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 belief data quality in experimental economics. We show that marginal incentives raise effort and fundamentally reshape reported inflation expectations: lowering upward bias by 3.4 percentage points, reducing disagreement by 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 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).

Despite their importance, most surveys and RCTs measure inflation expectations without accuracy-based marginal incentives. By contrast, in experimental economics, such performance-based payments are a long-established tool for eliciting truthful beliefs and ensuring internal validity (Smith 1976), with a rich literature showing they reduce noise and misreporting.[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 design a controlled experiment to make two central contributions. First, we provide causal evidence on how marginal incentives affect the entire distribution of reported inflation expectations. Second, we quantify how marginal incentives impact belief updating and find that the estimated learning rate in an information provision RCT triples.

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.

[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), Trautmann and van de Kuilen (2014), Charness et al. (2021), Schotter and Trevino (2014), Schlag et al. (2015).

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

[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).

Marginal incentives significantly shift the distribution of inflation expectations, aligning inflation expectations with those of professional forecasters. Participants exposed to marginal incentives provide less extreme forecasts and reduced upward bias, with mean point forecasts falling from 6.1% to 2.7%. Additionally, incentivized inflation forecasts exhibit one-third less cross-sectional disagreement (the standard deviation of point expectations drops from 23.78 to 16.98), and become more consistent with professional forecasts. Further, incentives eliminate the gender gap in inflation expectations – a persistent puzzle in the existing survey literature. These patterns emerge for point and distributional forecasts. 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 outside information during the task.

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.

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, implying that the substantial shifts in belief distributions reflect more careful reporting of beliefs rather than information acquisition alone.

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, bridging survey and experimental methods through targeted incentive design enables more accurate measurement of expectations, better estimates of learning, and sharper evaluation of policy interventions in macroeconomic models.

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 truthful reporting Amid 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 2014, 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 is divided on whether incentivized elicitations improve belief accuracy. 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 sometimes be informative and that

[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).

marginal incentives may not always be necessary. 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.

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 (Danz et al. 2022, Abeler et al. 2023, Drobot et al. 2025, Charness et al. 2021). 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. Importantly, incentivizing survey responses allows us to distinguish better between genuine rational inattention to inflation and mere inattention to the survey itself, thus reducing measurement noise.[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

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

[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.

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-9) and posteriors as probabilistic forecasts (see Figure A-17). 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-12). This is the information provision intervention. 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. Moreover, inflation expectations are particularly well-suited to incentivization, as they are verifiable and it is not too impractical to incentivize them.[10] Worth noting is that we adopted the welcoming language of the SCE intended to activate participants' intrinsic motivation (see Figure A-3).

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 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, aligning with the forecast period. This delayed payment controls the timing of bonus payments necessary for other treatments and avoids potential selection effects.

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

[10] 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.

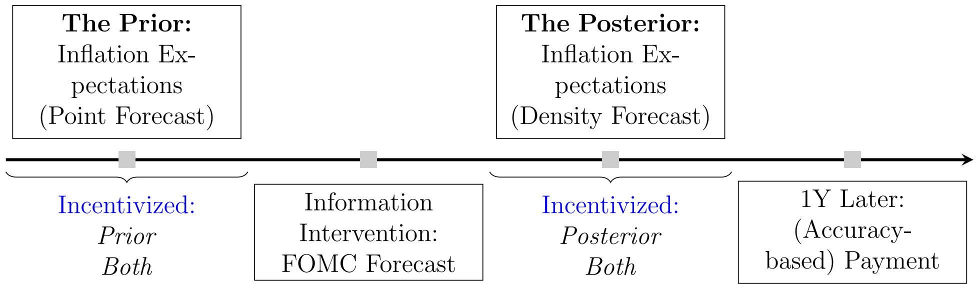


Figure 1: Experimental Design

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.

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.[11] This scoring rule is common in learning-to-forecast experiments in experimental macroeconomics and is easy to explain.[12] 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

[11] 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.

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

would earn $10*.2=$2.[13] 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.

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*) 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

[13]While our incentives are not incentive-compatible, they are simple and have been used in experimental economics in several settings, including learning to forecast ones. We opted for simplicity because previous experimental studies suggest that simpler incentives can be more effective than more complex, incentive-compatible designs (e.g., Charness et al. (2021) or Danz et al. (2022)). In Drobot et al. (2025), we focus on the role of incentive-compatibility and complexity in designing incentives in the context of inflation expectations and find support for the use of simple incentives.

[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).

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 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] while also increasing attention to the provided information.

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.*

Hypothesis 2 (Learning Rates): *In the RCT setting, marginal incentives increase learning rates, with incentivized participants adjusting their beliefs more substantially toward the provided information signal.*

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 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 without changing participants' perceptions of the data-generating process, introducing asymmetric information, or altering other fundamental aspects of the decision environment.

[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.

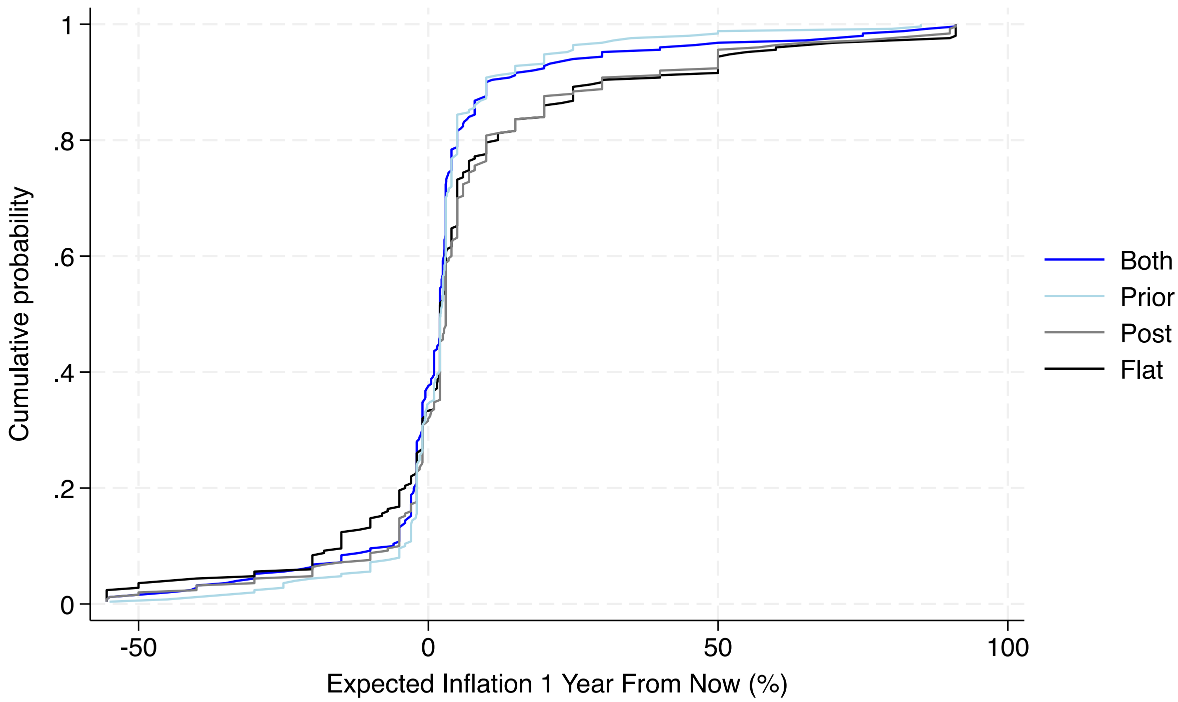


Figure 2: CDFs of Expected Inflation By Treatment

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.

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

Table 2: Summary Statistics and Variance Comparison of Inflation Expectations

	Mean	Std. Dev.	N
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	$p < .001$
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 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*.

Our first hypothesis (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.

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). Table 4 compares each treatment to the most recent mean PCE forecast from the Survey of Professional Forecasters (SPF). Expectations under *Flat* (5.20%) 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.[17]

Table 4: Comparing Experimental Data to Professional Forecasts from SPF

	Treatment	SPF Mean (Std. Dev.)	Treatment Mean (Std. Dev.)	Difference	Welch's t-stat	p-value
Unincentivized	*Flat*	2.11 (0.286)	5.52 (24.910)	-3.41	2.158	0.032
	Post	2.11 (0.286)	6.75 (22.617)	-4.64	3.244	0.001
Incentivized	*Prior*	2.11 (0.286)	2.80 (14.313)	-0.69	0.761	0.447
	Both	2.11 (0.286)	2.65 (19.319)	-0.44	0.434	0.665

Notes: This table compares data the Survey of Professional Forecasters (SPF) to data from participants in *Flat*, *Post*, *Prior* and *Both* using Welch's t-tests. For comparison, the most recent inflation report preceding our experiment was 2.5% (July inflation, released August 14th). 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. Note that the sample size for SPF (N=33) is considerably smaller than those of our survey, so we use Welch's t-test to account for this. Treatments' data are winsorized at the 1% and 99% levels.

Since realized inflation is not yet available, we assess accuracy indirectly by examining how incentive schemes affect participants' hypothetical payoffs. (Figure A-1, Table A-6; additional details in the Appendix). Using the FOMC's 2025 median inflation forecast as the benchmark for realized inflation, incentivized groups are expected to earn substantially more

[17] We currently lack access to the microdata from the New York Fed SCE, preventing formal comparison tests. However, summary statistics provided by the Fed for September 2024 show a winsorized mean of 6.03 and standard deviation of 17.3, broadly consistent with the means of our unincentivized samples but with somewhat lower dispersion. This may reflect that some of the SCE respondents are experienced, e.g., see Kim and Binder (2023).

than unincentivized groups. In *Both*, predicted payoffs rise by 24–34% depending on specification, while in *Prior* the increase is 33–49%. These findings highlight that marginal incentives not only reshape expectations but also meaningfully affect proxied earnings and thus expected accuracy.

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.

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.[18] 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) 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.

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 3 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

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

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.

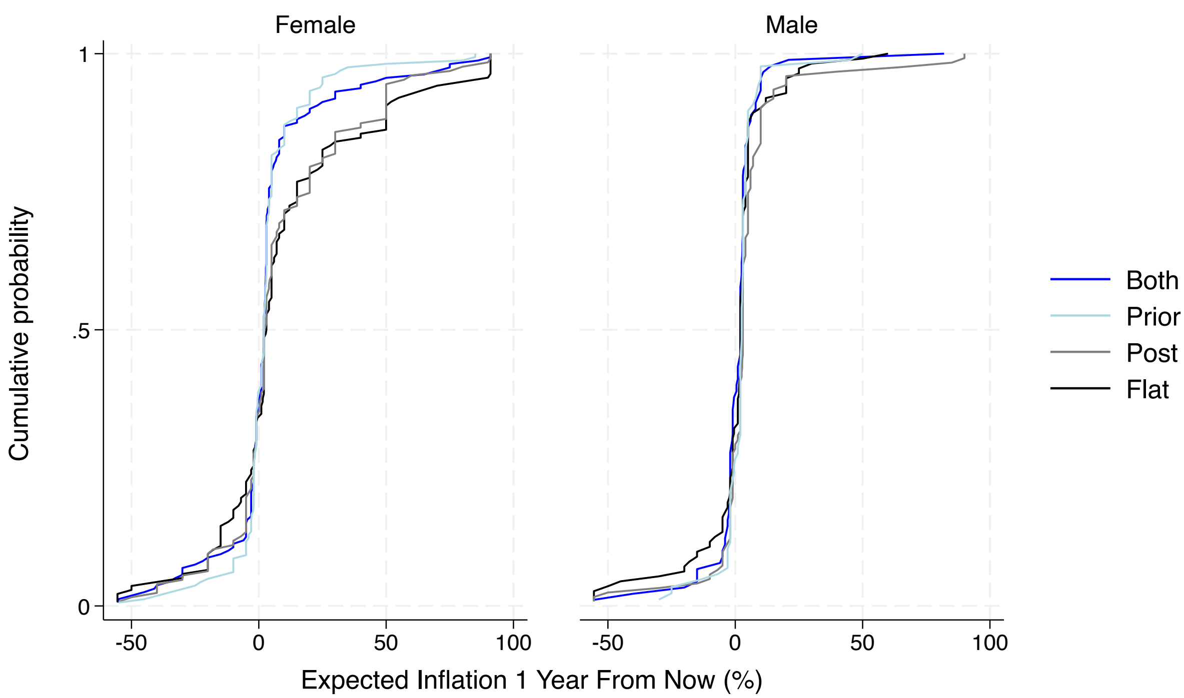


Figure 3: Effects of Incentives on Expectations by Gender

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.

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 rather than exert cognitive effort to recall information when reporting expectations.

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 suggest 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).

Under flat incentives, respondents have lower motivation to exert effort, retrieve information, 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 4. This suggests that respondents in incentivized treatments consider a broader range of values, moving away from simple extrapolation.

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, which is sensible given the timing of the survey.[19]

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

[19] The survey was conducted a few months before the 2024 Presidential Election and a few weeks prior to the Federal Reserve's first interest rate cut in four years.

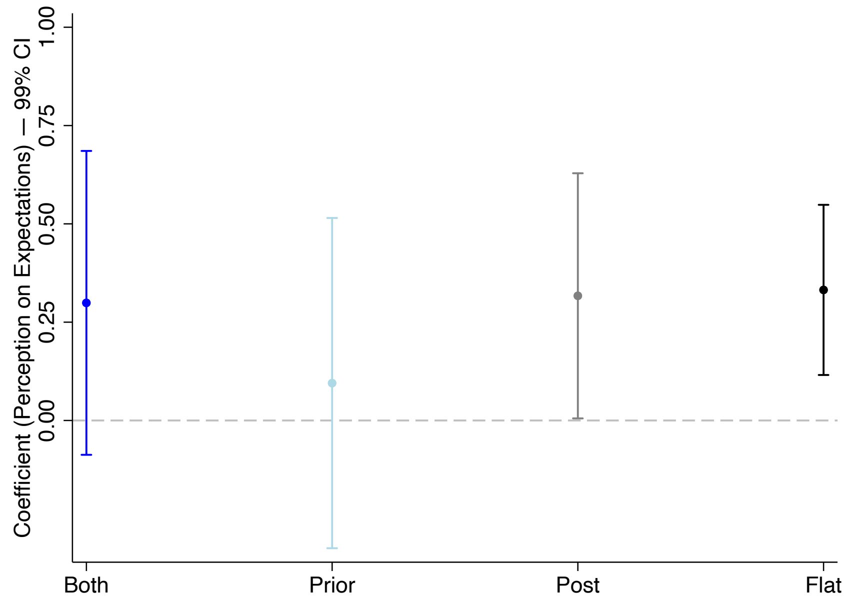


Figure 4: Inflation Expectations and Perceptions

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.

3.3.2 Attention to the Survey

Cognitive effort can also be assessed by attention to the survey itself. Following Bracha and Tang (2024), we measure inattention using Absolute Perception Error (APE) – the absolute difference between a respondent's perceived inflation and the most recent actual inflation rate (2.5% in July, released August 14th). Intuitively, the further a respondent's perception deviates from actual inflation, the less attention they are likely to pay to the survey (particularly the survey questions on inflation perceptions).

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.

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.

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 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.[20]

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.

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, especially when applied early in the survey, promote more deliberate reporting of beliefs and updates.

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

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).[21] 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 5, 91.4% of unincentivized 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.

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.

[21] 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.

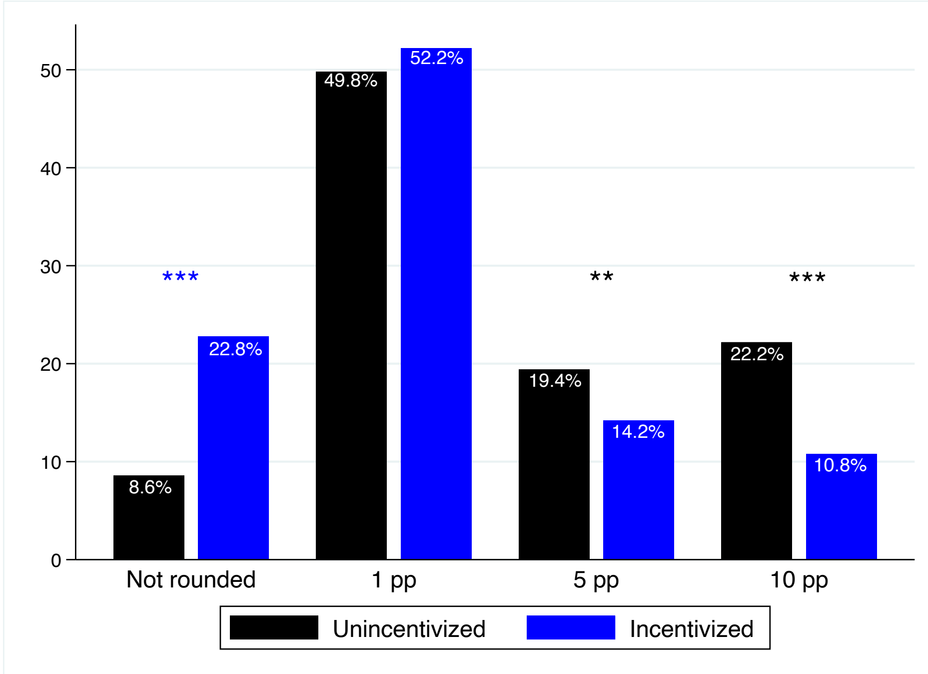


Figure 5: Percentage of Forecasts By Rounding Behavior

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: $^{*}\, p < 0.10$; $^{**}\, p < 0.05$; $^{***}\, p < 0.01$.

3.4 Effect of Incentives in Information Provision Experiments

We now assess the role that marginal incentives play in a simple information provision experiment. We find that marginal incentives can effectively bridge perception gaps, suggesting that RCTs without marginal incentives may systematically underestimate the impact of information on beliefs.

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").[22] We use the same bin-based elicitation strategy for density forecasts as the one employed by the New York Fed's SCE. [23] Figure 6 shows the average weight assigned to each of the ten possible inflation bins for incentivized participants (blue solid line) and those who were not (black solid line). Incentives shift more weight toward

[22] 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-2 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.

[23] 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.

the bin containing both the Fed's inflation target and the signal, suggesting that participants focus more deliberately on the provided information.

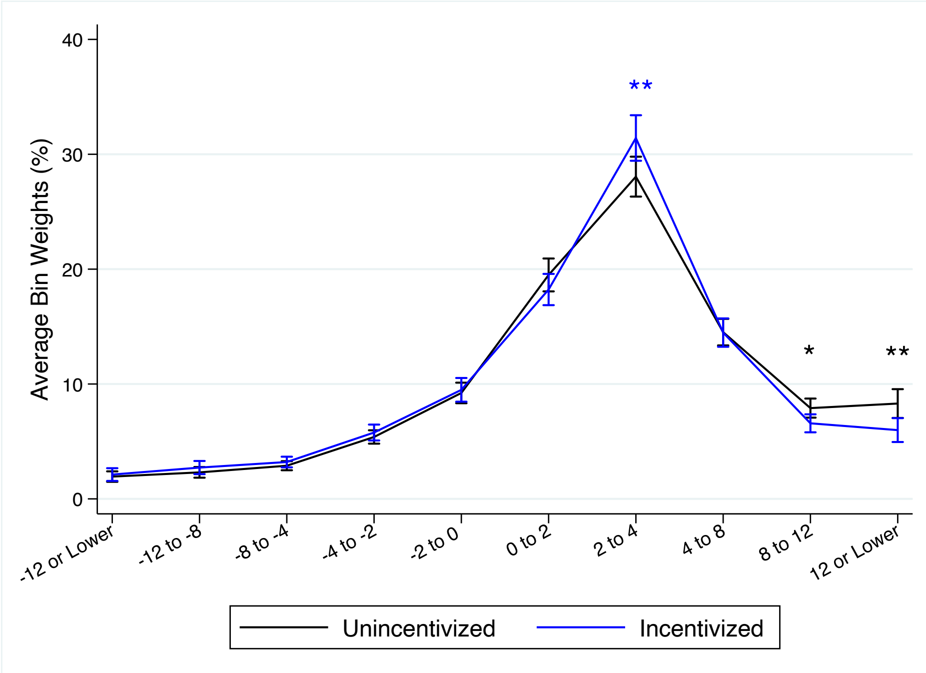


Figure 6: Average Bin Weights Across Incentives

Notes: This figure shows the average weight participants placed into the respective bins, distinguishing between unincentivized (*Flat* and *Prior*) and 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 incentivized and unincentivized treatments. Blue stars indicate incentivized subjects placed more weight into that bin, on average, and black stars the opposite. Significance levels are indicated as follows: $^{*}\, p < 0.10$; $^{**}\, p < 0.05$; $^{***}\, p < 0.01$.

To quantify the effect of incentives, we follow Haaland et al. (2023) and estimate *learning rates*, which capture the extent to which respondents adjust their forecasts toward 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*. 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.[24]

The results in Table 9 show that incentivizing posteriors significantly increases learning from information (β_1).[25] For every percentage-point perception gap, beliefs move 0.043 pp closer to the signal relative to *Flat*. *Both* also raises responsiveness (0.020), though less strongly. By contrast, *Prior* slightly dampens updating, but this effect becomes insignificant once controls are included. Importantly, participants in *Both* still learn substantially from the signal despite their incentivized priors, indicating that effects on priors cannot be explained simply by acquiring information.

Table 9: Effect of Incentives on Learning Rates

	(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.

[24] 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 ??, 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.

[25] 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*).

We quantify the importance of marginal incentives for belief updating with an illustrative decomposition exercise in Appendix A5, which accounts for the role of mechanical updating – apparent forecast revisions that arise purely from the change in elicitation format (from point to density forecasts). This exercise suggests that, after accounting for this artifact, 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 is consistent with rational inattention. Incentives raise the benefits of processing new information while holding costs constant, thereby reducing inattention (Maćkowiak and Wiederholt 2024; Maćkowiak et al. 2023). 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 exogenous conditions (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, without undermining the validity of prior approaches.

4 Do Incentives Improve Expectation Quality?

A natural question is whether marginal incentives bring us closer to households' genuine economic beliefs, which guide their behavior. This matters because in macroeconomic models, economic expectations guide economic decisions that aggregate into the dynamics that monetary policy seeks to manage. From this perspective, beliefs that are more predictive of planned behavior provide a more valid measure of the expectations that matter for policy.

Beyond predictive power, we also examine 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 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 Are incentivized inflation expectations more truthful and meaningful? To answer this, we assess how point forecasts of inflation expectations correlate with spending plans over the same horizon. Under the consumption Euler Equation, this correlation is expected to be positive, ceteris paribus.[26] We show in Table 10 that incentivized inflation expectations correlate positively and significantly with spending plans, while unincentivized inflation expectations do not.

Table 10: Correlating Inflation Expectations to Spending

	(1) Unincentivized	(2) Incentivized
Point forecast	0.113	0.218*
	(0.0791)	(0.115)
Constant	18.30***	-2.068
	(6.856)	(3.311)
Controls	Yes	Yes
N	257	257

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). 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.

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

[26] 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)".

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.[27] 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 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). This suggests that incentivizing near-term beliefs can improve the quality of longer-term expectations as well.

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.

Searching for information Another concern is that incentive schemes induce strategic behavior, such as searching for information online, that does not reflect genuine beliefs.

[27] 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).

This is a valid concern – both for our experiment and for any survey conducted online. Participants might engage in actions aimed at maximizing their payoffs rather than truthfully or thoughtfully revealing their expectations (e.g., Grewenig et al. 2022).[28] However, we find little evidence in support of this channel. While 8–15% of respondents reported searching for inflation information, the rate was only slightly higher for incentivized treatment groups (Figure 7).[29] Thus, increased search cannot account for the broad differences we document. 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 perhaps acquiring information would not be a bad thing after all.

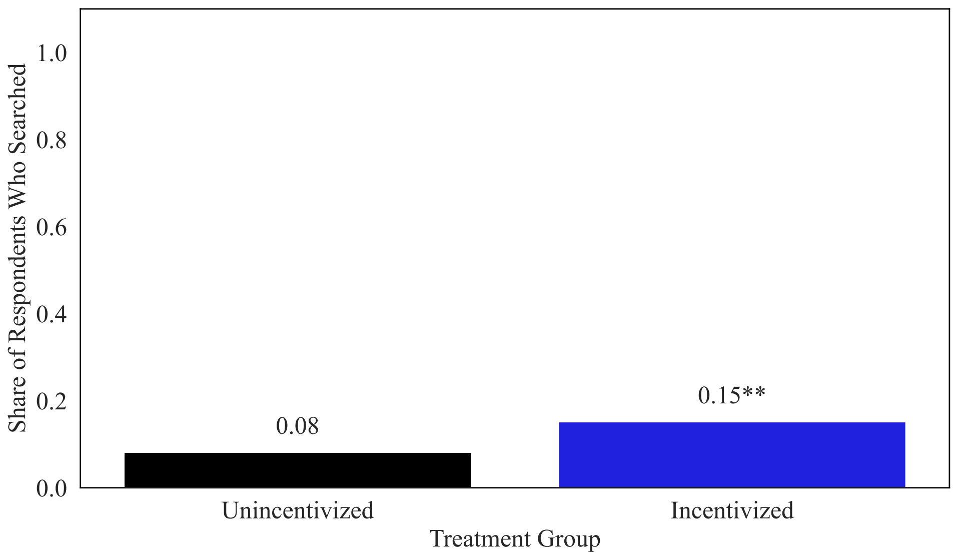


Figure 7: Information Search by Treatment Group

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). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Finally, incentives must be carefully designed. Overly complex schemes can confuse respon-

[28] 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.

[29] 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.

dents and reduce truthful reporting (Danz et al. 2022).[30] In Drobot et al. (2025), we compare the simple 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-based Implications

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

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

[30] 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.

of expectations formation: (i) a heuristic rule calibrated to expectations under marginal incentives, and (ii) a heuristic rule calibrated to expectations without incentives.

5.1 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.

5.2 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:

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

Table 13 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 14.

Table 13: 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.

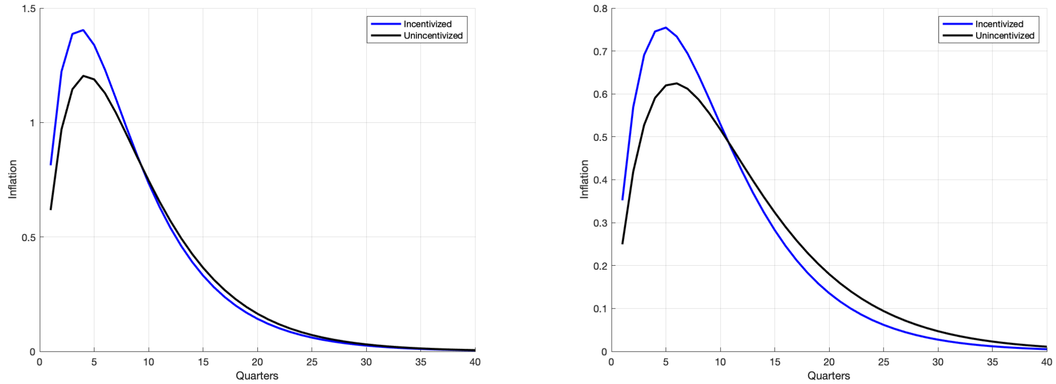
Table 14: Model Parameter Values

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).

5.3 Results and Implications

Figure 8 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.



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

Figure 8: 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.

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 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 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.

These results have important implications. For researchers, they suggest that unincentivized 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 payments alter attention, effort, and subsequently reported beliefs. Without incentives, information treatments may appear muted, especially when inflation and policy are already salient, leading to understated effects or even false negatives. By contrast, incentivized respondents exhibit higher learning rates and a reshaped distribution of beliefs, changing how we interpret household responsiveness to central bank communication. 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: $\$10 \times 2^{-\|\pi-\mathbb{E}(\pi)\|}$	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: Each value in this table represents the percentage of the sample belonging to the corresponding category. Survey of Consumer Expectations (SCE) sample values are taken from Armantier et al. (2017).

We consider how our various incentive schemes impact participants' hypothetical payoffs. To do this, we assume the FOMC's forecast of median inflation for 2025 (π_{2025}) is closer to the realized value in expectation. Using this as a basis for comparison, we calculate a participant i's forecast error as $error_i = |\pi_{2025} - \mathbb{E}_i(\pi_{2025})|$ and her hypothetical bonus

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

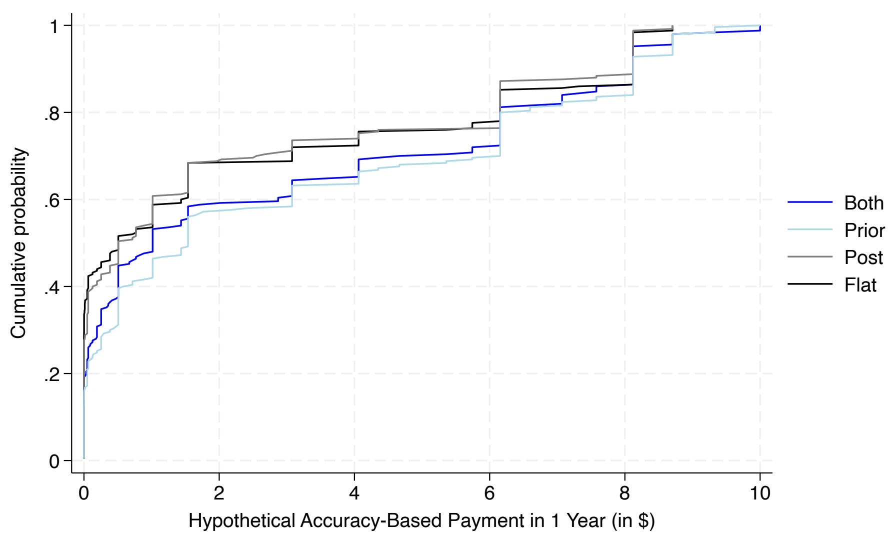
Test Type	Test Statistic	p-value
Mann-Whitney U Test (Distributions)	112515.0	0.006
Mood's Test (Median)	1.024	0.312
Wald's Test (IQR)	-2.487	0.013

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.

payment as $10 * (2^{-error_i})$. We depict the distribution of payoffs calculated this way across treatments in Figure A-1 and explore the significance of these results in Table A-6.

The punchline is that marginal incentives significantly increase hypothetical earnings. In *Both*, we predict in Table A-6 that payoffs will increase between approximately 24% ($p < .1$) in our baseline regression specification and 34% ($p < .05$) in a specification controlling for gender, education, and economic sentiment. In *Prior*, hypothetical earnings increase between 33% ($p < .01$) in our baseline specification and 49% in our full specification.

Figure A-1: Hypothetical Earnings from Inflation Expectations (The Priors)



Notes: This figure shows how treatments impact the hypothetical payoffs of participants calculated comparing point forecasts formed before receiving the FOMC's 2025 inflation forecast. This shows – assuming the FOMC's forecast is correct in expectation – that expected payoffs are significantly higher for subjects facing marginal incentives. 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.

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: Hypothetical Earnings From Point Forecasts of Inflation (The Priors)

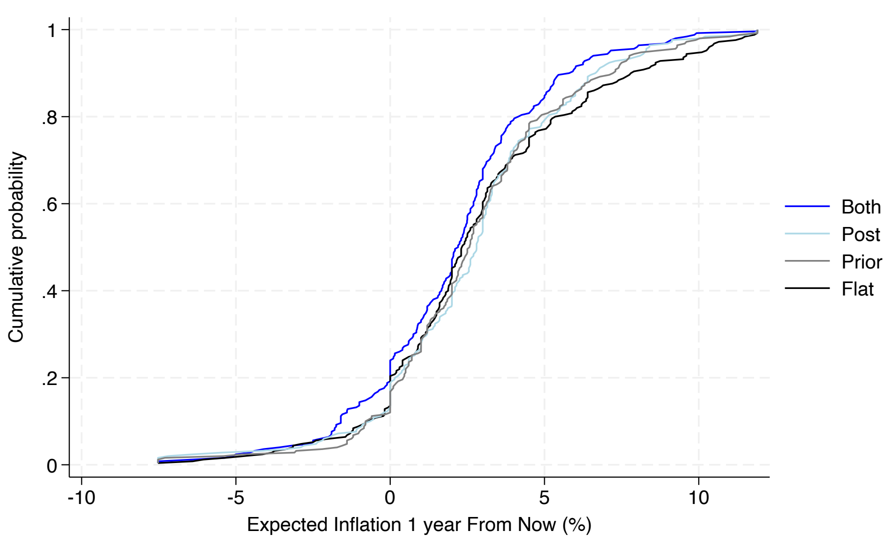
	(1) prior_hypothetical_payoff	(2) prior_hypothetical_payoff
Post	0.0138	-0.251
	(0.129)	(0.296)
Both	0.228*	0.514*
	(0.129)	(0.295)
Prior	0.394***	0.880***
	(0.129)	(0.295)
Constant	0.610***	-2.043
	(0.0909)	(1.470)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses

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

Notes: This table shows the effect of treatments on hypothetical earnings, as proxied by distance between reported priors and the FOMC's median 2025 forecast. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels.

Figure A-2: 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-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)	(2)
	Seconds	Second
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.

A2 Power Analysis

Table A-11: 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.

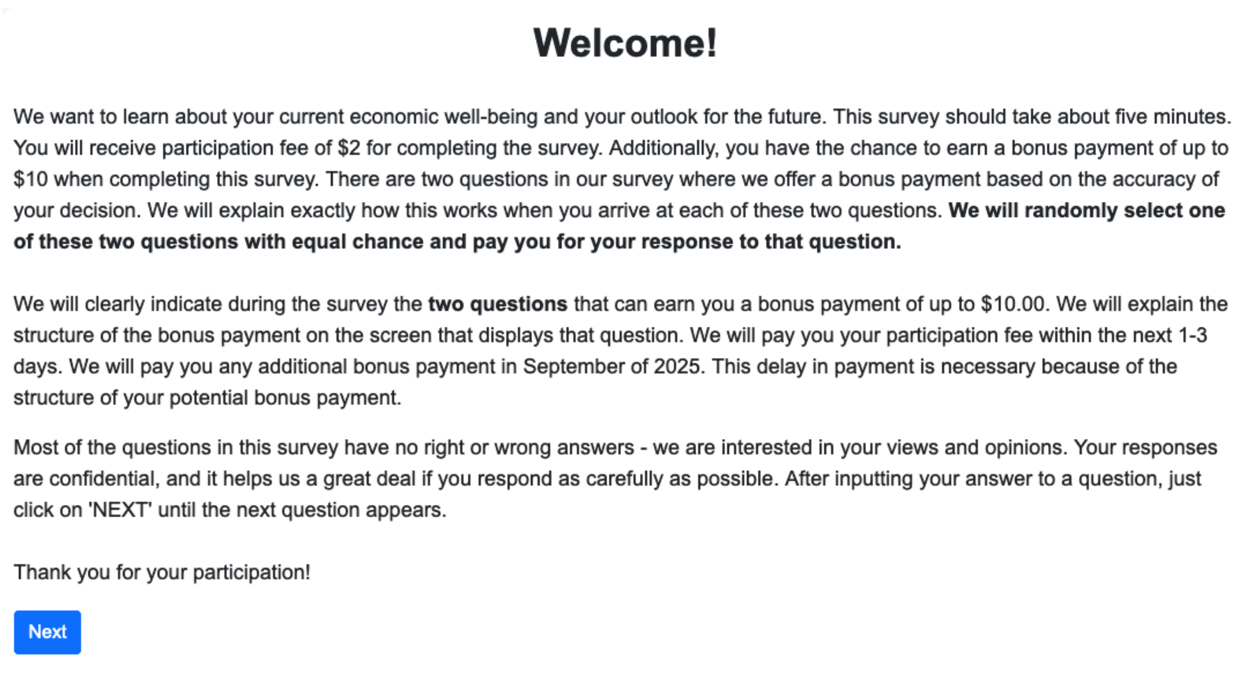
Conventional thresholds for interpreting the magnitude of effect sizes:

- **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.



Welcome!

We want to learn about your current economic well-being and your outlook for the future. This survey should take about five minutes. You will receive participation fee of $2 for completing the survey. Additionally, you have the chance to earn a bonus payment of up to $10 when completing this survey. There are two questions in our survey where we offer a bonus payment based on the accuracy of your decision. We will explain exactly how this works when you arrive at each of these two questions. **We will randomly select one of these two questions with equal chance and pay you for your response to that question.**

We will clearly indicate during the survey the **two questions** that can earn you a bonus payment of up to $10.00. We will explain the structure of the bonus payment on the screen that displays that question. We will pay you your participation fee within the next 1-3 days. We will pay you any additional bonus payment in September of 2025. This delay in payment is necessary because of the structure of your potential bonus payment.

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 as carefully as possible. After inputting your answer to a question, just click on 'NEXT' until the next question appears.

Thank you for your participation!

Next

Figure A-3: 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.

Please answer the following questions about your financial well-being:

Do you think you (and any family living with you) are financially better or worse off these days than you were twelve months ago? --------

And looking ahead, do you think you (and any family living with you) will be financially better or worse off twelve months from now than you are these days? --------

Looking ahead, do you think the economy in the United States will be stronger or weaker twelve months from now than these days? --------

Next

Figure A-4: General Questions

Next, we would like to ask you for your expectations about the economy. Of course, no one can know the future. These questions have no right or wrong answers - we are interested in your views and opinions.

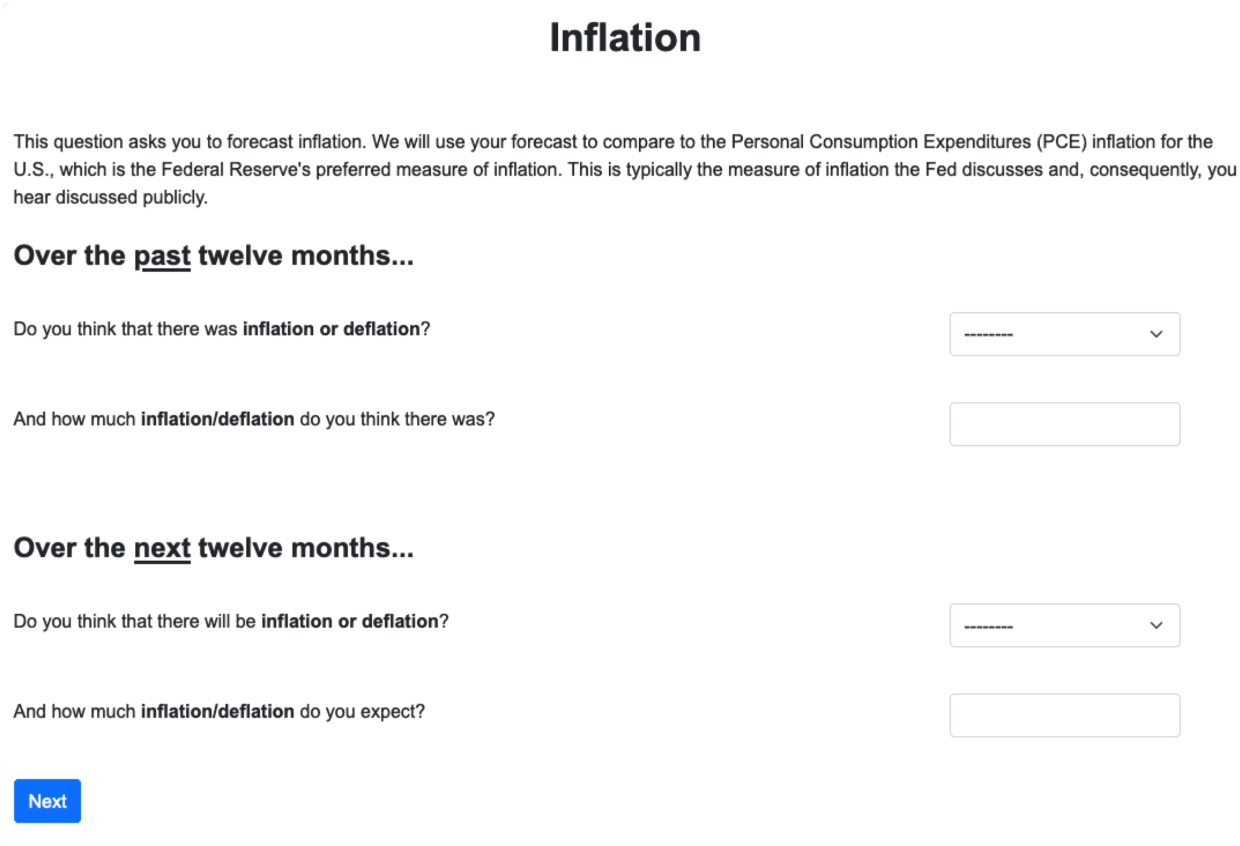
In some of the following questions, we will ask you to think about the percent chance of something happening in the future. Your answers can range from 0 to 100, where 0 means there is absolutely no chance, and 100 means that it is absolutely certain.

For example, numbers like:

2 and 5 percent may indicate "almost no chance"
18 percent or so may mean "not much chance"
47 or 52 percent chance may be a "pretty even chance"
83 percent or so may mean a "very good chance"
95 or 98 percent chance may be "almost certain"

Next

Figure A-5: Explanations



Inflation

This question asks you to forecast inflation. We will use your forecast to compare to the Personal Consumption Expenditures (PCE) inflation for the U.S., which is the Federal Reserve's preferred measure of inflation. This is typically the measure of inflation the Fed discusses and, consequently, you hear discussed publicly.

Over the past twelve months...

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

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

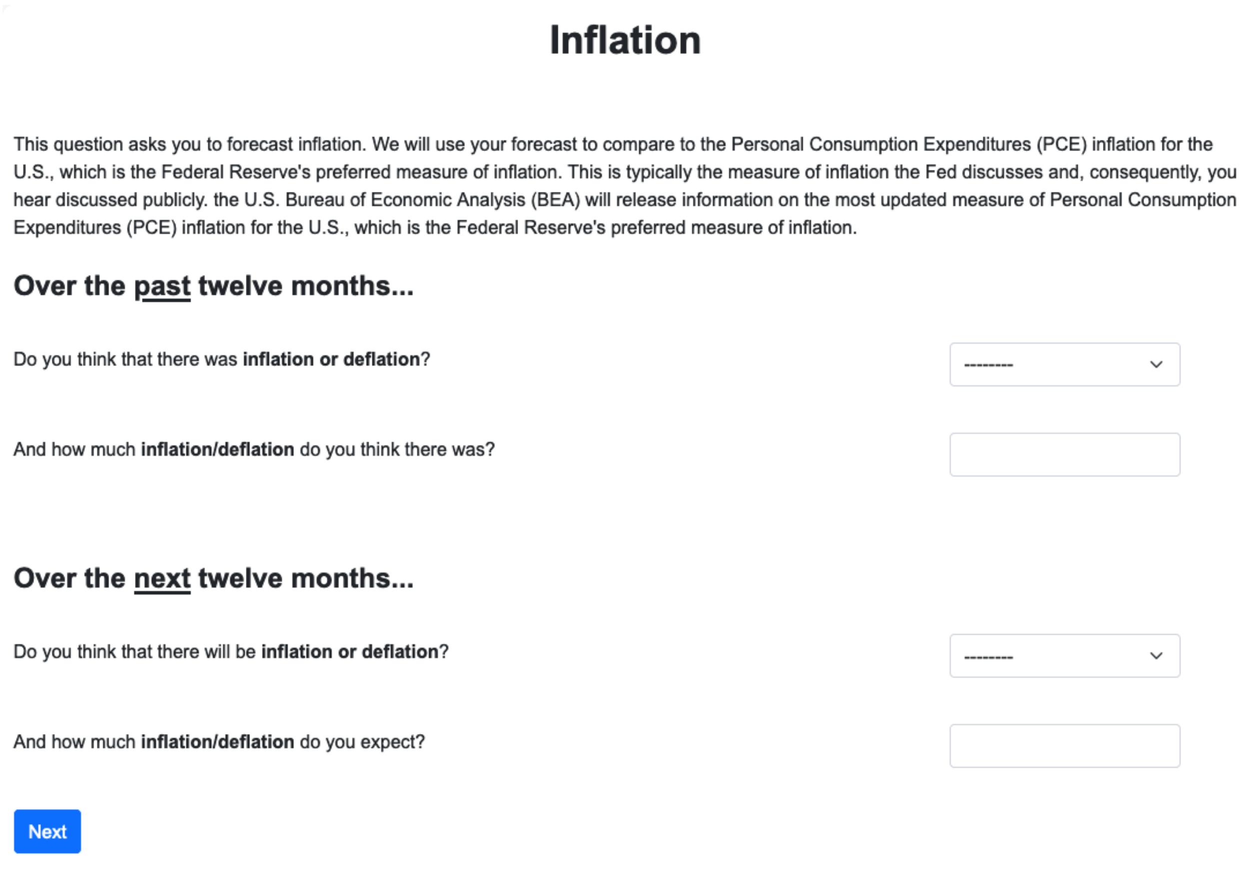
Over the next twelve months...

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

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

Next

Figure A-6: Inflation Point Forecast (Flat)



Inflation

This question asks you to forecast inflation. We will use your forecast to compare to the Personal Consumption Expenditures (PCE) inflation for the U.S., which is the Federal Reserve's preferred measure of inflation. This is typically the measure of inflation the Fed discusses and, consequently, you hear discussed publicly. 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.

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 (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-8: Inflation Point Forecast (Prior)

Inflation

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.

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?

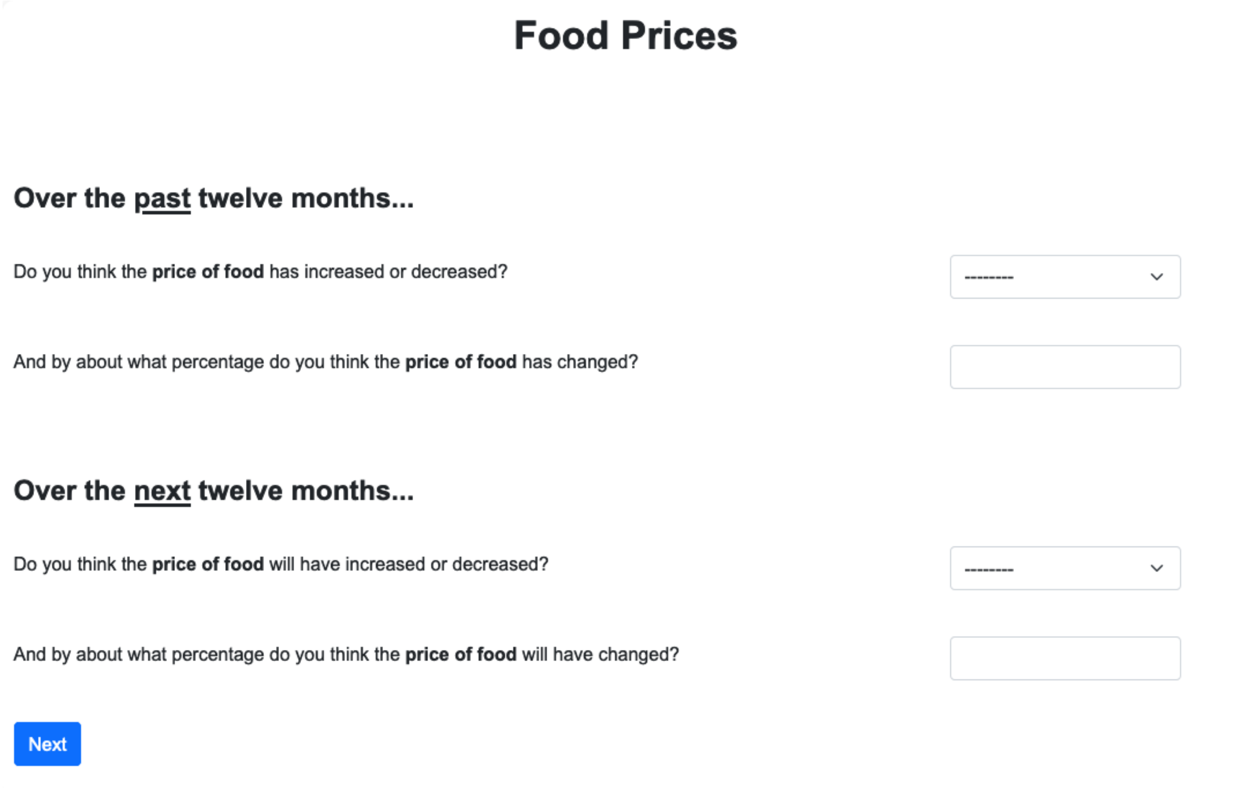
Over the next twelve months...

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

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

Next

Figure A-9: Inflation Point Forecast (Both)



Food Prices

Over the past twelve months...

Do you think the **price of food** has increased or decreased? --------

And by about what percentage do you think the **price of food** has changed?

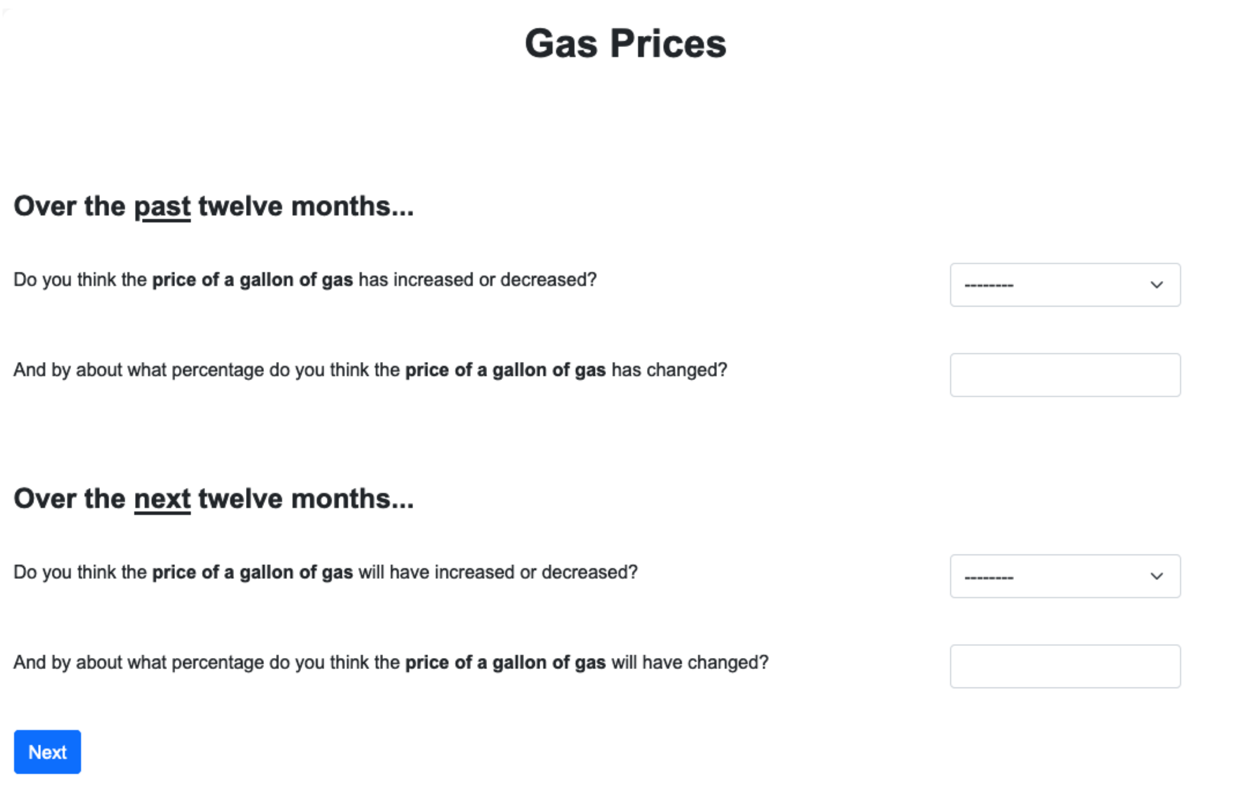
Over the next twelve months...

Do you think the **price of food** will have increased or decreased? --------

And by about what percentage do you think the **price of food** will have changed?

Next

Figure A-10: Food Point Forecast



Gas Prices

Over the past twelve months...

Do you think the **price of a gallon of gas** has increased or decreased? --------

And by about what percentage do you think the **price of a gallon of gas** has changed?

Over the next twelve months...

Do you think the **price of a gallon of gas** will have increased or decreased? --------

And by about what percentage do you think the **price of a gallon of gas** will have changed?

Next

Figure A-11: 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-12: 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-13: Food Density Forecast

Gas Prices

Now we would like you to think about the different things that may happen to **gas 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 gas 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 gas 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 a gallon of gas will increase by 12% or higher: %

the price of a gallon of gas will increase by between 8% and 12%: %

the price of a gallon of gas will increase by between 4% and 8%: %

the price of a gallon of gas will increase by between 2% and 4%: %

the price of a gallon of gas will increase by between 0% and 2%: %

the price of a gallon of gas will decrease by between -2% and 0%: %

the price of a gallon of gas will decrease by between -4% and -2%: %

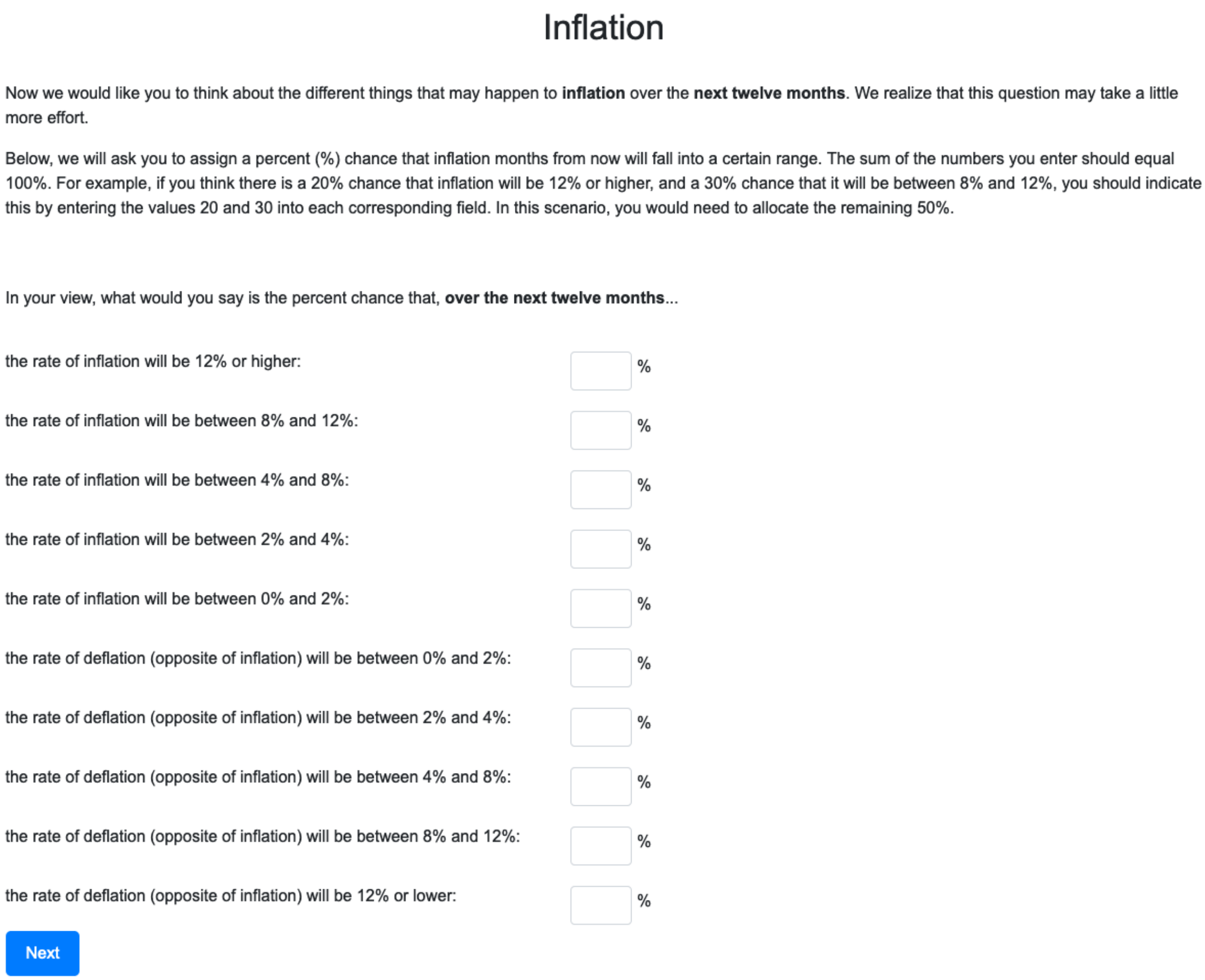
the price of a gallon of gas will decrease by between -8% and -4%: %

the price of a gallon of gas will decrease by between -12% and -8%: %

the price of a gallon of gas will decrease by -12% or lower: %

Next

Figure A-14: Gas Density Forecast



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%.

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 (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-16: 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%: ____ %

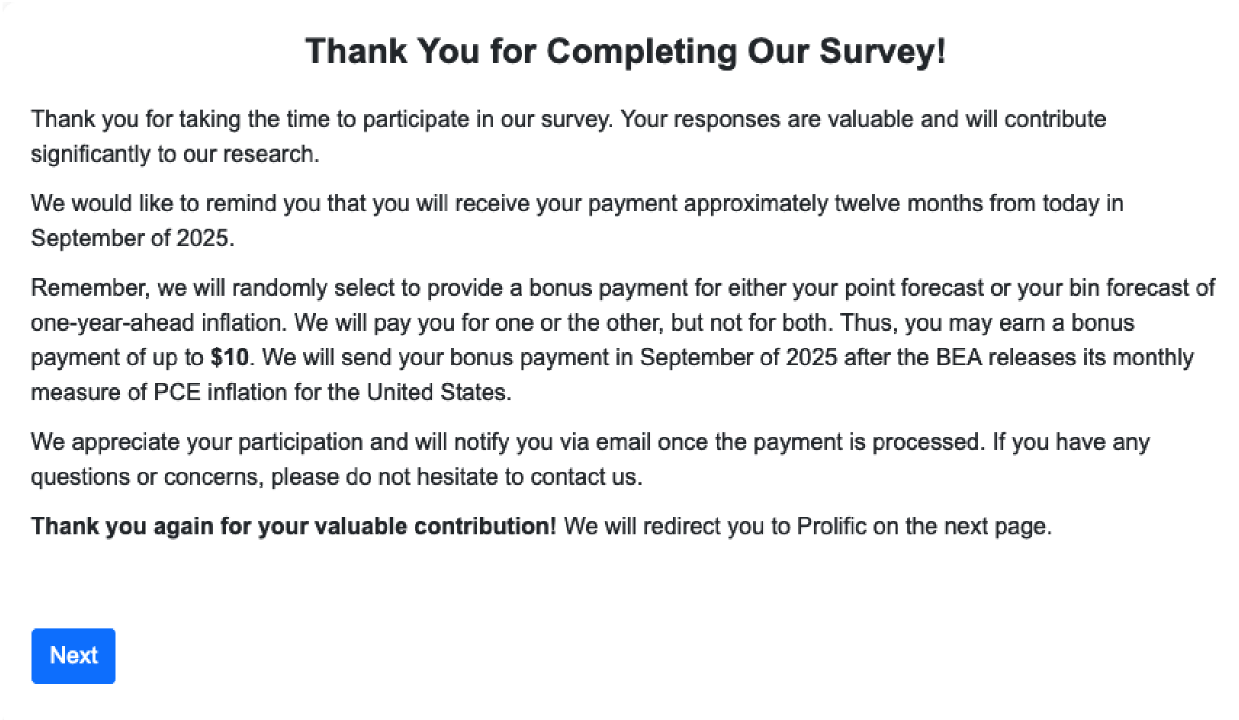
the rate of deflation (opposite of inflation) will be between 4% and 8%: ____ %

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-17: Inflation Density Forecast (Both)



Thank You for Completing Our Survey!

Thank you for taking the time to participate in our survey. Your responses are valuable and will contribute significantly to our research.

We would like to remind you that you will receive your payment approximately twelve months from today in September of 2025.

Remember, we will randomly select to provide a bonus payment for either your point forecast or your bin forecast of one-year-ahead inflation. We will pay you for one or the other, but not for both. Thus, you may earn a bonus payment of up to **$10**. We will send your bonus payment in September of 2025 after the BEA releases its monthly measure of PCE inflation for the United States.

We appreciate your participation and will notify you via email once the payment is processed. If you have any questions or concerns, please do not hesitate to contact us.

Thank you again for your valuable contribution! We will redirect you to Prolific on the next page.

Next

Figure A-18: 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-12 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-12 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-12 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-12: Incentivizing Updated Expectations: Intercept and Slope Estimates

	No Controls		With Controls	
Treatment	**Intercept** (1)	**Slope** (2)	**Intercept** (3)	**Slope** (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-13 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-13: 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-19 and Figure A-20 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-21 and Figure A-22, respectively. The wording used to elicit the one-year-ahead point and density forecasts is shown in Figure A-23 and Figure A-24. Similarly, the (unincentivized) three-year-ahead counterparts are presented in Figure A-25 and Figure A-26. The spending question is shown in Figure A-27. Figure A-28 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-19: 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-20: 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-21: 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-22: 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-23: 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-24: 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-25: Three-Year-Ahead Point Forecast

And in your view, what would you say is the percent chance that, **over the 12-month period between March 2027 and March 2028 ...**

(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-26: Three-Year-Ahead Density Forecast

Now think about your total household spending, 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).

Over the next 12 months, what do you expect will happen to the total spending of all members of your household (including you)?

Please choose one.

Over the next 12 months**, I expect my total household spending to...**

- ○ increase by 0% or more
- ○ decrease by 0% or more

By about what percent do you expect your total household spending to increase? 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 by

Figure A-27: 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.

In providing your estimate for the inflation rate over the next 12 months, did you consult any source?

Please select only one.

- ○ ⋆ Yes, I used online sources (please specify)
- ○ ⋆ Yes, I used other sources (please specify)
- ○ No

Figure A-28: Searching for Information