Incentivizing Inflation Expectations*

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Abstract

Inflation expectations are crucial for economic modeling and policymaking. Despite evidence supporting the use of marginal incentives for eliciting accurate beliefs, all major surveys of macroeconomic beliefs pay a flat participation fee. This lack of marginal incentives extends to many information provision experiments—often designed as randomized controlled trials (RCTs). In a large-scale online study, we introduce marginal incentives into a standard survey of inflation expectations. Marginal incentives significantly alter expectation distributions, reducing mean forecasts, 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 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 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 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 economic decision-making, expectations 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.

Despite their growing prevalence, 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.

[1] For a review of this literature, see D'Acunto and Weber (2024). 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 incentive-compatible elicitations. 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.

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 transfer to households. By contrast, Armantier et al. (2015), find a strong correlation between non-incentivized and incentivized measures of inflation expectations, except for respondents of lower education and financial literacy, suggesting overall that marginal incentives might not always be necessary. Similarly, 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. While Armantier et al. (2015) incentivize participants indirectly through financial decisions tied to inflation outcomes via a multiple choice list of lotteries, 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.[3] 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, we provide marginal incentives to all participants. Additionally, we directly incentivize updating in our study 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 expect significantly less price volatility on average (means of point expectations fall from 6.1% without marginal incentives to 2.7% with such incentives) and 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, 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 significantly strong that estimated learning rates under marginal incentives led to a qualitatively different conclusion

[3] Incentives in Armantier et al. (2015) involve subjects choosing between two fixed investment options (inflation-sensitive or flat payout) in a multiple price list with 10 lotteries. The authors lose around a sixth of participants due to non-rationalizable behavior (i.e., multiple switching points). Such list elicitation methods typically yield non-rationalizable behavior in 10% to 25% of respondents (Bruner 2011), suggesting they may be complex.

(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. 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 can be used to provide estimates of the proportion of the measurement error due to the absence of marginal incentives.

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 practical 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.[4]

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-5) and posteriors as probabilistic forecasts (see Figure A-11). Between these measures, participants received a summary of

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

the Federal Open Market Committee's most recent inflation expectations, including median forecasts for 2024 and 2025 and corresponding range forecasts (see Figure A-8). 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. 2017, Bruine de Bruin et al. 2010). 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.

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 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. 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

[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 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 3.3) 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.

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 participants 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	Unincentivised	Unincentivised
Prior	Incentivised	Unincentivised
Post	Unincentivised	Incentivised
Both	Incentivised	Incentivised

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.

2.1 Hypotheses

Before moving onto results, we offer two hypotheses regarding the impact of marginal incentives in our experiment:

- **Survey-Based Beliefs**: Marginal incentives applied to prior inflation expectations will lead to significantly different distributions of elicited inflation data. More specifically, we anticipate marginal incentives leading to distributions featuring lower mean inflation expectations and lower variance in expectations. Further, we predict marginal incentives will lead to significantly reduced forecast errors.

- **Learning Rates**: In the context of the RCT, we expect that marginal incentives will increase the learning rates, meaning that participants will adjust their beliefs more substantially and consistently in response to the information provision compared to those without incentives.

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 in experimental economics (Smith 1976).

2.2 Data

We collect 1000 observations - 250 per treatment - from US residents via Prolific on September 14, 2024. 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 elicited expectations, highlighting that incentivized expectations become more consistent 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.

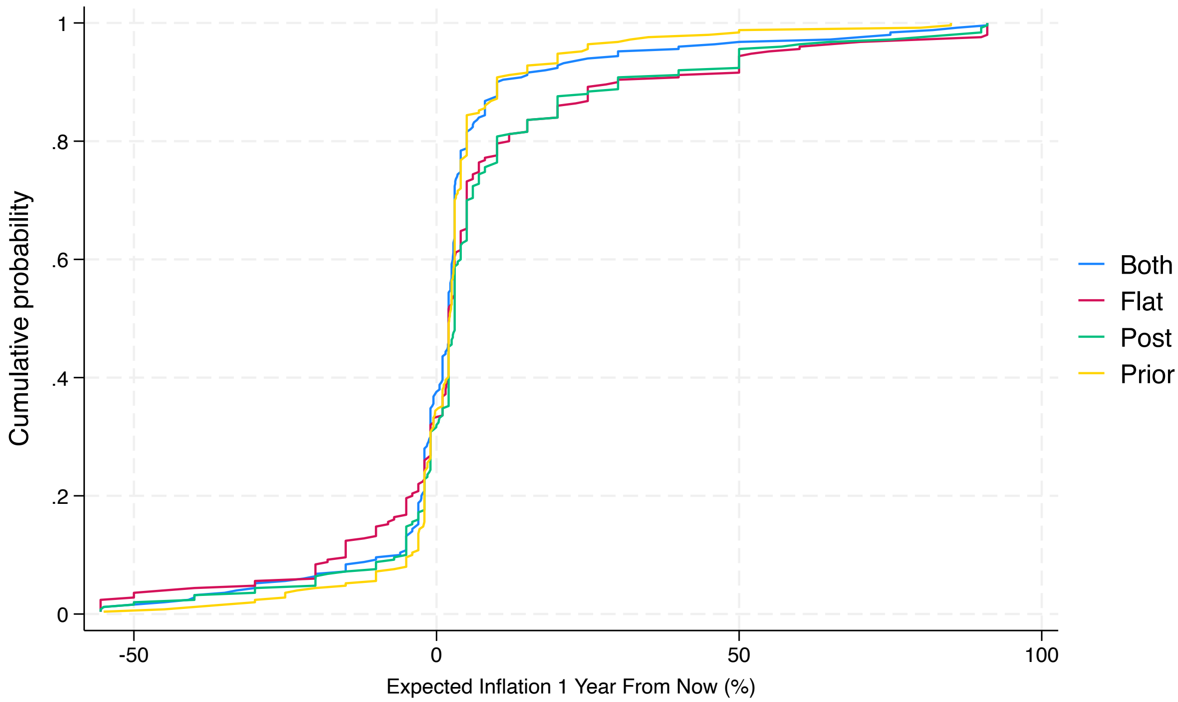
3.1 The Effect of Incentives on Elicited Expectations

We first consider whether marginal incentives influence respondents' one-year-ahead inflation expectations, 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.

Our results show that imposing marginal incentives when eliciting macroeconomic beliefs (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 aggregate price volatility. 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

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.

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 more 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

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*.

are equal ($p - values < 0.001$). Thus, imposing marginal incentives reduces the mean of point inflation expectations and leads to less cross-sectional forecast disagreement.

To quantify the effect of incentives more rigorously, 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.

Our hypothesis, which we detail 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 expectations of price volatility, compared to the unincentivized *Flat* treatment. Specifically, respondents in the *Prior* group expect, on average, about half the inflation volatility compared to those in the *Flat* treatment ($p < 0.001$). The effect in *Both* is somewhat less pronounced but still substantial, with expected price volatility reduced by about a third relative to *Flat* ($p < .01$). These effects are robust to controlling for the expected direction of price change (column (2)), for a participant's sex (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)).

Consequently, two critical questions arise: What are we truly measuring under each incentive structure, and which design most consistently reflects the genuine beliefs we aim to capture? The fact that incentives can so dramatically reshape expectations raises concerns about the

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)
Sentiment	No	No	No	No	Yes
Constant	15.20****	16.86****	20.56****	21.56****	16.65****
	(1.411)	(2.179)	(2.422)	(2.443)	(2.326)
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.

validity of survey-based belief measures and the inferences drawn from such data. If belief elicitation is highly sensitive to the incentive structure, the reliability of conclusions based on these beliefs becomes questionable.

We compare data from each of our treatments to the most recent SCE microdata and the mean PCE forecast from the Survey of Professional Forecasters (SPF) in Table 4. While expectations under *Flat* (5.515%) closely track those from the SCE (6.178%), implementing marginal incentives in *Prior* leads to expectations (2.804%) 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

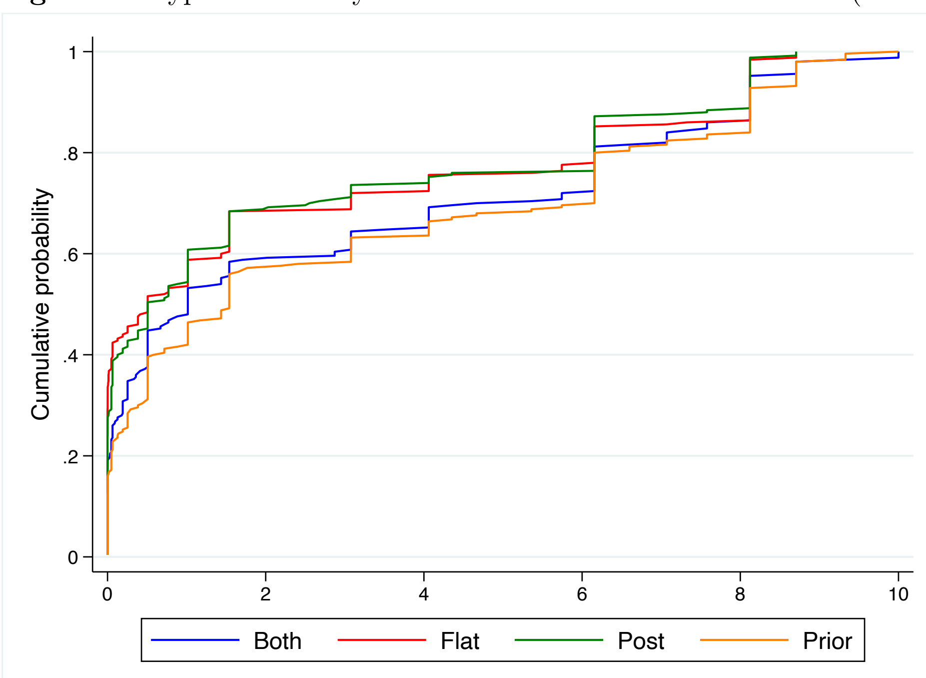
Table 4: Comparing Experimental Data to SCE and SPF

Comparison	Group 1 Mean (Std. Dev.)	Group 2 Mean (Std. Dev.)	Difference	t-statistic	*p*-value
SCE vs. Flat	6.178 (12.942)	5.515 (24.910)	-0.663	-0.598	0.550
SCE vs. Prior	6.178 (12.942)	2.804 (14.313)	-3.374	-3.648	$< .001$
SPF vs. Flat	2.11 (0.286)	5.515 (24.910)	3.405	.796	0.427
SPF vs. Prior	2.11 (0.286)	2.804 (14.313)	0.694	0.966	0.778

Notes: This table compares data from SCE participants (first two rows) and from the Survey of Professional Forecasters (rows 3 and 4) to data from participants in *Flat* and *Prior* using two-sample t-tests. We restrict our focus to inexperienced SCE participants from the most recently-available microdata from September of 2023 when inflation was reported at 3.7%. For comparison, the most recent inflation report preceding our experiment was 2.9% (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=34) is considerably smaller that those of the SCE and our survey.

the distribution of payoffs calculated this way across treatments in Figure 2 and explore the significance of these results in Table A-1. The punchline is that marginal incentives significantly increase hypothetical earnings. In *Both*, we predict in Table A-1 that payoffs will increase between approximately 24% ($p < .1$) in our baseline regression specification and 34% ($p < .05$) in a specification controlling for sex, education, and economic sentiment. In *Prior*, hypothetical earnings increase between 33% ($p < .01$) in our baseline specification and 49% in our full specification.

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.

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.[7] Concisely, female survey participants typically report significantly higher inflation expectations than do 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.

[7] 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)	(2)	(3)	(4)	(5)
	(Flat)	(Post)	(Both)	(Prior)	(All)
	$\mathbb{E}(\pi_{Prior})$	$\mathbb{E}(\pi_{Prior})$	$\mathbb{E}(\pi_{Prior})$	$\mathbb{E}(\pi_{Prior})$	$\mathbb{E}(\pi_{Prior})$
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 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.[8] Recall that we adopt the same point and bin elicitation strategies followed by the NY Fed in its Survey of Consumer Expectations.[9]

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

[8] 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-2. 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 binned inflation forecasts exhibit less disagreement and a lower mean expected inflation than point forecasts.

[9] 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.

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.[10]

Table 6: 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)
Sentiment	No	No	No	Yes
Constant	0.0185	0.102	0.0756	0.485**
	(0.177)	(0.200)	(0.218)	(0.246)
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.

[10] 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.

We show results from estimating various versions of Equation (2) in Table 6, 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. This 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.

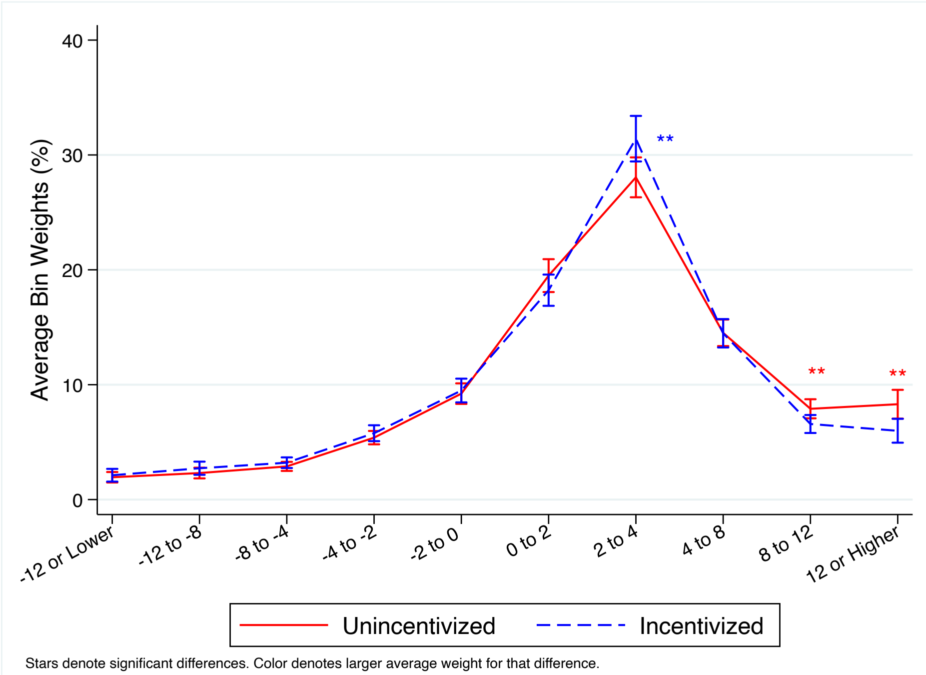
Our analysis demonstrates that implementing marginal incentives significantly enhances belief updating among subjects. Specifically, the positive and statistically significant coefficient for the interaction term ($\beta_{1,Post}$, $\beta_{1,Both}$) indicates that incentives designed to promote belief accuracy significantly amplify the magnitude of belief updating in response to discrepancies between their prior beliefs and the Federal Reserve's forecasts. This suggests that strategic incentives can effectively bridge perception gaps, leading to more consistent and responsive belief formation. More importantly, this suggests that RCTs implemented without marginal incentives may systematically underestimate the impact of information on beliefs. 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.

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.

3.3 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.

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.

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)).[11]

[11] We show the same results without winsorizing in Table A-3, located in appendix A1.

We report regression results in Table 7.

Table 7: 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)
Sentiment	No	No	No	Yes
Constant	567.4****	599.0****	625.0****	585.9****
	(18.39)	(19.88)	(21.97)	(25.59)
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.

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 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 more hypothetically accurate inflation expectations, with respondents predicting lower price volatility 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 under incentivized elicitation mechanisms. 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.

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 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 complex 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 and of economic expectations and informing more effective policy decisions.

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Appendix

A1 Other Tables and Figures

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

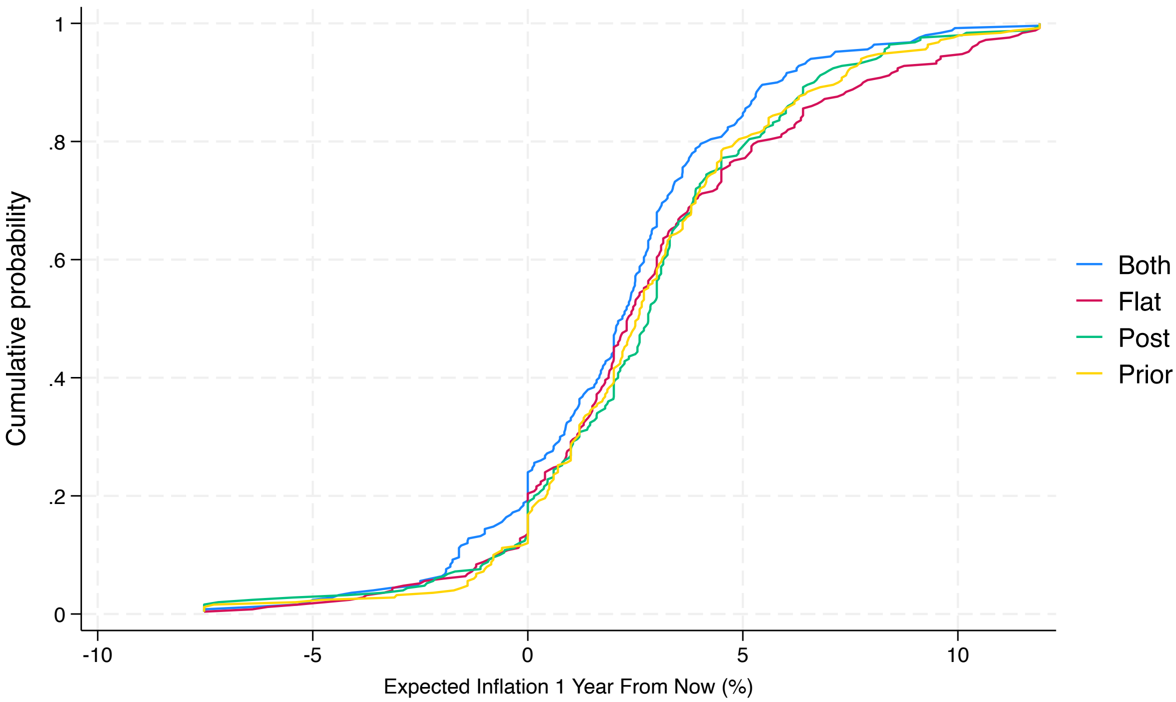
	(1)	(2)	(3)	(4)
	Payoff	Payoff	Payoff	Payoff
Post	-0.0606	-0.110	-0.150	-0.197
	(0.270)	(0.264)	(0.264)	(0.262)
Both	0.545*	0.644**	0.626**	0.613**
	(0.281)	(0.277)	(0.277)	(0.275)
Prior	0.755***	0.867***	0.880***	0.874***
	(0.282)	(0.275)	(0.275)	(0.274)
Male		1.127****	1.096****	1.112****
		(0.202)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

	(1)	(2)	(3)	(4)	(5)
	$\mathbb{E}(\pi_{Post.})$	$\mathbb{E}(\pi_{Post.})$	$\mathbb{E}(\pi_{Post.})$	$\mathbb{E}(\pi_{Post.})$	$\mathbb{E}(\pi_{Post.})$
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)
Sentiment	No	No	No	No	Yes
Constant	4.571****	4.613****	4.759****	4.971****	4.681****
	(0.171)	(0.175)	(0.192)	(0.207)	(0.216)
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-3: 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)
Sentiment	No	No	No	Yes
Constant	572.3****	607.2****	636.4****	590.7****
	(19.99)	(21.76)	(24.28)	(27.78)
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-4: 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:

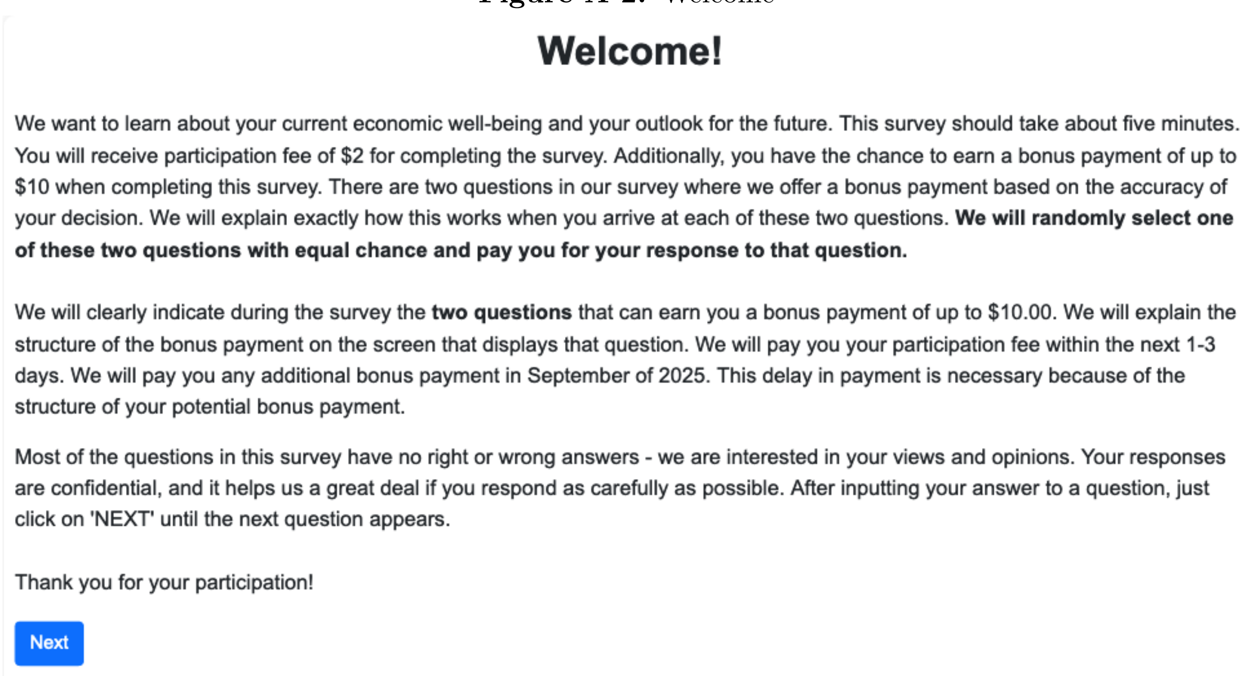
- **Small effect size:** $d = 0.2$
- **Medium effect size:** $d = 0.5$
- **Large effect size:** $d = 0.8$

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. Figures reflect the *Both* treatment group to showcase all possible incentivized questions. Screenshots of other treatments are available upon request.

Figure A-2: 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

Figure A-3: 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-4: 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.

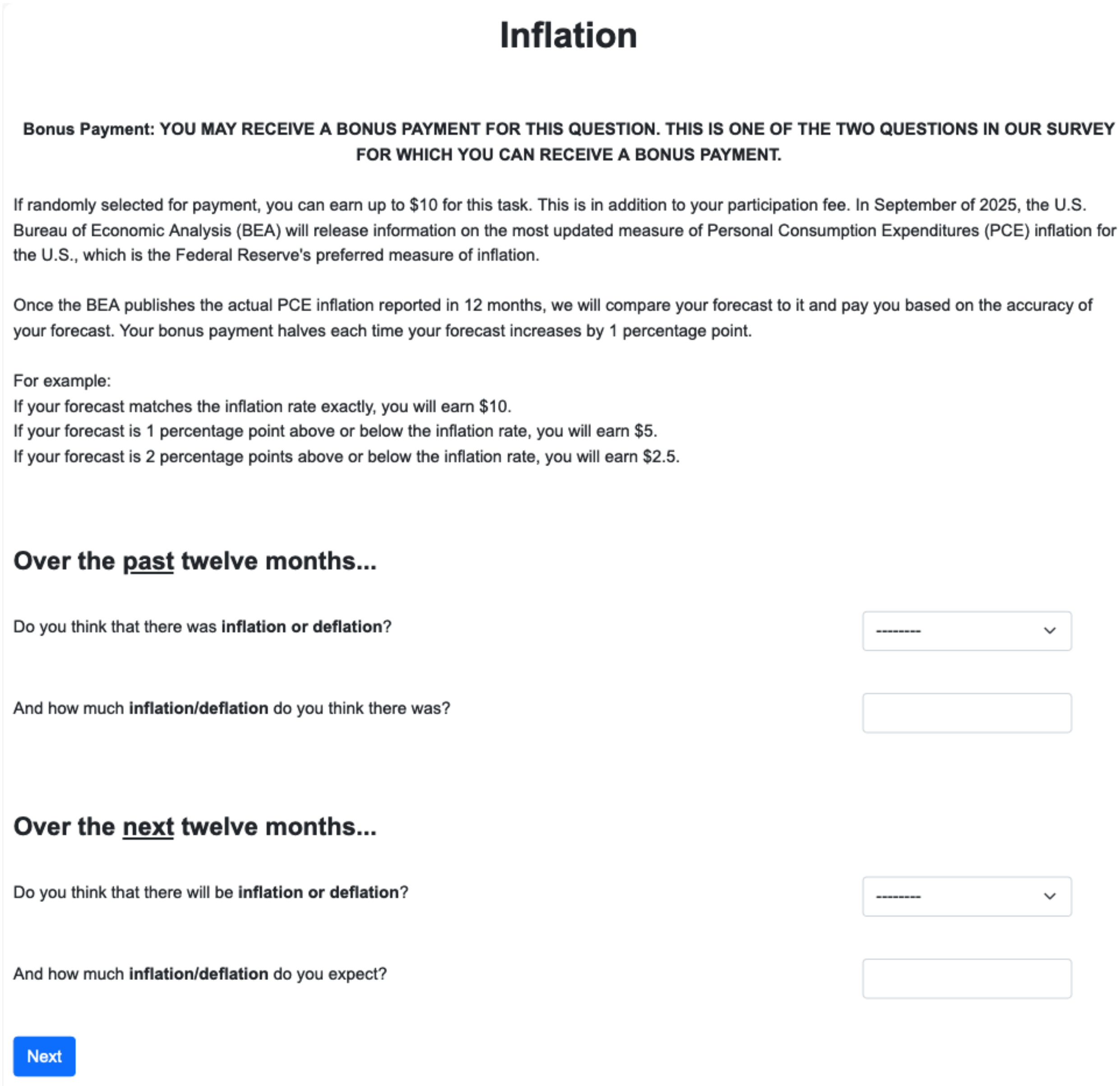
In some of the following questions, we will ask you to think about the percent chance of something happening in the future. Your answers can range from 0 to 100, where 0 means there is absolutely no chance, and 100 means that it is absolutely certain.

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-5: Inflation Point Forecast



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-6: Food Point Forecast

Food Prices

Over the <u>past</u> 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 <u>next</u> 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-7: Gas Point Forecast

Gas Prices

Over the <u>past</u> 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 <u>next</u> 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-8: 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-9: 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-10: 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-11: Inflation Bin Forecast

Inflation

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

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

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 August 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-12: 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