Incentivizing Inflation Expectations*

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Abstract

Accurate inflation expectations are crucial for economic modeling and policymaking. Despite the well-established importance of marginal incentives in experimental economics, all major surveys of inflation expectations pay flat participation fees. This lack of incentives extends to many information provision experiments – often designed as randomized controlled trials (RCTs). We show that marginal incentives significantly alter the expectations distribution, reduce upward bias and cross-sectional disagreement, close the gender-expectations gap, and increase effort. In an RCT, they lead to greater responsiveness to information. These findings underscore the importance of marginal incentives in surveys and experiments to enhance data validity and inform policymaking.

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1 Introduction

There is increasing recognition of the value of more flexible approaches to measurement, such as incorporating stated beliefs or choices, rather than relying solely on observed behavior. These approaches can support the development and estimation of richer and more realistic models while relaxing the need for strong identification assumptions, and they can help design more effective policies (e.g., see Almås et al. (2024)). One area where such approaches have gained particular prominence is in the study of macroeconomic beliefs – particularly inflation expectations – which are central to both empirical research and policymaking. Survey-based measures of household expectations and information provision experiments embedded within surveys have become increasingly prominent in macroeconomics. These tools have deepened our understanding of how households form expectations and have challenged core assumptions of rational expectations in macroeconomic models.[1] Central banks now routinely rely on data from such surveys to inform both conventional and unconventional monetary policy decisions.[2]

Despite their growing importance and potential relevance for quantitative assessments, most surveys and embedded RCTs rely on flat-fee or non-incentivized payment structures. This stands in contrast to experimental economics, where the use of marginal incentives – payments that are performance based – is a long-established method for eliciting truthful beliefs and ensuring internal validity (Smith 1976). A rich literature has demonstrated the advantages of such mechanisms for reducing noise and misreporting in elicited data.[3] Yet marginal incentives remain rare in macroeconomic surveys, and the few existing studies that use incentives for macroeconomic beliefs often use complex or indirect incentive schemes and report mixed results.[4] Moreover, few studies test how marginal incentives affect belief updating – a key margin in information provision experiments. This omission may introduce systematic measurement error and may lead to false negatives when detecting treatment effects or evaluating learning in information provision settings.

In this paper, we design a controlled experiment to make two central contributions. First, we provide causal evidence on how marginal incentives affect the elicitation of inflation expectations. Second, we quantify how marginal incentives impact belief updating and estimated learning rates in information provision experiments. Our design replicates portions of the

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

[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).

New York Fed's Survey of Consumer Expectations (SCE) methodology and incorporates a standard information provision intervention, following common RCT practices.[5] Participants were randomly assigned to one of four treatments: a flat-fee control group and three marginal incentive treatments applied to prior, posterior, or both sets of beliefs. Incentives were calibrated to equalize expected earnings across groups, ensuring comparability with standard survey remuneration. This allows us to cleanly estimate the effect of marginal incentives on belief distributions and information processing.

We find that marginal incentives significantly shift the entire distribution of reported inflation expectations. Participants exposed to marginal incentives provide less extreme forecasts, reduce upward bias (mean point forecasts fall from 6.1% to 2.7%), 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. In addition, we find that incentives eliminate the gender gap in inflation expectations – a persistent puzzle in the existing survey literature. These patterns emerge for both, elicited prior and posterior beliefs. We explain these effects with increased attention and effort: incentivized respondents round less and rely less on backward-looking heuristics. In the RCT setting, marginal incentives lead to higher learning rates, indicating stronger updating in response to new information than measured without incentives.

Marginal incentives impact the entire distribution of reported inflation expectations, but does this imply that beliefs are more informative? To address this, we run an additional experiment – this time without any information intervention and with additional questions on spending, longer-term expectations and information search behavior. We find that incentivized inflation expectations are more strongly correlated with respondents' spending plans, suggesting that incentives elicit more informative beliefs. In addition, incentives lead to stronger internal consistency between point and density forecasts. Incentivizing one-year-ahead expectations also lowers reported three-year-ahead expectations, indicating positive spillovers to longer-term beliefs. Finally, we observe only a small increase in reported search behavior, suggesting that the substantial shifts in elicited belief distributions are unlikely to be fully explained by increased information acquisition alone.

These results have important implications for macroeconomic research and policy design. First, we show that marginal incentives can substantially reduce measurement error in survey expectations, offering a low-cost tool to improve belief elicitation without altering survey content or increasing respondent burden. Second, our results have important implications for quantitative assessments. Calibrating a standard New Keynesian model to our data

[5]Our focus on the SCE is due to its widespread use in both academic research and policymaking (e.g., Armantier et al. 2024, D'Acunto and Weber 2024, and Weber et al. 2022).

reveals that unincentivized surveys may overstate inflation persistence, potentially leading to misattributed structural frictions and unnecessarily prolonged policy responses. Third, by enhancing 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, our findings demonstrate how bridging survey and experimental methods through targeted incentive design enables researchers to more accurately measure expectations, estimate learning from information, and evaluate 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 provides a model-based illustration of how incentive-induced changes in expectations affect inflation dynamics in a simple New Keynesian framework. Section 5 discusses the potential concerns about the use of incentives. Section 6 concludes.

1.1 Related Literature

Incentives and truthful reporting A large body of work in experimental economics has examined the role of incentives in belief elicitation. Following the induced value theory of Smith (1976), studies have shown 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). More recently, Charness et al. (2021) and others have argued that simple, non-incentive-compatible mechanisms may outperform more complex scoring rules, particularly when respondents have limited cognitive bandwidth or numeracy.

Incentives in macroeconomic belief surveys The macroeconomic literature is divided on whether incentivized elicitations improve belief accuracy. The problem of 'cheap talk' for elicited inflation expectations has been touched on by a few studies, raising doubt about data accuracy or reliability because respondents often lack proper economic incentives (Pesaran and Weale 2006, Manski 2004). For instance, Inoue et al. (2009) question the accuracy of reported inflation expectations, as they find that implicitly measuring inflation expectations through consumption data does a better job at predicting actual inflation than the reported beliefs, especially for the lower educated. Keane and Runkle (1990) question whether reported expectations are simply cheap talk or reflect actual beliefs. They find evidence for the latter – at least for the case of professional forecasters who have strong incentives to report rational and accurate expectations for reasons concerning their professional credibility and reputation. These circumstances do not directly apply to households. Armantier et al. (2015) find a strong correlation between non-incentivized inflation expectations and investment choices in an incentivized investment experiment, except for respondents of lower education and financial literacy, suggesting overall that marginal incentives might not always be necessary. Roth and Wohlfart (2020) report no significant effect of incentives on beliefs about the likelihood of a recession. Similarly, Andre et al. (2022) find no effects for incentives on reported unemployment expectations. Pooling unemployment and inflation expectations, the authors find no significant difference between incentivized and unincentivized beliefs overall in a joint test. However, they do find that incentivizing inflation expectations shifts these moderately closer to expert forecasts. In addition, incentives increase the time taken to respond, a measure for exerted effort. Notably, Andre et al. (2022) use a clever approach to explore whether incentives affect subjective beliefs by linking rewards to second-order beliefs – participants were incentivized to match the average expert's forecast rather than their own subjective inflation expectations. While this method provides valuable insights into how incentives might shape beliefs about expert opinion, it differs from approaches that focus on first-order beliefs, where forecasts are benchmarked against actual future outcomes.

Our approach builds on these insights but represents a significant departure from previous work by employing an incentive structure within a context closely aligned with the SCE. This ensures that the results from our treatments can be readily interpreted against a backdrop of previous studies, thus facilitating the interpretation and integration of our findings into the existing literature. Additionally, our incentive structure is both more direct and less complex. Our experiment *directly* incentivizes both point and probabilistic inflation forecasts, ensuring participants are motivated to provide accurate predictions and limiting the potential for confusion driven through complex incentives. Indeed, Danz et al. (2022), Abeler et al. (2023) and Drobot et al. (2025) demonstrate that complex incentive schemes can lead to misunderstandings, potentially resulting in less accurate or truthful reporting (see also Charness et al. (2021)). We also directly incentivize updating in our study. This requires participants to update their beliefs after receiving new information, a crucial component that allows us to observe how marginal incentives affect not just initial beliefs but also learning and belief adjustments over time. This design is crucial for understanding how participants process and incorporate new information, something previous studies have not fully explored.

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. In the field, the incentives to pay attention can arise from changing economic conditions (Braitsch and Mitchell 2022, Bracha and Tang 2024, Weber et al. 2025, Wabitsch 2024) or from endogenous factors such as individual stakes and relevance (Gaglianone et al. 2022).

2 Experimental Design

We pursue two primary objectives that shape our experimental design. First, we investigate whether and how the implementation of marginal incentives alters survey-based belief measures. Second, we examine whether marginal incentives can influence belief updating in a survey-based RCT, a widely adopted methodology in experimental macroeconomics. To achieve these goals, our experiment must generate reliable survey-based beliefs free from the influence of extraneous information provision while simultaneously conducting an information provision experiment.[6]

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 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 (Armantier et al. 2024, 2017, Bruine de Bruin et al. 2010). We focus on the SCE

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

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.[7] Worth noting is that we adopted the welcoming language of the SCE intended to activate participants' intrinsic motivation (see Figure A-3).

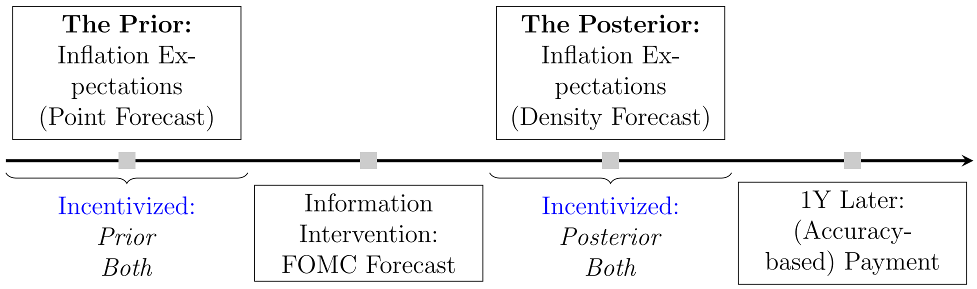


Figure 1: Experimental Design

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.

We implemented a between-subjects design by randomizing 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.

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 bin 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

[7] 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.

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.[8] This scoring rule is common in learning-to-forecast experiments in experimental macroeconomics and is easy to explain.[9] In *Post*, we pay participants $\$10 * weight_i$ where $0 \leq weight_i \leq 1$ is the probability weight assigned by the participant to bin i that contains realized inflation. For example, if inflation turns out to be 5% and a participant assigned probability weight .2 to the bin for 4% to 8%, then the participant would earn $10*.2=$2.[10] 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 together form a design that cleanly isolates the role of marginal incentives in belief formation and updating within a survey-based information provision experiment. The *Flat* treatment serves as a benchmark without marginal incentives, which aligns with the incentives in widely-used economic surveys.[11] The *Prior* and *Post* treatments allow us to examine how incentives applied at different stages – before or after information provision – affect both the level of expectations and the degree of updating. For example, directing effort via incentives in the *Prior* treatment may reduce responsiveness to new information relative to *Flat*, while incentivizing the *Post* forecast may amplify updating by encouraging greater attention to the provided information. These treatments are particularly informative for understanding how incentivized attention or cognitive effort influences

[8]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 forecast is fundamentally a setting 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.

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

[10]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.

[11]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).

learning. The *Both* treatment holds incentives constant across the information intervention, allowing us to test whether consistency in incentive structure affects 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 analyzed average forecast errors using New York Fed's one-year-ahead forecasts and actual inflation data from FRED. The average forecast error was 1.68 pp across the entire historical sample and 1.16 pp in the most recent six observations. Based on an estimated annual discount rate ($\beta = 0.8$) from Warner and Pleeter (2001), we set the maximum payoff for a perfect forecast so that a participant's expected earnings in present-value terms align with the time-value for participants in the New York Fed's SCE. For our 5-minute survey, this results in a total payout of about \$6, with 33% (\$2) allocated as a show-up fee.

In *Prior*, we apply this marginal incentive scheme to the point forecast of inflation collected before the information provision. In *Post*, the scheme is applied to probabilistic forecasts collected after the information provision. In *Both*, we inform participants we will impose marginal incentives on either the point or probabilistic forecast with equal probability, but not both.

2.1 Hypotheses

Before moving on to the results, we offer two hypotheses regarding the impact of marginal incentives in our experiment. These hypotheses are grounded in the induced value theory, namely the notion that performance-based financial incentives enhance cognitive effort and reduce biases in self-reported data, leading to more reliable and valid measures of economic beliefs. This logic is the basis of the foundational principle of employing marginal incentives to discipline choice data and reduce measurement error in experimental economics (Smith 1976, Smith and Walker 1993, and Camerer and Hogarth 1999). While the induced value theory was originally developed for valuation tasks (like auctions or market experiments), its logic can be extended to belief elicitation (see Schotter and Trevino 2014, Schlag et al. 2015, Charness et al. 2021, Healy and Leo 2026 for reviews). Specifically, in our context, we expect incentives to reduce upward bias, forecast errors, and the occurrence of outliers.[12] Further, they will increase individuals' attention to the provided information.

Hypothesis 1 (Survey-Based Beliefs): *The cross-sectional distribution of inflation expectations exhibits reduced upward bias (lower mean) and disagreement (lower variance) with marginal incentives. Further, with marginal incentives, forecasts are closer to those of professional forecasters.*

Hypothesis 2 (Learning Rates): *In the context of the RCT, marginal incentives increase the learning rates. Participants who receive marginal incentives adjust their beliefs more substantially and consistently in response to the information provided, compared to those without such incentives.*

2.2 Data

We collected 1,000 observations – 250 per treatment – from US 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 groups and with the SCE sample).[13] 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

[12] In the inflation expectations literature, upward bias refers to the tendency of individuals or survey respondents to systematically overestimate future inflation compared to actual inflation outcomes. This bias has been widely documented in household surveys and was also present at the time of our survey.

[13] 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.

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 how 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 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), which do not include marginal incentives. The *Flat* and *Post* treatments mimic the typical approach used in all major macroeconomic surveys and therefore reflect the incentive mechanism underlying the majority of the data used in belief-based empirical macroeconomics research.

Our results show that imposing marginal incentives when eliciting inflation expectations (i.e., in *Prior* and *Both*) generate significantly different belief distributions than do flat-fee incentives (i.e., the *Flat* and *Post* treatments). The primary impact of these incentives manifests in the expectations of respondents who foresee inflation, rather than deflation. Under marginal incentives, respondents expecting inflation predict significantly lower price growth relative to unincentivized treatments. For those anticipating deflation, we similarly observe a muted expectation of price change under marginal incentives, suggesting that the incentives temper both inflationary and deflationary beliefs.

This distinction arises despite holding constant across treatments all other aspects of the incentives, including the timing and expected amounts of payments. We show that merely altering the structure of belief elicitation in a feasible way that imposes no additional cost relative to prevailing approaches can substantially change the nature of respondents' reported expectations. Importantly, this change occurs without modifying participants' perceptions of the data-generating process, introducing asymmetric information, or altering other fundamental aspects of the decision environment.

Table 2 summarizes the mean and standard deviation of point forecasts for participants who faced marginal incentives or not. The unincentivized group has a significantly higher mean (6.13) compared to the incentivized group (2.73), and the standard deviation is also larger in

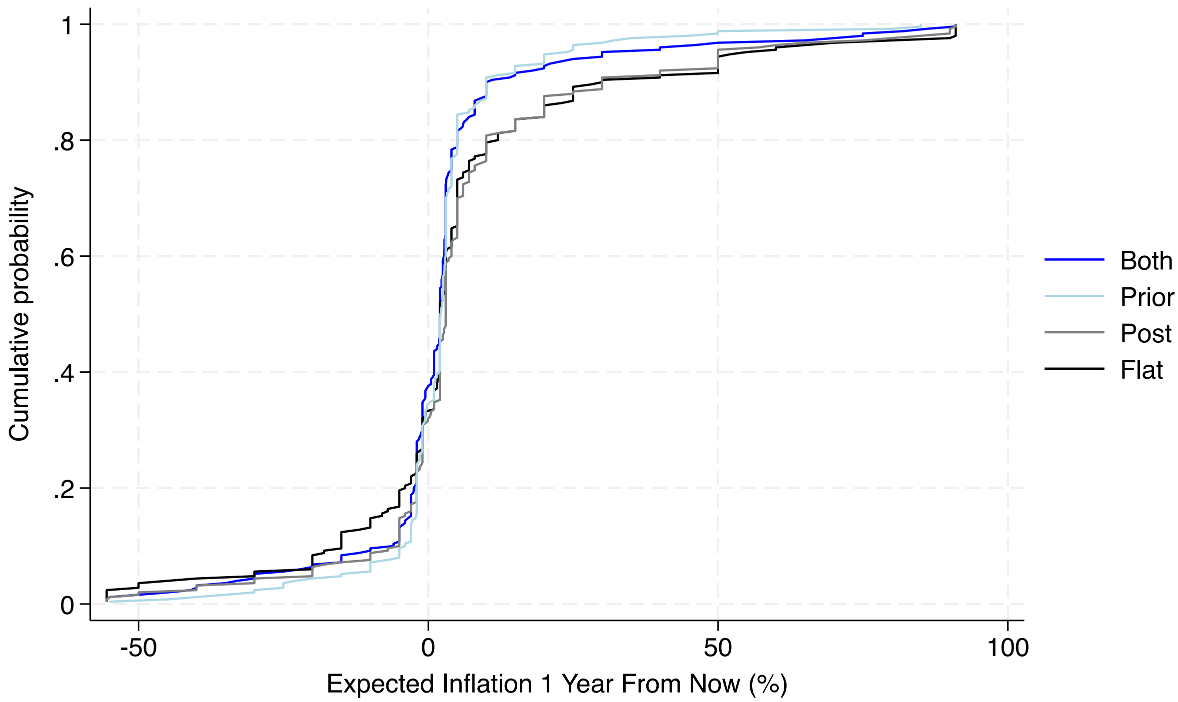


Figure 2: CDFs of Expected Inflation By Treatment

Notes: The 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.

the non-incentivized group (23.78 vs. 16.98), indicating higher cross-sectional disagreement among unincentivized forecasters.

We test for the equality of variance across incentive schemes using both Levene's tests and the F-test for variance ratios. All tests strongly reject the null hypothesis that the variances are equal ($p-values < 0.001$). Thus, imposing marginal incentives reduces the mean of point inflation expectations and leads to lower cross-sectional forecast disagreement. Moreover, the incentivized group exhibits a lower median and lower IQR (Table A-3) – robust measures of central tendency and disagreement that are not sensitive to outliers and are the preferred metrics reported by the SCE.

Our first hypothesis, detailed in Section 2, posits that marginal incentives significantly alter the distribution of inflation expectations. The results strongly support this hypothesis. As shown in Table A-4, the coefficients for *Both* and *Prior* indicate that marginal incentives significantly reduce expectations of inflation and deflation rates, compared to the unincentivized *Flat* treatment. Specifically, respondents in the *Prior* group report significantly lower expectations for price changes compared to those in the *Flat* treatment, with absolute forecast values approximately half as large on average ($p < 0.001$). The effect in *Both* is somewhat

Table 2: Summary Statistics and Variance Comparison of Inflation Expectations

	Mean	Standard Deviation	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*.

less pronounced but still substantial, with respondents providing significantly lower absolute forecasts – about a third lower on average compared to *Flat* ($p < 0.01$). These effects are robust to controlling for age, race, gender, education, income, political affiliation, primary grocery shopper status, economic sentiment, and state.

As an additional exercise, we focus on the impact of incentives on extreme values and define the highest 10% of absolute prior inflation expectations as extreme forecasts. The logistic regression results in Table A-4 columns (3)-(4) and Table A-5 indicate that respondents in the non-incentivized groups are more likely to report extreme values compared to the *Prior* and *Both* groups. Specifically, the *Flat* group is 222% more likely, the *Post* group is 181% more likely than in *Prior*. Interestingly, also the *Both* group is 91% more likely than in *Prior* to provide such forecasts.

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.

We also find evidence that marginal incentives reduce upward bias and align respondents' expectations more closely to those of professional forecasters, who have historically exhibited

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.

greater accuracy (Carroll 2003). Specifically, we compare data from each of our treatments with the most recent mean PCE forecast from the Survey of Professional Forecasters (SPF) in Table 4. While expectations under *Flat* (5.20%) and *Post* (6.75%) are relatively high and significantly higher than those of professional forecasters, implementing marginal incentives in *Prior* and *Both* leads to expectations (2.80% and 2.65% respectively) that align more closely with professional forecasts from the SPF (2.11%).[14]

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 that 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.

We also consider how our various incentive schemes impact participants' hypothetical payoffs. To do this, we assume the Fed'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 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.

[14] Unfortunately, we do not have access yet to the microdata from the New York Fed SCE, which prevents us from conducting formal comparison tests between their data and ours. However, the New York Fed provided us with summary statistics, specifically the mean and standard deviation of winsorized forecasts for September 2024, which are 6.03 and 17.3, respectively. These figures suggest that the mean forecasts in our unincentivized samples are similar to those in the SCE, while the standard deviation tends to be higher in our data. This is perhaps due to the fact that some of the SCE respondents are experienced, e.g., see Kim and Binder (2023).

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.

The influence of incentives on expectations highlights the need for careful consideration when interpreting survey-based belief measures and the conclusions drawn from them. If belief elicitation is highly sensitive to the presence of incentives, it becomes crucial to either incorporate incentives to enhance experimental control, reliability, and accuracy or correct for potential biases that may arise in their absence.

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.[15] Concisely, female survey participants typically report significantly higher inflation expectations than men (e.g., Bruine de Bruin et al. 2010 or D'Acunto et al. 2021).

Within the context of our study, we find that the implementation of marginal incentives entirely resolves this puzzle, aligning inflation expectations across genders. We estimate a series of OLS regressions for each treatment condition: *Flat*, *Post*, *Both*, and *Prior* where we project inflation expectations gathered before the information provision experiment (i.e. priors) onto an indicator variable denoting whether a participant was female. This method enables us to independently assess the impact of gender within each specific treatment context.

The regression equation for each treatment T is specified as:

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

The regression results, summarized in Table 5, reveal how incentives impact the gender gap in inflation expectations. In the absence of marginal incentives (*Flat*), female respondents have significantly higher inflation expectations (6.995, $p < .05$) than do their male counterparts. This finding is consistent with existing empirical literature that suggests that women often report higher inflation expectations. This result also appears in *Post*, albeit muted and only

[15]Note that we use the terms gender and sex interchangeably in this paper, as is common in related literature, though we recognize that they may not always align. For accuracy, the variable we use 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: The 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.

marginally significant.

Remarkably, marginal incentives eliminate the significant difference in inflation expectations across genders. This is true for *Both* (3.878, $p > .1$) and *Prior* (2.757, $p > .1$).

Further, Figure 3 shows that marginal incentives eliminate the gender difference in expectations because they act significantly more strongly on belief formation for females than they do for males.

These findings suggest that the puzzle of gendered expectations – where women report higher inflation expectations than men – diminishes by using marginal incentives. Specifically, women appear to respond more strongly to incentives during belief elicitation, leading to more moderated and comparable expectations with men. This responsiveness effectively resolves the observed gender discrepancies in survey-based belief measures, as incentivized belief elicitation promotes more consistent and aligned inflation expectations across genders.

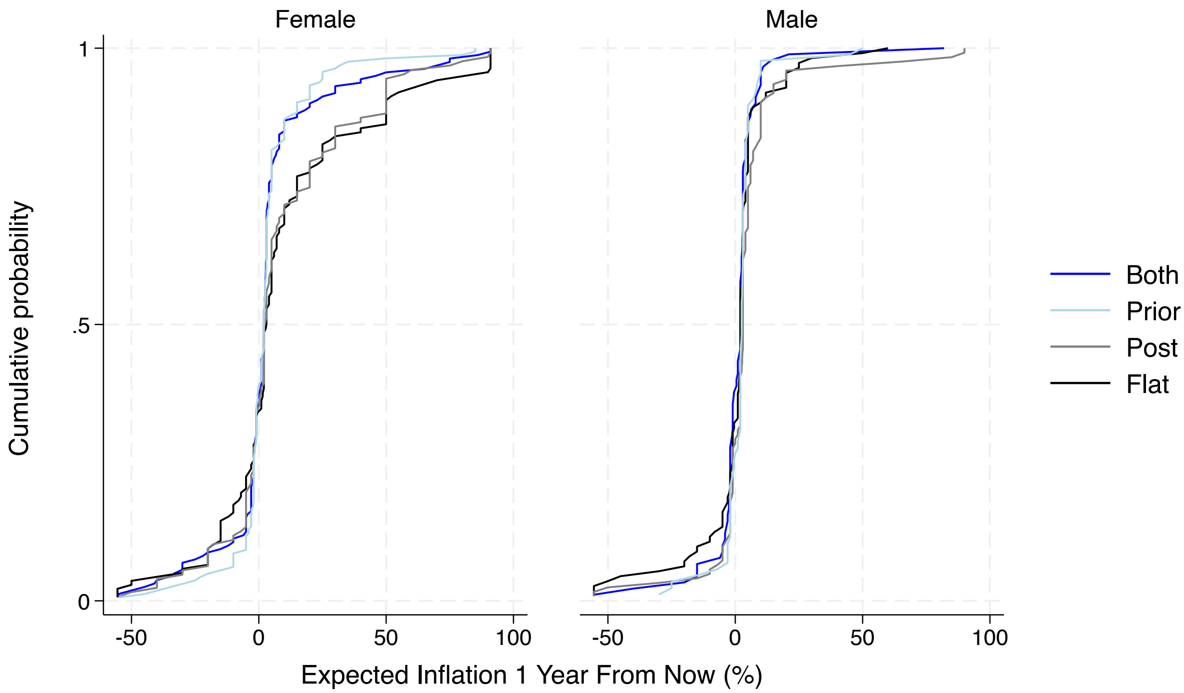


Figure 3: Effects of Incentives on Expectations by Gender

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

3.3 Incentives Raise Effort and Attention

Why do incentivized expectations become more consistent with the SPF? 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 rules of thumb, 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 2024, Jo and Koplack 2025, Drobot 2025).

Under flat incentives, there is lower or little motivation to exert effort, retrieve information, recall knowledge, or offer more accurate responses. In contrast, when incentives are introduced, they increase the benefits of effort, leading to forecasts that are more informed. Below, we present evidence consistent with this interpretation.

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 the link between perceptions and expectations, suggesting that respondents move away from simple extrapolation of (perceived) past inflation.

Figure 4 shows this relationship, estimated by regressions where expected inflation is the dependent variable and perceived inflation is the independent variable. The findings indicate that in the incentivized group (*Prior*), this relationship is no longer statistically significant (see columns (1)-(4) in Table 6). Thus, incentives shift the range of values respondents consider likely, leading them to form expectations within a more informed range.

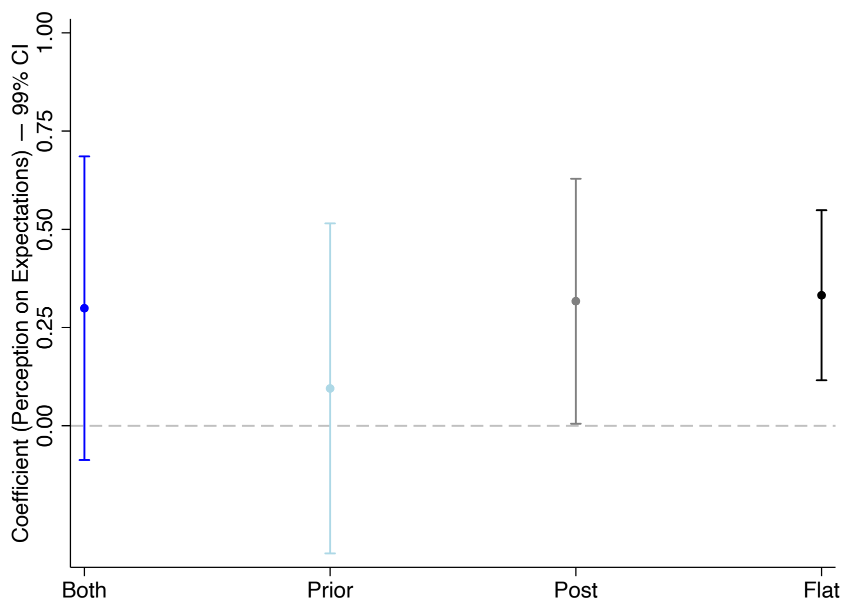


Figure 4: Inflation Expectations and Perceptions

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

This result is particularly striking, given that perceptions and expectations are elicited on the same survey page. One possible interpretation is that marginal incentives reduce reliance on simple backward-looking forecasting heuristics, which is a sensible response given the specific time period during which the survey was conducted.[16] Instead, incentivized respondents

[16] 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.

seem to engage in more deliberate recall of information to generate more intentional inflation forecasts.

This finding has important implications. They challenge the common assumption that inflation expectations closely track perceptions. Our results suggest that this link may reflect a survey response heuristic rather than a genuine belief formation heuristic. Recognizing this distinction is crucial for both measurement and modeling of expectations in economic research, which we show in Section 4.

3.3.2 Attention to the Survey

Another way to determine cognitive effort is through the attention paid to the survey. In a similar spirit to Bracha and Tang (2024), who examine perception errors in economic decision-making, we construct a measure of survey inattention as an Absolute Perception Error (APE) – the absolute difference between a respondent's perceived inflation and the most recent actual inflation rate available at the time of the survey.[17] 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).

We find that marginal incentives play a crucial role in significantly reducing the APE gap. Incentivized groups exhibit significantly lower inattention (or, equivalently, greater attention) to the survey (see column (5) of Table 6). This is presumably because of spillover effects associated with incentives for the questions on inflation expectations. Specifically, respondents may use inflation perceptions as an input in their inflation expectations.

We observe a gender gap, as women have noticeably higher APE (see Table A-8). This result complements Braitsch and Mitchell (2022), who construct a measure of inattention based on the consistency of responses to the SCE point and density forecast questions and show that women are less attentive than men when forming inflation expectations.

3.3.3 Survey Completion Time

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

We quantify effort using survey completion time, a common metric in survey research used to

[17] The most recent PCE inflation preceding our experiment was 2.5% which was July inflation released August 14th.

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.

approximate the cognitive resources participants allocate 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. By comparing completion times across different incentive treatments, we can assess whether marginal incentives motivate participants to devote more time – and presumably more cognitive effort – to the survey tasks.

We estimate a series of ordinary least squares (OLS) regressions with data winsorized at the 5th and 95th percentiles to analyze the impact of marginal incentives on completion time. The regression equation is specified as

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

where $CompletionTime_i$ is the total time (in seconds) participant i took to complete the survey. $Treatment_{i,j}$ are dummy variables indicating the incentivized treatment group $j \in \{Post, Both, Prior\}$ to which participant i was assigned, with the Flat treatment serving as the reference group. $\mathbf{X}_i$ is a vector of control variables, including participant age, race, gender, education, income level, political affiliation, primary grocery shopper status, economic sentiment, and state of residence (included in column (2)). We report regression results in Table 7.[18]

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: The 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.

Coefficients for *Both* and *Prior* are positive and statistically significant across all specifications, indicating that participants in these groups took significantly longer to complete the survey compared to those in the *Flat* treatment. Specifically, participants in the *Both* treatment spent approximately 110 to 111 seconds more on the survey than those in the *Flat* group – a substantial increase given the survey's average completion time. Those in the *Prior* treatment took about 58 to 62 seconds longer than participants in the *Flat* treatment. Although the coefficient for the *Post* treatment is positive, it is only marginally significant when we control for demographic characteristics, suggesting that marginal incentives applied only after the information provision do not significantly affect overall completion time.

These results support the hypothesis that marginal incentives enhance participant effort during belief elicitation, particularly when the incentives are applied at the initial stages of the survey, as in the *Prior* and *Both* treatments. The increased completion times indicate that participants are investing more effort into responses, leading to more thoughtful belief formation.

[18] We show the same results without winsorizing in Table A-10, located in appendix A1.

3.3.4 Rounding Behavior

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

We define a forecast as rounded if the reported value is a multiple of 1, 5, or 10 percentage points (pp).[19] This captures meaningful reductions in numerical precision and serves as our main behavioral measure of satisficing. We interpret a lower likelihood of rounding as evidence that participants are exerting greater cognitive effort in forming and reporting their expectations. Based on this definition, we construct two measures: (1) a binary indicator for whether a participant rounded their point forecast, and (2) a categorical variable capturing the degree of rounding. Using these, we show that marginal incentives significantly reduce rounding behavior – consistent with the interpretation that participants exert greater effort when precision is rewarded.

Supporting this interpretation, Figure 5 shows that 91.4% of unincentivized participants rounded their forecasts, compared to only 77.2% of those in incentivized treatments, which yields a 14.2 percentage point difference ($p < 0.01$). Probit regressions confirm that incentives tied to prior beliefs significantly reduce the likelihood of rounding (see Table 8). Further, Figure 5 illustrates that incentivized participants not only round less often but also round less coarsely. This pattern reinforces our interpretation that marginal incentives increase participant effort – not just in time spent, but also in the cognitive precision applied when forming and reporting expectations.

These findings contribute to a growing literature on the determinants of rounding behavior in inflation expectations. For example, Binder (2017) shows that rounding in survey-based belief measures 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.

[19] 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.

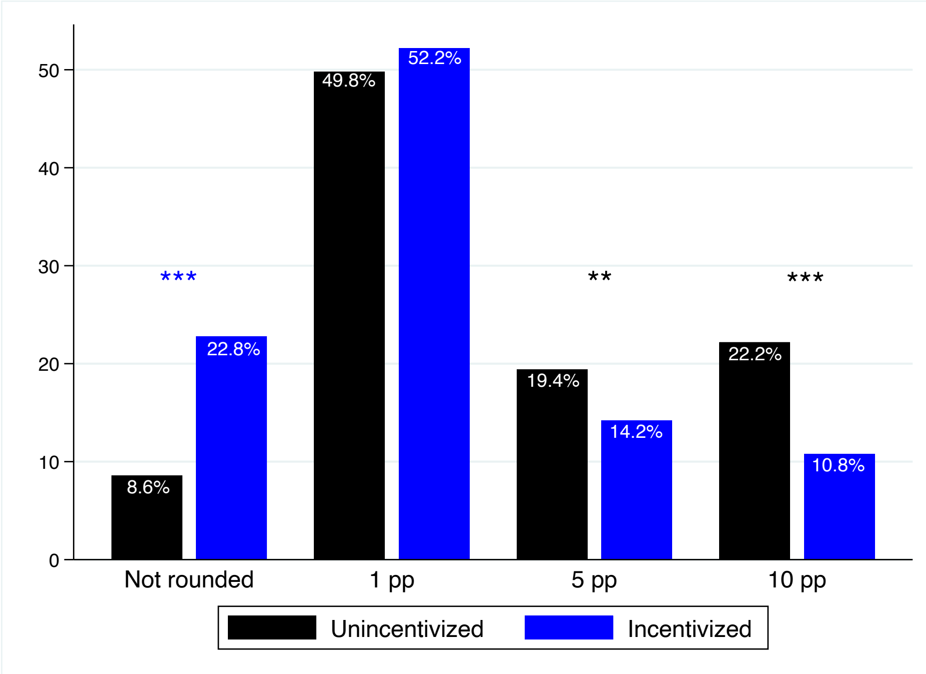


Figure 5: Percentage of Forecasts By Rounding Behavior

Notes: Stars indicate significance levels from the test of the equality of proportions as follows: $^{*}\,p < 0.10$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

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, we provide each participant with a summary of the Fed's outlook on how inflation might evolve in 2025. We then collect data from a binned inflation forecast to estimate each subject's updated inflation expectation ("posterior").[20] We use bin elicitation strategy similar to that employed by the New York Fed in the SCE.[21] Figure 6 shows the average weight assigned to each of the ten possible inflation bins for participants who faced marginal incentives (blue solid line) and those who did not (black solid line). Interestingly, imposing marginal incentives shifts significantly more weight toward the bin containing both the long-run inflation target and the

[20] We depict these expectations in Figure A-2 and examine whether marginal incentives affect the distribution of these expectations across treatments in Table A-9. For brevity, both are reported in appendix A1. Similar to the findings of D'Acunto et al. (2023) for the SCE data, we observe that binned inflation forecasts exhibit lower disagreement and a lower mean expected inflation than point forecasts.

[21] 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.

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 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.

signal. We estimate the mean of the density forecast ("posterior") using the center of each bin and ±12% for the open-ended bins, and treat it as our measure of interest throughout this section.

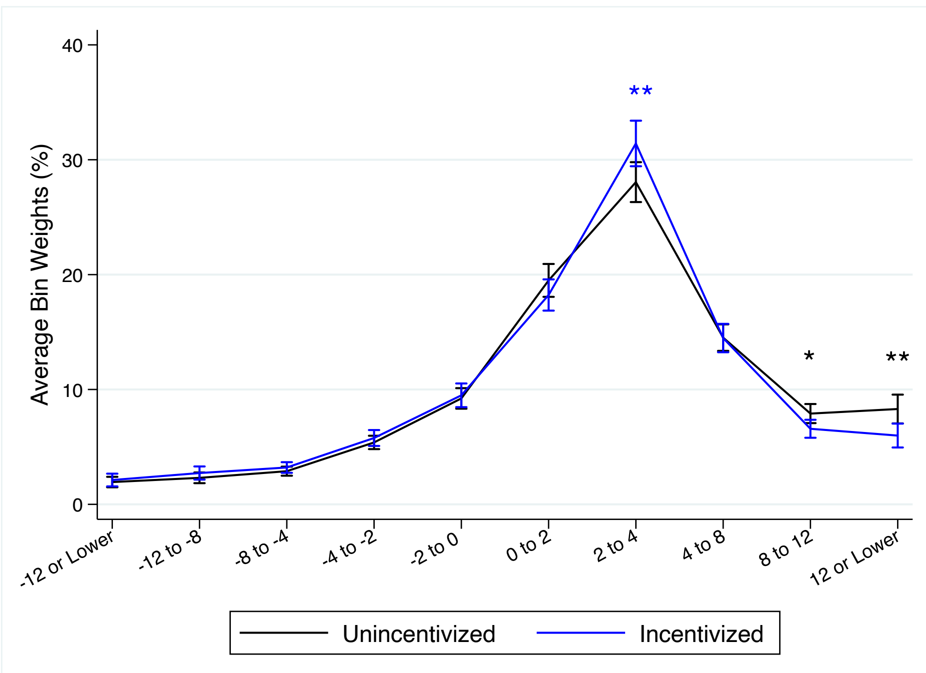


Figure 6: Average Bin Weights Across Incentives

Notes: The 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 on forecast updating in response to the information treatment, we follow Haaland et al. (2023) and estimate *learning rates*, defined as 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, $Treatment_{i,j}$ is an indicator variable denoting which incentive structure $j \in \{Post, Both, Prior\}$ a respondent faced, and $PercGap_i$ (Perception Gap) is the distance between the Fed's forecast of median PCE inflation in 2025 and respondent i's prior. Thus, β_1 captures the extent to which incentive structure drives belief updating relative to our baseline (unincentivized) treatment *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.[22]

Table 9 reports the results from estimating Equation (2) using Huber regressions to control for influential observations. β_1 is our primary coefficient of interest. 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 find that incentivizing the posterior forecast (*Post*) leads to noticeably greater updating toward the signal compared to the *Flat*. This effect is both statistically significant and robust to the inclusion of demographic controls. Similarly, incentivizing both the prior and posterior forecasts (*Both*) also increases responsiveness to the signal. Given that participants still learned from the provided information, despite their incentivized prior belief, further indicates that our findings on the priors of inflation expectations cannot fully be explained by acquiring information. However, this effect is only marginally significant. The results for the *Prior* treatment group are less clear. We observe a negative coefficient, suggesting reduced updating relative to the *Flat*. However, this effect becomes statistically insignificant once demographic controls are included.

Our analysis demonstrates that implementing marginal incentives significantly enhances belief updating among subjects. Specifically, the positive and statistically significant coefficient for the interaction term ($\beta_1^{(Post)}$, $\beta_1^{(Both)}$) indicates that incentives designed to promote forecast accuracy significantly amplify the magnitude of belief updating in response to dis-

[22] Haaland et al. (2023) argue that if priors are balanced across treatment, the researcher could use the posterior as the dependent variable. We cannot do that here, since treatment variation can induce systematic differences in the prior. Nevertheless, we explore this sort of specification, used for example in Coibion et al. (2022), in appendix A6 and obtain results qualitatively identical to those discussed here in our main specification. An additional benefit of this specification is that we do not need to make any assumptions about the signal.

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: The 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.

crepancies between their prior beliefs and the Federal Reserve's forecasts. This result is particularly noteworthy given that our survey was conducted at a time when the recent surge in inflation was still fresh in respondents' memory, presumably making inflation more salient (Weber et al. 2025; Bracha and Tang 2024), and when the uncertainty of inflation was still relatively high.[23] This result has direct implications for the ability of central bank forecasts to coordinate and guide inflation expectations.

It is important to note, however, that our marginal incentives treatments raise the benefit of getting future inflation right, while keeping the cost of information constant (i.e., information is readily and freely provided, but processing costs remain), which impacts rational inattention to information provision (e.g., see Maćkowiak and Wiederholt 2024 or Maćkowiak et al.

[23] We present a decomposition exercise that quantifies the role of mechanical updating – apparent forecast revisions that arise purely from the change in elicitation format (from point to binned forecasts) in appendix A5. While illustrative, this exercise suggests that, after accounting for this artifact, marginal incentives increase signal-driven updating by more than threefold.

2023). Unincentivized RCTs might underestimate learning rates if participants disregard provided information due to incorrectly assessing their processing costs and their benefits from the provided information. By contrast, incentivizing RCTs might pick up learning effects from participants who misperceive the benefit of accurate inflation expectations without marginal incentives. Therefore, compared to unincentivized RCTs, the learning rates we find can be viewed as the upper bound to the potential impact of information on beliefs.

Similar to laboratory experiments, incentives can significantly influence the outcomes of survey-based information provision experiments. 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 (Weber et al. 2025, Wabitsch 2024) dynamically shift household attention to inflation, making the effects of information treatments potentially time- and sample-dependent. This suggests that the survey-based experiments might potentially benefit from additional control in form of marginal incentives. Moreover, by reducing extreme values and response variability, incentives can lower the need for extensive data cleaning and reduce the required sample size, making experiments more cost-effective without undermining the validity of prior approaches.

4 Model-based Implications

Our findings have implications for how incentives shape the conclusions that can be drawn from integrating survey data with economic models.

To illustrate this point, we focus on 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 model. In our setup, agents form expectations as a weighted combination of backward-looking and model-consistent components. We simulate the model under two regimes of expectations formation: (i) a heuristic rule calibrated to expectations under marginal incentives, and (ii) a heuristic rule calibrated to expectations without incentives.

We consider a standard three-equation New Keynesian model consisting of a New Keynesian 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:

$$u_t = \rho_u u_{t-1} + \varepsilon_t^u, \quad \varepsilon_t^u \sim \mathcal{N}(0, \sigma_u^2) \tag{6}$$
$$v_t = \rho_v v_{t-1} + \varepsilon_t^v, \quad \varepsilon_t^v \sim \mathcal{N}(0, \sigma_v^2). \tag{7}$$

Households form expectations via a convex combination of a model-consistent forecast and a backward-looking heuristic:

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

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.

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:

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

The resulting treatment-level calibrations are provided in Table 10.

Table 10: Empirically Implied Calibration of η

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

Notes: The 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.

For our simulations, we adopt two values of η: a heuristic under incentives with $\eta = 0.75$

(from the incentivized *Prior* and *Both* treatments) and a heuristic without incentives with $\eta = 0.60$ (from the *Post* and *Flat* treatments). These calibrations enable us to compare how differences in the formation of expectations, grounded in observed behavior, affect the transmission and persistence of shocks in a stylized macroeconomic environment. All model parameters are shown in Table 11.

Table 11: Model Parameter Values

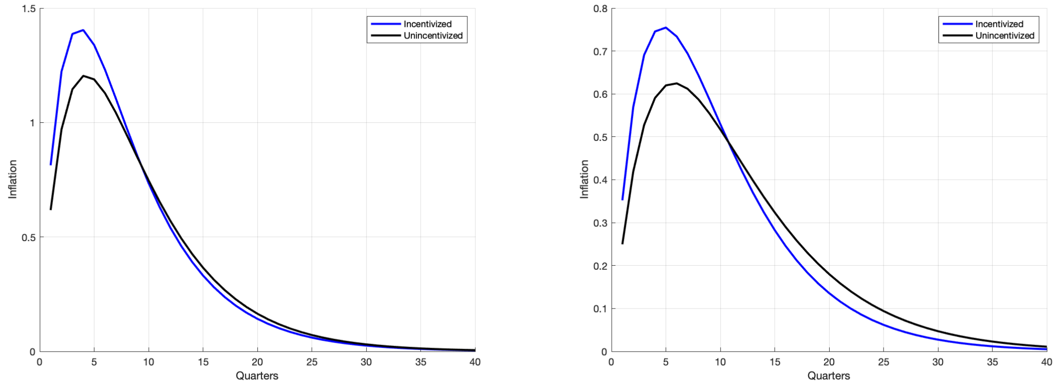
Parameter	Description	Value
β	Discount factor	0.99
σ	Intertemporal elasticity of substitution	1
ϕ_π	Taylor rule response to inflation	1.5
ϕ_y	Taylor rule response to output	0 or 0.5
κ	Slope of the Phillips Curve	0.104
ρ_u	Persistence of demand (IS) shock	0.841
ρ_v	Persistence of cost-push shock	0.841
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: The 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).

Figure 7 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. The figure shows that even modest increases in backward-lookingness substantially alter the dynamics of inflation. Notably, the unincentivized treatment – which exhibits the greatest reliance on lagged inflation – produces the more persistent inflation path, with elevated inflation lasting longer than in the incentivized case.

These findings highlight two critical methodological and policy-relevant implications that arise from how we measure expectations. First, empirical analyses seeking to understand the sources of inflation persistence must account carefully for the method used to elicit expectations. Our results suggest that researchers using unincentivized survey data may attribute excessive persistence in inflation to sluggish, backward-looking expectations rather than structural factors such as price rigidity. Consequently, reliance on unincentivized expectations data risks attenuating the perceived role of underlying structural mechanisms in macroeconomic models, potentially skewing our understanding of inflation dynamics.

Second, there are substantial policy implications arising from this mismeasurement of expectations. Policymakers may calibrate macroeconomic models using survey-based inflation



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

Figure 7: Inflation Dynamics under different expectation regimes

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

expectations to guide monetary policy decisions. If unincentivized survey data overstate the backward-lookingness of expectations, policymakers might mistakenly infer greater inertia in expectation formation than truly exists. This misconception could lead them to pursue overly aggressive or unnecessarily prolonged policy interventions, based on the belief that inflation will only respond slowly to shocks and therefore requires sustained pressure. However, as our analysis demonstrates, agents may be more forward-looking in reality, which they reveal when facing tangible incentives. Thus, accurate measurement of expectations is not merely academically relevant but is crucial for calibrating monetary policy.

5 Discussion

While our study underscores the benefits of incorporating marginal incentives, some cautionary notes are warranted. Two critical questions arise: What exactly are we measuring when incentives are used, and do they allow us to accurately capture the genuine beliefs we aim to elicit? These questions are particularly challenging, as it is impossible to directly observe: i) respondents' true expectations, or ii) the expectations they actually use to make decisions. However, we provide evidence shedding light on these questions with a follow-up wave of the experiment that included additional questions on spending and searching for information online, while not including an information provision intervention, making the density forecast more comparable to the SCE. Details on the collected data are found in appendix A4.

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.[24] Table 12 shows that incentivized inflation expectations correlate positively and significantly with spending plans, while unincentivized inflation expectations do not.

Table 12: Correlation Between Inflation Expectations and Nominal Spending

	(1)	(2)
	Unincentivized	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: The table presents the relationship between expected inflation and expected spending. 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 common concern about data from inflation expectations surveys is that expectations reported as point forecasts are inconsistent with those reported as bin forecasts. Since our follow-up wave did not provide any information between the point and the density forecast, we can assess the consistency between the two forecasts. In Table 13, 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.[25] This further suggests stronger reliability of incentivized inflation expectations.

[24]Spending was elicited, asking 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)".

[25]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)

Table 13: Absolute Distance Between Point Forecast and Mean/Median of Density Forecast

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

Notes: This table shows consistency between respondents' point forecasts and density forecasts, where there was no information provision intervention between the two forecasts. Point forecast data 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, and incentivizing expectations in three years might be administratively impractical. What we show is that incentivizing short-term inflation expectations (i.e., for one-year-ahead inflation) already generates positive spillover effects to longer-term expectations. Similar to short-run expectations, the distribution of longer-term expectations lowers and becomes less dispersed, suggesting more anchored inflation expectations and less disagreement (see Table 14).

Table 14: Summary Statistics and Variance Comparison of Long-Run Inflation Expectations

	Mean	**Standard Deviation**	**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.

Genuine beliefs vs. searching for information Another concern is that incentive schemes may induce behavior that does not reflect genuine beliefs but rather strategic reporting. For instance, are respondents simply searching for information online? 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. For example, 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.

However, we find little evidence in support of this channel. We ask respondents after the experiment, whether they looked up external information.[26] Regardless of incentives, about 8%-15% of respondents report having searched for information about inflation rates online. While incentives indeed slightly increase the acquisition of external information (see Figure 8), the difference between incentivized and unincentivized respondents is negligibly small, especially considering how incentives impact the entire distribution of inflation expectations. In other words, searching for information online cannot fully explain our results. 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. In addition, if we want 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.

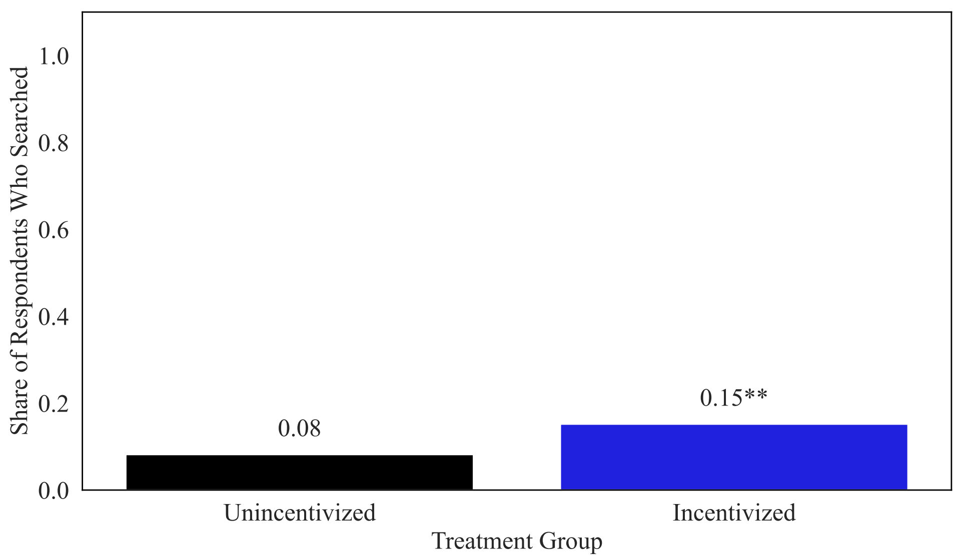


Figure 8: Information Search by Treatment Group

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 indicates the statistical significance of the difference (z-test). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Thus, several indicators suggest that beliefs elicited with incentives are more informative than those elicited without incentives. While further research is desirable to better under-

[26] We assured them that their answer would not impact their payoff in any way.

stand belief elicitation generally, an interpretation of our findings is that the incentivized expectations more accurately reflect households beliefs in the field for whom inflation expectations matter – e.g., those who are making important consumption decisions, and thus are more likely to be informed and respond to new information.

Finally, could marginal incentives confuse respondents? Danz et al. (2022) provide evidence that more complex incentive schemes, while theoretically incentive-compatible, can lead to misunderstandings, potentially resulting in less truthful reporting. 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. 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. These findings are consistent with 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 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."*

6 Conclusion

Our experiment demonstrates that marginal incentives significantly impact the elicitation of macroeconomic beliefs and learning rates in information provision experiments in the context of inflation expectations' elicitation. Specifically, imposing incentives leads to household inflation expectations that are more consistent with expectations of professional forecasters, with respondents predicting lower and less extreme values, and exhibiting less cross-sectional forecast disagreement. The reduction in cross-sectional disagreement and convergence towards expert predictions suggests that incentivized surveys may provide policymakers with more reliable measurements to understand household expectations, a crucial input for those focused on managing inflation dynamics and developing economic forecasting or quantitative models. In particular, this change in incentive structure does not need to increase the cost of collecting expectations. Policymakers and researchers can leverage our approach to quantify or estimate the extent of measurement error due to the absence of incentives by incorporating incentives into subsamples of respondents.

Our results support recent rational inattention models (e.g., Maćkowiak and Wiederholt 2024), showing that even modest incentives can endogenously shift attention and increase learning from information, thereby altering how belief formation processes are measured in survey-based macroeconomic research. For instance, the efficacy of information provision may appear muted in unincentivized settings – especially when attention to inflation and monetary policy is already high – leading to understated treatment effects in RCTs, as has recently been the case. Consistent with this, we find that incentivized respondents exhibit higher learning rates, suggesting that insufficient experimental control over incentives can produce qualitatively different conclusions, including false negatives where information treatments appear ineffective. Crucially, the incentive structure itself alters the underlying distribution of beliefs, which may change how we interpret the responsiveness of households to central bank communication. Our findings thus imply that central banks can enhance the effectiveness of their communication strategies by lowering the cognitive costs of paying attention – e.g., via simplified messaging – or by increasing the perceived benefits of holding accurate inflation 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.

Notably, the same incentive structures that lead to higher learning rates also close the gender gap in inflation beliefs in point forecasts of one-year-ahead inflation, offering a complementary explanation to a long-standing puzzle in the belief-based macroeconomic literature. This suggests that marginal incentives can mitigate some systematic biases observed in survey-based beliefs elicited via flat-fee incentives and serve as a diagnostic tool to discern which biases are more likely to be robust.

In conclusion, our study suggests that incorporating marginal incentives into surveys positively enhances elicited beliefs, which could improve the robustness of empirical findings in macroeconomic research and quantitative evaluations of macroeconomic models. By motivating participants to engage more deeply when forming beliefs, incentivized mechanisms can lead to more reliable data.

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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	Posterior	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 the 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).

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

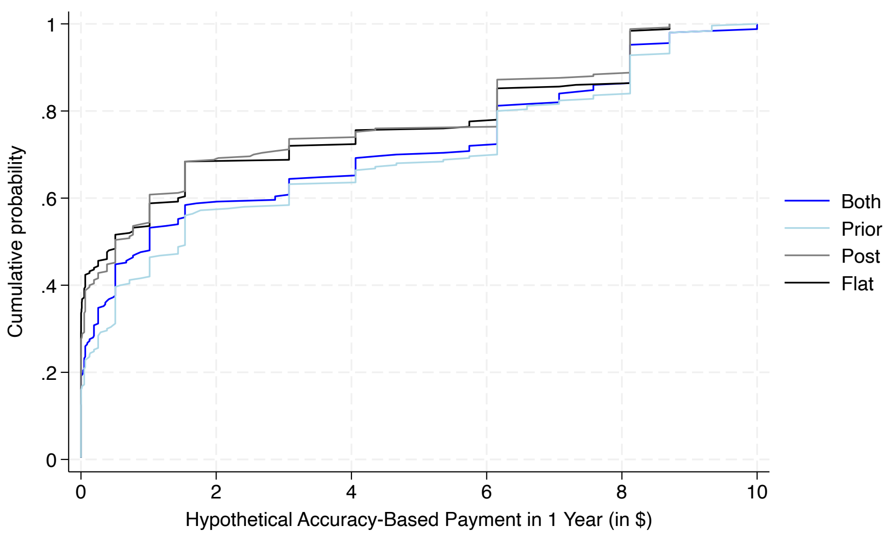
	Median	**IQR**	**N**
Unincentivized	2.7	8.3	500
Incentivized	2.0	6.0	500
All Data	2.0	7.0	1,000

Test for Equality of Medians and IQRs

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

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

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



Notes: The figure shows how treatments impact the hypothetical payoffs of participants calculated comparing point forecasts formed before information provision the Fed's 2025 inflation forecast. This shows – assuming the Fed'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)	(2)	(3)	(4)
	$\lvert\mathbb{E}(\pi\_prior)\rvert$	$\lvert\mathbb{E}(\pi\_prior)\rvert$	EV	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: 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. The table shows the effect of treatments on reported inflation expectations (the priors), relative to the *Flat* treatment. 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).

Table A-6: Hypothetical Earnings From Point Forecasts of Inflation (The Priors)

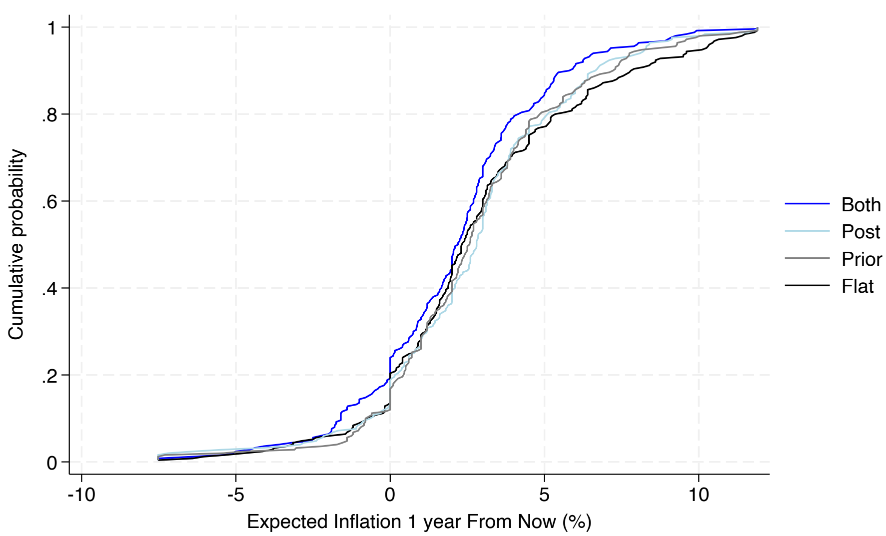
	(1)	(2)
	prior_hypothetical_payoff	prior_hypothetical_payoff
Post	0.0138	-0.251
	(0.129)	(0.296)
Both	0.228*	0.514*
	(0.129)	(0.295)
Prior	0.394***	0.880***
	(0.129)	(0.295)
Constant	0.610****	-2.043
	(0.0909)	(1.470)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$, **** $p < .001$

Notes: The table shows the effect of treatments on hypothetical earnings, as proxied by distance between reported priors and the Fed'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: The 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: The 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**	**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	-0.0000332
	(0.267)	(0.258)
Constant	2.603***	7.431***
	(0.189)	(1.287)
Controls	No	Yes
N	1000	1000

Standard errors in parentheses

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

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

Table A-10: Effects of Incentives on Time

	(1) Seconds	(2) 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

* $p < .1$, ** $p < .05$, *** $p < .01$, **** $p < .001$

Notes: The 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:

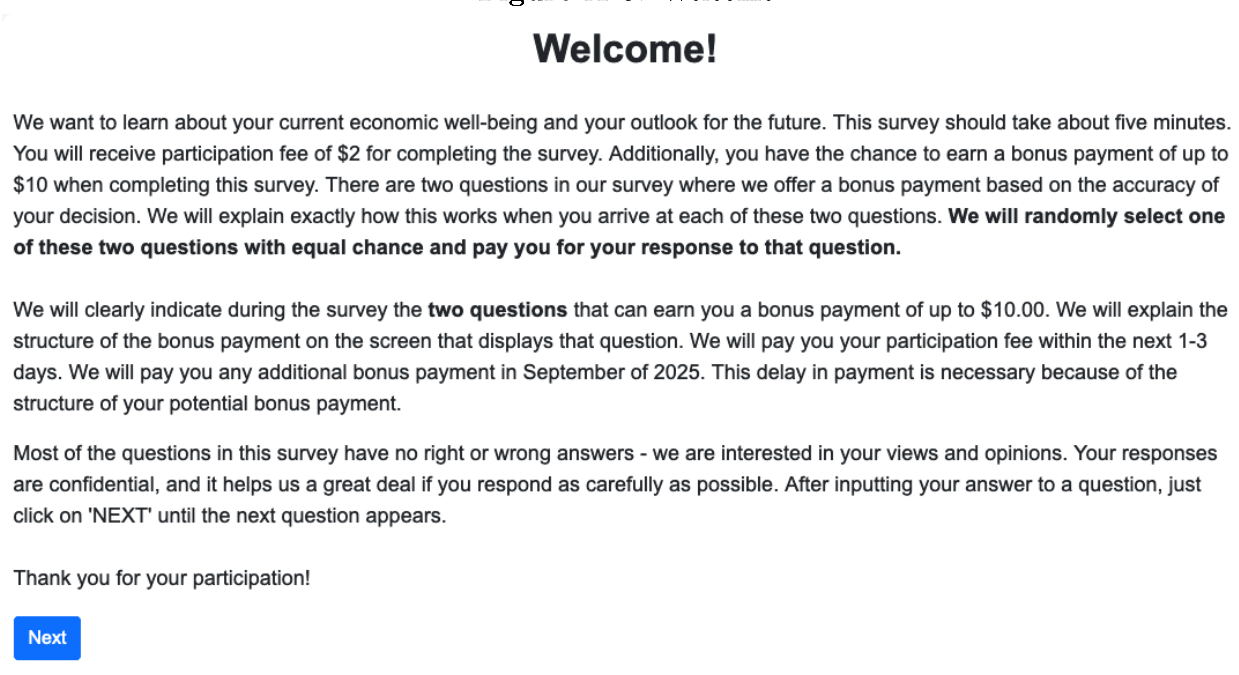
- **Small effect size:** $d = 0.2$
- **Medium effect size:** $d = 0.5$
- **Large effect size:** $d = 0.8$

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.

Figure A-3: Welcome


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

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

Figure A-4: General Questions

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-5: Explanations

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-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.

Over the past twelve months...

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

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

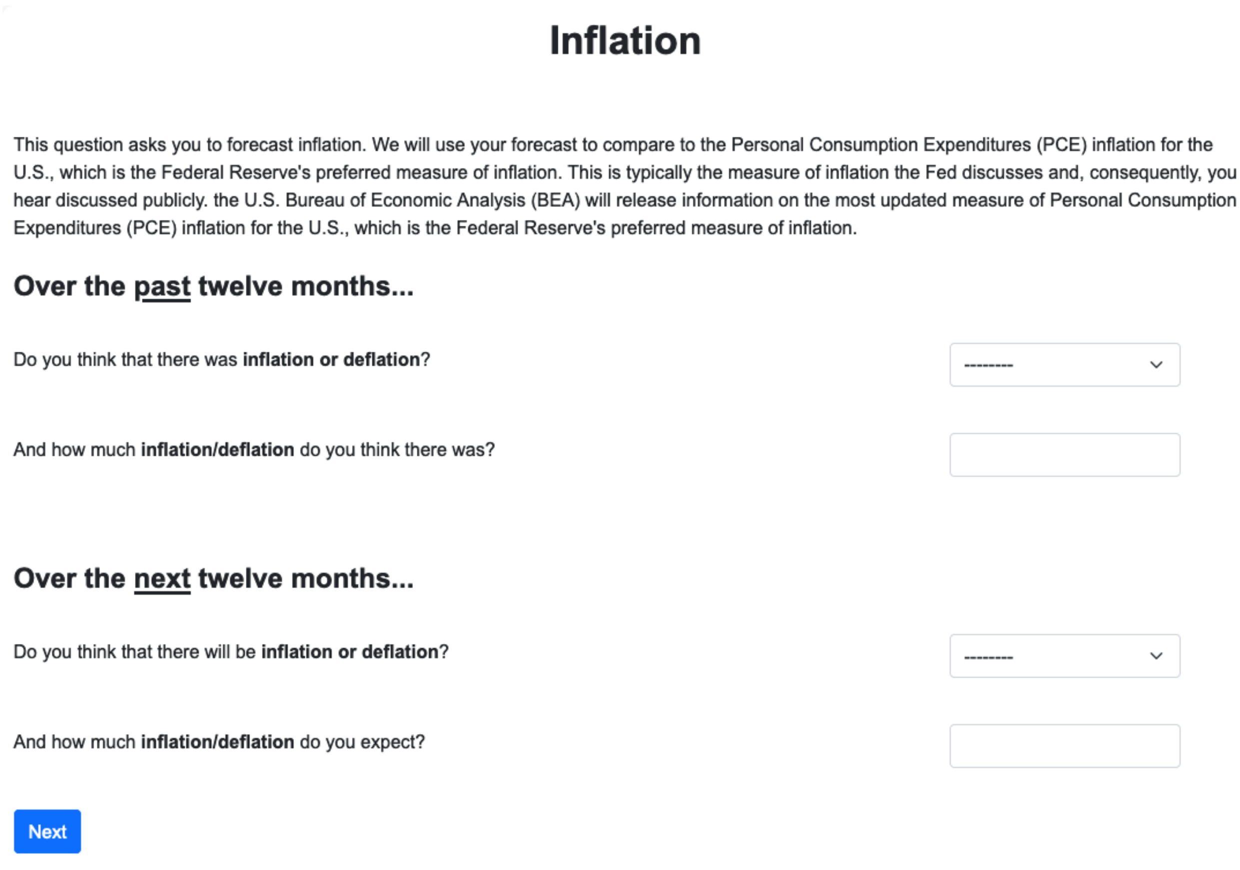
Over the next twelve months...

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

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-8: Inflation Point Forecast (Prior)

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-9: Inflation Point Forecast (Both)

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?

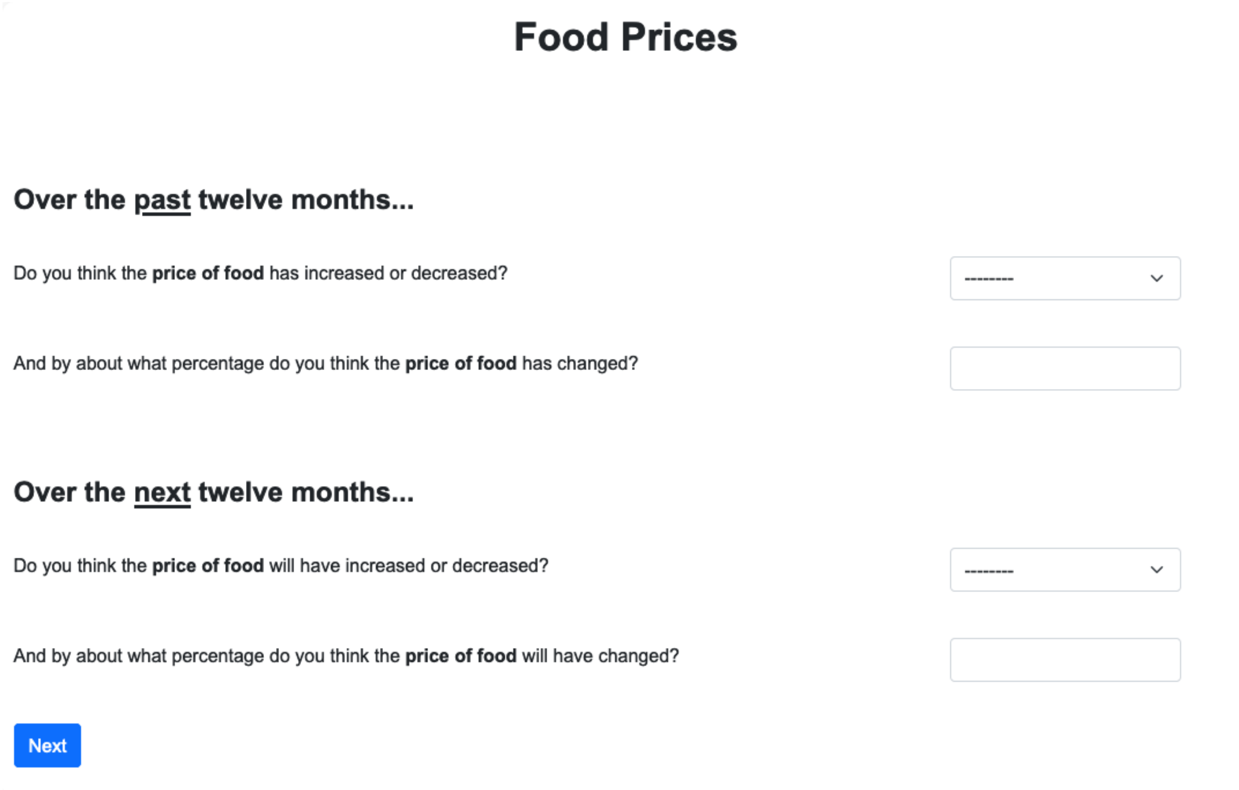
Over the next twelve months...

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

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

Next

Figure A-10: Food Point Forecast



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?

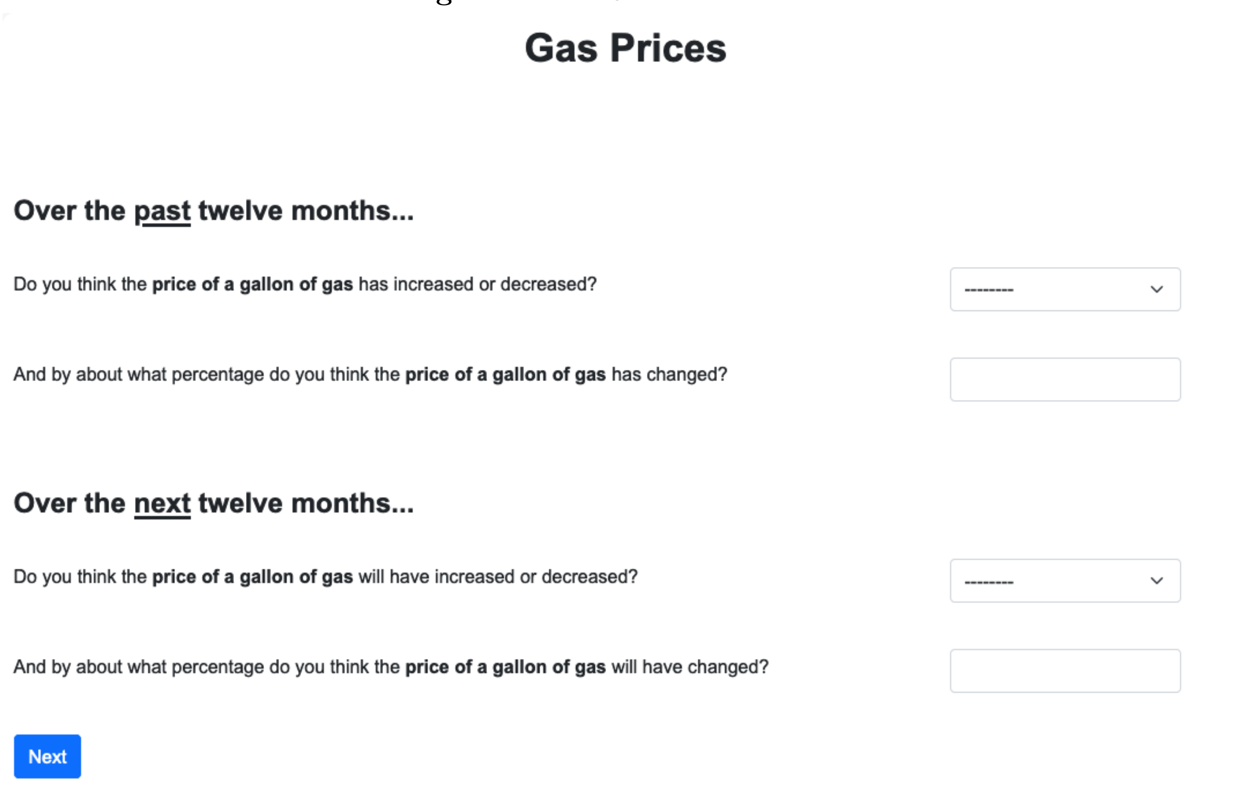
Over the next twelve months...

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-11: Gas 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-12: Information Intervention

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-13: Food Bin Forecast

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-14: Gas Bin 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-15: Inflation Bin 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%.

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 Bin 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 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-17: Inflation Bin Forecast (Both)

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-18: End of Survey

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

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 Follow-up Wave

We conducted the follow-up survey in March 2025 using the Prolific platform. To ensure that the new sample was not influenced by the information treatments or incentive structures used in the September 2024 wave, we recruited entirely new respondents from a nationally representative adult population. Participants from the original study were not eligible to take part in the follow-up survey.

A total of 1,024 responses were collected. Respondents were randomly assigned to one of four groups: *Control*, *Both*, *Both + Formula*, and *Median*. These groups varied in the incentive schemes used to elicit one-year-ahead point forecasts and bin forecasts. All other questions were not incentivized, and respondents were explicitly informed of this distinction.

In this paper, we focus exclusively on the *Control* (unincentivized) and *Both* (incentivized) groups, whose incentive schemes correspond exactly to the Flat and Both, respectively, in the September 2024 wave. Each group comprises 257 respondents.

Importantly, the March 2025 wave did not include an information provision experiment. The objective of this wave was to replicate the Survey of Consumer Expectations (SCE); 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. In particular, the sequence used to elicit short- and long-term inflation expectations exactly mirrors that of the SCE.

Figure A-19 and Figure A-20 display the welcome screens. Instructions for the point forecast and bin forecast tasks in the Both group are shown in Figure A-21 and Figure A-22. The one-year-ahead point forecast and bin forecast questions appear in Figure A-23 and Figure A-24, while the three-year-ahead counterparts — neither of which were incentivized — are presented in Figure A-25 and Figure A-26. The spending question is shown in Figure A-27.

In addition to the core SCE questions, we also asked respondents whether they consulted any external sources when forming their forecasts (Figure A-28). Respondents were assured that their answer to this question would have no bearing on their payment.

Figure A-19: Welcome Screen (Control Group)

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 ${e://Field/NextMonthNextYear}.

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 Screen (Both Group)

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 ${e://Field/NextMonthNextYear} 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-21: Both Group Incentives Information (Point Forecast)

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 ${e://Field/NextMonthNextYear}, 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-22: Both Group Incentives Information (Bin Forecast)

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 ${e://Field/NextMonthNextYear}, 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-23: One-Year-Ahead Point Forecast Question

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-24: One-Year-Ahead Bin Forecast Question

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-25: Three-Year-Ahead Point Forecast Question

Now we would like you to think about inflation further into the future. **Over the 12-month period between ${e://Field/TwoYears} and ${e://Field/ThreeYears},** 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 ${e://Field/TwoYears} and ${e://Field/ThreeYears}, I expect the rate of inflation/deflation to be

Figure A-26: Three-Year-Ahead Bin Forecast Question

And in your view, what would you say is the percent chance that, **over the 12-month period between ${e://Field/TwoYears} and ${e://Field/ThreeYears} ...**

(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-27: Spending Question

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

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

Figure A-28: Search Question

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

A5 Quantifying the Impact of Incentives on Learning

In our main results, we estimate how marginal incentives influence signal uptake in a simple information provision experiment embedded in our survey (Section 3.4). However, our experimental design may introduce a *mechanical artifact*: the act of shifting the elicitation from point to density forecasts can itself create apparent "updating," even in the absence of a central bank signal.

To contextualize our experimental estimates, we conduct a decomposition exercise to quantify three distinct components of observed updating:

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

2. **Signal-driven updating:** True updating in response to the signal, net of any mechanical artifact.

3. **Incentive-driven updating:** Additional updating induced by marginal incentives, measured relative to true signal-driven updating.

Step 1 (Mechanical baseline): We first measure 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 we provide no central bank signal. The estimated slope,

$$\text{Updating}_i = \alpha + \beta_{\text{placebo}} \cdot \textit{percgap}_i + \varepsilon_i,$$

captures purely mechanical updating, where $\textit{percgap}_i$ is defined as the (counterfactual) signal we would have displayed.[1]

Step 2 (Signal-driven updating): Next, we estimate the slope in the signal-only treatment without incentives (T1). The incremental slope relative to the placebo captures true signal-driven updating net of mechanical artifacts:

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

[1] We take this value (2.7%) from the Federal Reserve, using the exact time approporiate analog of what we provided subjects in our main survey.

Step 3 (Incentive-driven updating): Finally, we consider the incentivized signal treatment (T2). We first compute an *ncentive share*:

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

which reflects 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 measures how much marginal incentives amplify true signal updating after accounting for mechanical effects.[2]

Table A-12 summarizes these results. The placebo slope ($\beta_{\text{placebo}} = 0.910$) is large, consistent with a mechanical artifact of shifting to density forecasts.

Engelberg et al. (2009) shows using SPF data that about 20% of professional forecasters exhibit similar mechanical shifts in their forecasts when providing both point and distributional forecasts of inflation. In the context of signal uptake and belief formation, Coibion et al. (2022) find similar results when shifting between distributional and point forecasts of inflation. In their study, where subjects transition from distribution to point predictions of inflation, they estimate that a pure control group recieving no signal update their inflation expectation

Netting out this artifact yields a true 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:

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

These calculations demonstrate that accounting for the mechanical artifact of point-to-density elicitation is crucial: the mechanical component is substantial relative to the true signal effect. After netting it out, including marginal incentives more than triples 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:

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

Table A-12: 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. "Net Effect" is relative to the placebo coefficient. The "Incentive Share" and "Incentive-Induced Increase (net)" quantify how much marginal incentives amplify true signal-driven updating, net of mechanical effects.

incentives meaningfully amplify net signal responsiveness.

A6 Alternative Specification for Signal Uptake Results

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

$$E_i^{\text{post}} = a + b \cdot \text{Treat}_i + \psi \cdot E_i^{\text{pre}} + \gamma \cdot (\text{Treat}_i \times E_i^{\text{pre}}) + \varepsilon_i, \tag{A.10}$$

where E_i^{post} is respondent i's *posterior* point expectation , E_i^{pre} is the *prior* point expectation elicited immediately before treatment, and Treat_i are treatment indicators (with Treat_Reg = 1 as the control: signal, no incentives). We estimate (Equation (A.10)) via Huber regressions and report for each treatment (i) the **Intercept** and (ii) the **Slope**, which measures how strongly posteriors load on priors.

Here is how we interpret these results:

- **Intercept (level shift).** For each treatment, the "Intercept" reported in Table A-13 is the predicted posterior if the prior were zero. For *Flat*, this is simply a in Equation (A.10). For any other treatment, the intercept is the shift relative to *Flat* induced by that treatment's incentive scheme. This corresponds to $a + b$ in Equation (A.10). Because all respondents observe the same signal, the intercept can be viewed as the treatment-specific *anchor* toward which respondents converge as they discount their prior.

- **Slope (weight on prior).** For *Flat*, the slope reported in Table A-13 corresponds to ψ in Equation (A.10), the marginal effect of the prior on the posterior in the base group. For any other treatment, the total slope is $\psi + \gamma$, where γ is the treatment-specific interaction term in Equation (A.10). Note, Table A-13 reports slopes for incentivized treatments relative to *Flat* (i.e., reports γ independently for these treatments). A smaller slope ($\gamma < 0$) means respondents put *less* weight on their prior and move *more* on 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.

- **Effect of incentives on uptake.** Relative to control, 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*.

Table A-13: Incentive Effects on Posterior Expectations: Intercept and Slope Estimates

Treatment (1)	**Intercept** (2)	**Slope** (3)
Flat	2.355	0.0793***
	(0.175)	(0.00642)
Relative to control group		
Post	0.163	−0.0402***
	(0.250)	(0.00906)
Both	−0.374	−0.0187*
	(0.246)	(0.01062)
Prior	0.00332	0.0372***
	(0.248)	(0.01364)
N	999	

Notes: This table reports intercepts and slopes from Equation (A.10) estimated using Huber robust regressions. Stars denote significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.