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

Original Pre-Analysis Plan and Implementation Report

Stanislao Maldonado[1] and Santiago I. Sautua[2]

Tecnologico de Monterrey

May 2026

[1]Email: stanislao.maldonado@tec.mx
[2]Email: santiago.sautua@tec.mx

Abstract

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

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

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

Outline

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

Implementation-report tables

Table B1. Summary of PAP alignment and implementation clarifications

Table B2. Outcome-construction crosswalk for PAP analyses

Table B3. Pre-exposure belief comparisons

Table B4. Pre-exposure between-arm prior comparison

Table B5. Direct exposure effects on beliefs

Table B6. Direct exposure effects on regime choices

Table B7. Order effects

Table B8. Factual information effects on beliefs

Table B9. Factual information effects on regime choices

Table B10. Direct exposure versus factual information

Table B11. Agency effects on beliefs

Table B12. Agency effects on regime choices

Table B13. DRETS and regime choices

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

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

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

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

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

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

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

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

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

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

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

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

Table B26. PAP-specified regression and LPM specifications

Table B27. PAP Section 6.2 survey index variables

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

Table B29. Precision diagnostics for selected heterogeneity analyses

Part I. Original pre-analysis plan

Voters' Demand for Price Caps
Pre-Analysis Plan[‡]

Stanislao Maldonado[§]
Tecnologico de Monterrey

Santiago Sautua
Tecnologico de Monterrey

This version: February 22, 2023
For the latest version of this document, click here.

Abstract

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

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

JEL Classification Numbers: C93, D73, O15.

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

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

1 Introduction

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

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

2 Research questions

This study will address the following questions:

- Do individuals who lack specialized knowledge in economics tend to support price caps when they have little experience with such policy? Do they anticipate the consequences of price caps in competitive markets?

- In practice, a typical citizen is never in charge of implementing a price cap, and hence is not responsible for its consequences. Does this lack of agency increase support for price caps?

- Does direct exposure to a price cap lead to more negative expectations and reduce support for this policy?

- Does information about market outcomes under a price cap regime lead to more negative expectations and reduce support for this policy, even in the absence of personal experience? How does such information compare with direct exposure to a price cap?

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

3 Experiment

3.1 General aspects

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

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

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

[Figure 1 about here]

3.2 Market Task

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

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

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

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

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

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

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

[Table 1 about here]

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

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

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

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

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

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

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

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

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

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

3.3 Belief Elicitation Task

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

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

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

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

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

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

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

3.4 Distribution and Risk Equivalent Tasks (DRETS)

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

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

[Table 2 about here]

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

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

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

3.5 Payment

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

4 Main hypotheses

We next summarize our main hypotheses:[5]

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

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

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

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

5 Empirical analysis

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

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

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

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

5.1 Pre-exposure beliefs about price caps

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

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

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

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

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

$$Prior_{ig} = \beta_0 + \beta_1 \, FME_{ig} + \epsilon_{ig}. \tag{2}$$

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

5.2 Effects of direct exposure to a price cap

5.2.1 Effect on beliefs about the quantity traded

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

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

The alternative hypothesis is

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

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

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

The alternative hypothesis is

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

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

5.2.2 Effect on choice of regime

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

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

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

The alternative hypothesis is

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

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

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

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

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

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

The alternative hypothesis is

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

5.2.3 Order effects

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

$$Cap_{ig}^{final} = \beta_0 + \beta_1 \, FME_{ig} + \epsilon_{ig}^{final}. \tag{12}$$

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

5.3 Effects of external information about price caps

5.3.1 Effect on beliefs about the quantity traded

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

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

The alternative hypothesis is

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

5.3.2 Effect on choice of regime

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

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

The alternative hypothesis is

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

5.3.3 External information versus direct exposure

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

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

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

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

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

5.4 Decisive versus hypothetical choosers

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

5.4.1 Effect of pivotality on beliefs about the quantity traded

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

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

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

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

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

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

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

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

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

5.4.2 Effect of pivotality on regime choices

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

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

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

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

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

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

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

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

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

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

5.5 Association between choices made in DRETS and regime choices

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

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

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

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

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

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

6 Pre-experiment survey

6.1 Questionnaire

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

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

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

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

6.2 Indexes

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

- If necessary, reorient some of the K outcomes so that beneficial effects are consistently scored higher. As a general rule, we will orient the outcomes in a way that higher scores are related with more pro-left views.

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

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

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

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

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

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

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

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

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

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

7 Inference issues

7.1 Adjusting for multiple comparisons

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

7.2 Power

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

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

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

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

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

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

References

Anderson, M. 2008. "Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training projects." *Journal of the American Statistical Association*, 103(484): 1481-1495.

Benjamini, Y., and Hochberg, Y. 1995. "Controlling the false discovery rate: A practical and powerful approach to multiple testing." *Journal of the Royal Statistical Society*, 57(1): 289-300.

Brennan, G. 2008. "Lessons for ethics from economics?" *Interdisciplary Core Philosophy*, 18: 249-271.

Brennan, G., and Hamlin, A. 1998. "Expressive voting and electoral equilibrium." *Public Choice*, 95(1/2): 149-175.

Brennan G., and Lomasky, L. 1993. *Democracy and decision: The pure theory of electoral preference.* Cambridge University Press.

Caplan, B. 2000. "Rational irrationality: A framework for the neoclassical-behavioral debate." *Eastern Economic Journal*, 26(2): 191-211.

Caplan, B. 2001. "Rational ignorance versus rational irrationality." *Kyklos*, 54(1): 3-26.

Caplan, B. 2007. *The myth of the rational voter: Why democracies choose bad policies.* Princeton University Press.

Cohen, J. 1988. *Statistical power analysis for the behavioral sciences.* Lawrence Erlbaum Associates.

Dal Bó, E., Dal Bó, P., and Eyster, E. 2018. "The demand for bad policy when voters underappreciate equilibrium effects." *Review of Economic Studies*, 85(2): 964-998.

Greiner, B. 2004. "The Online Recruitment System ORSEE 2.0 - A guide for the organization of experiments in economics." Working Paper Series in Economics 10, University of Cologne, Department of Economics.

Hamlin, A., and Jennings, C. 2011. "Expressive political behaviour: Foundations, scope and implications." *British Journal of Political Science*, 41: 645-670.

Kling, M. 2004. "Moving to opportunity and tranquility: Neighborhood effects on adult economic self-sufficiency and health from a randomized housing voucher experiment." KSG Working Paper No. RWP04-035.

Lehmann, E. 2006. *Nonparametrics: Statistical methods based on ranks.* Springer Publishing.

Schuessler, A. 2000. "Expressive voting." *Rationality and Society*, 12(1).

Wiesehomeier, N. 2012. "Attitudes, ideological associations and the left–right divide in Latin America. " *Journal of Politics in Latin America*, 4(1): 3–33.

Figure 1: Timeline of an experimental session by treatment arm

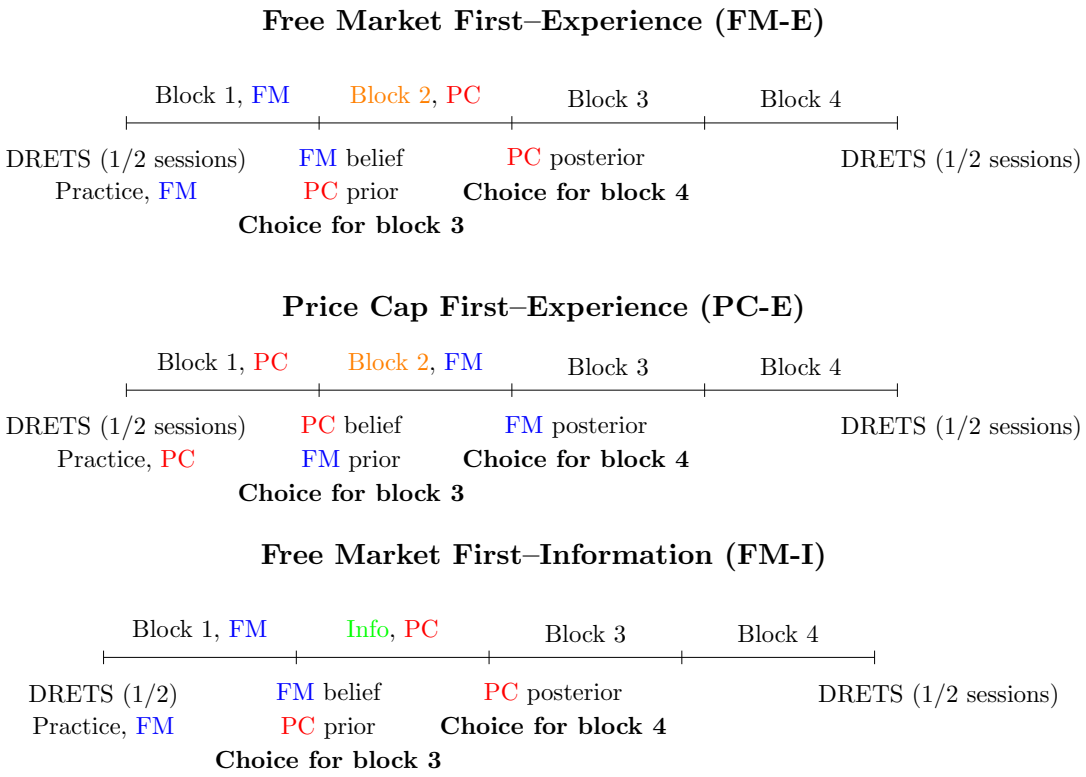


Table 1: Market Task

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

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

Table 2: Distribution and Risk Equivalent Tasks (DRETS)

Panel A: Lottery choice task

	Low-risk lottery		High-risk lottery	
Outcome	3	2	5	0
Probability	0.5	0.5	0.5	0.5
Expected value	2		2	
Variance	250		6,250	

Panel B: Distribution task

Subject	Low-inequality allocation	High-inequality allocation
1	3	5
2	3	5
3	2	0
4	2	0
Sum of payoffs	10	10

Note: All payoffs are in ECU.

APPENDIX: Pre-experiment Survey

Section I

1. What is your gender?
   Male — Female — Other

2. How old are you?
   (Record your age in fulfilled years.)

3. Where were you born?
   (Enter the city and country.)

4. What university do you study at?

5. What degree(s) do you study at university?

6. What semester(s) of your degree(s) are you currently in?

7. Have you taken economics courses?
   Yes — No

8. What is your stratum?
   (1 is the poorest stratum, 6 the richest stratum)
   1 — 2 — 3 — 4 — 5 — 6

9. Do you consider yourself a white, half-caste, indigenous, black, mulatto or other person?
   White — Half-caste — Indigenous — Black — Mulatto — Other

10. Consider a scale of 1 to 10 that goes from left to right, in which 1 means "left" and 10 means "right." Nowadays, when talking about political tendencies, many people talk about those who are more sympathetic to the left or the right.

    According to the meaning that the terms "left" and "right" have to you, when you think about your political point of view, where are you on this scale?
    Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

11. Where would you place the Historical Pact?
    Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

12. Where would you place the Democratic Center?
    Left 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Right

13. Thinking of political parties or coalitions in Colombia, how close politically do you feel to:
    Liga de Gobernantes Anticorrupción
    Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

    Colombia Justa Libres

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

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

Movimiento de Salvación Nacional
Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

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

G.S.C. Colombia Piensa en Grande
Not close 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Very close

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

14. In the first round of the last presidential election you voted for:
    - Liga de Gobernantes Anticorrupción
    - Colombia Justa Libres
    - Coalición Centro Esperanza
    - Movimiento de Salvación Nacional
    - Pacto Histórico
    - G.S.C. Colombia Piensa en Grande
    - Verde Oxígeno
    - Did not vote
15. Regardless of whether you voted in the first round of the last presidential election, which candidate did you support?
    - Rodolfo Hernández (Liga de Gobernantes Anticorrupción)
    - John Milton Rodríguez (Colombia Justa Libres)
    - Federico Gutiérrez (Equipo por Colombia)
    - Enrique Gómez Martínez (Movimiento de Salvación Nacional)
    - Gustavo Petro (Pacto Histórico)
    - Luis Pérez (G.S.C. Colombia Piensa en Grande)
    - Ingrid Betancourt (Verde Oxígeno)
    - Sergio Fajardo (Coalición Centro Esperanza)
    - None of the above
16. In the second round of the last presidential election you voted for:
    - Liga de Gobernantes Anticorrupción
    - Pacto Histórico
    - Did not vote

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

Section II

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

19. Regardless of which political party is in charge, the Colombian State, instead of the private sector, should be the owner of the country's most important companies and industries.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

20. Regardless of which political party is in charge, the Colombian State must implement firm policies to reduce income inequality between rich and poor.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

21. Regardless of which political party is in charge, the Colombian State must spend more on helping the poor.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

22. It is unfair that the rich pay a lot in taxes but receive little in state services.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

23. Regardless of which political party is in charge, the Colombian State must regulate the prices of essential goods such as food and medicines.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

24. Regardless of which political party is in charge, the Colombian State must guarantee a minimum wage to cover the basic family basket.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

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

26. Regardless of which political party is in charge, the Colombian State must regulate imports to protect Colombian producers and consumers.
    Strongly disagree 1 — 2 — 3 — 4 — 5 — 6 — 7 — 8 — 9 — 10 Strongly agree

27. Some people consider that the State tries to do many things that should be left to individuals and businesses. Others believe that the State should do more. What stance comes closest to your position?
    The State does too much — The State does the right thing — The State should do more.

Part II. PAP implementation, clarifications, and deviations

1. Overview and scope of this implementation report

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

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

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

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

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

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

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

2. Summary of alignment with the PAP

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

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

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

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

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

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

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

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

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

3. Implementation clarifications

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

5. PAP Section 5.2: Effects of direct exposure

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

7. PAP Section 5.3: Factual information

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

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

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

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

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

8. PAP Section 5.3.3: Direct exposure versus factual information

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

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

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

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

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

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

9. PAP Section 5.4: Agency effects

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

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

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

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

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

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

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

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

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

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

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

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

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

10. PAP Section 5.5: DRETS and regime choices

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

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

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

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

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

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

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

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

11. PAP Section 6.2: Survey indexes and intervention attitudes

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

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

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

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

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

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

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

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

12. PAP Section 7: Multiple testing and inference

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

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

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

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

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

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

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

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

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

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

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

13. Precision and limitations

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

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

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

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

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

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

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

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

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

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

14. Summary of PAP alignment

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

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

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

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

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

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

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

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

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

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

Transparency note.

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

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

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

Table B1: Summary of PAP alignment and implementation clarifications

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

Table B2: Outcome-construction crosswalk for PAP analyses

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

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

Table B3: Pre-exposure belief comparisons

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

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

Table B4: Pre-exposure between-arm prior comparison

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

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

Table B5: Direct exposure effects on beliefs

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

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

Table B6: Direct exposure effects on regime choices

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

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

Table B7: Order effects

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

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

Table B8: Factual information effects on beliefs

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

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

Table B9: Factual information effects on regime choices

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

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

Table B10: Direct exposure versus factual information

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

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

Table B11: Agency effects on beliefs

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

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

Table B12: Agency effects on regime choices

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

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

Table B13: DRETS and regime choices

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Table B26: PAP-specified regression and LPM specifications

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

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

Table B27: PAP Section 6.2 survey index variables

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

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

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

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

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

Table B29: Precision diagnostics for selected heterogeneity analyses

Panel A. DRETS cell-size diagnostics

Arm	DRETS type	Timing	Sample	N	PC share	Alternative	Approx. MDD
FME	LL	Initial	Full	163	0.681	LL < 0.5	0.097
FME	LL	Initial	Restricted	108	0.778	LL < 0.5	0.120
FME	LL	Final	Full	163	0.393	LL < 0.5	0.097
FME	LL	Final	Restricted	108	0.407	LL < 0.5	0.120
FME	HH	Initial	Full	19	0.789	HH > 0.5	0.285
FME	HH	Initial	Restricted	10	0.900	HH > 0.5	0.393
FME	HH	Final	Full	19	0.579	HH > 0.5	0.285
FME	HH	Final	Restricted	10	0.600	HH > 0.5	0.393
FMI	LL	Initial	Full	159	0.755	LL < 0.5	0.099
FMI	LL	Initial	Restricted	113	0.858	LL < 0.5	0.117
FMI	LL	Final	Full	159	0.616	LL < 0.5	0.099
FMI	LL	Final	Restricted	113	0.646	LL < 0.5	0.117
FMI	HH	Initial	Full	16	0.500	HH > 0.5	0.311
FMI	HH	Initial	Restricted	11	0.727	HH > 0.5	0.375
FMI	HH	Final	Full	16	0.375	HH > 0.5	0.311
FMI	HH	Final	Restricted	11	0.455	HH > 0.5	0.375
PCE	LL	Initial	Full	176	0.790	LL < 0.5	0.094
PCE	LL	Initial	Restricted	124	0.887	LL < 0.5	0.112
PCE	LL	Final	Full	176	0.511	LL < 0.5	0.094
PCE	LL	Final	Restricted	124	0.524	LL < 0.5	0.112
PCE	HH	Initial	Full	16	0.438	HH > 0.5	0.311
PCE	HH	Initial	Restricted	7	0.714	HH > 0.5	0.470
PCE	HH	Final	Full	16	0.438	HH > 0.5	0.311
PCE	HH	Final	Restricted	7	0.571	HH > 0.5	0.470

Panel B. Regression precision diagnostics

Analysis	Sample	Coefficient of interest	Estimate	Clustered SE	Approx. MDE	N	Clusters
Exper.-by-intervention index, PC support change	FME/FMI full	Exper. × intervention	-0.071	0.103	0.287	523	28
Exper.-by-intervention index, belief updating	FME/FMI full	Exper. × intervention	0.075	0.039	0.108	523	28
Decisive-by-prior belief, posterior belief	FME full	Prior belief × decisive	-0.211	0.127	0.356	264	14
Eq. (26), choice revision agency LPM	FME full	Initial PC choice × decisive	-0.636	0.135	0.378	264	14
Exper.-by-intervention index, PC support change	FME/FMI restricted	Exper. × intervention	-0.115	0.097	0.271	351	28
Exper.-by-intervention index, belief updating	FME/FMI restricted	Exper. × intervention	0.043	0.044	0.123	351	28
Decisive-by-prior belief, posterior belief	FME restricted	Prior belief × decisive	-0.204	0.108	0.301	171	14
Eq. (26), choice revision agency LPM	FME restricted	Initial PC choice × decisive	-0.543	0.171	0.479	171	14

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