Incentivizing Inflation Expectations*

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Abstract

Accurate inflation expectations are central to economic modeling and policy. Yet major surveys elicit them without performance-based marginal incentives, despite their well-established importance to data quality in experimental economics. We show that marginal incentives raise effort and fundamentally reshape reported inflation expectations by reducing noise: lowering upward bias by 3.4 percentage points, reducing disagreement by about one-third, closing the gender gap, and tripling learning rates in an RCT. Incentivized expectations are more consistent and better predict spending. Calibrating a simple New Keynesian model, we demonstrate that these differences matter: marginal incentives sharpen empirical inference and improve policy guidance – without increasing participant total remuneration.

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1 Introduction

Macroeconomic beliefs, particularly inflation expectations, are central to research and policymaking. Surveys and RCT-style information provision experiments have become key tools for measuring household expectations, deepening our understanding of expectation formation and challenging core rational expectations assumptions in macroeconomic models.[1] Central banks now routinely use such survey data and experiments to guide both conventional and unconventional monetary policy.[2] This reflects a broader shift in economics toward more flexible measurement approaches that incorporate beliefs and choices, beyond observed behavior, thereby supporting richer models, while relaxing identification assumptions, and ultimately improving policy design (e.g., Almås et al. 2024). Because these survey measures are widely used to estimate expectation formation, disagreement, and learning in macroeconomic models, any systematic measurement error in belief elicitation can distort empirical inference and policy evaluation.

Despite their central role in macroeconomic research and policy analysis, most surveys and information-provision experiments measure inflation expectations without marginal incentives, raising the possibility that survey design itself may affect measured expectations. By contrast, in experimental economics, such performance-based payments are a long-established tool for ensuring internal validity (Smith 1976), with a rich literature showing they reduce noise and misreporting in belief elicitation, if appropriately designed.[3] Yet such incentives remain rare in macroeconomic surveys, and the few studies that use them rely on complex or indirect designs and report mixed results.[4] Moreover, we know little about their effect on belief updating – a key margin in information-provision experiments – potentially risking systematic measurement error.

In this paper, we use a controlled experiment to quantify how survey design affects measured inflation expectations and belief updating, and therefore how expectations data used in macroeconomic research may be affected by measurement.

We make several contributions. First, we provide causal evidence that marginal incentives have large effects on the *entire* distribution of reported inflation expectations. Second, we show that marginal incentives impact belief updating. Third, we validate incentivized expectations by showing that they provide a more consistent measure of beliefs, as reflected

[1] See D'Acunto and Weber (2024b) and Weber et al. (2022) for reviews and Coibion and Gorodnichenko (2015a), Coibion et al. (2018) for examples.

[2] See Haaland et al. (2023) for a review of information provision experiments.

[3] See, e.g., Nelson and Bessler (1989), Palfrey and Wang (2009), Gächter and Renner (2010), Wang (2011), Schotter and Trevino (2014), Trautmann and van de Kuilen (2015), Schlag et al. (2015), Charness et al. (2021), Danz et al. (2022).

[4] See Armantier et al. (2015), Roth and Wohlfart (2020), Andre et al. (2022).

in several indicators, including stronger correlation with spending plans and greater internal consistency between point and density forecasts. Fourth, we present evidence that incentives operate primarily by increasing respondents' introspection and attention rather than by inducing external information acquisition. Finally, we show that differences in measured expectations induced by survey design can translate into different quantitative implications in standard macroeconomic models.

Our design replicates core elements of the Survey of Consumer Expectations (SCE), administered by the Federal Reserve Bank of New York (New York Fed), and embeds projections by the Federal Open Market Committee's (FOMC) as a standard RCT-style information intervention.[5] Participants were randomly assigned to a flat-fee control or one of three marginal-incentive treatments (*Prior*, *Posterior*, or *Both*), with expected earnings equalized across groups and calibrated to the SCE's remuneration.

Marginal incentives significantly shift the distribution of inflation expectations. In our data, the average reported one-year inflation expectation falls from 6.1% in the unincentivized treatment to 2.7%, and participants report less extreme forecasts when incentives are introduced. Additionally, incentivized inflation forecasts exhibit about a 30 percent reduction in cross-sectional disagreement, and become more consistent with professional forecasts. Further, incentives reduce the gender gap in inflation expectations – a persistent puzzle in the existing survey literature. These patterns suggest that modest changes in survey design can substantially affect measured expectations, even when the underlying economic environment and information remain unchanged.

We attribute these effects to increased attention and effort. Incentivized respondents round less in their point forecasts, rely less on backward-looking heuristics, and spend more time on forecasting tasks. They also do not differ substantially from flat-fee subjects when self-reporting the use of external information during the task. This evidence suggests that part of the variation observed in survey-based expectations may reflect differences in reporting effort.

In the RCT setting, incentives reshape both the distribution of posteriors and the strength of learning. Incentivized respondents assign markedly less weight to extreme inflation outcomes and more weight to bins centered on the FOMC's signal, yielding posterior distributions that are tighter and more closely aligned with the target. In our cleanest comparison, we find that learning rates roughly tripled in response to marginal incentives. Further, we find that responsiveness to the information treatment is weaker when the prior is incentivized, which is consistent with respondents forming more attentive priors. This pattern is in line with Weber

[5] We focus on the SCE given its wide use in research and policymaking (e.g., Armantier et al. 2024, D'Acunto and Weber 2024b, Weber et al. 2022).

et al. (2025), who attribute stronger priors to high-inflation environments that increase the salience of inflation.

Importantly, we also show that incentivized expectations are more behaviorally informative using a second survey wave that omits the RCT but adds modules on spending, longer-term expectations, and information search. Incentivized forecasts correlate more strongly with spending plans, improve consistency between point and density forecasts, and lower three-year expectations when one-year forecasts are incentivized, indicating spillovers across horizons. Reported search behavior rises only modestly and does not affect the results, implying that the substantial shifts in belief distributions reflect more careful reporting of beliefs rather than information acquisition. These validation exercises suggest that incentivized expectations capture economically meaningful beliefs more closely than unincentivized survey responses.

These results have important implications for macroeconomic research and policy design. First, marginal incentives substantially reduce measured biases in survey expectations, offering a low-cost tool to improve belief elicitation without changing survey content or increasing respondent burden. Second, our results have important implications for quantitative assessments. Calibrating a standard New Keynesian model to our data shows that unincentivized surveys may overstate inflation persistence, potentially leading to misattributed structural frictions and unnecessarily prolonged policy responses. Similarly, incentivized data suggests lower disagreement, which is associated with more effective transmission of both conventional monetary policy and forward guidance. Third, by raising attention, effort, and thereby compliance, marginal incentives increase the experimental control researchers have in information provision studies, improving internal validity of estimated learning rates. More broadly, by bridging survey and experimental methods, our results show that incentives can be used as a measurement tool to identify how survey design affects reported expectations and therefore the interpretation of belief data used in macroeconomic research and policy.

The remainder of this paper is organized as follows. Section 2 describes the experimental design. Section 3 presents the main empirical findings, focusing on the effects of marginal incentives on inflation expectations, learning rates, and the drivers of effects such as attention and effort. Section 4 shows incentives improve belief data quality. Section 5 provides a model-based illustration of how incentive-induced changes in expectations affect inflation dynamics in a simple New Keynesian framework. Section 6 concludes.

1.1 Related Literature

Incentives and belief reporting Reflecting a shift toward more flexible measurement, a growing literature explores improvements in survey design through alternative non-incentivized elicitation methods (Becker et al. 2023, Boctor et al. 2024, Goldfayn-Frank et al. 2025, Pavlova 2025).[6] A natural question is whether incorporating incentives can further improve belief elicitation. A large experimental economics literature, building on the induced value theory of Smith (1976), shows that marginal incentives reduce noise and misreporting by aligning participants' interests with truthful reporting (Nelson and Bessler 1989, Palfrey and Wang 2009, Gächter and Renner 2010, Wang 2011, Trautmann and van de Kuilen 2015, Schotter and Trevino 2014, Schlag et al. 2015). As for how to design such incentives, Charness et al. (2021) argue that simple, non-incentive-compatible mechanisms may outperform complex scoring rules, especially when respondents face limited cognitive bandwidth or numeracy.

Incentives in macroeconomic belief surveys The macroeconomic literature on whether incentivized elicitations improve belief accuracy is still limited and mixed. Some argue that survey expectations are "cheap talk" given weak incentives (Pesaran and Weale 2006, Manski 2004). Keane and Runkle (1990) find that a lack of incentives does not affect professional forecasts, though professionals face reputational incentives absent for households. Household inflation expectations do a worse job of predicting inflation than their consumption decisions (Inoue et al. 2009), suggesting misalignment between reported expectations and behavior. Evidence on whether incentives improve household expectations is mixed: Armantier et al. (2015) find unincentivized expectations correlate with incentivized investment, except among the less educated, implying that unincentivized measures can still be informative. For beliefs about recessions and unemployment, incentives appear not to matter (Roth and Wohlfart 2020, Andre et al. 2022). By contrast, incentivizing inflation expectations shifts beliefs closer to expert forecasts and increases response times, a measure of effort (Andre et al. 2022).[7] Notably, Andre et al. (2022) use a clever design linking rewards to second-order beliefs: participants were incentivized to match the average expert forecast rather than their own. While this sheds light on how incentives shape beliefs about expert opinion, it differs from approaches focusing on first-order beliefs, where forecasts are evaluated against realized outcomes.

[6] These studies often focus particularly on density forecast elicitation. As part of this shift, economists increasingly recognize the importance of validating new measures. For an examination of qualitative self-assessment and incentivized methods in the context of preference elicitation, see Chapman et al. (2025).

[7] The authors pool unemployment and inflation expectations and therefore conclude no overall difference between incentivized and unincentivized beliefs in a joint test.

We build on this literature but differ in three important ways. First, we implement incentives within a survey closely aligned with the SCE, ensuring comparability with prior studies. Second, we use a simple scheme that directly incentivizes accuracy of both point and density forecasts, limiting potential confusion common under complex methods that often reduce truthful reporting (Charness et al. 2021, Danz et al. 2022, Abeler et al. 2023, Drobot et al. 2025). Finally, we are the first study how incentives affect belief updating in RCTs with information provision, allowing us to evaluate not only initial reporting but also learning.

Incentives and rational inattention Our paper also contributes to the literature on rational inattention (Sims 2003, Maćkowiak and Wiederholt 2024). While this literature emphasizes that processing all available information is costly and that individuals face cognitive limitations, our findings highlight the crucial role of incentives in shaping attention and eliciting expectation formation more broadly. Marginal incentives raise the return to accuracy in the inflation expectations submission task. This can improve measured expectations by reducing reporting noise and cognitive errors through higher effort.[8] In the field, the incentives to pay attention can arise from changing economic conditions (Braitsch and Mitchell 2022, Pfäuti 2023, Bracha and Tang 2024, Wabitsch 2024, Weber et al. 2025, Link et al. 2025) or from endogenous factors such as individual stakes and relevance (Gaglianone et al. 2022).

2 Experimental Design

Our experimental design is guided by two primary objectives. First, we investigate whether and how the implementation of marginal incentives alters survey-based belief measures. Second, we examine whether marginal incentives can influence belief updating in a survey-based RCT, a widely adopted methodology in experimental macroeconomics. To achieve these goals, our experiment must generate reliable survey-based beliefs free from the influence of extraneous information provision while simultaneously conducting an information provision experiment.[9]

To address these objectives, we designed an individual-choice survey that elicits both prior and posterior one-year-ahead expectations of annual inflation from each participant. Figure 1 visualizes the key steps of the experiment. Specifically, we elicited priors as point expectations (see Figure A-8) and posteriors as probabilistic forecasts (see Figure A-16). We

[8] In the absence of marginal incentives, the strategy that maximizes survey payment per unit of time may simply be to complete the survey as quickly as possible. Under such conditions, it could be considered rational for participants to devote minimal attention to the survey.

[9] The complete survey is shown in Appendix A3. We use oTree to code the interface (Chen et al. 2016).

deliberately employ two differently worded questions to facilitate comparability with related studies in the literature. Although not universal, varying the wording of elicitation questions before and after an experimental intervention is a common design feature of information provision studies including in the context of inflation expectations (e.g., Haaland et al. 2023). This practice is intended to mitigate concerns that repeating an identical question may induce anchoring, consistency pressures, demand effects, or other response biases.[10]

In addition to eliciting priors, we also asked for their point beliefs about inflation over the past 12 months to control for perceived inflation (these were not incentivized in any of the treatments). Between these measures, participants received a summary of the Federal Open Market Committee's (FOMC) most recent inflation expectations, including median forecasts for 2024 and 2025 and corresponding range forecasts (see Figure A-11). This is the information provision intervention. Traditionally, RCTs include a no-information control group, which is particularly important in settings where priors and posteriors are elicited using different methods. Such a control group provides a benchmark for any purely mechanical updating in reported beliefs that may occur even in the absence of new information, thereby allowing the effect of the treatment (or incentives) to be identified relative to that baseline. In our study, we do not include a pure no-information control group for two reasons: (1) the effect of this specific information treatment is already well documented in the literature, and (2) our objective is to identify the incremental learning induced by incentives (i.e., learning in incentivized groups relative to otherwise identical, non-incentivized groups). Additionally, we collected participants' expectations for food and gas prices both before and after the information provision, ensuring that questions focused on inflation were adequately separated from the information provision and from each other to minimize bias (Haaland et al. 2023, Stantcheva 2023). Importantly, we based the wording and response options on the carefully designed New York Fed's Survey of Consumer Expectations (Bruine de Bruin et al. 2010, Armantier et al. 2017, 2024). We focus on the SCE and inflation expectations due to their central role in both academic research and policy discussions.[11] Moreover, inflation expectations are particularly well-suited to incentivization, as they are verifiable and it is not too impractical to incentivize them. Worth noting is that we adopted the welcoming language of the SCE intended to activate participants' intrinsic motivation (see Figure A-2).

We implemented a between-subjects design, randomly assigning participants into one of four treatments, summarized in Table 1. Our baseline treatment, *Flat*, provides participants with a fixed fee without any marginal incentives. To match the time-value of money earned by

[10] We leave a direct comparison using identical wording to future research.

[11] The SCE measures U.S. households' expectations on key economic variables like inflation, aiding policymakers and researchers in understanding consumer sentiment and behavior. For example, it helps the Federal Reserve assess inflation expectations, guide interest rate decisions, and forecast spending and savings trends. Its questions are also widely used in academic research to study the formation of inflation expectations.

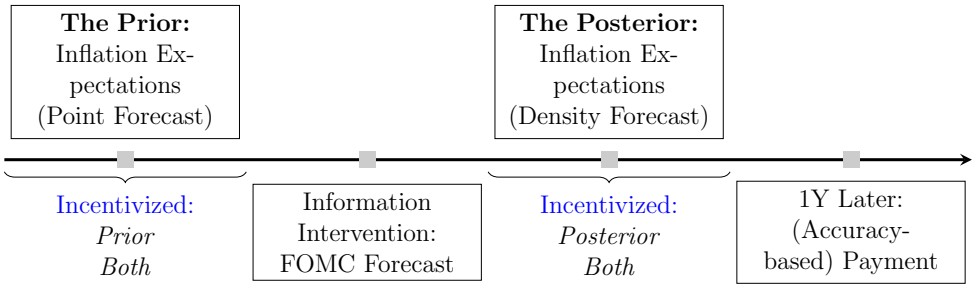


Figure 1: Experimental Design

Notes: The figure provides a simplified overview of the key steps in our survey (from left to right) as completed by all participants. Below the curly brackets are the two treatments in which inflation expectations were incentivized. In the other treatments, participants still provided their inflation expectations, but without an accuracy-based future payment for these responses.

participants in the SCE, we scaled the *Flat* payment accordingly. This payment is divided into two parts: a fixed fee of $2 paid immediately upon survey completion and an additional $4 paid in September 2025 (one year later), aligning with the forecast period. This delayed payment controls the timing of bonus payments necessary for other treatments and avoids potential selection effects.

Table 1: Overview of Treatments

Treatment	Prior	Posterior
Flat	Unincentivized	Unincentivized
Prior	Incentivized	Unincentivized
Post	Unincentivized	Incentivized
Both	Incentivized	Incentivized

Notes: The table shows the four treatments that differ in incentivizing elicited prior and/or posterior inflation expectations (before and after information provision). Priors are elicited using point forecast questions, while posteriors are elicited using probabilistic density forecast questions.

The three additional treatments introduce marginal incentives based on the accuracy of participants' one-year-ahead inflation forecasts. In *Prior*, participants receive a bonus payment contingent on the forecast error relative to the realized annual Personal Consumption Expenditures (PCE) inflation reported by the Bureau of Economic Analysis (BEA) in September 2025. A perfect forecast earns a bonus of $10. Each additional percentage point (pp) forecast error reduces the bonus by half.[12] This scoring rule is common in learning-to-forecast

[12]While Armantier and Treich (2013) highlight the potential for Proper Scoring Rules (PSRs) to distort beliefs when respondents have financial stakes or hedging opportunities, our inflation forecasting experiment differs in several key ways. Unlike prediction markets or controlled probabilistic events, our respondents forecast a well-known macroeconomic variable, allowing them to anchor beliefs onto experience, news, or forecasts from credible institutions. This can minimize the distortions typically associated with PSRs in more abstract or game-theoretic settings. Further, inflation forecasting is fundamentally a task of ambiguity rather than risk, and our participants lack opportunities to hedge. Additionally, incentives in our setting weaken the link between inflation perceptions and expectations, and appeared to enhance attention and effort (see Section 3.3), while aligning forecasts more closely with professional expectations, consistent with thoughtful engagement rather than distortion.

experiments in experimental macroeconomics and is easy to explain.[13] In *Post*, we pay participants $\$10 * weight_i$ where $0 \leq weight_i \leq 1$ is the probability weight assigned by the participant to bin i that contains realized inflation. For example, if inflation turns out to be 5% and a participant assigned probability weight .2 to the bin for 4% to 8%, then the participant would earn \$10*.2=\$2. For *Both*, a subject faced either the point or probabilistic marginal incentive scheme with equal likelihood. Table A-1 gives an overview of the payment structure by treatment.

A final design consideration concerns the incentive scheme. We deliberately adopt a simple incentive scheme even though it is not strictly incentive-compatible in the mechanism-design sense. While our payments do not implement a strictly proper scoring rule and therefore do not guarantee truthful reporting as a dominant strategy, they provide clear marginal rewards for accuracy and therefore increase the incentive to carefully consider responses. Achieving full incentive compatibility would require a more complex scoring mechanism, which may increase cognitive load and reduce transparency for participants. Indeed, we opted for simplicity because a growing experimental literature shows that more complex, theoretically incentive-compatible mechanisms do not necessarily improve belief elicitation and may even reduce performance when participants do not fully understand the payoff structure (e.g., Charness et al. 2021; Danz et al. 2022). In settings involving expectation formation and learning, simple and transparent incentives perform at least as well as more complex scoring rules. Moreover, Drobot et al. (2025) specifically study the trade-off between incentive compatibility and complexity in the context of inflation expectations and provide evidence supporting the use of simple incentive schemes such as ours. Taken together, this evidence suggests that, in practice, simplicity can enhance effective incentive provision even when strict theoretical incentive compatibility is relaxed. This approach aligns with an emerging view that effective belief elicitation requires a balance between theoretical properness but also cognitive tractability.

The four treatments cleanly isolate the role of marginal incentives in reported beliefs and their updates within a survey-based information provision experiment. The *Flat* treatment serves as a benchmark without marginal incentives, consistent with the incentives in widely-used economic surveys.[14] The *Prior* and *Post* treatments introduce incentives before or after information provision, respectively, allowing us to test whether incentives dampen (*Prior*)

[13] See McMahon and Rholes (2023) and Rholes and Petersen (2021) for examples. It elicits the median and is incentive-compatible under risk neutrality.

[14] Examples include the New York Fed's Survey of Consumer Expectations (SCE), the University of Michigan's Survey of Consumers, the Understanding America Study (UAS), the Panel Study of Income Dynamics (PSID), the Health and Retirement Study (HRS), the American Life Panel (ALP), the European Central Bank's Consumer Expectations Survey (CES), and the Bundesbank's Panel on Household Finances and Expectations (PHF-E).

or amplify (*Post*) responsiveness to new information by encouraging greater attention to the provided information. These treatments are particularly informative for understanding how incentivized attention and cognitive effort influence learning. The *Both* treatment applies incentives across both stages, testing whether consistency in incentive structure alters updating dynamics.

This design enables three key comparisons:

1. Comparing *Flat* to *Prior* and *Post* reveals whether marginal incentives shift beliefs at distinct stages of the elicitation process. Further, from a methodological perspective, these comparisons reveal whether paying for one forecast with certainty (*Prior* or *Post*) versus the probabilistic payment of one of the two (*Both*) affects the effectiveness of incentives.

2. Comparing *Post* to *Both* isolates whether holding incentives constant across information provision affects the magnitude or direction of belief updating.

3. Comparing *Flat* to *Both* provides a clean test of whether marginal incentives systematically distort or enhance survey-based belief updating.

To calibrate incentives, we used historical forecast errors from the New York Fed's SCE together with actual inflation outcomes from FRED. The average forecast error was 1.68 pp across the full historical sample (1.16 pp in recent years). Using a standard discount factor ($\beta = 0.8$) from Warner and Pleeter (2001), we set maximum payoffs such that expected present-value earnings matched the time-value of SCE participation. For our 5-minute survey, this implied total compensation of about \$6, with 33% (\$2) allocated as a show-up fee. Incentives were then applied as follows: in *Prior*, to point forecasts before information provision; in *Post*, to probabilistic forecasts afterward; and in *Both*, randomly to one of the two with equal probability.

2.1 Hypotheses

Before moving on to the results, we outline two hypotheses on the impact of marginal incentives. These hypotheses are grounded in induced value theory, which holds that performance-based financial incentives enhance cognitive effort and reduce biases in behavior and self-reported data, thereby improving reliability and reducing measurement error (Smith 1976, Smith and Walker 1993, and Camerer and Hogarth 1999). Although originally developed for valuation tasks such as auctions, this logic extends to belief elicitation (see Schotter and Trevino 2014, Schlag et al. 2015, Charness et al. 2021, Healy and Leo 2026 for reviews). In our context, we expect incentives to reduce upward bias, forecast errors, and extreme

outliers.[15]

Hypothesis 1 (Survey-Based Expectations): *Marginal incentives reduce upward bias (lower mean) and disagreement (lower variance) in the distribution of inflation expectations, aligning forecasts more closely with those of professional forecasters.*

The second hypothesis is about the impact of information provision on belief updating depending on the incentive structure implemented in our treatments. In information-provision RCTs, a natural object of interest is the extent to which respondents incorporate the provided signal when moving from priors to posteriors. Marginal incentives can affect this responsiveness through their impact on attention and cognitive effort, but the sign depends on the timing of incentives. When incentives apply only to the posterior report (*Post*), they raise the return to carefully processing the provided information and translating it into a more accurate posterior, implying a clear prediction of stronger updating toward the signal relative to the flat-fee benchmark (*Flat*). By contrast, when incentives are present before the signal is received (*Prior*) or at both stages (*Both*), they may also increase effort in forming priors (e.g., through recall, internal deliberation, or information acquisition), thereby leading to stronger priors. Stronger priors can attenuate subsequent updating toward the signal, especially in treatment *Prior* where posterior expectations are not incentivized, so the net effect of incentives on measured responsiveness is weaker in these treatments and could even overturn in treatment *Prior*.

Hypothesis 2 (Learning Rates): *Relative to Flat, marginal incentives in treatment Post increase the estimated learning rate, i.e., the weight placed on the signal, by raising attention and compliance with the provided information signal. The effect is ambiguous in treatment Both and Prior, relative to Flat, because incentives can also strengthen priors.*

2.2 Data

We collected 1,000 observations – 250 per treatment – from U.S. residents via Prolific on September 14, 2024. Prolific provides information on participants' demographic characteristics such as age, gender, income, or race. Our random sample matches the SCE quite well in terms of respondent characteristics, which are fairly balanced across treatments (see Table A-2 in Appendix A1 for a comparison of demographic characteristics across treatment

[15] In the inflation expectations literature, upward bias refers to the systematic overestimation of future inflation relative to realized outcomes, a pattern widely documented in household surveys and present at the time of our study.

groups and with the SCE sample).[16] The chosen sample size is based on power calculations (see Appendix A2). With few exceptions, we winsorize data at the 1% and 99% levels to mitigate the impact of extreme outliers on our main results.

3 Results

This section details the results of our survey. We first show how incentives affect substantial parts of the elicited expectations distribution, highlighting that incentivized expectations are lower, become more consistent with professional forecasts from the Survey of Professional Forecasters (SPF), exhibit lower disagreement, and diminish the puzzle of gendered expectations. We then show that this is because incentives raise participants' effort and attention, reducing common survey response heuristics such as reporting inflation perceptions as expectations. And finally, we show how incentives within an information provision experiment affect belief updating and raise measured learning rates.

3.1 The Effect of Incentives on Elicited Expectations

We first consider whether marginal incentives influence respondents' one-year-ahead inflation expectations, measured as point forecasts, which we illustrate in Figure 2. This figure shows the cumulative distribution functions (CDFs) of inflation expectations across the different treatment groups, expressed in percentage points. The treatments imposing marginal incentives – *Both* (blue curve) and *Prior* (light blue curve) – are contrasted with *Flat* (black curve) and *Post* (gray curve) that do not include marginal incentives. The *Flat* and *Post* treatments mimic the typical approach of major macroeconomic survey data, reflecting the incentive mechanism underlying most belief-based empirical macroeconomics research.

Marginal incentives generate significantly different distributions than flat-fee payments. In particular, respondents in incentivized treatments report less extreme forecasts: inflationary expectations are muted relative to unincentivized treatments, and deflationary expectations are also less pronounced. This effect arises despite identical timing and expected payment amounts across treatments, showing that modest changes in incentive structure can substantially alter reported expectations. Because the treatments hold fixed the survey structure and the information shown, these findings suggest that incentives have an impact on respondents' cognitive effort and attention, and potentially information acquisition (see Section 4

[16] Coincidentally, there is a relatively higher proportion of females in treatments *Prior* and *Both*. Previous studies have shown that females tend to have higher inflation expectations. Since we observe higher inflation expectations in the *Flat* treatment, we do not believe this affected our results. Further, we control for gender in our regressions.

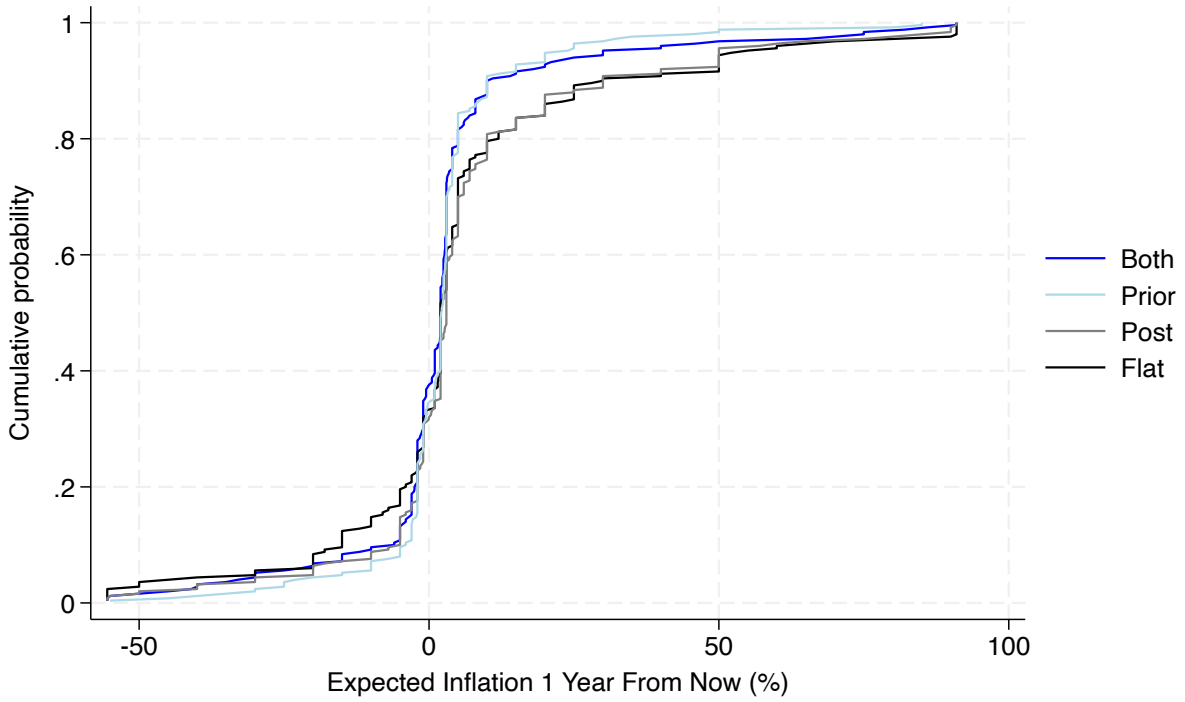


Figure 2: CDFs of Expected Inflation By Treatment

Notes: This figure shows cumulative distribution functions (CDFs) of inflation expectations across the different treatment groups, expressed in percentage points. Data are winsorized at the 1% and 99% levels. Treatments *Both* and *Prior* are incentivized and shown in shades of blue, while treatments *Post* and *Flat* are unincentivized, shown in gray and black.

for evidence that the latter channel is unlikely to be the primary mechanism). Overall, this evidence is consistent with more careful belief reporting under incentives.

Table 2 summarizes the mean and standard deviation of point forecasts. The unincentivized group has a significantly higher mean (6.13) compared to the incentivized group (2.73), with a correspondingly greater standard deviation (23.78 vs. 16.98), indicating higher cross-sectional disagreement among unincentivized forecasters.

Equality-of-means and variance tests confirm these differences. Welch's t-test rejects equality of means ($p < .01$), and both Levene's tests and the F-test strongly reject equality of variances ($p < .01$). Thus, marginal incentives lower both the level and cross-sectional disagreement of inflation expectations. Incentivized participants also exhibit lower medians and interquartile ranges (Table A-3), which are more robust measures that are less sensitive to outliers. Measurement of the level of expectations can distort empirical inference and lead to misguided policy decisions, as we illustrate in Section 5. Similarly, measuring disagreement accurately is important because of its implications for policy transmission: in high-disagreement states, contractionary shocks can perversely raise inflation or invert market responses, while in low-disagreement states they restore conventional effects (Falck et al.

Table 2: Summary Statistics and Variance Comparison of Inflation Expectations

	Mean	Std. Dev.	N
SCE	6.43	19.68	240
Unincentivized	6.13	23.78	500
Incentivized	2.73	16.98	500
All Data	4.43	20.72	1,000

Test for Equality of Means and Variances

Test Type	Test Statistic	p-value
Welch's t-test (Difference in Means)	-2.61	$p < .01$
Levene's Test (Mean)	31.54	$p < .001$
Levene's Test (Median)	21.31	$p < .001$
Levene's Test (Winsorized Mean)	23.32	$p < .001$
F-Test (Variance Ratio)	1.9594	$p < .001$

Notes: This table shows mean and variances of one year ahead inflation expectations for inexperience participants in the SCE's September 2024 wave and the elicited prior belief of inflation $\mathbb{E}(\pi_{Prior})$ by incentivization scheme. *Unincentivized* is comprised of treatments *Flat* and *Posterior*, while *Incentivized* is comprised of *Both* and *Prior*. The tests assess differences in means and variances between the incentivized and unincentivized groups in our survey. Data are winsorized at the 1% and 99% levels.

2021, Barbera et al. 2023). High disagreement also weakens the impact of forward guidance and conventional policy (Dong et al. 2024).

Incentives Shift the Entire Distribution. Our first hypothesis (see Section 2) posits that marginal incentives alter the distribution of inflation expectations. The results strongly support this hypothesis. As shown in Table A-4, coefficients for *Both* and *Prior* indicate significantly lower inflation and deflation forecasts relative to the unincentivized *Flat* treatment. Respondents in *Prior* report significantly lower expectations for price changes compared to those in the *Flat* treatment, with absolute forecast values approximately half as large on average ($p < 0.01$). The effect in *Both* is somewhat smaller but still sizable, with forecasts about one-third lower on average ($p < 0.01$). These effects are robust to controlling for age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state.

We next examine extreme forecasts, defined as the top 10% of absolute prior expectations. Logistic regressions in columns (3)–(4) of Table A-4 and Table A-5 show unincentivized respondents are more likely to report extremes than those in *Prior* or *Both*. Specifically, *Flat* participants are 222% more likely and *Post* participants are 181% more likely than those in *Prior* to give extreme forecasts. Interestingly, even *Both* participants are 91% more likely to provide extreme forecasts, suggesting that randomizing incentive payments is less effective than incentivizing with certainty.

Importantly, while incentives reduce extreme forecasts, simply increasing the level of winsorization in the unincentivized group is not a sufficient remedy. Incentives have substantive

effects on the entire distribution. To demonstrate this, we conduct Kolmogorov–Smirnov tests to assess whether the distributions of expectations differ between incentivized and unincentivized participants. As shown in Table 3, we find statistically significant differences across a wide range of winsorization thresholds. Distributions are significantly different ($p < 0.01$) including the 25–75% range, indicating differences in expectations are not confined just to the extremes. Only at narrower central cuts (i.e., 45–55%), which cap 90% of outliers, does statistical significance weaken, as expected due to fewer unique values in the remaining sample (i.e., 10). These results suggest that incentives shift the overall distribution of expectations rather than merely affecting outliers.

Table 3: Kolmogorov–Smirnov Tests Across Winsorization Cuts

Winsorization Cut (%)	D-statistic	p-value	Unique Values
1–99	0.1400	0.000	119
5–95	0.1400	0.000	95
10–90	0.1400	0.000	78
25–75	0.1400	0.000	47
40–60	0.1080	0.006	12
45–55	0.0860	0.050	10

Notes: This table reports Kolmogorov–Smirnov tests assessing whether the distributions of inflation expectations (priors) differ between incentivized and unincentivized groups. Unique values refer to the number of distinct values across the entire sample, comprising all treatment groups.

Comparisons to Professional and SCE Forecasts. We also find evidence that marginal incentives reduce upward bias and align respondents' expectations more closely to those of professional forecasters, who have historically forecasted inflation more accurately (Carroll 2003). Panel A of Table 4 compares each treatment to mean PCE forecast from the Survey of Professional Forecasters (SPF) collected over the most cleanly comparable time sample.[17] Expectations under *Flat* (5.52%) and *Post* (6.75%) are significantly higher than the SPF benchmark (2.11%), while incentivized forecasts under *Prior* (2.80%) and *Both* (2.65%) are much closer.

Panel B of Table 4 compares each treatment to contemporaneous summary statistics from the SCE. The unincentivized treatments (*Flat* and *Post*) are statistically indistinguishable from the SCE benchmark. By contrast, the incentivized treatments (*Prior* and *Both*) differ significantly from the SCE, exhibiting substantially lower average expectations. This pattern suggests that our unincentivized baseline closely replicates one-year-ahead inflation expectations collected in large-scale household survey data.

[17] Data from the SPF are for the mean PCE inflation forecast for Q4 2024 to Q4 2025 (PCEB) from the Q3 2024 survey, which most closely aligns with our experimental time frame of September 2024 to September 2025.

Table 4: Comparisons to External Professional (SPF) and Household Forecasts (SCE)

	Treatment	Mean	Std. Dev.	Difference	p-value
Panel A: Comparison to SPF benchmark (mean: 2.11%, std. dev.: 0.286)					
Unincentivized	Flat	5.52	(24.910)	-3.41	0.032
	Post	6.75	(22.617)	-4.64	0.001
Incentivized	Prior	2.80	(14.313)	-0.69	0.447
	Both	2.65	(19.319)	-0.44	0.665
Panel B: Comparison to SCE benchmark (mean: 6.43%, std. dev.: 19.68)					
Unincentivized	Flat	5.52	(24.910)	0.91	0.590
	Post	6.75	(22.617)	-0.32	0.840
Incentivized	Prior	2.80	(14.313)	3.63	0.011
	Both	2.65	(19.319)	3.78	0.015

Notes: Panel A compares treatment means to the SPF mean PCE inflation forecast for Q4 2024–Q4 2025 (PCEB) from the Q3 2024 survey. Panel B compares to contemporaneous SCE summary statistics (inexperienced respondents, September 2024). Difference is benchmark minus treatment mean. Welch's t-test accounts for unequal variances and sample sizes; SPF sample is N=33. Experimental data are winsorized at the 1st and 99th percentiles.

Overall, these results show that elicited expectations are highly sensitive to incentive design, requiring caution when interpreting unincentivized surveys. Researchers could either incorporate incentives to improve reliability and control or carefully attribute the biases that arise without them, which is partially addressed in the behavioral macroeconomics literature by removing extreme forecasts (deflationary and very high inflationary forecasts) and applying Huber weights.[18]

Forecast Errors and Accuracy To evaluate whether marginal incentives improve forecast accuracy, we compare our participants' forecast to realized PCE inflation.[19] Figure 3 reports mean signed forecast errors by treatment. In the unincentivized treatments (*Flat* and *Post*), respondents systematically overpredict inflation. Mean signed errors are positive and economically large, indicating substantial upward bias in reported expectations. By contrast, treatments that include marginal incentives (*Prior* and *Both*) are statistically indistinguishable from zero. Thus, incentives do not merely compress the distribution of reported beliefs; they substantially reduce upward bias.

Figure 4 reports mean absolute forecast errors. In the *Prior* treatment, mean absolute error falls by approximately 7–8 percentage points relative to *Flat*; *Both* yields a similarly sizable reduction. For comparison, the mean absolute forecast error among inexperienced SCE respondents over the same period is 11.63 – unincentivized treatments perform comparably

[18] As a few examples, see the data-cleaning procedures in Coibion et al. (2023) or Piccolo and Gorodnichenko (2025).

[19] August 2025 PCE inflation was 2.74%.

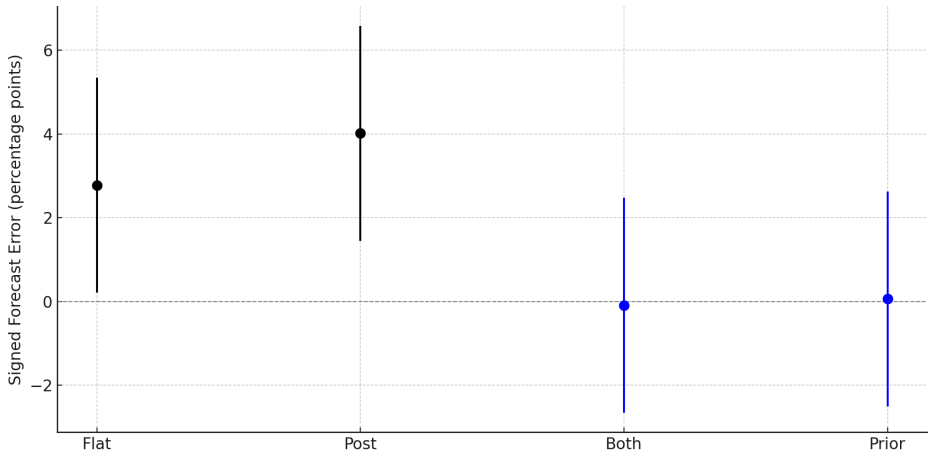


Figure 3: Forecast Errors

Notes: Mean signed forecast errors by treatment, with 95% confidence intervals. Flat and Post treatments exhibit significant positive bias (overprediction), while Both and Prior treatments are centered near zero. Blue indicates treatments with marginal incentives.

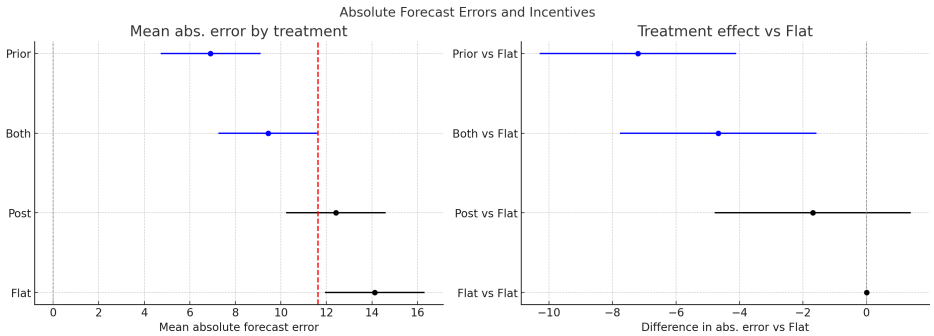


Figure 4: Left panel: Mean absolute forecast errors by treatment. The dashed red line indicates the SCE benchmark. Right panel: Estimated treatment effects relative to the Flat benchmark. Blue indicates treatments with marginal incentives. 95% confidence intervals in both panels.

to this benchmark, while incentivized treatments substantially outperform it.

Marginal incentives thus improve forecast accuracy along two margins: eliminating systematic upward bias and reducing the overall magnitude of forecast errors.

Heterogeneity in Responses to Incentives. The effect of incentives on reported inflation expectations may differ across individuals that may naturally be more informed about or attentive to inflation. For example, Piccolo and Gorodnichenko (2025) document that homeowners are less responsive to an inflation-rate information treatment, suggesting greater awareness. Similarly, de Silva and Mei (2025) find that households who are close to purchasing a home have more informed and accurate interest rate expectations.

Table A-6 splits the sample by financial decision-making authority, investment experience, recent housing market involvement, income, and education, and reports incentive effects on prior inflation expectations within each subgroup. Two results stand out. First, incentives reduce expected inflation across all subgroups and both values of every split – including among the financially experienced and primary decision-makers – with effects ranging from -2.4 to -5.2 percentage points. This indicates that incentives impact not only the inattentive or financially unsophisticated. Second, effects are more consistently significant among lower-income, lower-education, and those currently not making important economic decisions like buying or selling a house, suggesting these groups are more susceptible to incentives. However, formal difference tests (columns 6–7) fail to reject homogeneity across any split (all $p > 0.37$). Across the economic and financial characteristics considered here, no demographic dimension significantly moderates the treatment effect. Gender, however, is a striking exception – a dimension along which incentives do not merely shift the level of expectations but eliminate a well-documented and persistent gap entirely.

3.2 Incentives Close the Gender Gap in Inflation Expectations

There is a long-standing strand of the survey-based belief literature, summarized recently in Reiche (2023), that documents and attempts to rationalize gender differences in inflation expectations.[20] Concisely, female survey participants typically report significantly higher inflation expectations than men (e.g., Bryan and Venkatu 2001, Bruine de Bruin et al. 2010 or D'Acunto et al. 2021).

We find that marginal incentives entirely resolve this puzzle, aligning inflation expectations across genders. To demonstrate this, we estimate separate ordinary least squares (OLS)

[20] We use the terms gender and sex interchangeably in this paper, following convention in related literature, though our variable specifically measures sex.

regressions for each Treatment$_j$, where $j \in \{Flat, Post, Both, Prior\}$, projecting prior inflation expectations onto a female indicator:

$$\mathbb{E}_{i,j}(\pi_{Prior}) = \beta_{0,j} + \beta_{1,j}\text{Female}_i + \epsilon_i.$$

Results in Table 5 confirm the conventional finding in unincentivized treatments. In *Flat*, women report significantly higher expectations than men (6.995, $p < 0.05$), and in *Post* the difference is smaller and only marginally significant.

Table 5: Effects of Incentives on the Gender Expectations Gap

	(1) Flat	(2) Post	(3) Both	(4) Prior
Female	6.995**	5.437*	3.878	2.757
	(3.238)	(3.062)	(2.740)	(2.288)
Constant	-14.78**	16.76*	-2.095	4.578
	(7.158)	(8.529)	(7.175)	(6.437)
Controls	Yes	Yes	Yes	Yes
N	249	250	249	250

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of treatments on reported inflation expectations (the priors) by gender. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels. We control for these individual-level characteristics: age, race, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state of residence. Results without control variables are even stronger and reported in Table A-7.

By contrast, under marginal incentives the gap disappears: Expectations in *Both* (3.88, $p > 0.1$) and *Prior* (2.76, $p > 0.1$) are statistically indistinguishable across genders. Figure 5 shows that this convergence arises because incentives exert a stronger moderating effect on women's expectations.

The gender gap in inflation expectations diminishes under marginal incentives because women's forecasts adjust more strongly, aligning with men's. This responsiveness resolves the observed discrepancy and shows that incentivized belief elicitation yields more consistent measures across genders. A common interpretation of the elicited gender gap is rooted in experiences such as grocery shopping and associated salience of higher prices (D'Acunto et al. 2021). As we discuss in Section 3.3, our results are consistent with this interpretation: without incentives, respondents are likely to rely on recent experiences as a low-cost heuristic, reporting what comes to mind first (e.g., recent shopping experiences), rather than exerting cognitive effort to recall information when reporting expectations.

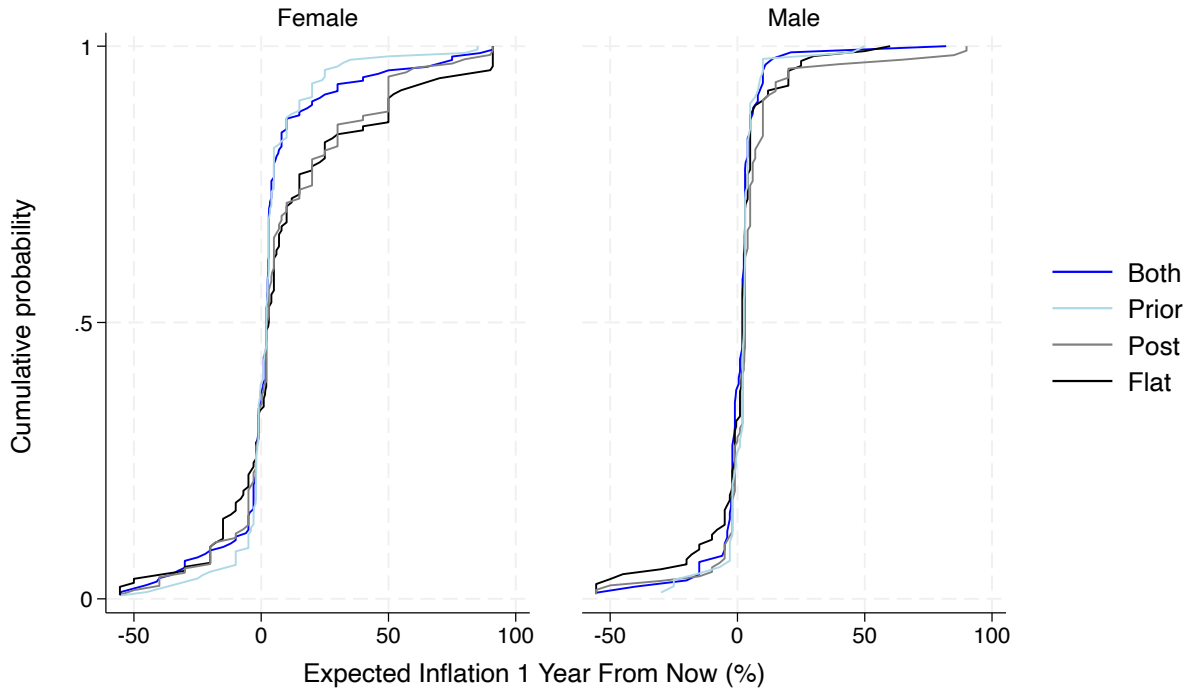


Figure 5: Effects of Incentives on Expectations by Gender

Notes: This figure shows cumulative distribution functions (CDFs) of inflation expectations by gender across the different treatment groups, expressed in percentage points. Data are winsorized at the 1% and 99% levels. Treatments *Both* and *Prior* are incentivized and shown in shades of blue, while treatments *Post* and *Flat* are unincentivized, shown in gray and black.

3.3 Incentives Raise Effort and Attention

Why do incentivized expectations become more consistent with the SPF and across genders? A key factor appears to be cognitive effort. Rational inattention theory suggests that respondents do not fully process or recall all relevant economic information (e.g., inflation trends, interest rates) because of cognitive costs (see Maćkowiak et al. 2023 for a review). Indeed, a number of studies document that households tend to simplify by relying on heuristics, such as recent price experiences (like gas or groceries), or media headlines (e.g., Coibion and Gorodnichenko 2015b, Binder 2018, D'Acunto et al. 2021, Kilian and Zhou 2022, Aidala et al. 2024, D'Acunto and Weber 2024a, Jo and Koplack 2025, Drobot 2025) and that attention is endogenous to the economic environment (e.g., Bracha and Tang 2024, Pfäuti 2023, or Weber et al. 2025).

Under flat incentives, respondents have lower motivation to exert effort, retrieve information (from memory or other sources), or provide accurate forecasts. Introducing marginal incentives increases the payoff to effort, yielding more informed expectations. We present evidence consistent with this mechanism.

3.3.1 Decoupling Inflation Expectations and Perceptions

A common heuristic for forming inflation expectations is to rely on inflation perceptions, resulting in individuals reporting future expected inflation that resembles their currently perceived inflation levels (e.g., Weber et al. 2022, Huber et al. 2023, Anesti et al. 2024). We find that incentives weaken this link. In regressions of expectations on perceptions, the relationship becomes insignificant in the incentivized *Prior* group (columns (1)–(4) of Table 6), as illustrated in Figure 6. This suggests that respondents in incentivized treatments consider a broader range of values, moving away from simple extrapolation.

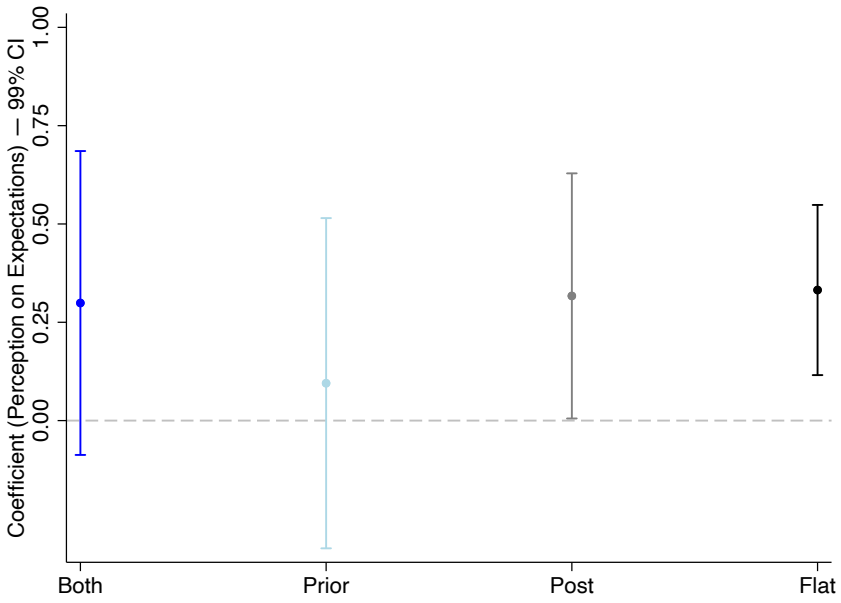


Figure 6: Inflation Expectations and Perceptions

Notes: This figure shows the relationship between perceived inflation and inflation expectations (prior point forecasts). The plotted coefficients are estimated by OLS regressions of inflation expectations on perceptions, including control variables (see Table 6 for details). Data are winsorized at the 1% and 99% levels. Treatments *Both* and *Prior* are incentivized and shown in shades of blue, while treatments *Post* and *Flat* are unincentivized, shown in gray and black. We include 99% confidence intervals.

This result is particularly striking, given that perceptions and expectations are elicited on the same survey page. One interpretation is that marginal incentives reduce reliance on simple backward-looking heuristics, prompting more deliberate recall and intentional forecasting. This distinction is especially plausible in our setting: the survey was conducted a few months before the 2024 Presidential Election and only a few weeks before the Federal Reserve's first interest rate cut in four years, so respondents may have expected near-term policy and economic changes, making their expectations more forward-looking and less mechanically tied to their current perceptions.

These findings have important implications, as they suggest that the well-documented cor-

relation between perceptions and expectations may partly reflect a survey response heuristic rather than genuine belief formation. Recognizing this distinction is crucial for measurement, modeling, and policymaking as we explore in Section 5.

3.3.2 Attention to the Survey

Cognitive effort can also be proxied using respondents' performance on questions that require attention to current macroeconomic conditions. Following Bracha and Tang (2024), we define Absolute Perception Error (APE) as the absolute difference between a respondent's perceived inflation rate and the most recent realized inflation rate (2.5% for July, released on August 14). In Bracha and Tang (2024), APE is used as a measure of inattention to the macroeconomic environment. We adopt the same statistic as a proxy for attention in our survey module on inflation perceptions: it summarizes how closely a respondent's reported perception aligns with a salient benchmark and therefore provides a simple index of response accuracy/noise in this part of the questionnaire.

Marginal incentives substantially reduce APE, indicating higher attentiveness (column (5) of Table 6). This likely reflects spillovers: Incentives applied to expectations increase care in reporting perceptions, which often serve as inputs into expectations.

We also observe a gender gap, with women exhibiting noticeably higher APE (see Table A-8). This aligns with Braitsch and Mitchell (2022), who construct an inattention measure based on the consistency of SCE point and density forecasts and show that women are less attentive than men when forming inflation expectations.

3.3.3 Survey Completion Time

An essential consideration in survey-based research is the amount of effort participants invest when responding to questions, particularly when eliciting complex beliefs such as inflation expectations. Or similarly, how compliant participants are in considering provisioned information in RCTs (Knotek et al. 2024).

We measure effort using survey completion time, a standard proxy for cognitive resources allocated to answering questions (Malhotra 2008). We designed the survey take approximately five minutes, but anticipated variation based on individual differences in reading speed, comprehension, and the effort invested in considering responses. Comparing completion times across treatments allows us to test whether marginal incentives increase the time – and thus the effort – participants devote to the survey task.

We estimate OLS regressions with completion time as the dependent variable, winsorized at

Table 6: Effects of Incentives on Perceptions and Inattention

	(1) Flat $\mathbb{E}(\pi_{Prior})$	(2) Post $\mathbb{E}(\pi_{Prior})$	(3) Both $\mathbb{E}(\pi_{Prior})$	(4) Prior $\mathbb{E}(\pi_{Prior})$	(5) APE
Perception	0.332***	0.317***	0.299**	0.095	
	(0.084)	(0.121)	(0.150)	(0.163)	
Post					-1.392
					(2.186)
Both					-8.588***
					(2.110)
Prior					-11.042***
					(1.913)
Constant	-10.550	13.920	-5.189	6.445	18.857***
	(6.921)	(9.921)	(7.423)	(6.881)	(4.502)
Controls	Yes	Yes	Yes	Yes	Yes
N	250	250	250	250	1000

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: Columns (1) through (4) show the correlation between perceived and expected inflation and demonstrate that marginal incentives break the link between the two measures. Column (5) shows the effect of treatment on Absolute Perception Error (APE). Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels. We control for these individual-level characteristics: age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state of residence.

the 5th and 95th percentiles. Treatments *Post*, *Both*, and *Prior* enter as dummies, with *Flat* as the reference category:

$$\text{CompletionTime}_i = \alpha + \sum_j \gamma_j \text{Treatment}_{i,j} + \beta \mathbf{X}_i + \epsilon_i, \tag{1}$$

where CompletionTime_i is the total time (in seconds) participant i took to complete the survey. $\text{Treatment}_{i,j}$ indicates the participant's treatment group j. $\mathbf{X}_i$ is a vector of control variables (age, race, gender, education, income level, political affiliation, primary grocery shopper status, economic sentiment, and state of residence). Results are shown in Table 7.[21]

Participants in *Both* and *Prior* spent significantly more time than those in *Flat*. The *Both* treatment increased completion time by about 111 seconds, while *Prior* increased it by 58–62 seconds. These effects are substantial relative to the survey's average length. *Post* has a positive but only marginally significant effect once controls are included, suggesting incentives applied after information provision only weakly alter overall effort.

These results support the interpretation that marginal incentives raise participant effort,

[21] We show the same results without winsorizing in Table A-10 in Appendix A1.

Table 7: Effect of Incentives on Completion Time

	(1) Completion Time	(2) Completion Time
Post	19.94	48.87*
	(25.86)	(26.00)
Both	110.8***	110.5***
	(26.80)	(26.48)
Prior	58.43**	62.11**
	(25.29)	(24.96)
Constant	567.4***	528.9***
	(18.39)	(64.76)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of treatments on effort, as proxied by completion times. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 5% and 95% levels due to the relatively high variation in completion time. In column (2) we control for these individual-level characteristics: age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state of residence.

especially when applied early in the survey, promote more deliberate reporting of beliefs and updates. Alternatively, incentivized respondents might spend more time searching for additional information to form their forecasts. However, as discussed later in Section 4, we find evidence that incentives only modestly increase information search, but not enough to materially affect our main results.

3.3.4 Rounding Behavior

Another behavioral proxy for effort is the numerical precision in reported forecasts. According to satisficing theory (Simon 1956, Krosnick 1991), individuals conserve effort when precision has low marginal value, often defaulting to rounded responses (e.g., whole numbers or focal points). In our setting, if marginal incentives increase the perceived value of accuracy, they should lead participants to provide more precise, less rounded forecasts.

We classify a forecast as rounded if the reported value is a multiple of 1, 5, or 10 percentage points (pp).[22] We construct two measures: (i) a binary indicator for whether a forecast is rounded, and (ii) a categorical measure of rounding degree. A lower incidence of rounding is interpreted as greater cognitive effort.

Incentives substantially reduce rounding. As shown in Figure 7, 91.4% of unincentivized

[22] For instance, forecasts of 10 or 20 are classified as rounded to 10 pp; forecasts such as 5 or 15 are classified as rounded to 5 pp; and values like 7.0 or 3.0 are classified as rounded to 1 pp. Forecasts such as 7.3 or 3.7 are classified as not rounded.

participants rounded their forecasts, compared to 77.2% in incentivized treatments – a 14.2 pp decline ($p < 0.01$). Probit regressions confirm that incentives tied to prior beliefs significantly reduce the probability of rounding (see Table 8). Moreover, incentivized participants round less coarsely, reinforcing that incentives increase not only time spent but also the precision of reported beliefs.

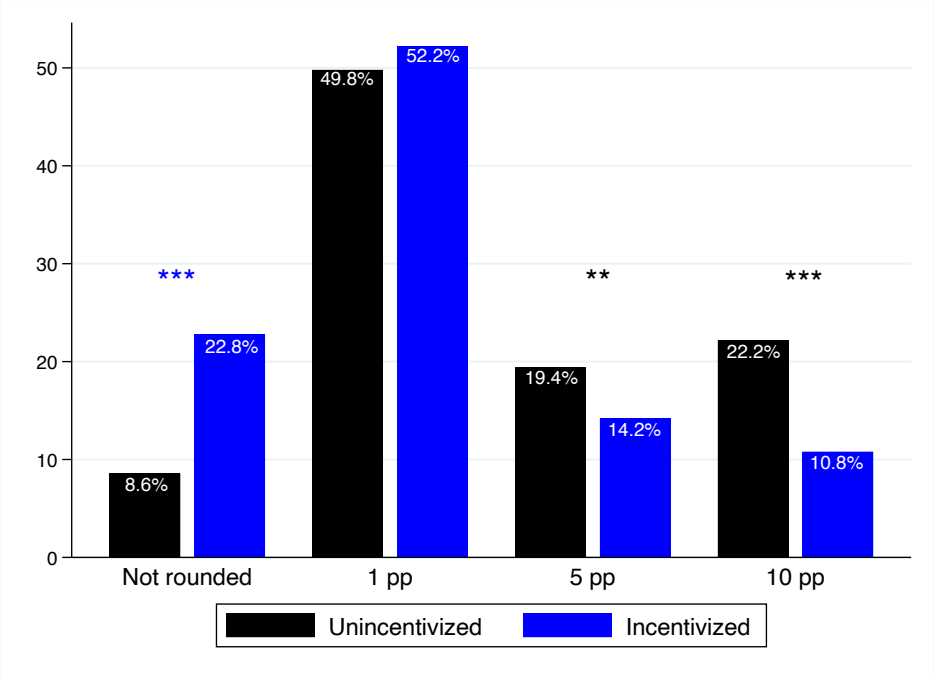


Figure 7: Shares of Forecasts By Rounding Behavior

Notes: This figure shows the share of participants who rounded their forecasts by incentive group. Stars indicate significance levels from tests of equality of proportions: $^{*}\,p < 0.10$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

Table 8: The Probability of Rounding in Inflation Expectations

	(1)	(2)
Incentivized	-0.142^{***}	-0.149^{***}
	(0.023)	(0.022)
Controls	No	Yes
N	1,000	1,000

Robust standard errors in parentheses.
$^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$

Notes: Table reports marginal effects from Probit regressions with robust standard errors. Rounding is defined as any rounding behavior to the nearest 1, 5, or 10 pp. Point forecasts are winsorized at the 1st and 99th percentiles before rounding classification. Column (1) includes no controls, while column (2) controls for age, sex, education, employment status, primary shopper status, and whether a respondent earns above or below median income.

These results contribute to the literature linking rounding to uncertainty and effort. Binder (2017) shows that rounding can proxy for forecast uncertainty, while McMahon et al. (2025) use incentivized forecasting experiments to show that both individual-level uncertainty and the complexity of the forecasting environment causally affect rounding behavior.

3.4 Effect of Incentives in Information Provision Experiments

We now assess how the timing of marginal incentives affect beliefs updating in a simple information provision experiment. As discussed in Section 2.1, the sign of incentive effects on responsiveness is clear when incentives apply only to the posterior report. We therefore treat the comparison of *Post* to *Flat* as the primary test of Hypothesis 2. Because incentives may affect both attention to the signal and the strength of prior beliefs, the net effect on measured learning rates in treatments *Prior* and *Both* relative to *Flat* is theoretically ambiguous.

Recall that after eliciting a point expectation for one-year-ahead inflation, each participant received the FOMC's outlook for 2025 and then reported a density forecast – their updated inflation expectation ("posterior").[23] We use the same bin-based elicitation strategy for density forecasts as the one employed by the New York Fed's SCE.[24] Figure 8 provides a visualization of the impact of incentives on posterior beliefs and shows the average weight assigned to each of the ten possible inflation bins for posterior-incentivized participants (blue solid line) and those who were not (black solid line). Incentives shift more weight toward the bin containing both the Fed's inflation target and the signal.

To quantify the effect of incentives on updating, we follow Haaland et al. (2023) and estimate *learning rates*, which capture the extent to which respondents adjust their forecasts in response to the signal:

$$\text{Updating}_i = \beta_0 + \sum_j \beta_{1,j} \text{Treatment}_{i,j} \times \text{PercGap}_i + \sum_j \beta_{2,j} \text{Treatment}_{i,j} + \beta_3 \text{PercGap}_i + \epsilon_i \quad (2)$$

where Updating_i is the distance between respondent i's posterior and prior one-year-ahead inflation expectation, and PercGap_i (Perception Gap) is the distance between the FOMC's forecast of median PCE inflation in 2025 and respondent i's prior. Again, $\text{Treatment}_{i,j}$ $j \in \{Post, Both, Prior\}$ indicates the incentive structure a participant faced. The key parameter, β_1, measures how incentives alter responsiveness to the signal relative to *Flat*.

[23] We compute posterior means using bin centers, assigning ±12% to open-ended bins, and use this measure throughout the analysis. We depict these expectations in Figure A-1 and report treatment effects on these expectations in Table A-9 (both in Appendix A1). Consistent with D'Acunto et al. (2023) for the SCE, we observe that density inflation forecasts exhibit lower disagreement and lower mean expected inflation than point forecasts. Alternatively, fitting a beta distribution and using the implied mean is another common method in the literature. However, D'Acunto et al. (2023) shows that it yields quite similar results, so we choose the simpler method.

[24] Becker et al. (2023) provide evidence that the number, center, and width of bins can meaningfully influence respondents' expectations. Although beyond the scope of this paper, future research could explore whether and how this interacts with marginal incentives.

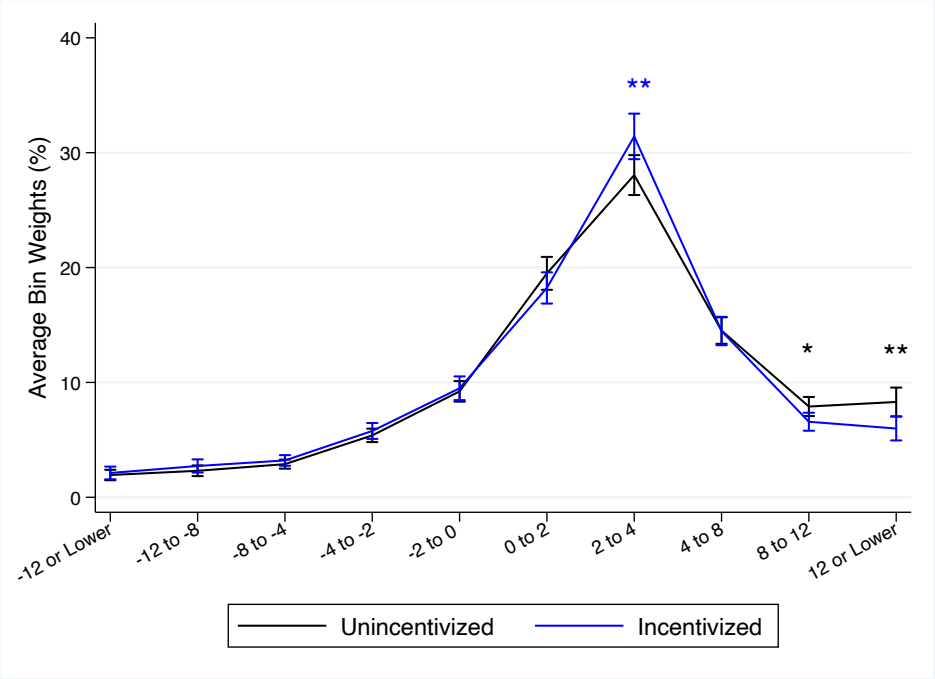


Figure 8: Average Bin Weights Across Incentives

Notes: This figure shows the average weight participants placed into the respective bins, distinguishing between posterior-unincentivized (*Flat* and *Prior*) and posterior-incentivized (*Post* and *Both*) treatments. Data are winsorized at the 1% and 99% levels. Stars denote significant differences in weights assigned to a bin, on average, between posterior incentivized and unincentivized treatments. Blue stars indicate posterior incentivized subjects placed more weight into that bin, on average, and black stars the opposite. Significance levels are indicated as follows: $^{*}\, p < 0.10$; $^{**}\, p < 0.05$; $^{***}\, p < 0.01$.

β_2 captures the average treatment effect on respondents' beliefs that does not depend on individual priors, and β_3 measures the extent to which changes in beliefs depend on the perception gap.[25]

Table 9 provides the main test of Hypothesis 2. The results show that the effect of incentives on measured learning rates depends on the stage of the elicitation at which they are applied. When incentives apply only to posterior forecasts (*Post*), responsiveness to the signal increases significantly relative to the *Flat* treatment, consistent with the hypothesis. Specifically, relative to Flat, incentives in *Post* increase revision magnitudes: for each additional 1 pp gap between the prior and the signal, the absolute posterior revision is 0.043 pp larger.[26] The estimates for *Both* and *Prior* are smaller and statistically weaker, consistent

[25] Haaland et al. (2023) argue that if priors are balanced across treatments, researchers can use the posterior as a dependent variable (see Coibion et al. (2022) for an example). Because our treatments induce systematic differences in priors, this approach is not suitable as our primary econometric specification. Nevertheless, we explore this alternative specification in Appendix A4, and obtain results qualitatively identical to those from our main specification. An additional advantage of this approach is that it does not require assumptions about the signal.

[26] Since all groups receive the same information signal, β_1 captures the extent to which incentivized participants update their beliefs toward the signal, relative to the unincentivized group (*Flat*).

with the mechanism discussed in Section 2.1: when incentives are present before the signal, they strengthen the priors, reducing subsequent revisions. This interpretation is in line with the findings of Weber et al. (2025), who show that respondents are more attentive in high-inflation environments – where inflation information is more salient and readily available – leading to stronger priors and weaker responses to additional information. In our setting, incentivizing the prior increases the returns to being attentive and therefore similarly reduces responsiveness to the information treatment.

Table 9: Effect of Incentives on Updating

	(1)	(2)
Post × PercGap	0.037***	0.043***
	(0.011)	(0.011)
Both × PercGap	0.020*	0.020*
	(0.011)	(0.012)
Prior × PercGap	-0.032**	-0.014
	(0.014)	(0.014)
Post	0.116	0.258
	(0.246)	(0.250)
Both	-0.393	-0.427*
	(0.244)	(0.248)
Prior	0.115	0.087
	(0.244)	(0.248)
PercGap	0.915***	0.925***
	(0.007)	(0.007)
Constant	0.210	0.122
	(0.173)	(0.867)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses.
* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of incentives on learning rates. These are relative to our baseline treatment *Flat*. Regressions are estimated by Huber-robust regression. Point forecast data are winsorized at the 1% and 99% levels. Column (2) includes controls for individual-level characteristics such as age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state of residence.

Because priors and posteriors are elicited using different formats (point vs density forecasts), part of the measured prior–posterior revision may reflect an elicitation-format artifact rather than signal-driven updating. To account for this, we quantify the importance of marginal incentives for belief updating with an illustrative decomposition exercise in Appendix A5 that isolates the net signal-driven component. This exercise suggests that, after netting out the mechanical component, marginal incentives increase signal-driven updating more than threefold.

Taken together, these results highlight that incentives materially affect measured learning rates. Incentivizing posterior forecasts significantly increases responsiveness to central bank signals. This is especially noteworthy given the inflationary environment at the time of our survey, which likely made inflation particularly salient (Weber et al. 2025; Bracha and Tang 2024). This has direct implications for the ability of central bank forecasts to coordinate and guide inflation expectations.

This finding also suggest that unincentivized RCTs may thus underestimate learning if participants undervalue the benefits of reporting accurate forecasts, whereas incentivized RCTs capture learning among individuals who might otherwise ignore the signal. Hence, our estimated learning rates can be viewed as an upper bound on the potential effect of information provision.

More broadly, the findings underscore that incentives, much like in laboratory experiments, can meaningfully alter survey-based information provision results. While many studies have produced meaningful and important results without incorporating incentives, an increasing body of research shows that both endogenous motivations (Piccolo and Gorodnichenko 2025) and economic environments (Pfäuti 2023, Wabitsch 2024, Weber et al. 2025, Link et al. 2025) dynamically shift household attention to inflation, making the effects of information treatments potentially time- and sample-dependent. Introducing marginal incentives offers an additional layer of experimental control, helping reduce extreme responses, lower variance, and improve cost-effectiveness. We view this not as a critique of information-provision RCTs per se, but as an interpretation and measurement point: unincentivized RCTs identify belief updating under low-stakes, potentially low-attention reporting. Introducing marginal incentives provides a benchmark for updating when respondents exert greater effort, helping distinguish limited learning from limited attention/noisy measurement.

4 Do Incentives Improve Expectations Data Quality?

A natural question is whether marginal incentives bring us closer to the beliefs households use to guide their behavior and make important decisions. This matters because in macroeconomic models, economic expectations affect economic decisions (e.g., spending, saving, investment, price- and wage-setting) that aggregate into the dynamics that monetary policy seeks to manage. Crucially, this issue extends to standard unincentivized surveys and may be particularly salient in these settings. Without accuracy stakes, (some) respondents have weaker incentives to invest effort in reporting beliefs, increasing the scope for noise or errors. In our setting, it is useful to distinguish two mechanisms through which incentives may affect reported expectations. First, incentives may reduce cognitive errors and survey noise by

increasing attention, effort, and deliberation based on respondents' pre-existing knowledge (the *internal deliberation channel*). Second, incentives may prompt external information acquisition (e.g., online search), which can change beliefs during the survey (the *external information acquisition channel*). Deliberation and recall of existing knowledge can in principle affect a respondent's stated expectations even without new external information. We treat such changes as part of the internal deliberation channel because they reflect increased introspection and reduced cognitive error in reporting expectations from a respondent's pre-existing information set. Presumably they also better capture the beliefs they would use in making decisions they care about.

We present several pieces of evidence that incentives improve the reliability and coherence of the elicited expectations, and in support of the internal deliberation channel as a primary channel. Beliefs that are more predictive of planned behavior provide a higher-quality measure of the expectations that matter for policy. Below, we show that incentivized expectations are more strongly associated with both nominal and real spending changes. Beyond the relationship between inflation expectations and spending, we also evaluate data quality through assessing whether incentives improve the internal consistency of beliefs, whether their effects extend to longer-run expectations that are central for policymakers, and whether our findings simply reflect respondents looking up information online (external information acquisition channel) rather than exerting greater effort.

We explore these questions in a follow-up wave of our experiment that excluded the information-provision intervention but added modules on spending behavior and information search. This design allows us to compare incentivized and unincentivized forecasts along dimensions of informativeness, consistency, and external validity, in a way directly comparable to the SCE. We provide details in Appendix A6.

Spending Inflation expectations may matter for a range of economic outcomes; here, we focus specifically on their relationship with spending. In this context, we ask whether incentivized inflation expectations provide a more meaningful and behaviorally relevant measure of beliefs. We use respondents' spending plans as an external validity check for our inflation expectation measures. If reported inflation expectations capture the beliefs households actually use when planning future expenditures, they should be systematically related to expected spending over the same horizon. In particular, they should covary with both expected nominal spending growth and, to the extent that households anticipate adjusting quantities, with intended real spending changes. A stronger relationship under incentives would therefore indicate that incentivized inflation expectations more closely reflect behaviorally relevant beliefs rather than reporting noise. Spending is a primary margin through

which expectations translate into aggregate demand, making the relationship between inflation expectations and spending plans a natural validation exercise.

For nominal spending, the logic is straightforward: expected nominal spending growth and expected inflation share a nominal component, so their positive comovement provides a simple coherence check. For real spending, the interpretation is more nuanced, as the empirical evidence is mixed and there are offsetting channels at play, such as intertemporal substitution, income effects, and uncertainty (e.g., see Jiang et al. 2024).[27] Further, we used a question from the New York Fed SCE to elicit spending. Specifically, we asked respondents about the percentage increase or decrease in total household spending: "By about what percent do you expect your total household spending to increase/decrease? Please give your best guess. Please enter a number greater than 0 or equal to 0. Over the next 12 months, I expect my total household spending to increase/decrease by ___", where total household spending was defined as "including groceries, clothing, personal care, housing (such as rent, mortgage payments, utilities, maintenance, home improvements), medical expenses (including health insurance), transportation, recreation and entertainment, education, and any large items (such as home appliances, electronics, furniture, or car payments)". Because both inflation expectations and spending plans are measured over the same 12-month horizon, the standard Euler-equation prediction of intertemporal substitution—where higher expected inflation shifts consumption from the future to the present—does not directly apply in our setting. Instead, higher expected inflation can mechanically increase expected nominal spending through anticipated price changes, even if real consumption remains unchanged. The relationship with real consumption is more ambiguous, as intertemporal substitution operates within the horizon.

However, to better understand whether individuals would also make changes to real consumption, we followed up with an additional question (not asked by the New York Fed SCE): "You mentioned that you expect your household spending to increase/decrease. Is this increase/decrease primarily because: [Options: You plan to purchase more/less goods and services; The prices of the goods and services you buy are rising/going down; You plan to purchase higher/lower quality goods and services that cost more/less; Other reasons (please specify)]. Please choose all that apply." Table 10 shows that inflation expectations correlate positively with spending plans, with the association generally larger and more precisely es-

[27] A positive relationship between inflation expectations and spending (or spending growth) is a central empirical and policy-relevant channel that has received substantial attention in recent work; see D'Acunto et al. (2022), Burke and Ozdagli (2023), Duca-Radu et al. (2021), Binder and Brunet (2022), Coibion et al. (2022), and Ichiue and Nishiguchi (2015). However, the overall evidence on the relationship between inflation expectations and real spending is mixed. Recent work by Jiang et al. (2024) suggests that around 70% of households would not change their consumption in response to higher inflation expectations, 25% would decrease it, and the remaining 5% would increase it.

timated under incentives. This pattern is consistent with incentivized inflation expectations providing a more accurate measure of the beliefs households use when forming spending plans. As expected, the relationship is stronger for nominal spending, namely when we restrict the sample to respondents who link changes in expected spending to future higher prices. Taken together, these findings provide further evidence that incentives primarily reduce reporting error in inflation expectations. Consistent with this mechanism, incentivized respondents are also more likely to rationalize their spending forecasts in terms of planned quantity adjustments (see Figure 9), which provides suggestive evidence that incentives improve the behavioral relevance of reported inflation expectations not only with respect to nominal spending, but also with respect to the real component of spending decisions.

Table 10: Correlating Inflation Expectations to Spending

	All		**Price Factor**	
	(1)	(2)	(3)	(4)
	Unincentivized	Incentivized	Unincentivized	Incentivized
Point forecast	0.113	0.218*	0.169**	0.288***
	(0.079)	(0.115)	(0.0749)	(0.109)
Constant	18.30***	-2.068	19.79**	-4.426
	(6.856)	(3.311)	(9.035)	(2.870)
Controls	Yes	Yes	Yes	Yes
N	257	257	178	167

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table presents the relationship between expected inflation (point forecasts) and expected spending (nominal). The first two columns show the results for the entire dataset. The other two columns show results for the case then we restrict the sample to only those observations where respondents associate change in spending with changes in future prices. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels. We control for these individual-level characteristics: age, race, gender, education, income, political affiliation, primary grocery shopper status, and state of residence.

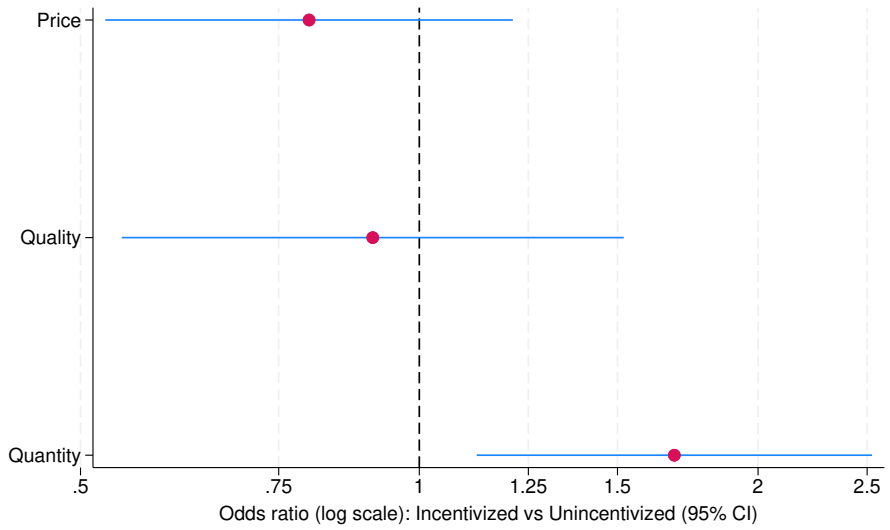


Figure 9: Incentives and the Reported Drivers of Spending Expectations

Notes: The figure plots odds ratios (log scale) from logit regressions for selecting expected spending-change drivers (Price/Quality/Quantity) in the Incentivized group relative to the Unincentivized group. Whiskers indicate 95% confidence intervals; the dashed vertical line at 1 denotes no difference between groups. Controls include demographics and state fixed effects. We use data from the follow-up survey described in Appendix A6.

Consistency A well-known feature in survey data is divergence between point and density forecasts. Because our follow-up wave did not provide additional information linking the two, we can use it to assess how marginal incentives affect this forecast consistency issue. In Table 11, we show that incentivized expectations exhibit greater internal consistency, as reflected in a smaller gap between the point forecast and the mean or median estimated from the elicited density forecast.[28] This indicates more coherent reporting under incentives.

Table 11: Consistency Between Point and Density Forecasts

	Abs Distance to Mean		Abs Distance to Median	
	Mean	**Std. Dev.**	**Mean**	**Std. Dev.**
Unincentivized	9.84	16.86	9.82	16.90
Incentivized	3.63	7.79	3.61	7.81

Notes: This table shows the absolute distance between participants' point forecasts and the mean or median of their density forecasts, where there was no information provision intervention between the two forecasts. Point forecasts are winsorized at the 1% and 99% levels.

Long-term expectations Policymakers care strongly about whether expectations remain anchored in the medium- and long-term, yet incentivizing expectations in three years might be administratively impractical. Although only short-run forecasts were incentivized, we

[28] To estimate mean and median of the density forecast, we follow the methodology used in the SCE by Armantier et al. (2017), which builds on Engelberg et al. (2009).

observe positive spillovers to longer horizons. Unincentivized three-year-ahead expectations were lower, less dispersed, and more tightly anchored among participants who faced short-run incentives (Table 12). We interpret spillovers to non-incentivized horizons as evidence that incentives can affect responses beyond the directly incentivized question. Importantly, such spillovers are consistent with the interpretation that incentives may activate a higher-effort reporting mode whose cost is paid once and then applied to subsequent questions at low marginal cost, reducing reporting noise. This suggests that incentivizing near-term beliefs can improve the quality of longer-term expectations as well.

Crucially, the existence of spillovers also speaks to the nature of the incentive effect itself: a respondent strategically reporting to match the incentivized measure of realized inflation would have no reason to revise their unincentivized three-year-ahead answer, since no payment depends on it. That three-year expectations shift thus suggests the effect operates mostly through genuine belief reporting rather than strategic forecast-matching. We do not claim that strategic behavior is entirely absent. We document that, among incentivized respondents, long-term forecasts differ noticeably between those who searched for additional information and those who did not, and are closer to the long-term forecasts of the unincentivized non-searchers (see Table A-11). However, this strategic component is too small to materially affect our main results. Interestingly, long-term forecasts are remarkably high among unincentivized respondents who searched, providing additional evidence that incentives shape the information acquisition process.

Table 12: Summary Statistics and Variance Comparison of Long-Run Inflation Expectations

	Mean	**Std. Dev.**	**N**
Unincentivized	6.95	19.15	257
"Incentivized"	4.24	14.61	257
All Data	5.60	17.07	514

Test for Equality of Means and Variances

Test Type	**Test Statistic**	**p-value**
Welch's t-test (Difference in Means)	-1.80	0.0721
Levene's Test (Mean)	10.16	0.0015
Levene's Test (Median)	6.74	0.0097
Levene's Test (Winsorized Mean)	7.36	0.0069
F-Test (Variance Ratio)	0.5822	0.0000

Notes: This table shows mean and variances of the elicited three-year-ahead point forecasts. All three-year-ahead forecasts were unincentivized. Instead, *Incentivized* refers to the one-year-ahead point forecast that a respondent faced.

External Information Acquisition Another concern is that incentive schemes may prompt respondents to search for additional information. This relates directly to channel (b): external information acquisition that can change beliefs during the survey. Such search could reflect payoff-motivated behavior in an online setting (a valid concern for any

incentivized belief elicitation conducted online; e.g., Grewenig et al. 2022), but it could also reflect genuine information acquisition that makes beliefs more informed.[29] However, we find little evidence in support of these channels. Only 8–15% of respondents reported searching for inflation information, and although incentivized treatment groups exhibit a higher rate (Figure 10), the magnitude remains economically small.[30] Further, the mean and standard deviations are very similar if we exclude respondents who reported searching for additional information (Table 13). Thus, external search alone does not account for the broad differences we document, even though it may contribute for a subset of respondents (Table A-11). In addition, our result on RCTs in Section 3.4 shows that even when the prior belief has already been incentivized, participants in the *Both* treatment group still learn from the provided information – something they should not do if they had already looked up the forecast information. Finally, if the aim is to elicit informed beliefs, or beliefs of households that are due to make important consumption decisions, then information acquisition may be desirable.

Table 13: Incentivized Group: Inflation Expectations by Search Behavior

	Mean	Std. Dev.	N
Searched and not searched	3.76	10.27	257
Not searched	3.81	10.66	218

Test for Equality of Means and Variances

Test Type	Test Statistic	p-value
Welch's t-test (Difference in Means)	-0.05	$p > .1$
Levene's Test (Mean)	0.17	$p > .1$
Levene's Test (Median)	0.17	$p > .1$
Levene's Test (Winsorized Mean)	0.24	$p > .1$
F-Test (Variance Ratio)	1.0771	$p > .1$

Notes: This table reports the mean and variance of one-year-ahead inflation expectations among incentivized respondents, by search behavior. The tests evaluate differences in means and variances between the full incentivized sample and the subsample of respondents who reported not searching for additional information when answering the survey questions. We use data from the follow-up survey described in Appendix A6. Data are winsorized at the 1% and 99% levels.

Incentives must be carefully designed. Overly complex schemes can confuse respondents and reduce truthful reporting (Danz et al. 2022).[31] In Drobot et al. (2025), we compare the simple

[29] Grewenig et al. (2022) find that providing incentives does not impact beliefs about personal earnings – which are readily available to participants – but improves beliefs about average public school spending, a less accessible piece of information for the average respondent. The authors highlight a trade-off between increased respondent effort and the risk of inducing online search activity when incentivizing beliefs in online surveys.

[30] After the experiment, we asked respondents whether they looked up external information, assuring them that their answer would not impact their payoff in any way.

[31] They find that truthful reporting increases when information about incentives is absent compared to a baseline condition that provides full details about how incentives are determined using a binarized scoring rule (BSR). This suggests that overly complicated incentive mechanisms may confuse participants, undermining the very accuracy they are intended to enhance.

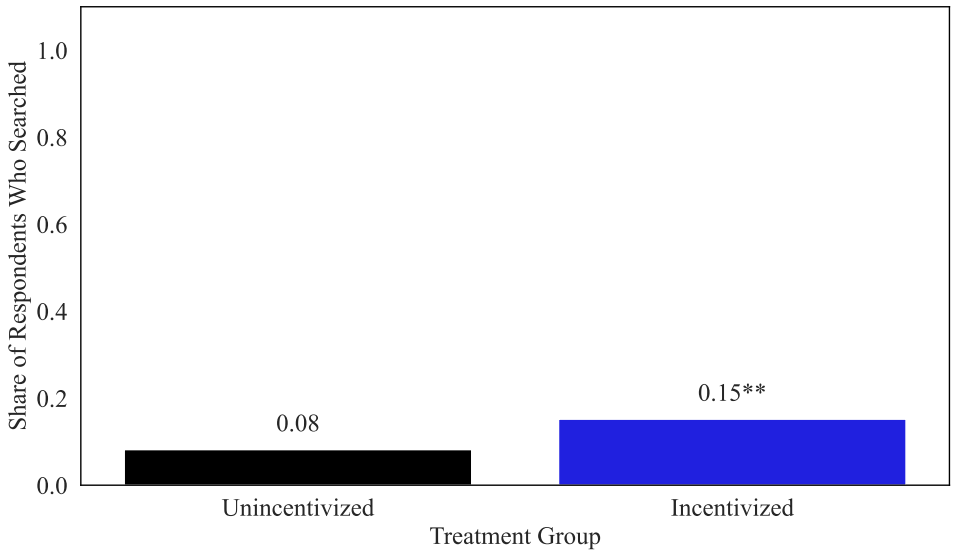


Figure 10: Information Search by Treatment Group

Notes: This figure shows the share of respondents who reported searching for information about inflation online, broken down by treatment group. Respondents who answered "No" to the search question were coded as not having searched; all others were considered to have searched. Specifically, we asked "In providing your estimate for the inflation rate over the next 12 months, did you consult any source?" The vertical axis represents the proportion of searchers within each group. Asterisks indicate the statistical significance of the difference (z-test). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

incentives we employed in this paper with more complex, incentive-compatible schemes, and find evidence that simpler incentives are more effective in eliciting more consistent inflation expectations. This echoes Danz et al. (2022), but in the context of inflation expectations.

Overall, while marginal incentives can improve data quality by motivating participants to invest more effort and report more consistent beliefs, careful consideration must be given to the design of these incentives. Simplicity and transparency are crucial to avoid inducing strategic behavior or confusion that could compromise the integrity of the data. Future studies should consider integrating such mechanisms to improve data quality also in the context of other macroeconomic expectations and correlating incentivized and unincentivized beliefs with relevant tasks, to better understand which are more informative and in which settings.

We acknowledge that in some cases balancing the benefits of increased effort and accuracy against the potential impracticality or difficulty of implementing incentives, or the risks of strategic behavior and misunderstanding, is essential for advancing survey-based measures of economic expectations and informing theoretical models, quantitative assessments, as well as more effective policy decisions. However, our findings underscore that it would be unwise to disregard decades of research in experimental economics. Whenever feasible, our recommendation is in line with the views expressed by Holt and Smith (2016): *"In the absence*

of a reliable set of criteria to determine when incentives matter and when they do not, it seems prudent to use incentives."

5 Model and Policy Implications

Our findings have implications for how survey data, when collected under different incentive schemes, affect the conclusions drawn from integrating expectations data into economic models and policy decisions.

We consider two implications here. First, we discuss our results about upward bias and cross-sectional disagreement within the context of a growing literature focused on how each can distort policy transmission. Second, we calibrate a simple, three-equation New Keynesian model to our experimental data to illustrate how differences in the degree of backward-lookingness shape the propagation of shocks and corresponding policy decisions. This exercise is intended as an illustrative calibration rather than a structural estimation. The goal is to demonstrate how differences in measured expectations induced by survey design can translate into different quantitative implications in standard macroeconomic models.

5.1 Upward bias and cross-sectional disagreement

A growing body of work demonstrates that the degree of disagreement among households and forecasters is not merely a statistical nuisance but a structural determinant of monetary policy transmission. Falck et al. (2021) show that in high-disagreement states, a contractionary monetary policy shock can perversely raise inflation by up to 0.7 percentage points, whereas in low-disagreement states the same shock reduces inflation by approximately 0.8 percentage points. Barbera et al. (2023) decompose this further, showing that it is specifically cyclical inflation disagreement that undermines monetary policy efficacy, with trend disagreement playing a lesser role. Dong et al. (2024) extend these findings to household expectations, demonstrating that disagreement weakens the power of both conventional rate changes and forward guidance: agents who perceive higher future inflation face lower perceived real rates and borrow more aggressively, hitting borrowing constraints and rendering aggregate consumption less responsive to policy rate changes.

We show that marginal incentives reduce cross-sectional disagreement by one-third (the standard deviation of point forecasts falls from 23.78 to 16.98) and lower mean expectations from 6.13% to 2.73%. If the incentivized distribution more closely reflects more thoughtful beliefs which presumably guide economic behavior, as our evidence suggests, then unincentivized

surveys systematically overstate both the level and the dispersion of inflation expectations.

Consider the policy implications of this mismeasurement through the lens of the findings above. A policymaker who calibrates their assessment of the economy using unincentivized survey expectations would perceive a high-disagreement, high-upward-bias regime. Under such conditions, contractionary monetary policy risks being counterproductive: tightening may fail to reduce inflation or, worse, raise it (Falck et al. 2021, Barbera et al. 2023). Forward guidance would appear less effective, as high disagreement dampens the consumption response to changes in expected future rates (Dong et al. 2024). Faced with this diagnosis, the rational policy response would be to resort to larger, more aggressive interventions because the perceived transmission mechanism appears impaired.

If, however, the true state of household beliefs more closely resembles what our incentivized treatments reveal, then this policy response constitutes a systematic overreaction. In the incentivized regime, standard transmission channels function as intended: rate increases reduce inflation, and forward guidance retains its potency. The distortionary cost of policy enacted on the basis of mismeasured expectations operates along three margins. First, the upward bias in the level of expectations (3.4 pp) can lead policymakers to perceive inflation expectations as de-anchored, triggering unnecessarily hawkish responses that depress output. Second, the overstated disagreement shifts the perceived economy into a regime in which conventional policy tools are weakened, prompting reliance on blunt, outsized interventions when more measured adjustments would suffice.

A third dimension is that greater apparent backward-lookingness implies excessive inflation persistence in model-based assessments, leading to prolonged restrictive policy stances beyond what the true expectation dynamics warrant. We dig more deeply into this concern in the following section (Section 5.2).

5.2 Heuristics and inertial inflation

We illustrate this by examining how different degrees of backward-lookingness – calibrated to match the correlation between perceived and expected inflation in our experimental data – shape the propagation of shocks in a standard three-equation New Keynesian (NK) model. In our model, agents form expectations as a weighted combination of backward-looking and rational expectations, which are model-consistent. We simulate the model under two regimes of expectations formation: (i) a heuristic rule calibrated to expectations under marginal incentives, and (ii) a heuristic rule calibrated to expectations without incentives.

5.2.1 Model Set-Up

The model consists of a NK Phillips Curve, a dynamic IS curve, and a Taylor-type monetary policy rule. The model includes two structural shocks: a demand shock u_t and a cost-push shock v_t.

$$\pi_t = \beta\,\mathbb{E}_t[\pi_{t+1}] + \kappa y_t + v_t \qquad \text{(New Keynesian Phillips Curve)} \tag{3}$$

$$y_t = \mathbb{E}_t[y_{t+1}] - \frac{1}{\sigma}(i_t - \mathbb{E}_t[\pi_{t+1}]) + u_t \qquad \text{(IS Curve)} \tag{4}$$

$$i_t = \phi_\pi \pi_t + \phi_y y_t \qquad \text{(Taylor Rule)} \tag{5}$$

The shocks follow AR(1) processes:

$$u_t = \rho_u u_{t-1} + \varepsilon_t^u, \quad \varepsilon_t^u \sim \mathcal{N}(0, \sigma_u^2) \tag{6}$$

$$v_t = \rho_v v_{t-1} + \varepsilon_t^v, \quad \varepsilon_t^v \sim \mathcal{N}(0, \sigma_v^2). \tag{7}$$

Expectations are a convex combination of rational and backward-looking components:

$$\mathbb{E}_t[\pi_{t+1}] = \eta \pi_{t+1}^{RE} + (1-\eta)\pi_{t-1}. \tag{8}$$

Here, π_{t+1}^{RE} denotes the model-consistent (rational) forecast of future inflation, while π_{t-1} represents a naive backward-looking expectation. The parameter $\eta \in [0,1]$ governs the degree of forward-lookingness: $\eta = 1$ yields fully rational expectations, whereas $\eta = 0$ corresponds to a purely backward-looking heuristic.

Calibration

To calibrate η, we use data from our experiment. For each treatment group, we compute the correlation between participants' point forecast of inflation ($e_{\pi,t}$) and their inflation perception ($p_{\pi,t-1}$). We then map these correlations into values of η using a simple linear approximation:

$$\eta \approx 1 - \text{Corr}(e_\pi, p_{\pi,t-1}). \tag{9}$$

Table 14 shows treatment-level calibrations. Incentivized treatments (*Prior*, *Both*) together yield $\eta \approx 0.75$, while unincentivized treatments (*Flat*, *Post*) together yield $\eta \approx 0.60$. These values guide our simulations, alongside standard NK parameters listed in Table 15.

Table 14: Empirically Implied Calibration of η

Treatment	**Corr**$(e_\pi, p_{\pi,t-1})$	**Implied** $\eta \approx 1 - \text{Corr}(e_\pi, p_{\pi,t-1})$
Flat	0.373	0.627
Post	0.421	0.579
Both	0.283	0.717
Prior	0.188	0.812

Notes: This table shows the empirical correlation between participant forecasts and inflation perceptions by treatments, and implied calibration of η. Treatments *Flat* and *Post* are unincentivized, while *Both* and *Prior* are incentivized.

Table 15: Model Parameter Values

Parameter	**Description**	**Value**
β	Discount factor	0.99
σ	Intertemporal elasticity of substitution	1
ϕ_π	Taylor rule response to inflation	1.5
ϕ_y	Taylor rule response to output	0 or 0.5
κ	Slope of the Phillips Curve	0.104
ρ_u	Persistence of demand (IS) shock	0.841
ρ_v	Persistence of cost-push shock	0.841
σ_u	Std. dev. of demand shock	1
σ_v	Std. dev. of cost-push shock	1
η	Weight on rational expectation	$\{0.75, 0.6\}$

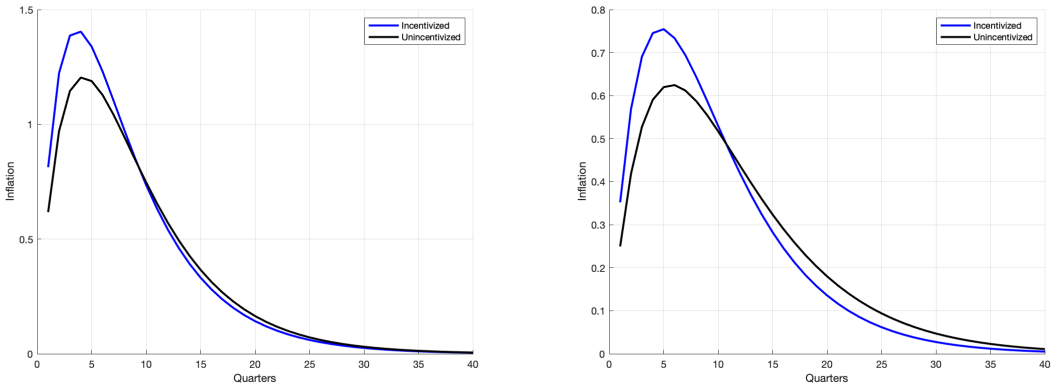
Notes: This table shows the model parameters used for the demand and cost-push shocks. The three values of η correspond to the incentivized and unincentivized expectations regimes, respectively. We let $\phi_y = .5$ for the demand shock simulation and $\phi_y = 0$ for the cost-push shock. We choose these values to align with standard calibrations of the New Keynesian model (Galí 2015).

5.2.2 Results and Implications

Figure 11 displays the impulse response of inflation to a one-standard-deviation cost-push shock in panel (a), and an analogous demand shock in panel (b), under two different expectations regimes. The blue and black solid lines correspond to heuristic expectations calibrated to match the degree of backward-lookingness observed in the incentivized and unincentivized treatments, respectively. This figure shows that even modest increases in backward-lookingness substantially alter the dynamics of inflation. Notably, the unincentivized treatment – which exhibits the greatest reliance on lagged inflation – produces the more persistent inflation path, with elevated inflation lasting longer than in the incentivized case.

These findings highlight two critical methodological and policy-relevant implications that arise from how we measure expectations.

First, for research: Empirical analyses of inflation expectations must account for the elicitation method. Reliance on unincentivized survey data may overstate persistence by attributing inflation dynamics to sluggish, backward-looking expectations rather than structural



(a) Response to cost-push shock **(b)** Response to demand shock

Figure 11: Inflation Dynamics under different expectation regimes

Notes: This figure shows impulse response functions for inflation following a one-standard-deviation cost-push shock (left) and demand shock (right) under different expectation regimes.

mechanisms such as price rigidity. This risks attenuating the perceived role of underlying frictions in macroeconomic models and thereby distorting our understanding of inflation dynamics.

Second, for policy: There are substantial policy implications arising from this mismeasurement of expectations. Calibrating models to unincentivized expectations may infer excessive inertia in inflation, prompting overly aggressive or prolonged policy interventions. Under incentives, however, households appear to be more forward-looking. Thus, the way expectations are measured directly affects monetary policy decisions.

6 Conclusion

Our experiment shows that marginal incentives fundamentally shape the elicitation of inflation expectations. Incentives produce forecasts that are lower, less extreme, and less dispersed, bringing household expectations closer to those of professional forecasters. They also increase learning from information provision, reduce reliance on simple heuristics, and narrow systematic biases such as the gender gap in expectations. We find strong evidence that these results are driven primarily by increased attention, which reduces reporting error, rather than by information acquisition. Our findings highlight that survey design choices can materially affect measured expectations. Incentives do not necessarily reveal a unique 'true' belief, but they provide a benchmark for expectations reported under higher attention and effort.

These findings have important implications. For researchers, they suggest that unincen-

tivized surveys may misrepresent household expectations. For policymakers, they highlight that survey design directly affects the beliefs used to calibrate policy rules. Incentivized surveys provide a more reliable measure of expectations, improving both empirical inference and the credibility of policy analysis.

Our results support rational inattention models, showing that even modest, structural changes to payment schemes that do not increase participant total payments alter attention, effort, and subsequently reported beliefs. Without incentives, the measured impact of information treatments may vary with the economic environment and with respondents' baseline attention to inflation (see Weber et al. (2025)). By introducing appropriate incentives, we can approximate environments with high or low attention, allowing for a more controlled evaluation of information treatments. These findings suggest central banks could improve communication effectiveness by either lowering cognitive costs (e.g., simplified messaging) or raising the perceived benefits of accurate expectations.

Incorporating incentives into survey-based macroeconomic research may improve the informativeness of elicited beliefs, offering a valuable complement to the commonly used unincentivized or flat-fee structures. The substantial reduction in potential forecast errors and heightened learning rates observed with marginal incentives indicate that incentivized elicitation might provide a more reliable measure of household expectations, which are critical for understanding expectations' formation, economic modeling, and policymaking.

In conclusion, incorporating marginal incentives into macroeconomic survey design improves the quality of elicited beliefs, strengthening the reliability of empirical research and the credibility of quantitative macroeconomic models. By motivating participants to engage more deeply when forming and reporting beliefs, incentivized mechanisms yield data that are more reliable, consistent and informative.

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Appendix

A1 Other Tables and Figures

Table A-1: Incentive Structure by Treatment

	Prior	***Post***	***Both***	***Flat***
Immediately	\$2	\$2	\$2	\$2
Expected Earnings in 1Y	\$4	\$4	\$4	\$4
Structure	Accuracy-based: $\$10 \times 2^{-\lvert\pi-\mathbb{E}(\pi)\rvert}$	Accuracy-based: $\$10 \times P$	Accuracy-based: Prior or Post	Fixed fee, time-value matched

Notes: This table provides an overview of the payment composition (amount, timing and incentive structure) by treatment. The top row indicates the treatment group. P represents the probability weight a participant assigned to the bin that contains realized inflation. In the *Both* treatment group, either the prior or the posterior forecast is chosen at random for payment with equal probability.

Table A-2: Sample Comparisons: Across Groups and SCE

	Flat	**Prior**	**Post**	**Both**	**Full Sample**	**SCE Sample**
Age						
Under 30	18.8	17.2	17.6	14.4	17.0	11.7
30-39	26.8	26.0	28.4	26.8	27.0	19.0
40-49	25.2	24.0	26.8	24.0	25.0	18.8
50-59	15.2	18.8	14.4	18.0	16.6	20.6
60 or over	14.0	14.0	12.8	16.8	14.4	29.9
Gender						
Female	54.8	65.2	50.8	63.6	58.6	48.1
Male	44.8	34.8	49.2	36.0	41.2	51.9
Prefer not to say	0.4			0.4	0.2	
Income						
Less than \$50,000	48.8	43.6	39.2	38.4	42.5	42.8
\$50,000-\$99,999	30.0	34.0	36.4	39.6	35.0	34.5
\$100,000 or more	21.2	22.4	24.4	22.0	22.5	22.7
Race/Ethnicity						
Asian	6.0	7.2	7.6	8.0	7.2	3.5
Black	14.4	13.6	6.4	14.0	12.1	10.4
White	73.2	69.6	73.6	70.8	71.8	81.8
Other	6.4	9.6	12.4	7.2	8.9	4.4

Notes: This table reports respondent demographic shares and compares them with corresponding estimates from the Survey of Consumer Expectations (SCE). SCE benchmarks are taken from Armantier et al. (2017).

Table A-3: Median and IQR Comparison of Inflation Expectations

	Median	IQR	N
Unincentivized	2.7	8.3	500
Incentivized	2.0	6.0	500
All Data	2.0	7.0	1,000

Test for Equality of Medians and IQRs

Test Type	Test Statistic	p-value
Mann-Whitney U Test (Distributions)	112515.0	0.006
Mood's Test (Median)	1.024	0.312
Wald's Test (IQR)	-2.487	0.013

Notes: This table shows median and IQR of the elicited prior belief of inflation $\mathbb{E}(\pi_{Prior})$ by incentive treatments. *Unincentivized* is comprised of treatments *Flat* and *Posterior*, while *Incentivized* is comprised of *Both* and *Prior*. Data are winsorized at the 1% and 99% levels.

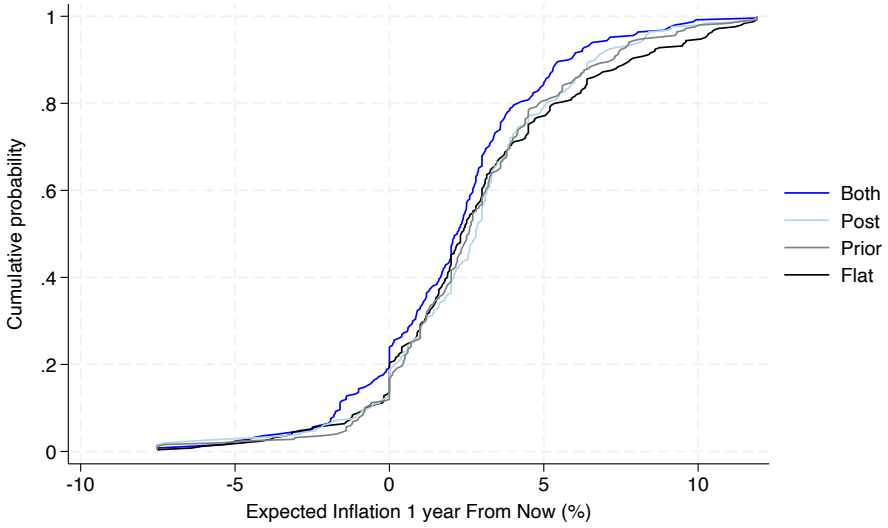


Figure A-1: CDFs of Inflation Expectations After Information Provision

Notes: This figure shows cumulative distribution functions (CDFs) of inflation expectations elicited after the information intervention. Expectations are shown by the different treatment groups and expressed in percentage points. Data are winsorized at the 1% and 99% levels.

Table A-4: Effects of Incentives on Inflation Expectations

	(1) $\lvert\mathbb{E}(\pi_{prior})\rvert$	(2) $\lvert\mathbb{E}(\pi_{prior})\rvert$	(3) EV	(4) EV
Flat			1.172***	1.504***
			(0.348)	(0.408)
Post	-1.770	-0.632	1.032***	1.703***
	(1.916)	(1.831)	(0.353)	(0.417)
Both	-5.268***	-5.883***	0.649*	0.816*
	(1.802)	(1.741)	(0.371)	(0.421)
Prior	-7.794***	-8.577***		
	(1.620)	(1.569)		
Constant	15.20***	11.95***	-2.987***	-3.023***
	(1.411)	(3.584)	(0.296)	(1.289)
Controls	No	Yes	No	Yes
Estimator	OLS	OLS	Logit	Logit
N	1000	1000	1000	877

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of treatments on reported inflation expectations (the priors), relative to the *Flat* treatment. Columns (1) and (2) present the results of the following regression: $|\mathbb{E}_i(\pi_{prior})| = \alpha + \sum_j \gamma_j Treatment_{i,j} + \beta \mathbb{X}_i + \epsilon_i$, where $j \in \{Post, Both, Prior\}$ denotes the incentive treatment groups, and $\mathbb{X}_i$ represents a vector of individual-level controls, including age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels. Columns (3) and (4) present the results of a logistic regression analyzing the relationship between treatment assignment and the likelihood of reporting an extreme forecast value (EV). Extreme values are defined as the highest 10% of absolute prior inflation expectations. *Prior* treatment group serves as the reference category.

Table A-5: Treatment Effects on Extreme Forecasts: Logistic Regression Results

Variable	**Coefficient**	**p-value**	**Odds Ratio**	**Interpretation**
Constant	-2.987***	0.000	0.050	Baseline probability of extreme forecast reporting is very low.
Both	0.649*	0.080	1.914	Respondents in *Both* group are 91% more likely to report an extreme forecast relative to *Prior* group, but the effect is only marginally significant.
Flat	1.172***	0.001	3.228	Respondents in *Flat* group are 222% more likely to report an extreme forecast (highly significant).
Post	1.032***	0.003	2.807	Respondents in *Post* group are 181% more likely to report an extreme forecast (highly significant).

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A-6: Incentives Effect Heterogeneity

Subgroup	No or Low		Yes or High		Difference Test	
	Effect	N	Effect	N	Diff	p-value
Primary decision-maker	-3.206	401	-3.428**	590	-0.222	0.933
	(2.026)		(1.716)		(2.655)	
Has investments	-5.184*	341	-2.498*	617	2.686	0.376
	(2.674)		(1.436)		(3.035)	
House buying	-4.841***	602	-2.868	140	1.973	0.655
	(1.664)		(4.087)		(4.413)	
High income	-4.261**	595	-2.387	405	1.874	0.464
	(1.829)		(1.791)		(2.560)	
High Education	-4.361**	491	-2.571	507	1.791	0.493
	(1.991)		(1.692)		(2.613)	

Notes: This table reports the mean difference in prior inflation expectations between incentivized (*Prior* and *Both*) and unincentivized (*Post* and *Flat*) respondents, separately for each subgroup. Effects are estimated as simple differences in means (Welch t-test), without controlling for additional covariates. *No/Low* denotes: not primary decision-maker, no investments, not actively buying a house, income below $70k, less than an undergraduate degree. *Yes/High* denotes the complement. Standard errors in parentheses. The difference column tests whether the treatment effect differs across subgroup values. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-7: Effects of Incentives on the Gender Expectations Gap (Without Control Variables)

	(1) Flat	(2) Post	(3) Both	(4) Prior
Female	9.403***	4.779*	2.685	1.082
	(2.956)	(2.836)	(2.213)	(1.634)
Constant	0.356	4.326***	0.932	2.099**
	(1.489)	(1.656)	(1.334)	(1.015)
Controls	No	No	No	No
N	249	250	249	250

Standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of treatments on reported inflation expectations (the priors) by gender. Regressions are estimated by OLS with robust standard errors without control variables. Data are winsorized at the 1% and 99% levels.

Table A-8: Summary Statistics and Variance Comparison of APE

	Mean	**Std. Dev.**	**Median**	**IQR**	**N**
Unincentivized - Male	10.43	19.18	2.8	6.7	235
Unincentivized - Female	23.48	28.03	9.8	30.0	265
Incentivized - Male	5.71	12.87	1.8	4.0	177
Incentivized - Female	11.52	19.87	2.8	11.7	323
All Data	13.41	22.16	3.2	12.0	1,000

Notes: This table shows mean, median, standard deviation and IQR of the absolute perception error (APE) by gender and incentive treatments. *Unincentivized* is comprised of treatments *Flat* and *Posterior*, while *Incentivized* is comprised of *Both* and *Prior*.

Table A-9: Effects of Incentives on Updated Inflation Expectations (Posteriors)

	(1) $\mathbb{E}(\pi_{posterior})$	(2) $\mathbb{E}(\pi_{posterior})$
Post	0.176	0.117
	(0.267)	(0.259)
Both	-0.497*	-0.657**
	(0.267)	(0.258)
Prior	0.0789	-3.32e-05
	(0.267)	(0.258)
Constant	2.603***	7.431***
	(0.189)	(1.287)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports the results of a series of OLS regressions (with robust standard errors) wherein we project inflation expectations estimated using participants' probabilistic inflation forecasts onto a series of dummies denoting treatment and other conditioning information.

Table A-10: Effects of Incentives on Time

	(1) Seconds	(2) Seconds
Post	20.14	50.88**
	(24.64)	(24.30)
Both	102.9***	93.32***
	(24.64)	(24.29)
Prior	74.02***	70.81***
	(24.64)	(24.22)
Constant	512.4***	499.2***
	(17.42)	(85.09)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table shows the effect of treatments on effort, as proxied by completion times. We use Huber regressions to account for the presence of extreme completion times.

Table A-11: Difference-in-means Tests (Welch) by Search Behavior and Treatment

Panel (a): One-Year Ahead Point Forecast						
Comparison (A vs B)	n_A	$\bar{y}_A$	n_B	$\bar{y}_B$	Diff $(A-B)$	p-value
Incentivized: searched vs not searched	39	3.51	218	3.81	-0.30	0.8401
Unincentivized: searched vs not searched	21	14.04	236	9.65	4.39	0.3592
Not searched: incentivized vs unincentivized	218	3.81	236	9.65	-5.84	0.0002***
Searched: incentivized vs unincentivized	39	3.51	21	14.04	-10.53	0.0339**
Panel (b): Three-Year Ahead Point Forecast						
Comparison (A vs B)	n_A	$\bar{y}_A$	n_B	$\bar{y}_B$	Diff $(A-B)$	p-value
Incentivized: searched vs not searched	39	6.65	218	3.77	2.88	0.2824
Unincentivized: searched vs not searched	21	12.21	236	6.37	5.84	0.2224
Not searched: incentivized vs unincentivized	218	3.77	236	6.37	-2.60	0.0759*
Searched: incentivized vs unincentivized	39	6.65	21	12.21	-5.56	0.2881

Notes: The table presents the results of the test for difference in means depending on search behavior and treatment group. We use data from the follow-up survey described in Appendix A6. Data are winsorized at the 1% and 99% levels. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

A2 Power Analysis

Table A-12: Sample Size Calculation

α	$1-\beta$	**N**	N_C	N_T	Δ	μ_C	μ_T	σ
0.01	0.80	1,172	586	586	0.2	0	0.2	1
0.01	0.80	524	262	262	0.3	0	0.3	1
0.01	0.80	296	148	148	0.4	0	0.4	1
0.01	0.80	192	96	96	0.5	0	0.5	1
0.01	0.80	134	67	67	0.6	0	0.6	1
0.01	0.80	100	50	50	0.7	0	0.7	1
0.01	0.80	78	39	39	0.8	0	0.8	1
0.01	0.80	62	31	31	0.9	0	0.9	1
0.01	0.80	52	26	26	1.0	0	1.0	1
0.05	0.80	788	394	394	0.2	0	0.2	1
0.05	0.80	352	176	176	0.3	0	0.3	1
0.05	0.80	200	100	100	0.4	0	0.4	1
0.05	0.80	128	64	64	0.5	0	0.5	1
0.05	0.80	90	45	45	0.6	0	0.6	1
0.05	0.80	68	34	34	0.7	0	0.7	1
0.05	0.80	52	26	26	0.8	0	0.8	1
0.05	0.80	42	21	21	0.9	0	0.9	1
0.05	0.80	34	17	17	1.0	0	1.0	1
0.10	0.80	620	310	310	0.2	0	0.2	1
0.10	0.80	278	139	139	0.3	0	0.3	1
0.10	0.80	156	78	78	0.4	0	0.4	1
0.10	0.80	102	51	51	0.5	0	0.5	1
0.10	0.80	72	36	36	0.6	0	0.6	1
0.10	0.80	52	26	26	0.7	0	0.7	1
0.10	0.80	42	21	21	0.8	0	0.8	1
0.10	0.80	32	16	16	0.9	0	0.9	1
0.10	0.80	28	14	14	1.0	0	1.0	1

Notes: Results are sorted by α and Cohen's D (i.e. μ_T).

Here we determine the necessary sample size for detecting effects of various magnitudes with a significance level of 0.05 and a power of 0.80. Effect magnitudes are specified in terms of Cohen's d, ranging from 0.2 to 1.0 in increments of 0.1. The effect magnitude(Cohen's d) is calculated as the standardized mean difference between the treatment and control groups. Specifically, Cohen's d is defined as:

$$d = \frac{M_1 - M_2}{SD_{pooled}}$$

where M_1 and M_2 are the means of the treatment and control groups, respectively, and SD_{pooled} is the pooled standard deviation of the two groups. We assume $M_1 = 0$, treating it as the control group.

Conventional thresholds for interpreting the magnitude of effect sizes:

- **Small effect size:** $d = 0.2$
- **Medium effect size:** $d = 0.5$
- **Large effect size:** $d = 0.8$

We base our sample size on this ex-ante power calculation. Our desire to precisely estimate null effects led us to choose a sample size of 250 subjects per treatment. This would allow us to detect small differences via pair-wise comparisons at a one-percent level of significance and $\beta = .8$.

A3 Inflation Expectations Survey

This section presents the full survey used in this study, which elicits inflation expectations and implements an information provision intervention.

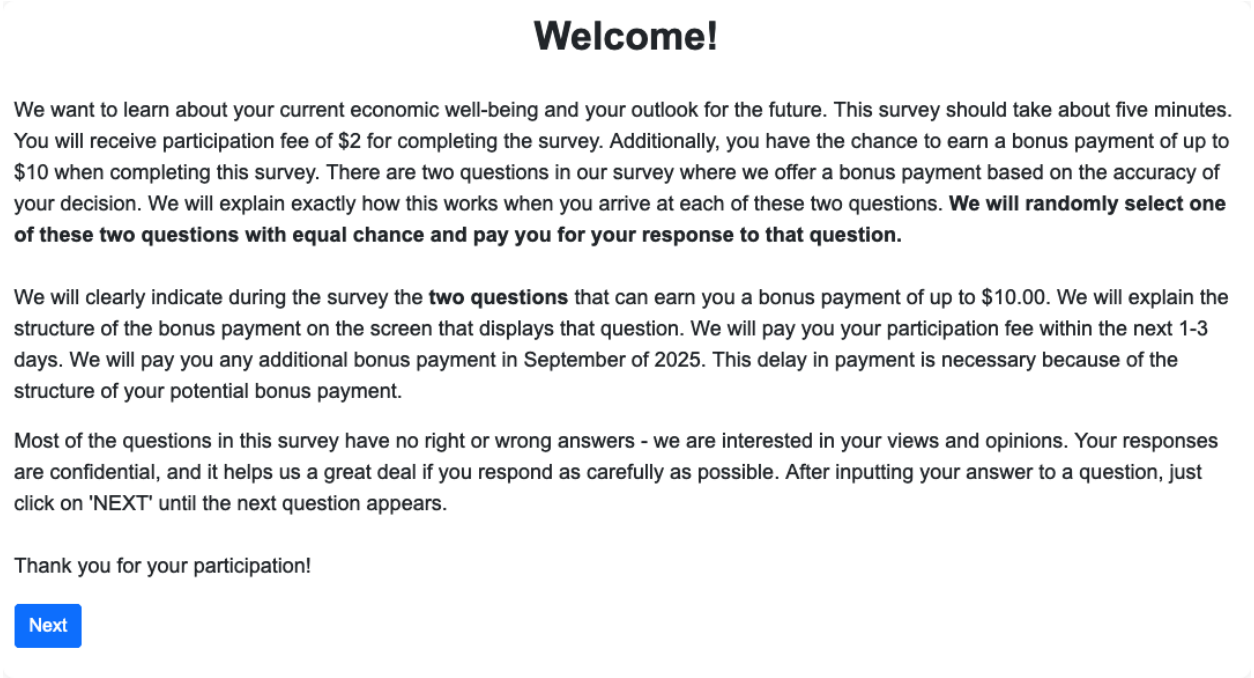


Figure A-2: Welcome

Notes: This figure shows the welcome page for the *Both* treatment group. Slight variations in wording occur between treatments to reflect the different incentive structures. Screenshots of other treatment groups are available upon request.

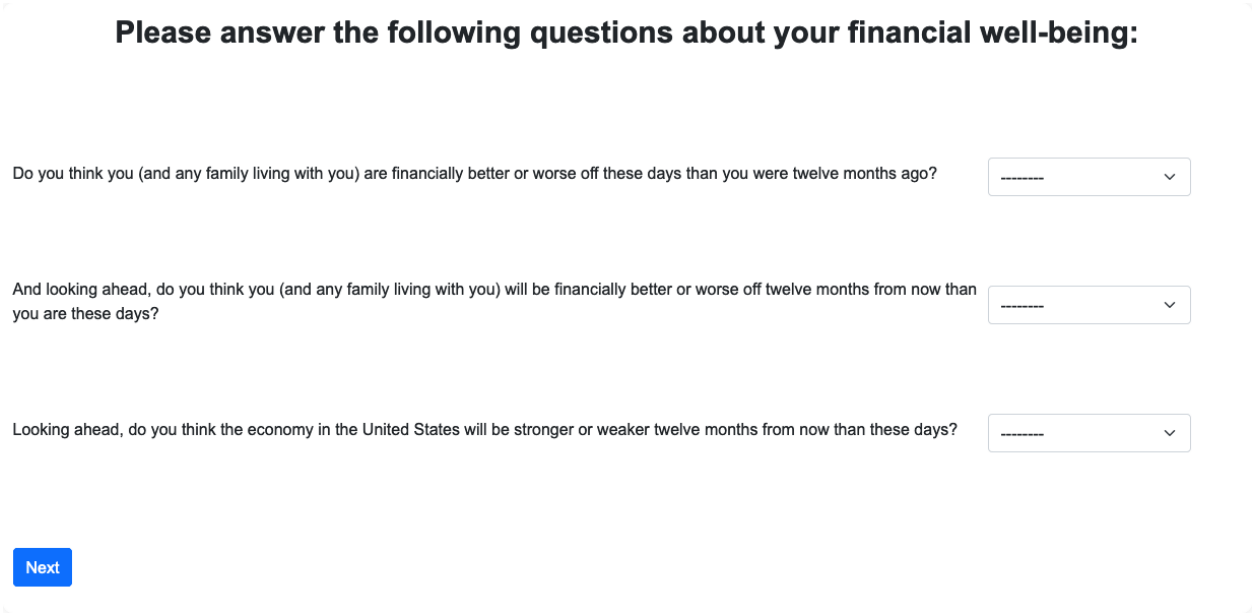


Figure A-3: General Questions

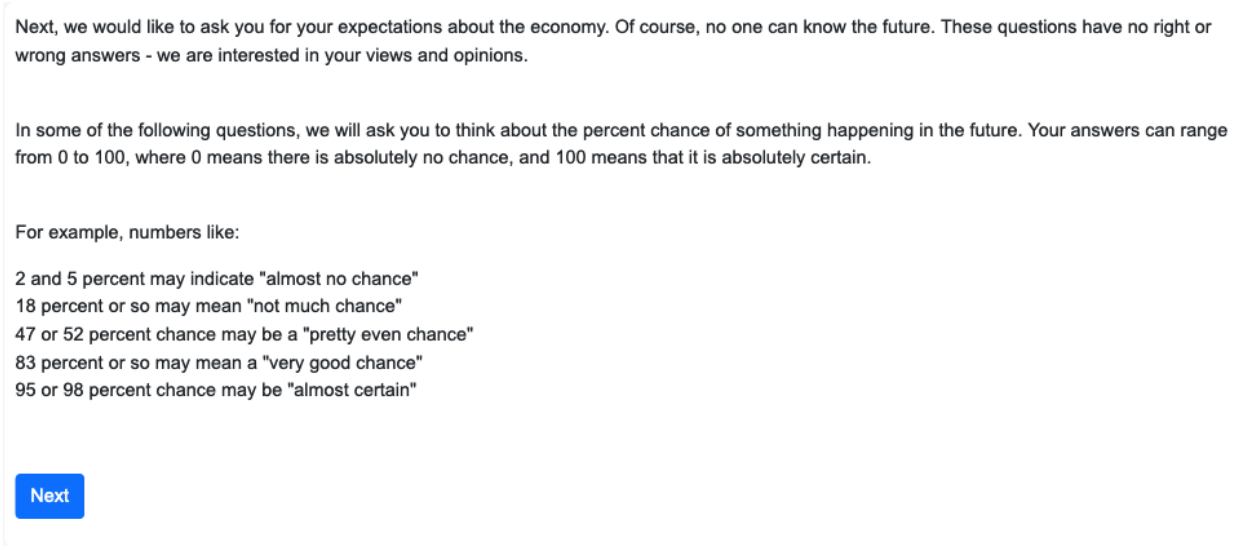


Figure A-4: Explanations

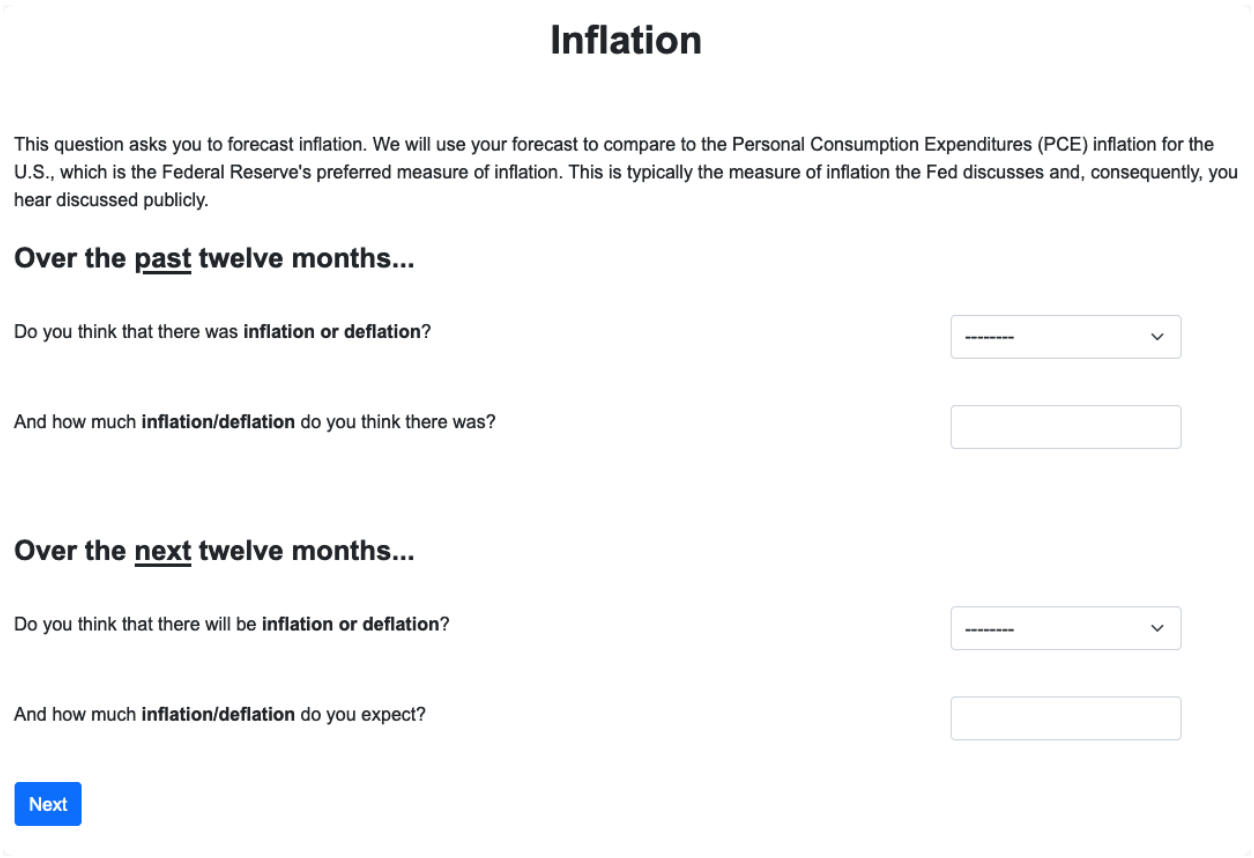


Figure A-5: Inflation Point Forecast (Flat)

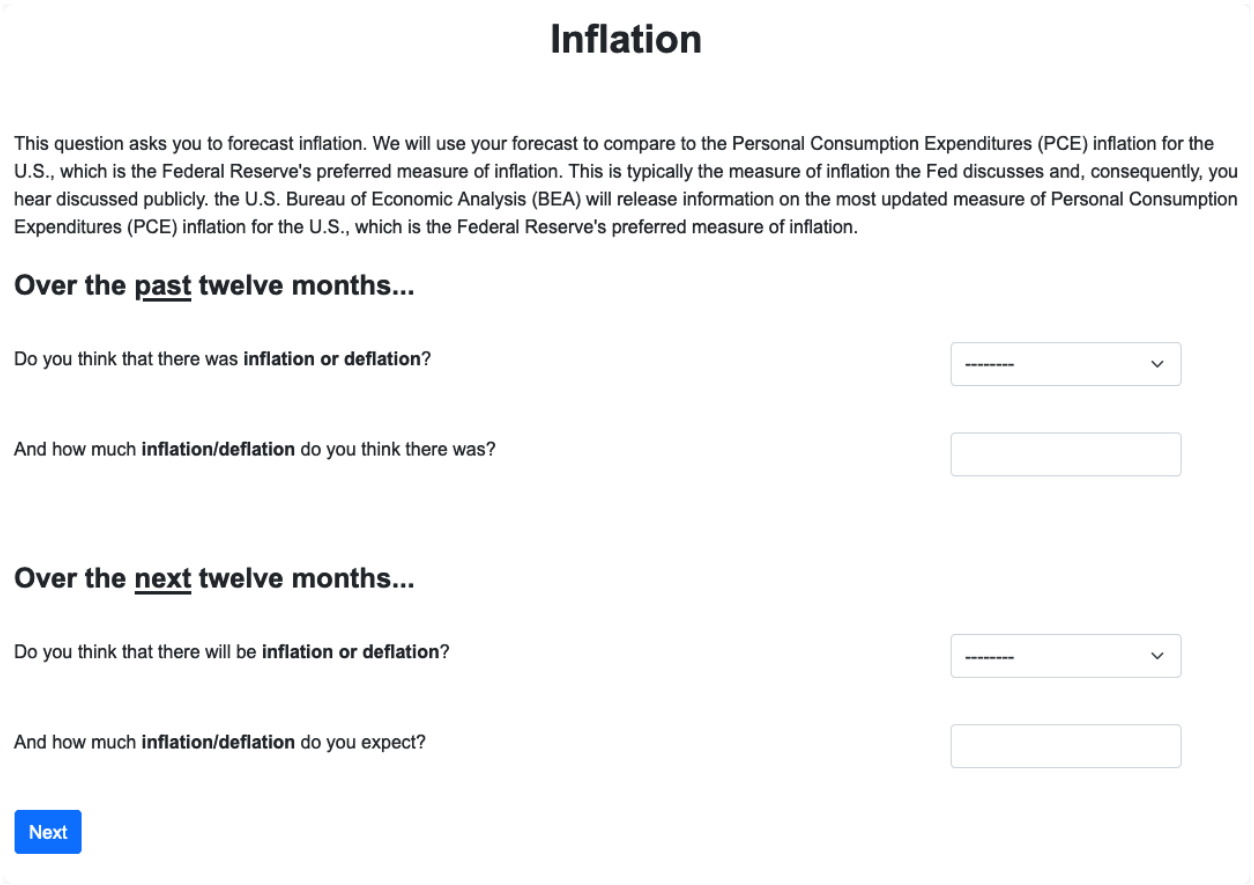


Figure A-6: Inflation Point Forecast (Posterior)

Inflation

Bonus Payment: YOU CAN RECEIVE A BONUS PAYMENT FOR THIS QUESTION. THIS IS THE ONLY QUESTION IN OUR SURVEY FOR WHICH YOU CAN RECEIVE A BONUS PAYMENT.

You can earn up to $10 for this task. This is in addition to your participation fee. In September of 2025, the U.S. Bureau of Economic Analysis (BEA) will release information on the most updated measure of Personal Consumption Expenditures (PCE) inflation for the U.S., which is the Federal Reserve's preferred measure of inflation.

Once the BEA publishes the actual PCE inflation reported in 12 months, we will compare your forecast to it and pay you based on the accuracy of your forecast. Your bonus payment halves each time your forecast increases by 1 percentage point.

For example:
If your forecast matches the inflation rate exactly, you will earn $10.
If your forecast is 1 percentage point above or below the inflation rate, you will earn $5.
If your forecast is 2 percentage points above or below the inflation rate, you will earn $2.5.

Over the past twelve months...

Do you think that there was **inflation or deflation**? --------

And how much **inflation/deflation** do you think there was?

Over the next twelve months...

Do you think that there will be **inflation or deflation**? --------

And how much **inflation/deflation** do you expect?

Next

Figure A-7: Inflation Point Forecast (Prior)

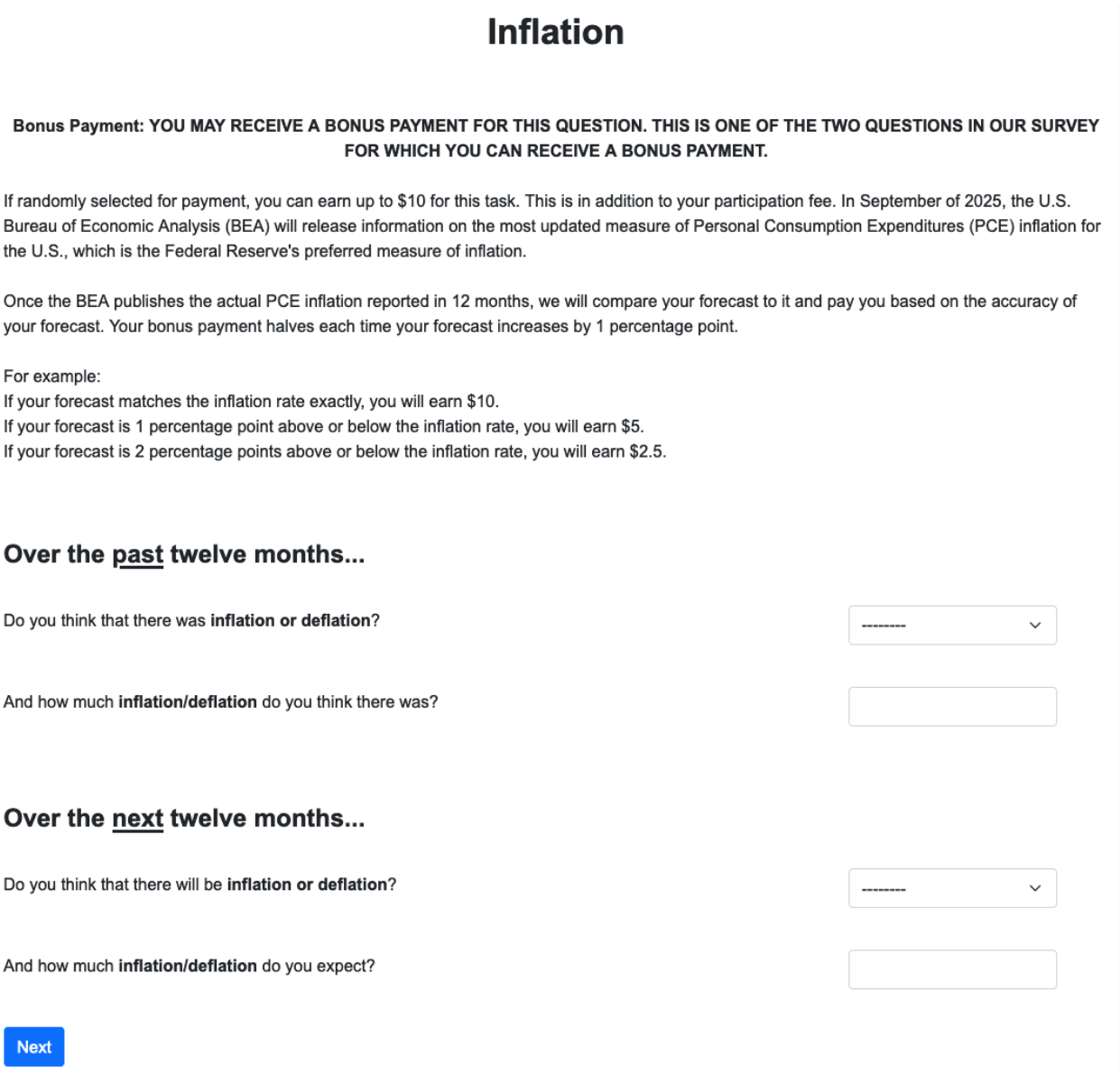


Figure A-8: Inflation Point Forecast (Both)

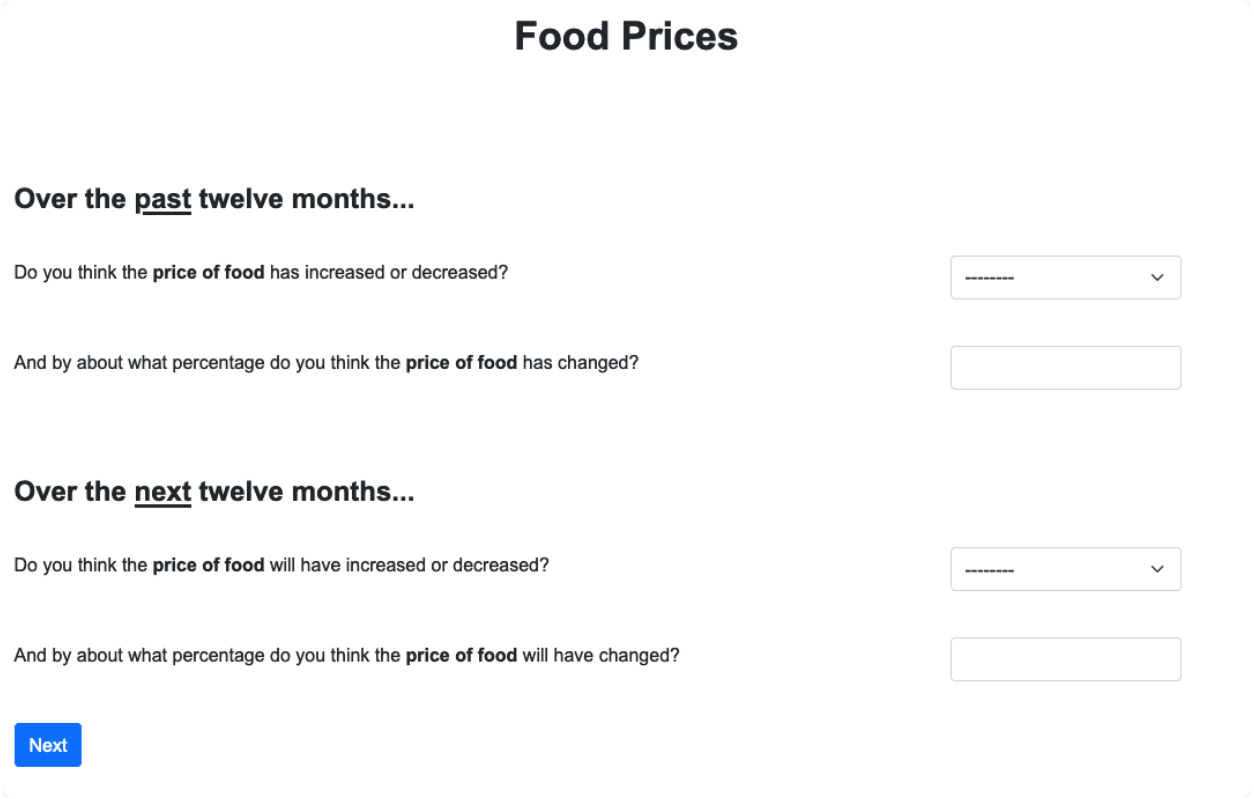


Figure A-9: Food Point Forecast

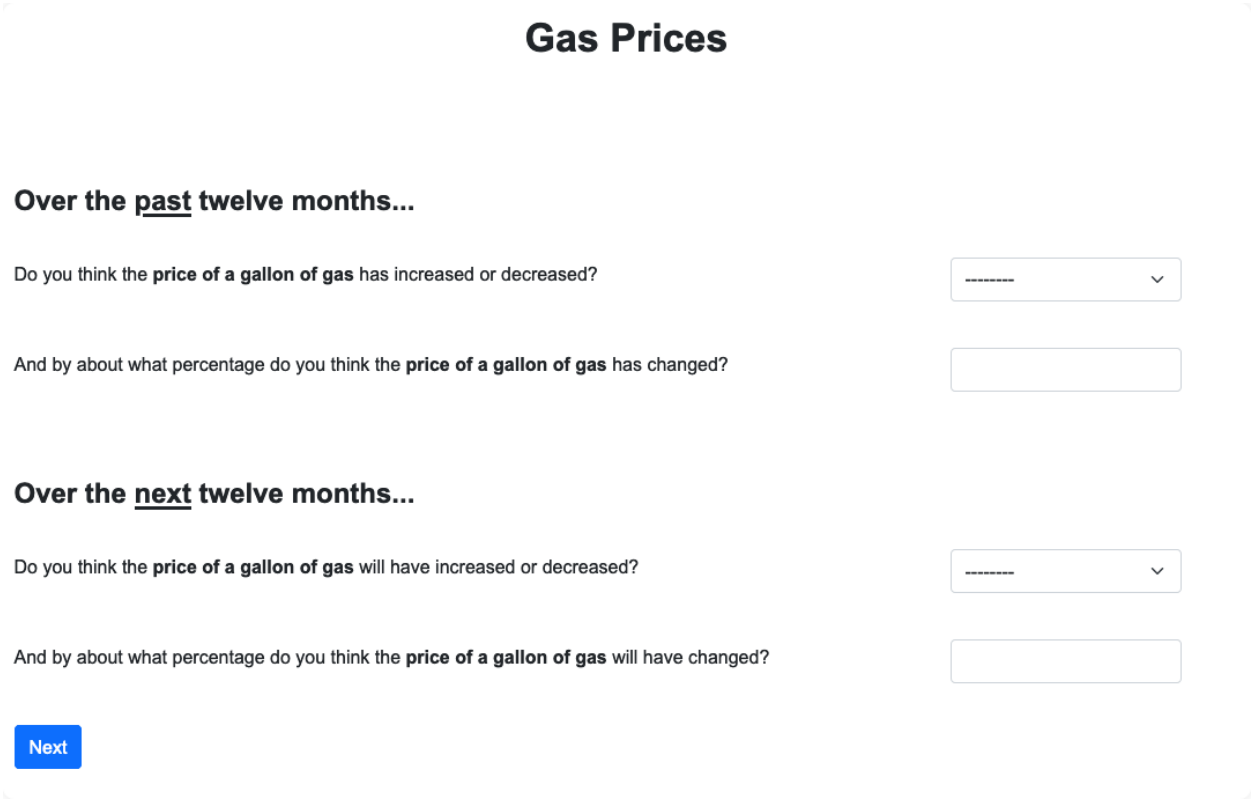


Figure A-10: Gas Point Forecast

We provide below the most recent official economic forecast data from the Federal Reserve, which is the central bank for the United States. Economic forecasts are very important for the Fed because policymakers there use forecasts to help them make good policy decisions when guiding our economy.

In conjunction with the Federal Open Market Committee (FOMC) meeting held on June 11–12, 2024, meeting participants submitted their projections of the most likely outcomes for inflation for each year from 2024 to 2026 and over the longer run. We have summarized these projections in the following table:

Variable	Median 2024	Range 2024	Median 2025	Range 2025
PCE inflation	2.6	2.5–3.0	2.3	2.2–2.5

Next

Figure A-11: Information Intervention

Food Prices

Now we would like you to think about the different things that may happen to **food prices** over the **next twelve months**. We realize that this question may take a little more effort.

Below, we will ask you to assign a percent (%) chance that food prices twelve months from now will fall into a certain range. The sum of the numbers you enter should equal 100%. For example, if you think there is a 20% chance that food prices will be 12% or higher, and a 30% chance that it will be between 8% and 12%, you should indicate this by entering the values 20 and 30 into each corresponding field. In this scenario, you would need to allocate the remaining 50%.

In your view, what would you say is the percent chance that, **over the next twelve months**...

the price of food will increase by 12% or higher: %

the price of food will increase by between 8% and 12%: %

the price of food will increase by between 4% and 8%: %

the price of food will increase by between 2% and 4%: %

the price of food will increase by between 0% and 2%: %

the price of food will decrease by between -2% and 0%: %

the price of food will decrease by between -4% and -2%: %

the price of food will decrease by between -8% and -4%: %

the price of food will decrease by between -12% and -8%: %

the price of food will decrease by -12% or lower: %

Next

Figure A-12: Food Density Forecast

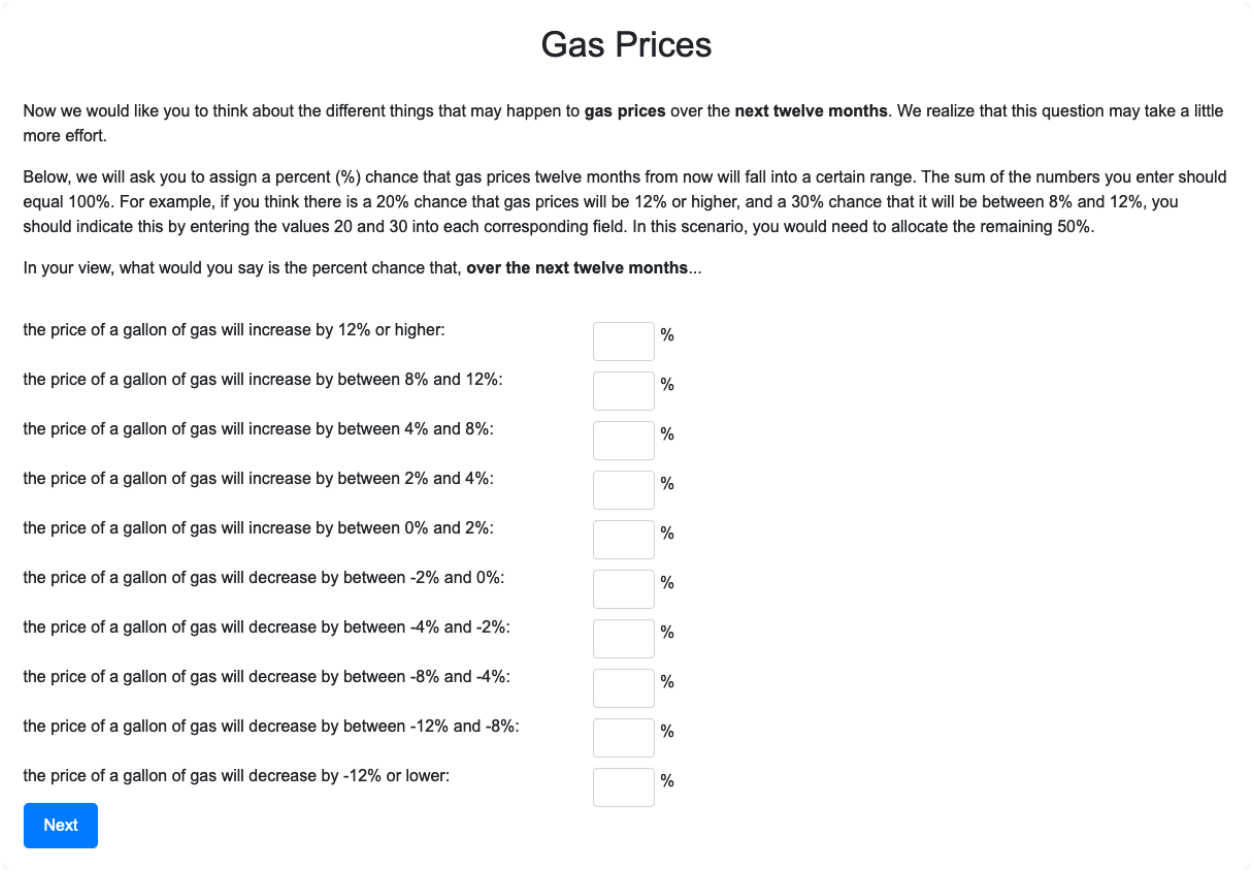


Figure A-13: Gas Density Forecast

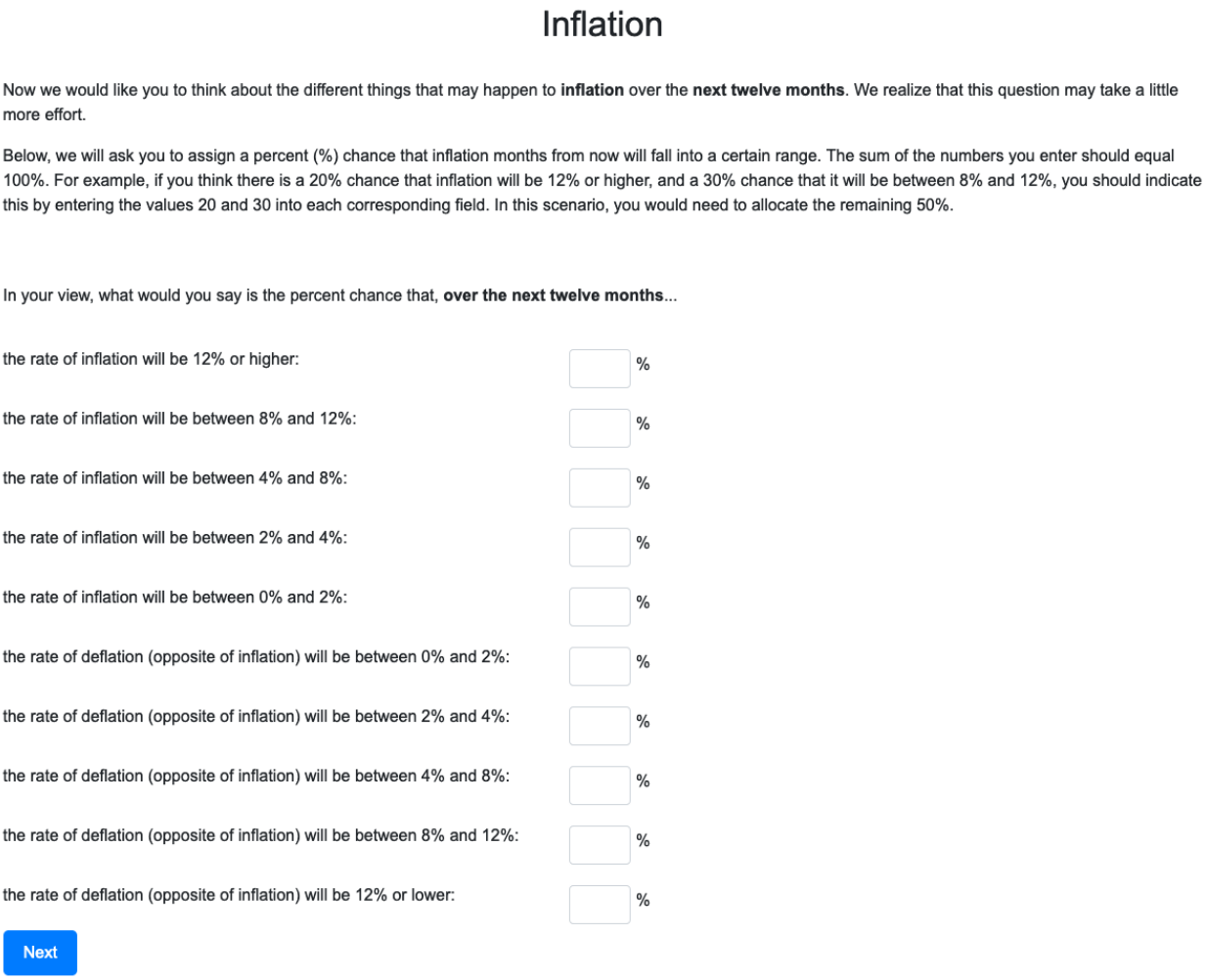


Figure A-14: Inflation Density Forecast (Flat and Prior)

Inflation

Now we would like you to think about the different things that may happen to **inflation** over the **next twelve months**. We realize that this question may take a little more effort.

Below, we will ask you to assign a percent (%) chance that inflation months from now will fall into a certain range. The sum of the numbers you enter should equal 100%. For example, if you think there is a 20% chance that inflation will be 12% or higher, and a 30% chance that it will be between 8% and 12%, you should indicate this by entering the values 20 and 30 into each corresponding field. In this scenario, you would need to allocate the remaining 50%.

Bonus Payment: YOU CAN RECEIVE A BONUS PAYMENT FOR THIS QUESTION. THIS IS THE ONLY QUESTION IN OUR SURVEY FOR WHICH YOU CAN RECEIVE A BONUS PAYMENT.

You can earn up to $10 for this task. This is in addition to your participation fee. In September of 2025, the U.S. Bureau of Economic Analysis (BEA) will release information on the most updated measure of Personal Consumption Expenditures (PCE) inflation for the U.S., which is the Federal Reserve's preferred measure of inflation. This value of inflation will fall into one of the bins you see here. Your bonus payment will be $10 multiplied by the weight (i.e. % chance) you assigned to that bin. For example:

- If you assign a 10% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.10 = $1.00.
- If you assign a 25% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.25 = $2.50.
- If you assign a 90% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.9 = $9.00.

In your view, what would you say is the percent chance that, **over the next twelve months**...

the rate of inflation will be 12% or higher:	%
the rate of inflation will be between 8% and 12%:	%
the rate of inflation will be between 4% and 8%:	%
the rate of inflation will be between 2% and 4%:	%
the rate of inflation will be between 0% and 2%:	%
the rate of deflation (opposite of inflation) will be between 0% and 2%:	%
the rate of deflation (opposite of inflation) will be between 2% and 4%:	%
the rate of deflation (opposite of inflation) will be between 4% and 8%:	%
the rate of deflation (opposite of inflation) will be between 8% and 12%:	%
the rate of deflation (opposite of inflation) will be 12% or lower:	%

Next

Figure A-15: Inflation Density Forecast (Posterior)

Inflation

Now we would like you to think about the different things that may happen to **inflation** over the **next twelve months**. We realize that this question may take a little more effort.

Below, we will ask you to assign a percent (%) chance that inflation months from now will fall into a certain range. The sum of the numbers you enter should equal 100%. For example, if you think there is a 20% chance that inflation will be 12% or higher, and a 30% chance that it will be between 8% and 12%, you should indicate this by entering the values 20 and 30 into each corresponding field. In this scenario, you would need to allocate the remaining 50%.

Bonus Payment: YOU MAY RECEIVE A BONUS PAYMENT FOR THIS QUESTION. THIS IS ONE OF THE TWO QUESTIONS IN OUR SURVEY FOR WHICH YOU CAN RECEIVE A BONUS PAYMENT.

If randomly selected for payment, you can earn up to $10 for this task. This is in addition to your participation fee. In September of 2025, the U.S. Bureau of Economic Analysis (BEA) will release information on the most updated measure of Personal Consumption Expenditures (PCE) inflation for the U.S., which is the Federal Reserve's preferred measure of inflation. This value of inflation will fall into one of the bins you see here. Your bonus payment will be $10 multiplied by the weight (i.e. % chance) you assigned to that bin. For example:

- If you assign a 10% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.10 = $1.00.
- If you assign a 25% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.25 = $2.50.
- If you assign a 90% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.9 = $9.00.

In your view, what would you say is the percent chance that, **over the next twelve months**...

the rate of inflation will be 12% or higher:	%
the rate of inflation will be between 8% and 12%:	%
the rate of inflation will be between 4% and 8%:	%
the rate of inflation will be between 2% and 4%:	%
the rate of inflation will be between 0% and 2%:	%
the rate of deflation (opposite of inflation) will be between 0% and 2%:	%
the rate of deflation (opposite of inflation) will be between 2% and 4%:	%
the rate of deflation (opposite of inflation) will be between 4% and 8%:	%
the rate of deflation (opposite of inflation) will be between 8% and 12%:	%
the rate of deflation (opposite of inflation) will be 12% or lower:	%

Next

Figure A-16: Inflation Density Forecast (Both)

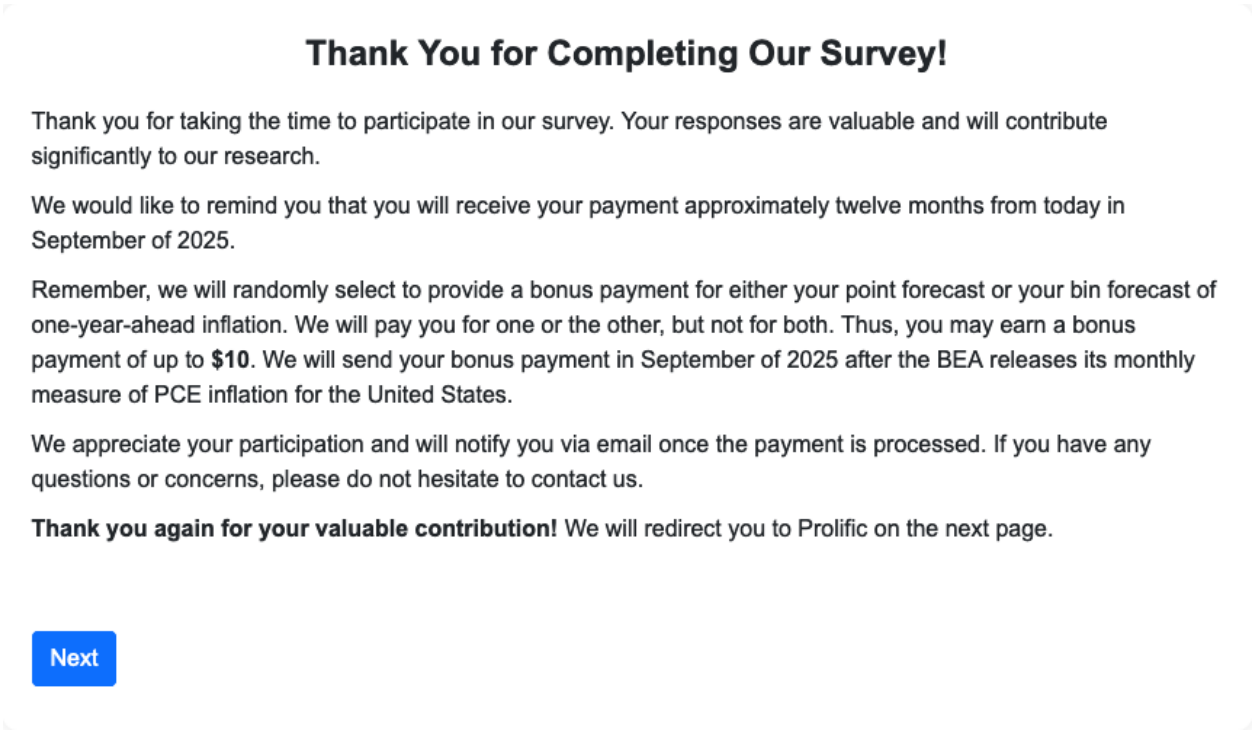


Figure A-17: End of Survey

Notes: This figure shows the final page for the *Both* treatment group. Slight variations in wording occur between treatments to reflect the different incentive structures. Screenshots of other treatment groups are available upon request.

A4 Alternative Specification for Belief Updating

We estimate the following specification, in the spirit of Coibion et al. (2022):

$$\mathbb{E}_i(\pi_{\text{Post}}) = a + \sum_j b_j \times \text{Treatment}_{i,j} + \sum_j \gamma_j \times \text{Treatment}_{i,j} \times \mathbb{E}_i(\pi_{\text{Prior}}) + \psi \times \mathbb{E}(\pi_{\text{Prior}}) + \varepsilon_i \quad \text{(A.10)}$$

where $\mathbb{E}_i(\pi_{\text{Post}})$ is respondent i's *posterior* point expectation, $\mathbb{E}_i(\pi_{\text{Prior}})$ is the *prior* point expectation elicited immediately before treatment, and $\text{Treatment}_{i,j}$ are treatment indicators with $j \in \{Post, Both, Prior\}$. Thus, *Flat* is the control group: signal, no incentives. We estimate Equation (A.10) via Huber regressions and report the **Intercept** and the **Slope** by treatment, which measures how strongly posteriors load on priors.

Here is how we interpret these results:

- **Intercept (level shift).** Under the Bayesian updating assumption, the intercept

reported in Table A-13 for the *Flat* treatment should reflect the contribution of the signal (i.e., weight multiplied by the signal) to the posterior. However, because we use different methods to elicit the prior and the posterior, it also captures a mechanical updating effect: the shift due to the change in elicitation format. For any other treatment, the intercept represents the shift relative to *Flat* induced by that treatment's incentive scheme. This corresponds to b in Equation (A.10).

- **Slope (weight on prior).** For *Flat*, the slope reported in Table A-13 corresponds to ψ in Equation (A.10), the marginal effect of the prior on the posterior in the base group (or simply weight on prior). For any other treatment, the total slope is $\psi+\gamma$, where γ is the treatment-specific interaction term in Equation (A.10). Note, Table A-13 reports slopes for incentivized treatments relative to *Flat* (i.e., reports γ independently for these treatments). A negative slope ($\gamma < 0$) means respondents put less weight on their prior and move more towards the signal communicated to all subjects in our RCT. In *Flat*, the estimated slope is 0.079, implying that, absent incentives, respondents carry roughly 8% of their prior into the posterior. Put differently, about 92% of the posterior reflects movement toward the signal, with only 8% persistence of the prior. Coibion et al. (2022) also reports a high weight on the signal (approximately 80%) when the information treatment is based on the FOMC's inflation forecast.

- **Effect of incentives on uptake.** Relative to control (*Flat*), incentives in *Post* reduce the slope by 0.040 ($p < 0.01$), roughly halving the weight on the prior from 0.079 to 0.039. This indicates stronger signal uptake with incentives. *Both* has a smaller slope reduction of 0.019. By contrast, *Prior* increases the slope by 0.037 ($p < 0.01$), implying weaker signal uptake than in *Flat*. In other words, respondents were discouraged from moving toward the signal, knowing that they had already completed the incentivized forecasting task and that the posterior forecasting task would not provide any additional benefits.

Table A-13: Incentivizing Updated Expectations: Intercept and Slope Estimates

	No Controls		With Controls	
Treatment	**Intercept**	**Slope**	**Intercept**	**Slope**
	(1)	(2)	(3)	(4)
Flat	2.355	0.079***	2.441	0.067***
	(0.175)	(0.006)	(0.173)	(0.007)
Relative to Control Group				
Post	0.163	−0.040***	0.152	−0.037***
	(0.251)	(0.009)	(0.246)	(0.009)
Both	−0.374	−0.019*	−0.410	−0.019*
	(0.247)	(0.011)	(0.243)	(0.010)
Prior	0.003	0.037***	0.020	0.030**
	(0.249)	(0.014)	(0.244)	(0.013)
N	1000		1000	

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: This table reports intercepts and slopes from Equation (A.10) estimated via Huber robust regressions. Columns (1)–(2) exclude controls; columns (3)–(4) include age, race, gender, income, education, political affiliation, and economic sentiment.

A5 Quantifying the Impact of Incentives on Learning

In Section 3.4, we estimate how marginal incentives influence signal uptake in a simple information provision experiment embedded in our survey. A potential complication is a *mechanical artifact*: the act of shifting the elicitation from a point to a density forecast can itself create apparent "updating," even in the absence of a signal. Using SPF data, Engelberg et al. (2009) find that about 20% of professional forecasters exhibit mechanical shifts in their inflation forecasts when providing both point and distributional forecasts. Similarly, Coibion et al. (2022) show that switching between distributional and point forecasts can generate apparent updating even in the absence of new information: in their study, a pure control group receiving no signal still seemingly updated its inflation expectations.

To account for this, we decompose observed updating into three components:

1. **Mechanical updating:** Apparent updating arising purely from the shift between elicitation formats (point to density).

2. **Signal-driven updating:** Genuine updating in response to the signal, net of mechanical effects.

3. **Incentive-driven updating:** Additional updating due to marginal incentives, relative to the signal-driven effect.

We provide rough estimates of these components by integrating our main experimental data with our follow-up wave that did not include an information provision intervention (for details on this wave see Appendix A6).

Step 1 (Mechanical updating): We first estimate updating in a placebo condition (*Flat, no signal, no incentives*) from our follow-up study. Subjects provide priors (point forecasts) and posteriors (density forecasts), but receive no signal. The estimated slope,

$$\text{Updating}_i = \alpha + \beta_{\text{placebo}}\text{PercGap}_i + \varepsilon_i,$$

captures purely mechanical updating, where $PercGap_i$ is defined as the (counterfactual) signal we would have displayed.[1]

Step 2 (Signal-driven updating): We then estimate the slope in the signal-only treatment without incentives. The net signal effect is:

$$\text{True Signal Effect} = \beta_{\text{signal}} - \beta_{\text{placebo}},$$

which isolates genuine responsiveness to the signal after removing mechanical effects.

Step 3 (Incentive-driven updating): Finally, we consider the incentivized signal treatment. We first compute an *incentive share*:

$$\text{Incentive Share} = \frac{\beta_{\text{incentive}} - \beta_{\text{signal}}}{\beta_{\text{incentive}} - \beta_{\text{placebo}}},$$

measuring the fraction of the total movement from placebo to incentivized updating attributable to incentives.

We also express this effect as a *percentage increase in signal-driven updating* (net of mechanics):

$$\text{Incentive-Induced Increase (Net)} = \frac{(\beta_{\text{incentive}} - \beta_{\text{signal}})}{(\beta_{\text{signal}} - \beta_{\text{placebo}})} \times 100,$$

which shows how much incentives amplify genuine signal-driven updating after accounting for mechanics.[2]

[1] We take the exact time-appropriate analogue of what we provided subjects in our main survey, namely an FOMC forecast of 2.7%.

[2] We use individual Huber regressions for this exercise for clarity. Results are qualitatively identical if we instead use coefficients from a pooled regression.

Table A-14 reports these results. The placebo slope ($\beta_{\text{placebo}} = 0.910$) is large, consistent with a mechanical artifact of shifting to density forecasts. Netting this out yields a genuine signal-driven effect of $\beta_{\text{signal}} - \beta_{\text{placebo}} = 0.028$. Adding marginal incentives raises the slope further to $\beta_{\text{incentive}} = 1.000$, implying:

$$\text{Upper Bound Incentive Share} \approx 69\%, \quad \text{and} \quad \text{Incentive-Induced Increase (Net)} \approx 221\%.$$

Table A-14: Decomposing Mechanical, Signal, and Incentive Effects on Updating

Treatment	**Slope (β)**	**Signal Effect**	**Incentive Effect**
Placebo (No Signal)	0.910 (0.010)	–	–
Signal (No Incentives)	0.938 (0.009)	0.028	–
Signal + Incentives (Posterior)	1.000 (0.001)	–	0.691
Derived Quantities:			
True Signal Effect:	$0.938 - 0.910 = 0.028$		
Incentive Share:	$\frac{1.000-0.938}{1.000-0.910} \times 100 = 69.1\%$		
Incentive-Induced Increase (Net):	$\frac{1.000-0.938}{0.938-0.910} \times 100 = 221\%$		

Notes: Coefficients are from regressions of updating on the perception gap by treatment group (standard errors in parentheses). The "Net Effect" is relative to the placebo coefficient. The "Incentive Share" and the "Incentive-Induced Increase (Net)" quantify how much marginal incentives amplify genuine signal-driven updating, net of mechanical effects.

This decomposition highlights that accounting for the mechanical artifact of point-to-density elicitation is crucial: the mechanical component is substantial relative to the genuine signal effect. After netting it out, marginal incentives more than triple the magnitude of signal-driven updating in our *Post* treatment.

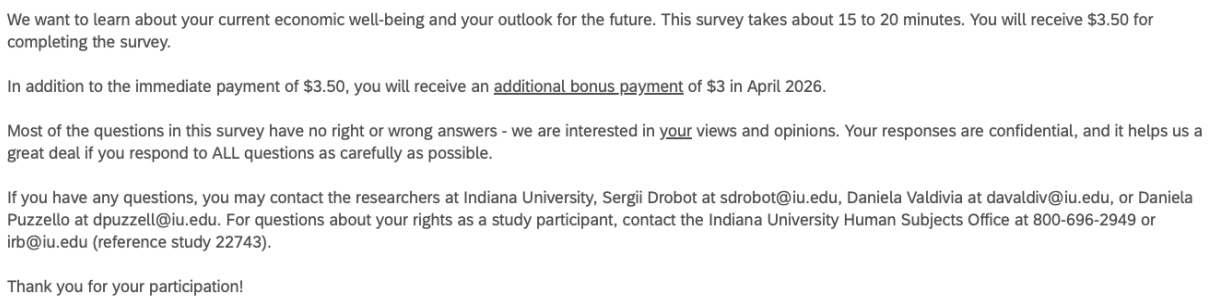
This exercise is best understood as providing order-of-magnitude guidance on the relative roles of mechanical artifacts, signal effects, and incentives. While precise magnitudes may vary (e.g., due to differences in timing across survey waves), the directional insight is clear: incentives meaningfully amplify net signal responsiveness.

A6 Follow-up Wave

We conducted the follow-up experiment in March 2025, recruiting participants from a nationally representative adult population via Prolific. To ensure that the new sample was not influenced by the information treatments or incentive structures used in the September 2024 wave, participants from the original study were not eligible to take part in this follow-up survey.

A total of 514 participants were randomly assigned to one of two treatment groups *Flat* or *Both*, with 257 participants in each group. As for the first wave, the *Flat* treatment elicits one-year-ahead inflation expectations without marginal incentives, whereas the *Both* treatment incentivizes both the point and density forecasts of these expectations with equal probability. Importantly, the March 2025 wave did not include an information provision experiment. This was done to replicate the Survey of Consumer Expectations (SCE) even more closely.[3] This also entails the (unincentivized) elicitation of longer-term inflation expectations, as is part of the SCE. In addition, we elicit participants' spending plans and ask whether they searched for information during the survey.[4]

Figure A-18 and Figure A-19 display the information shown to participants on the welcome page. Instructions on marginal incentives for incentivized point and density forecasts are shown in Figure A-20 and Figure A-21, respectively. The wording used to elicit the one-year-ahead point and density forecasts is shown in Figure A-22 and Figure A-23. Similarly, the (unincentivized) three-year-ahead counterparts are presented in Figure A-24 and Figure A-25. The spending question is shown in Figure A-26. Figure A-27 shows the question on whether respondents consulted external sources when forming their forecasts. They were assured that their responses would not affect their payment.


We want to learn about your current economic well-being and your outlook for the future. This survey takes about 15 to 20 minutes. You will receive $3.50 for completing the survey.

In addition to the immediate payment of $3.50, you will receive an additional bonus payment of $3 in April 2026.

Most of the questions in this survey have no right or wrong answers - we are interested in your views and opinions. Your responses are confidential, and it helps us a great deal if you respond to ALL questions as carefully as possible.

If you have any questions, you may contact the researchers at Indiana University, Sergii Drobot at sdrobot@iu.edu, Daniela Valdivia at davaldiv@iu.edu, or Daniela Puzzello at dpuzzell@iu.edu. For questions about your rights as a study participant, contact the Indiana University Human Subjects Office at 800-696-2949 or irb@iu.edu (reference study 22743).

Thank you for your participation!

Figure A-18: Welcome Page (*Flat*)

[3] Most survey items were adapted from the New York Fed's SCE instrument, and we adhered as closely as possible to the original ordering of questions.

[4] After eliciting inflation expectations, we reminded the *Both* group that all subsequent questions were not incentivized by providing the following message: "The following questions will not impact your bonus payment. Please answer them as accurately as possible."

We want to learn about your current economic well-being and your outlook for the future. This survey takes about 15 to 20 minutes. You will receive $3.50 for completing the survey.

In addition to the fixed payment of $3.50, you might receive an additional bonus payment in April 2026 depending on your answers to bonus questions, on real-world outcomes and chance.

Two bonus questions are clearly identified within the survey. At the end of the survey, the computer will randomly choose one bonus question for payment. Each bonus question has the same chance of being selected for payment. We will provide information about the bonus payment on the screen that displays these questions.

Most of the questions in this survey have no right or wrong answers - we are interested in your views and opinions. Your responses are confidential, and it helps us a great deal if you respond to ALL questions as carefully as possible.

If you have any questions, you may contact the researchers at Indiana University, Sergii Drobot at sdrobot@iu.edu, Daniela Valdivia at davaldiv@iu.edu, or Daniela Puzzello at dpuzzell@iu.edu. For questions about your rights as a study participant, contact the Indiana University Human Subjects Office at 800-696-2949 or irb@iu.edu (reference study 22743).

Thank you for your participation!

Figure A-19: Welcome Page (*Both*)

Bonus Payment: THE NEXT QUESTION IS ONE OF TWO QUESTIONS IN THE SURVEY ELIGIBLE FOR A BONUS PAYMENT, WITH EACH QUESTION HAVING AN EQUAL CHANCE OF BEING SELECTED FOR THE BONUS.

If this question is randomly selected for payment, you can earn up to $10 for this task. This is in addition to your participation fee. In April 2026, official inflation statistics will be released, providing updated measures for the inflation rate in the U.S.

How your bonus is calculated:
Your bonus payment is based on the accuracy of your forecast. Once the actual inflation rate is released, we will compare your forecast to it. Your bonus payment halves each time your forecast deviates from the actual inflation rate by 1 percentage point.

What this means:
You receive a reward for being accurate: the closer your forecast is to the actual inflation rate, the higher the bonus payment.

For example:
If your forecast matches the inflation rate exactly, you will earn $10.
If your forecast is 1 percentage point above or below the inflation rate, you will earn $5.
If your forecast is 2 percentage points above or below the inflation rate, you will earn $2.5.

Your goal is to provide your best forecast to maximize the bonus payment.

Figure A-20: Information on Incentives for Point Forecast (*Both*)

Bonus Payment: THE NEXT QUESTION IS ONE OF TWO QUESTIONS IN THE SURVEY ELIGIBLE FOR A BONUS PAYMENT, WITH EACH QUESTION HAVING AN EQUAL CHANCE OF BEING SELECTED FOR THE BONUS.

If this question is randomly selected for payment, you can earn up to $10 for this task. This is in addition to your participation fee. In April 2026, official inflation statistics will be released, providing updated measures for the inflation rate in the U.S. This value will fall into one of the bins you see below.

How your bonus is calculated:
Your bonus payment is based on the accuracy of your forecast. It will be $10 multiplied by the weight (i.e. % chance) you assigned to the bin where the actual inflation rate will fall.

What this means:
You receive a reward for being accurate: the higher the percent chance you assign to the bin where the actual inflation rate will fall, the higher the bonus payment. However, you are penalized for being inaccurate: assigning chances to bins where the actual inflation rate will not fall results in no bonus payment for these bins.

For example:
If you assign a 100% chance to a bin and the actual inflation falls into that bin, you will earn $10 ($10 with 100% chance).
If you assign a 100% chance to a bin and the actual inflation falls into another bin, you earn $0 ($10 with 0% chance).

Your goal is to distribute your chances carefully across the bins to maximize the bonus payment.

Figure A-21: Information on Incentives for Distributional Forecast (*Both*)

Over the next 12 months, do you think that there will be inflation or deflation? (Note: deflation is the opposite of inflation)

Please choose one.

- inflation
- deflation (the opposite of inflation)

What do you expect the rate of inflation/deflation to be **over the next 12 months**? Please give your best guess.

Please enter a number greater than 0 or equal to 0.

Over the next 12 months, I expect the rate of inflation/deflation to be

Figure A-22: One-Year-Ahead Point Forecast

Now we would like you to think about the different things that may happen to inflation **over the next 12 months**. We realize that this question may take a little more effort.

In your view, what would you say is the percent chance that, **over the next 12 months...**

(Please note: The numbers need to add up to 100.)

the rate of inflation will be 12% or higher	0
the rate of inflation will be between 8% and 12%	0
the rate of inflation will be between 4% and 8%	0
the rate of inflation will be between 2% and 4%	0
the rate of inflation will be between 0% and 2%	0
the rate of deflation (opposite of inflation) will be between 0% and 2%	0
the rate of deflation (opposite of inflation) will be between 2% and 4%	0
the rate of deflation (opposite of inflation) will be between 4% and 8%	0
the rate of deflation (opposite of inflation) will be between 8% and 12%	0
the rate of deflation (opposite of inflation) will be 12% or higher	0
Total	0

Figure A-23: One-Year-Ahead Density Forecast

Now we would like you to think about inflation further into the future. **Over the 12-month period between March 2027 and March 2028,** do you think that there will be inflation or deflation?

Please choose one.

- inflation
- deflation (the opposite of inflation)

What do you expect the rate of inflation/deflation to be over that period? Please give your best guess.

Please enter a number greater than 0 or equal to 0.

Over the 12-month period between March 2027 and March 2028, I expect the rate of inflation/deflation to be

Figure A-24: Three-Year-Ahead Point Forecast

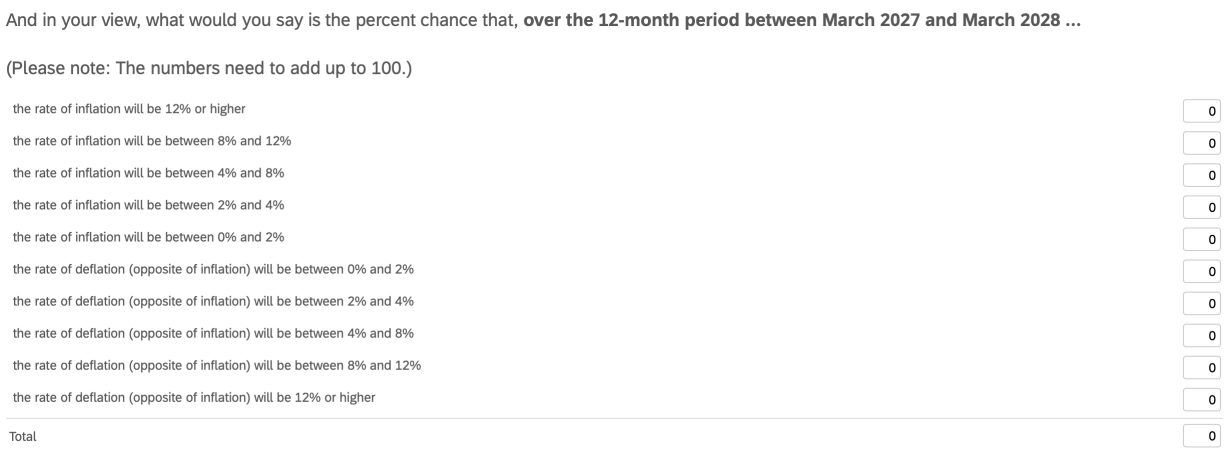


Figure A-25: Three-Year-Ahead Density Forecast

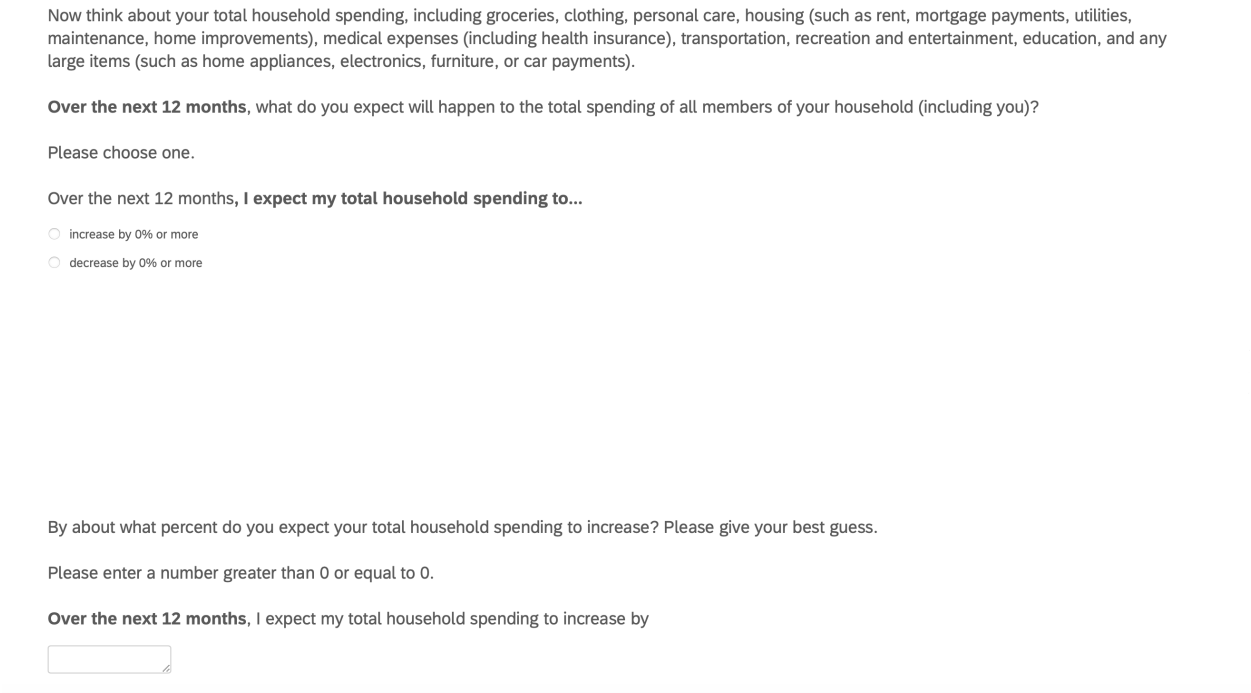


Figure A-26: Spending

Notes: The second part of the question is shown when a respondent selects "increase by 0% or more." If the respondent instead selects "decrease by 0% or more," the wording of the second part is adjusted accordingly to reflect that choice.

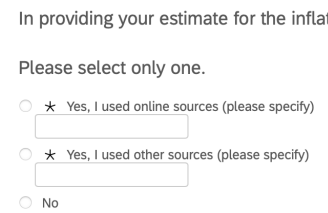


Figure A-27: Searching for Information