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

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Abstract

Despite broad expert consensus that price controls are economically inefficient, they remain popular among citizens. 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 real 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 through direct exposure to its consequences, and whether their regime choice was decisive for implementation or purely hypothetical. We found that both information and direct exposure reduced support for the price cap, but direct exposure was substantially more effective. However, a sizable share of the participants continued to support the cap after experiencing shortages. Political agency disciplined beliefs: participants with implementation responsibility were more responsive to adverse outcomes and less likely to endorse the policy. Overall, support for inefficient policies reflects not only misinformation but also self-serving beliefs that persist when individuals are not held accountable for policy outcomes.

Keywords: Price caps; experience; information; motivated beliefs; agency; inefficient economic policies.

JEL Classification Numbers: C91, D03, D72, D83.

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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 today (Guenette, 2020). In many developing countries, they continue to be a central tool to curb inflation and protect consumers, especially for 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 energy-price surge (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 such policy remains 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 too. A 2024 YouGov poll in the United States found that more than 70% of respondents were open 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 also documented strong support for rent control among residents of California (DiCamillo, 2017), London (London Mayor's Office, 2019), the broader 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 market experiment that cleanly separates the roles of factual information, personal experience, and political agency in shaping beliefs and policy support.

Previous research suggests that citizens sometimes support inefficient policies—or oppose efficiency-enhancing ones—because they misunderstand their effects. Following Blinder and

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

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 the persistent popularity of this policy. Instead, we propose that support for price caps may be sustained by *self-serving beliefs*, which are encouraged by citizens' lack of political 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 factual information about the effects of the price cap or personally experiencing its consequences. Following the literature on motivated beliefs (Brunnermeier and Parker, 2005; Bénabou, 2015; Molnar and Loewenstein, 2022), we assume that 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 political 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 lived experiences shape economic beliefs more powerfully than abstract

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

information (Malmendier, 2021). This implies that despite the abundance of information about price 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 real 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 political agency by assigning participants either a decisive role in implementing the market regime or a non-binding, 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 sidestep cognitive frictions and focus instead on the influence of motivated beliefs and political agency.

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—support

for a price cap—while holding the policy environment fixed. Survey experiments examining public attitudes toward price caps on consumer goods (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) 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—did not test for the role of factual information. By manipulating both factual information and experience through exogenously induced product shortages, our design allows 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 closest related work is Robbett and Matthews (2018), which examines how perceived pivotality in group decisions affects partisan bias.[3] Unlike that study, we focused on attitudes toward government intervention in markets rather than political partisanship, measuring endorsement of a pervasive economic policy such as price caps. Additionally, we compared individuals who were certain to be decisive with others who were certain to be non-decisive, instead of comparing individuals with varying but positive likelihoods of being decisive. One concern with the latter approach is that, due to an optimism bias (Sharot, 2011) or an 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 in the political agency spectrum, we avoided any confounding influence of misperceptions about agency. This feature enabled 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 political agency moderates this process: when individuals are accountable for implementation, beliefs and policy choices become more disciplined. These insights extend beyond price caps, offering a broader explanation for the enduring popularity of economically inefficient policies. As argued by Caplan (2007), when individuals bear no responsibility for policy

[3]In 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 matched 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.

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 due to 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, political agency helps to discipline beliefs.

In section 6, we discuss how our model differentiates 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 the Online Appendix contains the proofs of the propositions presented in this section.

2.1 Setup

Consider a competitive market for a homogeneous commodity where 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, where $p = p^*$ (the competitive equilibrium price), or (2) price cap, where $p = \bar{p} < p^*$. 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 achieve in the price-cap regime relative to the free-market regime, that is,

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

Note that $\hat{\phi}$ 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 of choosing 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 preferred regime 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, who is fully responsible for regime implementation, the instrumental value of choosing a regime equals its expected surplus. By contrast, for

a Hypothetical DM, whose choice does not affect which regime is implemented, the instrumental value of choosing either regime is zero.

When making her choice of market regime, the consumer is also guided by a 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 (respectively, free-market) regime is given by $D(-\hat{\phi})$ (respectively, $D(\hat{\phi})$). 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 of 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 of 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).[4] 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, i.e., 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

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

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. Note that, 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 an instrumental value. The only reason why a Hypothetical DM would be inclined to act on $\hat{\phi}$ is to avoid cognitive dissonance.

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

[4]Such 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.

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 the consumer feels good by believing that the price cap increases consumer welfare, 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.[5]

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 that she is distorting her excess surplus expectation against 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, motivated 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 $Prob(cap|\hat{\phi})$.) Note that 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 $C(\hat{\phi}) := 1^d \, 1^{\{\phi<0, \hat{\phi}>\phi\}} \Big(Prob(cap|\hat{\phi}) - Prob(cap|\phi) \Big) |\phi| + 1^d \, 1^{\{\phi>0, \hat{\phi}<\phi\}} \Big(Prob(cap|\phi) - Prob(cap|\hat{\phi}) \Big) |\phi|$.

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 such belief into the regime decision-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:

[5] Despite being honest, ϕ may be miscalibrated due to misinformation or cognitive mistakes in information processing. For example, $\hat{P}(buy|p = \bar{p})$ may be biased upward due to a misunderstanding of the equilibrium effects of a binding price cap (Dal Bó, Dal Bó, and Eyster, 2018).

$$Max_{\hat{\phi}} \quad \left[B(\hat{\phi}) - S(\hat{\phi} - \phi) - C(\hat{\phi})\right] \tag{3}$$
$$\text{s.t.} \quad \hat{\phi} \in [-(v - p^*), v - \bar{p}].^{6}$$

Without loss of generality, we assume an interior solution to (3). The consumer will have an inclination to be unrealistically optimistic about the price-cap regime, that is, to choose $\hat{\phi} \geq \phi$ (see Lemma 1 in Section A of the Online Appendix). 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 impacts $\hat{\phi}$ 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 that 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.

Proposition 1 considers a consumer who receives new information about the price-cap regime that would lead her to become less positive 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

[6] The 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 $\hat{P}(buy|p = p^*) = 0$.

originally thought (part 1).[7] 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 impacts $\hat{\phi}_1$ 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.*

[7] A Hypothetical DM would *not* update $\hat{\phi}$ if self-deception were costless. 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.

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 the Online Appendix). Additionally, recall (from part 1 of Proposition 1) that new negative information about the price cap leads both types of DM to become less positive about this policy. If the news is so strong that an honest consumer would come to regard the price-cap regime as the worst option (i.e., $\phi_1 < 0$), a Decisive DM will 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 strong, 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 would always coincide 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 (ORSEE) (Greiner, 2004), we invited students from several universities and academic majors to attend a laboratory session at the REBEL. Invitations were sent at various times, and only a unique subset 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 atti-

[8] To 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.

tudes toward government intervention in the economy. We used these variables as controls in our empirical analyses.

A few days after answering the survey, the participants attended the laboratory session. We recruited a total of 821 individuals who participated in one of 45 study sessions, which lasted for approximately 45 minutes. In each session, the participants were divided into groups of four. They 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 develop a unique personal code which 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 contains the survey translated into English. Section F of the Online Appendix contains the experimental procedures and instructions translated into English.

3.2 The commodity market

We faced the participants with 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 with real monetary incentives. Payoffs were expressed in experimental currency units (ECU), which were converted into Colombian pesos (COP) at the rate 1 ECU = 12,000 COP (equivalent to approximately 2.5 USD at the time of the experiment). In this commodity market, there were four automated sellers. At the beginning, there was a trial block of 2 unpaid shopping periods to allow buyers to familiarize themselves with the market. The trial block was followed by 4 blocks of 8 incentivized shopping periods each. Within a block, all periods featured the same market regime—either a *free-market* or a *price-cap* regime.

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 to 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, which 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 a valuation of 11 ECU in the remaining half. The assignment kept the demand schedule constant across periods, so that in every period 2 buyers valued the product at 12 ECU and the other 2 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 depending on the market regime.

[Figure 1 about here]

In shopping periods with a free-market regime, there was a unique competitive equilibrium of 4 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 2 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%.[9]

[9]In any given period, the price cap also increased the inequality in the between-subjects distribution

3.3 Payment scheme

The participants received a participation fee of 15,000 COP (equivalent to approximately 3 USD 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. Such 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 further describe below.

[Figure 2 about here]

Randomization into information treatment arms

Before taking part in the commodity market, all participants in a given study session were randomly assigned into one of three *information* treatment arms: Experience, Factual Information, and Reverse Experience. Treatment randomization was done at the session level, rather than the group level, to facilitate logistics. 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.

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 finished, the participants were reminded of the market price (9 ECU), the number of periods in that block in which they had made a purchase (8 out of 8), and their earnings (2 ECU in 4 periods and 3 ECU in the other 4). Next, they reported their beliefs about the frequency of purchases in their group during

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 of surplus; and, as mentioned earlier, the within-subject distribution of surplus over a block of periods was identical across buyers.

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 4 periods no buyer was able to buy, in 3 periods only 1 buyer made a purchase, and in the remaining period only 2 buyers were able to buy. Note that the participants' reports indirectly convey their perceived probability distributions over group purchases in the free-market regime. In the example, the fictitious buyer believes that 50% of the time there were no purchases, 37.5% of the time only 1 buyer was able to buy, and 12.5% of the time 2 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 of 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 such bias as it may be a key determinant of consumers' support for the price-cap regime.[10] 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.

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

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.[11]

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 market regime choices both before and after the participants received information about market outcomes under the price cap.

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

[11] The 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.

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 block 2 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 (4 out of 8), and their earnings (5 ECU in 4 periods and 0 ECU in the other 4). 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 8 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 the Online Appendix 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 the Online Appendix 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 themselves as white, and 60% were enrolled at Universidad del Rosario, with the vast majority 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) in the Online Appendix presents the balance tests for the information (agency) treatment arms. The socioeconomic characteristics of the participants are broadly balanced, with a few exceptions where the differences are small and therefore may be due to sampling variability.

Figure A1 in the Online Appendix 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-leaning 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-leaning 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 a substantial level of support for these policies. Approximately 75% of the participants supported price caps on food and medicines (i.e., reported a level of 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 trend was also found among economics/finance majors and those who had completed at least one economics course.[12]

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 was a key premise of our theoretical model (see Section 2.3), we regard the study sample as suitable to test our theoretical predictions.

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 to the model:

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

(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, i.e., $\hat{P} := [\hat{P}(buy|p = p^*) - \hat{P}(buy|p = \bar{p})]$, 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 achieve 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 $\hat{P} > 0$, i.e., 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 regime choices were inconsistent with their own beliefs. These individuals reported that the price cap did not reduce the probability of buying but failed to choose the price-cap regime.[13]

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 the Online Appendix, 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.

[13]While, in principle, this behavioral pattern might be explained by risk or inequality aversion, this was not the case in this study. If anything, risk-averse or inequality-averse participants were slightly *less* likely to exhibit this pattern. We therefore regard it as a genuine inconsistency.

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 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 this regime.*

Figure A3 in the Online Appendix 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.[14] While several participants anticipated that the price cap might 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.

[14]The 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 participants in the Experience group were somewhat more negative 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 *anticipation* of personal experience with the price-cap regime, as opposed to simply imagining a hypothetical price-cap regime, 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 aimed to investigate the effects of actually experiencing a price cap.

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 this regime.*

Figure A4 in the Online Appendix 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.

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 the Online Appendix, 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:

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

The dependent variable, Y_{it}, equals 1 if subject i believed in period t that the price cap reduced the probability of buying relative to the free market (respectively, if subject i chose

to impose the price cap in period t) and 0 otherwise, where $t = 0, 1$. 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. Standard errors were clustered by experimental session. 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.

The results on beliefs are presented in column 1 of Table 1. In line with Figure 5, after 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 the 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 chose to 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 re-estimated the specification from columns 1 and 3 of Table 1 using the continuous excess–probability measure as the dependent variable. Table A5 in the Online Appendix 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 a level of support for the price cap of 62% at the endline in the Factual Information

group. Again, 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 for this policy 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 the price-cap regime. As shown in column 4 of Table 1, again 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 the consequences of this policy.

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 the Online Appendix, 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 re-estimated 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 the Online Appendix 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 turn to investigating 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 in regime implementation should be most influential among participants who were initially positive about the price cap, hindering the potential of the information intervention to change beliefs.

Result 5 *Following the information intervention, both Hypothetical DMs and Decisive DMs became more negative about the price cap. However, among participants who initially believed that the price cap does not reduce the probability of buying, Decisive DMs became significantly more negative 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 susceptible to be affected by the information intervention. Indeed, approximately 89% of these participants maintained their negative views.[15]

[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 were initially positive about the price cap believed at the endline that it reduces the probability of buying, this percentage rises to 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 were initially positive about 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:

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

[15]Strictly speaking, most of these participants became even more negative 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).

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 (respectively, 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, political tendency, and attitudes toward government intervention in markets as described earlier in Section 4.[16] Standard errors were clustered by experimental session. 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.

Table 2 presents the results on posterior beliefs. Column 1 shows the results using the restricted sample and excluding controls. Among participants who were positive about 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]

Table 3 presents the results on final regime choices. Column 1 shows that among participants who were initially positive about the price cap, Decisive DMs were 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 were

[16] To reduce the number of covariates, we built one index that summarizes the variables measuring political tendency and another index that summarizes the variables measuring attitudes toward government intervention. Both indexes were constructed following the Inverse Covariance Weighted Index proposed by Anderson (2008).

already negative 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 due to 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 in the consequences of her regime choice on 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:

$$1_i^{PC_1=1} = \lambda_0 + \lambda_1 1_i^{\hat{P}_1>0} + \lambda_2 1_i^d + \lambda_3 \left(1_i^{\hat{P}_1>0} \times 1_i^d\right) + X_i' \lambda_4 + \epsilon_i. \quad (6)$$

The dependent variable, $1_i^{PC_1=1}$, 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^d and X_i are defined as in model (5). Standard errors were clustered by experimental session. 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.

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 such endogeneity of posterior beliefs 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 finally agreed that the price cap does (not) reduce the probability of buying, Decisive DMs could have been more negative (positive) 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 A7 in the Online Appendix, 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 where 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. However, 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 upwards, 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 original model did not account for self-deception costs. (The original model did not explicitly incorporate dissonance costs either, but Caplan's arguments hinted 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—became more negative 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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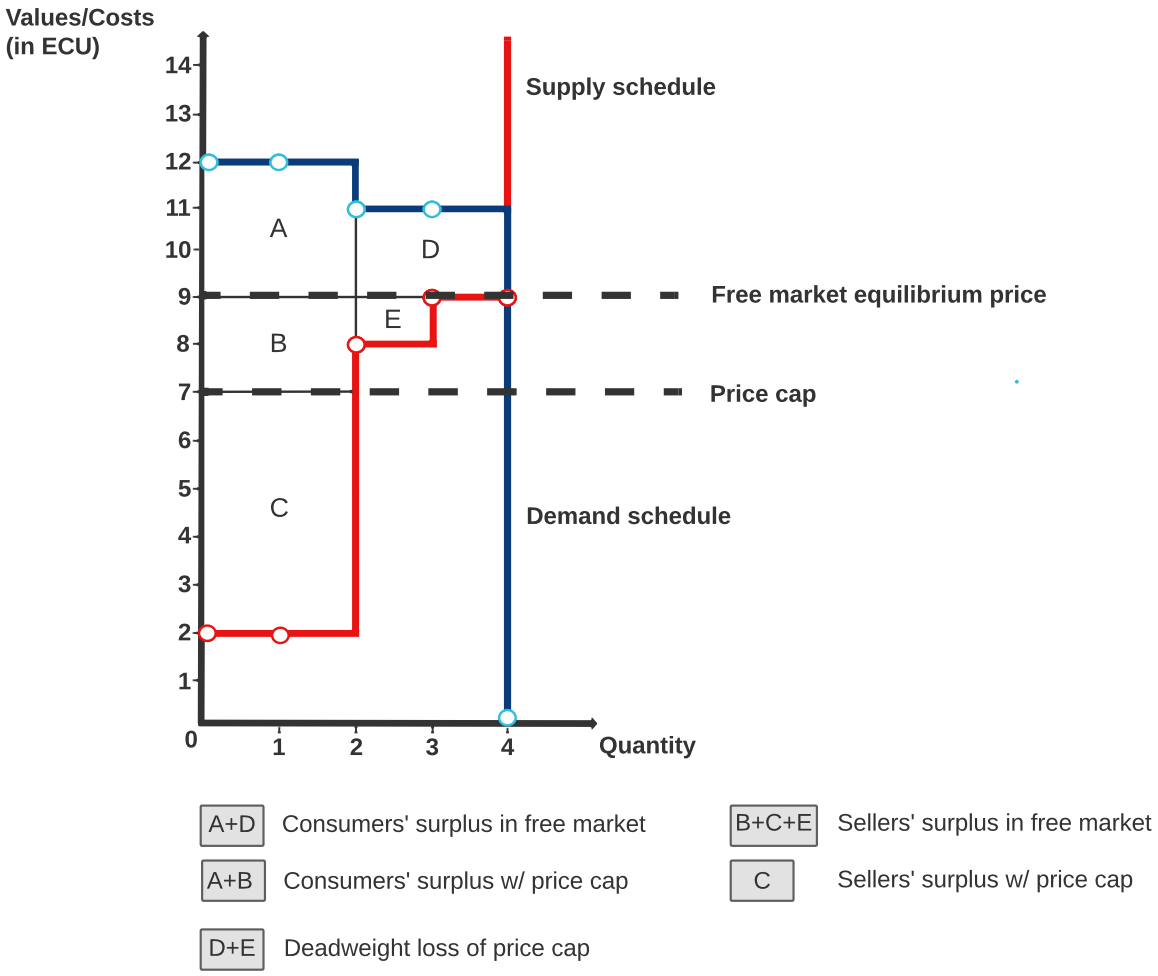
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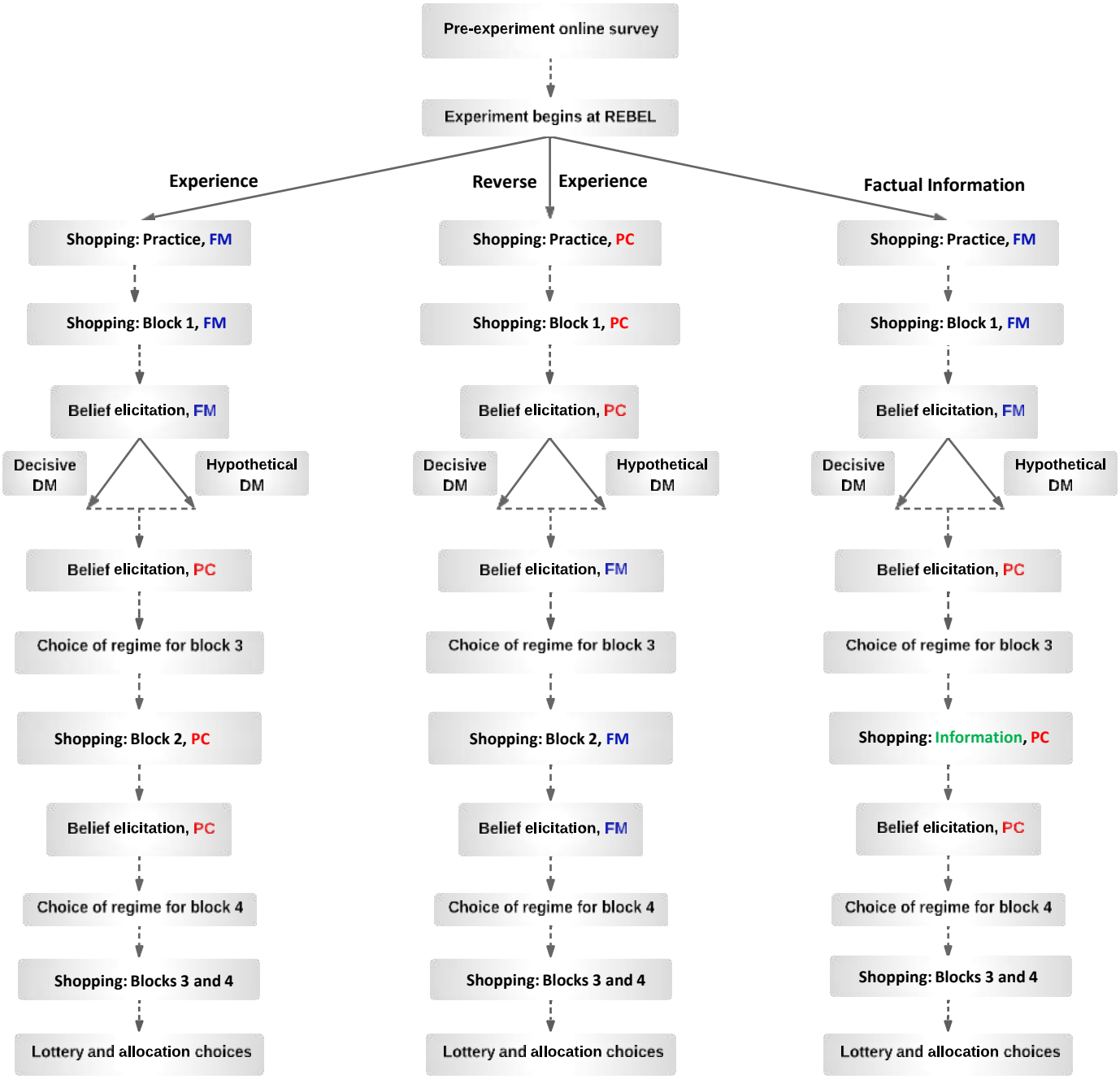
Figures

Figure 1: The experimental commodity market



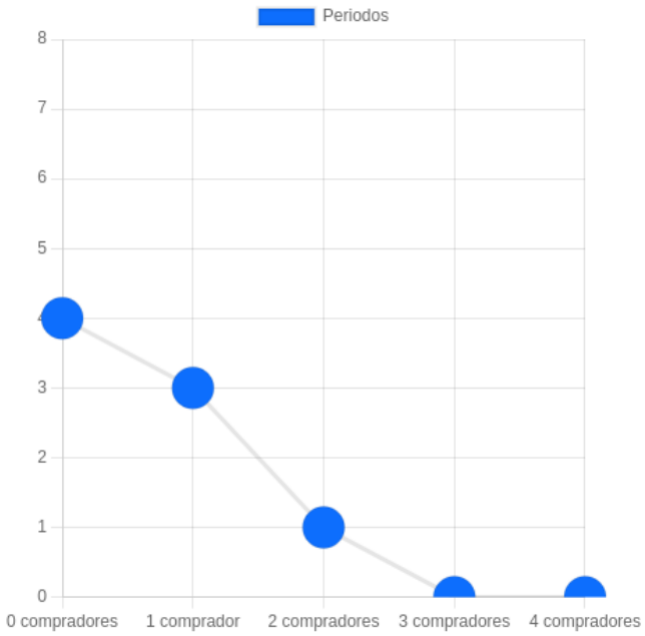
Notes: This figure illustrates the market environment used in the experiment, including buyers, sellers, and the market equilibrium.

Figure 2: Timeline of a laboratory session



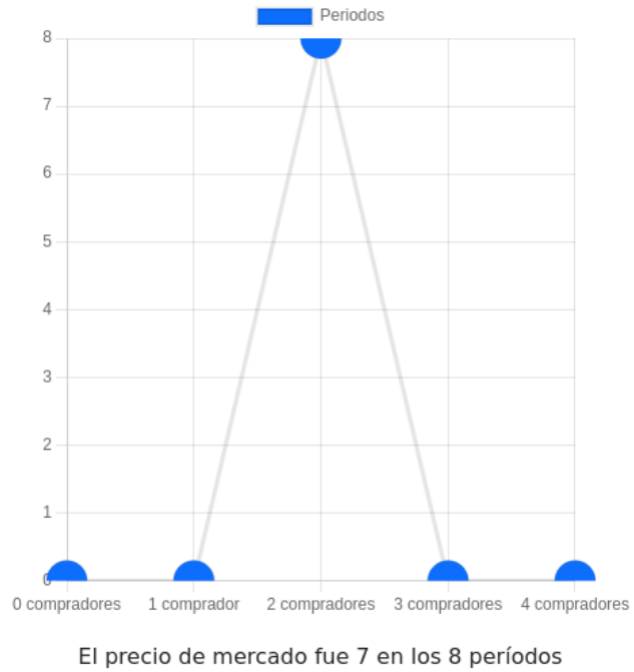
Notes: "FM" ("PC") indicates a free-market (price-cap) regime. In one-half of the sessions of each information treatment arm, the participants made the lottery and allocation choices before the commodity market was introduced.

Figure 3: Graphical interface used for belief elicitation



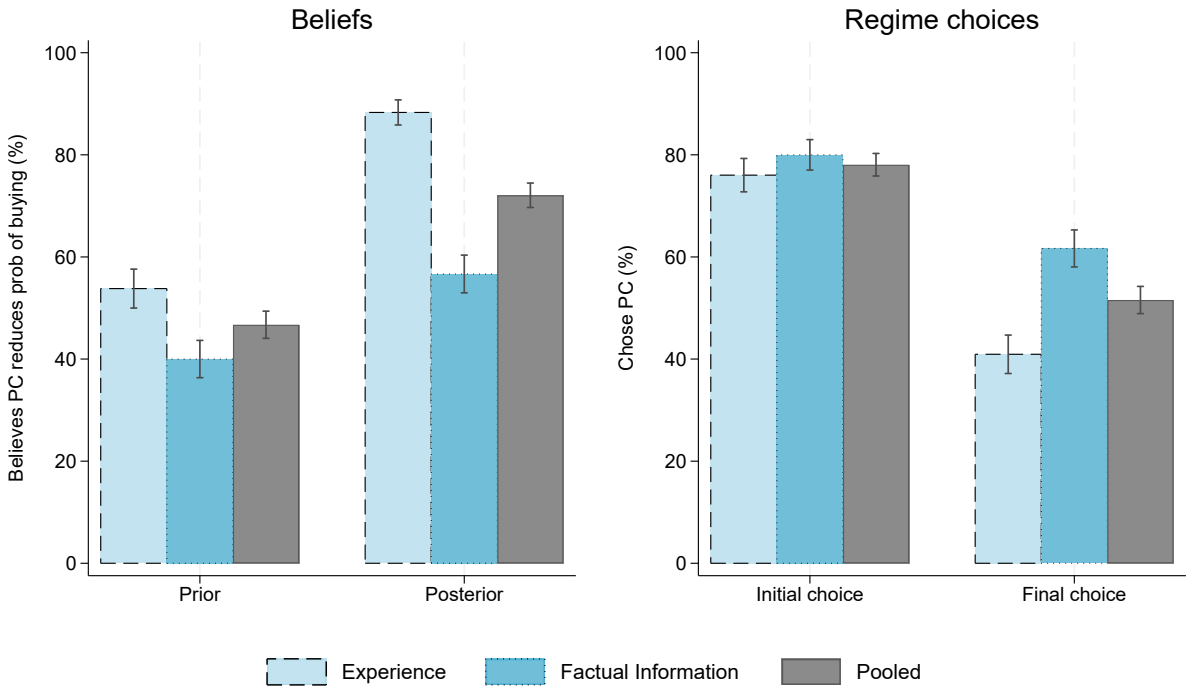
Notes: This figure illustrates the graphical interface that participants used to report their beliefs about group purchases during the experiment.

Figure 4: Information intervention in the Factual Information treatment arm



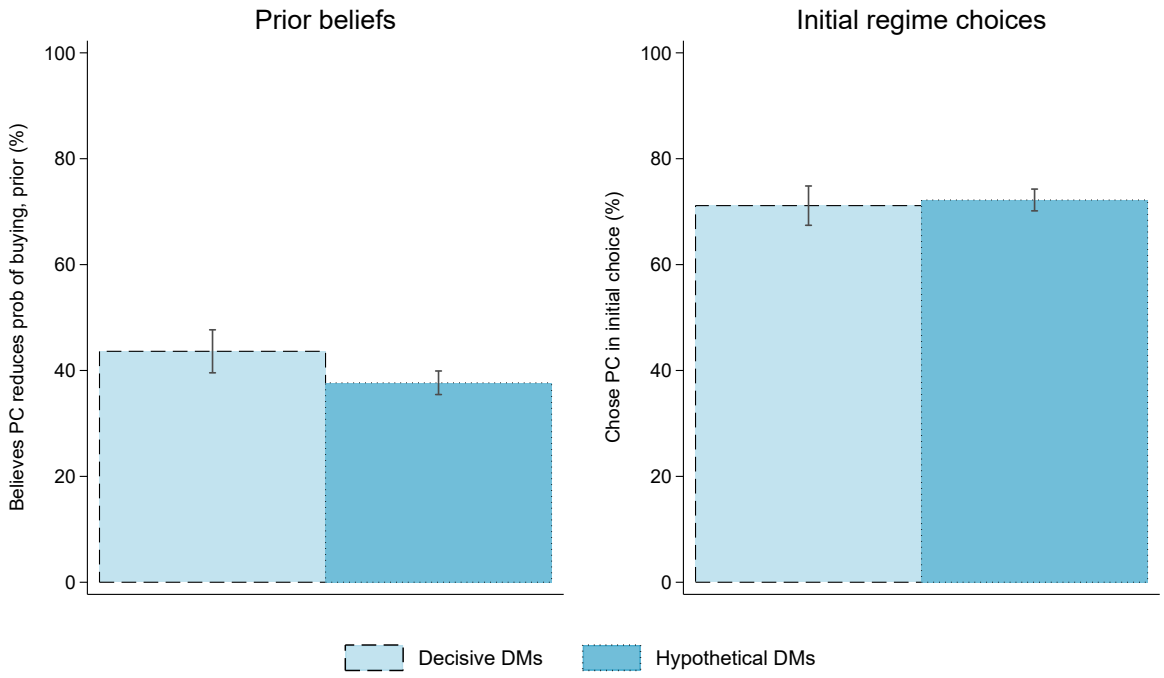
Notes: This figure displays the graphical interface used to provide factual information about the price cap in the Factual Information treatment arm. It shows the expected frequency of total purchases under the price cap.

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



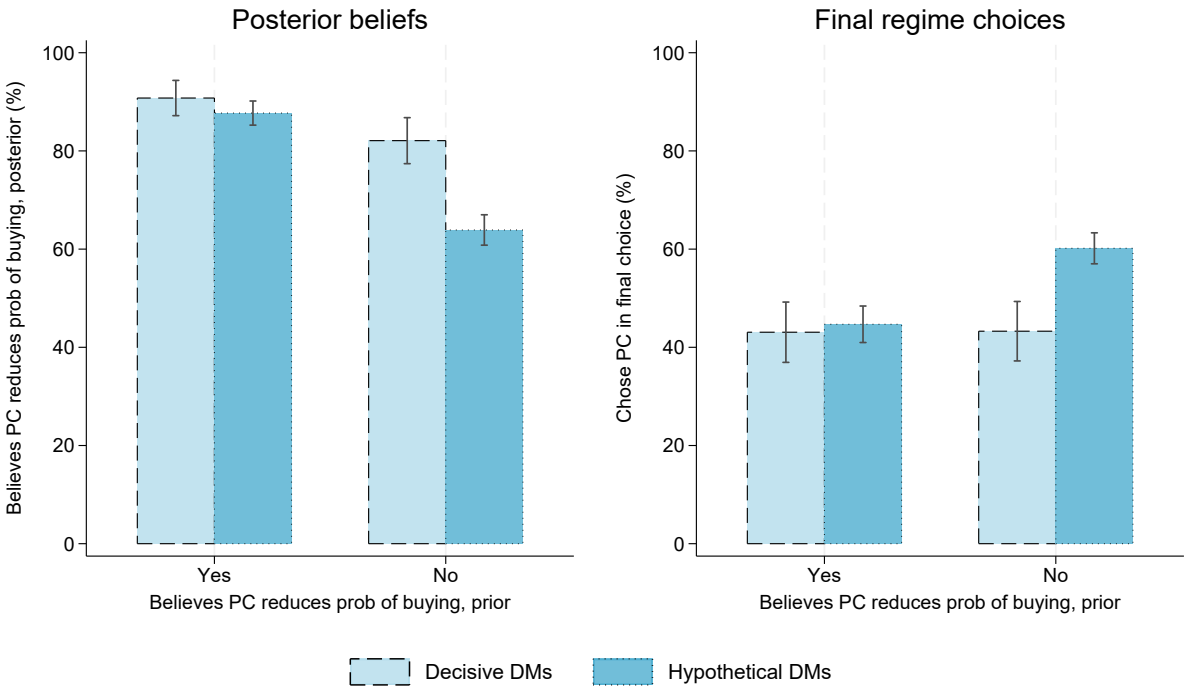
Notes: This figure summarizes beliefs and regime choices in the Experience and Factual Information treatment arms using the subsample of participants with consistent beliefs and choices. The left panel shows the percentage of participants who believe that the price cap reduces the probability of buying 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



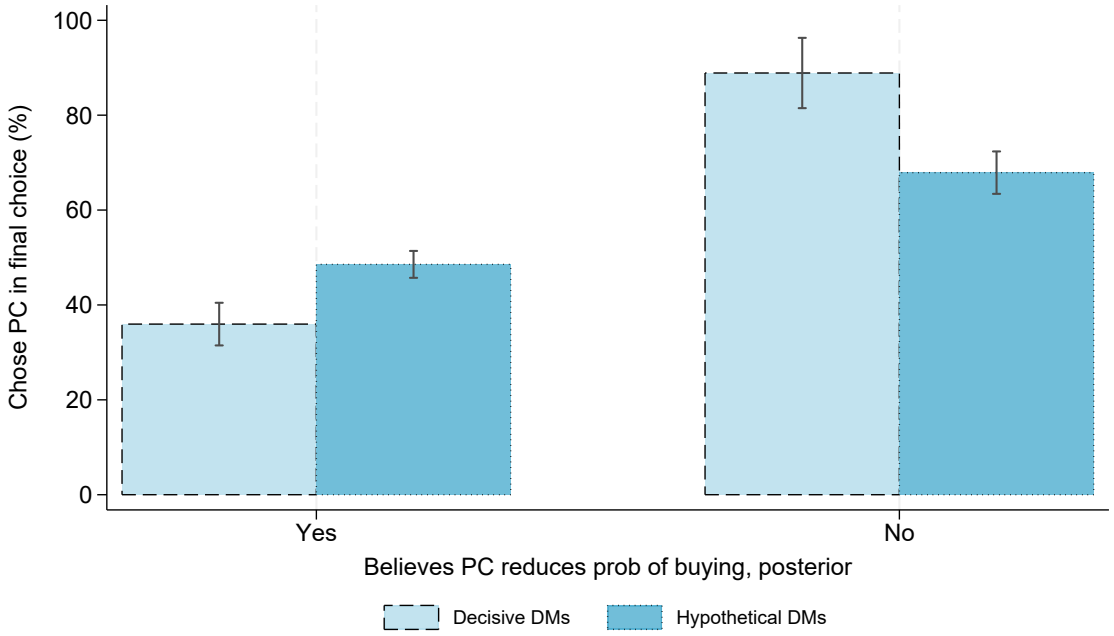
Notes: This figure compares prior beliefs and initial regime choices between Decisive DMs and Hypothetical DMs using the subsample of participants with consistent beliefs. The left panel shows the percentage of participants who believe that the price cap reduces the probability of buying. The right panel 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***	–0.183***	0.174***	–0.108***
	(0.042)	(0.031)	(0.041)	(0.031)
Experience (=1)	0.138***	–0.040	0.073*	–0.051
	(0.049)	(0.050)	(0.042)	(0.049)
Post×Experience	0.178***	–0.168***	0.137**	–0.142***
	(0.054)	(0.047)	(0.052)	(0.051)
Constant	0.400***	0.800***	0.355***	0.699***
	(0.038)	(0.032)	(0.036)	(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 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***	0.243***	0.271***	0.285***
	(0.047)	(0.050)	(0.045)	(0.047)
Decisive (= 1)	0.182**	0.206***	0.138**	0.146***
	(0.058)	(0.057)	(0.049)	(0.049)
PC reduces prob of buying (prior)×Decisive	–0.151*	–0.183**	–0.096	–0.108
	(0.080)	(0.075)	(0.068)	(0.071)
Constant	0.639***	0.709***	0.553***	0.663***
	(0.044)	(0.162)	(0.034)	(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 choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, one index that summarizes the variables measuring political tendency, and another index that summarizes the variables measuring 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 $\theta_2 + \theta_3 = 0$ in model (5) in the main text.

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**	–0.145**	–0.069	–0.070
	(0.058)	(0.060)	(0.048)	(0.050)
Decisive	–0.169**	–0.176**	–0.061	–0.053
	(0.079)	(0.076)	(0.061)	(0.062)
PC reduces prob of buying (prior)×Decisive	0.153	0.166	0.037	0.035
	(0.126)	(0.123)	(0.087)	(0.089)
Constant	0.602***	0.068	0.533***	0.070
	(0.039)	(0.193)	(0.032)	(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 choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, one index that summarizes the variables measuring political tendency, and another index that summarizes the variables measuring 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***	–0.147***	–0.113***	–0.083**
	(0.051)	(0.050)	(0.040)	(0.037)
Decisive (=1)	0.210***	0.213***	0.133*	0.146*
	(0.072)	(0.077)	(0.069)	(0.071)
PC reduces prob of buying (post)×Decisive	–0.336***	–0.342***	–0.224***	–0.233***
	(0.079)	(0.082)	(0.071)	(0.072)
Constant	0.679***	0.160	0.581***	0.131
	(0.047)	(0.209)	(0.036)	(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 choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, one index that summarizes the variables measuring political tendency, and another index that summarizes the variables measuring 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.

For Online Publication Only

Appendix 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 $C(\hat{\phi}) = 1^d \left(Prob(cap|\phi) - Prob(cap|\hat{\phi}) \right) |\phi|$. The marginal utility of a change in the excess surplus expectation is

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

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 $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ (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

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

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

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

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

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

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

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

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

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

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

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

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 $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ (see expression (2) in the main text); and (3) the fact that $|\tilde{\tilde{\phi}}_1| > |\tilde{\phi}_1|$ if $\tilde{\tilde{\phi}}_1 < 0$ and $\tilde{\phi}_1 < 0$.

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

Final regime choice

Combining the above result that $\hat{\phi}_1$ is increasing in ϕ_1 with the result that $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ ((see expression (2) in the main text), we obtain that $Prob_1(cap|\phi_1 = \tilde{\tilde{\phi}}_1) < Prob_1(cap|\phi_1 = \tilde{\phi}_1)$.

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 $\left[B(\hat{\phi}_0) - S(\hat{\phi}_0 - \phi_0) \right]$, 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 $Prob_1^d(cap)$ (respectively, $Prob_1^h(cap)$) 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

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

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

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

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

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

where the inequality follows from the result that $\frac{\partial Prob(cap)}{\partial \hat{\phi}} > 0$ (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

$$\begin{aligned} Prob_1^d(cap) &= \frac{1}{1 + exp\{|\hat{\phi}_1^d| + D(|\hat{\phi}_1^d|)\}} < \frac{1}{1 + exp\{D(|\hat{\phi}_1^d|)\}} \\ &< \frac{1}{1 + exp\{-D(\hat{\phi}_1^h) + D(-\hat{\phi}_1^h)\}} = Prob_1^h(cap), \end{aligned}$$

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.

[1] If 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

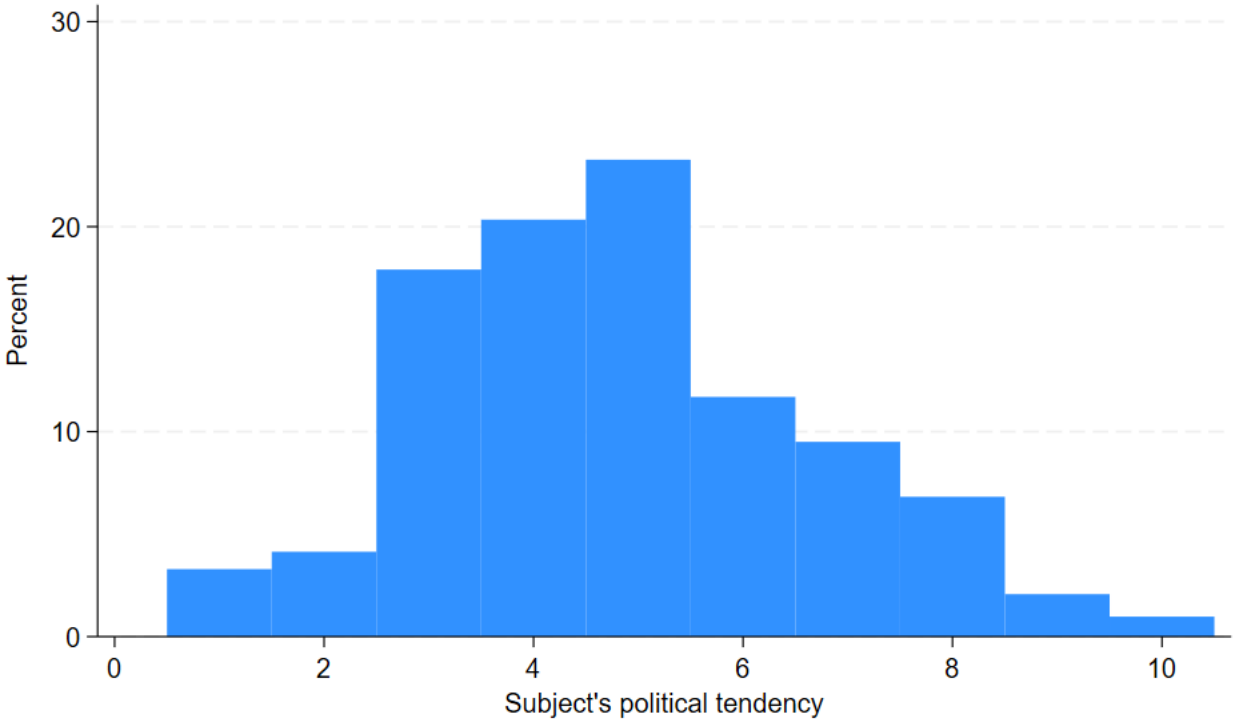
25. Regardless of which political party is in charge, the Colombian State must regulate the interest rate to ensure that credit is cheap.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

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.

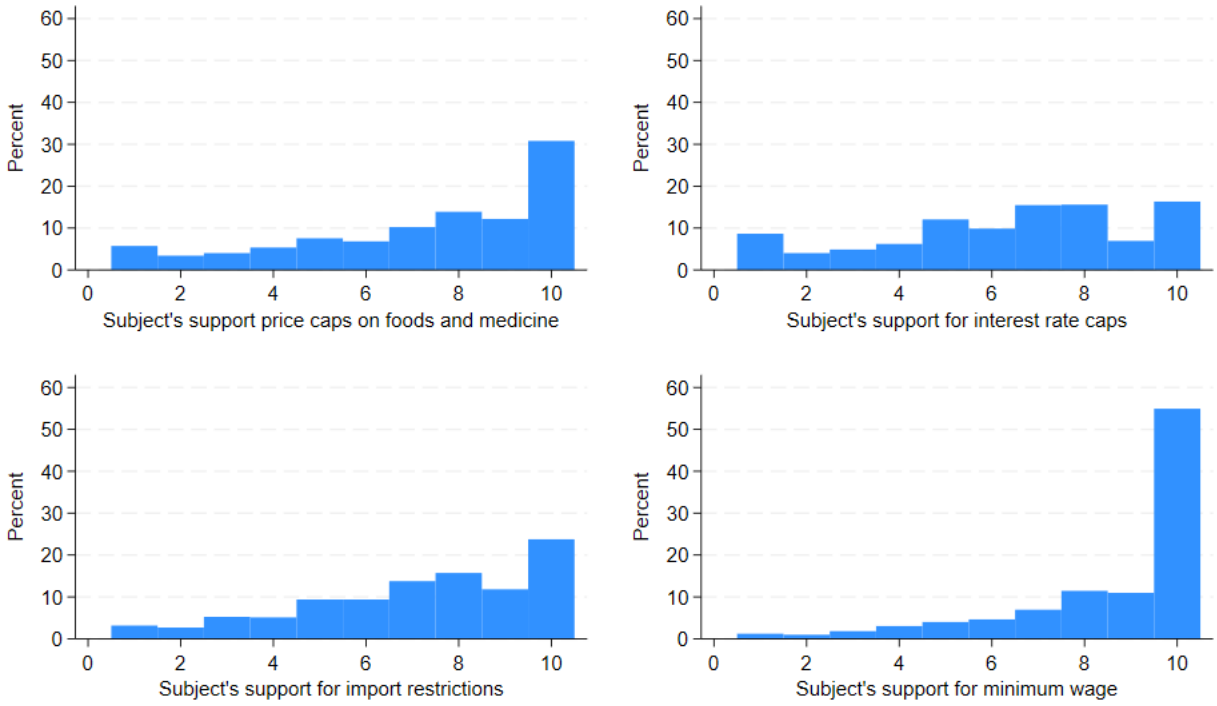
D Additional figures

Figure A1: Distribution of self-reported political tendency



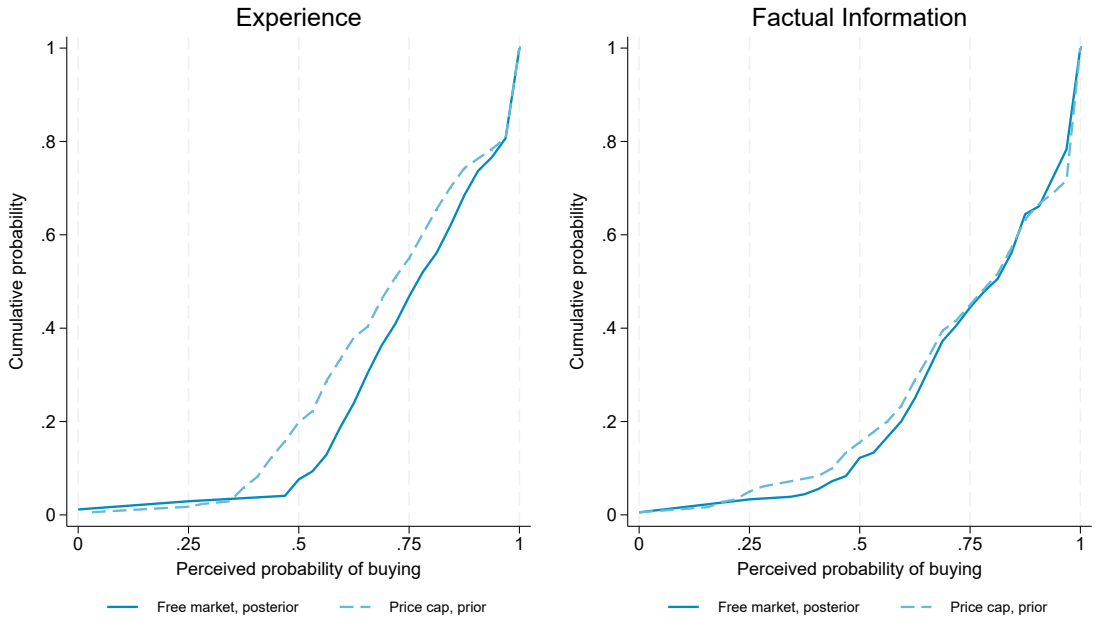
Notes: Self-reported political tendency ranges from 1 (left-leaning) to 10 (right-leaning).

Figure A2: Distribution of support for interventionist policies



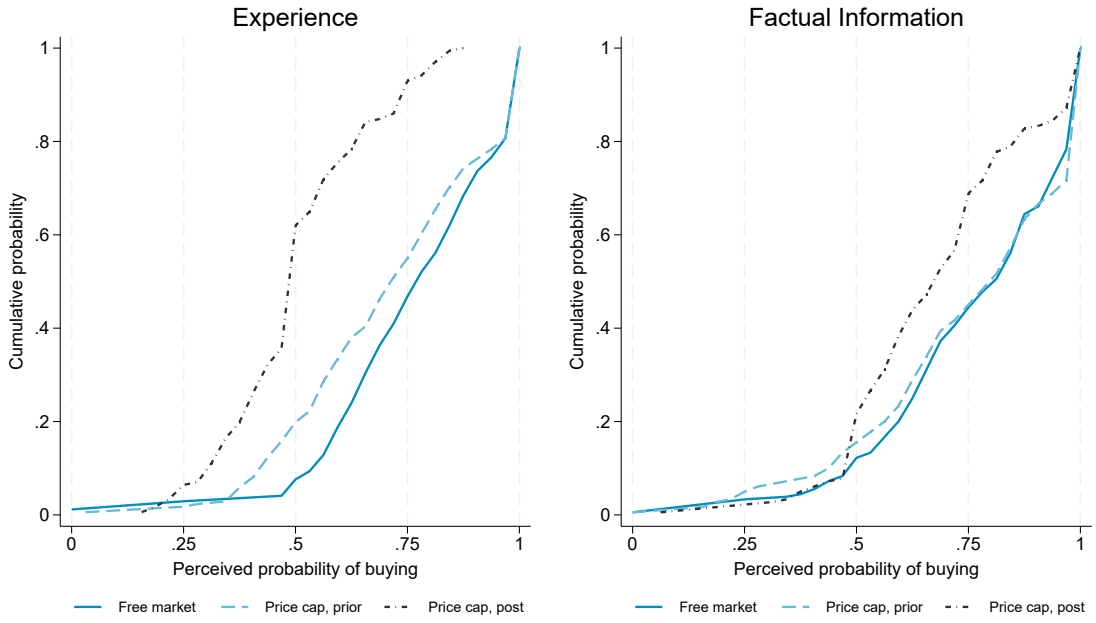
Notes: This figure displays the distribution of participants' support for four interventionist policies: (i) price caps on foods and medicines, (ii) interest rate caps, (iii) import restrictions, and (iv) a minimum wage. Each variable is measured on a 1–10 scale, where higher values indicate stronger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

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



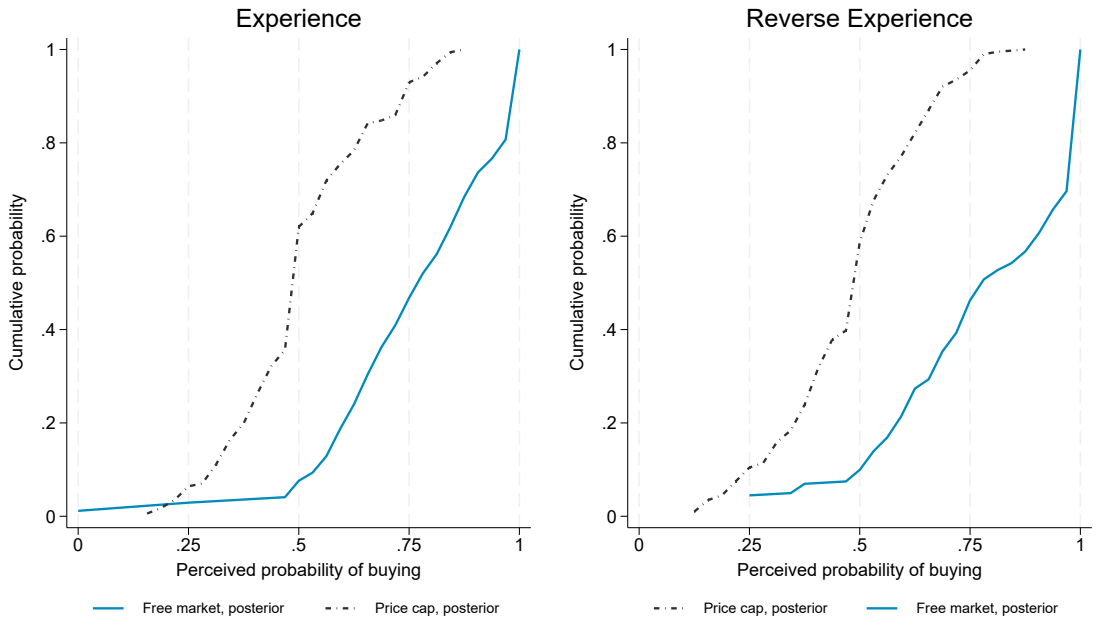
Notes: This figure shows the cumulative distribution of prior beliefs about the probability of buying in the price-cap regime for participants in the Experience and Factual Information treatment arms, using the subsample of participants with consistent beliefs and choices.

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

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

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 the Online Appendix 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 regime choices

Variable	**Beliefs**		**Choices**		**Beliefs and choices**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Experience (=1)	–0.065	–0.058	0.026	0.027	–0.027	–0.018
	(0.044)	(0.045)	(0.032)	(0.032)	(0.046)	(0.047)
Factual Information (=1)	–0.035	–0.033	–0.025	–0.026	0.020	0.023
	(0.034)	(0.035)	(0.025)	(0.026)	(0.036)	(0.039)
Decisive (=1)	–0.044	–0.047	0.002	–0.003	–0.038	–0.039
	(0.032)	(0.033)	(0.029)	(0.030)	(0.035)	(0.036)
Constant	0.803***	1.105***	0.856***	0.624***	0.684***	0.816***
	(0.026)	(0.034)	(0.021)	(0.070)	(0.029)	(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 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, one index that summarizes the variables measuring political tendency, and another index that summarizes the variables measuring 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***	0.066***
	(0.014)	(0.012)
Experience (=1)	0.036	0.026
	(0.022)	(0.018)
Post × Experience	0.135***	0.098***
	(0.028)	(0.024)
Constant	0.015	-0.018
	(0.016)	(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 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***	–0.368***	0.423***	–0.242***
	(0.046)	(0.039)	(0.046)	(0.028)
Experience (=1)	0.140***	–0.110**	0.096**	–0.094**
	(0.044)	(0.047)	(0.036)	(0.039)
Post×Experience	–0.113*	0.017	–0.112*	–0.008
	(0.056)	(0.053)	(0.056)	(0.050)
Constant	0.398***	0.871***	0.332***	0.742***
	(0.032)	(0.027)	(0.030)	(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 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: 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***	0.400***	0.422***	0.409***
	(0.019)	(0.023)	(0.015)	(0.016)
Decisive (=1)	-0.025	-0.020	-0.052	-0.048
	(0.036)	(0.031)	(0.033)	(0.031)
PC reduces prob of buying (post)×Decisive	0.033	0.031	0.038	0.035
	(0.040)	(0.037)	(0.038)	(0.036)
Constant	-0.115***	-0.042	-0.130***	-0.029
	(0.012)	(0.074)	(0.008)	(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 choices. Control variables in even-numbered columns include dummy variables for the information treatment arms, risk and distributional preferences, socioeconomic characteristics, one index that summarizes the variables measuring political tendency, and another index that summarizes the variables measuring 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 90 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 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:



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.]

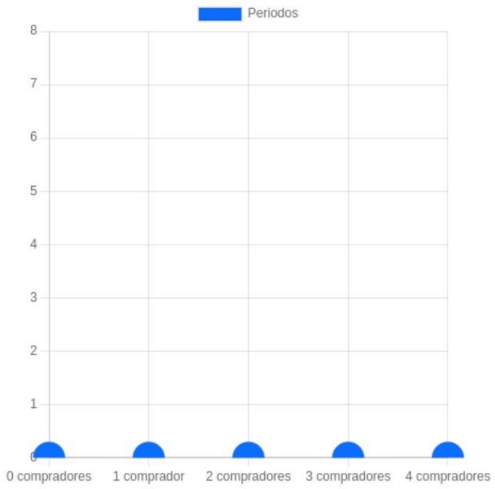
[*The participants complete block 1 of the shopping exercise.*]

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.

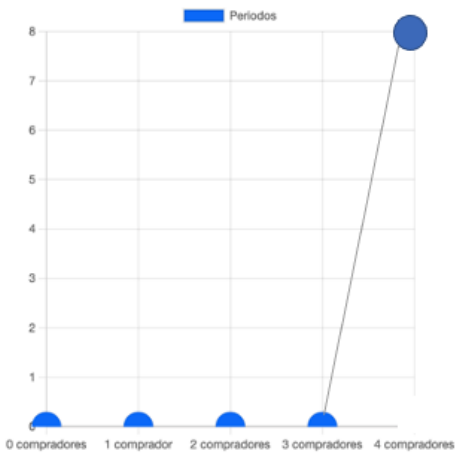
Please indicate how many periods you believe the number of buyers who were able to make a purchase was 0, how many periods it was 1, how many it was 2, how many it was 3, and how many it was 4.

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

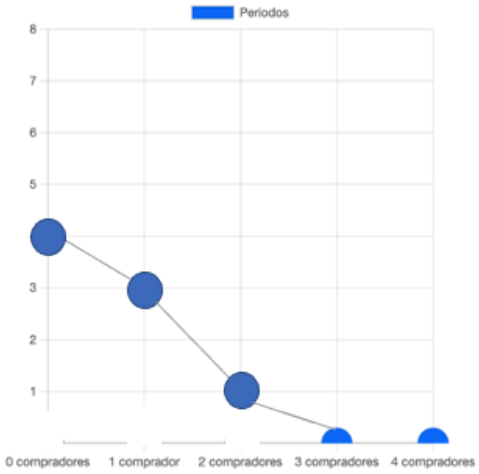


In this figure, there is a grid with five blue circles at the bottom. You now see the bottom half of each circle, but once you select a circle and move it upwards, you will be able to see it in full. To move a circle, simply place the mouse cursor over the circle and move it upwards. You can release 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.]

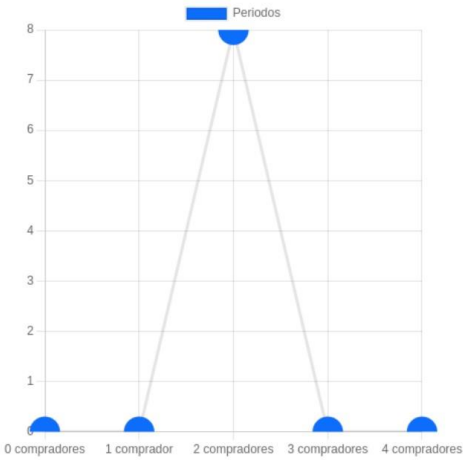
Please answer the following question:

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:]



El precio de mercado fue 7 en los 8 períodos

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*.]