Incentivizing Inflation Expectations*

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Abstract

Inflation expectations are crucial for economic modeling and policymaking. Despite the well-established role of incentives in experimental economics, all major surveys of inflation expectations pay a flat participation fee. This lack of marginal incentives extends to many information provision experiments—often designed as randomized controlled trials (RCTs). We introduce marginal incentives into a standard survey of inflation expectations. Marginal incentives significantly alter expectation distributions, reducing upward bias, cross-sectional disagreement, and closing the gender expectations gap. Further, in an embedded RCT, marginal incentives lead to greater responsiveness to information provision, contrasting with null effects under flat fees. These findings underscore the importance of marginal incentives for measurement in surveys and survey-based experiments to enhance data validity, strengthen empirical research, and better inform policymaking.

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1 Introduction

Macroeconomics has recently seen two significant trends: the widespread integration of survey-based beliefs into empirical research and policymaking, and the adoption of information provision experiments embedded within economic surveys. The incorporation of survey-based belief data has markedly advanced our understanding of household expectations, provided deep insights into how households form these expectations and tested long-held fundamental assumptions—rational expectations in particular—within macroeconomic theory.[1] Additionally, central banks have increasingly relied on survey-based measures of inflation expectations to inform both conventional and unconventional policy decisions, underscoring the practical importance of accurately eliciting, measuring, and interpreting these beliefs. Concurrently, adopting information provision experiments, particularly randomized controlled trials (RCTs), embedded into large-scale surveys has enabled researchers to establish causal relationships between information dissemination and expectations, economic decision-making, and economic outcomes, and central bank communication and expectations. These experiments typically involve providing participants with specific information or interventions and measuring the subsequent impact on their expectations and behaviors. By leveraging RCTs, economists can isolate the effects of information on expectations, thereby enhancing the robustness of their empirical findings.

Survey-based beliefs and information provision experiments almost always employ unincentivized or flat-fee incentive structures. In contrast, experimental economics has long recognized the value of marginal incentives in ensuring that participants reveal their true preferences and beliefs. The induced value theorem (Smith 1976) posits that marginal incentives align participants' self-interest with truthful reporting, thereby enhancing data quality.[2] Neglecting marginal incentives may inadvertently and unnecessarily introduce measurement errors and biases into macroeconomic belief surveys, undermining the reliability of empirical conclusions drawn from such data.

In this paper, we design an experiment to test how marginal incentives—rooted in the induced value theorem Smith (1976)—affect macroeconomic beliefs elicited via survey and learning rates within a simple information provision experiment. We replicate portions of the NY Fed's Survey of Consumer Expectations (SCE) methodology for eliciting inflation expectations and then add an information intervention following common RCT practices. Our

[1] For a review of this literature, see D'Acunto and Weber (2024) and Weber et al. (2022). For canonical examples, see Coibion and Gorodnichenko (2015), Coibion et al. (2018). For a nice review of information provision experiments, see Haaland et al. (2023).

[2] There is ample evidence demonstrating the advantage of marginal incentives. For example, Nelson and Bessler (1989) and Palfrey and Wang (2009) found incentive-compatible scoring rules outperform alternatives. Gächter and Renner (2010), Wang (2011), and Trautmann and van de Kuilen (2014) showed that incentivized elicitation methods dominate unincentivized ones.

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

Participants in our experiment were sorted into one of four treatment conditions: *Flat*, *Prior*, *Post*, and *Both*. *Flat* serves as our baseline group, where participants received a fixed payment for participation regardless of their responses. This exactly mimics the incentive structure active in all major surveys of expectations and the majority of information provision experiments. In the *Prior* group, participants were offered marginal incentives based on the accuracy of their point forecasts for one-year-ahead inflation before any information was provided. In the *Post* group, marginal incentives were applied after participants received the Federal Open Market Committee's (FOMC) inflation outlook, rewarding accurate probabilistic forecasts. *Both* combined these approaches, offering incentives for both prior and posterior forecasts. We calibrate incentives so that the time-value of expected total earnings is constant across all treatments and aligns with participation remuneration in the NY Fed's SCE. This design allows us to evaluate how different incentive structures influence elicited inflation expectations and the extent to which participants update their beliefs in response to new information.

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. Danz et al. (2022) and Abeler et al. (2023) demonstrate that complex incentive schemes can lead to misunderstandings, potentially resulting in less truthful reporting and shrouding. Additionally, their design relied on a lottery system where only a few participants were selected for payout, while we provide marginal incentives to all participants. 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.

Our findings reveal that imposing marginal incentives significantly alters the distribution of reported inflation expectations. Specifically, respondents subjected to marginal incentives provide significantly less extreme forecasts on average and exhibit a reduced upward bias (means of point expectations fall from 6.1% without marginal incentives to 2.7% with such incentives). Further, they exhibit significantly less cross-sectional forecast disagreement (the standard deviation of point expectations drops by a third from 23.78 to 16.98). These patterns emerge regardless of whether we consider elicited priors or posteriors. Incentives further cause elicited expectations to be more consistent with professional forecasts, and resolve gender differences in expectations. In the context of RCTs, marginal incentives significantly enhance estimated learning rates, indicating that participants adjusted their beliefs more substantially and consistently in response to provided information. This effect was so strong that the estimated learning rates under marginal incentives led to a qualitatively different conclusion (that central bank forecasts can coordinate and manage inflation expectations) than those estimated using a traditional flat-fee scheme. Finally, we find that incentives

directly raise the effort that respondents exert when answering the survey, as reflected in the time they spend completing the survey.

These results have profound implications for empirical macroeconomic research and policymaking. First, they suggest that the current reliance on unincentivized survey methods may lead to biased or inaccurate measures of economic expectations, potentially skewing research findings and policy decisions by adversely affecting measurement. Secondly, incorporating marginal incentives into belief elicitation processes can improve the validity of survey data, leading to more reliable insights into household expectations and behaviors. Lastly, our findings highlight the necessity for macroeconomic surveys and experiments to adopt incentive mechanisms to ensure the integrity and accuracy of the data they collect or at least to take them into account. Indeed, our approach could be incorporated in current surveys to provide estimates of the proportion of the measurement error due to the absence of marginal incentives.

Our research also speaks directly to the rational inattention literature originated with Sims (2003). 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 expectation formation more broadly. 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) or from endogenous factors such as individual stakes and relevance (Gaglianone et al. 2022).

By bridging the gap between experimental economics and macroeconomic survey methodologies, our study underscores the critical importance of incentive structures in the collection of survey-based macroeconomic beliefs and in information provision experiments. Adopting marginal incentives not only enhances data quality but also strengthens the empirical foundations upon which economic theories and policies are built. As survey-based beliefs continue to play a pivotal role in shaping economic models and policy frameworks, ensuring their accuracy through appropriate incentive mechanisms becomes indispensable for advancing both academic research and policymaking.

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 beliefs collected through a survey-based RCT, a widely adopted methodology in experimental macroeconomics. To achieve these aims, our experiment must generate reliable survey-based beliefs free from the influence of extraneous information provision while simultaneously conducting

an information provision experiment.[3]

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. Specifically, we elicited priors as point expectations (see Figure A-8) and posteriors as probabilistic forecasts (see Figure A-16). 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-11). 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, response options, and overall survey structure on the carefully designed New York Fed's Survey of Consumer Expectations (Armantier et al. 2024, 2017, Bruine de Bruin et al. 2010).[4] Worth noting is that we adopted the welcoming language of the SCE intended to activate participants' intrinsic motivation (see Figure A-2).

We implemented a between-subjects design 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.

The three additional treatments introduce marginal incentives based on the accuracy of participants' one-year-ahead inflation forecasts. In *Prior*, participants receive a bonus payment contingent on the forecast error relative to the realized annual Personal Consumption Expenditures (PCE) inflation reported by the Bureau of Economic Analysis (BEA) in September 2025. A perfect forecast earns a bonus of \$10. Each additional percentage-point (pp) forecast error reduces the bonus by half.[5] This scoring rule is common in learning-to-forecast

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

[4]The SCE measures U.S. consumers' 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.

[5]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

experiments in experimental macroeconomics.[6] 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.[7] For *Both*, a subject faced either the point or probabilistic marginal incentive scheme with equal likelihood.

To calibrate incentives, we analyzed average forecast errors using NY Fed's one-year-ahead forecasts and actual inflation data from FRED. The average forecast error was 1.68 pp across the entire 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 NY 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.

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.

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 appeared to enhance effort (see Section 3.4) and align forecasts more closely with professional expectations, consistent with thoughtful engagement rather than distortion.

[6] See McMahon and Rholes (2023) and Rholes and Petersen (2021) for examples.

[7] 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., Danz et al. (2022)). In a companion paper, we explore the role of incentive-compatibility in designing incentives in the context of the SCE.

2.1 Hypotheses

Before moving on to results, we offer two hypotheses regarding the impact of marginal incentives in our experiment. These hypotheses are grounded in the notion that 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 longstanding practice of employing marginal incentives to discipline choice data and reduce measurement error in experimental economics (Smith 1976, Smith and Walker 1993, Camerer and Hogarth 1999). In our context, we expect incentives to reduce upward bias, forecast errors, and the occurrence of outliers.[8] Further, they will increase individuals' attention to the provided information, even at a time where attention to inflation was high and thus learning rates from information are expected to be lower (Weber et al. 2025).

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. That is, participants adjust their beliefs more substantially and consistently in response to the information provision compared to those without incentives.*

2.2 Data

We collect 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 (see Table A-1 in appendix A1 for a comparison of demographic characteristics across treatment groups and with the SCE sample).[9] 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.

[8] 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.

[9] Coincidentally, there is a relatively higher proportion of females in treatments Prior and Both. Prior 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.

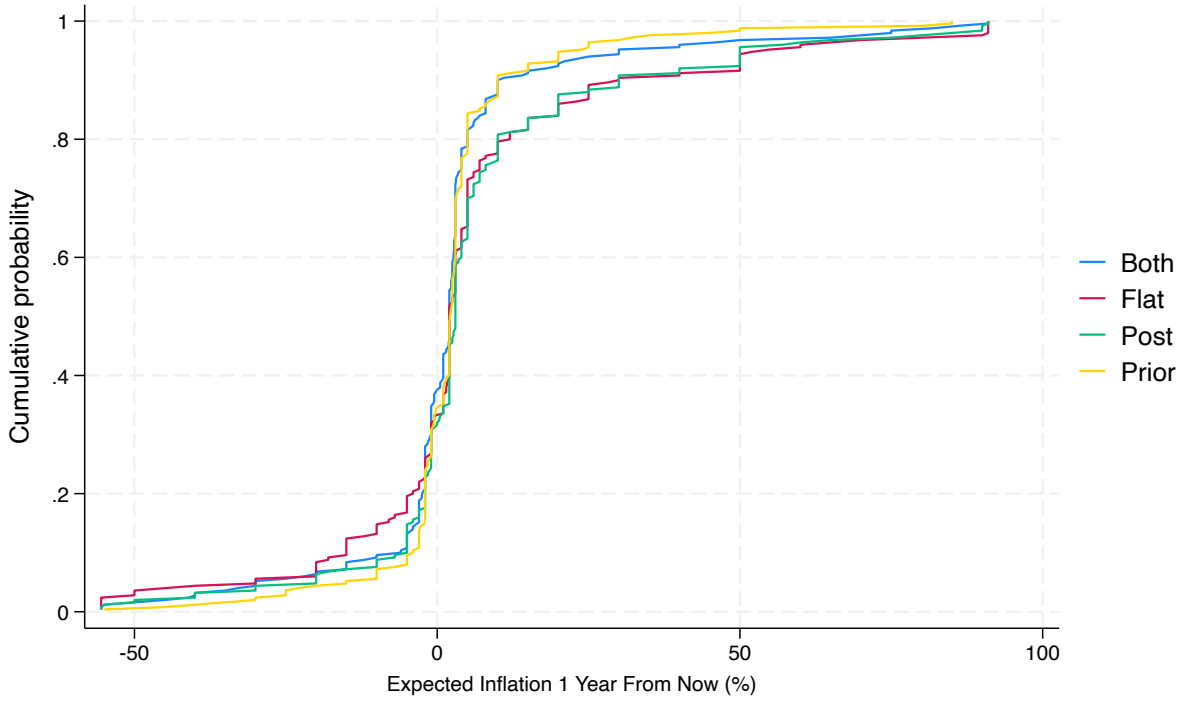
3 Results

This section details the results of our survey. We first show how incentives affect elicited expectations, highlighting that incentivized expectations become more consistent with SPF forecasts, exhibit lower disagreement, and diminish the puzzle of gendered expectations. Second, we show how incentives within an information provision experiment affect updated expectations and learning rates. Finally, we show how incentives raise participant effort.

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 1. 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* (yellow curve)—are contrasted with *Flat* (red curve) and *Post* (green curve), which do not include marginal incentives. The *Flat* and *Post* treatments mimic the typical approach used in all major macroeconomic surveys and thus reflect the incentive mechanism underlying the majority of belief data used in belief-based research in empirical macroeconomics.

Figure 1: CDF of Expected Inflation (Prior) 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.

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). More specifically, imposing marginal incentives during belief elicitation leads participants, on average, to expect significantly less extreme inflation values. These incentivized expectations appear more "reasonable" when evaluated against historical inflation data, current inflation trends, and the Fed's contemporaneous policy stance and outlook. Put simply, the beliefs elicited under marginal incentives resemble more informed expectations.

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

To quantify the effect of incentives on extreme forecasts, we estimate the following regression:

$$|\mathbb{E}(\pi_{prior})| = \alpha + \gamma_i Treatment_i + \beta\mathbb{X} + \epsilon \tag{1}$$

where $i \in Flat, Post, Both, Prior$ denotes the incentive treatment groups, and $\mathbb{X}$ represents a vector of control variables. The results of this regression are displayed in Table 3. Column (1) provides baseline results without controls, and each subsequent column progressively introduces additional covariates.

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 Variances

Test Type	Test Statistic	p-value
Levene's Test (Mean)	31.54	$p < .001$
Levene's Test (Median)	21.31	$p < .001$
Levene's Test (Trimmed Mean)	23.32	$p < .001$
F-Test (Variance Ratio)	1.9594	$p < .001$

Notes: This table shows mean and variances of the elicited prior belief of inflation $\mathbb{E}(\pi_{Prior})$ by incentive treatments. *Unincentivized* is comprised of treatments *Flat* and *Posterior*, while *Incentivized* is comprised of *Both* and *Prior*.

Our hypothesis, which we 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 3, the coefficients for *Both* and *Prior* indicate that marginal incentives significantly reduce extreme expectations of inflation rates, compared to the unincentivized *Flat* treatment. Specifically, respondents in the *Prior* group report significantly less extreme inflation expectations 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 less pronounced but still substantial, with respondents providing significantly less extreme inflation forecasts—about a third lower on average compared to *Flat* ($p < 0.01$). These effects are robust to controlling for the expected direction of price change (column (2)), for a participant's gender (column(3)), controlling whether a subject has at least an undergraduate degree (column (4)), and controlling for a participant's one-year-ahead economic sentiment (column (5)). As an additional exercise, we define the highest 10% of absolute prior inflation expectations as extreme forecasts. The logistic regression results in Table A-3 and Table A-4 indicate that respondents in the non-incentivized groups are more likely to report extreme values compared to the *Prior* group. Specifically, the *Flat* group is 222% more likely, the *Post* group is 181% more likely, and the *Both* group is 91% more likely to provide such forecasts.

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 reliability and accuracy or correct for potential biases that may arise in their absence. However, two critical questions arise: What are we truly measuring when incentives are used, and do they allow us to accurately capture the genuine

Table 3: Treatment Effects: Expected Inflation As Priors

	(1) $\mathbb{E}(\pi_{Prior})$	(2) $\mathbb{E}(\pi_{Prior})$	(3) $\mathbb{E}(\pi_{Prior})$	(4) $\mathbb{E}(\pi_{Prior})$	(5) $\mathbb{E}(\pi_{Prior})$
Post	-1.770	-1.785	-1.473	-1.314	-0.994
	(1.916)	(1.915)	(1.865)	(1.891)	(1.832)
Both	-5.268***	-5.213***	-5.828***	-5.757***	-6.052****
	(1.802)	(1.804)	(1.778)	(1.791)	(1.732)
Prior	-7.794****	-7.779****	-8.492****	-8.540****	-8.705****
	(1.620)	(1.620)	(1.610)	(1.612)	(1.564)
Deflation		-1.248	-1.609	-1.609	-0.0966
		(1.183)	(1.197)	(1.196)	(1.156)
Male			-7.175****	-7.052****	-6.907****
			(1.147)	(1.153)	(1.112)
Higher_Ed.				-2.162*	-2.017*
				(1.184)	(1.141)
Constant	15.20****	16.86****	20.56****	21.56****	16.65****
	(1.411)	(2.179)	(2.422)	(2.443)	(2.326)
Sentiment	No	No	No	No	Yes
N	1000	1000	1000	1000	1000

Robust standard errors in parentheses

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

Notes: 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.

beliefs we seek to understand? While further research is needed to fully address these questions, an interpretation of our findings is that the expectations of respondents in the incentivized treatments more accurately reflect those of households in the field, for whom inflation expectations matter and play a significant role in their decision-making.

We compare data from each of our treatments to the most recent mean PCE forecast from the Survey of Professional Forecasters (SPF) in Table 4. While expectations under *Flat* (5.50%) 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 2.64% respectively) that align more closely with professional forecasts from the SPF (2.11%).

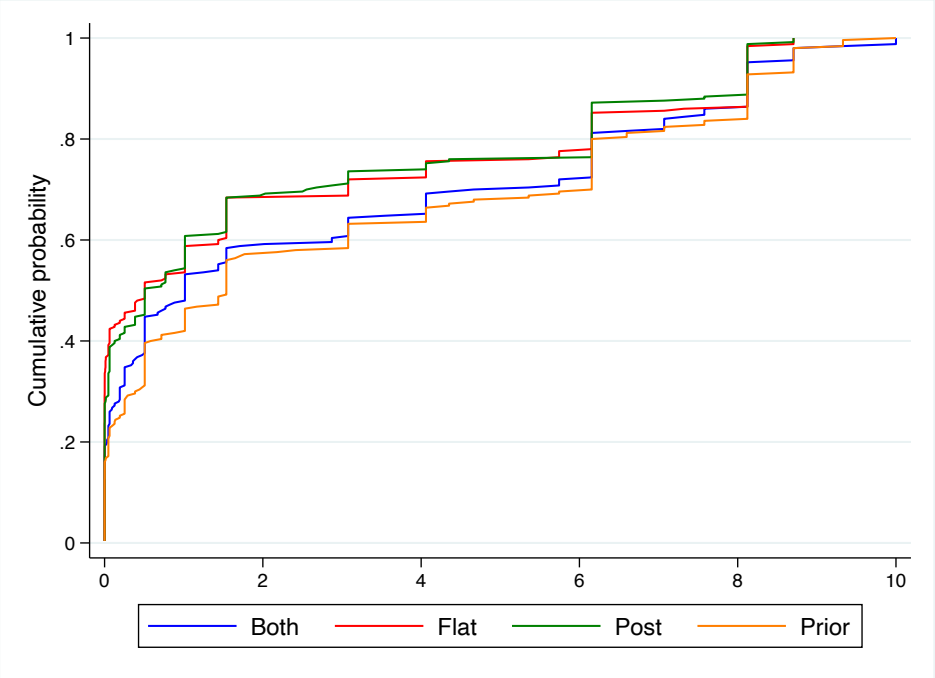
We also consider how our various incentive schemes impact participants' hypothetical payoffs. To do this, we assume the Fed's forecast of median inflation for 2025 (π_{2025}) is correct in expectation. Using this as a basis for comparison, we calculate a participant i's forecast error as $error_i = |\pi_{2025} - \mathbb{E}_i(\pi_{2025})|$ and her hypothetical bonus payment as $10 * (2^{-error_i})$. We depict the distribution of payoffs calculated this way across treatments in Figure 2 and explore the significance of these results in Table A-2.

Table 4: Comparing Experimental Data to SPF

	Treatment	SPF Mean (Std. Dev.)	Treatment Mean (Std. Dev.)	Difference	Welch's t-stat	p-value
Unincentivized	flat	2.11 (0.286)	5.50 (24.798)	-3.389	2.160	0.032
	post	2.11 (0.286)	6.75 (22.576)	-4.635	3.244	0.001
Incentivized	prior	2.11 (0.286)	2.80 (14.313)	-0.690	0.761	0.447
	both	2.11 (0.286)	2.64 (19.271)	-0.529	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.

Figure 2: Hypothetical Payoffs From Point Forecasts of Inflation (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.

The punchline is that marginal incentives significantly increase hypothetical earnings. In *Both*, we predict in Table A-2 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.

3.1.1 Incentives and A Common Puzzle in Survey-Based Inflation Beliefs

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.[10] Concisely, female survey participants typically report significantly higher inflation expectations than men. It is to this puzzle we now turn our attention. Our question is whether this puzzle survives the implementation of marginal incentives.

To do this, 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:

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

Additionally, we consider a final model where we pool our data across all four treatments and project inflation expectations onto a dummy variable capturing whether a participant was female, the incentive structure imposed on a participant, and the interaction of both terms. This specification allows us to test whether females exhibit a significantly different response to marginal incentives than males.

The interaction regression model is specified as:

$$\mathbb{E}(\pi_{Prior}) = \beta_0 + \beta_{1,i}\text{Treatment}_i + \beta_{2,i}\text{Female} + \beta_{3,i}(\text{Treatment}_i \times \text{Female}_i) + \epsilon_i$$

where our coefficient of interest is β_3.

The regression results, summarized in Table 5, reveal how incentives interact with gender to shape inflation expectations. In the absence of marginal incentives (*Flat*), female respondents have significantly higher inflation expectations (9.346, $p < .01$) than do their male counterparts. This finding is consistent with existing empirical literature that suggests that women often report higher inflation expectations or more pessimistic economic outlooks. This result also appears in *Post*, albeit muted and only marginally significant. Subjects knew they would eventually face marginal incentives in *Post*, but were unsure of when. It is possible a spill-over effect from the marginal incentives in the RCT portion of our survey heightened attention and effort sufficiently to mute the gender differences in inflation expectations to some extent.

[10] 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.

However, marginal incentives eliminate the significant difference in inflation expectations across genders. This is true for *Both* (2.68, $p > .1$) and *Prior* (1.08, $p > .1$). Further, we observe in column (5) that marginal incentives are killing the gender difference in expectations observed in *Flat* because they act significantly more strongly on belief formation for females than they do for males (i.e., *Prior*×Female $=-8.264$, $p < .05$, and *Both*×Female $=-6.67$, $p < .1$).

Table 5: Treatment Effects: Expected Inflation As Priors

	(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) All $\mathbb{E}(\pi_{Prior})$
Female	9.346***	4.779*	2.681	1.082	
	(2.941)	(2.836)	(2.204)	(1.634)	
Post					3.970*
					(2.227)
Both					0.576
					(1.999)
Prior					1.743
					(1.802)
Female					9.346***
					(2.941)
Post×Female					-4.568
					(4.085)
Both×Female					-6.665*
					(3.675)
Prior×Female					-8.264**
					(3.364)
Constant	0.356	4.326***	0.932	2.099**	0.356
	(1.489)	(1.656)	(1.334)	(1.015)	(1.489)
N	250	250	250	250	1000

Robust sandard 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.

These findings suggest that the puzzle of gendered expectations—where women report higher inflation expectations than men—diminishes under 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.

3.2 The Effect of Incentives on (In)Attention and Backward-Looking Heuristics

Why do incentivized expectations become more consistent with the SPF? A key factor appears to be (in)attention. In a similar spirit to Bracha and Tang (2024), who examine attention and perception errors in economic decision-making, we construct a measure of 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.[11] Intuitively, the further a respondent's perception deviates from actual inflation, the less attention they are likely to pay to inflation.

Column (1) of Table 6 shows that incentivized groups exhibit significantly lower inattention (or, equivalently, greater attention) to inflation, presumably because of spillover effects associated with incentives. At the same time, incentives appear to weaken the relationship between perceived and expected inflation. Columns (2-5) of Table 6 present regression results 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.

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. Instead, incentivized respondents seem to engage in more deliberate processing of information to generate more informed inflation forecasts.

As a result, we observe two simultaneous effects of incentives on inflation forecasts. First, incentives make forecasts more aligned with both realized inflation and professional forecasters, while also reducing disagreement among respondents. Second, incentives weaken the link between perceptions and expectations, suggesting that respondents move away from simple extrapolation of (perceived) past inflation. In other words, incentives shift the range of values respondents consider likely, leading them to form expectations within a more informed range.

Regarding the gender gap, we observe that women have noticeably higher APE (see Table A-5). 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. However, we find that marginal incentives play a crucial role here as well, significantly reducing the APE gap.

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

Table 6: Treatment Effect: Inattention and Perception

	(1) All APE	(2) Flat $\mathbb{E}(\pi_{Prior})$	(3) Post $\mathbb{E}(\pi_{Prior})$	(4) Both $\mathbb{E}(\pi_{Prior})$	(5) Prior $\mathbb{E}(\pi_{Prior})$
Perception		0.337***	0.354***	0.270**	0.165
		(0.079)	(0.103)	(0.131)	(0.145)
Post	-2.034				
	(2.175)				
Both	-8.875***				
	(2.050)				
Prior	-11.479***				
	(1.914)				
Deflation	2.770**	-29.908***	-23.802***	-19.134***	-14.827***
	(1.456)	(2.815)	(2.668)	(2.223)	(1.754)
Male	-9.239***	-4.491**	-2.081	-0.723	-2.170*
	(1.321)	(2.258)	(2.581)	(1.836)	(1.271)
Higher_Ed.	-3.589***	3.158	-3.996*	1.596	-0.244
	(1.362)	(2.288)	(2.173)	(1.715)	(1.529)
Constant	23.498***	9.496***	11.683***	6.225***	7.266***
	(2.060)	(2.272)	(3.089)	(1.578)	(1.379)
N	1000	250	250	250	250

Robust sandard errors in parentheses

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

Notes: The table shows the effect of treatment on Absolute Perception Error (APE) and the relationship between perceived inflation and prior point forecast. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels.

3.3 Effect of Incentives in Information Provision Experiments

We now focus on the role that marginal incentives play—or not—in a simple information provision experiment. We find that strategic incentives can effectively bridge perception gaps, suggesting that RCTs without marginal incentives may systematically underestimate the impact of information on beliefs.

After eliciting a point expectation of one-year-ahead inflation (alongside a few other measures), we provide each participant with a summary of the Fed's outlook on how inflation might evolve in 2025. We then ask subjects to provide probabilistic forecasts for the evolution of food, gas, and aggregate prices. This creates distance between the information provision and the elicitation of posterior beliefs. We finally collect data from a binned inflation forecast to estimate a subject's updated inflation expectation ("posteriors"), which we treat as our measure of interest throughout this section.[12] Recall that we adopt the same point and bin

[12]We depict these expectations in Figure A-1 and explore whether marginal incentives impact the distributions of these expectations collected across treatment in Table A-6. For the sake of brevity, we report both in appendix A1. Similar to what D'Acunto et al. (2023) demonstrates for the SCE data, we find that the

elicitation strategies followed by the NY Fed in its Survey of Consumer Expectations.[13]
As noted by Haaland et al. (2023), information provision experiments measuring beliefs and belief updating via quantitative measures typically quantify the extent to which respondents adjust their beliefs toward new information conveyed via some signal. They call this the *learning rate*, which one can estimate using

$$\text{Updating}_i = \beta_0 + \beta_1(\text{Treatment}_i \times \text{PercGap}_i) + \beta_2 \text{Treatment}_i + \beta_3 \text{PercGap}_i + \epsilon_i \qquad (2)$$

where Updating_i is the distance between respondent i's posterior and prior one-year-ahead inflation expectation, Treatment_i is an indicator variable denoting which incentive structure 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 for the same. Given these definitions, comparing across β_1 captures the extent to which incentive structure drives belief updating relative to our baseline 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.[14]
We show results from estimating various versions of Equation (2) in Table 7, where $\beta_{1,i}$ are our primary coefficients of interest. Column (1) shows our baseline specification with no additional controls, and columns (2) through (4) include additional controls for sex, educational attainment, and economic sentiment. Regardless of specification, $\beta_{1,Post}$ is significant at the 1% level, indicating that imposing marginal incentives in our simple information provision experiment led to significantly more belief updating. The same effect is true for *Both*, where subjects know that we will either pay for the accuracy of their prior or posterior belief about inflation. Interestingly, this effect (captured by $\beta_{1,Both}$ is roughly 30% smaller across all specifications and significant at only the 10% (baseline specification, column (1)) or 5% (columns (2) - (4)) levels.
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 discrepancies between their prior beliefs and the Federal Reserve's forecasts. This suggests

binned inflation forecasts exhibit less disagreement and a lower mean expected inflation than point forecasts.

[13] There is evidence from Becker et al. (2023) that the number, center, and width of bins can significantly alter expectations provided by survey respondents. Though beyond the scope of this paper, an interesting question for future research is whether and how this might interplay with marginal incentives.

[14] 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.

Table 7: Effect of Incentives on Learning Rates

	(1) Updating	(2) Updating	(3) Updating	(4) Updating
$\beta_{1,Post}$	0.0344***	0.0345***	0.0348***	0.0326***
	(0.0113)	(0.0113)	(0.0112)	(0.0111)
$\beta_{1,Both}$	0.0262*	0.0265**	0.0269**	0.0261**
	(0.0135)	(0.0134)	(0.0134)	(0.0126)
$\beta_{1,Prior}$	0.0189	0.0190	0.0194	0.0195
	(0.0230)	(0.0231)	(0.0232)	(0.0226)
$\beta_{2,Post}$	-0.448*	-0.444*	-0.452*	-0.492**
	(0.244)	(0.244)	(0.243)	(0.237)
$\beta_{2,Both}$	-0.212	-0.232	-0.239	-0.191
	(0.240)	(0.242)	(0.242)	(0.237)
$\beta_{2,Prior}$	-0.0648	-0.0881	-0.0904	-0.0511
	(0.239)	(0.241)	(0.241)	(0.236)
β_3	0.910****	0.909****	0.909****	0.913****
	(0.00772)	(0.00777)	(0.00779)	(0.00775)
Male		-0.155	-0.157	-0.0649
		(0.166)	(0.166)	(0.167)
Higher_Ed.			0.0598	0.0335
			(0.166)	(0.163)
Constant	0.0185	0.102	0.0756	0.485**
	(0.177)	(0.200)	(0.218)	(0.246)
Sentiment	No	No	No	Yes
N	1000	1000	1000	1000

Robust sandard errors in parentheses

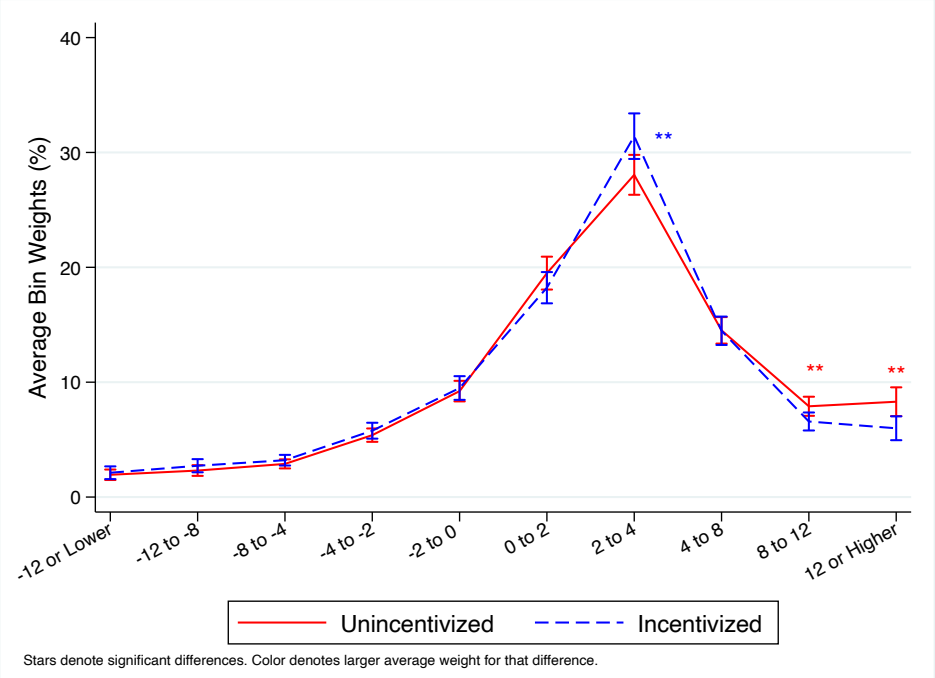
* $p < .1$, ** $p < .05$, *** $p < .01$, **** $p < .001$

Notes: The Table shows the effect of treatments on learning rates. These are relative to our baseline treatment *Flat*. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 1% and 99% levels.

that strategic incentives can effectively bridge perception gaps, leading to belief formation that is more responsive and consistent with professional forecasts. More importantly, this suggests that RCTs implemented without marginal incentives may systematically underestimate the impact of information on beliefs, especially in environments where expectations are well-anchored, such as during periods of low inflation (Weber et al. 2025; Bracha and Tang 2024). For this particular experiment, implementing marginal incentives leads to a qualitatively different conclusion about 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 the constant (i.e.,

Figure 3: 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.

information is readily and freely provided, but processing costs remain), which impacts rational inattention to information provision (Mackowiak and Wiederholt 2024). 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 might not perceive having accurate inflation expectations as beneficial 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.

Additionally, we can consider how marginal incentives changed the distribution of probabilities participants assigned to each possible inflation bin. Recall, values assigned to each bin when forming a bin forecast denote the participant's belief about the likelihood of realized inflation falling into that particular bin. Figure 3 depicts the average weight assigned to each of the ten possible inflation bins presented to subjects who faced marginal incentives (blue dashed line) or did not (red solid line). Interestingly, imposing marginal incentives shifts significantly more weight to the bin containing the Fed's median forecast for 2025 inflation communicated in the information intervention and significantly reduces weights assigned to more extreme bins. This further reinforces our result that marginal incentives significantly alter the efficacy of information provision in such experiments.

3.4 Incentives And Effort

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.

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

$$\text{CompletionTime}_i = \alpha + \gamma_1 \text{Post}_i + \gamma_2 \text{Both}_i + \gamma_3 \text{Prior}_i + \beta \mathbf{X}_i + \epsilon_i, \tag{3}$$

where CompletionTime_i is the total time (in seconds) participant i took to complete the survey. Post_i, Both_i, and Prior_i are dummy variables indicating the incentivized treatment group 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 gender (Male), education level (Higher_Ed), and forward-looking economic sentiment (included in(4)).[15]

We report regression results in Table 8.

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 104 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 50 to 58 seconds longer than participants in the *Flat* treatment. Although the coefficients for the *Post* treatment are positive, they are not statistically significant, 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

[15] We show the same results without winsorizing in Table A-7, located in appendix A1.

Table 8: Effect of Incentives on Effort

	(1) Completion Time	(2) Completion Time	(3) Completion Time	(4) Completion Time
Post	19.94	23.05	27.18	29.24
	(25.86)	(25.71)	(25.64)	(25.68)
Both	110.8****	104.6****	106.4****	104.0****
	(26.80)	(26.72)	(26.53)	(26.50)
Prior	58.43**	51.37**	50.11**	53.37**
	(25.29)	(25.13)	(25.16)	(25.20)
Male		-70.62****	-67.44****	-72.43****
		(18.63)	(18.60)	(18.56)
Higher_Ed.			-56.19***	-59.62***
			(18.26)	(18.26)
Constant	567.4****	599.0****	625.0****	585.9****
	(18.39)	(19.88)	(21.97)	(25.59)
Sentiment	No	No	No	Yes
N	1000	1000	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. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 5% and 95% levels.

formation.

The regression results also reveal significant effects of participant gender and education level on completion time. The coefficient for male participants is negative and highly significant across specifications, indicating that males spent approximately 67 to 72 seconds less on the survey than females. This suggests that female participants generally invest more time and effort into completing the survey tasks. Additionally, participants with at least an undergraduate degree spent about 56 to 60 seconds less on the survey compared to those without higher education. This may reflect greater familiarity with the survey content or more efficient processing of the information among more educated participants. The inclusion of economic sentiment in column (4) does not substantially alter the coefficients of interest, and the main findings regarding the impact of marginal incentives on completion time remain robust.

4 Discussion

Our experiment demonstrates that marginal incentives significantly impact the elicitation of macroeconomic beliefs and learning rates in information provision experiments. Specifically, imposing incentives leads to household inflation expectations that are more consistent

with expectations of experts, with respondents predicting less extreme values and exhibiting less cross-sectional forecast disagreement. Furthermore, these incentives increase the rate at which participants update their beliefs in response to new information, suggesting that central bank communication can more effectively coordinate expectations in environments where households have higher incentives to pay attention to inflation. Importantly, the changes in the underlying distributions of beliefs arising from different incentive structures lead to qualitatively different conclusions about the efficacy of central bank communication. Our findings suggest that central banks can boost the effectiveness of their communication policies by exploring innovative ways to incentivize households. Options include decreasing the cost of paying attention, for example, by improving their communication strategy.

These findings challenge the current reliance on unincentivized or flat-fee structures in most survey-based macroeconomic research, highlighting potential biases in elicited beliefs when incentives are absent. The substantial reduction in potential forecast errors and heightened learning rates observed with marginal incentives indicate that incentivized elicitation provides a more reliable measure of household expectations, which are critical for 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 simple resolution to a long-standing puzzle in the belief-based macroeconomic literature. This suggests that marginal incentives can mitigate systematic biases observed in survey-based beliefs elicited via flat-fee incentives.

Incorporating marginal incentives into survey design leads to more consistent measurements of inflation expectations across respondents and brings household inflation forecasts closer to professional forecasters' consensus views. This reduction in cross-sectional disagreement and convergence toward expert predictions suggests that incentivized surveys may provide policymakers with more reliable measurements for understanding household expectations—a crucial input for those focused on managing inflation dynamics and developing economic forecasting models. Notably, this change in incentive structure need not increase the cost of collecting expectations. Further, policymakers and researchers can leverage our approach to quantify the extent of measurement error due to the absence of incentives.

Our findings also relate to the literature on the impact of external conditions on RCTs. Studies suggest that extraneous information conditions can mute the effects observed in RCTs. For instance, during periods when attention to inflation and monetary policy is high—as has recently been the case—the efficacy of information provision may appear diminished in unincentivized settings. Our results from the flat-fee setting align with this, showing that information provision without incentives does not significantly affect beliefs. However, introducing marginal incentives leads to a qualitatively different conclusion: information provision becomes effective in shifting expectations. This highlights the importance of incentive

mechanisms in accurately assessing the impact of policy communications.

While our study underscores the benefits of incorporating marginal incentives, it is important to consider potential drawbacks. One concern is that incentive schemes may induce behavior that does not reflect genuine beliefs but rather strategic reporting, such as looking up information online. Participants might engage in actions aimed at maximizing their payoffs rather than truthfully 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. While our experimental setup minimizes this risk by controlling the information environment, it remains a potential issue in other survey contexts.

Additionally, Danz et al. (2022) provide evidence that complex incentive schemes 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.

Therefore, 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.

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. By motivating participants to engage more deeply when forming beliefs, incentivized mechanisms can lead to more reliable data. Future studies should consider integrating such mechanisms to improve data quality, while also being mindful of the potential pitfalls associated with incentive complexity. Balancing the benefits of increased effort and accuracy against the risks of strategic behavior and misunderstanding is essential for advancing survey-based measures of economic expectations and informing more effective policy decisions.

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Appendix

A1 Other Tables and Figures

Table A-1: 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-2: Hypothetical Earnings From Point Forecasts of Inflation (Priors)

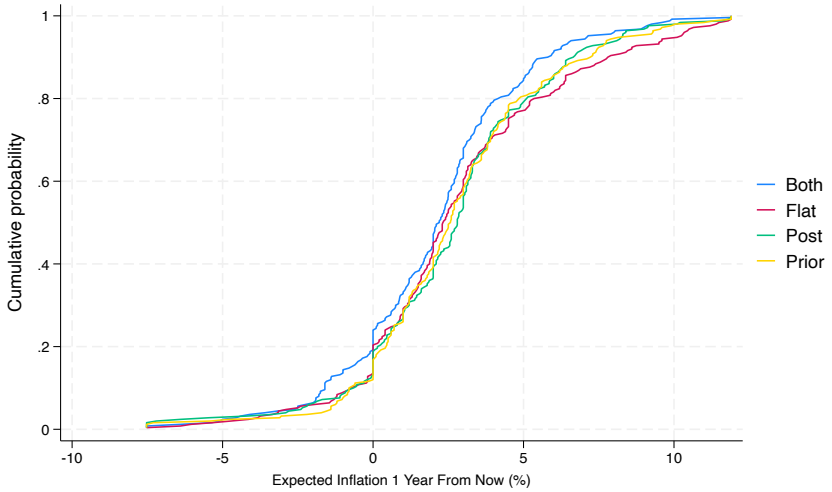
	(1) Payoff	(2) Payoff	(3) Payoff	(4) Payoff
Post	-0.0606	-0.110	-0.150	-0.197
	(0.270)	(0.264)	(0.264)	(0.262)
Both	0.545*	0.644**	0.626**	0.613**
	(0.281)	(0.277)	(0.277)	(0.275)
Prior	0.755***	0.867***	0.880***	0.874***
	(0.282)	(0.275)	(0.275)	(0.274)
Male		1.127****	1.096****	1.112****
		(0.202)	(0.202)	(0.202)
Higher_Ed.			0.546***	0.537***
			(0.195)	(0.195)
Constant	2.296****	1.791****	1.538****	1.795****
	(0.193)	(0.203)	(0.219)	(0.268)
Sentiment	No	No	No	Yes
N	1000	1000	1000	1000

Robust 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-1: CDFs by treatment 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-3: Treatment Effects on Extreme Forecasts: Logistic Regression Results

	(1) EV	(2) EV	(3) EV	(4) EV
Both	0.649*	0.651*	0.671*	0.688*
	(0.371)	(0.371)	(0.373)	(0.374)
Flat	1.172***	1.172***	1.300***	1.310***
	(0.348)	(0.348)	(0.351)	(0.352)
Post	1.032***	1.031***	1.205***	1.237***
	(0.353)	(0.353)	(0.357)	(0.358)
Deflation		-0.048	-0.094	-0.092
		(0.226)	(0.229)	(0.229)
Male			-1.248***	-1.232***
			(0.262)	(0.263)
Higher_Ed.				-0.333
				(0.218)
Constant	-2.987***	-2.971***	-2.658***	-2.523***
	(0.296)	(0.305)	(0.311)	(0.321)
N	1000	1000	1000	1000

Standard errors in parentheses

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

Notes: The table presents the results of a logistic regression analyzing the relationship between treatment assignment and the likelihood of reporting an extreme forecast value. Extreme values are defined as the highest 10% of absolute prior inflation expectations. *Prior* treatment group serves as the reference category.

Table A-4: 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-5: 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-6: Treatment Effects: Expected Inflation As Posterior

	(1) $\mathbb{E}(\pi_{Post.})$	(2) $\mathbb{E}(\pi_{Post.})$	(3) $\mathbb{E}(\pi_{Post.})$	(4) $\mathbb{E}(\pi_{Post.})$	(5) $\mathbb{E}(\pi_{Post.})$
Post	-0.162	-0.163	-0.149	-0.115	-0.0885
	(0.232)	(0.232)	(0.231)	(0.233)	(0.222)
Both	-0.642***	-0.624***	-0.652***	-0.637***	-0.688***
	(0.227)	(0.227)	(0.227)	(0.228)	(0.219)
Prior	-0.339	-0.344	-0.377	-0.387*	-0.426*
	(0.234)	(0.234)	(0.234)	(0.234)	(0.222)
Deflation		-0.316	-0.321	-0.315	-0.0673
		(0.246)	(0.245)	(0.245)	(0.243)
Male			-0.325**	-0.299*	-0.241
			(0.163)	(0.162)	(0.157)
Higher_Ed.				-0.459***	-0.430***
				(0.159)	(0.152)
Constant	4.571****	4.613****	4.759****	4.971****	4.681****
	(0.171)	(0.175)	(0.192)	(0.207)	(0.216)
Sentiment	No	No	No	No	Yes
N	1000	1000	1000	1000	1000

Robust standardd errors in parentheses

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

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. Data are winsorized at the 1% and 99% levels.

Table A-7: Effect of Incentives on Effort

	(1) Completion Time	(2) Completion Time	(3) Completion Time	(4) Completion Time
Post	27.06	30.49	35.13	37.26
	(29.01)	(28.86)	(28.75)	(28.86)
Both	127.5****	120.6****	122.7****	119.6****
	(30.55)	(30.47)	(30.26)	(30.23)
Prior	58.49**	50.69*	49.28*	53.15*
	(27.39)	(27.19)	(27.19)	(27.28)
Male		-77.99****	-74.41****	-80.11****
		(20.97)	(20.90)	(20.85)
Higher_Ed.			-63.14***	-67.40***
			(20.55)	(20.54)
Constant	572.3****	607.2****	636.4****	590.7****
	(19.99)	(21.76)	(24.28)	(27.78)
Sentiment	No	No	No	Yes
N	1000	1000	1000	1000

Robust 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. Regressions are estimated by OLS with robust standard errors. Data are winsorized at the 5% and 95% levels.

A2 Power Analysis

Table A-8: 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:

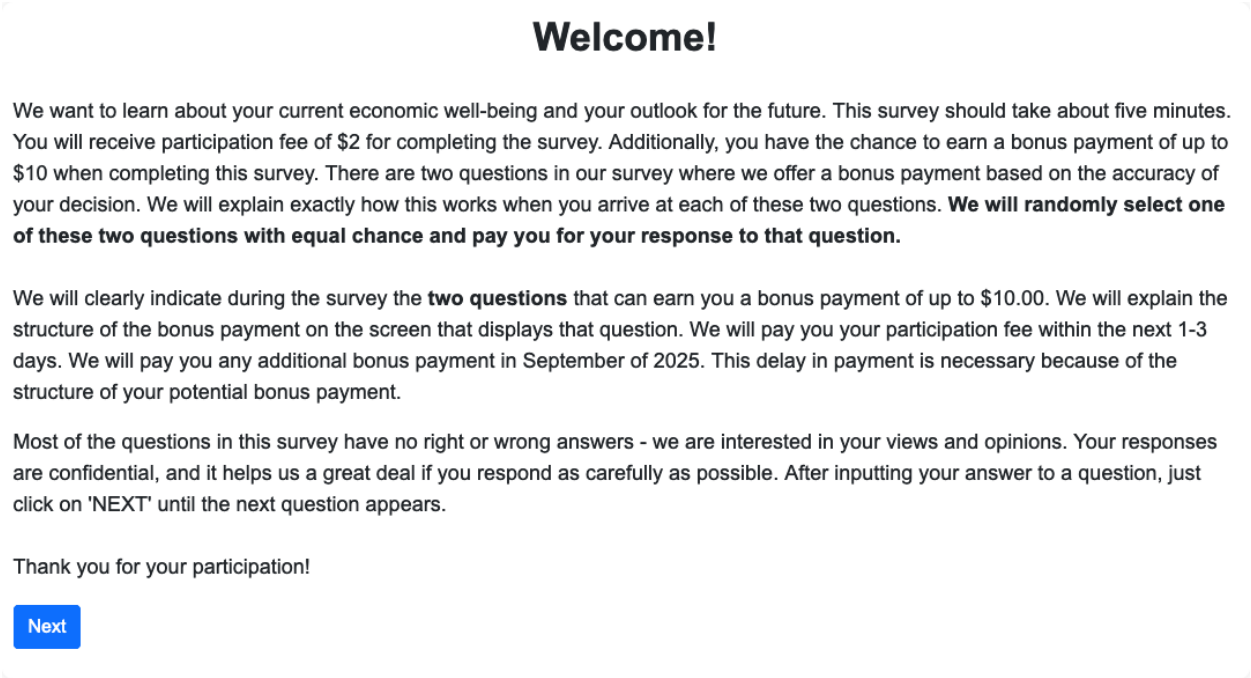
- **Small effect size:** $d = 0.2$
- **Medium effect size:** $d = 0.5$
- **Large effect size:** $d = 0.8$

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 Instrument

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

Figure A-2: Welcome



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

Figure A-3: General Questions

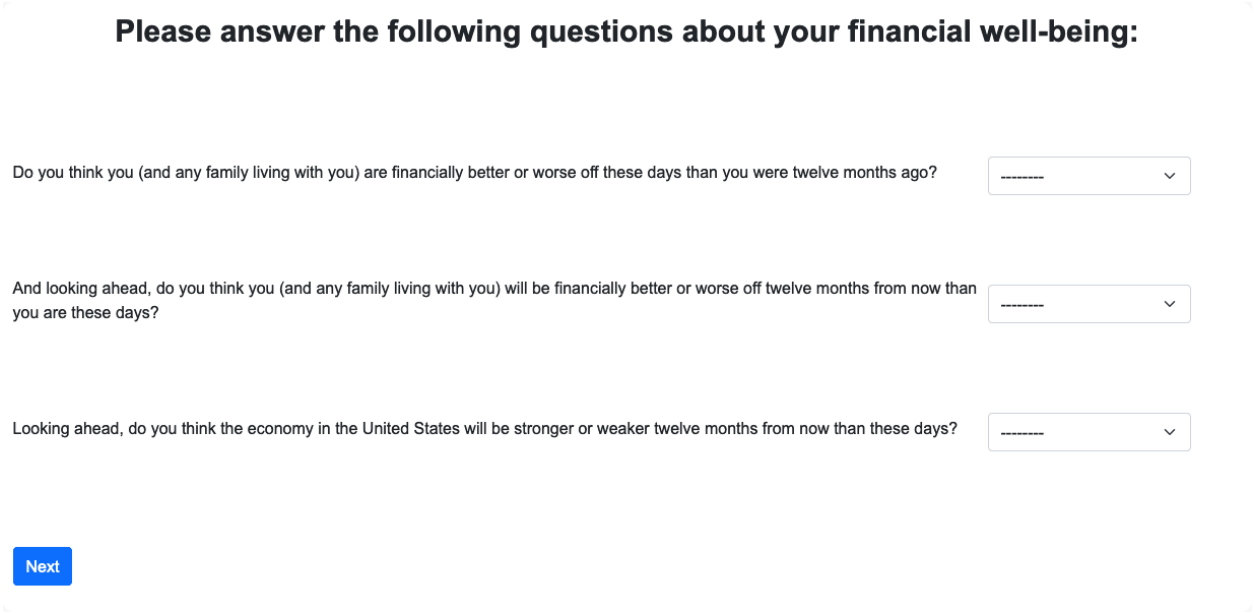


Figure A-4: Explanations

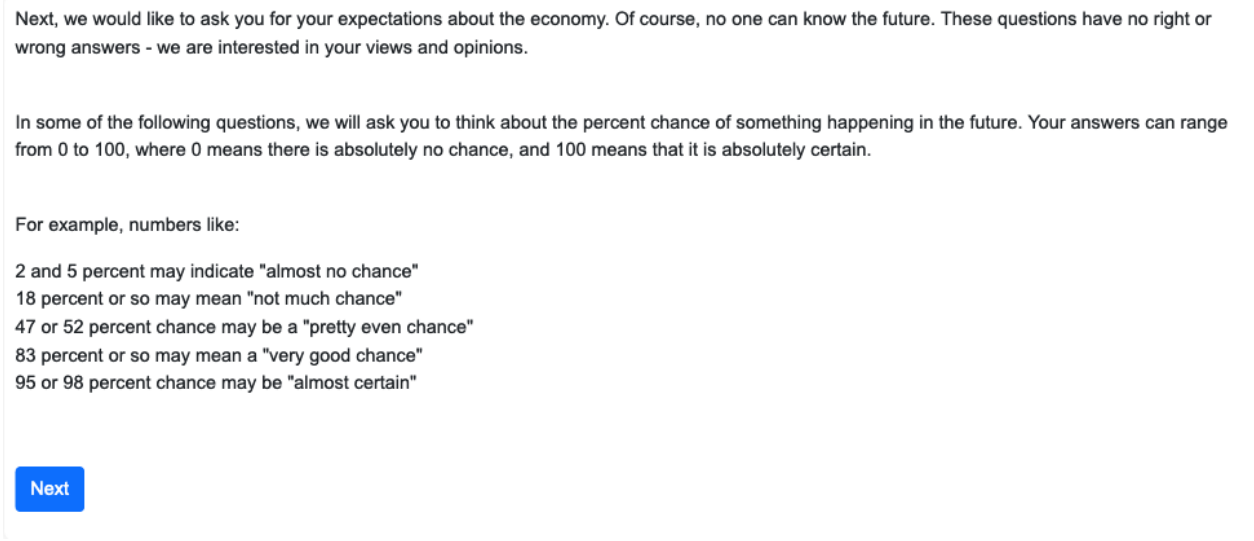


Figure A-5: Inflation Point Forecast (Flat)

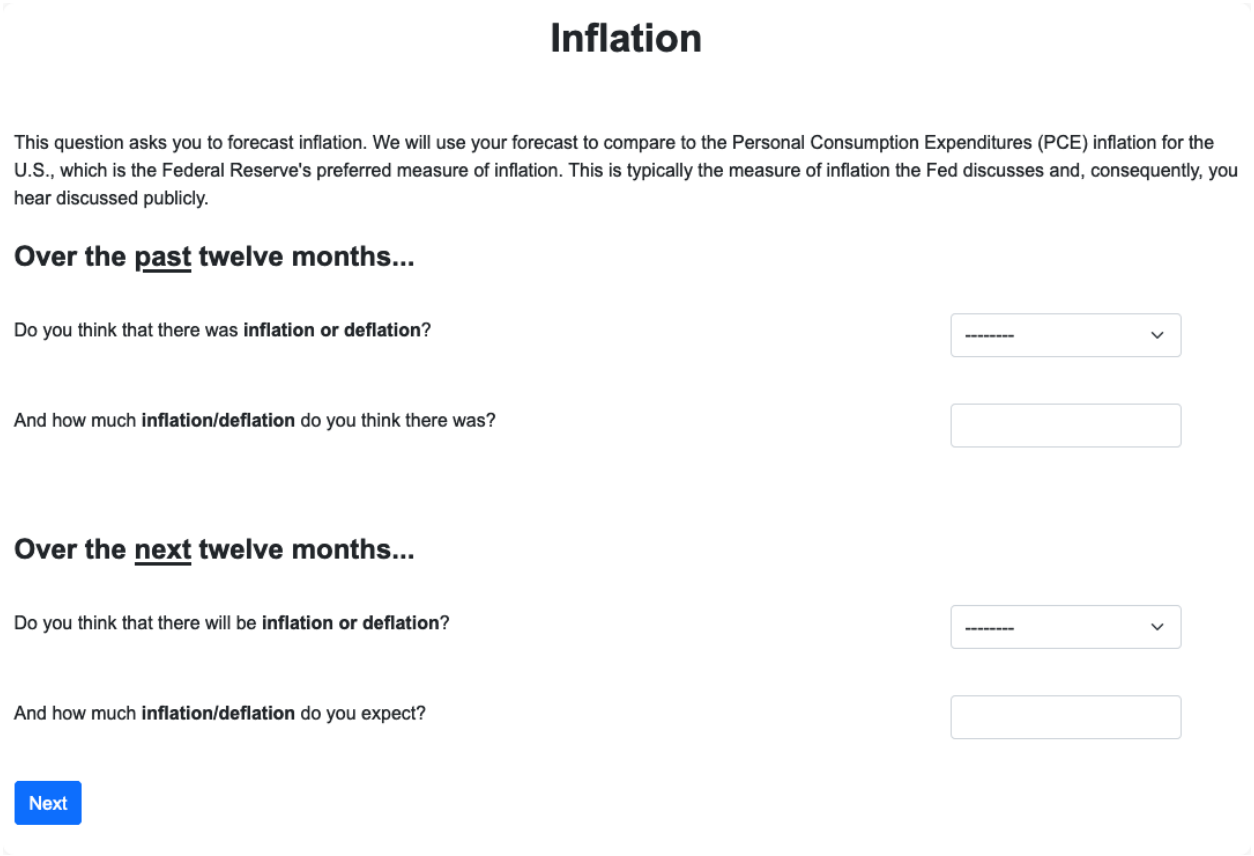


Figure A-6: Inflation Point Forecast (Posterior)

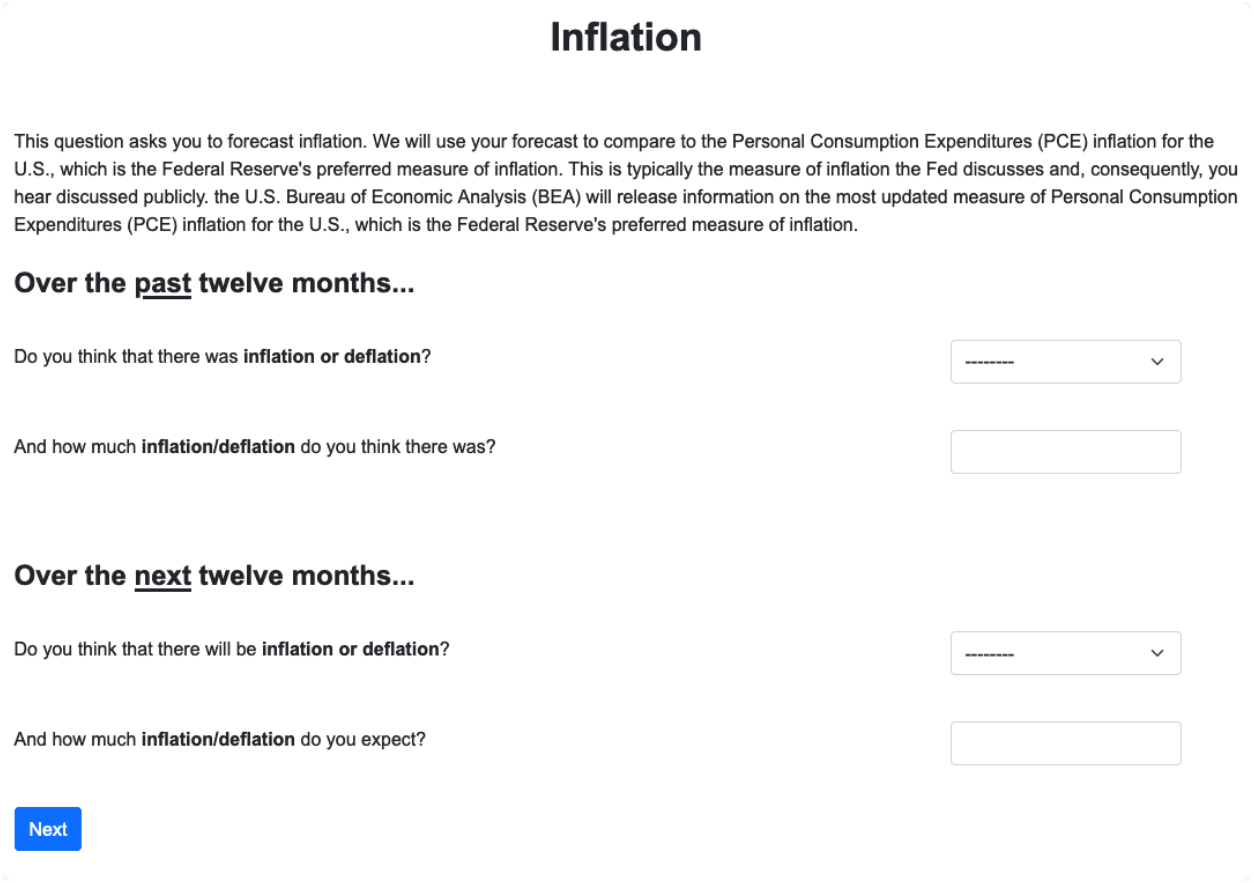


Figure A-7: Inflation Point Forecast (Prior)

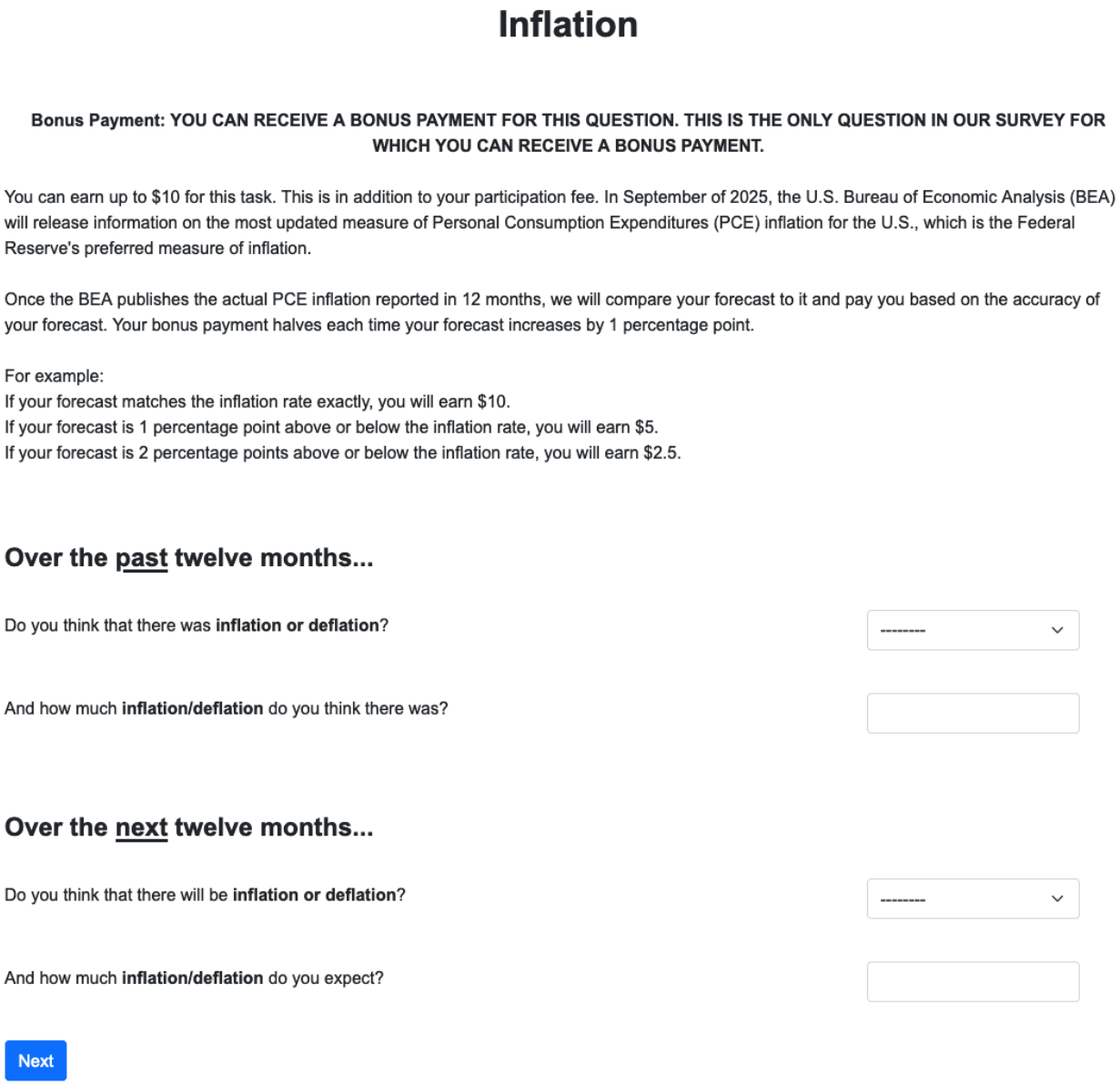


Figure A-8: Inflation Point Forecast (Both)

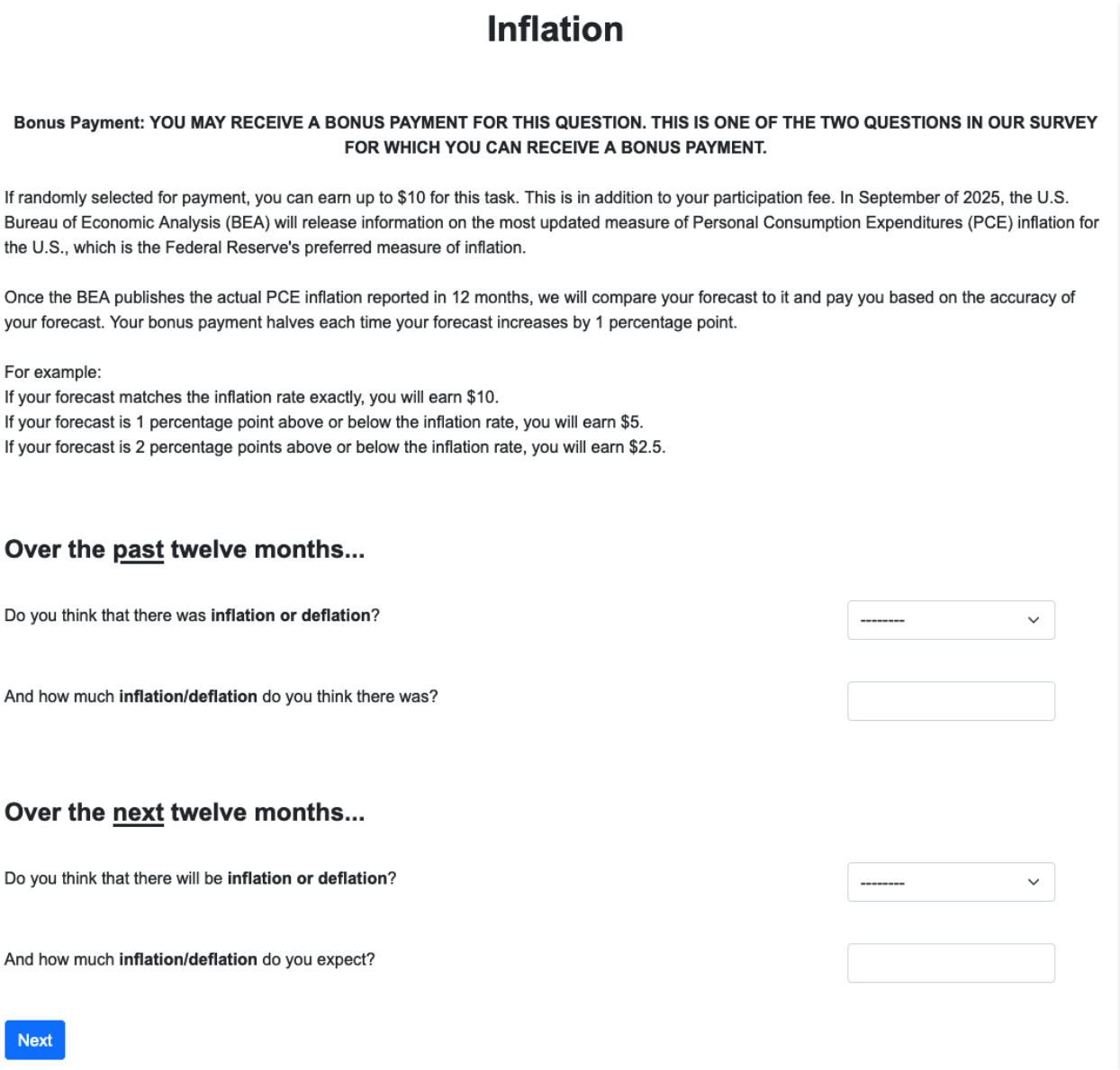


Figure A-9: Food Point Forecast

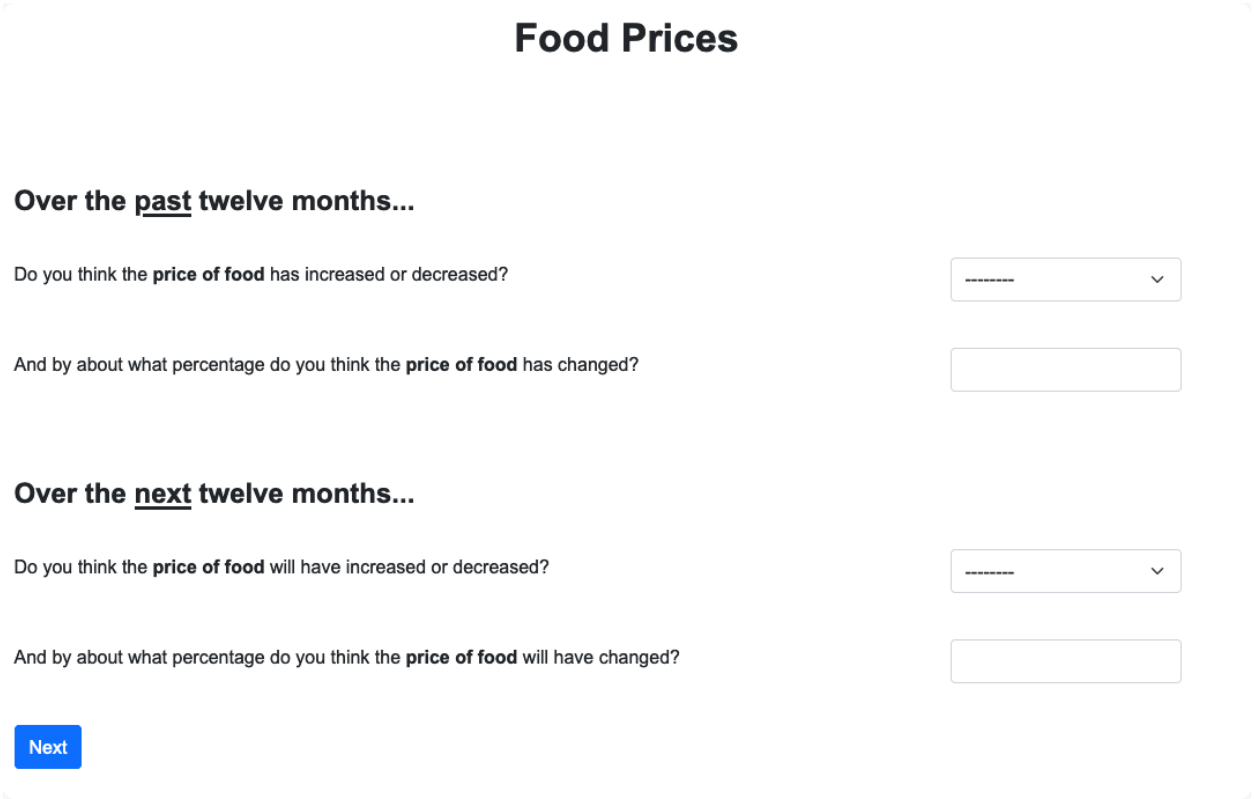


Figure A-10: Gas Point Forecast

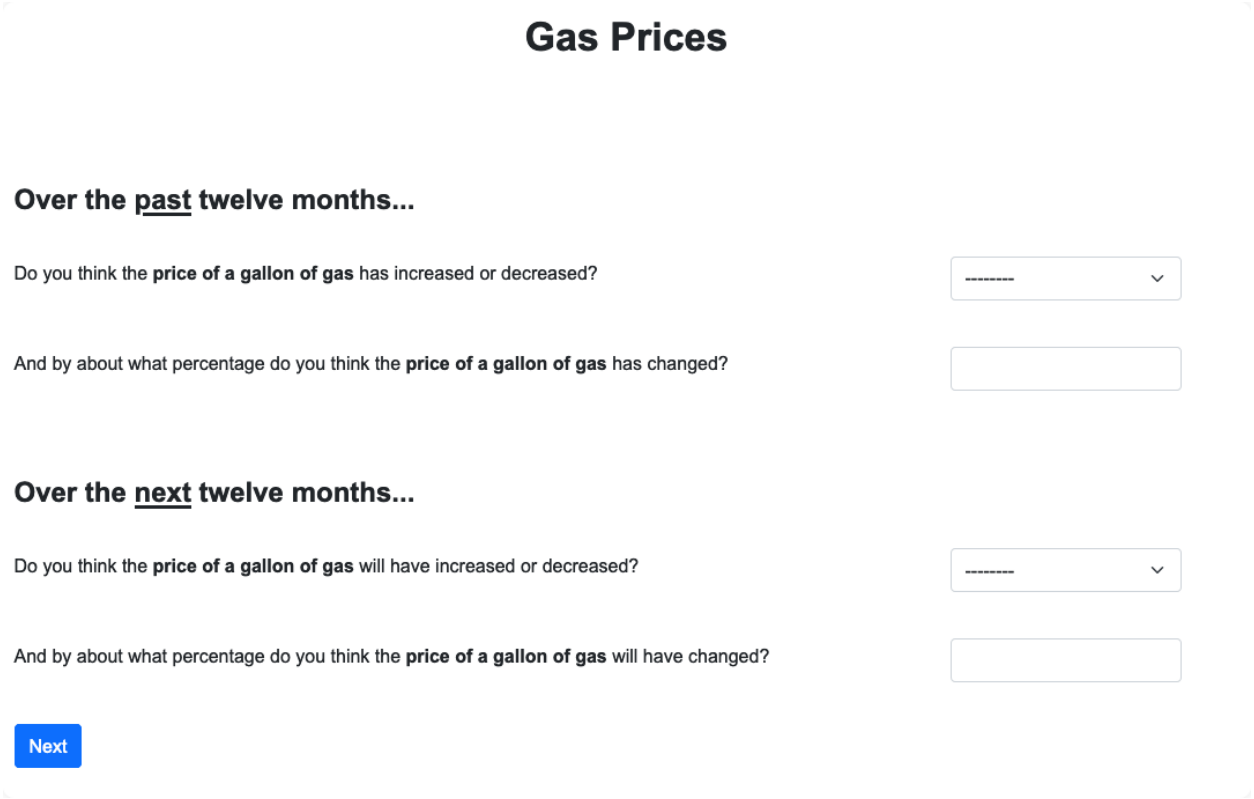


Figure A-11: Information Intervention

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-12: 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%: ____ %

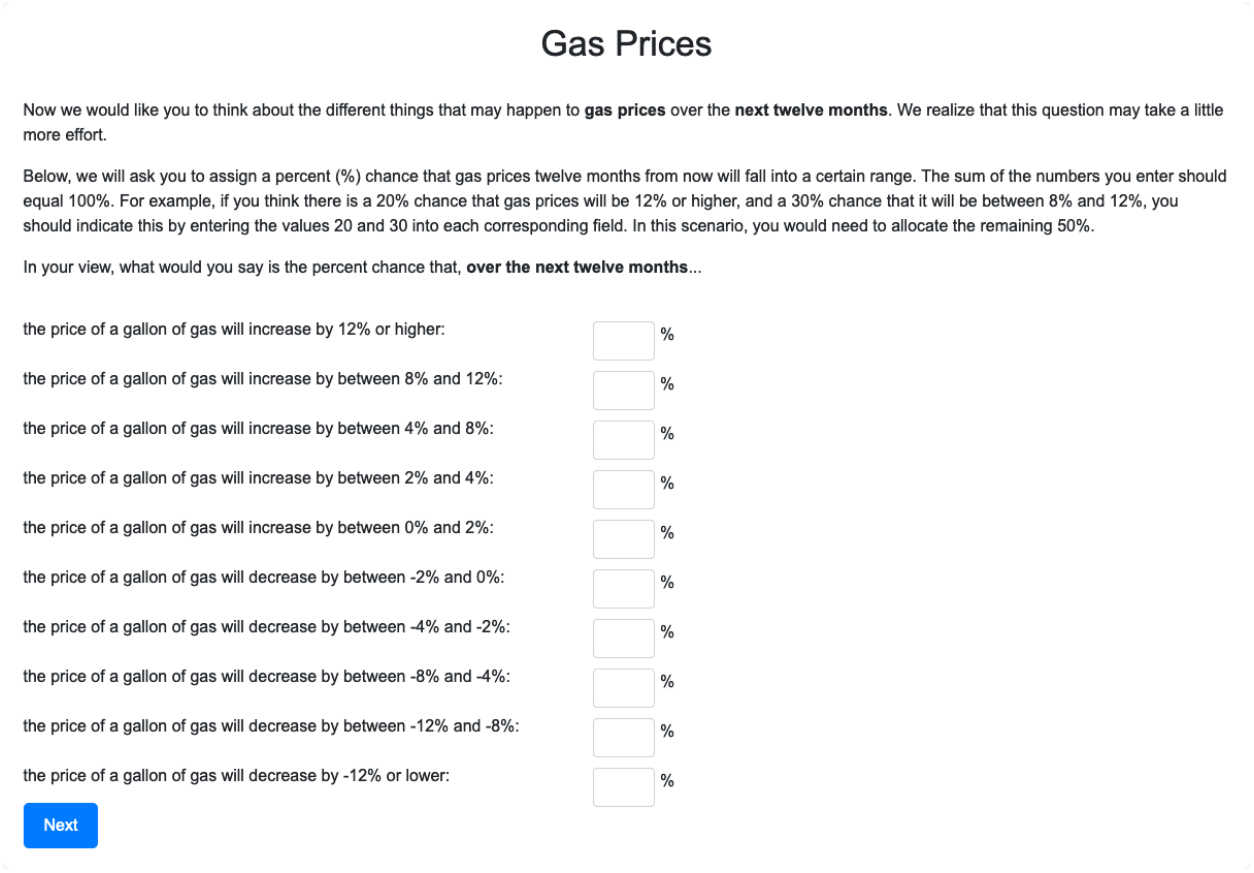
the price of food will decrease by between -8% and -4%: ____ %

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

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

Next

Figure A-13: 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-14: Inflation Bin Forecast (Flat and Prior)

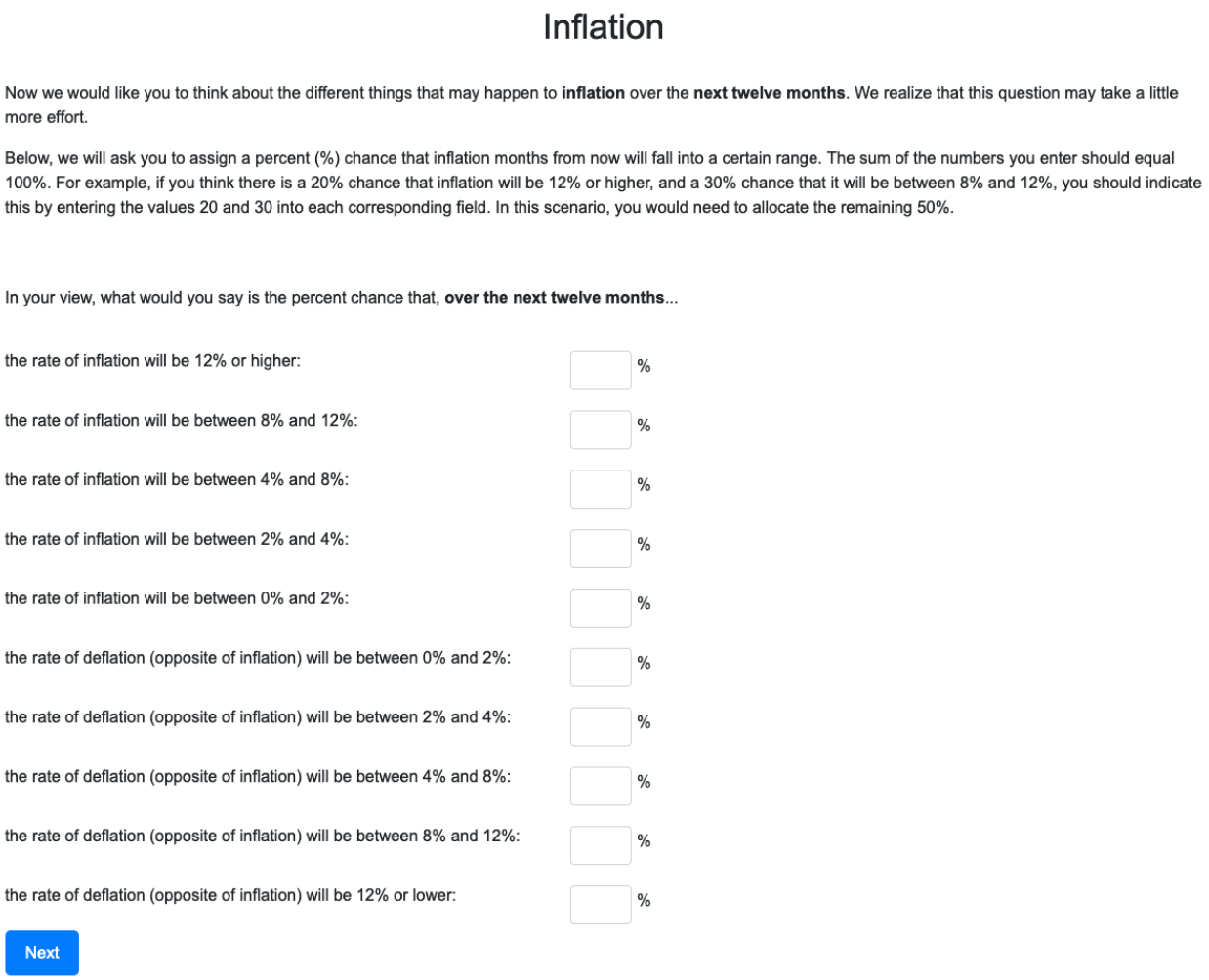
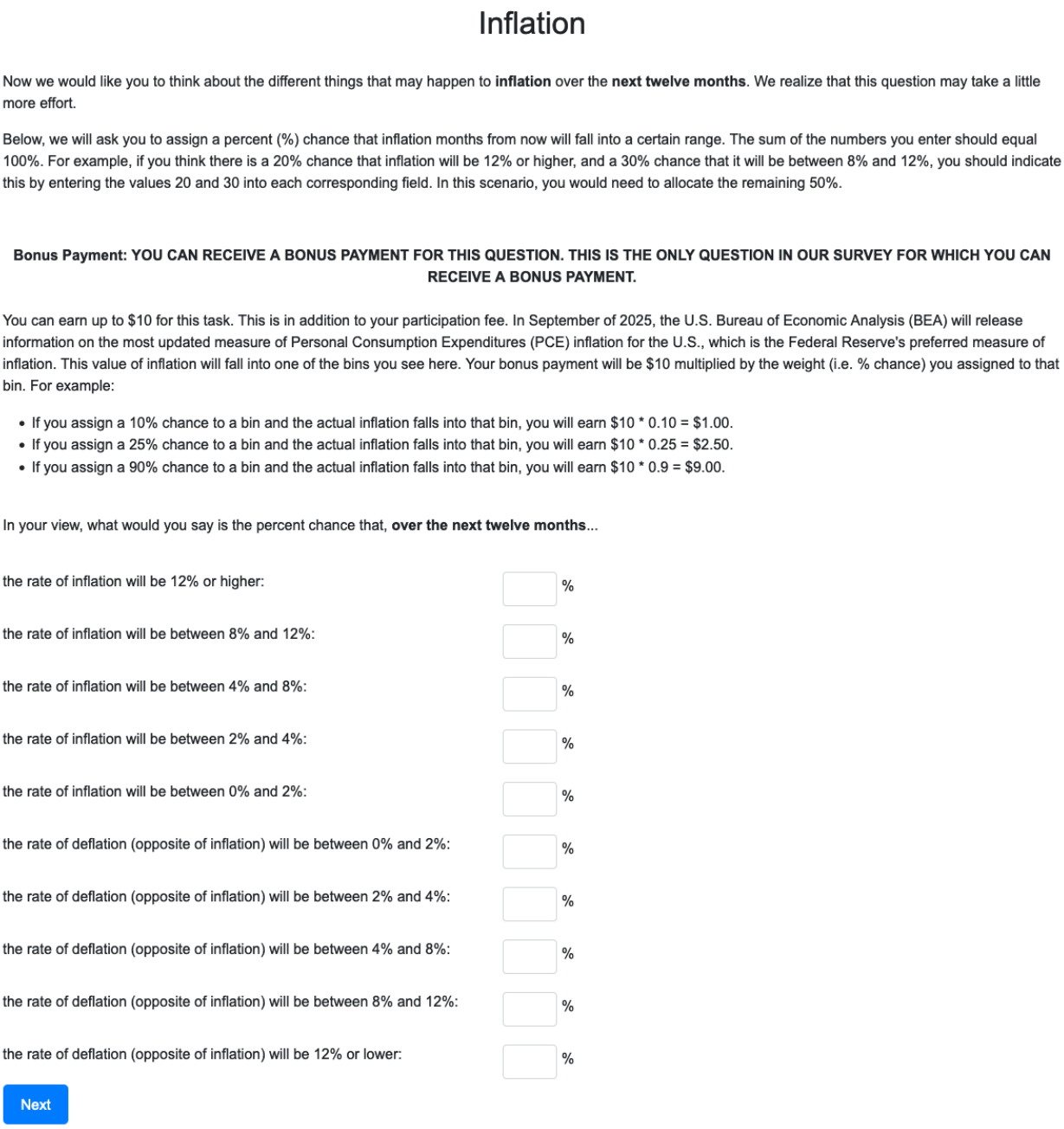


Figure A-15: Inflation Bin Forecast (Posterior)



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-16: 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:

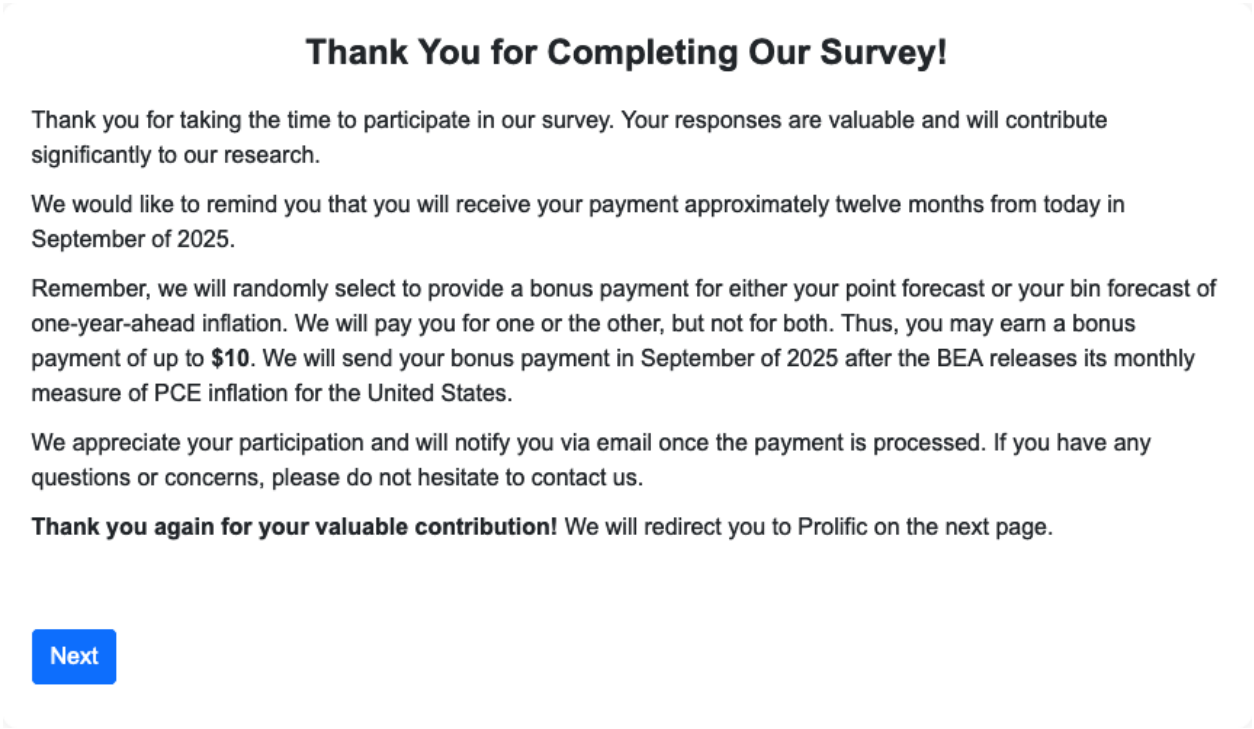
- If you assign a 10% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.10 = $1.00.
- If you assign a 25% chance to a bin and the actual inflation falls into that bin, you will earn $10 * 0.25 = $2.50.
- If you assign a 90% chance to a bin and the actual inflation falls 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-17: End of Survey



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