Rounding Up: Complexity, Satisficing, and Bias in Inflation Expectations*

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This Draft: April 2026

Abstract

Using over 17,000 incentivized inflation forecasts, we provide causal evidence that environmental complexity and subjective complexity are distinct drivers of rounding in survey responses. Experimental variation in shock volatility and central-bank communication regimes shows that both channels raise forecast uncertainty and the propensity to round, with subjective complexity the dominant force — explaining 58–86% of rounding depending on horizon and specification. Survey of Consumer Expectations microdata corroborate these findings: rounding declines with survey tenure, rises with inflation volatility, and inflates measured inflation expectations by nearly 7 percentage points among inexperienced respondents.

Keywords: Rounding, Expectation formation, Uncertainty

JEL Classification: C91, D84, E52

*McMahon gratefully acknowledges financial support from the European Research Council (Consolidator Grant Agreement 819131). Petersen thanks the Social Sciences and Humanities Research Council of Canada for generous financial support from Insight Development and Insight Grant programs. The studies received IRB approval from Simon Fraser University (#2012s0755). The views presented in the paper are those of the authors alone and do not represent the official views of anyone else. Any errors remain ours alone.

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1 Introduction

Heaping of answers at rounded or salient numbers is a pervasive feature of open-ended quantitative survey responses and is often interpreted as *satisficing* in the sense of respondents providing a "good enough" answer rather than an exact one (Simon 1956). Krosnick (1991) suggests that the probability that individual i satisfices in task τ is increasing in task difficulty ($D_{\tau,i}$), and decreasing in motivation (M_i) and ability (A_i). In economic surveys, such heaping behavior is sometimes attributed to greater difficulty arising from uncertainty about the underlying economic environment (Binder 2017), while other work shows that rounding reflects respondent-side limitations, including low ability or low financial confidence (Gideon et al. 2017, Reiche 2025). We refer to these potential drivers of rounding as *environmental complexity* – the inherent unpredictability of the forecasting environment – and *subjective complexity* — the cognitive burden imposed on the respondent by the task itself, which may be reduced by providing informational anchors. Both operate through the channel of individual izz's reported forecast uncertainty, though they need not do so exclusively.

To illustrate the distinction between them, consider an inflation-forecasting task in which a participant predicts next month's inflation. The task may be difficult because inflation is stochastic (*environmental complexity*),[1] or because forming a forecast requires recalling and filtering large amounts of information which respondents may find demanding (*subjective complexity*). Faced with these complexities, people may report a rounded value rather than compute a more precise figure. Providing an anchor, such as a communicated point forecast, can reduce this respondent-side difficulty, while less volatile economic conditions also helps. In practice, both forces can operate simultaneously, so observed rounding in inflation survey responses may reflect cognitive burden, unpredictability about future inflation, or both.

Using more than 17,000 incentivized, individually linked observations of inflation expectations and forecast uncertainty from an experiment that systematically varies environmental complexity and subjective complexity, we show that each mechanism has a large and distinct causal effect on rounding behavior. Our inflation-forecasting experiments introduce exogenous variation in each channel: we vary environmental complexity by changing shock volatility (including outlier shocks) in the demand-shocks process, and we vary subjective complexity by changing whether and how a central bank communicates its inflation outlook, which systematically affects the cognitive burden of forming a forecast.

We find that exogenous shocks to environmental complexity and subjective complexity both causally increase participants' expressed uncertainty and the probability that they round their inflation forecasts. Respondents satisfice more when the forecasting environment is more complex and when the forecast-formation task is more cognitively demanding (i.e. subjective complexity is higher). When estimated jointly, both channels remain distinct. A Shapley decomposition shows that subjective complexity explains between 58% and 69% of explained rounding at the shorter forecast horizon (t+1), rising to between 75% and 86% at the longer forecast horizon (t+2), with the precise share depending on whether environ-

[1] More generally, real-world forecasters also face uncertainty about the parameters governing the inflation process, although this is not a source of uncertainty in our experimental design.

mental complexity is measured at the sequence or period level. In both cases, environmental complexity remains a meaningful contributor, particularly at shorter horizons.

These findings establish that both drivers of rounding matter, but that respondent-side subjective complexity is the dominant channel, particularly at longer forecast horizons. We next examine whether this mechanism also helps explain a well-known pattern in survey data. In the FRBNY's Survey of Consumer Expectations, respondents repeatedly complete the same inflation-forecast elicitation task over time, and Kim and Binder (2023) show that both reported inflation expectations and inflation forecast uncertainty decline with survey tenure. Our experimental results suggest a simple mechanism: as repeated exposure reduces the subjective complexity of the task, respondents become less uncertain and less likely to round their forecasts. Since rounding mechanically imparts upward bias to reported inflation expectations, a decline in rounding over tenure would mechanically compress that bias and thereby help account for the decline in average reported expectations.

We show that most of the decline in expectations documented by Kim and Binder (2023) is driven by a reduction in rounding by respondents as their tenure increases. In the SCE, new respondents round 45% of the time, compared with 30% after ten months, a 14-percentage-point drop. Rounding inflates reported expectations: in their first month, rounders report forecasts that are approximately 6.9 percentage points higher than non-rounders. This rounding premium declines by roughly 3.8 percentage points over the first year of participation, while the corresponding decline for non-rounders is economically small and statistically insignificant. Thus, the large majority of the tenure-based decline in average inflation expectations reflects reduced rounding-induced bias. This upward bias implies that standard cross-sectional estimates of inflation expectations disproportionately reflect the responses of inexperienced, high-complexity respondents, with important implications for how researchers and policymakers interpret household expectations data. Further, we show rounding is not a benign reporting artifact: in both our experimental data and in the SCE, rounders produce systematically larger forecast errors, even after conditioning on their own reported forecast uncertainty. These errors are quantitatively meaningful, ranging from roughly 7 to 10 basis points in our experimental data and 4.5 to 6 percentage points in the SCE.

Our paper sits at the intersection of three related literatures. First, for the overall survey-expectations context, Fuster and Zafar (2022) provides a review. Second, a large literature links satisficing and heaping to task complexity and cognitive burden, including foundational work in survey methodology (Krosnick et al. 1996, Krosnick 1999), evidence in economic survey settings (Gideon et al. 2017), and related discussions in psychology (Oppenheimer et al. 2009, Mertens 2019), economics (Caplin et al. 2011, Artinger et al. 2022, Da Silveira and Lima 2022), and higher education research (Barge and Gehlbach 2012). This perspective also connects to recent work on complexity and decision-making (Deck and Jahedi 2015, Oprea 2020, Banovetz and Oprea 2023, Gabaix and Graeber 2024) and procedural complexity (Arrieta and Nielsen 2023). Third, Binder (2017) formalize an uncertainty-based interpretation of rounding and develop a method for inferring uncertainty from rounded responses that is now widely used in macroeconomic expectations research.

Our results align with the limited experimental evidence linking uncertainty and heaping.

Ruud et al. (2014) exogenously increase uncertainty by varying the difficulty of a color-detection task and show that harder tasks produce more heaping. However, environmental and subjective complexity move together in their design preventing a decomposition of the competing channels. Huttenlocher et al. (1990) show that recall tasks generate heaping as respondents round event dates to salient temporal anchors (7, 14, and 30 days), with more rounding for events further in the past; this mechanism is plausibly driven by memory load rather than uncertainty about the underlying event. Khaw et al. (2017) document a related preference for round numbers in laboratory probability-forecasting tasks. We differ from these papers in two key respects. First, we examine a forecasting task, which aligns more closely with how researchers and policymakers elicit economic expectations and forward-looking beliefs in surveys. Second, our experimental design allows us to identify the distinct causal effects of environmental complexity and subjective complexity on rounding and to decompose their contributions to observed heaping.

The rest of the paper is structured as follows. Section 2 describes the experimental environment and the data. In Section 3, document the empirical relationship between uncertainty and rounding in the experimental data, and estimate the causal effects of environmental and subjective complexity on rounding. Section 4 presents the SCE evidence on tenure, rounding, and inflation expectations. Section 5 concludes.

2 Experimental Design

2.1 Conceptual Framework

We study the drivers of rounding in survey responses, with a particular focus on the relationship between uncertainty and rounding, and on how different forms of task complexity shape both. The context of our study is individuals' inflation expectations, a canonical setting in macroeconomics in which respondents commonly round their responses. Figure 1 summarizes our conceptual framework.

Let $\mathbb{E}_{i,t}[\pi_{t+h}]$ denote participant i's point expectation of inflation at horizon h. Let $R_{i,t+h} \in \{0,1\}$ indicate whether individual i rounds their point forecast for horizon $h \in \{1,2\}$ at time t – we will define this variable more formally below. Let $U_{t+h,i}$ denote individual-level forecast uncertainty at horizon h. Our hypothesis, as in many other papers discussed above, is that the likelihood of rounding a response, $P(R_{i,t+h} = 1)$, is increasing in $U_{t+h,i}$.

Our specific contribution is to distinguish between two broad forms of task complexity which may drive participant uncertainty. As described above, environmental complexity ($C^E_{\tau,i}$) captures the causes of uncertainty caused by the task being inherently more difficult for participants such as because the underlying economic environment being more volatile. Subjective complexity ($C^S_{\tau,i}$) is about factors that make the task more difficult for a given participant such as their own lack of confidence answering on the topic they have to answer on. Under our maintained hypothesis $U_{t+h,i}$ is increasing in both $C^E_{\tau,i}$ and $C^S_{\tau,i}$ and by varying these

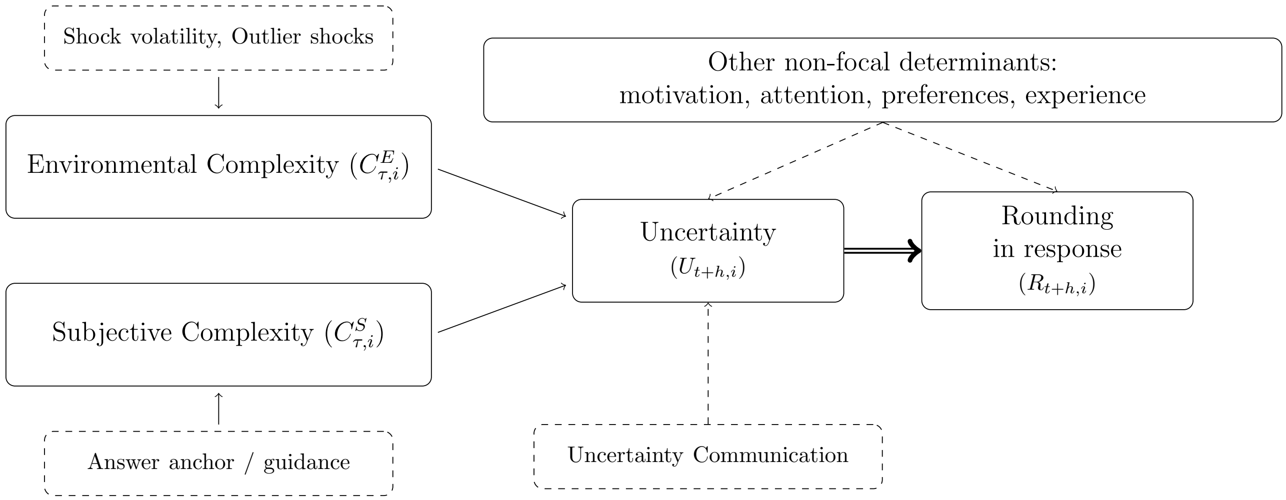


Figure 1: Conceptual framework.

two complexities experimentally, we can understand their role in uncertainty and ultimately rounding.

Note that we do not assume that environmental ($C^E_{\tau,i}$) and subjective ($C^S_{\tau,i}$) complexity are the only determinants of reported forecast imprecision or rounding. Motivation, attention, and other respondent characteristics may also affect both outcomes; even ability may affect these through subjective complexity, or directly. Owing to the random assignment of experimental treatments in a common incentivized environment, these other determinants are orthogonal to variation in complexity and therefore should not confound the causal effects we estimate. They may, however, contribute to residual variation in imprecision and rounding levels.

We will empirically study both reported uncertainty ($U_{t+h,i}$) and rounding behavior ($R_{t+h,i}$). This two-outcome design allows us to estimate (i) the total causal effects of environmental and subjective complexity on rounding, and (ii) mechanism-consistent effects on reported forecast imprecision, without imposing that reported forecast imprecision is the only pathway through which complexity affects rounding.

2.2 The Experimental Data

We study rounding using data from two closely related laboratory and online experiments reported in Rholes and Petersen (2021) and Petersen and Rholes (2022) (RPU hereafter). These experiments were originally designed to investigate how higher-order information in central bank communication shapes forecast formation. Their structure is ideally suited to our purposes: they generate large numbers of repeated, incentivized inflation forecasts from the same individuals, elicit subjective uncertainty at multiple horizons, and expose forecasters to controlled variation in the complexity of the forecasting task.

Across the two experiments, we observe 17,328 individual forecasting decisions from 293 unique subjects, collected in 54 sessions conducted between October 2019 and October 2021 at Texas A&M University and Simon Fraser University. Sessions were run both online and in physical laboratories. The interface was programmed in Redwood, and subjects were recruited through ORSEE (Greiner 2015) and SONA.

Participants acted as inflation forecasters in experimental economies governed by a linearized, three-equation New Keynesian (NK) model simplified to eliminate the need for expectations of the output gap. This yields a dynamical system closed under subjects' one- and two-period-ahead inflation expectations. Each session consisted of seven subjects who formed individual forecasts privately using common information for two independent repetitions of 30 periods each. Economies evolved according to persistent aggregate demand shocks and the aggregate inflation expectations formed by subjects in each period. In every period t, subjects submitted incentivized point forecasts for inflation in periods $t+1$ and $t+2$ and provided incentivized measures of their subjective forecast uncertainty for each horizon.[2]

Demand shocks followed an AR(1) process. For each 30-period repetition, the experimenter randomly assigned one of six pre-drew shock sequences at session level. This generates exogenous, session-level $\times$ repetition variation in shock volatility, which forms the basis for our shock-driven complexity measure used in the analysis.

At the start of each period, subjects observed past inflation, interest rates, shocks, their own past forecasts, and the current shock realization. When applicable, they also viewed the central bank's five-period-ahead inflation forecast, which varied by treatment: *NoComm* provided no projections, *Point* provided only a point forecast, *Point&Density* provided both point and density forecasts, and *DensityOnly* provided only a density forecast. These communication regimes induced experimental variation in task complexity (discussed in detail in subsection 2.1) and subjective uncertainty, which we exploit to quantify their distinct contributions to rounding behavior.

We incentivized point forecasts using a symmetric scoring rule that penalized absolute forecast error. A perfect forecast earned a fixed maximum payoff, and each one–percentage-point increase in forecast error reduced that payoff by half.[3] After submitting their point forecast, subjects provided a range forecast for inflation. This range had to contain the previously reported point forecast but was otherwise unrestricted. Range forecasts were incentivized using the piecewise scoring rule introduced in Rholes and Petersen (2021). If realized inflation fell outside the subject's range, they earned zero. If it fell inside, they received a positive payoff that decreased with the width of the range, thereby discouraging overly wide intervals.

After all forecasts were submitted, the software aggregated individual expectations by taking the median point forecast for each horizon, fed these aggregates into the NK model, revealed realized inflation and interest rates, and advanced to the next period.[4]

[2]See Appendix B for the experimental instructions and B.1 for screenshots of the experimental interface.

[3]Drobot et al. (2026) show this scoring rule outperforms other, more complex scoring rules for eliciting beliefs about future inflation.

[4]See Petersen and Rholes (2022) for a discussion of how simple averaging rules can generate unrealistically unstable inflation dynamics.

2.3 Experimental Variation

We propose that both higher environmental complexity (C^E_τ) and higher subjective complexity ($C^S_{\tau,i}$) raise the effective difficulty of the forecasting task, increasing the likelihood that an individual satisficies and reports a rounded forecast. Our empirical objective is to estimate the causal effects of these two channels on rounding and to assess the extent to which their effects operate through reported forecast imprecision. To do so we generate exogenous variation in environmental complexity and subjective complexity as follows:

1. ***Variation in Environmental Complexity (C^E)***

   (a) **Demand-shock sequences:** Across repetitions of the RPU environment, subjects face different realizations of the exogenous demand-shock process. Sequences with higher volatility widen the range of plausible inflation paths and make the forecasting environment more difficult. This between-sequence variation provides an exogenous source of environmental complexity.

   (b) **Outlier shocks:** As a robustness check, we also construct a period-level measure based on unusually large demand shocks (e.g., absolute shocks exceeding one within-sequence standard deviation). This finer measure relies on within-sequence variation and captures transient spikes in environmental complexity.

2. ***Variation in Subjective Complexity (C^S)***

   Another potential driver of rounding is that respondents may lack a clear sense of what a reasonable answer is. Someone who does not follow inflation developments closely may know that inflation is usually between 0% and 10%, yet have little basis for judging whether 2.2% or 3.4% is more appropriate in a particular case. In such situations, respondents may satisfice by reporting a rounded value such as 5%. Guided by this mechanism, the main dimension of subjective complexity we vary in our experiment is the degree to which respondents are given a plausible anchor for their answers.

   As described briefly above, there are four communications types:

   (a) **No CB communication (*NoComm*)**: Participants receive no guidance from the central bank about its outlook for inflation and therefore there is no answer anchor for these participants.

   (b) **CB Point Forecast (*Point*)**: Participants receive a five-period-ahead *point* forecast from the central bank, giving them an anchor of reasonable forecast values.

   (c) **CB Density Forecast (*DensityOnly*)**: Participants receive the central bank's five-period-ahead forecast (as in *Point*), but only as a density. Without the anchor effect of a point forecast, this communicates both a range of values that might be reasonable but also that the central bank is itself uncertain about outcomes.

   (d) **CB Point and Density Forecast *Point&Density***: Participants receive the central bank's point forecast as well as the distribution around it.

The experimental variation in communication yields two main specifications. First, we compare how adding the answer anchor reduces uncertainty and also rounding, compared to any of the other communication media (*Point* vs Others). Then we compare between the other variations to see the effects at a more granular level.

2.4 Classifying Rounding

We now turn to a definition of what it means for a forecast to be rounded in our experiments. Because rounding is inherently unit dependent, we define it in terms of basis points (bps). This is because our participants submit integer-valued forecasts in basis points for simulated economies with steady-state inflation equal to zero and mean-zero i.i.d. shocks. Thus, a participant expecting inflation to be 0.5 percentage points above steady state would report a forecast of 50. Figure 2 plots the distribution of reported forecasts. Although the vast majority of responses at both horizons lie below 100 basis points, the distribution still exhibits clear heaping at salient values such as 5, 10, 20, and 50 basis points.

We classify a forecast $\mathbb{E}_{i,t}[\pi_{t+h}]$ as rounded at the x-basis-point level if the reported forecast in basis points is exactly divisible by x. Formally, for any threshold x, we define

$$R^x_{i,t,h} = \mathbf{1}\{\mathbb{E}_{i,t}[\pi_{t+h}] \bmod x = 0\}. \tag{1}$$

This unit-based definition matters for interpreting rounding in our experimental environment. Measures commonly used in survey settings where respondents report inflation in percentage points are not appropriate here. For example, Binder (2017) define rounded inflation forecasts as those evenly divisible by five percentage points, such as 5%, 10%, and so on. In our setting, however, that threshold would correspond to 500 basis points and would therefore classify essentially no forecasts as rounded. Because participants report in integer basis points and nearly all forecasts lie well below that level, the relevant heaping thresholds in our data are much smaller.

Instead, we discipline our choice of threshold of rounding using empirical prevalence rather than numerical units. Using data from the Michigan Survey of Consumers and the New York Fed Survey of Consumer Expectations, Binder (2017) documents that approximately 44% of inflation forecasts are rounded using her five percentage point threshold. Table 1 shows that defining rounding as forecasts divisible by 10 basis points yields a closely comparable prevalence in our experimental data ($\approx 48\%$), whereas a finer threshold (5bps) classify a majority of forecasts as rounded and coarser thresholds (20bps or 50bps) yield far fewer instances of rounding. Our baseline definition of rounding, therefore, uses $x = 10$ in Equation 1 (10 basis points) as it yields a prevalence comparable to that observed in survey data.[5]

[5]We show in the appendix that our main results are robust to this specification. For example, A.1 shows that the relationship between forecast uncertainty and rounding holds for finer and more coarse definitions of rounding. We also show that results are robust to excluding forecasts of zero from our rounding definitions ().

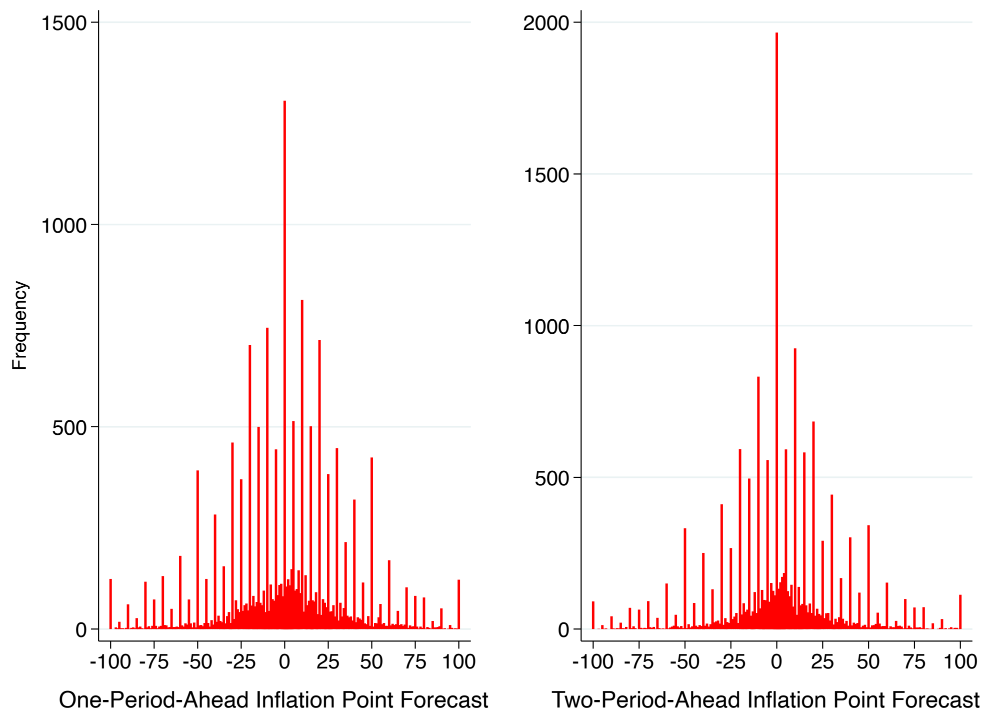


Figure 2: Distributions of inflation point forecasts.

Notes: Histogram distribution of individual-level one-period-ahead (left) and two-period-ahead (right) inflation forecasts.

Table 1 also documents how subjective uncertainty, $U_{t+h,i}$, varies across rounding definitions. As the rounding threshold becomes coarser, the proportion of forecasts classified as rounded declines sharply, while the average level of subjective uncertainty among rounded forecasts rises monotonically at both horizons. This pattern indicates that coarse rounding definitions disproportionately select high-uncertainty observations.

3 Results

We now explore the effect of our experimental variation on the outcomes of interest. For each horizon $h \in \{1, 2\}$ we estimate

$$Y^{(h)}_{i,s,t,r} = \alpha_{h,g} + \beta_{h,g} \times D(Treatment)_{i,t,h} + \lambda_t + \rho_r + \kappa_{covid} + \varepsilon^{(h)}_{i,s,t,r}. \quad (2)$$

That is, for each forecast horizon separately, we estimate regressions of the individual level outcomes ($Y_{i,t,h}$) on variables capturing our experimental variation ($D(Treatment)_{i,t,h}$) as well as controls. All specifications include period fixed effects (λ_t); where appropriate we add repetition fixed effects (ρ_r) to capture when subjects are doing their first or second stint working through a version of the forecasting task, and a COVID-era indicator (κ_{covid}). In the regressions, $g \in \{\text{Rep 1}, \text{Rep 2}, \text{Overall}\}$ indexes the sample and standard errors are

Table 1: Incidence of Rounding and Subjective Uncertainty by Rounding Definition

	Horizon $h = 1$		Horizon $h = 2$	
Rounding def.	Proportion rounding	Mean uncertainty	Proportion rounding	Mean uncertainty
5 bps	0.707	25.24	0.717	29.48
10 bps	0.482	27.06	0.501	32.18
20 bps	0.256	28.17	0.279	33.54
50 bps	0.151	31.41	0.182	38.06

Notes: "Proportion rounding" reports the proportion of forecasts classified as rounded under each definition. "Mean uncertainty" reports the mean subjective uncertainty (absolute expected forecast error) among rounding observations. There are 17,328 decisions total from 293 unique subjects. Uncertainty, expressed in basis points, increases monotonically as rounding definitions become coarser at both horizons.

clustered at the individual level. When $Y_{i,t,h}$ is a dummy variable, equation (2) is a linear probability model (LPM).

We include a COVID-era indicator to absorb across-session variation in the broader macroeconomic information environment. Period fixed effects control for common shocks across rounds within a given experimental session, but sessions were conducted over calendar time, including during the pandemic. Following Petersen and Rholes (2022), this matters because pandemic conditions plausibly shifted subjects' baseline uncertainty and their responsiveness to central-bank communication. Controlling for the COVID era therefore helps isolate the effect of our experimental variation from changes in the background uncertainty environment across sessions.

3.1 Relationship between Subjective Uncertainty and Rounding

We begin by exploring a simple correlation implied by the existing literature and taken as given in our conceptual framework: respondents are more likely to round their point forecasts when they report greater forecast uncertainty, $U_{t+h,i}$. Table 2 documents this relationship by regressing the 10bps rounding indicator on individual-level range width (standardized), separately by forecast horizon. These baseline regressions are purely descriptive and meant to establish that the link between forecast uncertainty ($U_{t+h,i}$) and rounding ($R_{t+h,i}$) exists in the experimental data.

We consider two versions of standardization of reported uncertainty. In Panel A, uncertainty is standardized within subject, so the coefficient is identified from within-person variation: it captures whether a respondent is more likely to round in periods when their reported uncertainty is high relative to their own typical level. In Panel B, uncertainty is standardized globally (across all observations), so the coefficient combines within-person and between-person variation: it captures both whether respondents round more when they are more uncertain than usual and whether respondents in environments with systematically higher uncertainty also round more on average. For this reason, the coefficients in Panels A and

	(1) $R_{i,t+1}$	(2) $R_{i,t+1}$	(3) $R_{i,t+1}$	(4) $R_{i,t+2}$	(5) $R_{i,t+2}$	(6) $R_{i,t+2}$
Panel A: Within-Subject						
U_{t+1}	0.046***	0.049***	0.040***			
	(0.005)	(0.005)	(0.005)			
U_{t+2}				0.039***	0.042***	0.033***
				(0.005)	(0.005)	(0.005)
Time FE	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Observations	17,154	17,154	16,769	17,268	17,268	16,514
Adj. R^2	0.008	0.013	0.027	0.006	0.011	0.028
Panel B: Global						
U_{t+1}	0.079***	0.080***	0.072***			
	(0.014)	(0.015)	(0.014)			
U_{t+2}				0.103***	0.106***	0.097***
				(0.013)	(0.013)	(0.013)
Time FE	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Observations	17,328	17,328	16,937	17,328	17,328	16,572
Adj. R^2	0.025	0.029	0.040	0.043	0.049	0.060

Table 2: Rounding (10bps) and individual uncertainty.

Notes: Columns (1) and (4) are baseline specifications. Columns (2) and (5) add period, repetition, and post-COVID session fixed effects. Columns (3) and (6) additionally include lagged absolute forecast error at the corresponding horizon and inflation volatility, defined as the absolute change in inflation over the two most recently observable inflation outturns. Panel A standardizes uncertainty within subject; Panel B standardizes uncertainty globally (across all treatments, sequences, and repetitions). Estimates are LPM coefficients; robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B need not be numerically comparable one-for-one. Panel A is closer to a within-subject behavioral margin, while Panel B reflects the pooled predictive relationship in the full sample. The fact that both panels yield positive and highly significant coefficients indicates that the uncertainty-rounding relationship is present both within respondents and in the cross-sectional/panel variation of the experimental data.

Consistent with the maintained idea in the literature, higher forecast uncertainty is strongly positively associated with rounding at both horizons across specifications. A one-standard-deviation increase in $U_{t+h,i}$ is associated with a 3–5 percentage-point increase in rounding in the within-subject standardization (Panel A) and a 7–10 percentage-point increase under global standardization (Panel B).

3.2 Experimental Variation in Environmental Complexity

To implement the tests of the environmental-complexity channel directly, we use exogenous variation in C^E from the two sources described above:

- We use a dummy variable of $D(High\, C^E_{seq.}) = 1$ to capture *sequence-level* complexity when a subject faces a high-volatility shock path across the 30 periods of a sequence. For each repetition, there were six unique shock sequences and high-volatility shock paths are the three with the highest standard deviation within each repetition.
- To capture *period-level* complexity, we use large absolute magnitude of the realized shock in a given period. We standardize shocks within each repetition (and globally for the pooled specification) and define the high-complexity indicator that equals one when the realized shock in a given period exceeds one standard deviation from the sample mean. That is: $D(High\, C^E_{period}) = \mathbf{1}\left[|z_{s,t,r}| > 1\right]$, where $z_{s,t,r} = (shock_{s,t,r} - \bar{\mu})/\bar{\sigma}$ and $(\bar{\mu}, \bar{\sigma})$ are computed over the relevant sample.

For each horizon $h \in \{1, 2\}$ we then estimate:

$$Y^{(h)}_{istr} = \alpha_{h,g} + \beta_{h,g} D(High\, C^E_k) + \lambda_t + \rho_r + \kappa_{covid} + \varepsilon^{(h)}_{istr}, \tag{3}$$

where $Y^{(h)}_{istr}$ is either forecast uncertainty ($U_{t+h,i}$, Panel A) or the 10-bps rounding indicator ($R_{i,t+h}$, Panel B), $g \in \{\text{Rep 1}, \text{Rep 2}, \text{Overall}\}$ indexes the sample, and k denotes *seq* or *prd*. All specifications include period fixed effects (λ_t); where appropriate we add repetition fixed effects (ρ_r) and a COVID-era indicator (κ_{covid}). Standard errors are clustered at the individual level. For the pooled "Overall" specification, the dummy variables are still given by each sequence's within-repetition classification, but the analysis is done with observations from both repetitions.

Panel A of Table 3 shows that sequence-level complexity has a strong effect on uncertainty in the first repetition, where high-volatility assignments raise uncertainty by 0.27 at both horizons. However, this effect vanishes in repetition 2. Pooled estimates remain positive and

Panel A: Individual Forecast Uncertainty

	$U_{t+1,i}$			$U_{t+2,i}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
$D(High\,C^E_{seq.})$	0.272***	-0.004	0.134**	0.269***	-0.030	0.120**
	(0.089)	(0.103)	(0.054)	(0.094)	(0.103)	(0.060)
Adj. R^2 (sequence)	0.021	0.000	0.008	0.022	0.002	0.008
Observations	8,687	8,641	17,328	8,687	8,641	17,328
$D(High\,C^E_{period})$	0.165***	0.136***	0.133***	0.118***	0.106***	0.106***
	(0.024)	(0.021)	(0.016)	(0.020)	(0.020)	(0.013)
Adj. R^2 (period)	0.006	0.004	0.007	0.006	0.004	0.007
Observations	8,687	8,641	17,328	8,687	8,641	17,328

Panel B: Rounding

	$R_{i,t+1}$			$R_{i,t+2}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
$D(High\,C^E_{seq.})$	0.100***	0.032	0.066***	0.092***	0.034	0.063***
	(0.030)	(0.032)	(0.020)	(0.031)	(0.034)	(0.020)
Adj. R^2 (sequence)	0.013	0.003	0.008	0.012	0.007	0.009
Observations	8,687	8,641	17,328	8,687	8,641	17,328
$D(High\,C^E_{period})$	0.052***	0.070***	0.055***	0.060***	0.045***	0.048***
	(0.012)	(0.012)	(0.009)	(0.011)	(0.012)	(0.008)
Adj. R^2 (period)	0.005	0.006	0.006	0.006	0.008	0.007
Observations	8,687	8,641	17,328	8,687	8,641	17,328

Table 3: Environmental Complexity, Uncertainty, and Rounding by Repetition

Notes: Sequence-level regressors use repetition-specific top-3 vs bottom-3 sequence-volatility splits (within each repetition, among 6 sequences). The "Overall" sequence specification pools both repetitions and uses the same within-repetition split. Period-level regressors are binary indicators $HighEnvUnc^{prd}_{s,t,r} = \mathbf{1}[|z| > 1]$: for Rep 1 and Rep 2 columns, z is standardized within repetition; for Overall, z is standardized globally. All models include period fixed effects; overall columns additionally include repetition fixed effects. A COVID-era indicator is included in all specifications. Standard errors clustered at the individual level are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

significant. Period-level effects are strong and significant within each repetition and pooled globally: moving into a high-shock period ($|z| > 1$) raises uncertainty, in standard deviation terms, by 0.165 (Repetition 1), 0.136 (Repetition 2), and 0.133 (Overall) at $t + 1$, and by 0.118, 0.106, and 0.106 at $t + 2$. All estimates are significant at the 1% level.

Panel B mirrors these patterns for rounding. Sequence-level assignments increase the probability of a rounded forecast by $9 - 10$ percentage points in repetition 1 at each forecast horizon but have little effect in repetition 2. In the pooled stacked specification, sequence-level effects remain positive and significant (6.6 and 6.3 percentage points at $t+1$ and $t+2$). Period-level complexity robustly raises rounding by roughly 4.5–7.0 percentage points in every subsample and at both horizons.

Taken together, these results reveal two distinct facets of environmental complexity. Sequence-level volatility strongly increases both uncertainty and rounding in the first repetition, but its effect disappears almost entirely by the second. Subjects appear to learn to cope with sustained volatility, such that simply being in a high-volatility setting itself does not necessarily increase either forecast uncertainty or rounding for an experienced forecaster. Period-level complexity tells a different story. An abrupt, outsized demand shock raises uncertainty and rounding by similar magnitudes in both repetitions, with no evidence of attenuation. This robustness to experience suggests that the cognitive burden imposed by a sudden, large deviation is not as malleable to experience; each such shock forces a real-time reassessment that taxes forecasting precision anew. The contrast implies that the environmental-complexity channel perhaps operates primarily through transient spikes in forecasting difficulty rather than through the overall turbulence of the environment, and that satisficing via rounded forecasts is the persistent behavioral response to these spikes.

3.3 Experimental Variation in Subjective Complexity

We next use the variation from randomized differences in central-bank communication to examine whether communication regimes that increase the cognitive demands of point forecasting lead to higher reported uncertainty and greater 10-bps rounding. We use two complementary regression specifications that both treat the *Point* communication regime as the natural benchmark. In the Point regime, the central bank provides an evolving, model-consistent point forecast of inflation for the next five periods. Because the forecast conveys a singular path with no corresponding uncertainty, it imposes the lowest cognitive burden for respondents who are asked to produce their own point forecasts of inflation. The remaining regimes (*NoComm*, *DensityOnly*, and *Point&Density*) either remove the point anchor or introduce additional distributional information, each of which plausibly increases the subjective complexity of forming a point forecast. We therefore define $D(High\, C^S) = 1$ for *NoComm*, *Point&Density*, and *DensityOnly*, and $D(High\, C^S) = 0$ for *Point* and estimate:

$$Y^{(h)}_{istr} = \alpha_{h,g} + \beta_{h,g}\, D(High\, C^S) + \lambda_t + \rho_r + \kappa_{covid} + \varepsilon^{(h)}_{istr}, \tag{4}$$

where, as before, $Y^{(h)}_{istr}$ is either uncertainty ($U_{t+h,i}$) or the 10-bps rounding indicator ($R_{i,t+h}$), and $g \in \{\text{Rep 1}, \text{Rep 2}, \text{Overall}\}$ indexes the sample.

Table 4 reports the results from estimating Equation 4. Across specifications, communication regimes that deviate from a simple point forecast increase both reported forecast uncertainty and rounding. The indicator for a non-*Point* communication regime is positive and statistically significant at both horizons in all time samples we consider. Thus, deviating from a simple focal anchor raises forecast uncertainty and the propensity to round relative to the *Point* benchmark.

Second, to explore the drivers further and examine the effects of each treatment in detail, we estimate:

$$Y^{(h)}_{istr} = \alpha_{h,g} + \sum_{c \in \mathcal{C}} \beta^{c}_{h,g}\, \mathbb{1}\{Comm_i = c\} + \lambda_t + \rho_r + \kappa_{covid} + \varepsilon^{(h)}_{istr}, \tag{5}$$

where $\mathcal{C} = \{NoComm, Point\&Density, DensityOnly\}$. *Point* is the omitted baseline communication regime. Table 5 reports the results.

Panel A: Individual Forecast Uncertainty

	$U_{t+1,i}$			$U_{t+2,i}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
$D(High\,C^S)$	0.376***	0.469***	0.422***	0.386***	0.463***	0.424***
	(0.081)	(0.089)	(0.081)	(0.086)	(0.094)	(0.087)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,687	8,641	17,328	8,687	8,641	17,328
Adj. R^2	0.032	0.043	0.040	0.035	0.046	0.042

Panel B: Rounding

	$R_{i,t+1}$			$R_{i,t+2}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
$D(High\,C^S)$	0.095***	0.077**	0.086***	0.117***	0.126***	0.121***
	(0.032)	(0.034)	(0.032)	(0.034)	(0.036)	(0.033)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,687	8,641	17,328	8,687	8,641	17,328
Adj. R^2	0.010	0.007	0.009	0.015	0.019	0.017

Table 4: Subjective Complexity (Communication), Uncertainty, and Rounding by Repetition

Notes: $HighCogUnc_i = 1$ for communication treatments NoComm, Point+Density, and DensityOnly, and 0 for Point. Panel A reports OLS estimates with individual uncertainty as the dependent variable; Panel B reports LPM estimates with the 10-bps rounding indicator as the dependent variable. Rep 1 and Rep 2 columns include subperiod and COVID-era fixed effects; Overall columns additionally include repetition fixed effects. Standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This specification shows that the previously identified effect on forecast uncertainty is not driven by a single treatment arm: all three non-*Point* communication regimes increase forecast uncertainty at both horizons relative to *Point*. The largest effects are under *DensityOnly* – increasing reported uncertainty by around 0.8-0.9pp for the one- and two-period-ahead forecast horizons, followed by *Point&Density* (around 0.4pp more uncertainty) and the smallest, but still positive and significant, effect is under *NoComm* $(0.2 - 0.3)$.

The pattern for rounding, however, is more nuanced. Both *DensityOnly* and *NoComm* produce sizable and statistically significant increases in the propensity to round: in our overall specification, *DensityOnly* raises rounding by 15.4 and 22.6 percentage points for the one- and two-period-ahead forecast horizons, respectively, while *NoComm* raises it by 9.1 and 12.9 percentage points. *Point&Density*, on the other hand, produces at most a weakly significant increase in rounding in the first repetition (7pp at $t+1$), but the effect attenuates with experience and is generally not statistically significant in the overall estimates. This suggests that communicating uncertainty does not necessarily increase the propensity to round, even when it sharply raises forecast uncertainty, provided the uncertainty surrounds a focal point. The central bank's point forecast thus serves as an anchor, reducing the cognitive burden of the forecast task.[6]

As treatments are randomly assigned and the underlying economic environment is held fixed, these effects reflect differences in information presentation rather than changes in fundamen-

[6] See Appendix A.2 for robustness of subjective complexity results to alternative definitions of rounding.

Panel A: Individual Forecast Uncertainty (Point Baseline)						
	$U_{t+1,i}$			$U_{t+2,i}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
NoComm	0.202**	0.281***	0.241**	0.231**	0.309***	0.269**
	(0.091)	(0.105)	(0.094)	(0.104)	(0.116)	(0.107)
Point&Density	0.388***	0.459***	0.423***	0.381***	0.407***	0.394***
	(0.109)	(0.119)	(0.109)	(0.114)	(0.116)	(0.111)
DensityOnly	0.739***	0.921***	0.829***	0.747***	0.948***	0.846***
	(0.166)	(0.187)	(0.171)	(0.165)	(0.182)	(0.168)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,687	8,641	17,328	8,687	8,641	17,328
Adj. R^2	0.058	0.077	0.069	0.057	0.082	0.070

Panel B: Rounding (10 bps)						
	$R_{i,t+1}$			$R_{i,t+2}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
NoComm	0.091**	0.090**	0.091**	0.123***	0.134***	0.129***
	(0.039)	(0.041)	(0.038)	(0.040)	(0.043)	(0.039)
Point&Density	0.070*	0.034	0.052	0.071*	0.067	0.069*
	(0.038)	(0.041)	(0.038)	(0.040)	(0.042)	(0.039)
DensityOnly	0.161***	0.148**	0.154***	0.212***	0.242***	0.226***
	(0.053)	(0.058)	(0.053)	(0.055)	(0.058)	(0.054)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,687	8,641	17,328	8,687	8,641	17,328
Adj. R^2	0.013	0.012	0.013	0.022	0.029	0.026

Table 5: Communication Regimes (Point Baseline), Uncertainty, and Rounding by Repetition

Notes: Time FE include controls, where appropriate, for period, repetition, and whether an experimental session took place before or after the onset of Covid. Coefficients are differences relative to the Point communication regime. Rep 1 and Rep 2 columns include subperiod and COVID-era fixed effects; Overall columns additionally include repetition fixed effects. Panel A reports OLS estimates for individual uncertainty; Panel B reports LPM estimates for 10-bps rounding. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tals. The pattern is therefore consistent with a subjective-complexity mechanism: when communication design removes a focal point or requires respondents to reconcile multiple pieces of information, the cognitive burden of forming a forecast increases, leading to higher reported uncertainty and greater reliance on coarse numerical reporting.

3.4 Shapley Values: Decomposing the drivers of satisficing

Although we experimentally manipulate both environmental and subjective complexity, the realized measures for each are not necessarily orthogonal, since both shape the difficulty of forming a precise forecast. As a result, regression coefficients alone cannot uniquely apportion their overlapping explanatory power. Shapley decomposition provides an order-invariant method to allocate this shared explanatory power across the two forms of complexity.

Formally, let $\mathcal{M} = \{C^E, C^S\}$ denote environmental and subjective complexity, respectively.

Let $v(S)$ denote the average predicted probability of rounding when a linear probability model includes only the regressors in $S \subseteq \mathcal{M}$ (together with time fixed effects).[7] The Shapley value for mechanism $m \in \mathcal{M}$ is

$$\phi_m = \sum_{S \subseteq \mathcal{M} \setminus \{m\}} \frac{|S|! \left(|\mathcal{M}| - |S| - 1\right)!}{|\mathcal{M}|!} \left[v(S \cup \{m\}) - v(S)\right].$$

Intuitively, the numerator, ϕ_m, captures how much adding either type of uncertainty increases the model-predicted probability of rounding after accounting for the fact that the two forces overlap: both raise task difficulty and therefore make satisficing more likely. The denominator, $v(C^E, C^S) - v(\emptyset)$, is the total increase in the predicted likelihood of rounding when both mechanisms operate. Dividing the two yields a Shapley share, which is the fraction of the overall difficulty-driven rise in rounding that we can attributable to each mechanism among individuals who actually rounded:

$$\frac{\phi_m}{v(C^E, C^S) - v(\emptyset)}.$$

As rounding is a binary outcome with a large mass at zero, we compute these contributions *conditional on the subsample with* $R_{i,t,h} = 1$, which lets us attribute the relative importance of each type of uncertainty *among observations that actually rounded*. The resulting shares should therefore be interpreted as explained variation in the fitted propensity to round within the set of rounders, not as a decomposition of the overall incidence of rounding in the sample.

Figure 3 shows the results. Both types of complexity contribute to rounding among rounders, but communication-induced subjective complexity (C^S) is the dominant channel. Under the sequence-level definition of environmental complexity, C^S accounts for 58% of explained rounding at $t+1$ and 75% at $t+2$, while C^E accounts for 42% and 25%, respectively. Using the period-level definition yields the same ranking and an even stronger tilt toward subjective complexity: C^S explains 69% at $t+1$ and 86% at $t+2$, versus 31% and 14% for C^E.[8]

There are two key takeaways about within-rounder attribution. First, environmental complexity is not irrelevant, especially at the short horizon, but its relative importance declines with horizon. Second, the communication channel becomes increasingly important at longer horizons, indicating that subjective complexity is a first-order driver of satisficing behavior in this setting. This pattern is robust across both sequence-based and period-based definitions of environmental complexity.

[7] We obtain quantitatively similar results when excluding time fixed effects.

[8] See Appendix A.4 for robustness to alternative definitions of rounding.

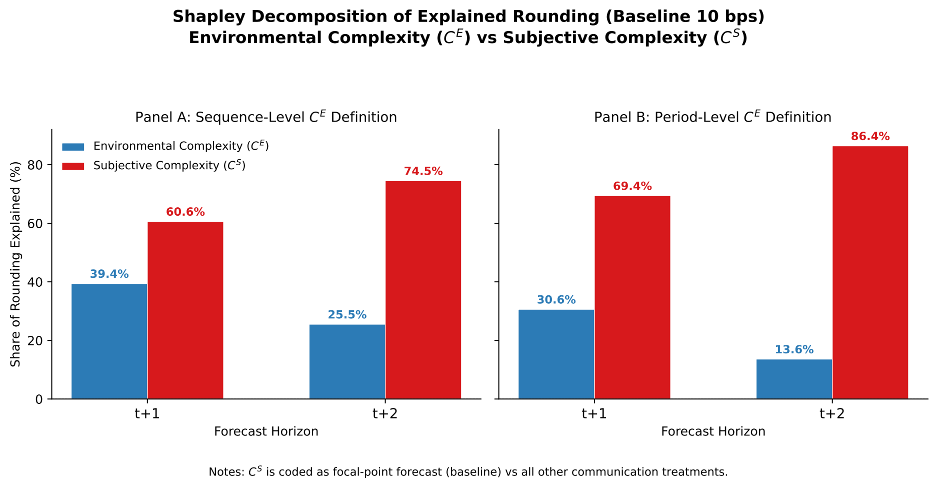


Figure 3: Shapley decomposition of explained rounding into C^E and C^S

The left panel (Panel A) defines *Environmental Complexity* (C^E) at the sequence level while the right panel (Panel B) defines C^E at the period level. *Subjective Complexity* (C^S) is coded as a focal-point communication contrast: $C^S = 1$ for non-Point treatments ($NoComm$, *Point&Density*, *DensityOnly*) and $C^S = 0$ for Point. Bars report each factor's Shapley share of explained 10-bps rounding at horizons $t+1$ and $t+2$ (among forecasts that exhibit rounding). Across both C^E definitions, C^S explains the larger share of rounding variation, especially at $t+2$.

4 Complexity and Rounding in the Real World

Our experimental results show that both environmental and subjective complexity induce rounding via the forecast uncertainty channel. A natural question is whether similar forces operate in real-world survey environments. To explore this, we use monthly microdata from June 2013 through March 2025 from the Federal Reserve Bank of New York's Survey of Consumer Expectations (SCE) to examine how respondents' experience with the survey affects their propensity to round. The SCE provides a useful survey-based setting for studying learning and subjective task complexity: its questionnaire, interface, and forecast-elicitation format are fixed over time, but respondents remain in the panel for multiple months.

4.1 Rounding and Demographics in the SCE

Following Binder (2017), we define rounding in the SCE as forecasts satisfying

$$\text{Rounded}_{it} = \mathbf{1}\{\,\text{forecast}_{it} \bmod 5 = 0\,\},$$

which defines any forecast that is a multiple of 5% as rounded. Table 6 reports rounding rates in the SCE by education, household income, and age group. The education gradient is striking: 51.8% of respondents with a high school diploma or less report forecasts that are multiples of 5, compared to 41.5% for those with some college and 26.6% for college graduates. The income gradient is similarly large, with rounding rates of 44.7% among respondents earning less than $50,000 per year versus 24.3% for those earning more than $100,000. Age differences are more modest: respondents under 40 round at 31.9%, compared to roughly 35% for older groups. These patterns are consistent with the experimental finding that lower cognitive capacity increases reliance on rounding heuristics, and they suggest that the numeracy channel documented in the laboratory operates in the field as well.

Table 6: Rounding Rates by Demographic Group in the SCE

	Rounded to 5 (%)	Rounded to 10 (%)	*N*
Education			
High school or less	51.8	25.3	20,332
Some college	41.5	17.5	57,800
College graduate	26.6	9.0	99,515
Household income			
Under $50,000	44.7	20.3	59,999
$50,000–$100,000	33.3	12.7	62,199
Over $100,000	24.3	7.5	54,381
Age			
Under 40	31.9	12.1	52,064
40 to 60	35.3	14.4	70,455
Over 60	35.6	14.3	55,643

Notes: Sample restricted to respondent–months within the first 12 months of panel participation. Rounding rates are unconditional shares of respondents whose one-year-ahead inflation forecast is a multiple of the indicated threshold. Inflation forecasts are winsorized at the 5th and 95th percentiles by month.

4.2 Subjective and Environmental Complexity in the SCE

This evidence from the SCE suggests that subjective complexity may be an important driver of rounding: more educated and higher-income households round significantly less. On its own, however, this pattern is difficult to interpret, since such households may differ along many dimensions other than their ability to process the forecasting task. An advantage of our experimental results is that we provide a cleaner, more direct mechanism. There, reducing subjective complexity by supplying respondents with a focal anchor lowers the propensity to round. This motivates our use of panel variation in the SCE to examine whether familiarity with the survey task is likewise associated with reduced rounding.

While the SCE does not contain a direct and changing measure of subjective task difficulty, if rounding partly reflects cognitive burden or difficulty in navigating the survey, then respondents should round less as they become familiar with the task's structure. Therefore, we group individuals by experience in the survey, which we call $Tenure_{i,t}$. We collapse survey respondents into five categories: first month ($Tenure_{i,t} = 0$), months 1–3 ($Tenure_{i,t} = 1$), months 4–6 ($Tenure_{i,t} = 2$), months 7–9 ($Tenure_{i,t} = 3$), and months 10–12 ($Tenure_{i,t} = 4$).

To proxy environmental complexity in a real-world setting, we use realized changes in the Personal Consumption Expenditures (PCE) price index. Using the absolute month-to-month percent change in PCE, we construct a binary indicator, $D(\text{High Vol}_{t-1})$, equal to one if the absolute change in the month preceding the survey exceeds one standard deviation above the sample mean. Using the lagged value ensures that the volatility measure reflects the inflationary environment respondents have recently experienced rather than contemporaneous price movements they are unlikely to observe in real time. As a robustness check, we also consider an indicator based on the rolling three-month standard deviation of monthly PCE percent changes, which captures slightly more persistent variation in price dynamics.

Armed with these proxies, we estimate the following regression models:

$$\text{Rounded}_{it} = \alpha + \beta_1 \cdot D(\text{High Vol}_{t-1}) + \sum_{k=1}^{4} \gamma_k \cdot D(\text{Tenure}_{it} = k) + \mu_t + u_i + \varepsilon_{it} \tag{6}$$

$$\begin{aligned} \text{Rounded}_{it} = \alpha + \beta_1 \cdot D(\text{High Vol}_{t-1}) + \sum_{k=1}^{4} \gamma_k \cdot D(\text{Tenure}_{it} = k)... \\ + \sum_{k=1}^{4} \delta_k \cdot \big(D(\text{High Vol}_{t-1}) \times D(\text{Tenure}_{it} = k)\big) + \mu_t + u_i + \varepsilon_{it} \end{aligned} \tag{7}$$

where Rounded_{it} is an indicator for whether respondent i's inflation forecast in month t is a multiple of five, $D(\text{Tenure}_{it} = k)$ are indicators for survey tenure groups (Months 1–3, 4–6, 7–9, and 10–12, with the respondent's first month as the omitted category), μ_t are month fixed effects, and u_i is an individual random effect.[9] Standard errors are clustered at the individual level.

Table 7 reports the results. Columns (1) and (3) present the estimation of equation (6) for the month-to-month and three-month rolling measures respectively. Columns (2) and (4) add interactions with survey tenure as in equation (7). The main coefficients of interest are β_1, which captures the effect of elevated inflation volatility on rounding, and the γ_k coefficients which capture the effect of experience within the survey. The δ_k coefficients on the interaction terms test whether the effect of being in a volatile environment varies with survey experience.

Across all columns, elevated inflation volatility is associated with significantly higher rates of rounding. The month-to-month indicator implies a 5.6 percentage point increase in the probability of rounding, while the three-month rolling measure implies a 9.8 percentage point

[9]These results are robust to an individual fixed-effects specification, which identifies the relationship using within-respondent variation over time and absorbs fixed differences in respondents' general propensity to round. See A.5 for details.

increase. The larger magnitude for the rolling measure likely reflects the fact that sustained volatility, rather than a single month's price jump, is more salient to respondents.

Table 7: Realized Inflation Volatility, Survey Tenure, and Rounding

	Dependent variable: Rounding indicator			
	Month-to-Month		3-Month Rolling	
	(1)	(2)	(3)	(4)
$D(\text{High Vol}_{t-1})$	0.056***	0.050**	0.098***	0.089***
	(0.017)	(0.024)	(0.017)	(0.021)
Months 1–3	−0.079***	−0.079***	−0.079***	−0.079***
	(0.003)	(0.003)	(0.003)	(0.003)
Months 4–6	−0.108***	−0.109***	−0.108***	−0.109***
	(0.004)	(0.004)	(0.004)	(0.004)
Months 7–9	−0.124***	−0.124***	−0.123***	−0.125***
	(0.004)	(0.004)	(0.004)	(0.004)
Months 10–12	−0.130***	−0.130***	−0.130***	−0.130***
	(0.004)	(0.004)	(0.004)	(0.004)
$D(\text{High Vol}_{t-1}) \times$ Months 1–3		−0.000		−0.004
		(0.020)		(0.015)
$D(\text{High Vol}_{t-1}) \times$ Months 4–6		0.017		0.015
		(0.020)		(0.015)
$D(\text{High Vol}_{t-1}) \times$ Months 7–9		0.000		0.019
		(0.021)		(0.016)
$D(\text{High Vol}_{t-1}) \times$ Months 10–12		0.011		0.013
		(0.023)		(0.018)
Month FE	Yes	Yes	Yes	Yes
Individual RE	Yes	Yes	Yes	Yes
Observations	177,014	177,014	174,063	174,063
Individuals	23,455	23,455	22,945	22,945
R^2 (overall)	0.027	0.027	0.027	0.027

Notes: Random effects GLS estimates. The dependent variable is an indicator for rounding to the nearest multiple of 5. High Volatility$_{t-1}$ is a binary indicator equal to one if the relevant measure of realized PCE inflation volatility in the prior month exceeds one standard deviation above its sample mean. Columns (1)–(2) define volatility as the absolute month-to-month percent change in PCE. Columns (3)–(4) define volatility as the rolling 3-month standard deviation of monthly PCE percent changes. Survey tenure groups are defined by months in the SCE panel: First Month (omitted), Months 1–3, Months 4–6, Months 7–9, and Months 10–12. Standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The tenure gradient is stable across all specifications. Relative to their first month in the panel, respondents in months 1–3 are approximately 7.9 percentage points less likely to round, with the reduction increasing monotonically to 13.0 percentage points for respondents in months 10–12. The monotonic decline in rounding with survey tenure reflects a reduction in the subjective complexity of forming inflation expectations, not changes in information or macroeconomic conditions. This pattern mirrors the experimental finding that experience reduces rounding, consistent with subjects developing more precise forecasting strategies over time and is in line with Kim and Binder (2023) who also observe a "learning-through-survey" effect in the SCE, Michigan Survey of Consumers and a survey of U.S. firms, whereby inflation expectations and uncertainty decline with tenure in the surveys. Because all tenure groups coexist in the same calendar months and face the same inflation data, news, and shocks, cohort effects and structural breaks cannot explain the pattern. Instead, two reinforcing learning processes are at work. First, respondents become familiar with the survey instrument itself—the layout, the question flow, the numeric entry boxes, and the meaning of the forecast question—which reduces the cognitive load imposed by the interface. Second, respondents become familiar with the forecast-formation task: repeatedly generating an inflation expectation each month lowers the perceived difficulty of the underlying problem, even though the information set is unchanged.

The SCE evidence mirrors the complexity channel identified in our experiment: repeated exposure to an identical forecasting task reduces subjective complexity and therefore reduces satisficing behavior. In this sense, survey tenure is an "experience-based complexity shock," and its effect is large—on the order of a 15-percentage-point decline in rounding over a the full survey participation cycle. This provides external validation for our central claim that subjective complexity is a distinct and powerful determinant of satisficing, separate from environmental complexity.

The interaction terms in Columns (2) and (4) are uniformly small and statistically insignificant. The effect of inflation volatility on rounding does not differ across tenure groups. This result implies that environmental complexity and survey experience operate as independent, additive influences on rounding behavior. Experienced respondents round less on average, and all respondents round more in volatile months, but experienced respondents are neither more nor less sensitive to volatility than new entrants. This finding is broadly consistent with the experimental evidence, where volatility defined at the period level remains a significant driver of rounding in both repetitions of the experiment and at both forecast horizons.

4.3 Rounding and Upward-Biased Inflation Expectations

Our results imply that cross-sectional expectations data disproportionately reflect higher rounding among inexperienced respondents, and that survey tenure mechanically influences measured inflation expectations. Individuals provide rounded numerical responses when a task is difficult, and these rounded responses differ systematically from their underlying true beliefs. To test this implication, we use the SCE data to ask: do rounded forecasts differ systematically from non-rounded forecasts, and does this gap narrow as the cognitive burden of the task diminishes with experience?

To quantify how rounding affects the inflation expectations that researchers observe, we estimate:

$$\mathbb{E}_{i,t}\left[\pi^{1y}\right] = \alpha + \sum_{k=1}^{4} \beta_k \cdot D(\text{Tenure}_{it} = k) + \theta \cdot \text{Rounded}_{it} + \sum_{k=1}^{4} \psi_k \cdot D(\text{Tenure}_{it} = k) \times \text{Rounded}_{it} + \gamma_t + \mathbf{X}'_{it}\delta + u_i + \varepsilon_{it}. \tag{8}$$

where $\mathbb{E}_{i,t}\left[\pi^{1y}\right]$ is respondent i's one-year-ahead inflation forecast at time t, $D(\text{Tenure}_{it} = k) \in \{0, 1, 2, 3, 4\}$ is a respondent's survey tenure category, γ_t denotes month-year fixed effects, $\mathbf{X}_{i,t}$ includes categorical controls for age, education, and household income, and u_i is a respondent-level random effect. Standard errors are clustered at the respondent level.

Table 8 reports the estimates. Rounded forecasts are substantially higher than non-rounded forecasts: in the first tenure category, rounded forecasts exceed non-rounded forecasts by approximately 6.89 percentage points ($p < 0.001$). The interaction terms show that this rounding premium declines sharply with survey experience, falling by roughly 3.8 percentage points by months 10–12.

These results imply that rounding in household inflation surveys is not innocuous noise. Because early-tenure respondents round more and report higher inflation when they do, cross-sectional expectations mechanically overweight high-inflation responses. Researchers using the SCE (or similar panels) should account for survey tenure and rounding behavior, as failing to do so can bias levels of inflation expectations upward and inflate measured disagreement.

Figure 4 shows that the decline in inflation expectations with survey tenure in the SCE is almost entirely concentrated among respondents who round. Within each tenure group, the distribution of forecasts for non-rounders is remarkably stable, with only modest downward drift. By contrast, the distribution for rounders shifts sharply left as respondents accumulate survey experience, with the upper tail collapsing and the modal forecast moving toward zero. As rounders overweight multiples of five, while inflation has typically been less than 5%, their declining share mechanically lowers average inflation expectations. Together with the regression results, this pattern indicates that widely reported tenure gradients in inflation expectations are driven primarily by reductions in satisficing rather than improvements in respondents' underlying information or forecasting ability. As the cognitive burden of the task diminishes through repeated exposure, respondents round less, and the inflation bias induced by rounding attenuates.

Table 8: Rounding Substantially Elevates Reported Inflation Expectations in the SCE

	$\mathbb{E}_{i,t}\left[\pi^{1y}\right]$
Rounded	6.887^{***}
	(0.103)
$D(\text{Tenure}_{it} = 1)$	-0.043
	(0.043)
$D(\text{Tenure}_{it} = 2)$	-0.076^{*}
	(0.045)
$D(\text{Tenure}_{it} = 3)$	-0.086^{*}
	(0.047)
$D(\text{Tenure}_{it} = 4)$	-0.067
	(0.051)
Rounded $\times$ $D(\text{Tenure}_{it} = 1)$	-2.150^{***}
	(0.112)
Rounded $\times$ $D(\text{Tenure}_{it} = 2)$	-3.195^{***}
	(0.121)
Rounded $\times$ $D(\text{Tenure}_{it} = 3)$	-3.589^{***}
	(0.125)
Rounded $\times$ $D(\text{Tenure}_{it} = 4)$	-3.848^{***}
	(0.137)
Constant	2.736^{***}
	(0.133)
Observations	175,974

Notes: Dependent variable is the 12-month inflation forecast. "Rounded" is an indicator denoting when an inflation forecast is a multiple of 5. Random-effects GLS with month-year fixed effects and controls for age, education, and household income. Robust standard errors clustered at the respondent level are shown in parentheses. $^{*}p < 0.10$, $^{***}p < 0.01$.

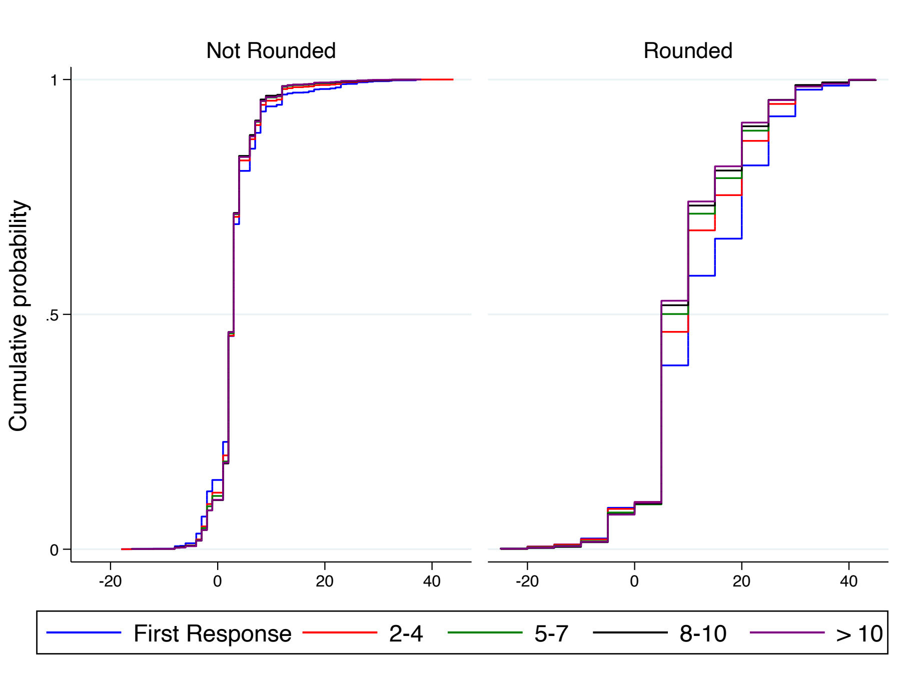


Figure 4: Inflation Expectations Over Tenure Groups By Rounding Status

<u>Notes:</u> Each panel plots the cumulative distribution of 12-month-ahead inflation forecasts from the SCE, separately by survey tenure group. Tenure groups reflect the number of times a respondent has participated in the survey: first response, 2–4, 5–7, 8–10, and more than 10. The right panel restricts to respondents whose forecast is a multiple of 5 ("Rounded"); the left panel restricts to all other respondents ("Not Rounded"). Forecasts are winsorized at the 5th and 95th percentiles by month-year.

4.4 Rounding And Forecast Errors

Finally, to further investigate whether rounding reflects genuine cognitive disengagement rather than a benign reporting convention, we examine whether rounders produce less accurate forecasts. If rounding is merely a cosmetic feature of how individuals report beliefs, it should bear no systematic relationship to forecast quality. If, however, rounding signals shallow processing of available information, rounders should exhibit larger forecast errors even after accounting for subjective uncertainty.

We estimate regressions of the form

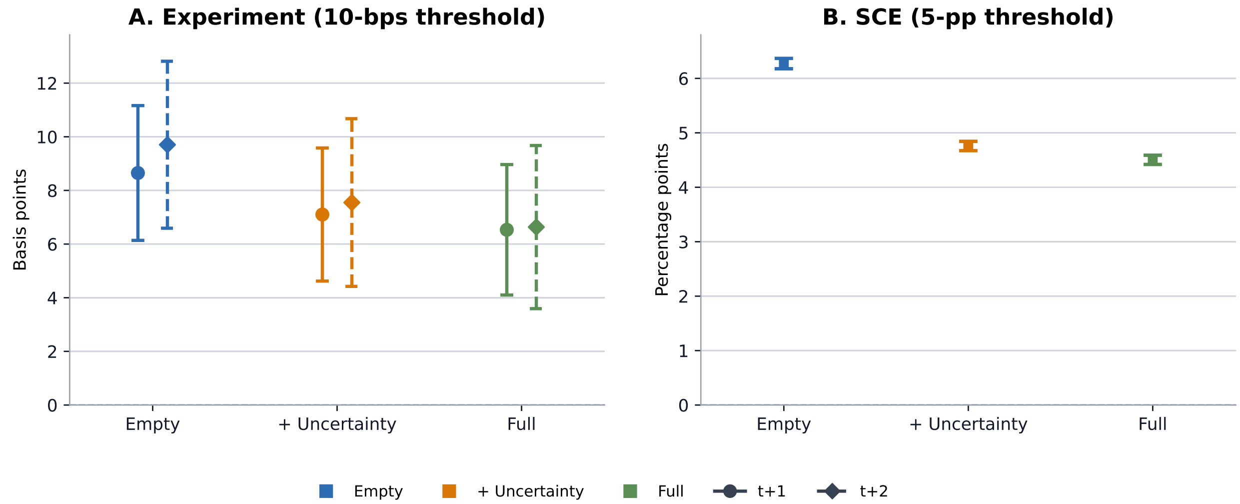
$$|e_{i,t+h}| = \alpha + \beta \cdot \text{Rounded}_{it} + \gamma' X_{i,t} + \varepsilon_{i,t}, \tag{9}$$

where $|e_{i,t+h}|$ is the absolute forecast error at horizon h. We present three specifications: a baseline with no controls (*Empty*), a specification that adds a standardized measure of subjective uncertainty (*+ Uncertainty*), and a full specification that additionally includes period fixed effects and individual-level controls (*Full*).

The inclusion of subjective uncertainty is critical. Rounders tend to report higher uncertainty, and uncertain forecasters mechanically produce larger errors on average. The question is whether rounding predicts forecast accuracy beyond what uncertainty alone can explain. In the experimental data, we measure uncertainty using the standardized width of each respondent's incentivized range forecasts. In the SCE, we proxy uncertainty with the standardized interquartile range of the density forecast distribution, which the survey elicits alongside the point forecast. The IQR is a natural analog: both capture the dispersion of the respondent's subjective probability distribution over future inflation. The full SCE specification additionally controls for a respondent's age, education, household income, and numeracy..

The results, presented in Figure 5, tell a consistent story across both datasets. The left figure reports results from the experimental data at horizons $t + 1$ and $t + 2$: rounders produce significantly larger absolute forecast errors. The unconditional gaps are substantial. For instance, rounders' forecast errors are approximately 8.7 basis points larger at $t + 1$ and 9.7 basis points larger at $t + 2$. Adding the uncertainty control reduces these coefficients by roughly 15 to 30 percent, confirming that part of the rounding–accuracy link operates through higher subjective uncertainty among rounders. However, the coefficients remain large and significant in all specifications. Including period fixed effects and repetition controls attenuates the estimates only modestly further, suggesting that the relationship is robust to experience.

The right figure presents the corresponding analysis using the NY Fed Survey of Consumer Expectations, where we observe a similar pattern despite differences in setting, question format, and population. Respondents whose 12-month-ahead inflation point forecasts are divisible by 5 percentage points have absolute forecast errors that are 6.3 percentage points larger unconditionally, falling to 4.5 percentage points after controlling for uncertainty and



In Panel A, circles and solid confidence intervals indicate t+1; diamonds and dashed confidence intervals indicate t+2.

Figure 5: Effect of rounding on absolute forecast errors. The left panel plots coefficients from regressions of absolute forecast errors on a rounding indicator in our experimental data, at horizons $t+1$ (left) and $t+2$ (right). The right panel reports the analogous exercise using the NY Fed Survey of Consumer Expectations, with a rounding threshold of 5 percentage points. We show three specifications: no controls (Empty), adding standardized measures of forecast uncertainty (+ Uncertainty), and a full specification with uncertainty, period fixed effects, and demographic or repetition controls (Full). Coefficients surrounded by 95% confidence intervals based on robust standard errors clustered at the individual level.

demographics. The step-down across specifications mirrors the experimental pattern: uncertainty explains a meaningful share of the gap, but a large and significant residual remains.

These results suggest that rounding is not a benign reporting artifact. Rather, rounding systematically predicts objectively worse forecast performance above and beyond what can be explained by the forecaster's own acknowledged uncertainty. This is consistent with rounding capturing some combination of limited information acquisition, coarser mental models, or lower cognitive effort in forming expectations. Regardless of the underlying mechanism, these forecast errors, to the extent that expectations inform economic decisions and that stated beliefs in the SCE correspond to true beliefs, carry meaningful welfare implications that impact differently across rounders and non-rounders. Further, it is worth recalling that subjects in our experiment were compensated via a scoring rule that directly rewarded forecast accuracy. The larger forecast errors associated with rounding were therefore costly in real terms: rounders earned measurably less than non-rounders. That this pattern persists despite explicit financial incentives for accuracy underscores that rounding is not simply a matter of carelessness that sharper incentives could correct.

5 Conclusion and Discussion

This paper provides causal evidence that rounding in quantitative survey responses reflects two distinct forms of task difficulty: environmental complexity, arising from the inherent unpredictability of the forecasting environment, and subjective complexity, arising from the cognitive burden imposed on the respondent. Using more than 17,000 incentivized, individually linked inflation forecasts from controlled laboratory and online experiments, we show that exogenous variation in shock volatility and in central bank communication regimes each independently increase both reported forecast uncertainty and the propensity to round. These effects are robust across forecast horizons, repetitions, and alternative definitions of rounding.

A Shapley decomposition reveals that while both channels contribute meaningfully to observed rounding, subjective complexity is the dominant driver, particularly at longer forecast horizons. At the one-period-ahead horizon, the two channels contribute roughly equally, but by the two-period-ahead horizon, the communication-induced subjective complexity channel accounts for approximately 65–85% of explained rounding, depending on the definition of environmental complexity employed. This asymmetry suggests that as the forecast horizon extends and the underlying economic environment becomes more difficult to map into a precise numerical expectation, the availability of a cognitive anchor becomes increasingly important in determining whether respondents satisfice.

The SCE evidence reinforces and extends these experimental findings. Rounding rates decline sharply and monotonically with survey tenure, a pattern we interpret as reflecting reductions in subjective complexity as respondents grow familiar with the elicitation task. This tenure gradient cannot be attributed to changes in the macroeconomic environment or to cohort effects, since tenure groups coexist within the same calendar months. Crucially, elevated inflation volatility raises rounding independently of tenure, and its effect does not vary across experience groups, consistent with environmental and subjective complexity operating as distinct, additive channels. Together, these patterns provide external validation for the laboratory findings and suggest that the mechanisms identified in our controlled setting generalize to real-world survey environments.

Beyond their methodological implications for survey design and the measurement of inflation expectations, our findings carry broader lessons for the interpretation of heaped responses in economic data. Rounding is not merely a harmless reporting convention: rounders produce systematically less accurate forecasts, even after conditioning on their own stated uncertainty, and rounded responses inflate cross-sectional estimates of average inflation expectations. These biases are largest among inexperienced, lower-income, and less-educated respondents — precisely those groups for whom the cognitive demands of the forecasting task are likely highest. Researchers and policymakers who draw on household expectations surveys should therefore account for rounding behavior and survey tenure when constructing measures of inflation expectations, as failing to do so risks overstating both the level of expectations and the degree of disagreement across the population.

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Online Appendix for:
Dissecting Heaping in Response Data: Causal Evidence on Complexity versus Uncertainty
by McMahon, Petersen and Rholes

A Robustness Analysis

A.1 Robustness to Alternative Rounding Definitions

The main text defines rounding at the 10-basis-point threshold. This appendix section shows that the uncertainty-rounding correlation is not an artifact of that particular threshold. Table A.1 re-estimates the baseline LPM from equation (X) using five alternative rounding definitions, ranging from multiples of 5 to multiples of 50 basis points.

The correlation between forecast uncertainty and rounding is positive and significant at the 1% level at every threshold and both forecast horizons. The coefficient is strongest at the 10-basis-point threshold used in the main text, but the pattern is monotonically sensible: as the rounding threshold coarsens, the baseline rounding rate falls (from 71% at 5 bps to 15% at 50 bps), and the coefficient on standardized uncertainty remains stable in the range of 0.048 to 0.106. Adding period fixed effects, repetition indicators, and a Covid dummy (columns labeled "FE") leaves the estimates essentially unchanged. Further controlling for the absolute forecast error and inflation volatility (columns labeled "Full") attenuates the coefficients modestly, consistent with the view that these controls partially absorb the same uncertainty channel, but all estimates remain strongly significant.

Two features of these results are worth noting. First, the 10-basis-point threshold produces the largest coefficient at both horizons, which is consistent with multiples of 10 being the most natural focal point for satisficing behavior. Second, the correlation strengthens at $t+2$ relative to $t+1$ across all thresholds, consistent with the longer horizon imposing greater cognitive difficulty and thus a tighter link between uncertainty and rounding.

Table A.1: Uncertainty and Rounding Across Threshold Definitions

	$t+1$ forecast			$t+2$ forecast		
Threshold	Empty	FE	Full	Empty	FE	Full
Panel A: Coefficient on standardized uncertainty (z-score)						
5 bps (71%/72%)	0.060***	0.061***	0.056***	0.077***	0.078***	0.074***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)
10 bps (48%/50%)	0.079***	0.080***	0.072***	0.103***	0.106***	0.098***
	(0.014)	(0.015)	(0.014)	(0.013)	(0.013)	(0.013)
20 bps (26%/28%)	0.054***	0.055***	0.048***	0.072***	0.072***	0.068***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
25 bps (21%/22%)	0.056***	0.057***	0.052***	0.081***	0.081***	0.078***
	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)
50 bps (15%/18%)	0.053***	0.054***	0.049***	0.077***	0.078***	0.075***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)
Panel B: Specification details						
Period FE, Rep, Covid		Yes	Yes		Yes	Yes
$\|$Forecast error$\|$, $\|\Delta\pi\|$			Yes			Yes
N	17,328	17,328	16,937	17,328	17,328	16,572

Notes: Each cell reports the coefficient on globally standardized forecast uncertainty (z-score of the subject's incentivized expected error) from a separate LPM regression of the rounding indicator on uncertainty. Parenthetical percentages next to each threshold denote the rounding rate at that threshold, averaged across $t+1$ and $t+2$. "FE" adds period, repetition, and Covid fixed effects. "Full" further adds the absolute forecast error and absolute inflation change as controls. Standard errors clustered at the individual level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A.2 Robustness of Categorical Subjective Complexity Results

Table A.2 shows that the main rounding results for our categorical definition of subjective complexity are robust to how we define rounding. Across all four thresholds (5, 10, 20, and 50 basis points) and both forecast horizons, the pattern is consistent: *DensityOnly* produces the largest and most significant increases in rounding relative to the Point baseline, while *Point&Density* coefficients are substantially smaller and generally insignificant. *NoComm* falls between these two extremes, with (typically) more modest but reliably significant effects at every threshold.

The bottom row isolates the difference between *DensityOnly* and *Point&Density*, which are our two regimes that provide identical density information but differ in whether the central bank also provides a focal point forecast. This contrast is significant at 10 bps and above for both horizons, and grows stronger as the rounding threshold coarsens. The pattern suggests that the focal point provided by the point forecast is the key mechanism reducing rounding behavior: without it, subjects facing the same density information round substantially more. The lone exception is the 5 bps threshold, where the contrast does not reach conventional significance.

	$R_{i,t+1}$				$R_{i,t+2}$			
	5 bps	10 bps	20 bps	50 bps	5 bps	10 bps	20 bps	50 bps
NoComm	0.120***	0.091**	0.048**	0.044**	0.132***	0.129***	0.067**	0.072***
	(0.042)	(0.038)	(0.024)	(0.020)	(0.043)	(0.039)	(0.027)	(0.025)
Point+Density	0.077*	0.052	0.026	0.012	0.088**	0.069*	0.039	0.047*
	(0.043)	(0.038)	(0.025)	(0.021)	(0.043)	(0.039)	(0.028)	(0.026)
DensityOnly	0.125**	0.154***	0.098***	0.106***	0.168***	0.226***	0.149***	0.169***
	(0.053)	(0.053)	(0.033)	(0.032)	(0.054)	(0.054)	(0.038)	(0.041)
DensityOnly − Point+Density	0.048	0.102*	0.072**	0.094***	0.080	0.157***	0.110***	0.122***
	(0.051)	(0.054)	(0.035)	(0.035)	(0.052)	(0.055)	(0.040)	(0.043)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A.2: Communication Regimes and Rounding: Robustness Across Thresholds

Notes: Each column reports LPM estimates from a separate regression of a rounding indicator on communication-regime dummies with *Point* as the base category. The bottom row reports the linear combination test of *DensityOnly* minus *Point&Density*. Time FE include period, repetition, and COVID-era fixed effects. Robust standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3 Environmental Complexity, Rounding, and Nonzero Forecasts

Table 3 in the main text shows that exogenous environmental complexity significantly raises 10-basis-point rounding. Because the main-text rounding indicators classify a forecast of exactly zero as rounded at every threshold, we begin by re-estimating the baseline specification after excluding zero forecasts and then examine whether the results generalize across coarser rounding definitions.

Table A.3 reports estimates for rounding definitions at 5, 10, 20, and 50 basis points, all excluding zeros. Panel A shows the sequence-level measure ($High\_C^E_{seq.}$). The coefficient is positive and significant in Rep 1 and Overall at every threshold, with magnitudes that decline smoothly as the definition coarsens: the Overall $R_{i,t+1}$ estimate moves from 0.081 at 5 bps to 0.040 at 50 bps, consistent with fewer forecasts qualifying as rounded under stricter criteria. Rep 2 remains weaker at almost all definitions of rounding, and insignificant at the finer grids (5, 10 bps). Overall, this suggest that the sequence-level complexity effect is noisier in the second repetition but present for very coarse definitions of rounding. Panel B shows the period-level measure ($High\_C^E_{period}$). Every coefficient is positive and significant at the 1% level across all four thresholds, both horizons, and all three sample splits.

A forecast of zero is mechanically a multiple of every rounding grid, so retaining zeros inflates measured rounding shares and attenuates complexity coefficients. Table A.4 shows that the gap between including and excluding zeros widens monotonically with the coarseness of the definition. Re-estimating the 10 bps specification without zeros (the R10 row of Table A.3) yields uniformly larger point estimates than the main table—for example, the Overall period-level coefficient rises from 0.055 to 0.093 for $R_{i,t+1}$ and from 0.048 to 0.100 for $R_{i,t+2}$. The main-text estimates are therefore conservative.

Taken together, these results confirm that our main findings related to environmental complexity are an artifact of neither the 10 bps definition of rounding we adopt or of including inflation forecasts of zero basis points in our main analysis.

Panel A: Sequence-level Complexity ($High\,C^{E}_{\text{seq}}$)						
	$R_{i,t+1}$			$R_{i,t+2}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
R5 (5 bps)	0.112***	0.051	0.081***	0.125***	0.035	0.080***
	(0.033)	(0.037)	(0.021)	(0.033)	(0.037)	(0.021)
R10 (10 bps)	0.121***	0.052	0.087***	0.120***	0.060*	0.090***
	(0.029)	(0.032)	(0.020)	(0.031)	(0.033)	(0.020)
R20 (20 bps)	0.066***	0.035**	0.051***	0.054***	0.044**	0.049***
	(0.017)	(0.018)	(0.012)	(0.018)	(0.018)	(0.012)
R50 (50 bps)	0.044***	0.036***	0.040***	0.032**	0.035***	0.033***
	(0.015)	(0.013)	(0.009)	(0.016)	(0.013)	(0.010)
Panel B: Period-level Complexity ($High\,C^{E}_{\text{period}}$)						
	$R_{i,t+1}$			$R_{i,t+2}$		
	Rep 1	Rep 2	Overall	Rep 1	Rep 2	Overall
R5 (5 bps)	0.066***	0.090***	0.068***	0.093***	0.073***	0.080***
	(0.012)	(0.012)	(0.008)	(0.010)	(0.012)	(0.007)
R10 (10 bps)	0.090***	0.107***	0.093***	0.107***	0.104***	0.100***
	(0.012)	(0.013)	(0.009)	(0.012)	(0.012)	(0.009)
R20 (20 bps)	0.072***	0.082***	0.076***	0.089***	0.079***	0.076***
	(0.011)	(0.011)	(0.007)	(0.011)	(0.011)	(0.008)
R50 (50 bps)	0.066***	0.061***	0.061***	0.058***	0.064***	0.059***
	(0.008)	(0.009)	(0.006)	(0.009)	(0.009)	(0.006)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A.3: Robustness: Environmental Complexity and Rounding Across Alternative Thresholds

Notes: Each cell reports the coefficient from a separate LPM regression of a rounding dummy onto the indicated environmental complexity measure, where the rounding definition varies by row. This table explicitly excludes all forecasts of zero basis points as a robustness check on our choice of retaining these observations in our main specifications. Panel A uses sequence-level environmental complexity ($High\_C^{E}_{seq.}$), and Panel B uses period-level environmental complexity ($High\_C^{E}_{period}$). Sample sizes are constant within Panel A (N = 7,485/7,330/14,815 for Rep 1/Rep 2/Overall). In Panel B, N ranges from 7,330 to 16,020 depending on threshold and horizon. Standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Horizon $h = 1$		Horizon $h = 2$	
Rounding def.	With zeros	Without zeros	With zeros	Without zeros
R5 (5 bps)	0.707	0.683	0.717	0.680
R10 (10 bps)	0.482	0.440	0.501	0.436
R20 (20 bps)	0.256	0.196	0.279	0.186
R50 (50 bps)	0.151	0.081	0.182	0.077
N	17,328	16,020	17,328	15,352

Table A.4: Proportion of Forecasts Classified as Rounded: With and Without Zero Forecasts

Notes: Each cell reports the proportion of forecasts classified as rounded under the indicated definition. "With zeros" retains all observations including forecasts of exactly zero; "Without zeros" excludes them. A forecast of zero is mechanically a multiple of every rounding grid, so its inclusion inflates measured rounding shares. The gap between the two columns widens monotonically with the coarseness of the rounding definition: at R50, excluding zeros reduces the measured rounding share by 7.0 percentage points for $h = 1$ and 10.5 percentage points for $h = 2$. Sample sizes differ across horizons after excluding zeros because the sets of zero forecasts differ between $t + 1$ and $t + 2$.

A.4 Shapely Decomposition Robustness

Table A.5 and Table A.6 repeat the Shapley decomposition from subsection 3.4 across alternative rounding definitions, first retaining zero forecasts (Table A.5) and then excluding them (Table A.6). With zeros included, demand's share of the decomposition erodes at coarser thresholds and turns negative at R20 and R50 in several specifications, indicating that forecasts of zero, which coincides with the steady-state equilibrium of the underlying model, absorb variation that perhaps more properly belongs to complexity. After excluding forecasts of zero, demand shares are positive throughout and, at both complexity definitions, increase as the rounding definition coarsens. At R20, for example, period-level complexity accounts for roughly 75% of the total effect at both horizons, compared with around 50% at R10. The total effect itself declines with threshold coarseness (from roughly 0.10 at R5 to 0.05 at R50), consistent with fewer forecasts qualifying as rounded under stricter criteria, but the relative contribution of demand grows. Communication continues to dominate at the finest thresholds (R5), where rounding is common enough that treatment-arm differences in forecasting style matter most.

Across both tables, subjective complexity of the information environment facing each forecaster accounts for the majority of predicted rounding in most specifications. When zero forecasts are excluded, communication's share exceeds 50% at R5 and R10 under both complexity definitions and both horizons. Even as the rounding definition coarsens and environmental complexity's share rises, communication remains substantial, falling below 30% only at R20 and R50 under the period-level measure. When zero forecasts are retained, communication's dominance is even more pronounced, exceeding 60% in nearly every cell. The overall pattern is clear: the subjective difficulty of the forecasting task, shaped by how information is communicated to the forecaster, is an important determinant of rounding behavior, and this conclusion is robust to the choice of rounding grid, forecast horizon, and complexity definition.

This pattern has implications for our interpretation of rounding-based uncertainty measures (Binder 2017), which exploit rounding in survey data to estimate uncertainty that we typically think reflects macroeconomic conditions. Our decomposition suggests subjective complexity, shaped by how information is communicated to the forecaster, consistently accounts for a significant portion of, and often the majority of, predicted rounding. To the extent that rounding-based indices capture variation in the subjective difficulty of the forecasting problem rather than (or in addition to) macroeconomic conditions, the uncertainty they measure may be more micro-founded than previously recognized.

Panel A: Sequence-level Complexity ($High\_C^E_{seq.}$)				
	$R_{i,t+1}$		$R_{i,t+2}$	
	C^E (%)	C^S (%)	C^E (%)	C^S (%)
R5 (5 bps)	31.4	68.6	24.1	75.9
R10 (10 bps)	39.4	60.6	25.5	74.5
R20 (20 bps)	33.3	66.7	9.4	90.6
R50 (50 bps)	20.4	79.6	-14.3	114.3
Panel B: Period-level Complexity ($High\_C^E_{period}$)				
	$R_{i,t+1}$		$R_{i,t+2}$	
	C^E (%)	C^S (%)	C^E (%)	C^S (%)
R5 (5 bps)	17.9	82.1	14.1	85.9
R10 (10 bps)	30.6	69.4	13.6	86.4
R20 (20 bps)	24.1	75.9	-11.5	111.5
R50 (50 bps)	-9.7	109.7	-45.5	145.5

Table A.5: Shapley Decomposition Across Alternative Rounding Thresholds: With Zeros

Notes: Shapley decomposition of predicted rounding into demand (complexity) and communication (Point vs. all others) shares, repeated across rounding definitions. Shares sum to 100 but may fall outside $[0, 100]$ when factors work in opposing directions.

Panel A: Sequence-level Complexity ($High\_C^E_{seq.}$)				
	$R_{i,t+1}$		$R_{i,t+2}$	
	C^E (%)	C^S (%)	C^E (%)	C^S (%)
R5 (5 bps)	35.9	64.1	32.1	67.9
R10 (10 bps)	47.8	52.2	39.6	60.4
R20 (20 bps)	55.6	44.4	50.9	49.1
R50 (50 bps)	53.0	47.0	31.2	68.8
Panel B: Period-level Complexity ($High\_C^E_{period}$)				
	$R_{i,t+1}$		$R_{i,t+2}$	
t	C^E (%)	C^S (%)	C^E (%)	C^S (%)
R5 (5 bps)	29.8	70.2	31.4	68.6
R10 (10 bps)	52.2	47.8	44.0	56.0
R20 (20 bps)	77.1	22.9	74.5	25.5
R50 (50 bps)	73.8	26.2	55.3	44.7

Table A.6: Shapley Decomposition: Demand Complexity vs. Communication (Point vs. All Others)

Notes: Shapley decomposition of predicted rounding into demand (complexity) and communication (Point vs. all others) shares, repeated across rounding definitions. Shares sum to 100. Forecasts of exactly zero are excluded.

A.5 Individual Fixed Effects Estimates of Tenure and Rounding

Table A.7 re-estimates the relationship between survey tenure and rounding using individual fixed effects, which identify the tenure coefficients entirely from within-person variation over time. The results confirm that the decline in rounding documented in Table 7 is not driven by selective attrition from the panel. The same individual rounds less as she gains experience: the predicted probability of rounding falls from 42.4% in a respondent's first month to 31.4% after ten or more months in the panel, closely mirroring the probit estimates.

Table A.7: Rounding and Survey Tenure: Individual Fixed Effects

	Rounded to 5
Months 1–3	-0.065^{***}
	(0.003)
Months 4–6	-0.091^{***}
	(0.004)
Months 7–9	-0.104^{***}
	(0.004)
Months 10–12	-0.109^{***}
	(0.004)
Predicted Pr(Round | First Month)	0.424
Predicted Pr(Round | Months 10–12)	0.314
Individual FE	Yes
Observations	178,252
Individuals	23,694
R^2 (within)	0.008

Notes: LPM estimates with individual fixed effects. The dependent variable is an indicator for rounding to the nearest multiple of 5. Survey tenure groups are defined by months in the SCE panel, with the respondent's first month as athe omitted category. Standard errors clustered at the individual level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.6 Environmental Complexity and Rounding: Robustness

This section reports robustness checks for the environmental complexity results presented in 4.2. There, our main specification uses random effects to accommodate year–month fixed effects alongside the monthly-level volatility indicator included with a single lag.

Here, we consider two alternative specifications. First, we replace the random effects estimator with individual fixed effects (without year–month fixed effects, since the volatility indicator varies only at the monthly level and would be absorbed). The results, shown in Table A.8, are qualitatively identical: months following an unusually large absolute change in PCE inflation are associated with a 6.2 percentage point increase in the probability of reporting a rounded forecast ($p < 0.001$), and months following a period of elevated 3-month rolling volatility are associated with a 3.4 percentage point increase ($p < 0.001$). The within-person tenure gradient remains large and precisely estimated in all columns. Results for rounding to multiples of 10 are similar in sign and significance, with predictably smaller magnitudes.

Second, we consider alternative lag structures for the volatility indicator. Our preferred specification lags the indicator by one month to capture respondents reacting to recently observed price changes. Appendix A.9 varies this timing choice. The contemporaneous indicator yields a coefficient of 8.6 percentage points, consistent with the strongest signal coming from the most recent price data. The one-month lag, our baseline specification, produces an estimate of 6.2 percentage points, and the two-month lag remains significant at 3.2 percentage points but is attenuated as the shock recedes from salience. The monotonically declining pattern across lags is consistent with respondents reacting to salient recent price movements and supports the use of a single lag in the main specification.

Table A.8: Environmental Complexity and Rounding: Individual Fixed Effects

	Rounded to 5		Rounded to 10	
	Month-to-month (1)	3-month rolling (2)	Month-to-month (3)	3-month rolling (4)
HighVol_{t-1}	0.062***	0.034***	0.035***	0.031***
	(0.007)	(0.006)	(0.006)	(0.005)
Months 1–3	−0.066***	−0.066***	−0.032***	−0.032***
	(0.004)	(0.004)	(0.003)	(0.003)
Months 4–6	−0.091***	−0.091***	−0.044***	−0.044***
	(0.004)	(0.004)	(0.004)	(0.004)
Months 7–9	−0.104***	−0.104***	−0.052***	−0.052***
	(0.004)	(0.004)	(0.004)	(0.004)
Months 10–12	−0.109***	−0.109***	−0.054***	−0.054***
	(0.005)	(0.005)	(0.004)	(0.004)
Individual FE	Yes	Yes	Yes	Yes
Year–month FE	No	No	No	No
Observations	177,014	177,014	177,014	177,014
Respondents	23,455	23,455	23,455	23,455

Notes: Linear probability model with individual fixed effects. The dependent variable is an indicator for whether the respondent's one-year-ahead inflation forecast is a multiple of 5 (Columns 1–2) or 10 (Columns 3–4). HighVol_{t-1} is a binary indicator equal to one if the relevant volatility measure in month $t-1$ exceeds one standard deviation above its sample mean. Columns (1) and (3) use the absolute month-to-month change in headline PCE inflation; Columns (2) and (4) use the rolling 3-month standard deviation. The omitted tenure category is the respondent's first month. Standard errors clustered at the respondent level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Alternative Lag Structures for the Volatility Indicator

	Contemporaneous (1)	One-month lag (2)	Two-month lag (3)
Month-to-month			
HighVol	0.086***	0.062***	0.032***
	(0.007)	(0.007)	(0.007)
Observations	178,252	177,014	175,821
3-month rolling			
HighVol	0.055***	0.034***	−0.003
	(0.006)	(0.006)	(0.006)
Observations	175,821	174,063	172,537
Individual FE	Yes	Yes	Yes
Tenure controls	Yes	Yes	Yes

Notes: Linear probability model with individual fixed effects. The dependent variable is an indicator for rounding to the nearest 5. HighVol is a binary indicator equal to one if the relevant volatility measure exceeds one standard deviation above its sample mean. The month-to-month measure uses the absolute change in headline PCE inflation; the 3-month rolling measure uses the rolling standard deviation. The 3-month rolling measure has fewer observations because its construction requires three months of PCE data, and the PCE series begins in June 2013. All specifications include tenure group controls. Standard errors clustered at the respondent level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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A.7 Robustness of Rounding and Inflation Bias Estimates

Table A.10 re-estimates Equation 8 under three specifications: the baseline random-effects GLS from the main text, individual fixed effects, and pooled OLS with month-year fixed effects. All columns include demographic controls (where estimable) and cluster standard errors at the respondent level. The key result is stable across specifications. Rounded forecasts are substantially higher than non-rounded forecasts, and this rounding premium declines sharply with survey tenure. The rounded × tenure interactions, which are the primary object of interest, are large, negative, and significant at the 1% level in every column.

Table A.10: Rounding and Inflation Bias: Specification Robustness

	(1) RE GLS	(2) Individual FE	(3) Pooled OLS
Rounded (×5)	6.887***	6.271***	8.316***
	(0.103)	(0.114)	(0.109)
Tenure effects			
Months 1–3	−0.043	0.115**	−0.175***
	(0.043)	(0.053)	(0.046)
Months 4–6	−0.076*	0.383***	−0.272***
	(0.045)	(0.084)	(0.049)
Months 7–9	−0.086*	0.687***	−0.260***
	(0.047)	(0.123)	(0.051)
Months 10–12	−0.067	0.978***	−0.239***
	(0.051)	(0.157)	(0.054)
Rounded × Tenure			
× Months 1–3	−2.150***	−2.006***	−1.633***
	(0.112)	(0.117)	(0.124)
× Months 4–6	−3.195***	−3.043***	−2.245***
	(0.121)	(0.127)	(0.136)
× Months 7–9	−3.589***	−3.420***	−2.559***
	(0.125)	(0.131)	(0.143)
× Months 10–12	−3.848***	−3.658***	−2.805***
	(0.137)	(0.142)	(0.158)
Month-year FE	Yes	Yes	Yes
Demographic controls	Yes	–	Yes
Individual effects	RE	FE	–
N	175,974	175,974	175,974
Individuals	23,357	23,357	23,357

Notes: Dependent variable is the one-year-ahead inflation forecast in percentage points. Tenure reference category is the first survey month. Demographic controls include categorical indicators for age, education, and household income; these are absorbed by individual fixed effects in column (2). Column (1) estimated via random-effects GLS; column (2) via within-estimator; column (3) via pooled OLS. Standard errors clustered at the respondent level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B Experimental Instructions

EXPERIMENTAL STUDY OF ECONOMIC DECISION MAKING

Welcome! You are here to participate in an economic experiment. If you read these instructions carefully and make appropriate decisions, you may earn a considerable amount of money. We will pay you this money in cash immediately after this experiment.

Each of you will earn \$10 for attending. This is your show-up fee. Throughout this experiment you will also earn points based on the decisions you make. Every point you earn is worth an additional \$0.50. We reserve the right to improve the show up fee in your favour if average payoffs are lower than expected.

During the experiment you are not allowed to communicate with other participants. Please raise your hand if you have any questions. An experimenter will answer your questions privately. You will be excluded from the experiment and deprived of all payments aside from the show-up fee if you do not comply with these instructions.

This experiment is based on a simple simulation that approximates fluctuations in a real economy. Your task is to serve as private forecasters and provide real–time forecasts about future inflation in this simulated economy. These instructions will explain what inflation and the interest rate are, how they move around in this economy, and how they depend on your forecasts. We will allow you to practice making forecasts for several unpaid periods before we begin paid periods in this experiment. You will then participate in two sequences of 30 paid periods, for a total of 60 paid periods of play.

In this simulation, households and firms (whose decisions are automated by the computer) will form forecasts identically to yours. So to some degree, outcomes that you will see in the game will depend on the way in which all of you form your forecasts. However, your earnings in this experiment depend on the accuracy of your individual forecasts.

You will also submit a measure of uncertainty about your forecast called your anticipated forecasting error. You will earn money if actual inflation is within the bounds of this error. Otherwise, you will earn nothing.

Please note that all values are given in basis points, a measurement often used in descriptions of the economy. All values can be positive, negative, or zero at any point in time.

Overview of the Economy

In each period, you will submit a forecast of inflation for the next two periods. For example, suppose it is now period 10. Then you will submit a forecast of inflation in period 11 and a forecast of inflation in period 12. By 'forecast of inflation' we mean your best guess of what inflation will be. The more accurate your guess, the more money you will earn.

Your forecasts should be given in basis points. Here are some examples of the relationship between basis points and percentages:

1% = 100 basis points

3.25% = 325 basis points

-0.5% = -50 basis points

-4.8% = -480 basis points

You can submit any forecast you wish, positive or negative or zero, but please only submit integers.

The economy consists of three main variables:

- **Inflation** – Inflation is the change in price that occurs between two periods.
- **Interest Rates** – The interest rate is the amount of money that people earn on savings. A higher interest rate entices consumers to save more and spend less on consumption. Thus, a higher interest rate puts downward pressure on inflation.
- **Shocks** – Shocks are changes to the amount consumers in the economy wish to purchase. Shocks change every period and are influenced by a random component and by past shocks. A positive shock today increases inflation today and vice versa.

Your goal in this experiment is to forecast future inflation as accurately as possible. Thus, we now provide detailed explanations of the factors that influence inflation and the relationships between the different variables in the economy.

Shocks:

Intuitively, you can think of shocks as weather shocks. Over the long run, the weather has no effect on how much consumers want to buy. However, from day to day, there may be random changes to the weather that do influence what people do and buy. You can think of a positive shock as unexpectedly nice weather. When the weather is especially nice, consumers are spending more time out of their homes and increasing their expenditures (for example, buying ice cream, going out for a nice dinner, or going to the beach). A negative shock can be thought of as unexpectedly terrible weather. This bad weather makes it so that people do now want to leave their homes, causing expenditures to be relatively low. Gradually, the shocks, like weather, will revert back to their long-run levels. As the shocks dissipate, new random events occur that will make consumers want to increase or decrease their spending. Shocks will have a precise value and will be displayed on your screen.

Whenever a positive shock occurs and spending increases, this will put upward pressure on prices (i.e. upward pressure on inflation). Conversely, a negative shock will put downward pressure on prices (i.e. downward pressure on inflation).

We calculate the values of a shock in each period as follows:

$$Shock_p = 0.57(Shock_{p-1}) + RandomComponent_p$$

- The random component is 0 on average
- Roughly two out of three times the shock will be between -138 and 138 basis points.
- 95% of the time the shock will be between -276 and 276 basis points

For example, shocks may evolve as follows:

$$\begin{aligned}
Shock_1 &= 30 \\
Shock_2 &= 30 \times 0.57 + New\ Draw \\
&= 17.1 + (newdraw) \\
Shock_2 &= 17.1 + (-150) \\
&= -132.9 \\
Shock_3 &= -132.9 \times .57 + New\ Draw \\
&= ...
\end{aligned}$$

Interest Rates: The central bank in this economy will adjust the nominal interest rate in each period to keep inflation as close to zero as possible. As inflation increases, the central bank will increase the nominal interest rate more than one-for-one with inflation. An increase in the nominal interest rate has a direct negative effect on consumer demand and production, and an indirect negative effect on inflation. Importantly, you will not observe the current interest rate when you are forming your inflation forecasts. After you submit your forecasts, the computer will solve for the current period's inflation using the median forecasts from all subjects in the room and the current-period shock shock (which you will see). It is important for you to realize that, even though the central bank is aiming for zero inflation, it will rarely accomplish this. This is because of the random shocks that occur in each period and the public's expectations. However, the central bank will keep the economy more stable than the economy would be in the absence of the central bank.

How the economy evolves:

Each period, you and the other forecasters in this room will submit your beliefs about inflation for the next period and the period after that. To be clear, if we are in period 10, you will submit an inflation forecast for period 11 and for period 12. The software will select the median of each of the two forecasts as the aggregate forecasts. The software uses the median, rather than the average forecast, so that a small number of subjects cannot have a significant effect on the economy.

These aggregate forecasts play an important role in determining inflation today. This is because inflation today is determined largely by aggregate forecasts about future inflation. If the majority of forecasters expect relatively high inflation tomorrow, then inflation today will be higher. The idea behind this is simple: If the professional forecasters communicate to the public that inflation is likely to rise tomorrow, consumers will spend more immediately to avoid paying the relatively higher prices tomorrow. This increase in demand today will cause prices to start rising today, and so inflation will increase today. Likewise, if the median forecaster predicts higher inflation for two days from now, households will need to have a bit more money tomorrow than they would otherwise to avoid paying the higher prices predicted for two days from now.

More precisely, inflation and interest rates evolve according to the following equations:

$$\begin{aligned}
\text{Inflation}_t &= 1.54(\text{Median forecast of Inflation}_{t+1}) - 0.58(\text{Median forecast of Inflation}_{t+2}) \\
&\quad +0.08(\text{Shock}_t) \\
\text{Interest Rate}_t &= 4.44(\text{Median forecast of Inflation}_{t+1}) - 3.12(\text{Median forecast of Inflation}_{t+2}) \\
&\quad +0.41(\text{Shock}_t)
\end{aligned}$$

Important information about this economy:

- The Central Bank sets the target inflation at zero. In order to achieve this target it will adjust the nominal interest rate in each period. In some cases the nominal interest rate can become negative.

- Expectations about tomorrow (if in period 10, this is your forecast for period 11) are self-fulfilling in this economy. If you forecast higher inflation tomorrow then inflation will grow higher in the current period. Similarly, a median forecast of lower inflation tomorrow will cause inflation to fall in the current period.

- Expectations about two days from now (if in period 10, this is your forecast for period 12) relate negatively to inflation today. If you forecast higher inflation for two-days from now, then inflation today will fall. If instead you forecast lower inflation for two days from now, inflation today will increase.

Score

Your forecasting score in each period will depend on the accuracy of the forecasts you formed in the previous two periods. At the end of each period, the software will evaluate how accurate your forecasts from one- and two-periods ago were about the inflation rate in the current period. The difference between these numbers forms your absolute forecast error. The larger this absolute error, the lower is your forecasting score in that period. The letter p in the following example stands for 'period'.

- Absolute Forecast Error = $\|$Your Forecast $-$ Actual Value$\|$
- Total $\text{Score}_p = 0.3(2^{-AbsoluteForecastError_{p-1}} + 2^{-AbsoluteForecastError_{p-2}})$

The maximum score you can earn for forecasting in each period is 0.60 points. Your score will decrease exponentially as your forecast error increases. Suppose your forecast errors for inflation is:

1. 0: Your score will be 0.6
2. 50: Your score will be 0.42
3. 100: Your score will be 0.30
4. 200: Your score will be 0.15
5. 300: Your score will be 0.075
6. 500: Your score will be 0.02
7. 1000: Your score will be 0

8. 2000: Your score will be 0

Making decisions in this experiment

During this experiment, your main screen will display information that will help you make forecasts and earn more points.

At the top left of the screen, you will see your subject number, the current period, time remaining, and the total number of points you've earned through the previous period. You will also see three history plots on your screen.

The top history plot displays past interest rates and current and past shocks.

The second history plot shows your 1-period-ahead points forecasts of inflation (blue dots), error bands that you create with your anticipated forecasting error (blue shading centered around your point forecasts) and actual inflation (red dots). Note that the difference between your forecasts of one-period-ahead inflation (blue dots) and the actual levels of inflation (red dots) constitutes your one-period-ahead forecast error in past periods.

The third history plot shows your 2-period-ahead point forecasts of inflation (orange dots), error bands that you create with your anticipated forecasting error (orange shading centered around your point forecasts), and actual inflation (red dots). Note that the difference between your forecasts of two-period-ahead inflation (orange dots) and the actual levels of inflation (red dots) constitute your two-period-ahead forecast error in past periods.

Note: this section read one of three ways depending upon treatment:

For NoComm, skip directly to "You have 65 seconds...

For Point treatments:
Both the second and third plots also contain the Central Bank's forecast of inflation for the next five periods (green). It is important to remember that the projections are simply a forecast and not a promise. The Central Bank uses the model discussed earlier in these instructions, and the current and expected future shocks, to form its projections. In particular, it predicts that the economy will return to zero levels of inflation in the near future.

For Point&Density treatments:
Both the second and third plots also contain the Central Bank's forecast of inflation for the next five periods (green). This forecasts also includes green shading, which represents

the Central Bank's level of uncertainty for its corresponding point projections. These bands will contain the correct realization of inflation about 66% of the time. It is important to remember that the projections are simply a forecast and not a promise. The Central Bank uses the model discussed earlier in these instructions, and the current and expected future shocks, to form its projections. In particular, it predicts that the economy will return to zero levels of inflation in the near future.

You have 65 seconds to make decisions in the first nine periods and only 50 seconds thereafter. You may submit both negative and positive forecasts and forecasts of 0. Please review your forecasts before pressing the SUBMIT button because you cannot revise your forecasts afterward.

The anticipated forecast error:

You must also submit a measure of how uncertain you are about your inflation forecasts. We call this your anticipated forecasting error.Note this value should always be positive and your error bounds are centered around your point forecast.

Suppose you forecast inflation tomorrow to be 10 basis points but feel more confident that actual inflation will fall between 5 and 15 basis points. You should indicate this by submitting an anticipated forecasting error of 5. This forms anticipated error bounds of 5 to 15 since $10 - 5 = 5$ and $10 + 5 = 15$. If actual inflation is any number from 5 to 15, we pay you. Otherwise, you earn nothing for this anticipated forecasting error.

If actual inflation falls within your anticipated forecast error bounds, then we pay your anticipated forecast error according to the following function:

$$AnticipatedErrorEarnings = \frac{15}{10+anticipated\ error}$$

Notice that your earnings for your anticipated forecast error decrease as your anticipated forecast error increases. However, it is important for you to understand that we pay you this amount ONLY if the realized value of inflation lies inside your anticipated forecasting error bands. If actual inflation is outside your anticipated forecasting error bands, then you earn 0 points for providing your anticipated forecasting error.

An example: Suppose it is period 3. Suppose in periods 1 and 2 you provided an inflation forecast of 10 basis points for period 3 inflation. Suppose your anticipated forecasting error in period 1 was 5 and in period 2 it was 10. Then your error bounds for period 1 are 5 to 15 and for period 2 are 0 to 20. Suppose actual inflation at the end of period 3 is 17. Then you earn 0 points for your anticipated forecast error provided in period 1. This is because 17 is not between 5 and 15. However, you would earn $\frac{15}{10+10} = .75$ points for your anticipated forecast error provided in period 2, since 17 is between 0 and 20.

Our software will randomly select (with equal probability) to pay you for **either** your point forecasts **or** for your anticipated forecast error in each period of play. **We will never pay for both in a single period**.

B.1 Screenshots

Figure B.6: NoComm screenshot

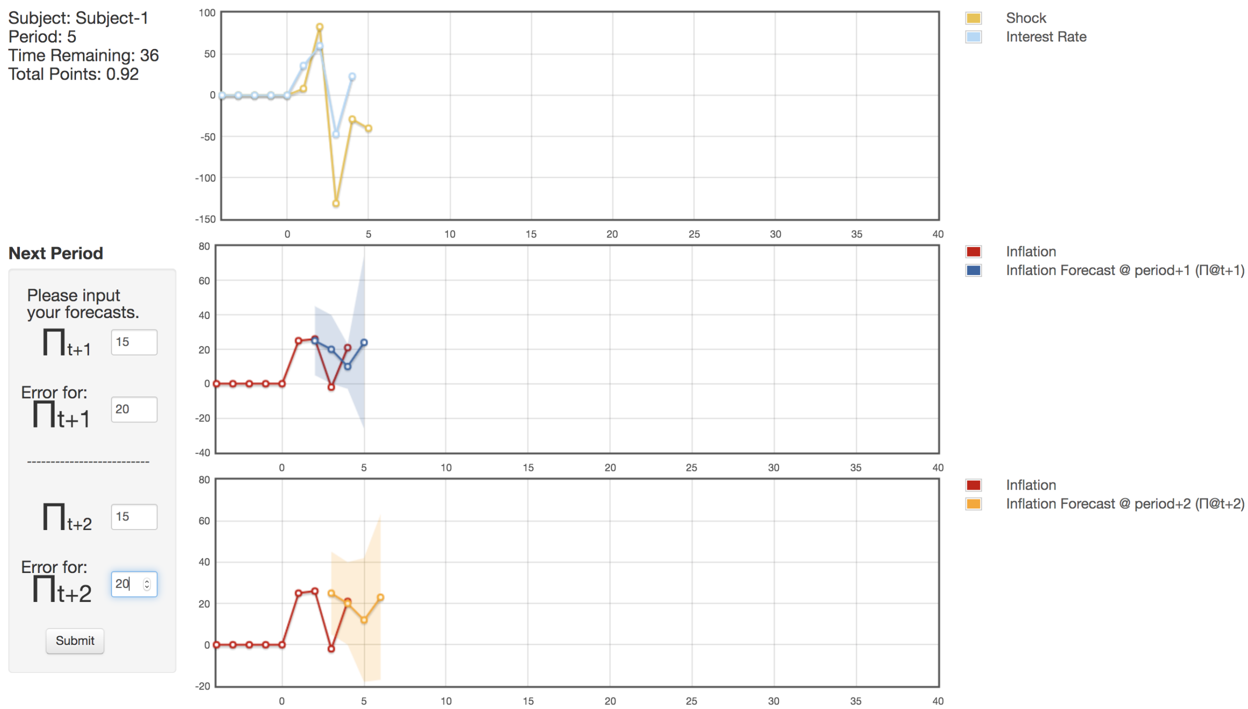

Figure B.7: Point screenshot

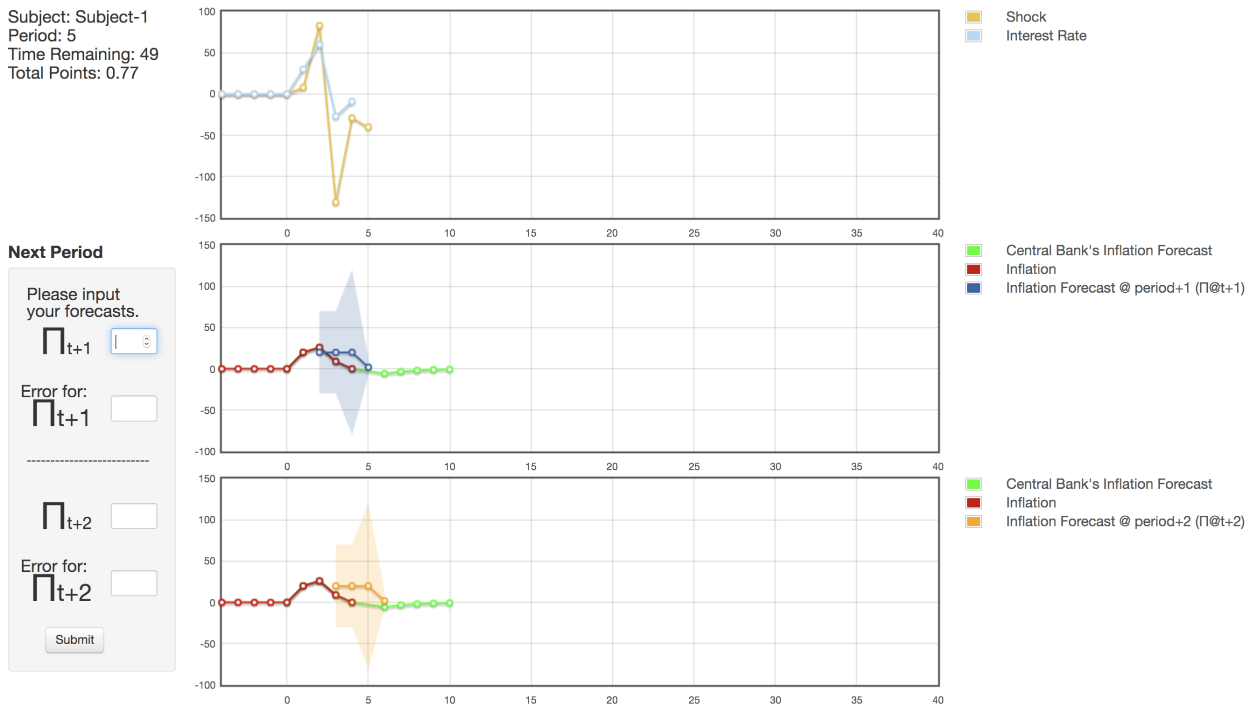

Figure B.8: Point&Density screenshot