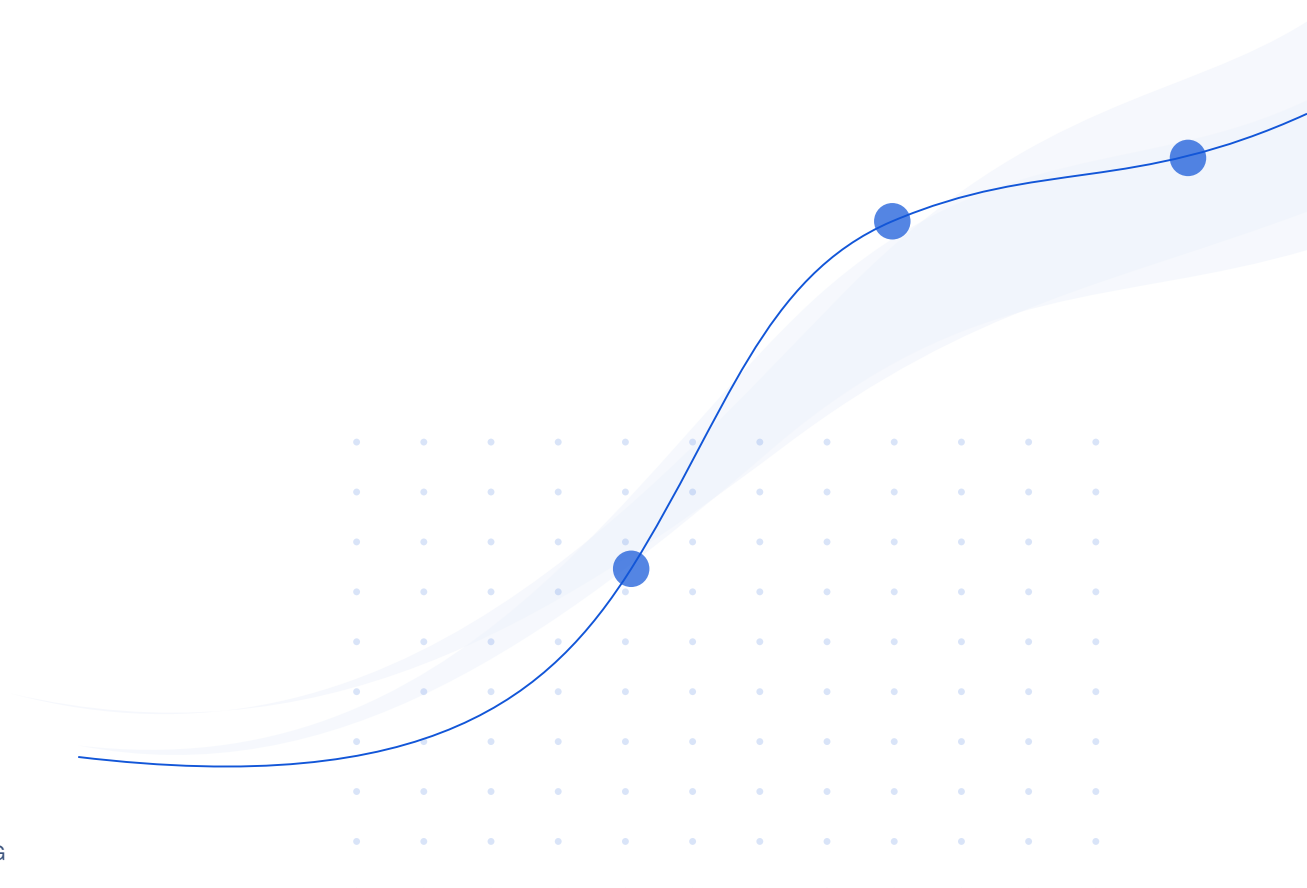


BEHAVIORAL AND EXPERIMENTAL ECONOMICS RESEARCH LAB

Why do citizens support price controls? The role of motivated beliefs

Stanislao Maldonado • Santiago I. Sautua

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Abstract

Despite broad expert consensus that price controls are economically inefficient, they remain popular. We study this puzzle through the lens of motivated beliefs: Individuals who favor government intervention may derive gratification from believing that price controls increase welfare. In a laboratory market, participants shopped under monetary incentives and chose between a free-market regime and a price-cap regime. We varied whether participants learned about the cap through factual information or direct exposure to its consequences, and whether their regime choice was decisive. We find that both information and exposure reduced support for the price cap, but exposure was substantially more effective. However, many participants continued to support the cap after experiencing shortages. Participants with decisive regime choices were less likely to endorse the price cap after adverse information or exposure. Thus, support for inefficient policies reflects not only misinformation but self-serving beliefs that persist when individuals are not held accountable for policy outcomes.

Keywords: price caps; experience; information; motivated beliefs; agency; inefficient policies

JEL Classifications: C91, D03, D72, D83

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[†]Email: stanislao.maldonado@tec.mx

[‡]Email: santiago.sautua@tec.mx

1 Introduction

Price caps, a common form of price control, have been pervasive throughout history (Schuettinger and Butler, 1979) and remain widely used across the world (Guenette, 2020). In many developing countries, they continue to be a central tool to curb inflation and protect consumers, especially when applied to politically sensitive goods such as food, fuel, utilities, and medicines. Price caps have also resurfaced in recent debates in several advanced economies in response to rising prices. For instance, Germany and the United Kingdom capped retail electricity and gas prices during the 2022-2023 energy price surge following Russia’s invasion of Ukraine (Federal Ministry for Economic Affairs and Climate Action, 2022; UK Government, 2023).

The continued use of price caps may be heavily influenced by public opinion, as they remain highly popular in surveys around the world. In Argentina—a country with chronically high inflation—targeted price controls on supermarket products were supported by 75% of respondents in a 2016 survey (Jueguen, 2016). Support extends to advanced economies. A 2024 YouGov poll in the United States found that more than 70% of respondents were amenable to price controls, especially in sectors such as housing and prescription drugs (Bourne, 2024). Similarly, a 2024 Guardian Essential poll in Australia reported that roughly the same share of respondents favored rent, grocery, and energy price caps to address the cost-of-living crisis (Karp, 2024). Other recent polls have documented strong support for rent control among residents of California (DiCamillo, 2017), London (London Mayor’s Office, 2019), the United Kingdom (Owen, 2015), and Germany (Müller and Gsottbauer, 2022; Dolls, Schüle, and Windsteiger, 2022).

This widespread support is puzzling given the strong consensus among economists that price caps reduce aggregate welfare.¹ Economists have long argued that when prices are set below market levels, producers face weaker incentives to supply and invest, while demand increases. The expected result is rationing, lower product quality, and the emergence of black markets (Cheung, 1974; Barzel, 1974; Friedman, 1975; Mankiw, 2020; Mulligan, 2025). Empirical evidence strongly supports these predictions (see, for example, Schuettinger and Butler (1979), Glaeser and Luttmer (2003), Diamond, McQuade, and Qian (2019), and Aparicio and Cavallo (2021)). A central question, then, is why consumers continue to support price caps despite their well-documented inefficiency. We address this question in a laboratory experiment that cleanly separates the roles of factual information, personal experience, and responsibility for policy outcomes in shaping beliefs and policy support.

Previous research suggests that citizens sometimes support inefficient policies—or oppose

¹See, for instance, the expert survey by the Kent A. Clark Center for Global Markets (2022).

efficiency-enhancing ones—because they misunderstand their effects. Following Blinder and Krueger (2004), several studies have documented belief distortions arising from misinformation and misunderstanding. For instance, individuals often fail to anticipate behavioral responses to policy (Dal Bó, Dal Bó, and Eyster, 2018) or misperceive tax incidence when it is less salient (Sausgruber and Tyran, 2011). Opponents of carbon taxes may neglect revenue redistribution and misjudge their personal benefits under tax-and-dividend schemes (Vora, 2023). Fiscal illusions, opaque tax design, and policy framing also limit support for efficiency-enhancing reforms (Sausgruber and Tyran, 2005; Kallbekken, Kroll, and Cherry, 2011), and people frequently misunderstand the social benefits of congestion pricing (Janusch et al., 2021).²

While misinformation and misunderstanding can explain some of the support for price caps, we hypothesize that these factors alone cannot fully account for their persistent popularity. Instead, we propose that support may be sustained by *self-serving beliefs*, which are encouraged by citizens’ lack of accountability for the consequences of their policy preferences.

We first develop a stylized model of consumer support for price caps in a competitive market. In the model, individuals form beliefs about the excess consumer surplus under a price-cap regime relative to a free-market regime and choose between the two based on these beliefs. Consumers can update their prior beliefs after either receiving information about the effects of the price cap or experiencing its consequences. Following the literature on motivated beliefs (Brunnermeier and Parker, 2005; Bénabou, 2015; Molnar and Loewenstein, 2022), we assume individuals care about how their beliefs align with their ideology or identity. In particular, consumers who favor government intervention may derive affective gratification from believing that price caps increase welfare. When forming beliefs about the price cap, consumers weigh the affective gratification of holding favorable views with the psychological discomfort and material costs associated with self-deception. Drawing on Caplan (2007), the model introduces agency, defined as having direct responsibility for policy outcomes, as a disciplining force that raises the material cost of maintaining distorted beliefs.

The model yields three key predictions about how consumers will respond to new information about the negative effects of price caps. First, both factual information and personal experience with price caps will lead consumers to revise their beliefs and reduce support for the policy. Second, personal experience will lead to larger belief revisions and a sharper decline in support, as it shapes economic beliefs more powerfully than factual information (Malmendier, 2021). This implies that despite the abundance of information about price

²Other studies have emphasized the role of fairness and moral concerns in attitudes toward policies. For example, Elias, Lacetera, and Macis (2022) find that opposition to surge pricing stems primarily from distributive concerns, while Landier and Thesmar (2022) link support for market mechanisms to moral worldviews.

caps, many citizens may continue to support them simply because they have not personally experienced product shortages. Third, when individuals are made responsible for policy outcomes, they will update their beliefs more sharply and show lower support for price caps, as decision-making responsibility increases the cost of self-deception. This mechanism helps to explain why consumers, who typically lack agency in policy decisions, tend to support price caps in opinion polls.

We tested these predictions using a laboratory experiment conducted with more than 800 university students in Bogotá, Colombia. The participants shopped for a fictitious commodity under monetary incentives. After engaging in a free-market regime, and before receiving any information about the potential outcomes of a price-cap regime, they chose between the two market regimes for subsequent shopping periods. After making their initial choice, they received information about the price-cap regime and were given the opportunity to update their choice of regime. In this setup, we implemented two independent interventions. First, participants were randomly assigned to one of two information conditions: some received factual information about the price cap, while others directly experienced its effects through market interaction. Second, we manipulated agency by assigning participants either a decisive role in implementing the market regime or a nonbinding, hypothetical role. In addition to eliciting market-regime choices, we elicited beliefs about the price cap before and after the information intervention.

This design allowed us to test how exposure to different sources of information and decision-making responsibility shape belief updating and support for the price cap. Importantly, by holding information constant across decision-making roles, our design controlled for individual differences in understanding of the effects of a price cap, thus allowing us to focus on the influence of motivated beliefs and agency. Our experimental design and the core reduced-form comparisons were pre-registered. Online Appendix II reproduces the original Pre-Analysis Plan (PAP) and documents how the analyses reported here map into the pre-specified tests, implementation clarifications, and theory-guided extensions.

Our findings align with the theoretical predictions. Initially, most of the participants had optimistic beliefs about the price cap and strongly supported it. Both factual information and direct experience led to the revision of beliefs and reduced support, but the effect was significantly stronger among those who personally experienced product shortages. Nevertheless, belief distortions proved remarkably persistent: A substantial share of participants continued to support the price cap even after directly experiencing its negative effects. However, participants with decision-making power were significantly more responsive to both types of information than those whose market-regime choices were hypothetical.

To our knowledge, this is the first study to provide a clean causal comparison of the

effects of factual information and direct experience on the *same* economic policy attitude—specifically, support for a price cap—while holding the policy environment fixed. Survey experiments examining public attitudes toward consumer-goods price caps (Elias, Lacetera, and Macis, 2022), rent control (Müller and Gsottbauer, 2022; Dolls, Schüle, and Windsteiger, 2022; Brandts et al., 2022), or minimum wages (Mishagina and Montmarquette, 2021) have focused exclusively on the role of factual information, as they could not manipulate experience exogenously. Conversely, research emphasizing the role of lived experience in shaping economic beliefs—such as Janusch et al. (2021) on congestion pricing, Weber et al. (2023) on inflation expectations, and Malmendier and Wachter (2024) on macroeconomic events—has not tested the role of factual information. By manipulating both factual information and experience through exogenously induced product shortages, our design allows for a direct comparison of how these two sources of learning shift beliefs and support for price caps.

Our study also contributes to the literature on political agency by testing whether accountability disciplines policy attitudes and belief distortion. The most closely related work is Robbett and Matthews (2018), which examines how perceived pivotality in group decisions affects partisan bias.³ Unlike that study, we focus on attitudes toward government intervention in markets rather than political partisanship, measuring endorsement of a pervasive economic policy such as price caps. Additionally, we compare individuals who are certain to be decisive with others who are certain to be nondecisive, instead of comparing individuals with varying but positive likelihoods of being decisive. One concern with the latter approach is that because of optimism bias (Sharot, 2011) or the illusion of control (Langer, 1975), individuals may overestimate their agency in voting schemes even when their vote is actually negligible. By removing the voting frame and comparing only the two extremes of the agency spectrum, we avoid any confounding influence of misperceptions about agency. This feature enables a sharp comparison between the position of a policy maker and that of a person surveyed in an opinion poll.

Our results yield two broader takeaways. First, they underscore the limited effectiveness of information alone in correcting motivated beliefs about price caps. Second, they show that agency moderates belief formation and policy choice: When individuals are accountable for implementation, beliefs and choices become more disciplined. These insights extend beyond price caps, offering a broader explanation for the enduring popularity of economically

³In that study, American political partisans were asked to answer a series of factual political questions. The participants either answered the questions for themselves or were selected into a group of 5 or 25 participants who voted on the answer. For each question that the majority of the group answered correctly, all group members received a financial reward. The study was designed to examine the hypothesis that as the perceived likelihood of being pivotal decreases, participants' votes should become more expressive of partisan affinities.

inefficient policies. As argued by Caplan (2007), when individuals bear no responsibility for policy outcomes, motivated beliefs can flourish. Furthermore, when policy choices are symbolic, citizens may have little incentive to significantly revise optimistic beliefs, even in the face of contradictory evidence. As demonstrated by Levy (2014), this may result in a political equilibrium in which pandering politicians avoid costly reforms and instead cater to public demand for superficially comforting policies. Once enacted, such policies become politically costly to remove because of citizens’ entrenched expectations and support, further reinforcing their persistence.

The remainder of the paper is organized as follows. Section 2 presents the theoretical model, which lays the groundwork for the laboratory experiment. Section 3 describes the experiment. Section 4 provides key descriptive statistics for our study sample. Section 5 details the empirical strategy adopted to test the theoretical predictions and presents the main results. Section 6 relates our theoretical model and the empirical results to alternative models of support for inefficient policies. Section 7 concludes with implications for policy design and directions for future research.

2 Theoretical model

In this section, we develop a stylized model of consumer support for a price cap in a competitive market. This model builds on the rational irrationality framework for political views and voting behavior (Caplan, 2007) and formalizes a simple intuition: Individuals may find it psychologically attractive to believe that a price cap works, but sustaining such optimistic beliefs is harder when the policy choice is consequential for them. Thus, when a citizen only reports a preferred policy (as in an opinion poll), she can easily indulge in motivated optimism; but when her choice is implemented (as for a policy maker), distorted beliefs become costly because they increase the likelihood of adopting a regime she would otherwise regard as inferior. Therefore, agency helps to discipline beliefs.

In Section 6, we discuss how our model differs from the rational irrationality framework and other standard frameworks in political economy addressing citizens’ support for inefficient policies, such as rational choice (Downs, 1957) and expressive voting (Brennan and Lomasky, 1993). Section A in Online Appendix I contains the proofs of the propositions presented in this section.

2.1 Setup

Consider a competitive market for a homogeneous commodity in which consumers seek to buy one unit and sellers seek to sell one unit. While consumers' valuations of the commodity and sellers' costs are private information, there is a uniform price p that is known to all market participants. For simplicity, we focus on a representative consumer (“the consumer”) whose valuation is $v > p$. While the consumer would like to buy the product, she does not know in advance whether it will be available. Before shopping, she forms a belief about the probability of finding the product (“the probability of buying”) at price p , which we denote by $\hat{P}(buy|p) \in [0, 1]$. Hence, her expected surplus from going shopping is $(v - p) \hat{P}(buy|p)$.

The market functions under one of two regimes: (1) free market, in which $p = p^*$ (the competitive equilibrium price), or (2) price cap, in which $p = \bar{p} < p^*$. The price cap reduces the probability of buying, but, as we shall see, the consumer's beliefs need not reflect this pattern. We define $\hat{\phi}$ as the *excess surplus* the consumer expects to attain in the price-cap regime relative to the free-market regime:

$$\hat{\phi} := (v - \bar{p}) \hat{P}(buy|p = \bar{p}) - (v - p^*) \hat{P}(buy|p = p^*) \quad (1)$$

$\hat{\phi}$ is strictly decreasing in $[\hat{P}(buy|p = p^*) - \hat{P}(buy|p = \bar{p})]$, which represents the perceived excess probability of buying in the free-market regime.

2.2 Consumer's choice of market regime

Before going shopping, the consumer has the opportunity to choose which of the two market regimes she would like to face. However, her choice may or may not be binding. The consumer's *agency* in regime implementation falls into one of two extreme cases. If she is a *decisive* decision-maker (“Decisive DM”), she knows that her choice of market regime will be implemented with certainty; this is akin to being a policy maker. By contrast, if she is a *hypothetical* decision-maker (“Hypothetical DM”), she knows that her choice of market regime has no chance of being implemented; this is akin to reporting a regime preference in an opinion poll. The consumer knows whether her choice will be decisive or hypothetical. Additionally, her choice is anonymous. (Anonymity rules out audience effects, social pressure, and reputational concerns as potential drivers of market-regime choices.)

To model regime choice, we draw on the standard calculus-of-voting framework, in which voters weigh the costs and expected benefits associated with casting a vote (Downs, 1957; Riker and Ordeshook, 1968). Here, choosing a regime is costless and mandatory, and hence we focus on how a consumer makes her choice and abstract from whether the consumer would make a choice at all if choosing were costly. The consumer weighs instrumental and

psychological motivations when deciding on a market regime.

The instrumental motivation concerns the influence of the consumer's regime choice on her expected surplus. For a Decisive DM, the instrumental value of choosing a regime equals its expected surplus. By contrast, for a Hypothetical DM, the instrumental value of choosing either regime is zero.⁴

When making her choice of market regime, the consumer is also guided by the psychological motivation to be consistent: If she deliberately chooses a regime despite expecting a surplus deficit, she experiences cognitive dissonance because her choice is inconsistent with her belief (Festinger, 1962; Wicklund and Brehm, 1976). Specifically, the dissonance cost of choosing the price-cap (free-market) regime is given by $D(-\hat{\phi})$ ($D(\hat{\phi})$). The function D satisfies $D(x) > 0$ if $x > 0$ and $D(x) = 0$ if $x \leq 0$. Hence, the consumer incurs a dissonance cost when choosing the price-cap (free-market) regime if $\hat{\phi} < 0$ ($\hat{\phi} > 0$). We further assume that $D'(x) > 0$ and $D''(x) > 0$ for $x > 0$.

Overall, the total utility from choosing the price-cap regime is $U_{cap} := 1^d (v - \bar{p}) \hat{P}(buy|p = \bar{p}) - D(-\hat{\phi}) + \epsilon_{cap}$, and the total utility from choosing the free-market regime is $U_{free} := 1^d (v - p^*) \hat{P}(buy|p = p^*) - D(\hat{\phi}) + \epsilon_{free}$, where 1^d is an indicator for a Decisive DM and ϵ_{cap} and ϵ_{free} represent idiosyncratic consumer preferences (McFadden, 1981; Anderson, De Palma, and Thisse, 1992).⁵ Following standard empirical models of consumer behavior (Nevo, 2011), we assume that ϵ_{cap} and ϵ_{free} are independent and follow a Type I extreme value distribution—that is, their cumulative distribution function F satisfies $F(\epsilon) = \exp(-e^{-\epsilon})$.

The consumer chooses the price-cap regime if, and only if, $U_{cap} \geq U_{free}$. Therefore, the probability of choosing the price-cap regime is

$$Prob(cap) = \frac{1}{1 + \exp\{-1^d \hat{\phi} - D(\hat{\phi}) + D(-\hat{\phi})\}}. \quad (2)$$

It follows from expression (2) that $Prob(cap)$ is increasing in $\hat{\phi}$, which means that the choice of market regime tends to be consistent with surplus expectations for both types of DM. In the absence of a dissonance cost (i.e., if $D(\hat{\phi}) = 0 \forall \hat{\phi}$), a Hypothetical DM's regime choice would *not* respond to changes in $\hat{\phi}$ because surplus expectations do not have instrumental value. The only reason why a Hypothetical DM would be inclined to act on $\hat{\phi}$ is to avoid cognitive dissonance.

⁴In practice, a Decisive DM's choice also affects the surplus of other consumers, not just her own. Assuming that a Decisive DM internalizes total consumer surplus, rather than only her own surplus, would strengthen the difference in instrumental motivation between Decisive and Hypothetical DMs and therefore would not affect the main predictions of the model.

⁵Such idiosyncratic preferences may be influenced by fairness or distributional considerations. We do not model these factors explicitly because they are not the primary focus of our study.

2.3 Consumer’s excess-surplus expectation

In contrast to standard models of consumer behavior, which assume that beliefs only serve as a guide for decision-making, we allow surplus expectations to be a direct source of utility for the consumer. The consumer holds a “statist ideology” (Bénabou, 2008) that supports widespread government intervention in the economy. As a result, she attaches affective value to $\hat{\phi}$ because a more positive view about the price-cap regime reinforces her ideology (Caplan, 2007; Bénabou and Tirole, 2016). We capture the affective utility derived from the excess-surplus expectation with the function $B(\hat{\phi})$, where $B' > 0$ and $B'' < 0$.

Because believing that the price cap increases consumer welfare makes the consumer feel good, she may engage in motivated reasoning, strategically manipulating her excess-surplus expectation (Caplan, 2007; Bénabou and Tirole, 2016). This manipulation implies a departure from a detached, intellectually honest excess-surplus expectation, which we denote ϕ .⁶

The consumer faces two potential costs from holding an unrealistically optimistic view about the price-cap regime (i.e., $\hat{\phi} > \phi$). First, when the consumer realizes she is distorting her excess-surplus expectation relative to her honest opinion, she may experience discomfort (Rabin, 1994; Konow, 2000; Sautua, 2022). We capture this self-deception cost with the function $S(\hat{\phi} - \phi)$, where $S > 0$ if $\hat{\phi} \neq \phi$ and $S = 0$ otherwise. We further assume that $S' < 0$ if $\hat{\phi} < \phi$, $S' > 0$ if $\hat{\phi} > \phi$, and $S'' > 0$.

Second, departures from the honest view about the price-cap regime distort decision-making. Motivated optimism increases the consumer’s probability of supporting the price-cap regime from $Prob(cap|\phi)$ to $Prob(cap|\hat{\phi})$ (see expression (2)). If the consumer genuinely believes that the free market outperforms the price-cap regime (i.e., $\phi < 0$), motivated optimism increases the consumer’s likelihood of making the wrong choice of regime, which would result in a surplus deficit of ϕ . (By the same token, there is an instrumental cost of maintaining a pessimistic expectation $\hat{\phi} < \phi$ if $\phi > 0$ because doing so reduces the probability of choosing the price-cap regime—which is honestly regarded as the best option—from $Prob(cap|\phi)$ to $Prob(cap|\hat{\phi})$.) Only a Decisive DM bears this instrumental cost of distorted beliefs. In summary, the expected instrumental cost of holding the belief $\hat{\phi}$ is given by $C(\hat{\phi}) := 1^d 1^{\{\phi < 0, \hat{\phi} > \phi\}} \left(Prob(cap|\hat{\phi}) - Prob(cap|\phi) \right) |\phi| + 1^d 1^{\{\phi > 0, \hat{\phi} < \phi\}} \left(Prob(cap|\phi) - Prob(cap|\hat{\phi}) \right) |\phi|$.

Following Brunnermeier and Parker (2005), Caplan (2007), and Bénabou and Tirole (2016), we assume that the consumer chooses her excess-surplus expectation $\hat{\phi}$ before choosing the market regime, anticipating that she will then carry her belief into the regime decision-

⁶Although the expectation is honest, ϕ may be miscalibrated because of misinformation or cognitive mistakes in information processing. For example, $\hat{P}(buy|p = \bar{p})$ may be biased upward because of a misunderstanding of the equilibrium effects of a binding price cap (Dal Bó, Dal Bó, and Eyster, 2018).

making process as described in Section 2.2. Overall, when choosing $\hat{\phi}$, the consumer balances her taste for a positive view about the price cap, the disutility of self-deception, and the expected instrumental cost of unrealistic beliefs. The consumer solves the following problem:

$$\begin{aligned} \text{Max}_{\hat{\phi}} \quad & \left[B(\hat{\phi}) - S(\hat{\phi} - \phi) - C(\hat{\phi}) \right] \\ \text{s.t.} \quad & \hat{\phi} \in [-(v - p^*), v - \bar{p}]^7 \end{aligned} \tag{3}$$

Without loss of generality, we assume an interior solution to (3). The consumer will be inclined to be unrealistically optimistic about the price-cap regime—that is, to choose $\hat{\phi} \geq \phi$ (see Lemma 1 in Section A of Online Appendix I). This is a straightforward implication of the consumer’s taste for positive views about the price-cap regime. We next focus on two factors that may reduce motivated optimism and hence endorsement of the price-cap regime: (1) new information about the reduced probability of buying in the price-cap regime and (2) agency in regime implementation.

2.4 The roles of information and agency

2.4.1 Information about the reduced probability of buying in the price-cap regime

We first analyze how new information about the reduced probability of buying in the price-cap regime affects $\hat{\phi}$ and $Prob(cap)$. We employ a “0” subscript to denote prior excess-surplus expectations and market-regime choice and a “1” subscript to denote their posterior counterparts. Decisive and Hypothetical DMs feature the same *qualitative* response to information, which we characterize in the following proposition.

Proposition 1 *Suppose new information about the probability of buying in the price-cap regime leads to $\phi_1 < \phi_0$. Then,*

1. $\hat{\phi}_1 < \hat{\phi}_0$;
2. $Prob_1(cap) < Prob_0(cap)$;
3. $\hat{\phi}_1$ and $Prob_1(cap)$ are strictly increasing in ϕ_1 .

⁷The lowest possible value of $\hat{\phi}$, $-(v - p^*)$, occurs when $\hat{P}(buy|p = \bar{p}) = 0$ and $\hat{P}(buy|p = p^*) = 1$. The largest possible value of $\hat{\phi}$, $v - \bar{p}$, occurs when $\hat{P}(buy|p = \bar{p}) = 1$ and $\hat{P}(buy|p = p^*) = 0$.

Proposition 1 considers a consumer who receives new information about the price-cap regime that would lead her to become adopt a less positive view about such regime if she were honest. A less positive honest view about the price cap increases the costs of motivated optimism and thus induces the consumer to believe that the price-cap regime is indeed worse than she originally thought (part 1). The downward adjustment in $\hat{\phi}$ makes the consumer less likely to choose the price-cap regime (part 2). Moreover, the larger the downward adjustment in the honest excess-surplus expectation, the larger the reduction in the optimal expectation, which in turn results in a larger reduction in the probability of choosing the price-cap regime (part 3).

Part 3 of Proposition 1 allows us to incorporate *experience effects* in a simple fashion. This term describes “the empirical finding that individuals living through and personally experiencing the realizations of macro, finance, and other economic processes respond to these experiences differently from people who are fully informed about the same outcomes, but did not personally experience them” (Malmendier 2021, p. 2858). In our setup, experience may be more effective than factual information in correcting a consumer’s faulty representation of how the market works. This can be accommodated by the following premise: Compared to a consumer who is simply informed that the price cap reduces the probability of buying, a consumer who experiences product shortages in the price-cap regime holds a smaller honest excess-surplus expectation, ϕ_1 . As a result, compared to factual information, personal exposure to the price-cap regime leads to a larger reduction in $\hat{\phi}$ and hence in $Prob(cap)$ even in the presence of motivated reasoning.

2.4.2 Agency in regime implementation

While new negative information about the price cap reduces the distortion in decision-making for both types of DM, its *quantitative* effect may depend on an individual’s decision-making role. We now analyze how agency in regime implementation affects $\hat{\phi}_1$ and $Prob_1(cap)$. We focus on a consumer whose honest prior belief is that the price-cap regime yields the largest surplus (i.e., $\phi_0 \geq 0$) and who then receives information that would lead her to change her mind if she were honest (i.e., $\phi_1 < 0$).

Proposition 2 *Suppose that $\phi_0 \geq 0$ and that new information about the probability of buying in the price-cap regime leads to $\phi_1 < 0$. Then,*

1. $\hat{\phi}_1$ will be smaller for a Decisive DM than for a Hypothetical DM;
2. $Prob_1(cap)$ will be smaller for a Decisive DM than for a Hypothetical DM if $\hat{\phi}_1 < 0$ for a Decisive DM.

To interpret Proposition 2, note that when $\phi_0 \geq 0$, both types of DM share the same *prior* excess-surplus expectation (see Lemma 2 in Section A of Online Appendix I). Additionally, recall (from part 1 of Proposition 1) that new negative information about the price cap leads both types of DM to become less optimistic about this policy. If the news was so compelling that an honest consumer would come to regard the price-cap regime as the worst option (i.e., $\phi_1 < 0$), a Decisive DM would hold a smaller posterior excess-surplus expectation compared with a Hypothetical DM (part 1). This is because when the news about the price cap is compelling, motivated optimism has an extra cost for a Decisive DM, as it increases the likelihood of implementing what she honestly regards as the worst option. Additionally, because a Decisive DM displays a stronger tendency to act on her (already less positive) belief compared with a Hypothetical DM, she is less likely to choose the price-cap regime (part 2).

We highlight the key role played by the consumer’s taste for positive views about the price cap in Proposition 2. If the consumer did not enjoy holding positive views about the price cap (i.e., $B(\hat{\phi}) = 0 \forall \hat{\phi}$), she would not have any incentive to engage in motivated reasoning, and hence her optimal excess-surplus expectation would equal her honest expectation.⁸ An important implication of setting $B(\hat{\phi}) = 0 \forall \hat{\phi}$ is that the excess-surplus expectation of a Decisive DM always coincides with that of a Hypothetical DM. Therefore, the Decisive-Hypothetical posterior expectations gap stated in part 1 of Proposition 2 is a smoking gun for motivated reasoning.

3 Experiment

3.1 General aspects

We conducted a laboratory experiment between October 2022 and March 2023 at Universidad del Rosario, in the city of Bogotá, using subjects randomly drawn from the student pool managed by the Rosario Experimental and Behavioral Economics Laboratory (REBEL). Using the Online Recruitment System for Economic Experiments (Greiner, 2004), we invited students from several universities and with several academic majors to attend a laboratory session at REBEL. Invitations were sent at various times, and a unique set of randomly chosen individuals from the large subject pool received the invitation each time. Those who accepted our invitation were immediately asked to complete an online survey, which collected information on socioeconomic characteristics, political tendency, and attitudes toward gov-

⁸To see this, substitute $B(\hat{\phi}) = 0$ into (3) and note that the first-order condition for utility maximization is $-S'(\hat{\phi} - \phi) - C'(\hat{\phi}) = 0$, which implies that $\hat{\phi} = \phi$ at the optimum.

ernment intervention in the economy.

A few days after answering the survey, the participants attended the laboratory session. We recruited a total of 821 individuals who participated in 1 of 45 laboratory sessions, which lasted for approximately 45 minutes. In each session, the participants were divided into groups of four. Using information from the pre-experiment survey, we constructed two indexes, one for political tendency and another for attitudes toward government intervention in the economy, and combined them into a single sorting measure. Within each session, participants were ranked according to this measure and assigned to groups based on adjacent ranks, so that members of each group had similar pre-experiment political and interventionist attitudes.⁹

Participants did not know the identities of those with whom they had been matched and were not allowed to communicate during the session. The experiment was computerized and the interface was programmed in oTree (Chen, Schonger, and Wickens, 2016). Payments were made through an online bank transfer within 48 hours of the end of the session. All the information collected in the survey and the laboratory experiment was anonymous. Before completing the survey, each respondent followed written instructions to create a unique personal code that was known only to her. We used these personal codes to match the participants' survey responses to the data from the experiment.

Section C of the Online Appendix I contains the survey translated into English. Section F contains the experimental procedures and instructions translated into English. The pre-registered design is described in PAP Part I, Section 3, and the mapping between the planned design and its implementation is summarized in Online Appendix II, Sections 2–3.

3.2 The commodity market

The participants engaged in a shopping exercise that recreated the market environment modeled in Section 2. All four members of a group were buyers who shopped for a hypothetical commodity under monetary incentives. Payoffs were expressed in experimental currency units (ECUs), which were converted into Colombian pesos (COP) at the rate 1 ECU = COP 12,000 (equivalent to approximately USD 2.5 at the time of the experiment). In this commodity market, there were four automated sellers. At the beginning, there was a trial

⁹The political tendency index combines respondents' self-placement on a left-right scale and their reported closeness to the political party Pacto Histórico, recoded so that higher values indicate more left-leaning positions. The index for attitudes toward government intervention combines eight survey items measuring support for state ownership, redistributive policies, pro-poor spending, taxation of the rich, price controls, minimum wages, interest-rate regulation, and import regulation, recoded so that higher values indicate stronger support for government intervention. Each item was standardized before aggregation. In the main specifications, we use inverse-covariance weighted indices following Anderson (2008); equal-weighted versions (Kling et al., 2004) produce very similar measures.

block of two unpaid shopping periods to allow buyers to familiarize themselves with the market. The trial block was followed by four blocks of eight incentivized shopping periods each. Within a block, all periods featured the same market regime: free market or price cap.

Following Smith et al. (1982) and Sausgruber and Tyran (2011), we used a simplified uniform-price sealed-bid/offer auction as the trading institution. Each buyer submitted a “purchase order” to buy one unit of the product by bidding their valuation. Each automated seller submitted an offer, equal to her cost, at which she would sell one unit of the product. Bids and offers were private information.

Market outcomes were determined as follows. In each period, bids were sorted from highest to lowest, whereas offers were sorted from lowest to highest. Each bid was then compared with its corresponding offer. In periods featuring a free-market regime, all bids exceeding their corresponding offers were accepted. The offer corresponding to the last unit traded determined the uniform market price p^* for all units traded. In periods featuring the fully enforceable binding price cap $\bar{p} < p^*$, units for which the cost exceeded $\bar{p}$ were not accepted in trade because sellers were not allowed to sell at a loss. A buyer’s surplus was the difference between the induced value and the uniform market price—or zero, if she was not able to buy the product.

This trading institution had three important advantages for our purposes. First, because buyers were required to bid their true value and sellers submitted offers at true cost, market outcomes were mechanically consistent with theoretical equilibrium outcomes, implying that the price cap always created excess demand and hence was inefficient in our setup. Second, shopping in our market was straightforward, thus allowing buyers to concentrate on the choice of market regime (explained below), which was the main focus of the study. Third, this trading institution allowed us to automate sellers, and seller automation was known to buyers. This enabled us to rule out considerations regarding the fairness of the distribution of total surplus between buyers and sellers.

In every block, each buyer was assigned, in random order, a valuation of 12 ECU in one-half of the shopping periods and 11 ECU in the remaining half. The assignment kept the demand schedule constant across periods, so that in every period two buyers valued the product at 12 ECU and the other two valued it at 11 ECU. Thus, while buyers differed in their valuations in any given period, their induced preferences were identical over the whole block. As a result, all buyers had an identical overall shopping experience over a block. Homogeneity in shopping experience for a given market regime was intended to reduce noise in the data and thus increase the statistical power of the experiment.

Figure 1 displays the demand and supply schedules in a shopping period, as well as the market price, purchases, and surpluses, under the different market regimes.

[Figure 1 about here]

In shopping periods with a free-market regime, there was a unique competitive equilibrium of four units traded at a price of 9 ECU. Buyers obtained a surplus of 2 or 3 ECU, depending on their valuation. By contrast, in periods with a price cap set to 7 ECU, only two buyers (who valued the product at 12 ECU) made a purchase. These buyers achieved a surplus of 5 ECU, whereas the surplus of the remaining two buyers was 0 ECU. Thus, on the demand side, the price cap reduced the probability of making a purchase from 100% to 50%.¹⁰

3.3 Payment scheme

The participants received a participation fee of COP 15,000 (equivalent to approximately USD 3 at the time of the experiment). At the beginning of the session, they were informed that they would participate in four blocks of shopping periods and two other tasks and that either one shopping period or one of the two remaining tasks would be randomly selected for payment at the end of the session. The payment was added to the participation fee.

3.4 Timeline of a laboratory session

Figure 2 summarizes the timeline of a laboratory session, which we also describe here.

[Figure 2 about here]

Randomization into information treatment arms

Before taking part in the commodity market, all participants in a given laboratory session were randomly assigned to one of three *information* treatment arms: Experience, Factual Information, and Reverse Experience.¹¹ In the remainder of this section, we describe the Experience and Factual Information treatment arms, which we used to test the predictions of the theoretical model. We defer the discussion of the supplementary Reverse Experience treatment arm, which we used to perform a robustness check, to Section 5.2.

¹⁰In any given period, the price cap also increased the inequality in the between-subjects distribution of consumer surplus compared with the free-market regime. However, after shopping, the participants observed only their own surplus and therefore did not learn the between-subjects distribution; and, as mentioned earlier, the within-subject distribution of surplus over a block of periods was identical across buyers.

¹¹The treatment-arm labels used here correspond to the PAP labels FM-E, FM-I, and PC-E, respectively.

Personal exposure to the free-market regime

In the Experience and Factual Information treatment arms, both the trial block and block 1 of the shopping exercise featured a free-market regime. Our goal was to provide the participants with personal experience with free-market outcomes.

Task: Elicitation of the perceived probability of buying in the free-market regime

Once block 1 of the shopping exercise concluded, the participants were reminded of the market price (9 ECU), the number of periods in that block in which they had made a purchase (eight out of eight), and their earnings (2 ECU in four periods and 3 ECU in the other four). Next, they reported their beliefs about the frequency of purchases in their group during block 1. Specifically, they answered the following question: “In each period, the number of buyers in your group who were able to make a purchase was 0, 1, 2, 3, or 4. Please indicate how many periods you believe the number of buyers who were able to make a purchase was 0, how many periods it was 1, how many it was 2, how many it was 3, and how many it was 4.”

The participants provided their answers using a graphical interface in which they had to position circles on a grid. The interface was explained using a few examples, one of which is shown in Figure 3. This example represents the belief that in four periods no buyer was able to buy, in three periods only one buyer made a purchase, and in the remaining period only two buyers were able to buy. The participants’ reports indirectly convey their perceived probability distributions over group purchases in the free-market regime. In the example, the fictitious buyer believed that 50% of the time there were no purchases, 37.5% of the time only one buyer was able to buy, and 12.5% of the time two buyers managed to buy.

[Figure 3 about here]

We used the participants’ reported beliefs on group purchases to infer their beliefs about the probability of buying in the free-market regime. We calculated a subject’s *perceived probability of buying* (denoted by $\hat{P}(buy|p)$ in the model in Section 2) as $\frac{1}{4} \cdot E(purchases)$, where $E(purchases)$ is the mean over the perceived distribution of group purchases. To illustrate this calculation, consider our running example from Figure 3. We infer that the number of group purchases expected by the fictitious buyer is $E(purchases) = \frac{4}{8} \cdot 0 + \frac{3}{8} \cdot 1 + \frac{1}{8} \cdot 2 + \frac{0}{8} \cdot 3 + \frac{0}{8} \cdot 4 = 0.625$. To calculate the perceived probability of an individual purchase, we divide $E(purchases)$ by 4, because there were four buyers in each group: $\hat{P}(buy|p) = \frac{1}{4} \cdot 0.625 = 0.16$.

This belief-elicitation task has several noteworthy aspects. First, we did not provide financial incentives for accuracy because doing so could have attenuated the bias resulting

from motivated reasoning, and we wanted to preserve the bias, as it may be a key determinant of consumers’ support for a price-cap regime.¹² Second, we inferred perceived probabilities of buying instead of directly asking the participants to report them because many individuals have difficulties in understanding, interpreting, and reporting probabilities (Tversky and Kahneman, 1974). Third, following best-practice recommendations (see, for example, Delavande (2014)), we used a visual interface to reduce cognitive demands and minimize confusion.

Announcement of the implementation of the price-cap regime

Once the participants had reported their beliefs about block 1 of the shopping exercise, those in the Experience treatment arm were informed that they would participate in a second block with the same structure as that of the first, except for one feature: the imposition of a price cap of 7 ECU. By contrast, the participants in the Factual Information treatment arm were asked to imagine a hypothetical block of shopping periods featuring a price cap of 7 ECU.¹³

Randomization into agency treatment arms

Before participating in block 2 of the shopping exercise, the participants were informed that they would have the opportunity to choose the regime to be implemented in blocks 3 and 4, namely a free market or a market with the 7 ECU price cap. Specifically, the participants were told that one buyer in each group would be randomly assigned to choose the regime for blocks 3 and 4 in her group, whereas the three remaining buyers would make hypothetical choices. Next, the computer program informed each subject whether she would be a Decisive DM or a Hypothetical DM. (We did not use these terms in the experimental instructions. We use them here to emphasize the connection with the theoretical model.)

This treatment manipulated the participants’ agency in regime implementation, making one buyer per group directly responsible for the decision on the market regime. By announcing decision-making roles at this point, our agency treatment had the potential to influence beliefs about the price-cap and free-market regime choices both before and after the participants received information about market outcomes under the price cap.

¹²Bullock et al. (2015), Prior, Sood, and Khanna (2015), and Robbett and Matthews (2018) find that partisan gaps in responses to factual political surveys in the US can be reduced with financial incentives or appeals to accuracy.

¹³The participants in both treatment arms were informed that automated sellers would not be allowed to make offers above 7 ECU, which ensured that the market price would not exceed 7 ECU.

Task: Elicitation of the prior probability of buying in the price-cap regime

Next, the participants in both treatment arms reported their perceived probability distributions over group purchases for the upcoming block 2, which had been announced to feature a 7 ECU price cap. We used the participants' reported beliefs on group purchases to infer their perceived prior probabilities of buying in the price-cap regime.

Task: Initial choice of market regime

Immediately after reporting their prior beliefs for block 2 of the shopping exercise, and before participating in block 2, the participants chose the market regime for block 3. (Subjects knew that the choice of the Decisive DM would not be revealed until the beginning of block 3.) The initial regime choice was intended to uncover buyers' support for the price cap *before* they received information about its market outcomes.

Implementation of the information intervention

Once the participants had made their initial choice of market regime, those in the Experience treatment arm completed the second block of shopping periods, thus gaining personal experience with the price-cap regime. Once block 2 finished, the participants were reminded of the market price (7 ECU), the number of periods in that block in which they had made a purchase (four out of eight), and their earnings (5 ECU in four periods and 0 ECU in the other four). By contrast, the participants in the Factual Information treatment arm, who had simply been asked to imagine the price cap during the second block, were informed of the frequency of total purchases in another group that had actually experienced the price cap. We shared the experience of the benchmark group with the participants in the Factual Information treatment arm using the same visual interface that we used to elicit beliefs. Figure 4 reproduces the graphical display of information, which showed that only 2 buyers had made a purchase in each of the eight periods. A legend situated below the graph mentioned that the market price had been 7 ECU in all periods.

[Figure 4 about here]

Our information intervention allowed us to manipulate the *source* of new information about the price cap: While the participants in the Experience treatment arm personally experienced the price cap, those in the Factual Information treatment arm received factual information about its effect on the quantity traded and market price. The way in which we presented the information to the participants in the Factual Information treatment arm has two main features that generally help to boost the effectiveness of information-provision

interventions (Haaland, Roth, and Wohlfart, 2023). First, our short and neutrally framed presentation minimized concerns about experimenter demand effects. Second, the graphical illustration of information was intended to increase the participants’ understanding of the message, which is crucial for the message to significantly affect beliefs.

Task: Elicitation of the posterior probability of buying in the price-cap regime

Next, the participants in both treatment arms were given the opportunity to revise their perceived probability distributions over group purchases for block 2. We used the participants’ reported beliefs on group purchases to infer their perceived posterior probabilities of buying in the price-cap regime.

Task: Final choice of market regime

After reporting their posterior beliefs for block 2 of the shopping exercise, and before participating in block 3, the participants chose the market regime for block 4. (Subjects knew that the choice of the Decisive DM would not be revealed until the beginning of block 4.) They were reminded of their previously assigned decision-making roles (Decisive vs. Hypothetical) before making a decision. The final regime choice was intended to uncover buyers’ support for the price cap *after* they received information about its market outcomes.

Participation in the third and fourth blocks of shopping periods

Once all participants had indicated their choice for block 4 of the shopping exercise, they participated in blocks 3 and 4 under the regime chosen by the Decisive DM for each block.

Task: Lottery and allocation choices

After participating in the shopping exercise, the participants made a choice between two individual gambles and then performed a distribution task. (See Section B in Online Appendix I for details on these two tasks. To control for order effects, in one-half of the sessions of each treatment arm, the participants faced these tasks before the commodity market was introduced.) We used these tasks to elicit the participants’ risk and distributional preferences, which could influence the choice of regime because this choice involved two risky gambles with distributional consequences. We used our experimental measures of risk and distributional preferences as control variables to boost statistical power.

4 Descriptive statistics of the study sample

Table A1 in Online Appendix I presents descriptive statistics for the participants in our experiment. The participants were on average 21 years old, 55% were women, 74% were from Bogotá, 63% lived in low- or middle-income neighborhoods, 36% identified as white, and 60% were enrolled at Universidad del Rosario, with the vast majority of these enrollees being undergraduate students. While only 24% were economics or finance majors, half of the participants had taken at least one economics course. Table A2 (A3) presents the balance tests for the information (agency) treatment arms. The socioeconomic characteristics of the participants are broadly balanced, with a few exceptions in which the differences are small and therefore may be due to sampling variability.

Figure A1 in Online Appendix I provides insight into the political tendency of the participants. Nearly 70% leaned to the left, with approximately 25% reporting values of 1, 2, or 3 on a 1–10 scale of political tendencies, which can be considered extreme-left positions. In contrast, only around 10% of the participants reported values of 8, 9, or 10 on this scale, indicating that a small fraction held extreme-right political views.

Figure A2 presents participants’ support for a range of interventionist policies, including price caps on food and medicines, interest rate caps, import restrictions, and a minimum wage. The data consistently indicate substantial support for these policies. Approximately 75% supported price caps on food and medicines (i.e., they reported support greater than 5 on a 1–10 scale), nearly 65% supported interest rate caps, 75% endorsed import restrictions, and nearly 90% supported a minimum wage. Notably, this pattern was also found among economics/finance majors and those who had completed at least one economics course.¹⁴

Overall, the survey data on political tendency and attitudes toward government intervention in the economy suggest that the participants in our study predominantly adhered to a “statist ideology” (Bénabou, 2008). Because this is a key premise of our theoretical model (see Section 2.3), we regard the study sample as suitable to test our theoretical predictions.

¹⁴While support from the latter group may be surprising, it is consistent with previous findings that standard instruction based on lectures on economic theory may not be sufficient to eradicate misconceptions about economic issues. For instance, Busom, Lopez-Mayan, and Panadés (2017) show that a one-semester formal course in economics hardly affected students’ belief that rent control is an effective policy to increase the availability of affordable housing.

5 Empirical results

5.1 Empirical strategy

In this section, we test the predictions from the model outlined in Section 2. Recall that the model features a consumer with a statist ideology who enjoys believing that a price-cap regime increases consumer welfare. The model then characterizes the consumer’s support for the price cap as the outcome of motivated beliefs. Its main predictions are that (1) both factual information about the consequences of the price cap and personal experience with the consequences will reduce belief distortions and support for the price-cap regime, (2) personal experience will have stronger effects than factual information, and (3) both types of information will have stronger effects among individuals assigned direct responsibility for policy implementation (i.e., Decisive DMs) than among those with no responsibility (i.e., Hypothetical DMs). The information and agency interventions embedded in our experimental design allow us to test these predictions.

The empirical analysis focuses on two primary outcomes that map directly onto the model: (1) beliefs about the excess surplus in the price-cap regime and (2) the choice between the two market regimes. We take a participant’s perceived excess probability of buying in the free-market regime—that is, $\hat{P} := [\hat{P}(\text{buy}|p = p^*) - \hat{P}(\text{buy}|p = \bar{p})]$ —as a proxy for the perceived excess surplus of the price-cap regime. This is a suitable proxy because, in theory, the excess surplus the consumer expects to attain in the price-cap regime is strictly decreasing in $\hat{P}$ (see expression (1)). For ease of exposition, our main analyses are based on a dummy variable that equals 1 if $\hat{P} > 0$ —that is, if a participant believed that the price cap reduces the probability of buying relative to the free market—but we show in supplementary analyses that the results are robust to using the underlying continuous excess-probability measure instead of this dummy variable. As described in Section 3.4, beliefs and regime choices were measured both before participants were informed about the consequences of the price cap (but after the agency treatment had been assigned) and after they received factual information or gained personal experience regarding the price-cap regime.

To ensure consistency between the theoretical model and the empirical implementation and reduce measurement noise, we imposed a set of sample restrictions. First, we excluded participants with internally inconsistent beliefs, defined as those who reported that there were periods in which no buyer in their group had made a purchase in the free-market regime (which was impossible because all buyers always made a purchase) or estimated that the fraction of periods with zero group purchases in the price-cap regime exceeded one-half (which was impossible because they had made a purchase themselves one-half of the time). Second, we excluded participants whose initial regime choices appeared inconsistent

with their stated prior beliefs. Specifically, these individuals initially reported that the price cap did not reduce the probability of purchase, yet they did not choose the price-cap regime. Although this behavioral pattern could, in principle, reflect risk aversion or inequality aversion, the evidence from the auxiliary lottery and allocation choices contradicts this interpretation.¹⁵ We therefore interpret this behavior as a genuine inconsistency.

Crucially, these restrictions do not introduce selection bias related to the experimental treatments. This was formally tested in a linear regression reported in Table A4 in Online Appendix I, which shows that the probability of being excluded based on these criteria is unrelated to both information and agency treatment assignments. The restrictions therefore preserve the internal validity of the experimental design. Nevertheless, in a series of robustness exercises, we demonstrate that the main results hold when using the full sample.

The main empirical results are organized into two parts. Section 5.2 presents the average effects of the information intervention on beliefs and regime choices regardless of the decision-making role. Section 5.3 shows the differential effects in the response of Decisive DMs and Hypothetical DMs to the information intervention.

5.2 Effects of the information intervention on beliefs and regime choices

We begin by examining baseline beliefs and regime choices, which the participants reported once they had participated in the free-market regime but before they received any information about the price cap.

Result 1 *Before the information intervention, most participants were optimistic about the probability of buying in the price-cap regime and supported it.*

Figure A3 in Online Appendix I plots, for each information treatment arm, participants' perceived probability of buying in the free market as well as their perceived probability of buying under the price cap reported at the baseline. At this stage, participants in the Experience and Factual Information groups had participated in the free-market regime but had not yet received any information about the consequences of the price cap. Beliefs were broadly similar across groups, which was expected because the information intervention had not been implemented yet.¹⁶ While several participants anticipated that the price cap might

¹⁵Participants exhibiting this behavioral pattern were *more* likely to take risks (χ^2 test; $p = 0.077$) and *more* likely to choose the more unequal allocation (χ^2 test; $p = 0.037$) in the auxiliary tasks than participants who did not exhibit this pattern.

¹⁶The perceived probability of buying in the free-market regime was indistinguishable between the two groups (Kolmogorov-Smirnov test, p -value = 0.658; Wilcoxon rank-sum test, p -value = 0.693), but the

reduce their chance of buying relative to the free-market regime (one-tailed Wilcoxon signed-rank test pooling both groups, p -value = 0.009), in general, the participants substantially overestimated their prospects under the price cap.

The participants’ optimism is highlighted in the left panel of Figure 5, which complements Figure A3 by summarizing the participants’ initial views about the excess probability of buying in the free market. Only 46% of the participants originally believed that the price cap would reduce the probability of buying relative to the free-market regime. Consistently, 78% of the participants initially chose the price-cap regime (see the right panel of Figure 5). These findings align with the strong support for interventionist policies identified in the pre-experiment survey.

[Figure 5 about here]

We now turn to endline beliefs and regime choices, which the participants reported after they received factual information or gained personal experience regarding the price-cap regime.

Result 2 *After the information intervention, most participants were pessimistic about the probability of buying in the price-cap regime and significantly less supportive of it.*

Figure A4 in Online Appendix I shows, for each information treatment arm, the change in the participants’ perceived probability of buying under the price cap from the baseline to the endline. In both groups, the participants adopted significantly more negative views about the price-cap regime after receiving information about the consequences of this policy (one-tailed Wilcoxon signed-rank test pooling both groups, p -value < 0.001). Complementing Figure A4, the left panel of Figure 5 shows that the percentage of participants who believed that the price cap reduced the probability of buying increased to 72% after the information intervention. Consistently, only 52% of the participants chose the price-cap regime at the endline (see the right panel of Figure 5).

These findings support parts 1 and 2 of Proposition 1, which predicted that negative information about the price cap would lead to a downward adjustment in the optimal excess-surplus expectation for the price-cap regime, thereby reducing support for this regime.

Next, we compare the effects of the information intervention between the Experience and Factual Information groups.

participants in the Experience group had somewhat more negative views about the perceived probability of buying under the price cap than those in the Factual Information group (Kolmogorov-Smirnov test, p -value = 0.062; Wilcoxon rank-sum test, p -value = 0.030). One possible explanation for this difference is that *anticipating* personal experience with the price-cap regime, as opposed to simply imagining a hypothetical one, led the participants in the Experience group to be more cautious even in their *prior* beliefs. This hypothesis is beyond the scope of our study, which aims to investigate the effects of actually experiencing a price cap.

Result 3 *Compared to receiving factual information on the consequences of the price cap, experiencing the price cap had substantially larger effects on beliefs and regime choices.*

As shown in Figure A4 in Online Appendix I, after the information intervention, the participants in the Experience group adjusted their beliefs about the price cap more strongly than those in the Factual Information group. In addition, the left panel of Figure 5 shows that the percentage of participants in the Experience (Factual Information) group who believed that the price cap reduced the probability of buying increased from 54% (40%) to 88% (57%). The right panel of Figure 5 shows a similar difference between the two groups in terms of the response of the regime choices to the information intervention. The percentage of participants who chose the price cap in the Experience (Factual Information) group decreased from 76% (80%) to 41% (62%).

To further compare the effects of the information intervention between groups, we estimated the following linear probability model using data on participants in the Experience and Factual Information treatment arms:

$$Y_{it} = \beta_0 + \beta_1 1_{t=1} + \beta_2 1_i^E + \beta_3 (1_{t=1} \times 1_i^E) + \epsilon_{it}. \quad (4)$$

In one specification, the dependent variable Y_{it} equals 1 if subject i believed in period t that the price cap reduces the probability of buying relative to the free market and 0 otherwise, where $t = 0, 1$. In another specification, Y_{it} equals 1 if subject i chose to impose the price cap in period t and 0 otherwise. In the experiment, $t = 0$ corresponds to the baseline (i.e., before the information intervention) and $t = 1$ to the endline (i.e., after the information intervention). $1_{t=1}$ is a dummy variable for the endline. 1_i^E is a dummy variable that equals 1 if subject i was assigned to the Experience treatment arm and 0 otherwise. Note that β_1 recovers the average effect of the information intervention on beliefs or regime choices of participants in the Factual Information group, whereas β_3 recovers the differential effect of the intervention among participants in the Experience group. Standard errors were clustered by experimental session, the level at which the information treatment was randomized (Abadie et al., 2023). Our conclusions are robust to alternative inference procedures that account for the limited number of clusters at the session level (Cameron and Miller, 2015).¹⁷

The results on beliefs are presented in column 1 of Table 1. In line with Figure 5, after

¹⁷The Experience and Factual Information comparison includes only 28 sessions, evenly split across treatment arms. Clustering with a small number of clusters is problematic because cluster-robust standard errors rely on large-cluster asymptotic approximations, which may understate sampling uncertainty and lead to over-rejection of the null hypothesis (Cameron, Gelbach, and Miller, 2008). Therefore, we implemented session-level randomization inference and wild-cluster bootstrap tests with 10,000 replications for all the information-treatment estimates. These checks leave the main conclusions unchanged at conventional statistical significance levels. The full set of inference checks is available upon request.

receiving factual information about the price cap, an additional 17% of the participants in the Factual Information group believed that the price cap reduced the probability of buying relative to the free market (one-tailed t -test, p -value < 0.001). Given that 40% of the participants had this belief at the baseline (measured by β_0), this implies that at the endline a total of 57% of the participants in the Factual Information group were pessimistic about the relative chances of buying under the price cap. In the Experience group, the effect was twice as large (one-tailed t -test, p -value $= 0.002$). Following personal experience with product shortages under the price cap, an additional 34% of the participants believed that the price cap reduced the probability of buying. Given that 54% of the participants had this belief at the baseline (measured by $\beta_0 + \beta_2$), at the endline a total of 88% of the participants in the Experience group were pessimistic about their relative prospects under the price cap. As shown in column 3 of Table 1, the results are robust to using the full sample instead of the restricted one.

For ease of exposition, we present the results on beliefs using a binary indicator that equals 1 if a participant believed that the price cap lowers the probability of buying instead of using our raw measure of beliefs, namely the participant’s perceived excess probability of buying under the free market. To verify that our results do not hinge on this discretization, we reestimated the specification from columns 1 and 3 of Table 1 using the continuous excess-probability measure as the dependent variable. Table A5 in Online Appendix I shows that our findings about information effects on beliefs are not driven by the discretization of the dependent variable.

[Table 1 about here]

The results on the choice of the regime are presented in column 2 of Table 1. The provision of factual information about the effect of the price cap on the probability of buying reduced the support for this regime by 18 percentage points (one-tailed t -test, p -value < 0.001). This implies 62% support for the price cap at the endline in the Factual Information group. In the Experience treatment arm, the effect of the intervention was twice as large (one-tailed t -test, p -value < 0.001): Experiencing the price cap reduced support by 35 percentage points. Nevertheless, even after experiencing the price cap, 41% of the participants chose this regime, revealing the persistence of the preference for it. As shown in column 4 of Table 1, the results are robust to using the full sample instead of the restricted one.

These patterns in beliefs and regime choices align with part 3 of Proposition 1, which—inspired by previous research on experience effects—predicted that personally experiencing the price cap would have stronger effects than a mere description of its consequences.

To supplement the above analyses, we assessed to what extent endline outcomes with personal experience were affected by the sequence in which participants encountered the two

market regimes. To this end, we compared endline beliefs and regime choices between the Experience and Reverse Experience groups. While the Experience group initially experienced the free-market regime and then the price-cap regime, the Reverse Experience group faced the two regimes in reverse order but encountered otherwise identical conditions.

As shown in Figure A5 in Online Appendix I, posterior beliefs about the probability of buying in each of the two regimes converged across the Experience and Reverse Experience treatment arms (Wilcoxon rank-sum test; p -value = 0.313 for the free-market regime and p -value = 0.360 for the price-cap regime). After exposure to both market regimes, the share of participants who believed that the price cap reduces the probability of buying relative to the free market reached 88% in the Experience group and 86% in the Reverse Experience group. Despite the similarity in beliefs between the two groups, support for the price cap remained higher in the Reverse Experience group (50%) than in the Experience group (41%), indicating some influence of initial experience on final preferences over the market regime.

To further test for order effects with respect to experience with market regimes, we reestimated model (4), replacing the Factual Information group with the Reverse Experience group. The null hypothesis of no order effects implies that $\beta_2 + \beta_3 = 0$.

Table A6 in Online Appendix I presents the estimates. The evidence for order effects is limited. In line with Figure A5, we did not detect a significant difference in posterior beliefs between the Experience and Reverse Experience treatment arms (two-tailed t -test, p -value = 0.493). However, we observed a modest difference in final regime choices. Compared to participants in the Reverse Experience group, those assigned to the Experience group were approximately 9 percentage points less likely to choose the price-cap regime at the endline (two-tailed t -test, p -value = 0.090). The results were similar using the full sample instead of the restricted one. Overall, the results indicate that the order of experience with market regimes did not significantly influence posterior beliefs and only marginally affected final regime choices.

5.3 Effects of the agency intervention on beliefs and regime choices

We now investigate how responsibility for regime implementation affected beliefs and regime choices. Taking advantage of the fact that our agency intervention was independent of our information intervention, in our main analysis of agency effects we pool observations across information treatment arms to boost statistical power, including dummy variables for these arms as control variables in our econometric model.

We begin by comparing baseline beliefs and regime choices between Decisive DMs and Hypothetical DMs.

Result 4 *Before the information intervention, there were no differences in beliefs and support for the price-cap regime between Decisive DMs and Hypothetical DMs.*

Figure 6 shows the share of participants in each agency treatment arm who initially believed that the price cap reduces the probability of buying relative to the free market (left panel) and the share of participants who chose the price-cap regime at the baseline (right panel). We did not find significant differences in prior beliefs or initial choices between the two groups (χ^2 -test; p -value = 0.195 for beliefs and p -value = 0.800 for choices).

[Figure 6 about here]

Next, we analyze how agency affected the response to the information intervention by comparing beliefs and regime choices of Decisive DMs and Hypothetical DMs at the endline. To this end, we first divide participants into two groups based on their *prior* beliefs—those who initially believed that the price cap does not reduce the probability of buying relative to the free market and those who believed the opposite—and then compare *final* outcomes between agency treatment arms conditional on prior beliefs. We do this because we expect differential agency effects in the response to information depending on the participants’ prior beliefs. Intuitively, lack of agency should be most consequential among participants who initially held positive views about the price cap, limiting the potential of the information intervention to change beliefs.

Result 5 *Following the information intervention, both Hypothetical DMs and Decisive DMs held more negative views of the price cap. However, among participants who initially believed that the price cap does not reduce the probability of buying, Decisive DMs came to hold significantly more negative views and were significantly less likely to support the price cap at the endline compared to Hypothetical DMs.*

The left panel of Figure 7 shows the percentage of participants in each agency treatment arm who believed at the endline that the price cap reduces the probability of buying, conditional on the participants’ prior beliefs. Among participants who already believed at the baseline that the price cap reduces the probability of buying, we found no significant differences in posterior beliefs between Decisive DMs and Hypothetical DMs (χ^2 -test, p -value = 0.507). As discussed above, this result was expected because this group of participants was the least likely to be affected by the information intervention. Indeed, approximately 89% of these participants maintained their negative views.¹⁸

¹⁸Strictly speaking, most of these participants became even more negative in their views about the price cap after the information intervention, as their perceived excess probability of buying in the free-market regime increased relative to the baseline (Wilcoxon signed-rank test, p -value < 0.001).

[Figure 7 about here]

The left panel of Figure 7 also shows that the intervention had a large effect among participants with initially positive views about the price cap, inducing most of them to switch to negative views. However, among these participants, we found significant differences in posterior beliefs between Decisive DMs and Hypothetical DMs (χ^2 -test, p -value = 0.005). While 64% of Hypothetical DMs who initially had positive views of the price cap believed at the endline that it reduces the probability of buying, this percentage is 82% among Decisive DMs. This finding suggests that agency significantly influenced the extent to which participants updated their beliefs about the price cap following the information intervention.

The right panel of Figure 7 displays the percentage of participants in each agency treatment arm who chose the price-cap regime after the information intervention, conditional on the participants' prior beliefs. As with regard to posterior beliefs, we found no differences in final regime choices between Decisive DMs and Hypothetical DMs who already held negative views about the price cap at the baseline (χ^2 -test, p -value = 0.822). In both treatment arms, around 40% of participants supported the price cap at the endline. By contrast, among participants who initially held positive views of the price cap, we found large differences between Decisive DMs and Hypothetical DMs in their support for the price-cap regime at the endline (χ^2 -test, p -value = 0.014). While 60% of the Hypothetical DMs chose the price-cap regime, only 43% of the Decisive DMs made the same decision. The larger drop in endorsement of the price cap among Decisive DMs is consistent with the larger downward adjustment in their perceived probability of buying under the price cap.

To assess the robustness of these patterns in posterior beliefs and final regime choices, we estimated the following linear probability model:

$$Y_{i1} = \theta_0 + \theta_1 1_i^{\hat{P}_0 > 0} + \theta_2 1_i^d + \theta_3 \left(1_i^{\hat{P}_0 > 0} \times 1_i^d \right) + X_i' \theta_4 + \epsilon_i \quad (5)$$

In one specification, the dependent variable Y_{i1} equals 1 if subject i believed at the endline that the price cap reduces the probability of buying relative to the free market and 0 otherwise. In another specification, Y_{i1} equals 1 if subject i chose to impose the price cap at the endline and 0 otherwise. $1_i^{\hat{P}_0 > 0}$ equals 1 if subject i believed at the baseline that the price cap reduces the probability of buying and 0 otherwise. 1_i^d equals 1 if subject i was assigned to the Decisive role and 0 otherwise. X_i is a vector of control variables including dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention in markets. Note that θ_2 (respectively, $\theta_2 + \theta_3$) recovers the average effect of being a Decisive DM on posterior beliefs or final regime choices among participants

who initially believed that the price cap does not (respectively, does) reduce the probability of buying. Standard errors were clustered by experimental session, a conservative choice considering the group-level assignment of the agency treatment.¹⁹

Table 2 presents the results on posterior beliefs. Column 1 shows the results using the restricted sample and excluding controls. Among participants who had positive views of the price cap at the baseline, Decisive DMs were approximately 18 percentage points more likely to believe at the endline that the price cap reduces the probability of buying relative to the free market (one-tailed t -test, p -value = 0.002). The magnitude of the agency effect remains essentially the same after adding control variables (column 2) and is similar when using the full sample instead of the restricted one (columns 3 and 4). Additionally, the estimate of θ_3 is negative and similar in magnitude to that of θ_2 in all four specifications, implying that we cannot reject the null hypothesis that $\theta_2 + \theta_3 = 0$ (see the p -values at the bottom of Table 2)—that is, there is no evidence of an agency effect in posterior beliefs among participants who already had negative views about the price cap at the baseline.

[Table 2 about here]

One factor omitted from the theoretical model is the role of attention and cognitive effort in information processing. Because Hypothetical DMs do not bear an instrumental cost from holding biased beliefs, they may have weaker incentives to pay attention to the news about the price cap or to exert effort in updating their prior beliefs accurately. Under this interpretation, the stronger belief updating observed among Decisive DMs could reflect greater attention or cognitive effort rather than motivated beliefs.

To evaluate this alternative mechanism, we reestimated the specifications in columns 2 and 4 of Table 2 including additional controls intended to capture inattention or careless belief updating. Specifically, we constructed one indicator that equals 1 if a participant exhibited belief inertia and 0 otherwise, and a second indicator that equals 1 if a participant updated her prior belief in the wrong direction and 0 otherwise. If the “attention hypothesis” accounted for the stronger updating among Decisive DMs, controlling for these proxies should substantially reduce the estimated effect of agency on posterior beliefs. However, Table A7 in Online Appendix I shows that including these controls leaves the estimated effects largely unchanged, thus rejecting the “attention hypothesis.”

Table 3 presents the results on final regime choices based on model (5). Column 1 shows that among participants who initially held positive views of the price cap, Decisive DMs were

¹⁹Our conclusions for the agency-treatment results are broadly robust to alternative inference procedures that account for the within-group assignment of agency. We implemented clustering at the session-by-group level, within-group randomization inference preserving the realized number of Decisive DMs in each group (Rosenbaum, 2002), and wild-cluster bootstrap tests at both the session and session-by-group levels (Cameron, Gelbach, and Miller, 2008). The full set of inference checks is available upon request.

17 percentage points less likely to endorse the price-cap regime at the endline (one-tailed t -test, p -value = 0.019). This result is unaffected by the inclusion of controls (column 2). While the estimated agency effect in regime choices is qualitatively similar when using the full sample instead of the restricted one, the inclusion of participants with a noisy decision-making process in the estimation attenuates the effect (columns 3 and 4). As for participants who already had negative views about the price cap at the baseline, there is no evidence of an agency effect in final regime choices in any specification (see the p -values at the bottom of Table 3).

[Table 3 about here]

Overall, the findings on agency effects are consistent with Proposition 2, which predicted that Decisive DMs would display a stronger response to negative information about the price cap compared to Hypothetical DMs. According to the model, while the consumer has a taste for the price-cap regime because of her statist ideology, being responsible for its implementation introduces an instrumental cost of maintaining optimistic beliefs about the price cap, thus mitigating resistance to information that challenges prior positive views. As a result, upon receiving negative news, Decisive DMs end up holding more negative views about the price cap compared to Hypothetical DMs, which translates into reduced endorsement of the price-cap regime.

To provide additional support for the theoretical model regarding the role of agency in regime implementation, we next examine another implication of the model resulting from expression (2). Consider a Decisive DM and a Hypothetical DM who hold the same belief about the excess surplus of the price-cap regime at the endline. It follows from expression (2) that if this perceived excess surplus is positive (negative), the Decisive DM will be more (less) likely to select the price-cap regime than the Hypothetical DM. That is, compared to a Hypothetical DM, a Decisive DM with the same belief is more likely to act on her belief. This is because a Decisive DM weighs the consequences of her regime choice for her surplus in addition to the psychological motives, while a Hypothetical DM's choice is driven only by psychological motives. To test this prediction, we compare Decisive DMs and Hypothetical DMs who had common posterior beliefs about the probability of buying (those with negative posterior views and, separately, those with positive views).

Result 6 *Compared to Hypothetical DMs, Decisive DMs were more likely to act on their perceived excess probability of buying in the free-market regime.*

Figure 8 provides graphical evidence for Result 6. It shows the probability of selecting the price-cap regime at the endline as a function of posterior beliefs about the probability

of buying, separately for Decisive DMs and Hypothetical DMs. Because this comparison holds posterior beliefs constant, any Decisive-Hypothetical gap reflects differences in how choices respond to beliefs rather than differences in belief updating. Consistent with the theoretical model, among participants who believed at the endline that the price cap reduces the probability of buying, Decisive DMs were significantly less likely to support the price cap than Hypothetical DMs (χ^2 -test, p -value = 0.021). In contrast, among participants who believed that the price cap does not reduce the probability of buying, Decisive DMs were more likely to support it (χ^2 -test, p -value = 0.069).

[Figure 8 about here]

To complement the evidence provided in Figure 8, we estimated the following linear probability model:

$$1_i^{PC1=1} = \lambda_0 + \lambda_1 1_i^{\hat{P}_1 > 0} + \lambda_2 1_i^d + \lambda_3 \left(1_i^{\hat{P}_1 > 0} \times 1_i^d \right) + X_i' \lambda_4 + \epsilon_i \quad (6)$$

The dependent variable, $1_i^{PC1=1}$, equals 1 if subject i chose to impose the price cap at the endline and 0 otherwise. $1_i^{\hat{P}_1 > 0}$ is a dummy variable that equals 1 if subject i believed at the endline that the price cap reduces the probability of buying. 1_i^d and X_i are defined as in model (5). Note that λ_2 (respectively, $\lambda_2 + \lambda_3$) recovers the Decisive-Hypothetical gap in the probability of choosing the price-cap regime among participants who believed at the endline that the price cap does not (respectively, does) reduce the probability of buying. Standard errors were clustered by experimental session.

Table 4 reports the estimates. Column 1 reports the results for the restricted sample without controls. The estimate of λ_2 indicates that among participants with positive posterior views about the price cap, Decisive DMs were 21 percentage points more likely to choose the price-cap regime (one-tailed t -test, p -value = 0.003). In contrast, the estimate of $\lambda_2 + \lambda_3$ indicates that among participants with negative posterior views about the price cap, Decisive DMs were approximately 13 percentage points less likely to choose the price-cap regime (one-tailed t -test, p -value = 0.006). The results are robust to adding controls (column 2) and to using the full sample instead of the restricted one (columns 3 and 4).

[Table 4 about here]

A potential drawback of the exercise discussed above is that posterior beliefs are endogenous, as they were influenced by the agency treatment itself (see Result 5). One concern stemming from this endogeneity is that beliefs may have been stronger among Decisive DMs than they were among Hypothetical DMs, even among individuals with shared qualitative views. Specifically, among participants who ultimately agreed that the price cap does

(not) reduce the probability of buying, Decisive DMs could have held more negative (positive) views than Hypothetical DMs about the price cap, expecting a larger (smaller) excess probability of buying in the free market. This would imply that conditioning on whether participants believed at the endline that the price cap lowers the probability of buying, as we did, would not enable a clean comparison across individuals with different decision-making roles but identical beliefs.

To assess whether this was an issue in practice, we compared the magnitude of the perceived excess probability of buying under the free market between Decisive DMs and Hypothetical DMs conditional on the sign of the perceived excess probability. The results, presented in Table A8 in Online Appendix I, show no evidence that the agency treatment affected belief intensity conditional on belief direction (two-tailed t -test; p -value = 0.486 for participants who believed that the price cap does not lower the probability of buying and p -value = 0.693 for participants who believed the opposite). This suggests that the observed behavioral differences between Decisive and Hypothetical DMs who shared the same qualitative views were indeed driven by the agency treatment itself, not by more subtle differences in posterior beliefs.

Overall, the evidence shows that final regime choices were more aligned with posterior beliefs among Decisive DMs than they were among Hypothetical DMs, as predicted by the theoretical model.

6 Comparison with other models

Our theoretical model is an enriched version of the rational irrationality framework for political views and voting behavior (Caplan, 2007). It also embeds the rational choice model (Downs, 1957) and expressive voting (Brennan and Lomasky, 1993) as special cases. In this section we compare our model with these alternative models both theoretically and in terms of their ability to explain the beliefs and regime choices observed in our experiment.

6.1 Rational choice

In rational choice theory (Downs, 1957), voters are fully rational agents who base their beliefs about the consequences of policies on available information and choose the regime that maximizes their expected material payoff. The rational choice model is a limiting case of our model in which individuals neither have preferences over their beliefs about the price cap (i.e., $B(\hat{\phi}) = 0$ for all $\hat{\phi}$) nor face dissonance costs (i.e., $D(\hat{\phi}) = 0$ for all $\hat{\phi}$). In this case, belief distortion disappears (i.e., $\hat{\phi} = \phi$ solves (3)) and the voter chooses the regime based

purely on material considerations. Our model allows consumers to derive utility not only from material payoffs but also from the satisfaction of holding certain beliefs.

The rational choice model makes two key predictions in our setting. First, Decisive DMs and Hypothetical DMs in our experiment, who held similar beliefs at the baseline (see Result 4), should update their prior beliefs in a similar fashion following the information intervention and hence hold similar posterior beliefs. This prediction was rejected by Result 5, which shows a significant gap in posterior beliefs between decision-making roles. Second, Hypothetical DMs should be indifferent between the free-market regime and the price-cap regime because they face neither instrumental nor dissonance costs. This implies that the probability that a Hypothetical DM chooses the price-cap regime should always equal one-half. (Note from expression (2) that if $D(\hat{\phi}) = 0$ for all $\hat{\phi}$, then $Prob(cap|d = 0) = \frac{1}{1+exp\{0\}} = \frac{1}{2}$.) However, while 51% of Hypothetical DMs supported the price-cap regime at the endline, 70% of them had chosen it at the baseline.

6.2 Expressive voting

Expressive voting models (Brennan and Lomasky, 1993) propose that voters derive utility from the mere act of expressing support for policies that align with their values or identities, regardless of the policy’s material consequences. In this framework, voters may support inefficient policies because doing so provides intrinsic expressive satisfaction. Beliefs about policy consequences are assumed to be accurate; expressive utility modifies the regime-choice margin but does not affect belief formation.

Our model can be easily modified to capture expressive utility explicitly through a parameter $\kappa > 0$, which adds an intrinsic payoff to the utility of the price-cap regime, representing a taste for “ideological cheerleading” (Brennan and Buchanan, 1984). Additionally, dissonance is psychologically neutral (i.e., $D(\hat{\phi}) = 0$ for all $\hat{\phi}$). Thus, the utility of the price-cap regime becomes $U_{cap} = 1^d(v - \bar{p})\hat{P}(buy|p = \bar{p}) + \kappa + \epsilon_{cap}$. Expressive utility κ shifts the probability of supporting the price cap upward, independent of beliefs.

Like the rational choice model, expressive voting predicts that Decisive DMs and Hypothetical DMs should hold similar posterior beliefs, which was rejected by Result 5. It also predicts that the probability that a Hypothetical DM chooses the price-cap regime should be constant. (Note from expression (2) that if $D(\hat{\phi}) = 0$ for all $\hat{\phi}$, then $Prob(cap|d = 0) = \frac{1}{1+exp\{\phi\}}$.) As discussed above, this prediction was also rejected by the experimental data.

6.3 Rational irrationality

The theory of rational irrationality developed by Caplan (2007) argues that voters systematically hold biased beliefs about policies because it is privately cheap to be wrong. Because an individual voter’s influence on policy outcomes is negligible, the cost of holding incorrect beliefs is effectively zero, leading voters to indulge in biases that reflect their preferences, values, or ideology.

Our model formalizes Caplan’s core intuition about the relationship between agency in policy implementation and belief distortion. For Hypothetical DMs, the instrumental cost of holding biased beliefs about the price cap is zero (i.e., $C(\hat{\phi}) = 0$ for all $\hat{\phi}$), allowing them to distort beliefs to maximize affective utility with no material consequence. For Decisive DMs, belief distortion becomes costly, leading to less biased beliefs. This yields a clear comparative static: Belief distortions decline as the instrumental consequences of decisions increase. Result 5 supports this key prediction.

While in our model individuals balance the affective utility of optimistic beliefs about the price cap against both the instrumental costs of being wrong when their decisions carry real consequences and the discomfort of self-deception, Caplan’s model does not account for self-deception costs. (The original model does not explicitly incorporate dissonance costs either, but Caplan’s arguments hint at them.) If self-deception were costless (i.e., $S(\hat{\phi} - \phi) = 0$ for all $\hat{\phi}, \phi$), a Hypothetical DM would *not* update her prior belief following the information intervention. (To see this, substitute $S(\hat{\phi} - \phi) = 0$ and $C(\hat{\phi}) = 0$ into (3). Utility becomes $B(\hat{\phi})$ and is thus unaffected by ϕ .) However, in the experiment, Hypothetical DMs—like Decisive DMs—came to hold more negative views about the price cap following the information intervention (see Result 5). This finding is consistent with a positive self-deception cost and hence supports our enriched version of the rational irrationality framework.

7 Concluding remarks

Combining theory and experimental evidence, this paper investigated why citizens support economically inefficient policies, focusing on price caps as a salient case. Despite widespread expert consensus about their adverse consequences, price caps remain politically popular across the world. Our approach was premised on the idea that this support can be traced to the way individuals form and sustain beliefs when they are shielded from the consequences of their policy preferences.

Our theoretical model predicted that belief distortions are more likely to arise and persist when individuals are not responsible for the outcomes of their policy preferences and when

they lack experiential feedback to challenge their optimistic beliefs. The experimental results supported these predictions. We found that most participants held optimistic prior beliefs about the effects of a price cap in a competitive market and initially supported this policy. While providing factual information about shortages reduced support for the policy, personal experience with the price cap led to significantly larger belief revisions and a sharper decline in support. Nevertheless, belief distortions persisted among many participants even in the face of direct experience. However, individuals who were in charge of deciding whether to impose the price cap responded more strongly to negative information compared with individuals who made only hypothetical regime choices.

Because our experiment was intentionally stylized, further research is needed to assess the external validity of our findings. The laboratory market abstracted from several real-world factors that may shape attitudes toward price controls, including distributional and fairness considerations—in part because the selling side was automated and did not represent identifiable stakeholders. Moreover, the experiment captured short-run learning in a controlled setting. In real-world environments, repeated exposure, social narratives, and partisan cues may interact with experience and accountability in ways that either amplify or attenuate belief updating. These considerations underscore the importance of complementary field evidence to examine whether experience-based feedback and accountability mechanisms similarly moderate support for inefficient policies in naturally occurring political contexts.

While we focused on the case of price caps, our findings provide general insights for attempts to reduce support for inefficient policies. From a policy perspective, our results highlight the limitations of information-provision campaigns. Efforts to correct self-serving beliefs about economic policies may be insufficient unless they are coupled with mechanisms that activate a sense of personal responsibility or material consequences. More broadly, our findings underscore the importance of institutional arrangements that create feedback and accountability in political decision-making. When citizens are insulated from the outcomes of their policy preferences, belief distortions may persist, sustaining demand for welfare-reducing policies.

Ideally, future research should explore how these mechanisms operate in real-world democratic environments. Identifying scalable interventions that foster a sense of agency—through participatory decision-making, citizen deliberation, and more direct forms of accountability—may be essential for increasing support for evidence-based policies and improving the quality of democratic choices. Participatory decision-making can take the form of participatory budgeting or community-driven allocation of public resources, which has been shown to improve the alignment between citizen preferences and public spending and to make policy trade-

offs more salient (Olken, 2010; Wampler, 2007). Citizen deliberation can be implemented through citizens’ assemblies or deliberative mini-publics that review evidence, hear competing arguments, and produce recommendations; such designs have been found to promote belief updating and reduce reliance on expressive preferences (Fishkin, 2009; Landemore, 2020). These approaches can be complemented by accountability mechanisms such as structured public feedback on policy performance and transparent reporting of policy outcomes, which strengthen incentives for politicians and voters to respond to information and reduce the persistence of distorted beliefs (Ferraz and Finan, 2011; Besley and Burgess, 2002; Snyder and Strömberg, 2010).

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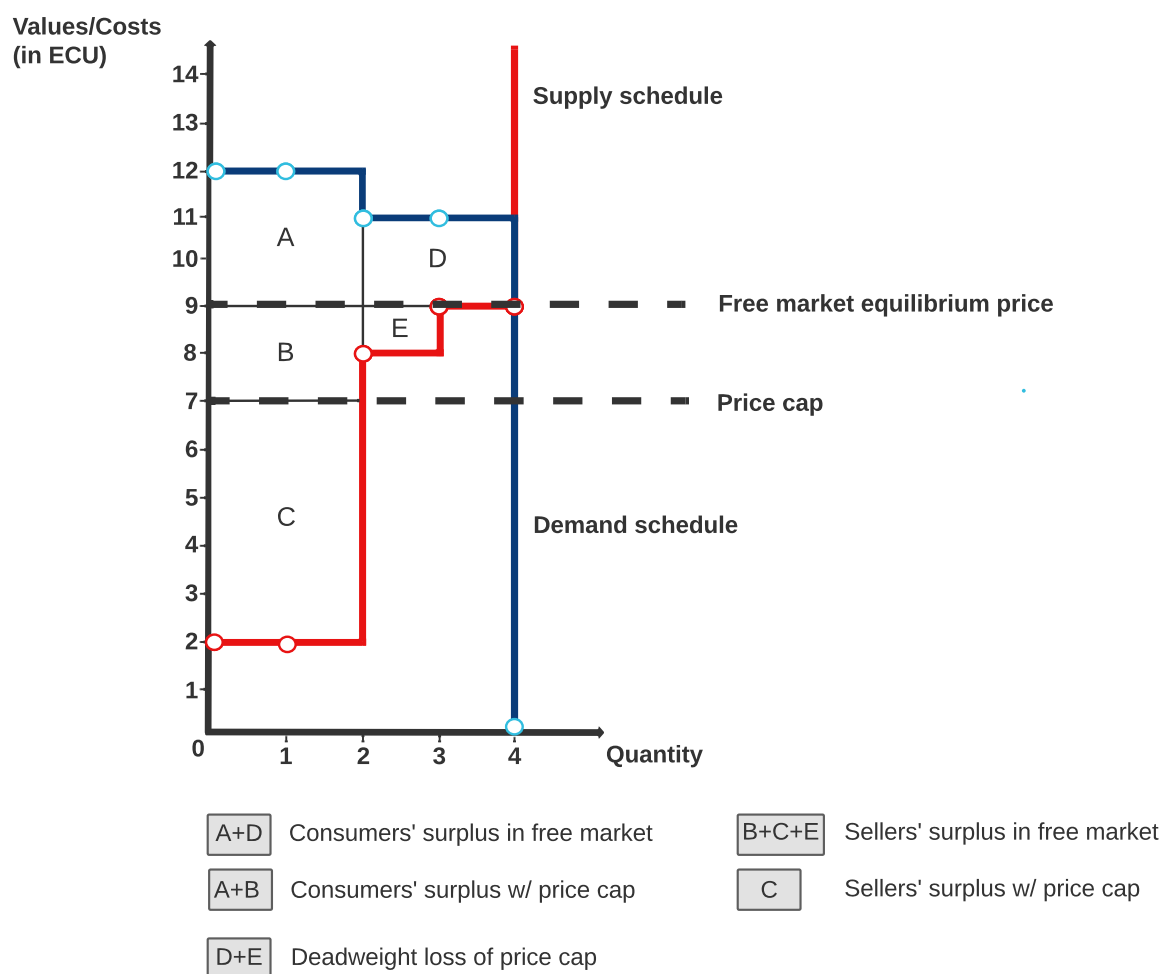
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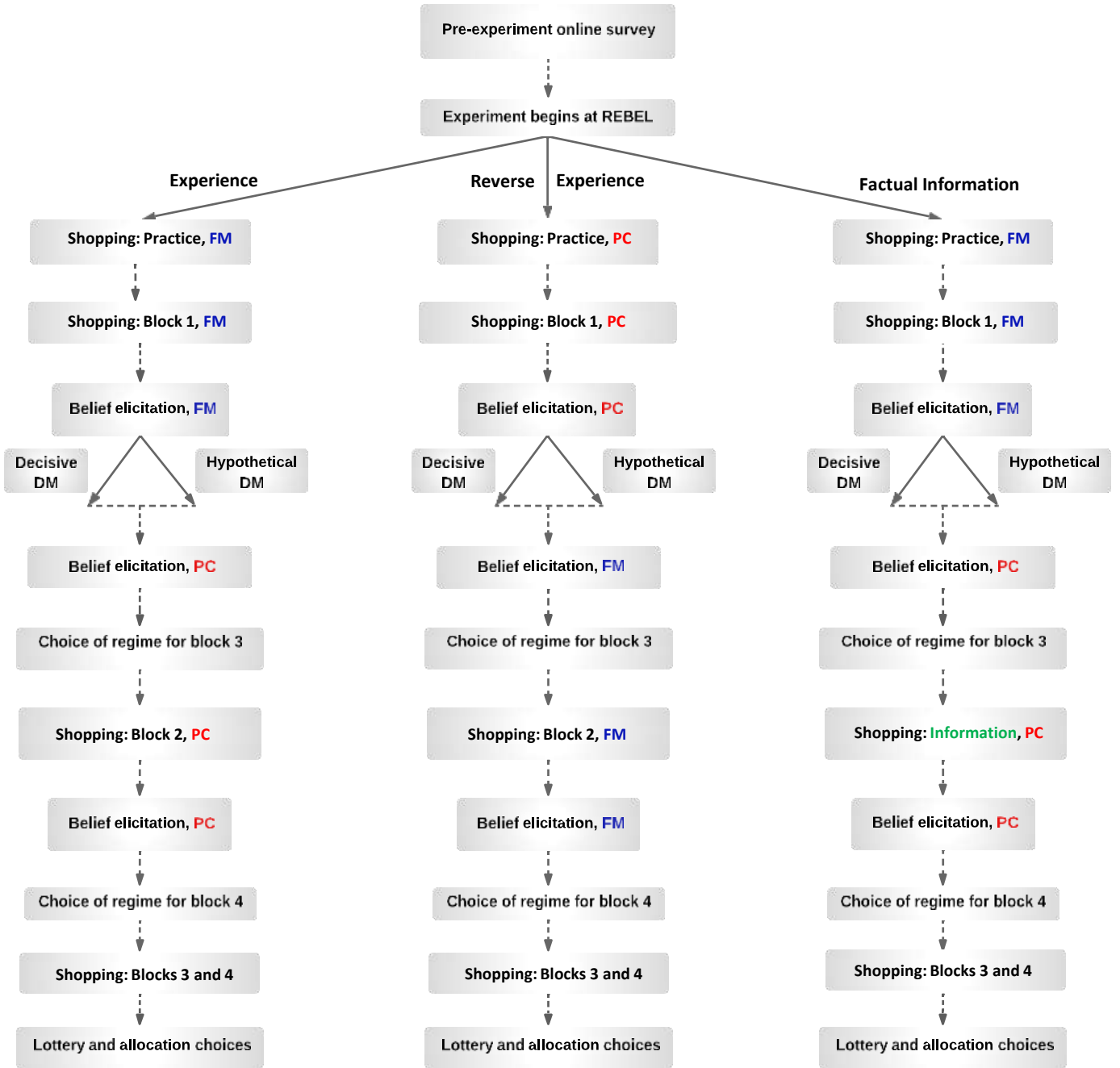
Figures

Figure 1: The experimental commodity market



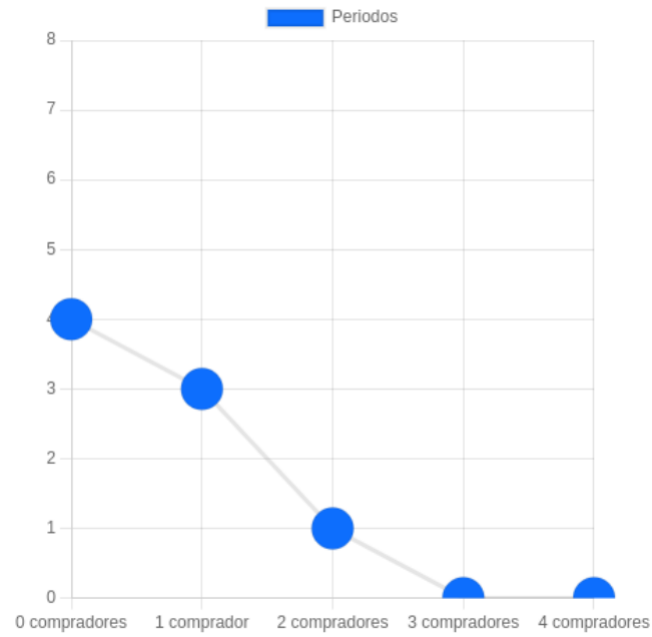
Notes: This figure illustrates the market environment used in the experiment, including buyers, sellers, and the market equilibrium.

Figure 2: Timeline of a laboratory session



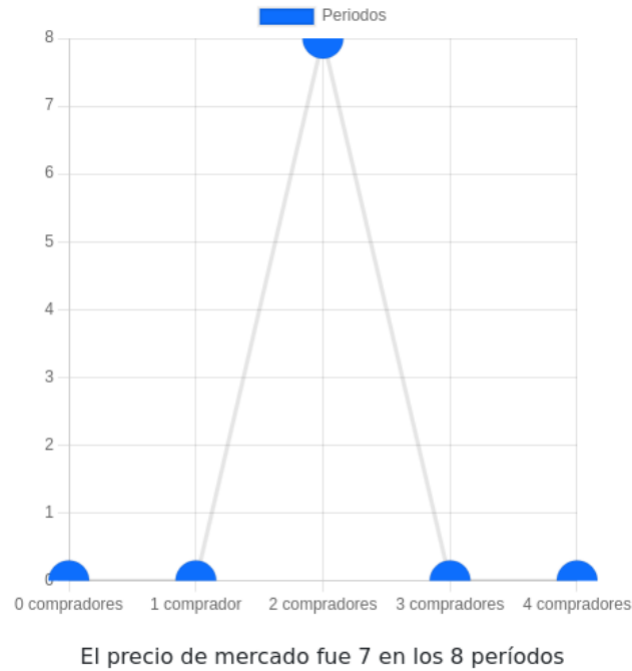
Notes: “FM” (“PC”) indicates a free-market (price-cap) regime. In one-half of the sessions of each information treatment arm, the participants made the lottery and allocation choices before the commodity market was introduced.

Figure 3: Graphical interface used for belief elicitation



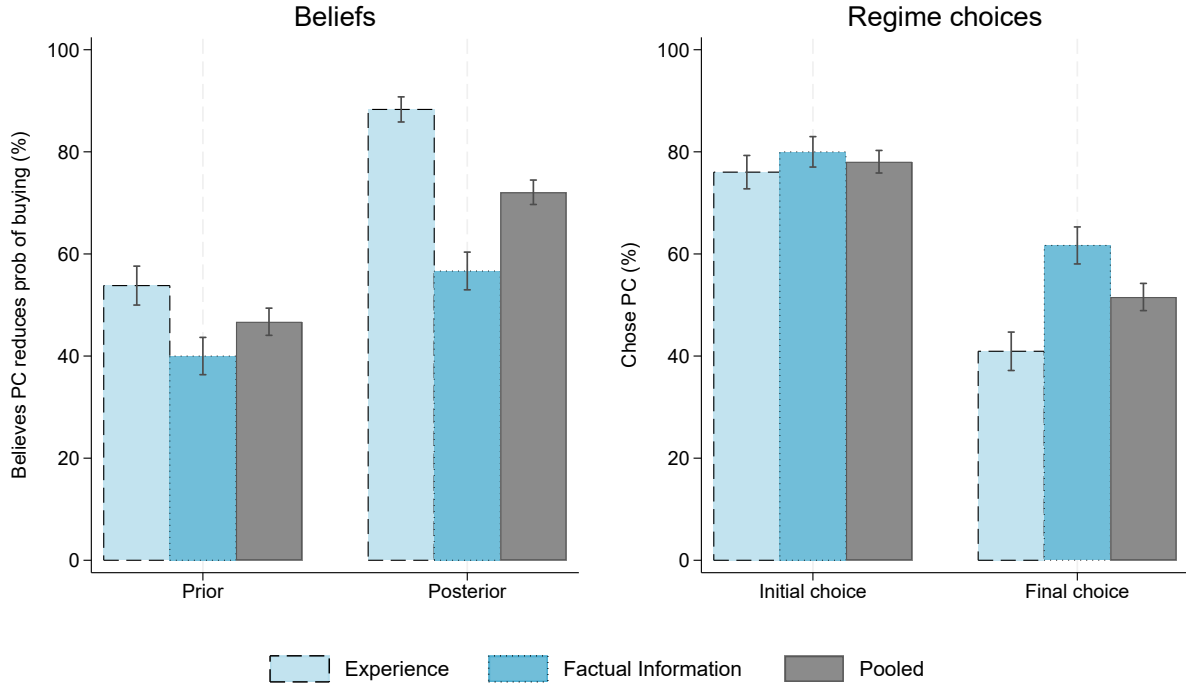
Notes: This figure illustrates the graphical interface that participants used to report their beliefs about group purchases during the experiment. The Spanish words “comprador,” “compradores,” and “periodos” mean “buyer,” “buyers,” and “periods,” respectively.

Figure 4: Information intervention in the Factual Information treatment arm



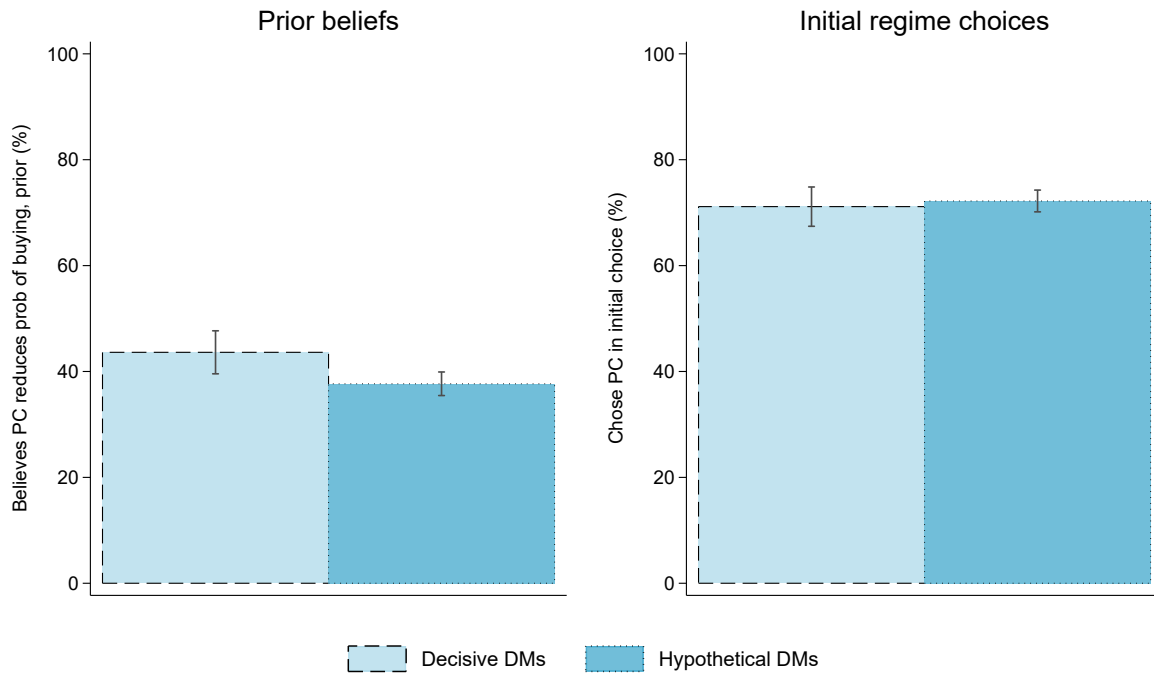
Notes: This figure displays the graphical interface used to provide factual information about the price cap in the Factual Information treatment arm. It shows the expected frequency of total purchases under the price cap. The Spanish words “comprador,” “compradores,” and “periodos” mean “buyer,” “buyers,” and “periods,” respectively. The sentence “El precio de mercado fue 7 en los 8 periodos” means “The market price was 7 in all 8 periods.”

Figure 5: Beliefs and regime choices in the Experience and Factual Information groups



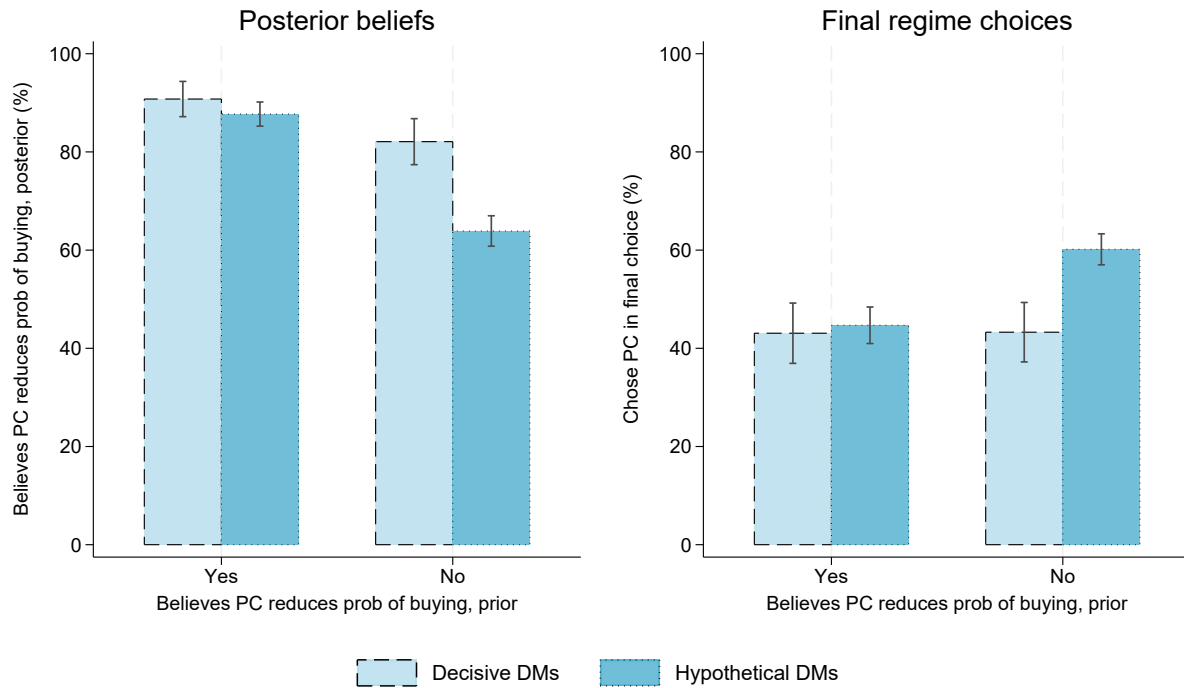
Notes: This figure summarizes beliefs and regime choices in the Experience and Factual Information treatment arms using the subsample of participants with consistent beliefs and choices. The left panel shows the percentage of participants who believed that the price cap reduces the probability of buying relative to the free market, both at the baseline and the endline. The right panel displays the corresponding regime choices. Error bars represent 95% confidence intervals clustered by experimental session.

Figure 6: Prior beliefs and initial regime choices by agency treatment arm



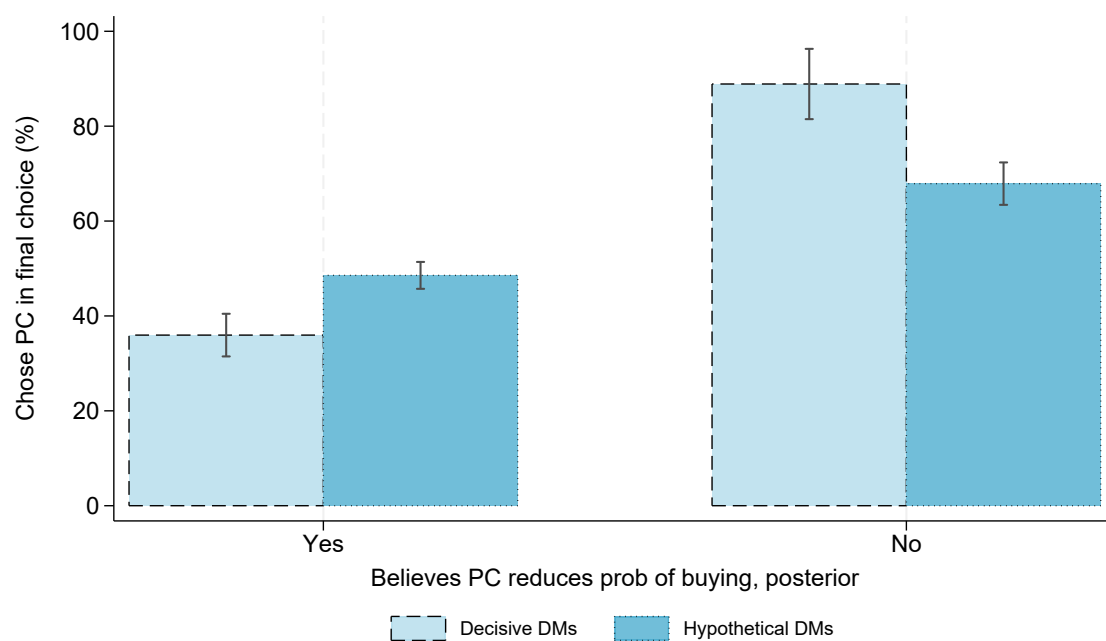
Notes: This figure compares prior beliefs and initial regime choices between Decisive DMs and Hypothetical DMs using the subsample of participants with consistent beliefs. The left panel shows the percentage of participants who believe that the price cap reduces the probability of buying. The right panel displays the corresponding regime choices. Error bars represent 95% confidence intervals clustered by experimental session.

Figure 7: Posterior beliefs and final regime choices by agency treatment arm



Notes: This figure compares posterior beliefs and final regime choices between Decisive DMs and Hypothetical DMs using the subsample of participants with consistent beliefs. The left panel shows the percentage of participants who believed at the endline that the price cap reduces the probability of buying, conditional on the participants' prior beliefs. The right panel displays the corresponding final regime choices. Error bars represent 95% confidence intervals clustered by experimental session.

Figure 8: Final regime choices by agency treatment arm conditional on posterior beliefs



Notes: This figure compares final regime choices between Decisive and Hypothetical DMs conditional on the participants' posterior beliefs. Error bars represent 95% confidence intervals clustered by experimental session.

Tables

Table 1: Effects of experience vs. factual information on beliefs and regime choices

Variable	PC reduces prob of buying (1)	Chose PC (2)	PC reduces prob of buying (3)	Chose PC (4)
Post (= 1)	0.167*** (0.042)	-0.183*** (0.031)	0.174*** (0.041)	-0.108*** (0.031)
Experience (=1)	0.138*** (0.049)	-0.040 (0.050)	0.073* (0.042)	-0.051 (0.049)
Post × Experience	0.178*** (0.054)	-0.168*** (0.047)	0.137** (0.052)	-0.142*** (0.051)
Constant	0.400*** (0.038)	0.800*** (0.032)	0.355*** (0.036)	0.699*** (0.035)
Omitted treatment arm	FI	FI	FI	FI
Sample	Restricted	Restricted	Full	Full
Number of participants	351	351	523	523
Number of observations	702	702	1046	1046
Number of clusters	28	28	28	28

Notes: Each column reports the results of a linear probability model with the dependent variable indicated in the column header. The first outcome, “PC reduces the prob of buying,” is a dummy variable that equals 1 if the participant believed that the price cap reduces the probability of buying and 0 otherwise. The second outcome, “Chose PC,” is a dummy variable that equals 1 if the participant selected the price-cap regime and 0 otherwise. The key explanatory variables are “Post,” a dummy variable for endline observations; “Experience,” a dummy variable for the Experience treatment arm; and their interaction. “FI” stands for “Factual Information.” Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols *, **, and *** denote that $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively, using a two-tailed t -test.

Table 2: Effects of agency on posterior beliefs conditional on prior beliefs

Variable	PC reduces prob of buying (posterior)			
	(1)	(2)	(3)	(4)
PC reduces prob of buying (prior) (=1)	0.238*** (0.047)	0.243*** (0.050)	0.271*** (0.045)	0.285*** (0.047)
Decisive (= 1)	0.182** (0.058)	0.206*** (0.057)	0.138** (0.049)	0.146*** (0.049)
PC reduces prob of buying (prior)×Decisive	−0.151* (0.080)	−0.183** (0.075)	−0.096 (0.068)	−0.108 (0.071)
Constant	0.639*** (0.044)	0.709*** (0.162)	0.553*** (0.034)	0.663*** (0.149)
Controls	No	Yes	No	Yes
Sample	Restricted	Restricted	Full	Full
Number of participants	552	552	821	820
Number of clusters	45	45	45	45
p -val H_0 : [Decisive+PC reduces prob of buying (prior)×Decisive = 0]	0.445	0.570	0.274	0.361

Notes: The outcome, “PC reduces prob of buying (posterior),” is a dummy variable that equals 1 if the participant believed at the endline that the price cap reduces the probability of buying relative to the free market and 0 otherwise. The key independent variables are a dummy variable that equals 1 if a participant believed at the baseline that the price cap reduces the probability of buying and 0 otherwise; a dummy variable for Decisive DMs; and their interaction. Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indices were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols *, **, and *** denote that $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively, using a two-tailed t -test. The reported p -values are from t -tests of the null hypothesis that $\theta_2 + \theta_3 = 0$ in model (5).

Table 3: Effects of agency on final regime choices conditional on prior beliefs

Variable	Chose PC in final regime choice			
	(1)	(2)	(3)	(4)
PC reduces prob of buying (prior)	−0.155** (0.058)	−0.145** (0.060)	−0.069 (0.048)	−0.070 (0.050)
Decisive	−0.169** (0.079)	−0.176** (0.076)	−0.061 (0.061)	−0.053 (0.062)
PC reduces prob of buying (prior)×Decisive	0.153 (0.126)	0.166 (0.123)	0.037 (0.087)	0.035 (0.089)
Constant	0.602*** (0.039)	0.068 (0.193)	0.533*** (0.032)	0.070 (0.123)
Controls	No	Yes	No	Yes
Sample	Restricted	Restricted	Full	Full
Number of participants	552	552	821	820
Number of clusters	45	45	45	45
p -val H_0 : [Decisive + PC reduces prob of buying (prior)×Decisive = 0]	0.835	0.905	0.678	0.783

Notes: The outcome, “Chose PC in final regime choice,” is a dummy variable that equals 1 if the participant selected the price-cap regime in the final regime choice and 0 otherwise. The key independent variables are a dummy variable that equals 1 if the participant believed at the baseline that the price cap reduces the probability of buying and 0 otherwise; a dummy variable for Decisive DMs; and their interaction. Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Control variables in columns 2 and 4 include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols ** and *** denote that $p < 0.05$ and $p < 0.01$, respectively, using a two-tailed t -test. The reported p -values are from t -tests of the null hypothesis that $\theta_2 + \theta_3 = 0$ in model (5) in the main text.

Table 4: Decisive-Hypothetical gap in final regime choices conditional on posterior beliefs

Variable	Chose PC in final regime choice			
	(1)	(2)	(3)	(4)
PC reduces prob of buying (post) (=1)	-0.193*** (0.051)	-0.147*** (0.050)	-0.113*** (0.040)	-0.083** (0.037)
Decisive (=1)	0.210*** (0.072)	0.213*** (0.077)	0.133* (0.069)	0.146* (0.071)
PC reduces prob of buying (post)×Decisive	-0.336*** (0.079)	-0.342*** (0.082)	-0.224*** (0.071)	-0.233*** (0.072)
Constant	0.679*** (0.047)	0.160 (0.209)	0.581*** (0.036)	0.131 (0.131)
Controls	No	Yes	No	Yes
Sample	Restricted	Restricted	Full	Full
Number of participants	552	552	821	820
Number of clusters	45	45	45	45
p -val H_0 : [Decisive+PC reduces prob of buying (post)×Decisive ≥ 0]	0.006	0.005	0.032	0.038

Notes: The outcome, “Chose PC in final regime choice,” equals 1 if the participant selected the price-cap regime in the final regime choice and 0 otherwise. The key independent variables are a dummy variable that equals 1 if the participant believed at the endline that the price cap reduces the probability of buying and 0 otherwise; a dummy variable for Decisive DMs; and their interaction. Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Control variables in columns 2 and 4 include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols *, **, and *** denote that $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively, using a two-tailed t -test. The reported p -values are from t -tests of the null hypothesis that $\lambda_2 + \lambda_3 \geq 0$ in model (6) in the main text.

Online Appendix I for “Why do citizens support price controls? The role of motivated beliefs”

Stanislao Maldonado and Santiago I. Sautua

A Proofs of theoretical results from Section 2

Lemma 1 $\hat{\phi} \geq \phi$.

Proof: By contradiction, suppose that $\hat{\phi} < \phi$. We consider two possible cases regarding the sign of ϕ :

Case 1: $\phi \leq 0$. Using the definition of $C(\hat{\phi})$ in the main text, we have that $C(\hat{\phi}) = 0$. The marginal utility of a change in the excess-surplus expectation is

$$B'(\hat{\phi}) - S'(\hat{\phi} - \phi) > 0,$$

where the inequality follows from (1) the assumption that $B' > 0$ and (2) the assumption that $S' < 0$ for any $\hat{\phi}$ such that $\hat{\phi} < \phi$.

Case 2: $\phi > 0$. Using the definition of $C(\hat{\phi})$ in the main text, we have that $C(\hat{\phi}) = 1^d \left(\text{Prob}(\text{cap}|\phi) - \text{Prob}(\text{cap}|\hat{\phi}) \right) |\phi|$. The marginal utility of a change in the excess-surplus expectation is

$$B'(\hat{\phi}) - S'(\hat{\phi} - \phi) + 1^d \frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} |\phi| > 0,$$

where the inequality follows from (1) the assumption that $B' > 0$, (2) the assumption that $S' < 0$ for any $\hat{\phi}$ such that $\hat{\phi} < \phi$, and (3) the result that $\frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text).

In both cases, the consumer’s utility increases if she raises her excess-surplus expectation. Therefore, we reach a contradiction, which implies that $\hat{\phi} \geq \phi$.

Proof of Proposition 1

Part 1

Given that $\hat{\phi}_0 \geq \phi_0$ (see Lemma 1), the optimal prior excess-surplus expectation satisfies the first-order condition

$$B'(\hat{\phi}_0) - S'(\hat{\phi}_0 - \phi_0) - 1^d 1^{\{\phi < 0, \hat{\phi} > \phi\}} \frac{\partial Prob(cap)}{\partial \hat{\phi}} \Big|_{\hat{\phi}=\hat{\phi}_0} |\phi_0| = 0. \quad (A1)$$

Note that $\hat{\phi}_0 \geq \phi_0 > \phi_1$, where the first inequality follows from Lemma 1 and the second from the premise of Proposition 1. If, given ϕ_1 , the consumer were to maintain her prior expectation $\hat{\phi}_0$, the marginal utility of a change in her expectation would be

$$B'(\hat{\phi}_0) - S'(\hat{\phi}_0 - \phi_1) - 1^d 1^{\{\phi_1 < 0\}} \frac{\partial Prob(cap)}{\partial \hat{\phi}} \Big|_{\hat{\phi}=\hat{\phi}_0} |\phi_1|.$$

Substituting $B'(\hat{\phi}_0)$ from (A1), the above expression becomes

$$S'(\hat{\phi}_0 - \phi_0) - S'(\hat{\phi}_0 - \phi_1) + 1^d \frac{\partial Prob(cap)}{\partial \hat{\phi}} \Big|_{\hat{\phi}=\hat{\phi}_0} \left(1^{\{\phi_0 < 0, \hat{\phi}_0 > \phi_0\}} |\phi_0| - 1^{\{\phi_1 < 0\}} |\phi_1| \right) < 0,$$

where the inequality follows from: (1) the combination of Lemma 1, which implies that $\hat{\phi}_0 \geq \phi_0 > \phi_1$, and the assumption that $S'' > 0$; (2) the result that $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text); and (3) the fact that $|\phi_1| > |\phi_0|$ if $\phi_0 < 0$ and $\phi_1 < 0$ (given the premise that $\phi_1 < \phi_0$).

As it turns out, the consumer's utility increases if she reduces her excess-surplus expectation. Therefore, we obtain that $\hat{\phi}_1 < \hat{\phi}_0$.

Part 2

Combining the result that $\hat{\phi}_1 < \hat{\phi}_0$ established in part 1 of this proposition with the result that $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text), we obtain that $Prob_1(cap) < Prob_0(cap)$.

Part 3

Consider two values of the honest posterior excess-surplus expectation, $\tilde{\phi}_1$ and $\tilde{\tilde{\phi}}_1$, such that $\tilde{\tilde{\phi}}_1 < \tilde{\phi}_1$.

Optimal posterior excess-surplus expectation

Given $\tilde{\phi}_1$, the solution to (3) satisfies the first-order condition

$$B'(\hat{\phi}_1) - S'(\hat{\phi}_1 - \tilde{\phi}_1) - 1^d 1^{\{\tilde{\phi}_1 < 0, \hat{\phi}_1 > \tilde{\phi}_1\}} \frac{\partial Prob(cap)}{\partial \hat{\phi}} \Big|_{\hat{\phi}=\hat{\phi}_1} |\tilde{\phi}_1| = 0. \quad (A2)$$

Note that $\hat{\phi}_1 \geq \tilde{\phi}_1 > \tilde{\tilde{\phi}}_1$, where the first inequality follows from Lemma 1. If, given $\tilde{\tilde{\phi}}_1$, the consumer were to hold the same posterior expectation as the one corresponding to $\tilde{\phi}_1$, the marginal utility of a change in her expectation would be

$$B'(\hat{\phi}_1) - S'(\hat{\phi}_1 - \tilde{\tilde{\phi}}_1) - 1^d 1_{\{\tilde{\tilde{\phi}}_1 < 0\}} \left. \frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} \right|_{\hat{\phi}=\hat{\phi}_1} |\tilde{\tilde{\phi}}_1|.$$

Substituting $B'(\hat{\phi}_1)$ from (A2), the above expression becomes

$$S'(\hat{\phi}_1 - \tilde{\phi}_1) - S'(\hat{\phi}_1 - \tilde{\tilde{\phi}}_1) + 1^d \left. \frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} \right|_{\hat{\phi}=\hat{\phi}_1} \left(1_{\{\tilde{\phi}_1 < 0, \hat{\phi}_1 > \tilde{\phi}_1\}} |\tilde{\phi}_1| - 1_{\{\tilde{\tilde{\phi}}_1 < 0\}} |\tilde{\tilde{\phi}}_1| \right) < 0,$$

where the inequality follows from: (1) the combination of Lemma 1, which implies that $\hat{\phi}_1 \geq \tilde{\phi}_1 > \tilde{\tilde{\phi}}_1$, and the assumption that $S'' > 0$; (2) the result that $\frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text); and (3) the fact that $|\tilde{\phi}_1| > |\tilde{\tilde{\phi}}_1|$ if $\tilde{\phi}_1 < 0$ and $\tilde{\tilde{\phi}}_1 < 0$.

As it turns out, the consumer's utility increases if she reduces her excess-surplus expectation. Therefore, we obtain that the optimal expectation when $\phi_1 = \tilde{\phi}_1$ must be smaller than that corresponding to $\phi_1 = \tilde{\tilde{\phi}}_1$.

Final regime choice

Combining the above result that $\hat{\phi}_1$ is increasing in ϕ_1 with the result that $\frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} > 0$ ((see expression (2) in the main text), we obtain that $\text{Prob}_1(\text{cap}|\phi_1 = \tilde{\tilde{\phi}}_1) < \text{Prob}_1(\text{cap}|\phi_1 = \tilde{\phi}_1)$.

Lemma 2 *If $\phi_0 \geq 0$, then $\hat{\phi}_0$ will be the same for a Decisive DM and a Hypothetical DM. Additionally, $\hat{\phi}_0 \geq 0$.*

Proof: Suppose that $\phi_0 \geq 0$. Then, Lemma 1 implies that $\hat{\phi}_0 \geq \phi_0 \geq 0$. Therefore, from the definition of $C(\hat{\phi})$ in the main text, $C(\hat{\phi}_0) = 0$ for both types of DM. Because both types of DM choose $\hat{\phi}_0$ to maximize $[B(\hat{\phi}_0) - S(\hat{\phi}_0 - \phi_0)]$, they share the same optimal prior expectation.

Proof of Proposition 2

Let $\hat{\phi}_1^d$ (respectively, $\hat{\phi}_1^h$) denote the optimal posterior excess-surplus expectation of a Decisive (respectively, Hypothetical) DM. Similarly, let $\text{Prob}_1^d(\text{cap})$ (respectively, $\text{Prob}_1^h(\text{cap})$) denote the probability that a Decisive (respectively, Hypothetical) DM will choose the price-cap regime after receiving the new information about the price cap.

Part 1

For a Hypothetical DM, the solution to (3) given $\phi = \phi_1 < 0$ satisfies the first-order condition

$$B'(\hat{\phi}_1^h) - S'(\hat{\phi}_1^h - \phi_1) = 0. \quad (\text{A3})$$

If, given $\phi = \phi_1 < 0$, a Decisive DM were to hold the same posterior expectation as that of a Hypothetical DM, the marginal utility of a change in her expectation would be

$$B'(\hat{\phi}_1^h) - S'(\hat{\phi}_1^h - \phi_1) - \left. \frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} \right|_{\hat{\phi}=\hat{\phi}_1^h} |\phi_1| = 0.$$

Substituting $B'(\hat{\phi}_1^h) - S'(\hat{\phi}_1^h - \phi_1)$ from (A3), the above expression becomes

$$- \left. \frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} \right|_{\hat{\phi}=\hat{\phi}_1^h} |\phi_1| < 0,$$

where the inequality follows from the result that $\frac{\partial \text{Prob}(\text{cap})}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text).

As it turns out, a Decisive DM's utility increases if she reduces her excess-surplus expectation from $\hat{\phi}_1^h$, which implies that $\hat{\phi}_1^d < \hat{\phi}_1^h$.

Part 2

Suppose that $\hat{\phi}_1^d < 0$. Then, using expression (2) in the main text, we have that

$$\begin{aligned} \text{Prob}_1^d(\text{cap}) &= \frac{1}{1 + \exp\{|\hat{\phi}_1^d| + D(|\hat{\phi}_1^d|)\}} < \frac{1}{1 + \exp\{D(|\hat{\phi}_1^d|)\}} \\ &< \frac{1}{1 + \exp\{-D(\hat{\phi}_1^h) + D(-\hat{\phi}_1^h)\}} = \text{Prob}_1^h(\text{cap}), \end{aligned}$$

where the second inequality follows from the result that $\hat{\phi}_1^d < \hat{\phi}_1^h$ established in part 1 of this proposition.

B Lottery choice and distribution tasks

In the lottery choice task, the participants chose between a low-risk lottery, which paid either 3 ECU or 2 ECU with equal probability, and a high-risk lottery, which paid either 5 ECU or 0 ECU with equal probability. Risk-averse participants were expected to choose

the low-risk lottery over the high-risk lottery because the former is a mean-preserving shrink of the latter.

In the distribution task, the participants were asked to allocate ECU among members of another group from their session. Subjects chose between a low-inequality allocation, where 2 individuals received 3 ECU and the other 2 received 2 ECU, and a high-inequality allocation, where 2 individuals received 5 ECU while the other 2 received nothing. Because the participants were materially unaffected by their choice of allocation and efficiency considerations did not apply (i.e., total income was the same in both allocations), this task captured pure distributional preferences. Hence, an inequality-averse subject was expected to choose the low-inequality allocation.¹

One noteworthy feature of the lottery choice and distribution tasks is that they jointly replicated the individual risk and distributional consequences of the free-market and price-cap regimes without the market frame. (The low-risk (high-risk) lottery and the low-inequality (high-inequality) allocation correspond to the free-market (price-cap) regime.) Due to the close match between these two tasks and the shopping exercise, we expected our measures of risk and distributional preferences to be valid for our experimental commodity market.

C Pre-experiment online survey

C.1 Overview

The participants first reported a set of demographic and socioeconomic characteristics, including gender, age, place of birth, university, academic major, socioeconomic stratum, and ethnic origin.

Next, they answered a few questions designed to measure their political tendency. Capturing ideology using surveys is challenging when it comes to the left-right divide, yet some studies have demonstrated the empirical relevance of using this approach in Latin America (see, for instance, Wiesehomeier and Doyle (2012)). Thus, we asked the participants to place themselves on the left-right spectrum using a 1-10 scale. We also asked a set of complementary questions to approximate ideology: The participants' ranking of political parties on the left-right spectrum, candidate choices in the first and second round of the 2022 presidential election in Colombia, and support for presidential candidates.

¹If the distribution task was selected for payment, individual payoffs were determined as follows. Suppose that members of group A decided on the allocation for group B and vice versa. Then, a member of group A was randomly chosen and her allocation choice was implemented in group B, while the allocation choice of a randomly chosen member of group B was implemented in group A.

Last, we elicited the participants' attitudes toward government intervention in the economy using questions regarding government ownership of public enterprises, policies to address poverty and inequality, taxation of the rich, price controls for medicines and food, minimum wage, and import and interest rate controls.

C.2 Questionnaire

Part I

1. What is your gender?
Male — Female — Other
2. How old are you? (Record your age in fulfilled years.)
3. Where were you born? (Enter the city and country.)
4. What university do you attend?
5. What degree(s) do you study at university?
6. What semester(s) of your degree(s) are you currently in?
7. Have you taken economics courses?
Yes — No
8. What is your stratum?
(1 is the poorest stratum, 6 the richest stratum)
1 — 2 — 3 — 4 — 5 — 6
9. Do you consider yourself a white, half-caste, indigenous, black, mulatto or other person?
White — Half-caste — Indigenous — Black — Mulatto — Other
10. Consider a scale of 1 to 10 that goes from left to right, in which 1 means "left" and 10 means "right." Nowadays, when talking about political tendencies, many people talk about those who are more sympathetic to the left or the right.

According to the meaning that the terms “left” and “right” have to you, when you think about your political point of view, where are you on this scale?

Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

11. Where would you place Pacto Histórico?

Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

12. Where would you place Centro Democrático?

Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

13. Thinking of political parties or coalitions in Colombia, how close politically do you feel to:

Liga de Gobernantes Anticorrupción

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Colombia Justa Libres

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Coalición Centro Esperanza

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Movimiento de Salvación Nacional

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Pacto Histórico

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

G.S.C. Colombia Piensa en Grande

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Verde Oxígeno

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

14. In the first round of the last presidential election you voted for:

- Liga de Gobernantes Anticorrupción
- Colombia Justa Libres

- Coalición Centro Esperanza
 - Movimiento de Salvación Nacional
 - Pacto Histórico
 - G.S.C. Colombia Piensa en Grande
 - Verde Oxígeno
 - Did not vote
15. Regardless of whether you voted in the first round of the last presidential election, which candidate did you support?
- Rodolfo Hernández (Liga de Gobernantes Anticorrupción)
 - John Milton Rodríguez (Colombia Justa Libres)
 - Federico Gutiérrez (Equipo por Colombia)
 - Enrique Gómez Martínez (Movimiento de Salvación Nacional)
 - Gustavo Petro (Pacto Histórico)
 - Luis Pérez (G.S.C. Colombia Piensa en Grande)
 - Ingrid Betancourt (Verde Oxígeno)
 - Sergio Fajardo (Coalición Centro Esperanza)
 - None of the above
16. In the second round of the last presidential election you voted for:
- Liga de Gobernantes Anticorrupción
 - Pacto Histórico
 - Did not vote
17. Regardless of whether you voted in the second round of the last presidential election, which candidate did you support?
- Liga de Gobernantes Anticorrupción
 - Pacto Histórico
 - None of the above
18. Which political party would you never vote for?
- Liga de Gobernantes Anticorrupción
 - Colombia Justa Libres

- Coalición Centro Esperanza
- Movimiento de Salvación Nacional
- Pacto Histórico
- G.S.C. Colombia Piensa en Grande
- Verde Oxígeno

Part II

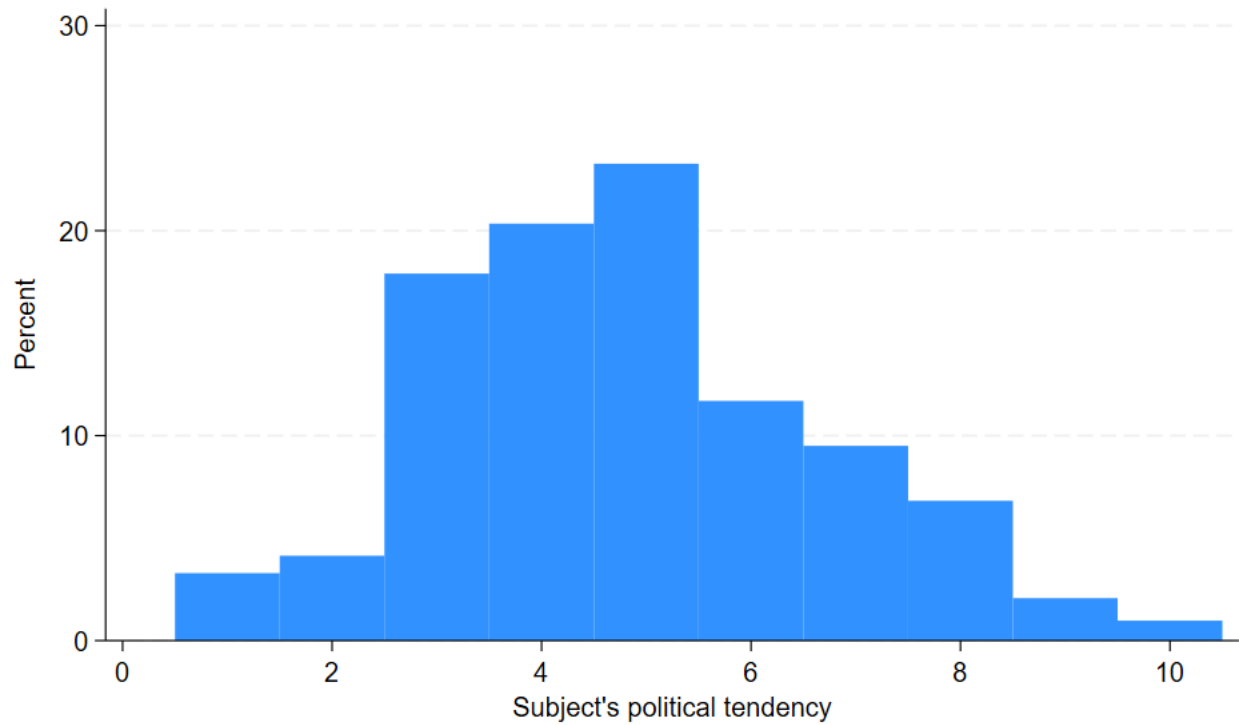
To what extent do you agree or disagree with the following statements? 1 represents “strongly disagree” and 10 represents “strongly agree.”

19. Regardless of which political party is in charge, the Colombian State, instead of the private sector, should be the owner of the country’s most important companies and industries.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
20. Regardless of which political party is in charge, the Colombian State must implement firm policies to reduce income inequality between rich and poor.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
21. Regardless of which political party is in charge, the Colombian State must spend more on helping the poor.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
22. It is unfair that the rich pay a lot in taxes but receive little in state services.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
23. Regardless of which political party is in charge, the Colombian State must regulate the prices of essential goods such as food and medicines.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
24. Regardless of which political party is in charge, the Colombian State must guarantee a minimum wage to cover the basic family basket.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

25. Regardless of which political party is in charge, the Colombian State must regulate the interest rate to ensure that credit is cheap.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
26. Regardless of which political party is in charge, the Colombian State must regulate imports to protect Colombian producers and consumers.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
27. Some people consider that the State tries to do many things that should be left to individuals and businesses. Others believe that the State should do more. What stance comes closest to your position?
The State does too much — The State does the right thing — The State should do more.

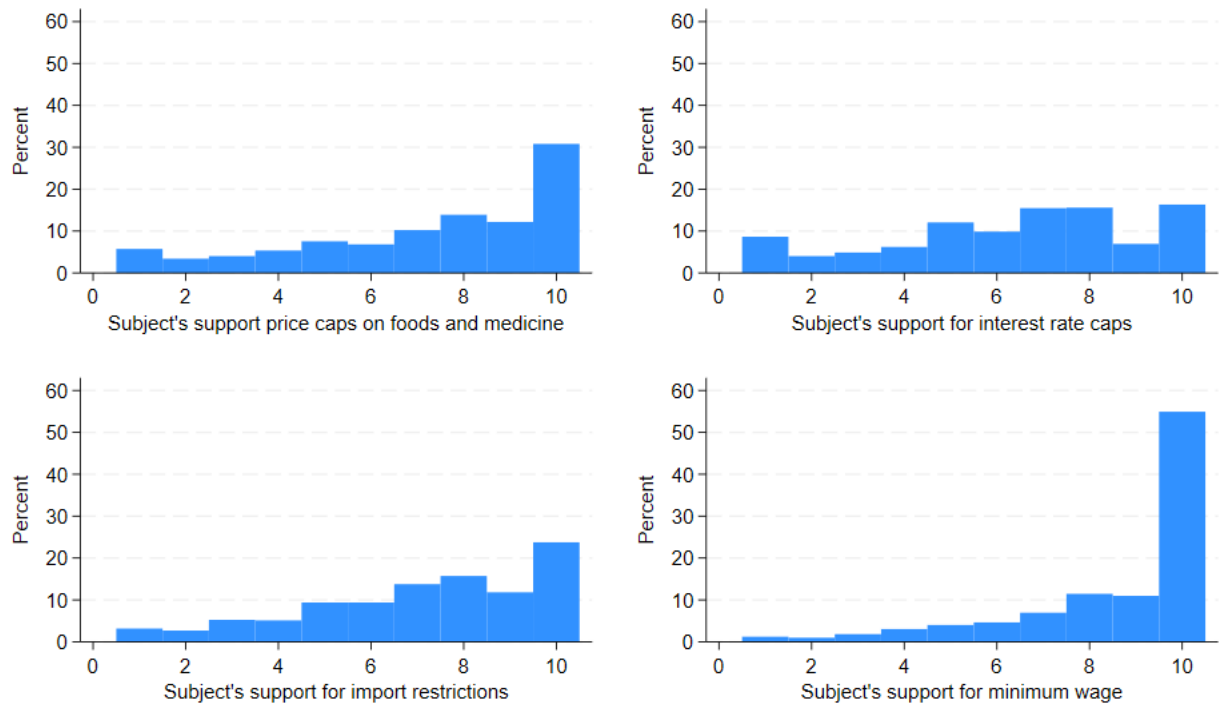
D Additional figures

Figure A1: Distribution of self-reported political tendency



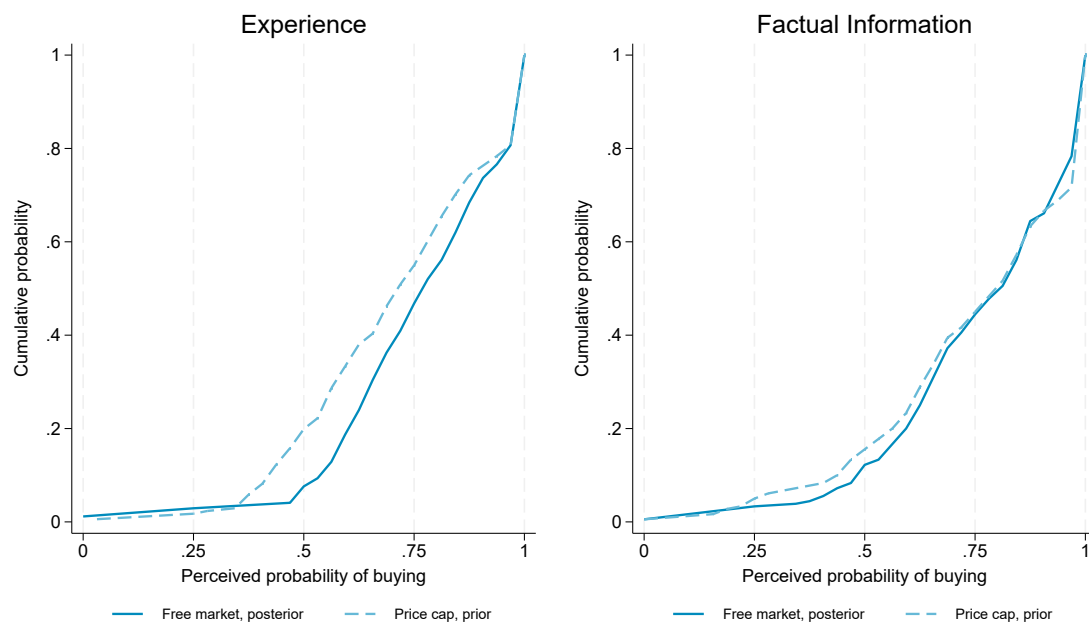
Notes: Self-reported political tendency ranges from 1 (left-leaning) to 10 (right-leaning).

Figure A2: Distribution of support for interventionist policies



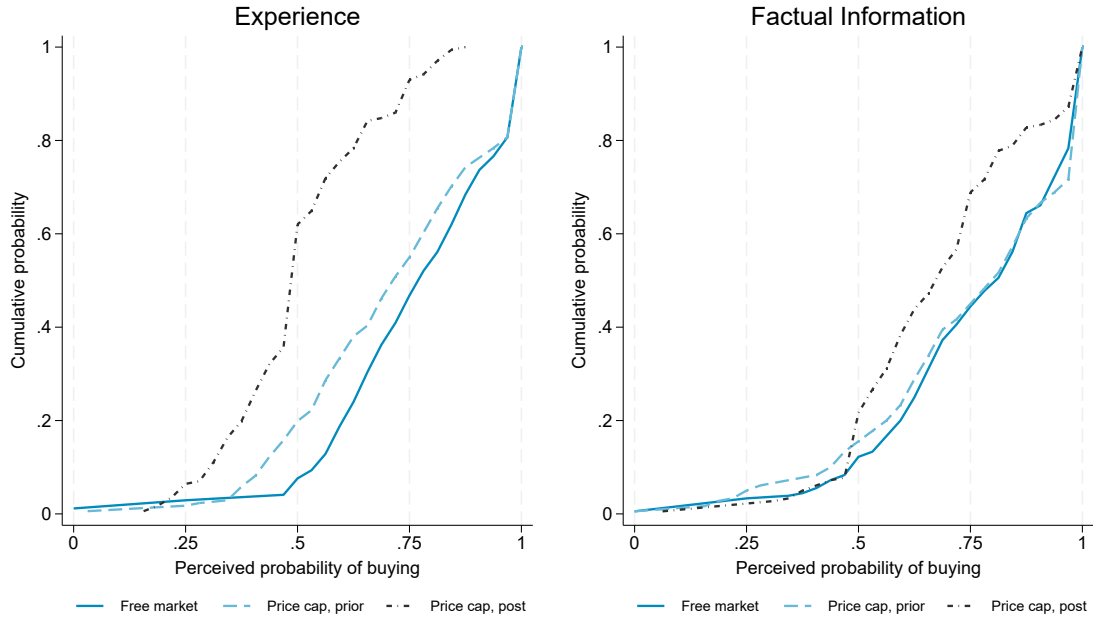
Notes: This figure displays the distribution of participants' support for four interventionist policies: (i) price caps on foods and medicines, (ii) interest rate caps, (iii) import restrictions, and (iv) a minimum wage. Each variable is measured on a 1–10 scale, where higher values indicate stronger support for government intervention.

Figure A3: Baseline beliefs in the Experience and Factual Information treatment arms



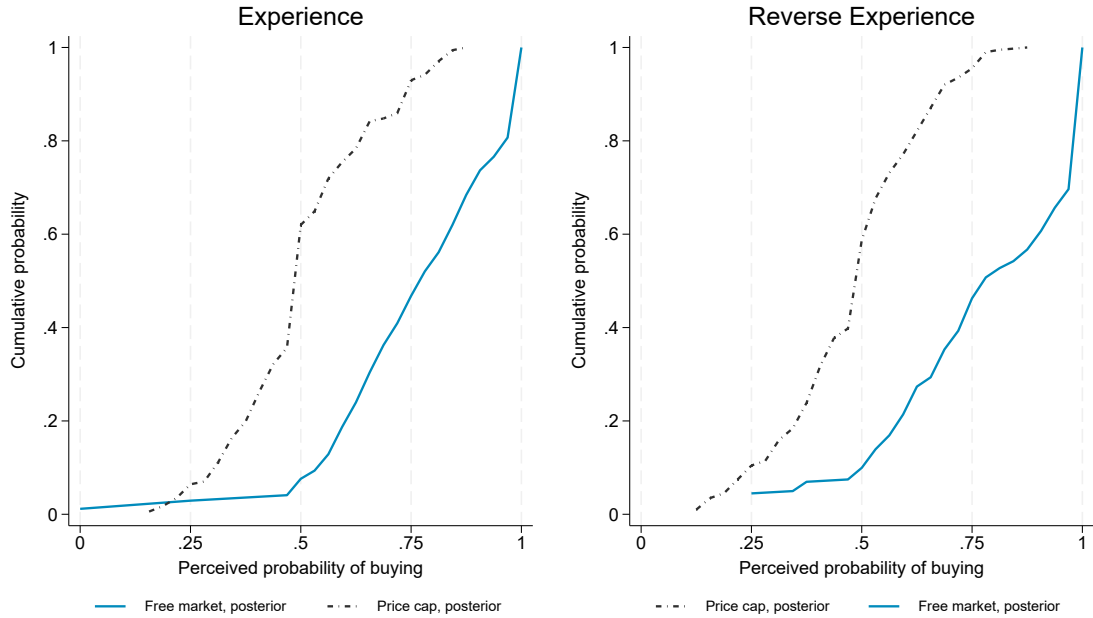
Notes: This figure shows the cumulative distribution of prior beliefs about the probability of buying in the price-cap regime for participants in the Experience and Factual Information treatment arms, using the subsample of participants with consistent beliefs and choices.

Figure A4: Belief updating in the Experience and Factual Information treatment arms



Notes: This figure shows the cumulative distribution of prior and posterior beliefs about the probability of buying in the price-cap regime for participants in the Experience and Factual Information treatment arms, using the subsample of participants with consistent beliefs and choices.

Figure A5: Posterior beliefs in the Experience and Reverse Experience treatment arms



Notes: This figure shows the cumulative distribution of posterior beliefs about the probability of buying in each of the two market regimes for participants in the Experience and Reverse Experience treatment arms, using the subsample of participants with consistent beliefs and choices.

E Additional tables

Table A1: Descriptive statistics of study sample

Variable	Mean	Std. Dev	Min	Max	N
Age	21.68	3.079	16	44	821
Female (=1)	0.548	0.498	0	1	821
Born in Bogotá (=1)	0.737	0.441	0	1	821
Enrolled at Universidad del Rosario (=1)	0.600	0.490	0	1	821
Economics/Finance major (=1)	0.236	0.425	0	1	821
Graduate student (=1)	0.026	0.158	0	1	821
Has taken economics courses (=1)	0.501	0.500	0	1	821
Low- or middle-income neighborhood (=1)	0.622	0.485	0	1	821
Self-reported as white (=1)	0.357	0.479	0	1	821
Political tendency	4.826	1.865	1	10	821
Position of Pacto Histórico	2.801	1.667	1	10	821
Position of Centro Democrático	8.593	1.860	1	10	821
Support: State ownership of companies	4.163	2.568	1	10	821
Support: State must implement policies to reduce inequality	8.024	2.246	1	10	821
Support: State must spend more on helping the poor	6.431	2.187	1	10	821
Support: State must regulate prices	7.252	2.785	1	10	821
Support: State must guarantee a minimum wage	8.569	2.121	1	10	821
Support: State must regulate the interest rate	6.343	2.742	1	10	821
Support: State must regulate imports	7.138	2.517	1	10	821
Left-leaning political orientation (≤ 5 on the original scale)	0.689	0.463	0	1	821
Decisive DM	0.250	0.433	0	1	821
Chose high-risk lottery	0.262	0.440	0	1	821
Chose high-inequality allocation	0.193	0.395	0	1	820

Notes: Political tendency and support for government intervention are measured on 1–10 scales, where higher values indicate more right-leaning views or stronger support. See Section C of Online Appendix I for details on the pre-experiment survey that we used to collect data on socioeconomic characteristics, political tendency, and attitudes toward government intervention in markets.

Table A2: Covariate balance across information treatment arms

Variable	E	FI	RE	E vs. FI	E vs. RE	FI vs. RE	<i>p</i> -value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	21.917 (0.186)	21.811 (0.214)	21.366 (0.159)	0.106 (0.284)	0.551 (0.244)	0.445 (0.263)	0.077
Female (=1)	0.519 (0.031)	0.533 (0.031)	0.587 (0.029)	-0.014 (0.044)	-0.068 (0.042)	-0.054 (0.042)	0.224
Born in Bogotá (=1)	0.780 (0.026)	0.710 (0.028)	0.721 (0.026)	0.070 (0.038)	0.059 (0.037)	-0.011 (0.038)	0.145
Low- or middle-income neighborhood (=1)	0.633 (0.030)	0.637 (0.030)	0.601 (0.028)	-0.004 (0.042)	0.032 (0.041)	0.036 (0.041)	0.622
Self-reported as white (=1)	0.318 (0.029)	0.301 (0.029)	0.440 (0.029)	0.017 (0.041)	-0.121 (0.041)	-0.138 (0.041)	0.001
Enrolled at Universidad del Rosario (=1)	0.576 (0.030)	0.548 (0.031)	0.668 (0.027)	0.027 (0.043)	-0.092 (0.041)	-0.120 (0.041)	0.010
Graduate student (=1)	0.038 (0.012)	0.012 (0.007)	0.027 (0.009)	0.026 (0.014)	0.011 (0.015)	-0.015 (0.012)	0.161
Economics/Finance major (=1)	0.239 (0.026)	0.232 (0.026)	0.238 (0.025)	0.007 (0.037)	0.000 (0.036)	-0.007 (0.036)	0.978
Has taken economics courses (=1)	0.553 (0.031)	0.486 (0.031)	0.466 (0.029)	0.067 (0.044)	0.087 (0.042)	0.020 (0.042)	0.106
Political tendency	4.973 (0.119)	4.726 (0.108)	4.782 (0.110)	0.248 (0.161)	0.192 (0.162)	-0.056 (0.155)	0.278
Position of Pacto Histórico	2.871 (0.105)	2.853 (0.104)	2.695 (0.094)	0.018 (0.148)	0.177 (0.141)	0.159 (0.140)	0.380
Position of Centro Democrático	8.644 (0.119)	8.571 (0.117)	8.567 (0.103)	0.073 (0.167)	0.077 (0.157)	0.004 (0.155)	0.865
Support: State ownership of companies	4.205 (0.157)	4.205 (0.160)	4.091 (0.150)	0.000 (0.224)	0.114 (0.217)	0.114 (0.219)	0.830
Support: State must implement policies to reduce inequality	8.159 (0.126)	7.869 (0.146)	8.040 (0.135)	0.290 (0.192)	0.119 (0.186)	-0.172 (0.198)	0.332
Support: State must spend more on helping the poor	6.333 (0.131)	6.344 (0.140)	6.594 (0.126)	-0.010 (0.192)	-0.261 (0.182)	-0.250 (0.188)	0.274
Support: State must regulate prices	7.258 (0.171)	7.062 (0.177)	7.413 (0.158)	0.196 (0.246)	-0.155 (0.233)	-0.351 (0.237)	0.333
Support: State must guarantee a minimum wage	8.530 (0.133)	8.444 (0.133)	8.711 (0.120)	0.086 (0.188)	-0.181 (0.179)	-0.267 (0.178)	0.312
Support: State must regulate the interest rate	6.269 (0.176)	6.359 (0.170)	6.396 (0.153)	-0.090 (0.245)	-0.127 (0.233)	-0.037 (0.228)	0.856
Support: State must regulate imports	6.958 (0.156)	7.147 (0.155)	7.289 (0.145)	-0.188 (0.220)	-0.330 (0.213)	-0.142 (0.213)	0.299
Chose high-risk lottery	0.250 (0.027)	0.243 (0.027)	0.289 (0.026)	0.007 (0.038)	-0.039 (0.038)	-0.045 (0.038)	0.416
Chose high-inequality allocation	0.202 (0.025)	0.205 (0.025)	0.174 (0.022)	-0.003 (0.035)	0.027 (0.033)	0.030 (0.033)	0.606

Notes: Columns 1 to 3 report, for each treatment arm, the mean and standard error (in parentheses) for each covariate. Columns 4 to 6 report the difference in means and the corresponding standard error (in parentheses) for each pairwise comparison across treatment arms. Column 7 displays, for each covariate, the *p*-value of a joint orthogonality test assessing the null hypothesis that the means are equal across treatment arms. E stands for “Experience,” RE stands for “Reverse Experience,” and FI stands for “Factual Information.” Political tendency and support for government intervention are measured on 1–10 scales, where higher values indicate more right-leaning views or stronger support. See Section C of Online Appendix I for details on the pre-experiment survey that we used to collect data on socioeconomic characteristics, political tendency, and attitudes toward government intervention in markets.

Table A3: Covariate balance between agency treatment arms

Variable	Hypothetical (1)	Decisive (2)	H vs. D (3)	<i>p</i> -value (4)
Age	21.800 (0.131)	21.332 (0.170)	0.469 (0.248)	0.059
Female (=1)	0.550 (0.020)	0.541 (0.035)	0.009 (0.040)	0.825
Born in Bogotá (=1)	0.732 (0.018)	0.751 (0.030)	-0.019 (0.036)	0.592
Low- or middle-income neighborhood (=1)	0.614 (0.020)	0.649 (0.033)	-0.035 (0.039)	0.369
Self-reported as white (=1)	0.344 (0.019)	0.395 (0.034)	-0.051 (0.039)	0.187
Enrolled at Universidad del Rosario (=1)	0.601 (0.020)	0.600 (0.034)	0.001 (0.040)	0.987
Graduate student (=1)	0.026 (0.006)	0.024 (0.011)	0.002 (0.013)	0.901
Economics/Finance major (=1)	0.247 (0.017)	0.205 (0.028)	0.042 (0.034)	0.222
Has taken Economics courses (=1)	0.513 (0.020)	0.463 (0.035)	0.050 (0.040)	0.219
Political tendency	4.867 (0.076)	4.702 (0.128)	0.164 (0.150)	0.274
Position of Pacto Histórico	2.802 (0.068)	2.800 (0.111)	0.002 (0.134)	0.988
Position of Centro Democrático	8.531 (0.078)	8.780 (0.114)	-0.250 (0.150)	0.096
Support: State ownership of companies	4.096 (0.104)	4.366 (0.174)	-0.270 (0.207)	0.192
Support: State must implement policies to reduce inequality	8.019 (0.090)	8.039 (0.161)	-0.020 (0.181)	0.914
Support: State must spend more on helping the poor	6.386 (0.088)	6.566 (0.151)	-0.179 (0.176)	0.309
Support: State must regulate prices	7.172 (0.113)	7.493 (0.191)	-0.321 (0.224)	0.153
Support: State must guarantee a minimum wage	8.555 (0.086)	8.610 (0.148)	-0.055 (0.171)	0.750
Support: State must regulate the interest rate	6.278 (0.111)	6.541 (0.189)	-0.264 (0.221)	0.233
Support: State must regulate imports	7.063 (0.103)	7.361 (0.168)	-0.298 (0.203)	0.143
Chose high-risk lottery	0.265 (0.018)	0.254 (0.030)	0.011 (0.035)	0.758
Chose high-inequality allocation	0.187 (0.016)	0.210 (0.029)	-0.023 (0.032)	0.475

Notes: Columns 1 and 2 report, for each treatment arm, the mean and standard error (in parentheses) for each covariate. Column 3 reports the difference in means and the corresponding standard error (in parentheses) for the comparison between treatment arms. Column 4 displays, for each covariate, the *p*-value of an orthogonality test assessing the null hypothesis that the means are equal between treatment arms. Political tendency and support for government intervention are measured on 1–10 scales, where higher values indicate more right-leaning views or stronger support. See Section C of Online Appendix I for details on the pre-experiment survey that we used to collect data on socioeconomic characteristics, political tendency, and attitudes toward government intervention in markets.

Table A4: Correlates of consistent beliefs and initial regime choices

Variable	Beliefs		Initial choices		Beliefs and choices	
	(1)	(2)	(3)	(4)	(5)	(6)
Experience (=1)	-0.065 (0.044)	-0.058 (0.045)	0.026 (0.032)	0.027 (0.032)	-0.027 (0.046)	-0.018 (0.047)
Factual Information (=1)	-0.035 (0.034)	-0.033 (0.035)	-0.025 (0.025)	-0.026 (0.026)	0.020 (0.036)	0.023 (0.039)
Decisive (=1)	-0.044 (0.032)	-0.047 (0.033)	0.002 (0.029)	-0.003 (0.030)	-0.038 (0.035)	-0.039 (0.036)
Constant	0.803*** (0.026)	1.105*** (0.034)	0.856*** (0.021)	0.624*** (0.070)	0.684*** (0.029)	0.816*** (0.118)
Controls	No	Yes	No	Yes	No	Yes
Number of participants	821	820	821	820	821	820
Number of clusters	45	45	45	45	45	45

Notes: Each column reports the results of a linear regression where the dependent variable is a dummy variable equal to 1 if the subject exhibited consistent beliefs about the probability of buying, consistent initial regime choices, or both, as indicated. “Experience” (“Factual Information”) is a dummy variable for the Experience (“Factual Information”) treatment arm. Control variables in even-numbered columns include risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbol *** denotes that $p < 0.01$ using a two-tailed t -test.

Table A5: Effect of experience vs. factual information on the perceived excess probability of buying in the free market

Variable	Perceived excess prob of buying in FM	
	(1)	(2)
Post (=1)	0.070*** (0.014)	0.066*** (0.012)
Experience (=1)	0.036 (0.022)	0.026 (0.018)
Post $\times$ Experience	0.135*** (0.028)	0.098*** (0.024)
Constant	0.015 (0.016)	-0.018 (0.014)
Omitted treatment arm	FI	FI
Sample	Restricted	Full
Number of participants	351	523
Number of observations	702	1046
Number of clusters	28	28

Notes: The outcome, “Perceived excess prob of buying in FM,” is defined as the participant’s perceived probability of buying in the free-market regime minus the perceived probability of buying in the price-cap regime. The key explanatory variables are: “Post,” a dummy variable for endline observations; “Experience,” a dummy variable for the Experience treatment arm; and their interaction. FI stands for “Factual Information.” Column 1 restricts the sample to participants with consistent beliefs and initial regime choices. Huber-White standard errors clustered by experimental session are reported in parentheses. The symbol *** denotes that $p < 0.01$ using a two-tailed t -test.

Table A6: Effects of order of experience with market regimes on beliefs and regime choices

Variable	PC reduces prob of buying (1)	Chose PC (2)	PC reduces prob of buying (3)	Chose PC (4)
Post (=1)	0.458*** (0.046)	-0.368*** (0.039)	0.423*** (0.046)	-0.242*** (0.028)
Experience (=1)	0.140*** (0.044)	-0.110** (0.047)	0.096** (0.036)	-0.094** (0.039)
Post×Experience	-0.113* (0.056)	0.017 (0.053)	-0.112* (0.056)	-0.008 (0.050)
Constant	0.398*** (0.032)	0.871*** (0.027)	0.332*** (0.030)	0.742*** (0.020)
Omitted treatment arm	RE	RE	RE	RE
Sample	Restricted	Restricted	Full	Full
Number of participants	372	372	562	562
Number of observations	744	744	1124	1124
Number of clusters	31	31	31	31
p -val H_0 : [Experience+ Post×Experience = 0]	0.493	0.090	0.710	0.025

Notes: Each column reports the results of a linear probability model with the dependent variable indicated in the column header. The first outcome, “PC reduces the prob of buying,” is a dummy variable that equals 1 if the participant believes that the price cap reduces the probability of buying and 0 otherwise. The second outcome, “Chose PC,” is a dummy variable that equals 1 if the participant selected the price-cap regime and 0 otherwise. The key explanatory variables are: “Post,” a dummy variable for endline observations; “Experience,” a dummy variable for the Experience treatment arm; and their interaction. RE stands for “Reverse Experience.” Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols *, **, and *** denote that $p < 0.10$, $p < 0.05$, and $p < 0.01$, respectively, using a two-tailed t -test. The reported p -values are from t -tests of the null hypothesis that $\beta_2 + \beta_3 = 0$ in model (4) in the main text.

Table A7: Assessment of the “attention hypothesis” of belief updating

Variable	PC reduces prob of buying (posterior)	
	(1)	(2)
PC reduces prob of buying (prior) (=1)	0.312*** (0.044)	0.330*** (0.045)
Decisive (= 1)	0.155*** (0.050)	0.136*** (0.044)
PC reduces prob of buying (prior)×Decisive	−0.163** (0.068)	−0.104 (0.065)
Belief inertia	−0.343*** (0.054)	−0.389*** (0.046)
Wrong update	−0.448*** (0.059)	−0.416*** (0.056)
Constant	0.690*** (0.140)	0.658*** (0.131)
Controls	Yes	Yes
Sample	Restricted	Full
Number of participants	552	820
Number of clusters	45	45
p -val H_0 : [Decisive+PC reduces prob of buying (prior)×Decisive = 0]	0.831	0.438

Notes: The outcome, “PC reduces prob of buying (posterior),” is a dummy variable that equals 1 if the participant believed at the endline that the price cap reduces the probability of buying relative to the free market and 0 otherwise. The key independent variables are a dummy variable that equals 1 if a participant believed at the baseline that the price cap reduces the probability of buying and 0 otherwise; a dummy variable for Decisive DMs; their interaction; a dummy variable that equals 1 if a participant exhibited belief inertia and 0 otherwise; and a dummy variable that equals 1 if a participant updated her prior belief in the wrong direction and 0 otherwise. Column 1 restricts the sample to participants with consistent beliefs and initial regime choices. Control variables include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbols ** and *** denote that $p < 0.05$ and $p < 0.01$, respectively, using a two-tailed t -test. The reported p -values are from t -tests of the null hypothesis that $\theta_2 + \theta_3 = 0$ in model (5) in the main text.

Table A8: Decisive-Hypothetical gap in intensity of posterior beliefs

Variable	Perceived excess prob of buying in FM (post)			
	(1)	(2)	(3)	(4)
PC reduces prob of buying (post) (=1)	0.426*** (0.019)	0.400*** (0.023)	0.422*** (0.015)	0.409*** (0.016)
Decisive (=1)	-0.025 (0.036)	-0.020 (0.031)	-0.052 (0.033)	-0.048 (0.031)
PC reduces prob of buying (post)×Decisive	0.033 (0.040)	0.031 (0.037)	0.038 (0.038)	0.035 (0.036)
Constant	-0.115*** (0.012)	-0.042 (0.074)	-0.130*** (0.008)	-0.029 (0.062)
Controls	No	Yes	No	Yes
Sample	Restricted	Restricted	Full	Full
Number of participants	552	552	821	820
Number of clusters	45	45	45	45
p -val H_0 : [Decisive+PC reduces prob of buying (post)× Decisive = 0]	0.693	0.560	0.445	0.490

Notes: The outcome, “Perceived excess prob of buying in FM (post),” is defined as the participant’s perceived probability of buying in the free-market regime minus the perceived probability of buying in the price-cap regime, measured at the endline. The key independent variables are: a dummy variable that equals 1 if the participant believed at the endline that the price cap reduces the probability of buying and 0 otherwise; a dummy variable for Decisive DMs; and their interaction. Columns 1 and 2 restrict the sample to participants with consistent beliefs and initial regime choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, the political tendency index, and the index for attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008). Huber-White standard errors clustered by experimental session are reported in parentheses. The symbol *** denotes that $p < 0.01$ using a two-tailed t -test.

SECTION F: PROTOCOL AND INSTRUCTIONS FOR LABORATORY SESSIONS

Below are the experimental instructions translated into English, along with a description of the procedures followed during the sessions. Procedural details are interspersed throughout the instructions; they appear in italics, light blue, and within brackets. Differences across treatment arms are indicated in green and also appear in brackets.

General Instructions

Welcome to this session. This session will last about 45 minutes. You will participate in a study on decision-making. All your decisions will be anonymous and your identity will be kept private.

Participants have been randomly divided into groups of 4. Your group will consist of the same participants throughout the entire session. However, you will not know who these participants are, and they will not know who you are.

Before we begin, we ask you not to talk to other participants during the session. If you have a question, please raise your hand. We will gladly answer your questions in private. If you do not comply with this rule, we will be forced to exclude you from the session. We appreciate your cooperation.

Payment

Your earnings during the session will first be calculated in points. The points will be converted to COP at the end of the session. We will apply the following conversion rate:

1 point = 12,000 COP

You will receive **1.25 points** for completing the session. **You could also earn additional money.** These additional earnings will depend on what you and other participants in your group do during the session. We will explain this in detail later.

This session is divided into 6 sections. In four of them, you will participate in a shopping exercise. You will do two other tasks in the remaining two sections. We will give you instructions at the beginning of each section.

[Factual Information treatment arm only: You will have the opportunity to earn money in 5 of the 6 sections.]

At the end of the session, one of the 6 [Factual Information treatment arm only: 5] sections will be randomly selected, and we will pay you the earnings you obtained in that section. All sections have an equal chance of being selected. Your earnings will be added to your initial payment of 1.25 points.

We remind you that payments will be made digitally only to bank accounts authorized by Universidad del Rosario. Payments will be reflected within the next 48 hours once you have completed and submitted the payment form that we will share with you at the end of the session.

[The order of sections 1-6 displayed below applied to one-half of the sessions of each information treatment arm. In the other half of the sessions, the participants first faced the lottery choice task (section 5 below), then the allocation choice task (section 6 below), and finally the shopping exercise (sections 1-4 below).]

Section 1

In this section, the 4 participants of a group will participate in a **market** as **buyers** of a hypothetical product. In addition, there are 4 sellers in each group. While all buyers are people, the sellers are computers, and from now on, we will call them **automated sellers**.

The market will operate for 8 periods.

In each period, each buyer can buy **1 unit** of the product, and each automated seller can sell **1 unit** of the product. **You can earn money by making a purchase.**

How can you earn money?

The product has a value for each participant that will appear on her computer. The value that the product has for you is called **your value**.

In each period, you can earn money if you buy 1 unit of the product at a market price below your value.

The value of the product can change throughout the periods and may differ among buyers. In each period, you will know only your value; you will not know the values of other buyers, and other buyers will not know your value.

Let's see an example.

The screenshot shows a web interface for a market simulation. It contains the following text and elements:

- Su grupo es: **1**
- Sección: **1**
- Período actual: **1**
- Número de períodos en la Sección 1: **8**
- Su valor: **10**
- ¿Solicitud aceptada?: **Sí**
- Precio de mercado: **6**
- Sus ganancias: **4**
- A green button labeled "Enviar solicitud de compra"
- At the bottom, two blue buttons labeled "Retroceder" and "Siguiente"

The first row (below the top area) shows your value. In this example, your value is 10 points.

If you buy the product in a period, you must pay the market price (we will soon explain how it is determined).

In each period, your earnings are calculated by subtracting the price paid from your value, that is:

Earnings = Your value *minus* Market price.

If you do not buy the product in a period, your value expires. On the other hand, in that case, you do not pay a price. Therefore, your earnings in that period are zero.

How can you buy the product?

In each period, you will send a **purchase order** to the sellers indicating your value (you will soon see how to do it). This will be your “bid.”

Without observing the bids, each automated seller must do two things: (1) decide if they want to sell, and (2) if they decide to sell, make an “offer.”

Automated sellers are not allowed to make an offer below their cost. Costs can differ among sellers. Neither you nor the other buyers in your group will know the cost of any of the sellers. Sellers can make money by selling at a price above their cost.

Once each buyer has sent her purchase order, and each automated seller has decided whether they want to sell and at what price, bids and offers are organized to determine the exchanges.

How are bids and offers organized?

Bids are sorted from **highest to lowest**, while **offers** are sorted from **lowest to highest**. The highest bids are matched with the lowest offers. **Units for which the bid is greater than or equal to the offer are sold**, while the remaining units are not sold.

Let’s look at an example.

Solicitud	Precio sugerido	¿Solicitud aceptada?	Precio de mercado
10	5	Sí	6
8	6	Sí	6
5	7	No	6
4	8	No	6

The first column shows the four bids sorted from highest to lowest. The second column shows the four offers sorted from lowest to highest. The third column indicates, for each bid, whether it was accepted.

The first bid (10) is higher than the offer for that unit (5). Therefore, the first bid is accepted (as shown in the third column), and that unit is sold to the buyer who made the bid. The same happens with the second bid (8), as it is higher than the offer for that unit (6).

On the other hand, the third bid (5) is lower than the corresponding offer (7), and thus is rejected. The same happens with the fourth bid.

How is the market price determined?

In each period, the market price is equal to the offer corresponding to the last unit sold.

Let’s continue with the previous example.

As we just saw, the first 2 units are sold because the bids are higher than the corresponding offers from the automated sellers.

The offer for the last unit sold (the second one) is 6, so the market price is set at 6. This price appears in the fourth column.

Note that at the market price, the first 2 buyers are willing to buy (as their value is higher than 6) and the first 2 automated sellers are willing to sell (as the market price is greater than or equal to their offers).

Review

Let's review the previous example so you can see how everything will be displayed on your computer screen.

In each period, you will see the following screen:



The screenshot shows a web-based interface for a bidding experiment. It contains the following text and elements:

- Su grupo es: **1**
- Sección: **1**
- Periodo actual: **1**
- Número de periodos en la Sección 1: **8**
- Su valor: **10**
- ¿Solicitud aceptada?: **Sí**
- Precio de mercado: **6**
- Sus ganancias: **4**
- A green button labeled "Enviar solicitud de compra" is positioned to the right of the "¿Solicitud aceptada?" field.
- At the bottom, there are two blue buttons: "Retroceder" and "Siguiente".

In the top area, you will first see your group's number (in this example: 1). Then you will see the Section, the current period's number (in this example: 1), and the total number of periods in the Section (8).

Below that, you will see your value.

On the right-hand side of the screen, there is a button that says, "**Submit bid.**" To submit the bid, simply click on the button.

Once all bids and offers have been collected and sorted, you will be able to see (1) whether your bid has been accepted, (2) the market price, and (3) your earnings.

In the example, your value is 10, your bid has been accepted, and the market price is 6. Therefore, your earnings in this period are $10 - 6 = 4$.

Remember that Section 1 features 8 periods. Once the last period is over, you will see a summary of the 8 periods. First, you will see the most frequent market price. Second, you will see the number of periods in which you were able to buy. Third, you will see a summary of your earnings per period.

[The participants answer a few comprehension questions and are given time to resolve any doubts.]

Practice periods

Now you will participate in 2 unpaid practice periods. Then you will have the opportunity to ask questions.

[Reverse Experience treatment arm only: Automated sellers are NOT allowed to submit an offer above 7. This ensures that the market price does not exceed 7.]

[Once the last practice period is over, the participants receive the following instructions.]

Once the first real period begins, there will be no more questions until the last period of Section 1 is completed, so please raise your hand now if you have any questions.

If Section 1 is selected as the section-that-counts for payment, we will randomly select one of the periods and you will receive the earnings from that period. All periods have the same probability of being selected.

[Reverse Experience treatment arm only: Remember that in the practice periods, automated sellers were not allowed to submit an offer above 7, which ensured that the market price did not exceed 7. This rule will also apply to the following real periods.]

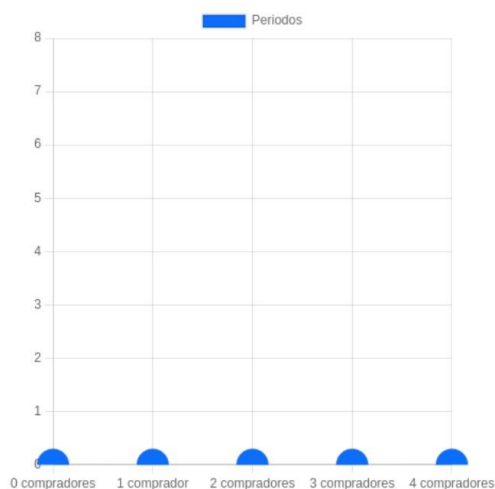
[The participants complete block 1 of the shopping exercise.]

In a moment, you will answer the following question about the 8 periods in which you just participated:

In each period, the number of buyers in your group who were able to make a purchase was 0, 1, 2, 3, or 4.

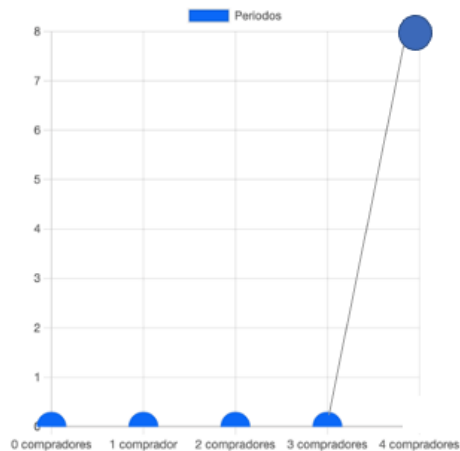
Please indicate how many periods you believe the number of buyers who were able to make a purchase was 0, how many periods it was 1, how many it was 2, how many it was 3, and how many it was 4.

To answer this question, you will need to use the following figure:

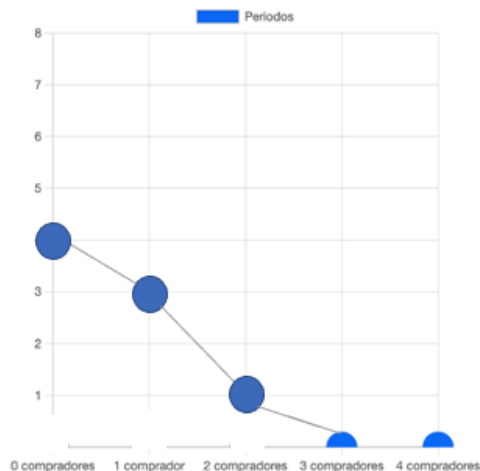


In this figure, there is a grid with five blue circles at the bottom. You now see the bottom half of each circle, but once you select a circle and move it upwards, you will be able to see it in full. To move a circle, simply place the mouse cursor over the circle and move it upwards. You can release the cursor once you are satisfied with the circle's position. Keep in mind that the sum of the numbers you provide must be eight (8) because this is the total number of periods in Section 1. If you violate this rule, you will not be able to advance to the next page.

Let's see how this works with an example. Suppose that you believe that all buyers were able to buy in all eight (8) periods. You would then have to move the last circle (4 buyers) to the highest part of the grid (8 periods). At the end, the figure should look like this on your screen:



Let's look at another example. Now imagine that you believe that in four (4) periods no buyer was able to buy, in three (3) periods only one buyer was able to buy, and in one (1) period only two buyers were able to buy ($4+3+1=8$). Then, you would need to move the first circle (0 buyers) to the position indicating 4 periods, the second circle (1 buyer) to the position indicating 3 periods, and the third circle (2 buyers) to the position indicating 1 period. At the end, the figure should look like this on your screen:



[The participants report their expectations regarding block 1 of the shopping exercise.]

Section 2

In Section 2, you will participate in another 8 periods of the shopping exercise with your group. If Section 2 is selected as the section-that-counts for payment, we will randomly select one of the periods, and you will receive the earnings from that period. All periods have the same probability of being selected. [Factual Information treatment arm only: In Section 2, we will ask you to imagine a **hypothetical** situation, that is, a situation that will not occur. Imagine that you participate in another 8 periods of the shopping exercise with your group.]

The structure and rules of these periods will be the same as in Section 1, except for one difference: [Factual Information treatment arm only: Suppose that the structure and rules of these periods are the same as in Section 1, except for one difference:]

In Section 2, automated sellers are not allowed to submit an offer above 7. This ensures that the market price does not exceed 7. [Reverse Experience treatment arm only: In Section 2, automated sellers are allowed to submit any offer above their cost, including offers above 7.]

Before you participate in the periods of Section 2, one of the buyers in your group will be randomly selected (by a program of the main computer of the laboratory). The buyer who was randomly selected in your group will choose the market rules for Section 3, where there will be 8 more periods. [Factual Information treatment arm only: At some point, you will tell us what you think might happen in these hypothetical periods. Before that, one of the buyers in your group will be randomly selected (by a program of the main computer of the laboratory). The buyer who was randomly selected in your group will choose the market rules for Section 3, where there will be 8 real periods.]

If Section 3 is selected as the section-that-counts for payment, we will randomly select one of the periods, and you will receive the earnings from that period.

The selected buyer will choose one of the following two options:

- In **Section 3**, allow automated sellers to submit any offer above their cost (as in Section 1). [Reverse Experience treatment arm only: In Section 3, do not allow automated sellers to submit an offer above 7, which ensures that the market price does not exceed 7 (as in Section 1).]
- In **Section 3**, do not allow automated sellers to submit an offer above 7, which ensures that the market price does not exceed 7 (as in Section 2). [Reverse Experience treatment arm only: In Section 3, allow automated sellers to submit any offer above their cost (as in Section 2).]

The choice made by the selected buyer will be revealed once we reach Section 3.

Afterwards, the same selected buyer will determine the market rules for Section 4, choosing one of the two options mentioned above.

Even if you were not selected to determine the market rules, we would like to know your preference, so we will ask you to indicate which option you prefer.

Now you will see on your screen whether you have been selected to determine the market rules.

[A buyer is randomly selected in each group. Each buyer sees on her screen whether she has been selected.]

Before choosing the market rules for Section 3, we would like to know your expectations about the upcoming [Factual Information treatment arm only: hypothetical] periods of Section 2.

Remember that in Section 2 automated sellers are not allowed to submit an offer above 7. [Reverse Experience treatment arm only: Remember that in Section 2 automated sellers are allowed to submit any offer above their cost.]

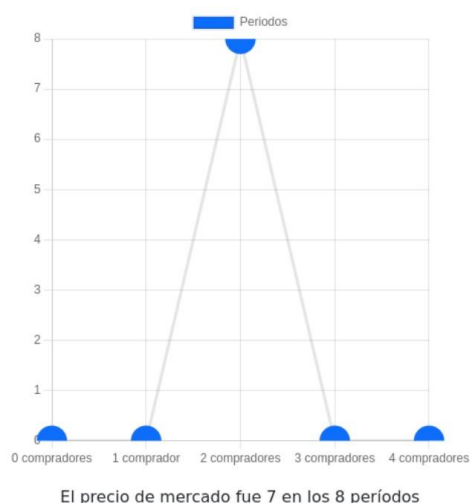
Please answer the following question:

In each period of **Section 2**, the number of buyers in your group who are able to make a purchase will [Factual Information treatment arm: would] be 0, 1, 2, 3, or 4.

Please indicate how many periods you believe the number of buyers who are able to make a purchase will [Factual Information treatment arm: would] be 0, how many periods it will [Factual Information treatment arm: would] be 1, how many it will [Factual Information treatment arm: would] be 2, how many it will [Factual Information treatment arm: would] be 3, and how many it will [Factual Information treatment arm: would] be 4.

[The participants report their expectations regarding block 2 of the shopping exercise and then make their choice of market regime for block 3. Next, participants in the Experience and Reverse Experience treatment arms complete block 2 of the shopping exercise, while participants in the Factual Information treatment arm receive factual information about the price cap regime.]

[Factual Information treatment arm only: It turns out that a group of buyers in a different session participated in the situation we asked you to imagine; that is, they participated in 8 periods in which automated sellers were not allowed to submit an offer above 7. These buyers' experience in that situation is summarized in the following figure:]



Now please answer the following question about the 8 periods in which you just participated. [Factual Information treatment arm only: Now please answer the following question about the hypothetical 8 periods.]

In each period, the number of buyers in your group who were [Factual Information treatment arm only: would have been] able to make a purchase was [Factual Information treatment arm only: is] 0, 1, 2, 3, or 4.

Please indicate how many periods you believe the number of buyers who were [Factual Information treatment arm only: would have been] able to make a purchase was [Factual Information treatment arm only: is] 0, how many periods it was [Factual Information treatment arm only: is] 1, how many it was [Factual Information treatment arm only: is] 2, how many it was [Factual Information treatment arm only: is] 3, and how many it was [Factual Information treatment arm only: is] 4.

[The participants report their updated expectations regarding block 2 of the shopping exercise.]

Section 3

Remember that in this section you will participate in another 8 periods of the shopping exercise with your group. In a moment, we will reveal the market rules that were chosen earlier by the randomly selected buyer in your group.

Before you participate in the periods in Section 3, the selected buyer in your group will choose the market rules for **Section 4**, where there will be another 8 periods. If Section 4 is selected as the section-that-counts for payment, we will randomly select one of the periods, and you will receive the earnings from that period.

Now, the selected buyer will choose one of the following two options:

- In **Section 4**, allow automated sellers to submit any offer above their cost (as in Section 1). *[Reverse Experience treatment arm only: In **Section 4**, do not allow automated sellers to submit an offer above 7, ensuring that the market price will not exceed 7 (as in Section 1).]*
- In **Section 4**, do not allow automated sellers to submit an offer above 7, ensuring that the market price will not exceed 7 (as in Section 2). *[Reverse Experience treatment arm only: In **Section 4**, allow automated sellers to submit any offer above their cost (as in Section 2).]*

The choice made by the selected buyer will be revealed once we reach Section 4.

As before, even if you were not selected to determine the market rules, we would like to know your preference.

[All participants make their choice of market regime for block 4 of the shopping exercise. Next, they learn the regime that applies to block 3 and complete block 3.]

Section 4

[The participants learn the regime that applies to block 4 of the shopping exercise and complete block 4.]

Section 5

The computer will now randomly assign you a red or blue ticket. Half of the participants will receive a red ticket, and the other half will receive a blue ticket. You will use the ticket to participate in a lottery that we will describe in a moment.

[Tickets are randomly assigned. Each participant sees the color on her screen.]

To implement the lottery, we will use a plastic urn containing **5 red balls and 5 blue balls**. You will choose between two lotteries with real payoffs. Once you have made your choice, the instructor will randomly draw a ball from the urn in front of all participants and will show it to all of them.

You can choose between the following two lotteries:

- Lottery 1:
 - If the color of the ball drawn matches the color of your ticket, you receive **3 points**.
 - If the colors are different, you receive **2 points**.
- Lottery 2:
 - If the color of the ball drawn matches the color of your ticket, you receive **5 points**.
 - If the colors are different, you receive **0 points**.

If Section 5 is selected as the section-that-counts for payment, you will receive the earnings from the chosen lottery.

[The participants choose a lottery. Next, the instructor draws a ball from the urn and payoffs from this task are realized.]

Section 6

Remember that you are a member of group **#OwnGroupNumber#**. Consider group **#ReferenceGroupNumber#** of this same session.

We ask you to choose one of two possible money allocations among members of group **#ReferenceGroupNumber#**. The two possible allocations are:

- **Allocation 1:**
 - Two members randomly chosen from group **#ReferenceGroupNumber#** receive **3 points**, and the other two receive **2 points**.
- **Allocation 2:**
 - Two members randomly chosen from group **#ReferenceGroupNumber#** receive **5 points**, and the other two receive **0 points**.

If Section 6 is selected as the section-that-counts for payment, a member of **your group** will be randomly selected, and that person's allocation choice will be implemented in group **#ReferenceGroupNumber#**. Similarly, a member of group **#ReferenceGroupNumber#** will be randomly selected, and that person's allocation choice will be implemented in your group.

[The participants choose an allocation. Next, the section-that-counts for payment is randomly selected. Finally, the participants complete the payment form and are dismissed.]

Online Appendix II for “Why do citizens support price controls? The role of motivated beliefs”

Original Pre-Analysis Plan and Implementation Report

Stanislao Maldonado¹ and Santiago I. Sautua²

Tecnologico de Monterrey

May 2026

¹Email: stanislao.maldonado@tec.mx

²Email: santiago.sautua@tec.mx

Abstract

This document combines the original pre-analysis plan for the study “Why do citizens support price controls? The role of motivated beliefs” with an implementation report that documents the empirical analyses conducted after data collection. The purpose of the document is to provide a transparent account of how the analyses reported in the manuscript relate to the design, hypotheses, outcomes, and statistical procedures specified in the original pre-analysis plan.

The first part of the document reproduces the original pre-analysis plan. The second part describes the implementation of the planned empirical strategy, reports the corresponding results, and discusses the extent to which each analysis follows the procedures specified *ex ante*. When the analyses deviate from the original plan, the report identifies the deviation, explains the reason for the adjustment, and clarifies how the revised analysis contributes to the interpretation of the findings. The document also presents additional analyses that were not part of the original pre-analysis plan but were included to improve comparability with the manuscript, address issues that became apparent during implementation, or provide a more complete assessment of the main hypotheses.

Overall, the report is intended to facilitate the evaluation of the study by making explicit which results correspond directly to the pre-specified analysis, which results reflect implementation adjustments, and which results should be interpreted as complementary or exploratory evidence. This structure allows readers to assess the consistency between the original research design and the final empirical analysis, while also documenting the analytical choices made after observing the data.

Outline

1. Part I. Original pre-analysis plan
2. Part II. PAP implementation, clarifications, and deviations
3. Part III. Tables supporting the implementation report

Implementation-report tables

- Table B1. Summary of PAP alignment and implementation clarifications
- Table B2. Outcome-construction crosswalk for PAP analyses
- Table B3. Pre-exposure belief comparisons
- Table B4. Pre-exposure between-arm prior comparison
- Table B5. Direct exposure effects on beliefs
- Table B6. Direct exposure effects on regime choices
- Table B7. Order effects
- Table B8. Factual information effects on beliefs
- Table B9. Factual information effects on regime choices
- Table B10. Direct exposure versus factual information
- Table B11. Agency effects on beliefs
- Table B12. Agency effects on regime choices
- Table B13. DRETS and regime choices
- Table B14. Main Benjamini-Hochberg adjustments: Panel A. DRETS associations
- Table B15. Main Benjamini-Hochberg adjustments: Panel B. Agency effects
- Table B16. Main Benjamini-Hochberg adjustments: Panel C. Direct exposure effects
- Table B17. Main Benjamini-Hochberg adjustments: Panel D. Direct exposure versus factual information
- Table B18. Main Benjamini-Hochberg adjustments: Panel E. Factual information effects
- Table B19. Main Benjamini-Hochberg adjustments: Panel F. Pre-exposure beliefs
- Table B20. Restricted-sample Benjamini-Hochberg robustness: Panel A. DRETS associations
- Table B21. Restricted-sample Benjamini-Hochberg robustness: Panel B. Agency effects
- Table B22. Restricted-sample Benjamini-Hochberg robustness: Panel C. Direct exposure effects

- Table B23. Restricted-sample Benjamini-Hochberg robustness: Panel D. Direct exposure versus factual information
- Table B24. Restricted-sample Benjamini-Hochberg robustness: Panel E. Factual information effects
- Table B25. Restricted-sample Benjamini-Hochberg robustness: Panel F. Pre-exposure beliefs
- Table B26. PAP-specified regression and LPM specifications
- Table B27. PAP Section 6.2 survey index variables
- Table B28. Survey-index associations with baseline support and information updating
- Table B29. Precision diagnostics for selected heterogeneity analyses

Part I. Original pre-analysis plan

Voters' Demand for Price Caps

Pre-Analysis Plan[‡]

Stanislao Maldonado[§]
Tecnologico de Monterrey

Santiago Sautua
Tecnologico de Monterrey

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For the latest version of this document, click here.

Abstract

We plan to conduct a laboratory experiment, combined with a survey, to study citizens' support for price caps in competitive markets. We are interested in understanding how such support is affected by external information about the effects of price caps, direct exposure to them, and attitudes toward market regulation.

Keywords: Price caps; market regulation; inefficient economic policies.

JEL Classification Numbers: C93, D73, O15.

[‡]This section reproduces the original version of the Pre-Analysis Plan (PAP) for this study, with minor edits to update our current affiliations. The original PAP can be downloaded from the AEA RCT Registry at <https://www.socialscienceregistry.org/trials/10626>. We thank participants of the Bogotá Experimental and Behavioral Economics Seminar (BEBES) for feedback on a previous version of this study's research design.

[§]Maldonado: Tecnologico de Monterrey, Department of Economics, stanislao.maldonado@tec.mx; Sautua: Tecnologico de Monterrey, Department of Economics, santiago.sautua@tec.mx.

1 Introduction

This document outlines a pre-analysis plan to study citizens' support for price caps in the laboratory. We will examine several factors that could drive such support. First, we will test whether limited exposure to price caps increases support for this policy. This may be the case if citizens fail to anticipate the negative effects of price caps on market outcomes. Second, because in practice citizens delegate the choice of policies to politicians, we will test whether citizens' lack of agency increases their support for price caps. Third, we will test whether providing information about the market consequences of price caps reduces citizens' support. Finally, we shall also study the role of citizens' political attitudes and their attitudes toward market regulation.

The plan is outlined as follows. Section 2 outlines the study's research questions. Section 3 describes the experimental design. Section 4 presents the main and auxiliary hypotheses. Section 5 describes the empirical analysis to test the study's hypotheses. Section 6 discusses the construction of indexes of political ideology and support for government intervention in the economy based on survey data. Finally, Section 7 discusses issues about inference.

2 Research questions

This study will address the following questions:

- Do individuals who lack specialized knowledge in economics tend to support price caps when they have little experience with such policy? Do they anticipate the consequences of price caps in competitive markets?
- In practice, a typical citizen is never in charge of implementing a price cap, and hence is not responsible for its consequences. Does this lack of agency increase support for price caps?
- Does direct exposure to a price cap lead to more negative expectations and reduce support for this policy?
- Does information about market outcomes under a price cap regime lead to more negative expectations and reduce support for this policy, even in the absence of personal experience? How does such information compare with direct exposure to a price cap?

- Does a general taste for government intervention undermine the effectiveness of information about price caps in reducing support for this policy?

3 Experiment

3.1 General aspects

Using the Online Recruitment System for Economic Experiments (ORSEE) (Greiner, 2004), we will invite students from several universities in Bogotá to attend a session at the Rosario Experimental and Behavioral Economics Laboratory (REBEL). Those who accept our invitation will be asked to complete an online survey, which collects information on socioeconomic characteristics, political attitudes, and attitudes toward government intervention in markets. A few days later, subjects will attend the session at the REBEL. On arrival at the laboratory, they will be divided into groups of four based on their political attitudes and attitudes toward government intervention (as measured by the survey), and they will participate in several tasks.¹

All the information collected in the survey and the laboratory experiment will be anonymous. Before completing the survey, respondents will follow written instructions to develop a unique respondent code, which we will then use to match subjects' responses to the data from the experiment. The personal code will be known only to the subject (that is, neither the researchers nor other participants will know it).

The Appendix contains the survey translated into English. Next, we describe each experimental task in detail. Figure 1 summarizes the timeline of an experimental session.

[Figure 1 about here]

3.2 Market Task

The Market Task involves trading a hypothetical commodity. It begins with a trial block of 2 trading periods. Following the trial block, there are 4 blocks of 8 trading periods each.

¹As described later in Section 6, survey questions will be used to create two indexes, one capturing political ideology and another capturing attitudes toward government intervention, and both indexes will be combined to create a summary index. In each session, participants will be ranked according to the value of this summary index. Then, they will be divided into groups of four based on such ranking, so that each group will be composed of subjects with similar values of the summary index. Higher values of these indexes reflect more pro-left and pro-intervention attitudes.

INSTITUTION. We use a simplified uniform-price sealed bid/offer auction as the trading institution. Each buyer submits a “purchase order” by bidding their valuation. Each seller submits a “suggested price” at which they offer the product, which coincides with their cost. Valuations and costs are private information.

In each period, valuations (suggested prices) are sorted from highest to lowest (lowest to highest). In a free market, all bids exceeding suggested prices are accepted. The suggested price corresponding to the last unit traded determines the uniform market price p^* for all units traded.

As we explain below, some trading periods feature a *binding price cap* $\bar{p} < p^*$ which is fully enforceable. Importantly, units for which the cost exceeds $\bar{p}$ are not accepted to trade because sellers cannot sell at a loss.

ENVIRONMENT. Our environment consists of 4 robot sellers and 4 human buyers interacting in the uniform-price sealed bid/offer auction described above.²

Trading periods are divided into 4 blocks. Within a block, all periods feature either a free market or a binding price cap. Table 1 summarizes the environment with the induced values and costs for buyers and sellers, as well as the market price and expected surplus in equilibrium depending on whether there is a price cap.

[Table 1 about here]

In trading periods with a free market, there is a unique competitive equilibrium of 4 units traded at a price of 9 experimental currency units (ECU). The surplus is distributed asymmetrically, with sellers earning around 60% of total surplus in equilibrium.

In the trading periods with a price cap, which is set to 7 ECU, only 2 buyers make a purchase. However, the lower price compensates on aggregate for the reduction in units such that the total consumer surplus remains unchanged in absolute terms. This feature isolates the effects of the price cap on the demand side from those on the supply side. On the demand side, the price cap affects both the likelihood of making a purchase and the distribution of consumer surplus. The reduction in traded units and price reduces producer surplus by one third, from 15 ECU to 10 ECU. As a result, efficiency is reduced by 20%.

²After conducting this experiment, we might examine the robustness of the main results to changes in the trading institution. For instance, we might implement a uniform-price double-auction call market (which would still ensure a single equilibrium price) or a posted-offers market.

Within a block, every period we randomly reshuffle valuations across buyers holding the demand curve constant, thus keeping expected payments constant across buyers.

TREATMENT ARMS. Subjects are randomly assigned into one of three treatment arms: Free Market First-Experience (FM-E), Free Market First-Information (FM-I), and Price Cap First-Experience (PC-E). To facilitate logistics, all subjects participating in a given experimental session are randomly assigned to the same treatment arm. In the FM-E and FM-I treatments, both the practice trials and the first block of the Market Task feature a free market. By contrast, in the PC-E treatment, both the practice trials and the first block of the Market Task feature a price cap of 7 ECU.

Once the first block finishes, subjects in the FM-E treatment are informed that a price cap of 7 ECU will be imposed in the second block, whereas subjects in the PC-E treatment are informed that the initially imposed price cap of 7 ECU will be removed in the second block. Subjects in the FM-I treatment do not trade during the second block.

INITIAL CHOICE OF REGIME. In all three treatments, before the second block begins, subjects choose the regime to be implemented in the *third* block, namely a free market or a market with the 7,000 price cap. Specifically, one buyer in each group is randomly assigned to choose the regime for the third block in his or her group. We refer to this person as the *decisive chooser*. Each of the three remaining buyers in the group also indicates which regime he or she would like to implement in the third block, but this choice is hypothetical. We refer to these three people as the *hypothetical choosers*. Importantly, each subject learns whether he or she is a decisive or hypothetical chooser *before* choosing the regime for the third block. At this point, subjects do not learn the choices of others.

FINAL CHOICE OF REGIME. Before the third block begins, subjects choose the regime to be implemented in the *fourth* block. In each group, the buyer who was previously assigned to be decisive remains decisive, while the others remain as hypothetical choosers. Buyers are reminded of their role before making a choice. At this point, subjects do not learn the choices of others.

Once all subjects have indicated their choice for the fourth block, they trade in the third and fourth blocks under the regime chosen by the decisive chooser for each block.³

³Subjects learn the regime of the fourth block only after the third block ends.

3.3 Belief Elicitation Task

Throughout the session, subjects report a series of beliefs, which we describe below. The reports are *not* monetarily incentivized.

Once the first block finishes, each subject is reminded of how many periods in that block (out of 8, the total number of periods) he or she made a purchase. Next, subjects in all treatments report how many periods they think their group purchased x units, for $x = 0, 1, 2, 3, 4$.

In the FM-E and FM-I treatments (PC-E treatment), this belief elicitation exercise uncovers purchase expectations regarding a free market (price cap) regime following exposure to that regime.

Before the second block begins, subjects in the FM-E and PC-E treatments indicate their purchase expectations for their group in the upcoming block. Additionally, they report the price they think their group will observe most often across periods (i.e., the modal price). Because subjects in the FM-I treatment do not trade during the second block, they consider a *hypothetical* block with a price cap of 7 ECU and report what market outcomes they think their group *would* experience in such a block. (As in the other treatments, such market outcomes are the modal price and the distribution of total purchases across periods.) In all treatment arms, belief elicitation precedes subjects' choice of regime for the third block.

In the FM-E and FM-I treatments (PC-E treatment), this belief elicitation exercise uncovers *prior* expectations about market outcomes under a price cap (free market) regime.

Once the second block finishes, each subject in the FM-E and PC-E treatment is reminded of how many periods in that block (out of 8) he or she made a purchase. Next, subjects in the FM-E and PC-E treatments are given the opportunity to revise their reports about the second block based on their own experience. Subjects in the FM-I treatment, who did not participate in the second block, are informed about the market outcomes experienced in that block by a group randomly chosen from the FM-E treatment. After receiving such information, subjects in the FM-I treatment revise their expectations about the hypothetical second block for their own group. In all treatment arms, belief elicitation precedes subjects' choice of regime for the fourth block.

In the FM-E and FM-I treatments (PC-E treatment), this belief elicitation exercise uncovers *posterior* expectations about market outcomes under a price cap (free market) regime.

3.4 Distribution and Risk Equivalent Tasks (DRETS)

In addition to participating in the Market and Belief Elicitation Tasks, the subjects make a choice between two individual lotteries and perform a distribution task. These two additional tasks jointly replicate the individual risk and distributional consequences of market equilibria without the market frame. Hence, we refer to these two tasks as the Distribution and Risk Equivalent Tasks (DRETS). The DRETS provide relevant information about subjects' risk and distributional preferences that complements reported expectations about market outcomes. We use such information to assess whether subjects' risk and distributional preferences could help explain their regime choices in the Market Task.

In one half of the sessions, the subjects face the DRETS before the Market Task is introduced, whereas in the other half of the sessions they face the DRETS once the Market and Belief Elicitation Tasks have been completed. Table 2 summarizes the DRETS.

[Table 2 about here]

In the lottery choice task, subjects choose between a low-risk lottery, which pays either 3 ECU or 2 ECU with equal probability, and a high-risk lottery, which pays either 5 ECU or 0 ECU with equal probability. Risk-averse subjects are expected to choose the low-risk lottery over the high-risk lottery because the former is a mean-preserving shrink of the latter. The key feature of this task is that the low-risk (high-risk) lottery replicates the individual surplus risk that a buyer faces in the Market Task in a period without (with) a price cap. Therefore, holding everything else constant, a risk-averse buyer who correctly perceives risk in the Market Task should be more likely to support the free market regime compared to a risk-seeking buyer.

In the distribution task, subjects must allocate income among members of another group from their session. The subjects choose between a low-inequality allocation, where 2 individuals receive 3 ECU and the other 2 receive 2 ECU, and a high-inequality allocation, where 2 individuals receive 5 ECU while the other 2 receive nothing. Because subjects are materially unaffected by their choice of allocation, and efficiency considerations do not apply (total income is the same in both allocations), this task captures pure distributional preferences. Hence, an inequality-averse subject is expected to choose the low-inequality allocation. Importantly, once we remove selfishness considerations from the Market Task, the low-inequality (high-inequality) allocation replicates the distribution

of buyers' surplus in a trading period without (with) a price cap. Therefore, holding everything else constant, an inequality-averse buyer who understands the distributional consequences of both regimes should be more likely to support a free market regime compared to a buyer who is not inequality averse.

3.5 Payment

At the beginning of the session, subjects are informed that either one trading period from the Market Task, or the lottery choice task, or the distribution task will be randomly selected for payment at the end of the session.⁴ Such payment is added to the participation fee.

4 Main hypotheses

We next summarize our main hypotheses:⁵

- Before receiving any specific information about the effects of the price cap, subjects in the FM-E and FM-I treatments will fail to anticipate that the cap leads to a shortage of the commodity. Hence, they will generally choose the price cap regime.
- In the FM-E and PC-E treatments, *direct exposure* to *both* regimes will lead to more negative expectations about the price cap compared with direct exposure to a *single* regime. Hence, direct exposure to both regimes will reduce the probability that subjects choose the price cap regime.
- In the FM-I treatment, subjects will hold more negative expectations about the price cap after receiving *external information* about its effects, and hence will be less likely to choose the price cap regime.
- In all treatments, the reduction in support for the price cap caused by information about its effects will be *less* pronounced among hypothetical choosers than among decisive choosers.

⁴If the distribution task is selected for payment, individual payoffs are determined as follows. Suppose members from group A decide on the allocation for group B, and vice versa. Then, one member from group A is randomly chosen and his or her choice of allocation is implemented in group B, while the choice of allocation of a randomly chosen member of group B is implemented in group A.

⁵Here we omit the theoretical framework that we used to derive our hypotheses. We will include such framework in the manuscript.

That is, when individuals are *not* accountable for the implementation of the price cap, information will be *less* effective in reducing support.

5 Empirical analysis

This section describes the statistical tests to be performed in this study. We will follow two complementary approaches depending on the hypothesis: (1) parametric and non-parametric tests to evaluate hypotheses using between and within-subjects designs, and (2) linear regressions for between and within-subjects designs.

We will primarily use non-parametric tests, but parametric tests will be preferred when suitable non-parametric tests are not available. Linear regressions will be preferred in cases where, in addition to the lack of suitable non-parametric tests, it is important to control for covariates or add fixed effects (e.g., group fixed effects). Given the nature of our experiment, Huber-White standard errors will be computed. As a robustness check, we will also implement different methodological alternatives to adjust statistical inference for clustered data at the session level. Finally, we also need to account for whether the dependent variable is continuous or discrete.

Non-parametric testing for between-subjects comparisons will be based on a **Mann-Whitney test**. We will use a variant of this test for non-clustered and clustered data. Alternatively, we will use a parametric **two-sample t-test** when outcomes are continuous and **Pearson’s Chi-squared test** for discrete outcomes. We will also use variants of these tests for clustered data as a robustness check. In particular, we will use block-bootstrap to relax distributional assumptions accounting for clustering. We will also use a linear regression to accommodate covariates. Standard errors will also be computed using Huber-White estimator and cluster/block-bootstrap as a robustness check for clustered data.

Non-parametric testing for within-subjects comparisons will be based on a **McNemar’s change test** for discrete outcomes and a **Wilcoxon signed-rank test** for continuous outcomes. When feasible, we will use variants of these tests for clustered data as robustness. Alternatively, we will use a parametric **paired-comparison t-test** for continuous outcomes using block-bootstrap to account for clustering and relax normality. As above, linear regressions will be used to accommodate covariates.

5.1 Pre-exposure beliefs about price caps

Within-variation in the FM-E and FM-I treatments. We run non-parametric tests for the initial difference in beliefs about each regime in the FM-E and FM-I treatment arms, exploiting a within-subjects design. We test the following null hypothesis:

$$H_0 : (PC_{ig}^{prior} - FM_{ig}) < 0. \quad (1)$$

PC_{ig}^{prior} is the prior belief about the quantity traded under the price cap regime held by subject i from group g . FM_{ig} is the (posterior) belief about the quantity traded under the free market regime. The null hypothesis is based on the premise that subjects (1) anticipate that the price cap will lead to a shortage of the commodity and (2) are not biased by their attitudes toward market regulation.

Between-variation in the FM-E and PC-E treatments. Alternatively, we can run the following regression comparing beliefs about the free market and price cap regimes in the FM-E and PC-E samples in a between-subjects design.

We create a new variable *Prior* that combines the priors about quantity traded under the price cap regime held by the FM-E sample and the priors about quantity traded under the free market regime held by the PC-E sample. We run a regression on a dummy variable for being assigned to the FM-E treatment arm using least squares and clustering standard errors at the group level:

$$Prior_{ig} = \beta_0 + \beta_1 FME_{ig} + \epsilon_{ig}. \quad (2)$$

The null hypothesis is that $\beta_1 < 0$, that is, subjects (1) anticipate that the price cap will lead to a shortage of the commodity and (2) are not biased by their attitudes toward market regulation.

5.2 Effects of direct exposure to a price cap

5.2.1 Effect on beliefs about the quantity traded

Within-variation in the FM-E and PC-E treatments. We run non-parametric tests for the change in beliefs about the quantity traded in the FM-E and PC-E treatment arms exploiting a within-subjects design. For the FM-E treatment arm, we test the following null hypothesis:

$$H_0 : (PC_{ig}^{post} - PC_{ig}^{prior}) = 0. \quad (3)$$

The alternative hypothesis is

$$H_A : (PC_{ig}^{post} - PC_{ig}^{prior}) < 0. \quad (4)$$

For the PC-E group, we test the following null hypothesis:

$$H_0 : (FM_{ig}^{post} - FM_{ig}^{prior}) = 0. \quad (5)$$

The alternative hypothesis is

$$H_A : (FM_{ig}^{post} - FM_{ig}^{prior}) > 0. \quad (6)$$

For both treatment arms, the null hypothesis is that exposure to a regime does not affect subjects' beliefs about the quantity traded under that regime. This is because subjects (1) already know the consequences of each regime and (2) are not biased by their attitudes toward market regulation.

5.2.2 Effect on choice of regime

Within-variation in the FM-E treatment. We run a non-parametric test for the change in regime choices in the FM-E treatment arm exploiting a within-subjects design. We test the following null hypothesis:

$$H_0 : (Cap_{ig}^{final} - Cap_{ig}^{initial}) = 0, \quad (7)$$

where $Cap_{ig}^{initial}$ (Cap_{ig}^{final}) is a dummy variable that equals 1 if subject i in group g chose the price cap regime the first (second) time and 0 otherwise. This test compares regime choices for the third block (made after experiencing only the free market regime) with choices for the fourth block (made after experiencing both regimes). The null hypothesis is that exposure to the price cap does not affect subjects' regime choice, because subjects already know the consequences of each regime and are not biased by their attitudes toward market regulation.

The alternative hypothesis is

$$H_A : (Cap_{ig}^{final} - Cap_{ig}^{initial}) < 0. \quad (8)$$

We can also run the following regression for the FM-E treatment arm:

$$Cap_{ig}^{final} = \beta_0 + \beta_1 Cap_{ig}^{initial} + \epsilon_{ig}^{final}. \quad (9)$$

The null hypothesis is that $\beta_0 = 0$ and $\beta_1 = 1$.

Between-variation in the FM-E and PC-E treatments. A supplementary test consists of comparing the choices for the fourth block in the PC-E group (i.e., following exposure to both regimes) with the choices for the third block in the FM-E group (i.e., following exposure to the free market regime only). We can test the following null hypothesis:

$$H_0 : (Cap_{ig}^{final})_{PC-E} - (Cap_{ig}^{initial})_{FM-E} = 0. \quad (10)$$

The alternative hypothesis is

$$H_A : (Cap_{ig}^{final})_{PC-E} - (Cap_{ig}^{initial})_{FM-E} < 0. \quad (11)$$

5.2.3 Order effects

We also test for order effects. We run the following regression pooling observations from the FM-E and PC-E groups:

$$Cap_{ig}^{final} = \beta_0 + \beta_1 FME_{ig} + \epsilon_{ig}^{final}. \quad (12)$$

The null hypothesis of no order effects implies that $\beta_1 = 0$.

5.3 Effects of external information about price caps

5.3.1 Effect on beliefs about the quantity traded

Within-variation in the FM-I group. We run non-parametric tests for the change in beliefs about trade in the FM-I treatment arm exploiting a within-subjects design. We test the following null hypothesis:

$$H_0 : (PC_{ig}^{post} - PC_{ig}^{prior}) = 0. \quad (13)$$

The alternative hypothesis is

$$H_A : (PC_{ig}^{post} - PC_{ig}^{prior}) < 0. \quad (14)$$

5.3.2 Effect on choice of regime

Within-variation in the FM-I group. We run a non-parametric test for the change in regime choices in the FM-I treatment arm exploiting a within-subjects design. We test the following null hypothesis:

$$H_0 : (Cap_{ig}^{final} - Cap_{ig}^{initial}) = 0. \quad (15)$$

The alternative hypothesis is

$$H_A : (Cap_{ig}^{final} - Cap_{ig}^{initial}) < 0. \quad (16)$$

5.3.3 External information versus direct exposure

Within- and between-variation in the FM-I and FM-E groups. We test whether direct exposure and external information have different effects on beliefs about trade and regime choices.

We first run non-parametric tests to assess differences in the change in beliefs about the quantity traded between the FM-I and FM-E treatment arms. We test the following null hypothesis:

$$H_0 : (PC_{ig}^{post} - PC_{ig}^{prior})_{FM-E} - (PC_{ig}^{post} - PC_{ig}^{prior})_{FM-I} = 0. \quad (17)$$

We then run non-parametric tests to assess differences in the change in regime choices between the FM-I and FM-E treatment arms. We test the following null hypothesis:

$$H_0 : (Cap_{ig}^{final} - Cap_{ig}^{initial})_{FM-E} - (Cap_{ig}^{final} - Cap_{ig}^{initial})_{FM-I} = 0. \quad (18)$$

5.4 Decisive versus hypothetical choosers

We assess whether being pivotal affects beliefs about trade and regime choices.

5.4.1 Effect of pivotality on beliefs about the quantity traded

Priors: Between-variation in the FM-E and PC-E groups. We run non-parametric tests for the difference in prior beliefs between decisive and hypothetical choosers in both the FM-E and PC-E groups exploiting a between-subjects design. We test the following null hypothesis for the FM-E treatment arm:

$$H_0 : (PC_{ig}^{prior})_{D=1} - (PC_{ig}^{prior})_{D=0} = 0, \quad (19)$$

where D is a dummy variable that equals 1 if subject i is a decisive chooser and 0 otherwise.

We also test the following null hypothesis for the PC-E treatment arm:

$$H_0 : (FM_{ig}^{prior})_{D=1} - (FM_{ig}^{prior})_{D=0} = 0. \quad (20)$$

Belief updating: Within- and between-variation in the FM-E group. We run non-parametric tests for the difference in belief updating between decisive and hypothetical choosers in the FM-E group, exploiting both a within- and between-subjects design. We test the following null hypothesis:

$$H_0 : (PC_{ig}^{post} - PC_{ig}^{prior})_{D=1} - (PC_{ig}^{post} - PC_{ig}^{prior})_{D=0} = 0. \quad (21)$$

By contrast, the model of *rational irrationality* (Caplan, 2000, 2001, 2007) predicts

$$H_A : (PC_{ig}^{post} - PC_{ig}^{prior})_{D=1} - (PC_{ig}^{post} - PC_{ig}^{prior})_{D=0} < 0. \quad (22)$$

5.4.2 Effect of pivotality on regime choices

Initial choices: Between-variation in the FM-E and PC-E groups. We run non-parametric tests for the difference in initial regime choices between decisive and hypothetical choosers in both the FM-E and PC-E groups, exploiting a between-subjects design.

For each of the FM-E and PC-E groups, we separately test the following null hypothesis:

$$H_0 : (Cap_{ig}^{initial})_{D=1} - (Cap_{ig}^{initial})_{D=0} = 0. \quad (23)$$

Revision of choices: Within- and between-variation in the FM-E group. We run non-parametric tests for the difference in choice revisions between decisive and hypothetical choosers in the FM-E group, exploiting both a within- and between-subjects design. We test the following null hypothesis:

$$H_0 : (Cap_{ig}^{final} - Cap_{ig}^{initial})_{D=1} - (Cap_{ig}^{final} - Cap_{ig}^{initial})_{D=0} = 0. \quad (24)$$

By contrast, both the model of *rational irrationality* (Caplan, 2000, 2001, 2007) and the model

of *expressive voting* (Brennan and Lomasky, 1993; Brennan and Hamlin, 1988; Schuessler, 2000; Brennan, 2008; Hamlin and Jennings, 2011) predict

$$H_A : (Cap_{ig}^{final} - Cap_{ig}^{initial})_{D=1} - (Cap_{ig}^{final} - Cap_{ig}^{initial})_{D=0} < 0. \quad (25)$$

Additionally, we run the following regression for the FM-E sample:

$$Cap_{ig}^{final} = \beta_0 + \beta_1 Cap_{ig}^{initial} + \beta_2 D_i + \beta_3 (Cap_{ig}^{initial} \times D_i) + \epsilon_{ig}^{final}. \quad (26)$$

5.5 Association between choices made in DRETS and regime choices

For subjects who chose both the low-risk lottery and the low-inequality allocation in the DRETS (denoted by LL), we test the following null hypothesis:

$$H_0 : Pr(Cap_{igj}^t = 1 | DRETS_{igj} = LL) < 0.5, \quad (27)$$

for $j \in \{FM - E, FM - I, PC - E\}$ and $t \in \{initial, final\}$.

For subjects who chose both the high-risk lottery and the high-inequality allocation in the DRETS (denoted by HH), we test the following null hypothesis:

$$H_0 : Pr(Cap_{igj}^t = 1 | DRETS_{igj} = HH) > 0.5, \quad (28)$$

for $j \in \{FM - E, FM - I, PC - E\}$ and $t \in \{initial, final\}$.

6 Pre-experiment survey

6.1 Questionnaire

Before the experiment, subjects will answer a set of questions about socioeconomic characteristics and related personal information. These include gender, age, place of birth, university, field of study, socioeconomic stratum, and ethnic origin.

Subjects will answer a set of questions to measure their location in the ideological spectrum (left-right). (See Section I of the survey in the Appendix.) Capturing ideology using surveys is challenging when it comes to the left-right divide, yet some studies have shown the empirical relevance of using this approach in Latin America (see, for instance, Wiesehomeier, (2012)). Therefore, we proceed with a set of questions to capture political ideology asking subjects to place them-

selves in the left-right divide using continuous scores (0-10). We also ask a set of related questions to approximate ideology: Subjects' ranking of political parties in the left-right divide, candidate choices in the first and second round of the 2022 presidential election in Colombia, and support for presidential candidates.

Subjects will also answer a set of questions to elicit their attitudes toward government intervention. (See Section II of the survey in the Appendix.) These refer to government ownership of public enterprises, policies to combat poverty and inequality, taxation of the rich, price controls for medicines and food, minimum wage, and import and interest rate controls.

6.2 Indexes

This information will be used to create two indexes, one capturing political ideology and another capturing support of government intervention in the economy. There are two alternatives to compute these indexes. First, we can use the **Mean Effect Index** suggested by Kling et al (2004). The algorithm is the following:

- If necessary, reorient some of the K outcomes so that beneficial effects are consistently scored higher. As a general rule, we will orient the outcomes in a way that higher scores are related with more pro-left views.
- For each outcome, calculate individual z -scores $\bar{y}_{ik}$ by subtracting off the control mean $\bar{y}_k^{Z=0}$ and dividing by the control standard deviation $\sigma_k^{y,Z=0}$:

$$\bar{y}_{ik} = \frac{y_{ik} - \bar{y}_k^{Z=0}}{\sigma_k^{y,Z=0}}. \quad (29)$$

- For each individual, sum the z -scores over outcomes, $\sum_{k=1}^K \bar{y}_{ik}$, and divide by K .

Alternatively, we can use the **Inverse Covariance Weighted Index** proposed by Anderson (2008). The algorithm is the following:

- If necessary, reorient some of the K outcomes so that beneficial effects are consistently scored higher.

- For each outcome, calculate individual z -scores $\bar{y}_{ik}$ by subtracting off the control mean $\bar{y}_k^{Z=0}$ and dividing by the control standard deviation $\sigma_k^{y,Z=0}$:

$$\bar{y}_{ik} = \frac{y_{ik} - \bar{y}_k^{Z=0}}{\sigma_k^{y,Z=0}}. \quad (30)$$

- For each individual, sum the z -scores across outcomes, $\sum_{k=1}^K \bar{y}_{ik}$, and divide by K .
- Construct and invert the (variance-)covariance matrix of the resultant matrix of z -scores $\hat{\Sigma}^{-1}$.
- The weighted indexed outcome $\bar{s}_i$ can be computed as:

$$\bar{s}_i = (\mathbf{1}^T \hat{\Sigma}^{-1} \mathbf{1})^{-1} (\mathbf{1}^T \hat{\Sigma}^{-1} \mathbf{y}_{ik}). \quad (31)$$

Following the experimental literature, we prefer Anderson's (2008) index.

7 Inference issues

7.1 Adjusting for multiple comparisons

Because many hypotheses will be tested in this study, we will implement several techniques to adjust for multiple comparisons. We will control for the false discovery rate using the approach suggested by Benjamini and Hochberg (1995).

7.2 Power

Because our design includes elements of between and within-subjects designs, power calculations will account for both designs. Because within-subjects designs are more efficient, sample size calculations for the between-subjects design will cover the within-subjects case. We will make some conservative choices for the main parameters in this exercise.

A critical parameter in power calculations is the effect size (Cohen, 1988). Because we do not have a previous benchmark for the effect size in the academic literature, we assume a small effect size of 0.2 standard deviations in the main outcome following Cohen's suggestion. We also assume 80% power and a one-sided test for difference in means with a 5% significance level. To account

for clustering, we assume a cluster size of 4 and an intra-cluster correlation of 0.5. We adjust for 3 comparisons using the Bonferroni approach. Under these assumptions, the minimum sample size is 2,208 subjects for a two-sample t-test for clustered data. For the case of discrete outcomes, we assume an effect size of 0.10 percentage change and keep all the other parameters at the same values. The minimum sample size is 2,176 subjects for a Pearson's Chi-squared test for clustered data.

These estimated sample sizes are larger than the usual standards in the experimental literature, so we will relax the above assumptions below. We will also account for covariates, the availability of information for several trading periods, and the inclusion of the baseline outcome in an ANCOVA design. These adjustments will substantially reduce the sample size requirements for a between-subjects design.

Using a within-subjects design will also contribute to achieving power with a lower sample size. For instance, for a t-test for paired means with the same assumptions as above, the minimum sample size is 240 subjects without accounting for clustering. Assuming a design effect of 2.5, then the estimated sample size is 600 subjects. If we use a McNemar test instead for discrete outcomes, the estimated sample size is similar. In both cases, we assume an autocorrelation of 0.5.

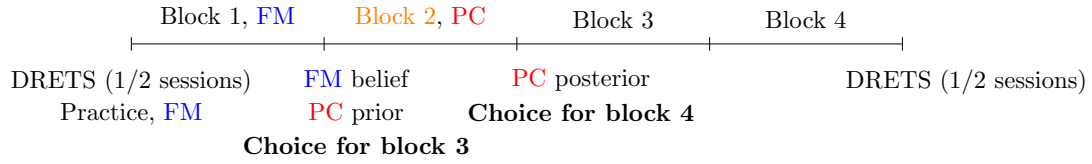
Performing exact power calculations for non-parametric tests is not feasible because we do not know the shape of the underlying distribution. We follow a rule of thumb proposed by Lehmann, (2006), who suggests adding 15% more subjects to the sample size estimated under a parametric approach. Hence, the estimated sample size is 690 subjects. This sample size is similar to the one used by Dal Bo et al (2018), the closest antecedent in the empirical literature. In their experiment, they used 768 subjects. Therefore, our power calculations suggest that a sample close to 700 subjects will be enough for this study.

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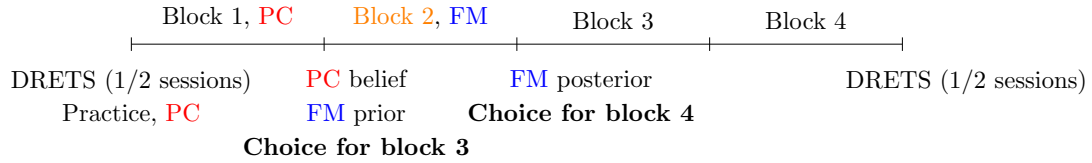
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Figure 1: Timeline of an experimental session by treatment arm

Free Market First-Experience (FM-E)



Price Cap First-Experience (PC-E)



Free Market First-Information (FM-I)

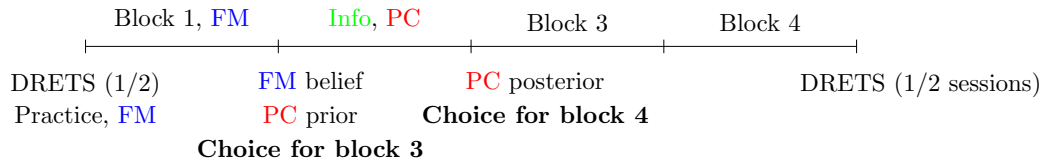


Table 1: Market Task

Buyer	Value	Free market		Price cap	
		CE units	Surplus	CE units	Surplus
b1	12	1	3	1	5
b2	12	1	3	1	5
b3	11	1	2	0	0
b4	11	<u>1</u>	<u>2</u>	<u>0</u>	<u>0</u>
Total		4	10	2	10
Seller	Cost	CE units	Surplus	CE units	Surplus
s1	2	1	7	1	5
s2	2	1	7	1	5
s3	8	1	1	0	0
s4	9	<u>1</u>	<u>0</u>	<u>0</u>	<u>0</u>
Total		4	15	2	10
CE price		9		7	
CE quantity		4		2	

Note: CE stands for “competitive equilibrium.” Values, costs, market prices, and surpluses are in ECU.

Table 2: Distribution and Risk Equivalent Tasks (DRETS)

Panel A: Lottery choice task				
	<u>Low-risk lottery</u>		<u>High-risk lottery</u>	
Outcome	3	2	5	0
Probability	0.5	0.5	0.5	0.5
Expected value	2		2	
Variance	250		6,250	
Panel B: Distribution task				
<u>Subject</u>	<u>Low-inequality allocation</u>		<u>High-inequality allocation</u>	
1	3		5	
2	3		5	
3	2		0	
4	2		0	
Sum of payoffs	10		10	

Note: All payoffs are in ECU.

APPENDIX: Pre-experiment Survey

Section I

1. What is your gender?
Male — Female — Other
2. How old are you?
(Record your age in fulfilled years.)
3. Where were you born?
(Enter the city and country.)
4. What university do you study at?
5. What degree(s) do you study at university?
6. What semester(s) of your degree(s) are you currently in?
7. Have you taken economics courses?
Yes — No
8. What is your stratum?
(1 is the poorest stratum, 6 the richest stratum)
1 — 2 — 3 — 4 — 5 — 6
9. Do you consider yourself a white, half-caste, indigenous, black, mulatto or other person?
White — Half-caste — Indigenous — Black — Mulatto — Other
10. Consider a scale of 1 to 10 that goes from left to right, in which 1 means “left” and 10 means “right.” Nowadays, when talking about political tendencies, many people talk about those who are more sympathetic to the left or the right.

According to the meaning that the terms “left” and “right” have to you, when you think about your political point of view, where are you on this scale?
Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right
11. Where would you place the Historical Pact?
Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right
12. Where would you place the Democratic Center?
Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right
13. Thinking of political parties or coalitions in Colombia, how close politically do you feel to:
Liga de Gobernantes Anticorrupción
Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Colombia Justa Libres

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Coalición Centro Esperanza

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Movimiento de Salvación Nacional

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Pacto Histórico

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

G.S.C. Colombia Piensa en Grande

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

Verde Oxígeno

Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

14. In the first round of the last presidential election you voted for:

- Liga de Gobernantes Anticorrupción
- Colombia Justa Libres
- Coalición Centro Esperanza
- Movimiento de Salvación Nacional
- Pacto Histórico
- G.S.C. Colombia Piensa en Grande
- Verde Oxígeno
- Did not vote

15. Regardless of whether you voted in the first round of the last presidential election, which candidate did you support?

- Rodolfo Hernández (Liga de Gobernantes Anticorrupción)
- John Milton Rodríguez (Colombia Justa Libres)
- Federico Gutiérrez (Equipo por Colombia)
- Enrique Gómez Martínez (Movimiento de Salvación Nacional)
- Gustavo Petro (Pacto Histórico)
- Luis Pérez (G.S.C. Colombia Piensa en Grande)
- Ingrid Betancourt (Verde Oxígeno)
- Sergio Fajardo (Coalición Centro Esperanza)
- None of the above

16. In the second round of the last presidential election you voted for:

- Liga de Gobernantes Anticorrupción
- Pacto Histórico
- Did not vote

17. Regardless of whether you voted in the second round of the last presidential election, which candidate did you support?
 - Liga de Gobernantes Anticorrupción
 - Pacto Histórico
 - None of the above
18. Which political party would you never vote for?
 - Liga de Gobernantes Anticorrupción
 - Colombia Justa Libres
 - Coalición Centro Esperanza
 - Movimiento de Salvación Nacional
 - Pacto Histórico
 - G.S.C. Colombia Piensa en Grande
 - Verde Oxígeno

Section II

To what extent do you agree or disagree with the following sentences? 1 represents “strongly disagree” and 10 represents “strongly agree.”

19. Regardless of which political party is in charge, the Colombian State, instead of the private sector, should be the owner of the country’s most important companies and industries.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
20. Regardless of which political party is in charge, the Colombian State must implement firm policies to reduce income inequality between rich and poor.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
21. Regardless of which political party is in charge, the Colombian State must spend more on helping the poor.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
22. It is unfair that the rich pay a lot in taxes but receive little in state services.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
23. Regardless of which political party is in charge, the Colombian State must regulate the prices of essential goods such as food and medicines.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
24. Regardless of which political party is in charge, the Colombian State must guarantee a minimum wage to cover the basic family basket.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

25. Regardless of which political party is in charge, the Colombian State must regulate the interest rate to ensure that credit is cheap.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
26. Regardless of which political party is in charge, the Colombian State must regulate imports to protect Colombian producers and consumers.
Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree
27. Some people consider that the State tries to do many things that should be left to individuals and businesses. Others believe that the State should do more. What stance comes closest to your position?
The State does too much — The State does the right thing — The State should do more.

Part II. PAP implementation, clarifications, and deviations

1. Overview and scope of this implementation report

This implementation report documents how the analyses conducted for the final manuscript relate to the original pre-analysis plan (PAP). Part I reproduces the original PAP. Part II provides implementation notes, clarifications, and discussion of deviations or changes in emphasis relative to the PAP. Part III reports the tables supporting these implementation notes.

The original PAP proposed a laboratory experiment to study citizens' support for price caps. The planned analyses focused on five related questions: whether participants initially failed to anticipate the shortage generated by a price cap; whether direct exposure to the price-cap regime changed beliefs and reduced support for the policy; whether factual information had similar effects in the absence of direct experience; whether decision-making responsibility affected beliefs and regime choices; and whether attitudes toward government intervention, as well as risk and distributional preferences elicited through the DRETS tasks, helped explain support for the price cap.

The experimental design was implemented in close accordance with the PAP. In particular, the treatment structure, the timing of belief elicitation, the elicitation of initial and final regime choices, the manipulation of decision-making roles, and the grouping of participants based on pre-experimental measures of ideology and support for government intervention followed the original plan. The main purpose of this report is therefore not to document changes in the experimental design, but rather to clarify how the PAP-specified analyses map into the final analysis-ready dataset and how the final manuscript relates to those analyses.

The final manuscript develops a more explicit theoretical interpretation than the PAP, centered on motivated beliefs and decision-making agency. This theoretical framework sharpened the interpretation of some empirical patterns, especially the analysis of agency effects conditional on participants' prior beliefs. For this reason, this report distinguishes between three types of analyses. First, it reports the PAP-specified primary tests, which provide the closest implementation of the original analysis plan. Second, it reports PAP-specified regressions and complementary robustness checks. Third, it identifies theory-guided extensions and auxiliary analyses that are useful for interpreting the final manuscript but were not the primary tests specified in the PAP.

Unless otherwise indicated, the full-sample PAP-specified tests are treated as the main inferential reference in this report. Restricted-sample results are reported for comparability with the mechanism-focused analyses in the manuscript and should be interpreted as robustness checks. Balance checks across information-treatment arms and agency status are not duplicated here because they are reported in the manuscript’s Online Appendix (Tables A2 and A3). Similarly, the manuscript reports a regression showing that assignment to the experimental treatments does not predict inclusion in the restricted sample (Table A4). This report therefore focuses on PAP implementation, interpretation of PAP-aligned results, and transparent discussion of deviations or changes in emphasis.

The tables in Part III follow the structure of the PAP. Tables B1 and B2 summarize the implementation crosswalk and the mapping between PAP concepts and final-data variables. Tables B3–B10 report the PAP-aligned analyses of pre-exposure beliefs, direct exposure, order effects, factual information, and the comparison between direct exposure and factual information. Tables B11–B12 report PAP-aligned average agency comparisons. Table B13 reports the DRETS analyses. Tables B14–B25 report Benjamini-Hochberg adjustments for the main PAP-specified tests and their restricted-sample counterparts. Table B26 reports PAP-specified regression and LPM specifications. Tables B27–B29 report auxiliary survey-index analyses and precision diagnostics for selected heterogeneity analyses.

Overall, this document should be read as a transparency supplement. It shows that the core experimental design and the main empirical comparisons proposed in the PAP were implemented, while also clarifying how the final manuscript refined the analysis and interpretation after the theoretical framework was developed and the data were analyzed.

2. Summary of alignment with the PAP

The implementation of the experiment is closely aligned with the original PAP. The core elements of the design were preserved: participants were assigned to the planned information-treatment arms, completed the sequence of market blocks described in the PAP, reported beliefs before and after the relevant information or experience, and made both initial and final regime choices. The agency manipulation also followed the PAP: within each group, one participant was assigned to make a decisive regime choice, while the remaining participants made hypothetical

choices. In addition, participants were grouped using pre-experimental measures of political ideology and attitudes toward government intervention, as specified in the original design.

The strongest alignment between the PAP and the final analysis concerns the reduced-form tests of information and experience. The PAP proposed testing whether direct exposure to the price-cap regime changed beliefs about market outcomes and reduced support for the price cap. The implementation report reproduces these analyses using the FME arm, where participants first experienced the free-market regime and then the price-cap regime. The corresponding PCE / Reverse Experience arm is also reported, allowing us to assess whether exposure to both regimes produces similar updating when the order of regimes is reversed. These analyses are reported in Tables B5–B7.

The report also closely implements the PAP’s tests of factual information. In the FMI arm, participants did not directly experience the price-cap regime before updating their beliefs and choices; instead, they received information about the outcomes generated by the price cap. Tables B8 and B9 report the corresponding belief-updating and regime-choice tests. Table B10 then implements the PAP’s direct comparison between personal experience and factual information by comparing changes in beliefs and regime choices across the FME and FMI arms. This comparison is one of the main areas in which the PAP and the final manuscript are most closely aligned.

The PAP-specified pre-exposure belief analyses are also implemented, with some clarifications required by the treatment-specific block mapping. Tables B3 and B4 report the within-arm and between-arm comparisons proposed in PAP Section 5.1. The between-arm comparison in Table B4 uses the correct PAP mapping: the price-cap prior in FME is compared with the free-market prior in PCE. This comparison does not require a price-cap prior in the PCE arm, because participants in that arm had already experienced the price-cap regime in block 1.

The agency analyses are implemented in two layers. First, Tables B11 and B12 report the PAP-specified average comparisons between decisive and hypothetical choosers. These tables provide the closest implementation of PAP Section 5.4. Second, the final manuscript goes beyond these average comparisons by studying how agency effects depend on participants’ prior beliefs about the price-cap regime. This prior-belief heterogeneity analysis is not a replacement for the PAP-specified tests; rather, it is a theory-guided extension motivated by the final model of motivated beliefs and decision-making responsibility.

The DRETS analyses proposed in the PAP are also reported. Table B13 implements the PAP-specified tests relating LL and HH DRETS types to initial and final regime choices. These analyses are included for transparency, but they are not emphasized in the main manuscript because some of the relevant cells, especially the HH type, are small. The resulting estimates are therefore imprecise and do not provide a robust basis for substantive claims about heterogeneity by risk and distributional preferences.

Finally, the implementation report follows the PAP’s concern with multiple hypothesis testing. Tables B14–B25 report Benjamini-Hochberg adjusted p-values for the main PAP-specified primary tests, separately by substantive family and by sample. The main adjustment uses full-sample PAP-specified tests. Restricted-sample adjustments are reported separately as robustness checks, in line with the manuscript’s use of the restricted sample for mechanism-focused analyses.

In sum, the experimental design and the main reduced-form empirical comparisons proposed in the PAP were implemented. The main differences between the PAP and the final manuscript concern emphasis and interpretation rather than the underlying design. The manuscript develops a more explicit theoretical framework centered on motivated beliefs and agency, uses some regression-based specifications to organize the evidence, and gives less prominence to underpowered secondary analyses such as DRETS-based heterogeneity. The implementation report documents these differences while showing that the core PAP-aligned analyses support the main empirical conclusions of the paper.

3. Implementation clarifications

This section summarizes the main implementation choices needed to interpret the PAP-aligned analyses reported below. These clarifications concern the mapping between the original PAP concepts and the final analysis-ready dataset, the role of the PCE / Reverse Experience arm, the distinction between full-sample and restricted-sample results, the treatment of PAP-specified regressions, and the construction of multiple-testing adjustments.

First, some belief variables have different interpretations across treatment arms because the regimes were experienced in different orders. In the FME and FMI arms, block 1 corresponds to the free-market regime, while block 2 corresponds to either direct exposure to the price-cap regime or factual information about the price-cap regime. Therefore, in these arms, the block 1 belief

is interpreted as a free-market belief, the block 2 prior belief is interpreted as a price-cap prior, and the block 2 posterior belief is interpreted as a price-cap posterior. By contrast, in the PCE / Reverse Experience arm, participants first experienced the price-cap regime and then the free-market regime. As a result, the block 2 prior belief in PCE is interpreted as a free-market prior, and the block 2 posterior belief is interpreted as a free-market posterior. Table B2 reports this mapping explicitly.

Second, the PCE / Reverse Experience arm does not contain a true price-cap prior in the same sense as the FME and FMI arms, because participants in PCE had already experienced the price-cap regime before reporting the belief used after block 1. This does not prevent implementation of PAP-5.1B / Eq. (2), because that comparison does not require a PCE price-cap prior. The PAP-specified between-arm comparison uses the price-cap prior in FME and the free-market prior in PCE. Accordingly, Table B4 implements this comparison using the block 2 prior belief in both arms, interpreted according to the treatment-specific mapping.

Third, the PAP-aligned analyses use the full sample as the primary reference. Restricted-sample results are reported only for comparability with the mechanism-focused analyses in the manuscript. The restricted sample is based on internal-consistency criteria for beliefs and choices and is useful for reducing noise in mechanism-oriented specifications. However, because this restriction was not the primary sample definition in the PAP, the full-sample results reported in this implementation report should be treated as the main PAP-aligned evidence. Balance checks across treatment arms and decision-making roles, as well as regressions assessing whether treatment assignment predicts inclusion in the restricted sample, are reported in the manuscript's Online Appendix and are not duplicated here.

Fourth, this report distinguishes between PAP-specified primary tests and PAP-specified regressions. The PAP emphasized non-parametric and exact tests for the main within- and between-subject comparisons, while also proposing several regression and LPM specifications. In this implementation report, the non-parametric and exact tests are used as the primary inferential reference for the PAP-aligned analysis. The PAP-specified regressions and LPMs are reported separately in Table B26. Complementary pooled specifications that resemble the manuscript's empirical presentation are included only as auxiliary analyses and are not treated as primary PAP tests.

Fifth, p-values are reported in a conservative manner. Unless otherwise indicated, Wilcoxon

signed-rank tests, Wilcoxon rank-sum tests, and McNemar/binomial tests are reported as two-sided, even when the PAP stated directional predictions. This choice makes the implementation conservative relative to the one-sided alternatives implied by several PAP hypotheses. The main exception is the DRETS analysis, where the PAP explicitly states directional predictions for LL and HH types; for these tests, Table B13 reports one-sided binomial p-values.

Sixth, the Benjamini-Hochberg adjustments are applied by substantive PAP family. The main multiple-testing adjustment uses full-sample PAP-specified primary tests only. PAP-specified regressions, manuscript-style pooled complements, auxiliary survey-index regressions, and precision diagnostics are excluded from the main Benjamini-Hochberg adjustment because they are not the primary non-parametric or exact tests specified in the PAP. Restricted-sample Benjamini-Hochberg adjustments are reported separately as robustness checks.

Seventh, the survey-index analyses should be interpreted as auxiliary. The PAP proposed constructing indexes of political ideology and support for government intervention and using them both for group formation and for studying heterogeneity in support for price caps. The final analysis-ready dataset already contains the relevant ideology and intervention indexes. This implementation report documents these variables and reports auxiliary regressions relating them to baseline support and belief updating. These regressions are useful for transparency, but they are not part of the main PAP-specified primary-test families.

Finally, the precision diagnostics reported in Table B29 are included to document the limitations of selected heterogeneity analyses. They should not be interpreted as observed post-hoc power calculations. Their purpose is narrower: to show the sample sizes, cell sizes, and approximate detectable differences associated with analyses that rely on small subgroups, especially the HH DRETS type. These diagnostics help explain why some preregistered secondary analyses are reported here for transparency but are not emphasized in the main manuscript.

4. PAP Section 5.1: Pre-exposure beliefs and initial support

PAP Section 5.1 proposed testing whether participants anticipated the shortage generated by the price-cap regime before receiving direct experience with, or factual information about, the price cap. The central idea was that if participants understood the market consequences of a binding price cap, their prior beliefs should imply a lower probability of buying under the price-cap regime

than under the free-market regime. Conversely, if participants failed to anticipate the shortage, prior beliefs about the price-cap regime would be similar to, or even more optimistic than, beliefs about the free-market regime.

Table B3 implements the within-subject comparisons proposed in the PAP for the FME and FMI arms. In both arms, participants first experienced the free-market regime and reported beliefs about that regime. They then reported prior beliefs about the price-cap regime before receiving either direct exposure to the price cap in FME or factual information about the price cap in FMI. The table therefore compares each participant's free-market belief with their own price-cap prior. The reported difference is the price-cap prior minus the free-market belief.

The full-sample results provide mixed evidence on whether participants anticipated the shortage generated by the price cap. In the FME arm, the mean free-market belief is 0.693 and the mean price-cap prior is 0.686, implying a very small difference of -0.007 that is not statistically significant. In the FMI arm, the mean free-market belief is 0.707 and the mean price-cap prior is 0.726, implying a small positive difference of 0.018. This pattern suggests that, in the full sample, participants did not clearly anticipate that the price cap would reduce the probability of buying before receiving information or experience. If anything, the FMI arm displays slightly more optimistic prior beliefs about the price-cap regime.

The restricted-sample results are somewhat different. In the FME arm, restricted-sample participants report a lower price-cap prior than free-market belief, with a difference of -0.051. This suggests that participants whose beliefs and choices satisfy the manuscript's internal-consistency criteria were more likely to anticipate the shortage generated by the price cap. In the FMI arm, the restricted-sample difference is smaller, -0.015, and not statistically significant. Because the PAP did not define the restricted sample as the primary sample, we interpret these results as robustness checks rather than as the main PAP-aligned evidence.

Table B4 implements the between-arm prior comparison proposed in PAP-5.1B / Eq. (2). This comparison uses the block 2 prior belief in both FME and PCE, but the economic interpretation of this variable differs across arms. In FME, the block 2 prior is the prior belief about the price-cap regime before price-cap exposure. In PCE, the block 2 prior is the prior belief about the free-market regime before free-market exposure. This is the correct implementation of the PAP comparison and does not require a price-cap prior in the PCE arm.

The between-arm comparison shows that FME participants report substantially higher block 2 priors than PCE participants. In the full sample, the mean is 0.686 in FME and 0.458 in PCE, with a difference of 0.228. In the restricted sample, the corresponding difference is 0.251. These differences are statistically significant. Substantively, this comparison indicates that participants assigned to FME expected a higher probability of buying under the price-cap regime than participants assigned to PCE expected under the free-market regime before experiencing that second regime. This result goes in the opposite direction of the sign predicted by the narrow PAP test in Eq. (2). Rather than indicating that participants correctly anticipated lower purchases under the price cap, it suggests substantial optimism about the price-cap regime before corrective information or experience. Instead, it reinforces the broader interpretation that prior beliefs before corrective information or experience were substantially distorted.

Overall, the PAP Section 5.1 analyses are implemented closely, with one important clarification: the between-arm test requires treatment-specific interpretation of the block 2 prior belief. The evidence from this block is consistent with the PAP's motivation that participants may fail to anticipate the negative consequences of a price cap before receiving direct experience or factual information. The full-sample within-subject tests show little evidence that participants clearly expected the price cap to reduce the probability of buying, while the between-arm comparison points to substantial optimism about the price-cap regime relative to the corresponding free-market prior in the reverse-order arm. These findings provide the baseline motivation for the subsequent PAP tests of direct exposure and factual information.

5. PAP Section 5.2: Effects of direct exposure

PAP Section 5.2 proposed testing whether direct exposure to both market regimes changes participants' beliefs about market outcomes and reduces support for the price-cap regime. The PAP specified two related tests. First, it proposed comparing prior and posterior beliefs within participants after direct exposure to the relevant regime. Second, it proposed comparing initial and final regime choices to assess whether exposure changes support for the price cap.

Table B5 reports the PAP-aligned belief-updating tests. In the FME arm, participants first experienced the free-market regime and then directly experienced the price-cap regime. The relevant comparison is therefore between their prior belief about the price-cap regime and their posterior

belief after experiencing the price cap. In the full sample, the mean price-cap belief falls from 0.686 to 0.521, a reduction of 0.164. In the restricted sample, the corresponding reduction is 0.206. Both changes are statistically significant. These results closely match the original PAP prediction that direct exposure to the price cap would lead participants to revise their beliefs downward.

The PCE / Reverse Experience arm provides the analogous test in the opposite order. Participants first experienced the price-cap regime and then directly experienced the free-market regime. The relevant comparison is therefore between their prior and posterior beliefs about the free-market regime. In the full sample, the mean free-market belief increases from 0.458 to 0.725, a change of 0.268. In the restricted sample, the increase is 0.312. These results indicate that direct exposure to the free-market regime also produces substantial belief updating when participants initially encounter the price-cap regime. Thus, the belief-updating evidence from both FME and PCE is consistent with the PAP's broader premise that direct exposure to market outcomes corrects distorted expectations.

Table B6 reports the corresponding effects of direct exposure on regime choices. In the FME arm, the share of participants choosing the price cap falls from 0.648 in the initial choice to 0.398 in the final choice, a reduction of 0.250. The switching pattern is also informative: 35.6 percent of participants switch from the price cap to the free market, while only 10.6 percent switch from the free market to the price cap. In the restricted sample, the decline in support is even larger, from 0.760 to 0.409, with a reduction of 0.351. These patterns are strongly consistent with the PAP prediction that direct exposure to the price-cap regime would reduce support for the price cap.

The PCE / Reverse Experience arm shows a similar reduction in price-cap support after participants have experienced both regimes. In the full sample, support for the price cap falls from 0.742 to 0.500, a reduction of 0.242. In the restricted sample, support falls from 0.871 to 0.502, a reduction of 0.368. These results suggest that the reduction in support is not unique to the FME sequence. Participants who first experienced the price cap and then the free market also became substantially less likely to choose the price-cap regime once they had encountered both regimes.

Overall, the PAP Section 5.2 analyses are among the closest implementations of the original analysis plan. The treatment arms, belief measures, timing of elicitation, and regime-choice comparisons correspond directly to the PAP. The results strongly support the PAP's prediction that direct exposure changes beliefs and reduces support for price caps. They also provide an important

bridge to the final manuscript: the manuscript’s emphasis on personal experience as a powerful source of belief revision is grounded in these preregistered within-subject comparisons.

6. PAP Section 5.2.3: Order effects and the Reverse Experience arm

PAP Section 5.2.3 proposed testing for order effects by comparing final outcomes in the FME and PCE arms. The purpose of this test was to assess whether participants who experienced the free-market regime first and the price-cap regime second reached similar final beliefs and regime choices as participants who experienced the same two regimes in the opposite order. In the final manuscript, the PCE arm is referred to as the Reverse Experience arm and is used primarily to evaluate this concern.

Table B7 reports the PAP-aligned order-effect tests. The table compares final beliefs and final regime choices between the FME and PCE arms after participants in both arms had been exposed to both market regimes. The belief outcomes use the treatment-specific mapping described in Table B2: final free-market beliefs correspond to block 1 beliefs in FME and block 2 posterior beliefs in PCE, while final price-cap beliefs correspond to block 2 posterior beliefs in FME and block 1 beliefs in PCE.

The belief results suggest substantial convergence across the two experience arms. In the full sample, the final free-market belief is 0.693 in FME and 0.725 in PCE, with a difference of -0.033. The final price-cap belief is 0.521 in FME and 0.505 in PCE, with a difference of 0.017. Neither difference is statistically significant at conventional levels. The restricted-sample results are similar: final free-market beliefs differ by only -0.010 and final price-cap beliefs differ by 0.025. These patterns suggest that, once participants directly experienced both regimes, posterior beliefs about market outcomes were not strongly affected by the order in which the regimes were encountered.

The choice results are somewhat different. In the full sample, final support for the price cap is 0.398 in FME and 0.500 in PCE, yielding a difference of -0.102. This difference is statistically significant in the full sample. In the restricted sample, the corresponding difference is -0.093, with a p-value of 0.073. Thus, while posterior beliefs about the two regimes largely converge across the two experience arms, final regime choices remain somewhat more favorable to the price cap in the PCE / Reverse Experience arm.

These results imply that the order of exposure has limited effects on beliefs but may have some

influence on final policy preferences. This distinction is useful for interpreting the role of the PCE arm in the final manuscript. The PCE arm was implemented as planned, but the manuscript uses it mainly as a robustness check for order effects rather than as a central treatment arm. This represents a change in emphasis rather than a change in implementation. The PAP-aligned results reported here show that the main belief-updating results are not driven by the order in which participants encountered the two regimes, while also documenting that final choices retain some sensitivity to initial experience.

Overall, the order-effect analysis supports the interpretation that direct exposure to both market regimes leads participants to learn similar facts about the free-market and price-cap regimes, regardless of exposure order. At the same time, the evidence on final choices suggests that early exposure may continue to shape preferences over regimes even after beliefs have converged. This nuance is consistent with the final manuscript’s decision to treat the Reverse Experience arm as an order-effect robustness exercise rather than as the main basis for the paper’s theoretical interpretation.

7. PAP Section 5.3: Factual information

PAP Section 5.3 proposed testing whether factual information about the market consequences of the price-cap regime changes participants’ beliefs and reduces support for the price cap. This test is implemented in the FMI arm. Participants in this arm first experienced the free-market regime, reported prior beliefs about a hypothetical price-cap block, made an initial regime choice, and then received factual information about the outcomes generated by the price cap in another group. They then revised their beliefs and made a final regime choice. The PAP-specified comparison is therefore within-subject: posterior beliefs and final choices are compared with prior beliefs and initial choices in the same treatment arm.

Table B8 reports the effect of factual information on beliefs. In the full sample, the mean prior belief about the probability of buying under the price-cap regime is 0.726, while the post-information belief is 0.660. This implies a reduction of 0.066, which is statistically significant. In the restricted sample, the corresponding reduction is 0.070. These results closely follow the PAP-specified test in Section 5.3.1 and support the prediction that factual information about price-cap outcomes leads participants to revise their beliefs in the expected direction.

Table B9 reports the corresponding effect on regime choices. In the full sample, initial support for the price cap in the FMI arm is 0.699, while final support is 0.591. The implied reduction is 0.108. The switching patterns show that 20.5 percent of participants switched from the price cap to the free market, while 9.7 percent switched from the free market to the price cap. In the restricted sample, support falls from 0.800 to 0.617, implying a larger reduction of 0.183. These results are consistent with PAP Section 5.3.2: factual information reduces support for the price-cap regime, although a substantial share of participants continues to support the price cap after receiving the information.

The comparison between belief updating and choice revision is informative. Factual information moves beliefs in the predicted direction, but the magnitude of belief revision is modest relative to the mechanical difference between the free-market and price-cap regimes in the experiment. Similarly, support for the price cap declines after information is provided, but remains above one-half in the full sample. This pattern is consistent with the final manuscript’s interpretation that factual information reduces misinformation but does not fully eliminate support for the price-cap regime.

Overall, the PAP Section 5.3 analyses are implemented directly and require no substantive deviation from the original plan. The FMI treatment arm was designed precisely to isolate the effect of factual information in the absence of direct experience, and Tables B8 and B9 report the corresponding PAP-specified within-subject tests. These results support the PAP prediction that information changes beliefs and choices, while also motivating the next PAP comparison: whether factual information is less effective than direct personal experience with the price cap.

8. PAP Section 5.3.3: Direct exposure versus factual information

PAP Section 5.3.3 proposed comparing the effects of direct exposure and factual information on beliefs and regime choices. This comparison uses the FME and FMI arms. In both arms, participants first experienced the free-market regime, reported prior beliefs about the price-cap regime, and made an initial regime choice. The difference between the two arms is how participants learned about the price-cap regime before revising their beliefs and making their final choice. In FME, participants directly experienced the price-cap regime. In FMI, participants received factual information about the outcomes generated by the price cap but did not directly experience the regime before updating their beliefs and choices.

Table B10 implements this comparison by comparing belief changes and choice changes across the FME and FMI arms. The belief-change outcome is the posterior belief minus the prior belief about the price-cap regime. The choice-change outcome is the final price-cap choice minus the initial price-cap choice. Negative values therefore indicate downward revision in beliefs about the price cap and lower support for the price-cap regime.

The results show that direct exposure produced substantially larger belief revision than factual information. In the full sample, the mean belief change is -0.164 in FME and -0.066 in FMI, implying a difference of -0.098. In the restricted sample, the corresponding difference is -0.135. Both differences are statistically significant. Thus, while both forms of learning reduced optimistic beliefs about the price cap, direct personal exposure produced a larger correction than factual information alone.

The same pattern appears for regime choices. In the full sample, price-cap support falls by 0.250 in FME and by 0.108 in FMI, implying a difference of -0.142. In the restricted sample, support falls by 0.351 in FME and by 0.183 in FMI, with a difference of -0.168. These results indicate that direct exposure was more effective than factual information in reducing support for the price-cap regime.

This analysis is closely aligned with the original PAP and is also central to the final manuscript. The PAP specified this comparison to assess whether direct experience and external information have different effects on beliefs and choices. The final manuscript gives this comparison a sharper theoretical interpretation by linking it to the idea that personal experience may discipline beliefs more strongly than abstract information. The empirical comparison itself, however, is the same one proposed in the PAP.

Overall, Table B10 provides strong PAP-aligned evidence that both information and experience reduce support for the price cap, but that direct exposure has a larger effect. This result helps explain why the final manuscript emphasizes the distinction between factual information and personal experience as a core empirical contribution.

9. PAP Section 5.4: Agency effects

PAP Section 5.4 proposed testing whether decision-making responsibility affects beliefs about market outcomes and choices over regimes. The underlying idea was that participants whose choices

were decisive for regime implementation should be less willing to sustain support for the price-cap regime after receiving information about its consequences. The PAP therefore focused on average comparisons between decisive and non-decisive participants, separately examining prior beliefs, initial choices, belief updating, and choice revisions.

Tables B11 and B12 report the PAP-specified average agency comparisons. Table B11 focuses on beliefs. In the FME arm, prior beliefs about the price-cap regime are very similar among non-decisive and decisive participants. In the full sample, the mean prior belief is 0.681 for non-decisive participants and 0.699 for decisive participants, with a difference of 0.018. The corresponding restricted-sample difference is also 0.018. Neither difference is statistically significant. Thus, before direct exposure to the price-cap regime, the PAP-aligned tests do not show meaningful differences in price-cap priors by decision-making role in the FME arm.

The evidence on belief updating is also limited in the average PAP tests. In the FME arm, non-decisive participants revise their price-cap beliefs downward by 0.158 in the full sample, while decisive participants revise them downward by 0.182. The difference is therefore -0.023, which is not statistically significant. In the restricted sample, the difference is even smaller. These results suggest that, when estimated as an unconditional average comparison, the agency manipulation does not generate a robust difference in belief updating.

Table B11 also reports the PCE comparison of free-market priors by decision-making role. In the full sample, decisive participants report a higher prior belief about the free-market regime than non-decisive participants, with a difference of 0.055. This difference is statistically significant in the full sample and marginal in the restricted sample. This result is useful descriptively, but it should be interpreted cautiously because the PCE comparison concerns free-market priors after participants have already experienced the price-cap regime, rather than prior beliefs about the price cap itself.

Table B12 reports the corresponding PAP-specified agency tests for regime choices. In the FME arm, initial support for the price cap is not statistically different between decisive and non-decisive participants. In the full sample, the initial price-cap choice share is 0.631 among non-decisive participants and 0.697 among decisive participants, with a difference of 0.066. In the restricted sample, the difference is 0.041. Neither difference is statistically significant. This supports the interpretation that the agency manipulation did not create large baseline differences in support for

the price cap before participants experienced the price-cap regime.

The strongest PAP-aligned agency result appears in choice revision in the FME arm. In the full sample, non-decisive participants reduce support for the price cap by 0.207, while decisive participants reduce support by 0.379. The difference is -0.172 and is statistically significant. This pattern is consistent with the PAP prediction that participants with decision-making responsibility would be more responsive to information about the consequences of the price cap. In the restricted sample, the corresponding difference is -0.122 and is not statistically significant. Thus, the evidence is directionally consistent with the PAP but less robust once the sample is restricted.

The PCE arm also shows a difference in initial choices by agency status. In the full sample, decisive participants are less likely than non-decisive participants to choose the price cap initially, with a difference of -0.136. This difference is statistically significant in the full sample but smaller and not statistically significant in the restricted sample. Because the PCE arm reverses the order of regime exposure, this result should be interpreted as part of the PAP-aligned average agency evidence rather than as the main test of the theoretical mechanism emphasized in the manuscript.

Table B26 reports the PAP-specified regression and LPM specifications. Eq. (26), estimated on the FME sample as specified in the PAP, shows a negative interaction between the initial price-cap choice and decisive status. In the full sample, the coefficient on this interaction is -0.636, and in the restricted sample it is -0.543. Because Eq. (26) is a PAP-specified regression rather than a primary non-parametric test, we interpret it as complementary evidence and do not include its p-values in the main Benjamini-Hochberg adjustment. These estimates reinforce the pattern from the non-parametric choice-revision test: among participants who initially supported the price cap, decisive participants were more likely to revise away from the price-cap regime.

Overall, the PAP Section 5.4 analyses provide partial support for the original agency prediction. The average comparisons show little evidence of baseline differences in FME prior beliefs or initial choices, which is reassuring for the interpretation of the agency manipulation. The clearest PAP-aligned evidence appears in regime-choice revision: decisive participants in FME were more likely to move away from the price cap after direct exposure. The evidence on belief updating is weaker, and some agency patterns are less robust in the restricted sample.

This is the main point at which the final manuscript goes beyond the original PAP. The PAP focused on average decisive-versus-hypothetical comparisons. The final manuscript develops a more

specific theoretical mechanism, according to which agency should matter most among participants whose prior beliefs make motivated updating relevant. For that reason, the manuscript conditions some agency analyses on participants' prior beliefs about the price-cap regime. We interpret those prior-belief heterogeneity analyses as a theory-guided extension of the PAP-specified average tests, not as a replacement for them. The PAP-aligned average results reported here provide the reduced-form benchmark against which the more refined agency analysis in the manuscript should be read.

10. PAP Section 5.5: DRETS and regime choices

PAP Section 5.5 proposed using the Distribution and Risk Equivalent Tasks (DRETS) to assess whether individual risk and distributional preferences help explain regime choices in the Market Task. The DRETS were designed to reproduce, outside the market frame, the individual risk and distributional consequences associated with the free-market and price-cap regimes. The PAP focused on two specific DRETS types. Participants classified as LL chose both the low-risk lottery and the low-inequality allocation. Participants classified as HH chose both the high-risk lottery and the high-inequality allocation. The PAP predicted that LL participants should be less likely to choose the price-cap regime, while HH participants should be more likely to choose it.

Table B13 reports the PAP-specified DRETS tests by treatment arm and timing. The tests compare the share of LL and HH participants choosing the price-cap regime against the benchmark of 0.5, separately for initial and final regime choices. Mixed DRETS types are excluded from the main LL/HH hypothesis tests, as the PAP focused specifically on the two polar types.

The results provide limited and uneven support for the DRETS predictions. Among LL participants, the PAP prediction is that support for the price cap should be below one-half. This prediction is not supported for initial choices: LL participants choose the price cap at high rates in all three treatment arms, with initial support equal to 0.681 in FME, 0.755 in FMI, and 0.790 in PCE. These patterns are opposite to the simple DRETS-based prediction. For final choices, the evidence is more favorable only in the FME arm: the share of LL participants choosing the price cap falls to 0.393 in the full sample and 0.407 in the restricted sample. These shares are significantly below one-half. However, the same pattern does not appear in FMI or PCE, where final support among LL participants remains close to or above one-half.

The HH results are also mixed. The PAP prediction is that HH participants should be more

likely to choose the price-cap regime. In the FME arm, initial support among HH participants is high, equal to 0.789 in the full sample and 0.900 in the restricted sample, which is consistent with the PAP prediction. However, this pattern weakens for final choices in FME and does not replicate clearly in the FMI or PCE arms. In FMI and PCE, the HH cells are small and the observed shares are unstable across samples and timing. The optional pooled panel is useful descriptively, but it is not treated as a primary PAP test and is not included in the main Benjamini-Hochberg adjustment.

The multiple-testing adjustment reinforces this cautious interpretation. In the full-sample DRETS family, the FME LL final-choice test remains significant after Benjamini-Hochberg adjustment, while the FME HH initial-choice test is only marginal after adjustment. The remaining DRETS tests do not survive adjustment. The restricted-sample DRETS results are even less robust after adjustment. Thus, although some individual tests are directionally consistent with the PAP, the overall DRETS evidence is not stable enough to support a strong substantive claim.

Table B29 helps explain why these results should be interpreted cautiously. The LL cells are reasonably large, but the HH cells are very small. For example, the full-sample HH cells contain only 19 participants in FME, 16 in FMI, and 16 in PCE. In the restricted sample, the HH cells fall to 10, 11, and 7 participants, respectively. As a result, the DRETS-based heterogeneity tests have limited precision, especially for HH participants. The precision diagnostics are included to document this limitation; they should not be interpreted as observed post-hoc power calculations.

Overall, the DRETS analyses were implemented as proposed in the PAP, but the results do not provide a robust basis for interpreting regime choices through risk and distributional preferences alone. The evidence is strongest for the decline in final price-cap support among LL participants in FME and for high initial support among HH participants in FME, but these patterns are not consistently replicated across arms, timing, samples, or multiple-testing adjustments. For this reason, the final manuscript does not emphasize DRETS-based heterogeneity. Instead, DRETS measures are used mainly as auxiliary controls, while the main interpretation of regime choices focuses on information, direct experience, motivated beliefs, and decision-making agency.

11. PAP Section 6.2: Survey indexes and intervention attitudes

PAP Section 6.2 proposed constructing two survey-based indexes: one measuring political ideology and another measuring support for government intervention in the economy. These indexes

served two purposes in the original design. First, they were used to form groups of participants with similar pre-experimental political and interventionist attitudes. Second, they were intended to help assess whether a general taste for government intervention is associated with support for price caps or with weaker responsiveness to information about their consequences.

Table B27 documents the index variables available in the final analysis-ready dataset. The table reports both the inverse covariance weighted (ICW) indexes and the mean-effect indexes (MEI) for political ideology and support for government intervention. The ICW indexes are the preferred measures, in line with the PAP’s stated preference for Anderson’s inverse covariance weighted index. The final dataset contains complete observations for all four indexes, with no missing values among the 821 participants.

Table B28 reports auxiliary regressions relating the ICW ideology and intervention indexes to baseline support for the price cap, prior beliefs, and updating. These analyses are included to address the PAP’s question of whether a general taste for government intervention helps explain support for price caps or weakens the effect of information. Because these regressions are not the main non-parametric tests specified in PAP Section 5, we interpret them as auxiliary PAP-6.2 analyses rather than as part of the primary PAP-test families.

The baseline associations in Columns 1 and 2 provide limited evidence that interventionist attitudes are related to initial support for price caps. The coefficient of the intervention index on initial price-cap support is positive but small and not statistically significant. The coefficient on the prior belief that the price cap reduces the probability of buying is positive and marginally significant. This suggests that participants with stronger support for government intervention are, if anything, slightly more likely to recognize that the price cap reduces the probability of buying. Political ideology, as measured by the ICW ideology index, is not strongly associated with either baseline support or prior beliefs in these specifications.

Columns 3 and 4 examine whether interventionist attitudes moderate updating in the FME and FMI arms. The coefficient on the interaction between the Experience treatment and the intervention index is negative for the change in price-cap support and positive for belief updating. Neither pattern provides strong evidence that a general taste for government intervention systematically prevents participants from responding to information or experience. The positive interaction in the belief-updating specification is marginally significant, suggesting some heterogeneity in how

interventionist participants update beliefs when exposed to direct experience rather than factual information, but the evidence is not strong enough to sustain a central claim.

Overall, the survey-index analyses document that the PAP’s index component was implemented and that the final data contain the intended measures of political ideology and support for government intervention. However, the auxiliary regressions do not show a robust or central role for these indexes in explaining the main treatment effects. This helps explain why the final manuscript does not organize the empirical narrative around political ideology or general interventionist attitudes. Instead, the manuscript emphasizes the experimentally induced sources of learning—factual information and direct experience—and the role of decision-making agency. The survey indexes remain useful as design variables, covariates, and auxiliary measures, but they do not provide the main mechanism emphasized in the final paper.

12. PAP Section 7: Multiple testing and inference

PAP Section 7 anticipated that the study would involve multiple hypothesis tests across several related outcomes and treatment comparisons. It therefore proposed adjusting for multiple comparisons using the Benjamini-Hochberg procedure to control the false discovery rate. Tables B14–B25 implement this adjustment for the main PAP-specified primary tests. The adjustments are organized by substantive PAP family rather than pooled across all tests in the appendix, because the PAP hypotheses correspond to distinct conceptual blocks: pre-exposure beliefs, direct exposure, factual information, direct exposure versus factual information, agency effects, and DRETS associations.

The main Benjamini-Hochberg adjustments use the full-sample PAP-specified primary tests. These are the main inferential reference in this implementation report. PAP-specified regressions, manuscript-style pooled complements, auxiliary survey-index regressions, and precision diagnostics are not included in the main Benjamini-Hochberg adjustment because they are not the primary non-parametric or exact tests specified in the PAP. Restricted-sample adjustments are reported separately in Tables B20–B25 and should be interpreted as robustness checks.

The multiple-testing adjustments reinforce the strongest PAP-aligned findings. The direct-exposure results remain highly robust after adjustment. In the full sample, both the FME belief-updating test and the PCE free-market belief-updating test remain significant after Benjamini-

Hochberg correction. The same is true for the direct-exposure effects on regime choices. Thus, the evidence that direct experience changes beliefs and reduces support for the price cap is not sensitive to the multiple-testing adjustment.

The factual-information results also remain robust. In the FMI arm, the reduction in price-cap beliefs after factual information and the reduction in price-cap support both remain significant after adjustment. This supports the PAP prediction that factual information about the consequences of the price cap affects beliefs and choices, even in the absence of direct personal experience.

The comparison between direct exposure and factual information is also robust to adjustment. Both the belief-change comparison and the choice-change comparison between FME and FMI remain significant after Benjamini-Hochberg correction. This is important because this comparison is central to both the PAP and the final manuscript. The adjusted results confirm that direct experience generated larger changes in beliefs and choices than factual information alone.

The evidence for agency effects is weaker and more nuanced after adjustment. In the full sample, the strongest agency-related results are the FME choice-revision comparison, the PCE initial-choice comparison, and the PCE free-market prior comparison. These results are directionally informative and have adjusted p-values close to conventional thresholds. However, they are less robust than the information and direct-exposure results, especially in the restricted-sample adjustment. This supports the interpretation that the PAP-specified average agency comparisons provide suggestive but not definitive evidence. The final manuscript's prior-belief heterogeneity analysis should therefore be read as a theory-guided extension that helps interpret why average agency effects may be muted.

The DRETS family shows the weakest evidence after multiple-testing adjustment. In the full sample, the FME LL final-choice result remains significant after adjustment, and the FME HH initial-choice result is marginal. Most other DRETS tests do not survive adjustment, and the restricted-sample DRETS adjustments are weaker. This pattern is consistent with the precision diagnostics reported in Table B29: the DRETS analyses, especially those involving HH participants, rely on small cells and have limited statistical precision. The multiple-testing results therefore support the decision not to emphasize DRETS-based heterogeneity in the main manuscript.

The pre-exposure belief results are also reported as a separate family. These tests are useful for documenting participants' baseline beliefs before information or direct exposure. However, they

are less central than the updating and choice-revision tests because they are more sensitive to the sample definition and to the treatment-specific interpretation of the belief variables. The full-sample results indicate that participants did not consistently anticipate the shortage generated by the price cap before receiving information or experience, while the restricted-sample results show somewhat stronger anticipation in the FME arm. These results provide useful baseline context but are not the main source of evidence for the paper’s conclusions.

Overall, the multiple-testing adjustments strengthen the interpretation that the most robust PAP-aligned findings concern direct exposure, factual information, and the comparison between them. These results remain strong after controlling the false discovery rate within substantive PAP families. By contrast, agency and DRETS results are more fragile in the average PAP-specified tests. This pattern is consistent with the final manuscript’s emphasis: the core empirical contribution rests on the robust effects of information and experience, while the agency mechanism is developed through a more specific theory-guided heterogeneity analysis and the DRETS results are treated as secondary.

13. Precision and limitations

The PAP included several analyses that are informative but more demanding in terms of statistical precision than the main within-subject tests of information and experience. This is especially true for heterogeneity analyses based on DRETS types, interactions with survey indexes, and agency effects conditional on prior beliefs. Table B29 summarizes precision diagnostics for these analyses. These diagnostics are included to help interpret the strength and limitations of the evidence; they are not intended to provide observed post-hoc power calculations.

Panel A of Table B29 reports cell-size diagnostics for the DRETS analyses. The LL cells are relatively large across treatment arms, with full-sample cell sizes ranging from 159 to 176 participants and restricted-sample cell sizes ranging from 108 to 124 participants. For these cells, the approximate minimum detectable deviation from the 0.5 benchmark is around 0.09 to 0.12. This means that the LL tests are reasonably informative for detecting large deviations from one-half, though they are still not well suited for detecting small differences.

The HH cells are much smaller. In the full sample, the HH cells contain only 19 participants in FME, 16 in FMI, and 16 in PCE. In the restricted sample, the corresponding cell sizes fall to

10, 11, and 7 participants. The approximate minimum detectable deviations are therefore much larger, ranging from about 0.285 to 0.470. These small cell sizes make the HH tests particularly imprecise. As a result, the absence of robust HH patterns should not be interpreted as strong evidence against the role of risk and distributional preferences. Rather, the correct interpretation is that the experiment was not well powered to draw precise conclusions for this small subgroup.

This precision limitation helps explain why the DRETS results are reported for transparency but are not emphasized in the main manuscript. The DRETS tasks were implemented as proposed in the PAP, and the LL/HH tests are reported in Table B13. However, the small HH cells and the instability of the estimates across treatment arms, timing, and sample definitions limit the substantive conclusions that can be drawn from these analyses. The final manuscript therefore uses DRETS measures primarily as auxiliary controls rather than as the basis for a central heterogeneity claim.

Panel B of Table B29 reports precision diagnostics for selected regression-based heterogeneity analyses. These include interactions between the Experience arm and the intervention index, interactions between prior beliefs and decisive status, and the PAP-specified agency LPM in Eq. (26). The estimates vary in precision. The experience-by-intervention interaction for belief updating is estimated with a relatively small standard error, while the corresponding interaction for changes in price-cap support is much less precise. This pattern suggests that the data are more informative about heterogeneity in belief updating than about heterogeneity in choice revision by interventionist attitudes.

The agency-related regression diagnostics also show an important distinction. The PAP-specified Eq. (26), which examines whether decisive participants who initially supported the price cap were more likely to revise their choice, is estimated with relatively strong precision and produces large coefficients in both the full and restricted samples. By contrast, the prior-belief-by-decisive interaction for posterior beliefs is less precise. This helps interpret the difference between the average PAP agency tests and the theory-guided agency analysis in the manuscript: some agency effects are visible in choice revision, but belief-based heterogeneity is statistically more demanding.

These precision diagnostics also clarify the role of restricted-sample estimates. The restricted sample is useful for comparability with the manuscript’s mechanism-focused specifications and can reduce noise from internally inconsistent belief or choice reports. At the same time, restricting

the sample reduces the number of observations, which can increase uncertainty in subgroup and interaction analyses. For this reason, restricted-sample results in this implementation report should be interpreted as robustness checks rather than as the primary PAP-aligned evidence.

Overall, the main precision limitation concerns secondary heterogeneity analyses, not the core PAP tests. The within-subject tests of direct exposure and factual information, as well as the comparison between direct exposure and factual information, are estimated precisely and remain robust after multiple-testing adjustment. By contrast, DRETS-based heterogeneity, some agency comparisons, and some survey-index interactions are less precise. This distinction supports the organization of the final manuscript: the main empirical claims rest on the robust effects of information and experience, while the more exploratory or less precise heterogeneity analyses are reported here for transparency and interpreted cautiously.

14. Summary of PAP alignment

This implementation report shows that the core experimental design and the main reduced-form comparisons proposed in the PAP were implemented. The treatment arms, the sequence of market regimes, the timing of belief elicitation, the initial and final regime choices, the agency manipulation, the DRETS tasks, and the survey-based ideology and intervention indexes all correspond closely to the original plan. The main differences between the PAP and the final manuscript concern the organization, emphasis, and interpretation of the empirical analysis rather than the underlying experimental design.

The strongest alignment with the PAP appears in the analyses of direct exposure, factual information, and the comparison between the two. The PAP proposed testing whether direct exposure to the price-cap regime would change beliefs and reduce support for the policy. The FME results show substantial downward revision in beliefs about the price cap and a large reduction in price-cap support after direct exposure. The PCE / Reverse Experience arm provides complementary evidence that participants also update strongly when they experience the free-market regime after first encountering the price cap. These results closely implement the original PAP and provide strong support for its core predictions.

The factual-information analyses also closely follow the PAP. In the FMI arm, participants revise beliefs downward and become less likely to choose the price-cap regime after receiving information

about the market outcomes generated by the price cap. These effects are smaller than those produced by direct exposure. The comparison between FME and FMI therefore confirms a central PAP-aligned result: both information and experience matter, but personal experience produces stronger updating and larger changes in regime choices.

The pre-exposure belief analyses are also implemented as proposed, but their interpretation is more nuanced. The full-sample within-subject comparisons show little evidence that participants clearly anticipated the shortage generated by the price cap before receiving information or experience. The restricted sample provides somewhat stronger evidence of anticipation in the FME arm. The between-arm comparison in PAP-5.1B / Eq. (2), implemented using the correct treatment-specific mapping, shows a large difference between FME price-cap priors and PCE free-market priors. Overall, these results support the PAP's broader motivation that participants entered the experiment with imperfect or distorted expectations about price-cap outcomes, even if the evidence is not uniform across all samples and comparisons.

The agency analyses are the main area where the final manuscript goes beyond the PAP. The PAP focused on average comparisons between decisive and hypothetical choosers. Those average tests are reported in this implementation report and provide partial support for the original agency prediction, especially in the FME choice-revision analysis and the PAP-specified agency LPM. However, the average belief-updating results are weaker, and some agency effects are less robust in the restricted sample. The final manuscript therefore develops a more specific theory-guided analysis that conditions agency effects on participants' prior beliefs. This prior-belief heterogeneity analysis should be interpreted as an extension of the PAP-specified average tests, not as a replacement for them.

The DRETS analyses were implemented as proposed in the PAP, but the results are not sufficiently precise or stable to support a central substantive claim. The LL and HH tests are reported by treatment arm and timing, as planned. Some individual results are directionally consistent with the PAP, especially for LL final choices in FME and HH initial choices in FME. However, the patterns do not replicate consistently across arms, samples, timing, or multiple-testing adjustments. Moreover, the HH cells are small, which limits the precision of the corresponding tests. For this reason, the final manuscript does not emphasize DRETS-based heterogeneity and instead uses DRETS measures primarily as auxiliary controls.

The survey-index analyses document that the ideology and government-intervention indexes proposed in the PAP were constructed and used. These indexes are useful for group formation, covariate adjustment, and auxiliary analyses of heterogeneity. However, the regressions relating the intervention index to baseline support and updating do not provide strong evidence that general interventionist attitudes are the main driver of the treatment effects. This helps explain why the final manuscript does not organize its main empirical narrative around political ideology or broad support for government intervention.

The multiple-testing adjustments reinforce the same hierarchy of evidence. The results for direct exposure, factual information, and direct exposure versus factual information remain strong after Benjamini-Hochberg adjustment. By contrast, the average agency tests and DRETS associations are more fragile after adjustment. This pattern supports the final manuscript’s organization: the main empirical claims rest on robust PAP-aligned evidence about information and experience, while agency is interpreted through a more specific theory-guided mechanism and DRETS are treated as secondary.

Overall, the final manuscript should be understood as a theory-guided refinement of a preregistered experimental design. The experiment followed the PAP in its core design and implemented the main PAP-specified empirical comparisons. The final paper differs from the PAP mainly in emphasis and interpretation: it develops a richer theoretical framework, organizes the empirical results through regression-based specifications, emphasizes prior-belief heterogeneity in the agency analysis, and gives less prominence to underpowered secondary analyses. These changes affect the interpretation and presentation of the results, but they do not alter the underlying experimental design or the central PAP-aligned findings.

Transparency note.

PAP-specified primary tests are reported as the main inferential reference. These include Wilcoxon signed-rank tests, Wilcoxon rank-sum tests, binomial tests, and exact McNemar/binomial tests where applicable.

Unless otherwise stated, Wilcoxon, rank-sum, and McNemar/binomial p-values are reported as two-sided. This is conservative for PAP hypotheses with a directional interpretation. DRETS tests use one-sided binomial p-values because the PAP states directional predictions for LL and HH types. PAP-specified regressions and LPMs are reported separately in this implementation report.

Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Balance checks are not duplicated here because they are reported in the manuscript's Online Appendix; this standalone PAP upload focuses on PAP implementation, deviations, and robustness analyses.

Table B1: Summary of PAP alignment and implementation clarifications

PAP item	Planned analysis	Implementation	Clarification or deviation
PAP-5.1B / Eq. (2)	Compare FME price-cap prior with PCE free-market prior.	Uses <code>block_2_prior_prob</code> in FME and PCE.	No PCE price-cap prior is required.
Eq. (26)	Agency LPM in the FM-E sample.	Estimated on the FME sample.	Pooled version reported only as manuscript-style complement.
Restricted sample	Primary PAP tests use full sample.	Restricted results reported separately.	Robustness and comparability with manuscript.
Benjamini-Hochberg	Adjust primary PAP tests by substantive family.	Full-sample PAP-specified primary tests used in main BH tables.	PAP regressions and pooled complements excluded from main BH.
Balance and restricted-sample checks	Assess baseline comparability across treatment arms and evaluate whether the restricted sample changes the study population.	Balance checks across information-treatment arms and agency status are reported in the manuscript's Online Appendix. Restricted-sample selection is assessed in the manuscript using regressions of restricted-sample inclusion on treatment assignment.	Not duplicated in this PAP appendix. This appendix focuses on PAP implementation, deviations, and robustness of PAP-specified analyses. Restricted-sample PAP results are reported as robustness/comparability checks.
Survey indexes and intervention attitudes	Study whether attitudes toward government intervention are related to support for price caps and responsiveness to information.	Index construction is documented, and auxiliary regressions relate the intervention index to baseline support and updating.	Auxiliary regression-based analysis; not included in the main primary-test BH adjustment.
Precision of heterogeneity analyses	Evaluate heterogeneity in regime choices, DRETS types, agency status, and intervention attitudes.	Primary PAP and auxiliary heterogeneity analyses are reported in the appendix; an auxiliary precision-diagnostics table reports cell sizes and approximate detectable effects/deviations.	No observed post-hoc power is reported. Precision diagnostics are included only to document limited statistical precision, especially for small HH DRETS cells.

Table B2: Outcome-construction crosswalk for PAP analyses

PAP concept	Final-data variable	FME map- ping	FMI map- ping	PCE map- ping	Notes
Free-market posterior	<code>block_1_prob</code>	Yes	Yes	No	In FME and FMI, block 1 is the free-market regime.
Price-cap prior	<code>block_2_prior_prob</code>	Yes	Yes	No	In FME and FMI, block 2 prior is elicited before price-cap exposure or factual information.
Free-market prior	<code>block_2_prior_prob</code>	No	No	Yes	In PCE, block 2 prior is elicited before free-market exposure. This is the relevant PCE prior for PAP-5.1B / Eq. (2).
Price-cap posterior	<code>block_2_post_prob</code>	Yes	Yes	No	In FME, this is after price-cap experience. In FMI, this is after factual information.
Free-market posterior in PCE	<code>block_2_post_prob</code>	No	No	Yes	In PCE, block 2 post is elicited after free-market exposure.
Initial price-cap choice	<code>price_cap_for_block_3</code>	Yes	Yes	Yes	Used to measure initial support for the price cap.
Final price-cap choice	<code>price_cap_for_block_4</code>	Yes	Yes	Yes	Used to measure final support for the price cap.

Notes: Derived manuscript-style variables are documented separately and are not used for the corrected Eq. (2).

Table B3: Pre-exposure belief comparisons

Arm	FM belief mean	PC prior mean	Difference	p-value	N
Panel A. Full sample					
FME	0.693	0.686	-0.007	0.796	264
FMI	0.707	0.726	0.018	0.087	259
Panel B. Restricted sample					
FME	0.772	0.721	-0.051	0.002	171
FMI	0.775	0.760	-0.015	0.779	180

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-values are from two-sided Wilcoxon signed-rank tests. FM belief is block_1_prob and PC prior is block_2_prior_prob within FME and FMI. Difference is PC prior mean minus FM belief mean. Restricted sample uses consistent_exp = 1 and consistent_choice_0 = 1. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B4: Pre-exposure between-arm prior comparison

Sample	FME mean	PCE mean	Difference	p-value	FME N	PCE N
Full sample	0.686	0.458	0.228	< 0.001	264	298
Restricted	0.721	0.470	0.251	< 0.001	171	201

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-value is from a two-sided Wilcoxon rank-sum test. PAP-5.1B / Eq. (2) compares block_2_prior_prob across FME and PCE. In FME this variable is the price-cap prior before price-cap exposure; in PCE it is the free-market prior before free-market exposure. Difference is FME mean minus PCE mean. The test does not require a PCE price-cap prior. Restricted sample uses consistent_exp = 1 and consistent_choice_0 = 1. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B5: Direct exposure effects on beliefs

Arm	Prior mean	Posterior mean	Difference	p-value	N
Panel A. Full sample					
FME	0.686	0.521	-0.164	< 0.001	264
PCE	0.458	0.725	0.268	< 0.001	298
Panel B. Restricted sample					
FME	0.721	0.515	-0.206	< 0.001	171
PCE	0.470	0.782	0.312	< 0.001	201

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-values are from two-sided Wilcoxon signed-rank tests. For FME, prior and posterior refer to price-cap beliefs. For PCE, prior and posterior refer to free-market beliefs. Difference is posterior mean minus prior mean. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B6: Direct exposure effects on regime choices

Arm	Initial PC	Final PC	Difference	PC to FM	FM to PC	p-value	N
Panel A. Full sample							
FME	0.648	0.398	-0.250	0.356	0.106	< 0.001	264
PCE	0.742	0.500	-0.242	0.332	0.091	< 0.001	298
Panel B. Restricted sample							
FME	0.760	0.409	-0.351	0.421	0.070	< 0.001	171
PCE	0.871	0.502	-0.368	0.423	0.055	< 0.001	201

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-value is the two-sided exact McNemar/binomial p-value computed from discordant choice pairs. Initial PC support is mean `price_cap_for_block_3` and final PC support is mean `price_cap_for_block_4`. Difference is final minus initial. Switch shares are computed among observations with nonmissing initial and final choices. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Shares and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B7: Order effects

Outcome	FME mean	PCE mean	Difference	p-value
Panel A. Full sample				
Final free-market belief	0.693	0.725	-0.033	0.073
Final price-cap belief	0.521	0.505	0.017	0.668
Final price-cap choice	0.398	0.500	-0.102	0.015
Panel B. Restricted sample				
Final free-market belief	0.772	0.782	-0.010	0.313
Final price-cap belief	0.515	0.490	0.025	0.360
Final price-cap choice	0.409	0.502	-0.093	0.073

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-values are from two-sided Wilcoxon rank-sum tests. Final free-market and price-cap beliefs use the treatment-specific mapping in `belief_variable_mapping.md`. Difference is FME mean minus PCE mean. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001 ; missing values are reported as $-$.

Table B8: Factual information effects on beliefs

Sample	PC prior mean	Post-info mean	Difference	p-value	N
Full sample	0.726	0.660	-0.066	< 0.001	259
Restricted	0.760	0.690	-0.070	< 0.001	180

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-value is from a two-sided Wilcoxon signed-rank test. PC prior mean is block_2_prior_prob and post-information mean is block_2_post_prob within FMI. Difference is post-information mean minus prior mean. Restricted sample uses consistent_exp = 1 and consistent_choice_0 = 1. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B9: Factual information effects on regime choices

Sample	Initial PC	Final PC	Difference	PC to FM	FM to PC	p-value	N
Full sample	0.699	0.591	-0.108	0.205	0.097	0.002	259
Restricted	0.800	0.617	-0.183	0.244	0.061	< 0.001	180

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-value is the two-sided exact McNemar/binomial p-value computed from discordant choice pairs. Initial PC support is mean price_cap_for_block_3 and final PC support is mean price_cap_for_block_4 within FMI. Difference is final minus initial. Switch shares are computed among observations with nonmissing initial and final choices. Restricted sample uses consistent_exp = 1 and consistent_choice_0 = 1. Shares and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B10: Direct exposure versus factual information

Outcome	FME change	FMI change	Difference	p-value	FME N	FMI N
Panel A. Full sample						
Belief change	-0.164	-0.066	-0.098	< 0.001	264	259
Choice change	-0.250	-0.108	-0.142	0.003	264	259
Panel B. Restricted sample						
Belief change	-0.206	-0.070	-0.135	< 0.001	171	180
Choice change	-0.351	-0.183	-0.168	0.003	171	180

Notes: PAP-specified primary tests are reported as the main inferential reference. The p-values are from two-sided Wilcoxon rank-sum tests. Belief change is `block_2_post_prob` minus `block_2_prior_prob`. Choice change is `price_cap_for_block_4` minus `price_cap_for_block_3`. Difference is FME change minus FMI change. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Changes and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001; missing values are reported as –.

Table B11: Agency effects on beliefs

Agency belief test	Non-decisive	Decisive	Difference	p-value	N
Panel A. Full sample					
FME: PC prior belief by decisive status	0.681	0.699	0.018	0.619	264
PCE: FM prior belief by decisive status	0.444	0.499	0.055	0.027	298
FME: PC belief update by decisive status	-0.158	-0.182	-0.023	0.563	264
Panel B. Restricted sample					
FME: PC prior belief by decisive status	0.716	0.734	0.018	0.597	171
PCE: FM prior belief by decisive status	0.454	0.524	0.071	0.053	201
FME: PC belief update by decisive status	-0.207	-0.202	0.005	0.966	171
Manuscript-style pooled complements, not included in the main BH adjustment					
Pooled prior belief, full	0.703	0.712	0.009	0.936	523
Pooled prior belief, restricted	0.736	0.757	0.021	0.544	351
Pooled belief update, full	-0.111	-0.129	-0.018	0.361	523
Pooled belief update, restricted	-0.128	-0.162	-0.034	0.121	351

Notes: PAP-specified primary tests are reported as the main inferential reference. Arm-specific agency p-values are from two-sided Wilcoxon rank-sum tests. Difference is decisive mean minus non-decisive mean. PCE prior belief refers to the free-market prior, because a true PCE price-cap prior is unavailable under the current block mapping. Pooled rows are manuscript-style complements and are not included in the main Benjamini-Hochberg adjustment. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Means and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001 ; missing values are reported as $-$.

Table B12: Agency effects on regime choices

Agency choice test	Non-decisive	Decisive	Difference	p-value	N
Panel A. Full sample					
FME: initial choice by decisive status	0.631	0.697	0.066	0.334	264
PCE: initial choice by decisive status	0.776	0.640	-0.136	0.020	298
FME: choice revision by decisive status	-0.207	-0.379	-0.172	0.018	264
Panel B. Restricted sample					
FME: initial choice by decisive status	0.750	0.791	0.041	0.590	171
PCE: initial choice by decisive status	0.890	0.804	-0.086	0.128	201
FME: choice revision by decisive status	-0.320	-0.442	-0.122	0.139	171
Manuscript-style pooled complements, not included in the main BH adjustment					
Pooled initial choice, full	0.703	0.683	-0.020	0.589	821
Pooled initial choice, restricted	0.817	0.803	-0.014	0.726	552
Pooled choice change, full	-0.195	-0.224	-0.030	0.264	821
Pooled choice change, restricted	-0.281	-0.371	-0.090	0.056	552

Notes: PAP-specified primary tests are reported as the main inferential reference. Arm-specific agency p-values are from two-sided Wilcoxon rank-sum tests. Difference is decisive share or mean change minus non-decisive share or mean change. Pooled rows are manuscript-style complements and are not included in the main Benjamini-Hochberg adjustment. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. Shares, changes, and differences are reported to three decimals. P-values below 0.001 are reported as < 0.001 ; missing values are reported as $-$.

Table B13: DRETS and regime choices

Arm	DRETS type	Timing	Full N	Full share	Full p-value	Restricted N	Restricted share	Restricted p-value
FME	LL	Initial	163	0.681	1.000	108	0.778	1.000
FME	LL	Final	163	0.393	0.004	108	0.407	0.034
FME	HH	Initial	19	0.789	0.010	10	0.900	0.011
FME	HH	Final	19	0.579	0.324	10	0.600	0.377
FMI	LL	Initial	159	0.755	1.000	113	0.858	1.000
FMI	LL	Final	159	0.616	0.999	113	0.646	0.999
FMI	HH	Initial	16	0.500	0.598	11	0.727	0.113
FMI	HH	Final	16	0.375	0.895	11	0.455	0.726
PCE	LL	Initial	176	0.790	1.000	124	0.887	1.000
PCE	LL	Final	176	0.511	0.647	124	0.524	0.735
PCE	HH	Initial	16	0.438	0.773	7	0.714	0.227
PCE	HH	Final	16	0.438	0.773	7	0.571	0.500
Optional pooled summary, not included in the main BH adjustment								
All arms	LL	Initial	498	0.743	1.000	345	0.843	1.000
All arms	LL	Final	498	0.506	0.623	345	0.528	0.859
All arms	HH	Initial	51	0.588	0.131	28	0.786	0.002
All arms	HH	Final	51	0.471	0.712	28	0.536	0.425

Notes: PAP-specified primary tests are reported as the main inferential reference. PAP-5.5 specifies tests by treatment arm, DRETS type, and timing; the pooled panel is descriptive only and is not included in the main Benjamini-Hochberg adjustment. LL tests use one-sided binomial p-values for the directional alternative that the price-cap choice share is less than 0.5; HH tests use one-sided binomial p-values for the directional alternative that the price-cap choice share is greater than 0.5. LL is defined by `high_risk_lottery=0` and `high_inequality_allocation=0`; HH is defined by `high_risk_lottery=1` and `high_inequality_allocation=1`. Mixed DRETS types are excluded from the main LL/HH hypothesis tests. The restricted sample requires `consistent_exp=1` and `consistent_choice_0=1`. P-values below 0.001 are reported as < 0.001 .

Table B14: Main Benjamini-Hochberg adjustments: Panel A. DRETS associations

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.5	FME: LL final price-cap choice vs. 0.5	Full	0.004	0.045
PAP-5.5	FME: HH initial price-cap choice vs. 0.5	Full	0.010	0.058
PAP-5.5	FME: HH final price-cap choice vs. 0.5	Full	0.324	1.000
PAP-5.5	FMI: HH initial price-cap choice vs. 0.5	Full	0.598	1.000
PAP-5.5	PCE: LL final price-cap choice vs. 0.5	Full	0.647	1.000
PAP-5.5	PCE: HH initial price-cap choice vs. 0.5	Full	0.773	1.000
PAP-5.5	PCE: HH final price-cap choice vs. 0.5	Full	0.773	1.000
PAP-5.5	FMI: HH final price-cap choice vs. 0.5	Full	0.895	1.000
PAP-5.5	FMI: LL final price-cap choice vs. 0.5	Full	0.999	1.000
PAP-5.5	FME: LL initial price-cap choice vs. 0.5	Full	1.000	1.000
PAP-5.5	FMI: LL initial price-cap choice vs. 0.5	Full	1.000	1.000
PAP-5.5	PCE: LL initial price-cap choice vs. 0.5	Full	1.000	1.000

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001 .

Table B15: Main Benjamini-Hochberg adjustments: Panel B. Agency effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.4.2	FME: choice revision by decisive status	Full	0.018	0.054
PAP-5.4.2	PCE: initial choice by decisive status	Full	0.020	0.054
PAP-5.4.1	PCE: FM prior belief by decisive status	Full	0.027	0.054
PAP-5.4.2	FME: initial choice by decisive status	Full	0.334	0.502
PAP-5.4.1	FME: PC belief update by decisive status	Full	0.563	0.619
PAP-5.4.1	FME: PC prior belief by decisive status	Full	0.619	0.619

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001 .

Table B16: Main Benjamini-Hochberg adjustments: Panel C. Direct exposure effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.2.1	PCE: FM prior vs. FM posterior	Full	< 0.001	< 0.001
PAP-5.2.1	FME: PC prior vs. PC posterior	Full	< 0.001	< 0.001
PAP-5.2.2	PCE: initial vs. final price-cap choice	Full	< 0.001	< 0.001
PAP-5.2.2	FME: initial vs. final price-cap choice	Full	< 0.001	< 0.001
PAP-5.2.3	Order effect: final price-cap choice	Full	0.015	0.021
PAP-5.2.3	Order effect: final free-market belief	Full	0.073	0.085
PAP-5.2.3	Order effect: final price-cap belief	Full	0.668	0.668

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B17: Main Benjamini-Hochberg adjustments: Panel D. Direct exposure versus factual information

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.3.3	FME vs. FMI: belief change	Full	< 0.001	< 0.001
PAP-5.3.3	FME vs. FMI: choice change	Full	0.003	0.003

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B18: Main Benjamini-Hochberg adjustments: Panel E. Factual information effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.3.1	FMI: PC prior vs. post-information belief	Full	< 0.001	< 0.001
PAP-5.3.2	FMI: initial vs. final price-cap choice	Full	0.002	0.002

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B19: Main Benjamini-Hochberg adjustments: Panel F. Pre-exposure beliefs

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.1B	FME PC prior vs. PCE FM prior	Full	< 0.001	< 0.001
PAP-5.1A	FMI: FM posterior vs. PC prior	Full	0.087	0.130
PAP-5.1A	FME: FM posterior vs. PC prior	Full	0.796	0.796

Notes: PAP-specified primary tests are reported as the main inferential reference. This main Benjamini-Hochberg table uses full-sample PAP-specified primary tests only. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg families are defined by substantive PAP block. Complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B20: Restricted-sample Benjamini-Hochberg robustness: Panel A. DRETS associations

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.5	FME: HH initial price-cap choice vs. 0.5	Restricted	0.011	0.129
PAP-5.5	FME: LL final price-cap choice vs. 0.5	Restricted	0.034	0.201
PAP-5.5	FMI: HH initial price-cap choice vs. 0.5	Restricted	0.113	0.453
PAP-5.5	PCE: HH initial price-cap choice vs. 0.5	Restricted	0.227	0.680
PAP-5.5	FME: HH final price-cap choice vs. 0.5	Restricted	0.377	0.905
PAP-5.5	PCE: HH final price-cap choice vs. 0.5	Restricted	0.500	1.000
PAP-5.5	FMI: HH final price-cap choice vs. 0.5	Restricted	0.726	1.000
PAP-5.5	PCE: LL final price-cap choice vs. 0.5	Restricted	0.735	1.000
PAP-5.5	FMI: LL final price-cap choice vs. 0.5	Restricted	0.999	1.000
PAP-5.5	FMI: LL initial price-cap choice vs. 0.5	Restricted	1.000	1.000
PAP-5.5	FME: LL initial price-cap choice vs. 0.5	Restricted	1.000	1.000
PAP-5.5	PCE: LL initial price-cap choice vs. 0.5	Restricted	1.000	1.000

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001 .

Table B21: Restricted-sample Benjamini-Hochberg robustness: Panel B. Agency effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.4.1	PCE: FM prior belief by decisive status	Restricted	0.053	0.277
PAP-5.4.2	PCE: initial choice by decisive status	Restricted	0.128	0.277
PAP-5.4.2	FME: choice revision by decisive status	Restricted	0.139	0.277
PAP-5.4.2	FME: initial choice by decisive status	Restricted	0.590	0.716
PAP-5.4.1	FME: PC prior belief by decisive status	Restricted	0.597	0.716
PAP-5.4.1	FME: PC belief update by decisive status	Restricted	0.966	0.966

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001 .

Table B22: Restricted-sample Benjamini-Hochberg robustness: Panel C. Direct exposure effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.2.1	PCE: FM prior vs. FM posterior	Restricted	< 0.001	< 0.001
PAP-5.2.1	FME: PC prior vs. PC posterior	Restricted	< 0.001	< 0.001
PAP-5.2.2	PCE: initial vs. final price-cap choice	Restricted	< 0.001	< 0.001
PAP-5.2.2	FME: initial vs. final price-cap choice	Restricted	< 0.001	< 0.001
PAP-5.2.3	Order effect: final price-cap choice	Restricted	0.073	0.102
PAP-5.2.3	Order effect: final free-market belief	Restricted	0.313	0.360
PAP-5.2.3	Order effect: final price-cap belief	Restricted	0.360	0.360

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001 .

Table B23: Restricted-sample Benjamini-Hochberg robustness: Panel D. Direct exposure versus factual information

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.3.3	FME vs. FMI: belief change	Restricted	< 0.001	< 0.001
PAP-5.3.3	FME vs. FMI: choice change	Restricted	0.003	0.003

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B24: Restricted-sample Benjamini-Hochberg robustness: Panel E. Factual information effects

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.3.1	FMI: PC prior vs. post-information belief	Restricted	< 0.001	< 0.001
PAP-5.3.2	FMI: initial vs. final price-cap choice	Restricted	< 0.001	< 0.001

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B25: Restricted-sample Benjamini-Hochberg robustness: Panel F. Pre-exposure beliefs

PAP item	Test	Sample	p-value	BH-adjusted p-value
PAP-5.1B	FME PC prior vs. PCE FM prior	Restricted	< 0.001	< 0.001
PAP-5.1A	FME: FM posterior vs. PC prior	Restricted	0.002	0.004
PAP-5.1A	FMI: FM posterior vs. PC prior	Restricted	0.779	0.779

Notes: Restricted-sample results are reported for comparability with the manuscript and should be interpreted as robustness. Restricted sample uses `consistent_exp = 1` and `consistent_choice_0 = 1`. PCE has no true price-cap prior under the current block mapping, but PAP-5.1B / Eq. (2) does not require one: it compares FME price-cap priors with PCE free-market priors. Benjamini-Hochberg adjustments use only PAP-specified primary tests within this restricted-sample robustness set; complementary manuscript-style LPMs are excluded. P-values below 0.001 are reported as < 0.001.

Table B26: PAP-specified regression and LPM specifications

PAP eq.	Outcome	Scope	Coefficient of interest	Coef.	SE	p-value	Joint p-value	N
Panel A. Full sample								
Eq. (2)	Prior belief	FME/PCE	FME vs. PCE	0.228	0.021	< 0.001	–	562
Eq. (9)	Final PC choice	FME	Initial PC choice	0.149	0.053	0.015	< 0.001	264
Eq. (12)	Final PC choice	FME/PCE	FME vs. PCE	-0.102	0.043	0.025	–	562
Eq. (26)	Final PC choice	FME	Initial PC choice x decisive	-0.636	0.135	< 0.001	–	264
Pooled complement	Final PC choice	All arms	Initial PC choice x decisive	-0.460	0.072	< 0.001	–	821
Panel B. Restricted sample								
Eq. (2)	Prior belief	FME/PCE	FME vs. PCE	0.251	0.025	< 0.001	–	372
Eq. (9)	Final PC choice	FME	Initial PC choice	0.153	0.079	0.075	< 0.001	171
Eq. (12)	Final PC choice	FME/PCE	FME vs. PCE	-0.093	0.053	0.090	–	372
Eq. (26)	Final PC choice	FME	Initial PC choice x decisive	-0.543	0.171	0.007	–	171
Pooled complement	Final PC choice	All arms	Initial PC choice x decisive	-0.343	0.122	0.007	–	552

Notes: This table reports regression and LPM specifications explicitly stated in the PAP. Standard errors are clustered by session (**session_code**). The restricted sample requires **consistent_exp**=1 and **consistent_choice_0**=1. For Eq. (2), **block_2_prior_prob** is the price-cap prior in FME and the free-market prior in PCE. This directly implements the PAP between-arm prior comparison. For Equation (9), the coefficient reported in the table is **beta_1** on initial price-cap choice, and the joint p-value tests $\beta_0 = 0$ and $\beta_1 = 1$. Eq. (26) is estimated on the FM-E sample, as specified in the PAP. The pooled specification is reported only as a manuscript-style complement. These regression p-values are not included in the main PAP-specified primary-test Benjamini-Hochberg adjustment. P-values below 0.001 are reported as < 0.001.

Table B27: PAP Section 6.2 survey index variables

Variable	N	Mean	SD	Missing
Ideology index (ICW)	821	0.000	0.845	0
Intervention index (ICW)	821	0.000	0.556	0
Ideology index (MEI)	821	0.000	0.845	0
Intervention index (MEI)	821	0.000	0.591	0

Notes: PAP Section 6.2 refers to survey indexes. This appendix does not reconstruct the indexes because the final analysis-ready dataset already contains `index2_ideology_ICW`, `index2_intervention_ICW`, `index2_ideology_MEI`, and `index2_intervention_MEI`. The ICW indexes, `index2_ideology_ICW` and `index2_intervention_ICW`, are the preferred variables used in manuscript-style specifications.

Table B28: Survey-index associations with baseline support and information updating

	Initial PC support (1)	Prior belief PC reduces buying (2)	Change in PC support (3)	Belief updating (4)
Intervention index (ICW)	0.016 (0.033)	0.058* (0.034)	0.048 (0.071)	-0.005 (0.027)
Ideology index (ICW)	0.019 (0.019)	0.007 (0.022)	-0.012 (0.032)	0.012 (0.013)
Experience $\times$ Intervention index	– –	– –	-0.071 (0.103)	0.075* (0.039)
Constant	0.698*** (0.018)	0.370*** (0.018)	-0.106*** (0.031)	-0.067*** (0.012)
N	821	821	523	523
Clusters	45	45	28	28

Notes: This table is an auxiliary PAP-6.2 analysis. The intervention index and ideology index are Anderson ICW indexes in the final dataset. Columns 1 and 2 examine baseline associations. Columns 3 and 4 restrict the sample to FME and FMI and test whether the intervention index predicts updating after information or experience. FMI is the omitted treatment arm in columns 3 and 4, so the coefficient on the intervention index refers to factual information; the interaction with FME captures the differential association under direct experience. Clustered standard errors by session are in parentheses. These p-values are not included in the main Benjamini-Hochberg adjustment because this table is auxiliary and regression-based. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B29: Precision diagnostics for selected heterogeneity analyses

Panel A. DRETS cell-size diagnostics							
Arm	DRETS type	Timing	Sample	N	PC share	Alternative	Approx. MDD
FME	LL	Initial	Full	163	0.681	LL < 0.5	0.097
FME	LL	Initial	Restricted	108	0.778	LL < 0.5	0.120
FME	LL	Final	Full	163	0.393	LL < 0.5	0.097
FME	LL	Final	Restricted	108	0.407	LL < 0.5	0.120
FME	HH	Initial	Full	19	0.789	HH > 0.5	0.285
FME	HH	Initial	Restricted	10	0.900	HH > 0.5	0.393
FME	HH	Final	Full	19	0.579	HH > 0.5	0.285
FME	HH	Final	Restricted	10	0.600	HH > 0.5	0.393
FMI	LL	Initial	Full	159	0.755	LL < 0.5	0.099
FMI	LL	Initial	Restricted	113	0.858	LL < 0.5	0.117
FMI	LL	Final	Full	159	0.616	LL < 0.5	0.099
FMI	LL	Final	Restricted	113	0.646	LL < 0.5	0.117
FMI	HH	Initial	Full	16	0.500	HH > 0.5	0.311
FMI	HH	Initial	Restricted	11	0.727	HH > 0.5	0.375
FMI	HH	Final	Full	16	0.375	HH > 0.5	0.311
FMI	HH	Final	Restricted	11	0.455	HH > 0.5	0.375
PCE	LL	Initial	Full	176	0.790	LL < 0.5	0.094
PCE	LL	Initial	Restricted	124	0.887	LL < 0.5	0.112
PCE	LL	Final	Full	176	0.511	LL < 0.5	0.094
PCE	LL	Final	Restricted	124	0.524	LL < 0.5	0.112
PCE	HH	Initial	Full	16	0.438	HH > 0.5	0.311
PCE	HH	Initial	Restricted	7	0.714	HH > 0.5	0.470
PCE	HH	Final	Full	16	0.438	HH > 0.5	0.311
PCE	HH	Final	Restricted	7	0.571	HH > 0.5	0.470
Panel B. Regression precision diagnostics							
Analysis	Sample	Coefficient of interest	Estimate	Clustered SE	Approx. MDE	N	Clusters
Exper.-by-intervention index, PC support change	FME/FMI full	Exper. × intervention	-0.071	0.103	0.287	523	28
Exper.-by-intervention index, belief updating	FME/FMI full	Exper. × intervention	0.075	0.039	0.108	523	28
Decisive-by-prior belief, posterior belief	FME full	Prior belief × decisive	-0.211	0.127	0.356	264	14
Eq. (26), choice revision agency LPM	FME full	Initial PC choice × decisive	-0.636	0.135	0.378	264	14
Exper.-by-intervention index, PC support change	FME/FMI restricted	Exper. × intervention	-0.115	0.097	0.271	351	28
Exper.-by-intervention index, belief updating	FME/FMI restricted	Exper. × intervention	0.043	0.044	0.123	351	28
Decisive-by-prior belief, posterior belief	FME restricted	Prior belief × decisive	-0.204	0.108	0.301	171	14
Eq. (26), choice revision agency LPM	FME restricted	Initial PC choice × decisive	-0.543	0.171	0.479	171	14

Notes: This table does not report observed post-hoc power. Minimum detectable deviations/effects are approximate precision diagnostics based on realized sample sizes or clustered standard errors. Panel A reports the approximate minimum detectable deviation from 0.5 for one-sided DRETS tests using $2.485\sqrt{0.25/N}$, where 2.485 equals 1.645 plus 0.84. Panel B reports approximate minimum detectable effects for selected regression coefficients using $2.80\times$ the clustered standard error. The diagnostics are included to summarize the limited precision of selected heterogeneity analyses, especially small HH DRETS cells. These diagnostics are not primary PAP tests and are not included in Benjamini-Hochberg adjustments. The restricted sample requires `consistent_exp=1` and `consistent_choice_0=1`.