B_i$$

$$\mu_{\Delta B,i} = \alpha + \beta S_i$$

$$\mathbf{S} = \begin{pmatrix} \sigma_{\Delta P} & 0 \\ 0 & \sigma_{\Delta B} \end{pmatrix} \mathbf{R} \begin{pmatrix} \sigma_{\Delta P} & 0 \\ 0 & \sigma_{\Delta B} \end{pmatrix}$$

$$\mathbf{R} = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}$$

$$\alpha, \beta \sim \mathcal{N}(0, 12.07)$$

$$\tau, \delta \sim \mathcal{N}(0, 0.25)$$

$$\sigma_{\Delta P} \sim \text{Exponential}(0.25)$$

$$\sigma_{\Delta B} \sim \text{Exponential}(12.07)$$

$$\rho \sim \text{LKJ}(2)$$

**Local preferences model (Equation 11)**

$$\begin{pmatrix} \Delta P_i \\ \Delta B_i \end{pmatrix} \sim \text{MVN}\left(\begin{pmatrix} \mu_{\Delta P,i} \\ \mu_{\Delta B,i} \end{pmatrix}, \mathbf{S}\right)$$

$$\mu_{\Delta P,i} = \tau + \delta \Delta B_i + \sum_{d=2}^{5} \theta_d D_{id} + \sum_{d=2}^{5} \gamma_d (D_{id} \times \Delta B_i)$$

$$\mu_{\Delta B,i} = \alpha + \beta S_i$$

$$\mathbf{S} = \begin{pmatrix} \sigma_{\Delta P} & 0 \\ 0 & \sigma_{\Delta B} \end{pmatrix} \mathbf{R} \begin{pmatrix} \sigma_{\Delta P} & 0 \\ 0 & \sigma_{\Delta B} \end{pmatrix}$$

$$\mathbf{R} = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}$$

$$\alpha, \beta \sim \mathcal{N}(0, 12.07)$$

$$\tau, \delta, \theta_d, \gamma_d \sim \mathcal{N}(0, 0.69)$$

$$\sigma_{\Delta P} \sim \text{Exponential}(0.69)$$

$$\sigma_{\Delta B} \sim \text{Exponential}(12.07)$$

$$\rho \sim \text{LKJ}(2)$$