Attention-Dependent Monetary Transmission to Household Beliefs*

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Abstract

The expectations channel of monetary policy is state dependent because households endogenously adjust attention to macroeconomic conditions. We develop a behavioral framework in which attention trades off forecast accuracy against cognitive cost, so monetary policy operates through an *expectations multiplier*. Using the Michigan Survey, we proxy attentiveness from whether households' reading of business conditions matches realized outcomes, measured before identified policy shocks arrive. Policy news moves inflation beliefs primarily among attentive households, especially those with greater economic exposure; others barely respond. In the aggregate, pass-through scales with attentiveness and strengthens in recessions and high-uncertainty periods, making monetary transmission nonlinear.

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1 Introduction

Central banks rely heavily on expectations as a key channel of monetary transmission. Yet the empirical strength of this channel varies widely across studies and over time, raising a fundamental question: when does monetary policy actually move household expectations—and through whom? This paper shows that the expectations channel of monetary policy is inherently state dependent because households endogenously adjust attention to macroeconomic conditions. Identical policy actions can generate sharply different belief responses depending on how much attention households devote to economic news.

We develop a minimal behavioral framework, following Gabaix (2020), in which households endogenously choose attention prior to the arrival of shocks and form expectations as an attention-weighted combination of a long-run anchor and a fully informed forecast. Attention balances forecast accuracy against cognitive costs and rises when macroeconomic risks increase. The model delivers a simple implication: monetary policy operates through an expectations multiplier. At the micro level, policy news should move beliefs primarily among attentive households; in the aggregate, pass-through should scale with economy-wide attentiveness and strengthen when uncertainty raises attention.

We test these predictions using the Michigan Survey of Consumers combined with externally identified monetary policy shocks. Exploiting the survey's rotating panel, we construct a predetermined measure of household attentiveness by benchmarking respondents' assessments of recent business conditions against realized macro outcomes, identifying which households are attentive before policy news arrives. A battery of checks supports reading this measure as attention rather than a confound: it is driven by the aggregate macroeconomic state—large movements in unemployment and financial stress—not by households' personal circumstances, and comoves with independent salience proxies outside the survey. Because the proxy is nonetheless noisy, we quantify the resulting measurement error and show that it biases our estimates toward zero rather than generating spurious results. We then trace how monetary policy surprises propagate into expectations at both the micro and aggregate levels.

Four main findings emerge. First, monetary policy shocks affect inflation expectations primarily among attentive households—those whose prior assessments of business conditions closely matched realized macroeconomic outcomes. Households classified as non-attentive, either because their assessments were inaccurate or because they reported not having heard about recent economic developments, show near-zero belief responses. This result is robust to alternative accuracy thresholds, and to the inclusion of rich survey-based controls for consumer sentiment and macroeconomic perceptions. Because the accuracy proxy is measured with noise—fewer than half of respondents (about 46%) receive the same classification across survey waves—our OLS estimates are attenuated, implying that the true attention-gating effect is substantially larger than the baseline point estimate. Second, in time series, aggregate pass-through varies with economy-wide attentiveness: contractionary policy shocks generate much larger downward revisions in inflation expectations during high-attention periods than in low-attention periods. Third, this expectations multiplier is itself state-dependent. Pass-through strengthens markedly in recessions and periods of elevated real or financial uncertainty, reflecting endogenous increases in attention. Fourth, conditional on attention, belief responses are largest among households with greater economic exposure—stockholders, homeowners, and higher-stakes demographic groups—indicating that these groups disproportionately transmit monetary policy into aggregate expectations.

These belief responses have real economic content. Attentive households do not merely revise inflation expectations; they also downgrade their labor-market and durable-spending outlook following contractionary shocks. This joint updating implies that attention governs not only nominal beliefs but also households' broader macroeconomic outlook, amplifying perceived real interest rates and strengthening the contractionary impulse on demand.

Our findings contribute to the literature on state-dependent monetary transmission. Prior work emphasizes nonlinear price adjustment (Vavra, 2014) and broader nonlinear propagation mechanisms (Tenreyro and Thwaites, 2016, Alpanda, Granziera and Zubairy, 2021). We highlight a complementary informational channel: in volatile environments, agents endogenously raise attention, amplifying the expectations response to policy. Using cross-country RCTs, Weber, Candia, Afrouzi, Ropele, Lluberas, Frache, Meyer, Kumar, Gorodnichenko, Georgarakos, Coibion, Kenny and Ponce (2025) show that both households and firms grow more attentive as inflation rises, becoming less responsive to exogenously provided information. On the firm side, countercyclical attention generates state-dependent monetary non-neutrality (Flynn and Sastry, 2024, Song and Stern, 2024). A complementary strand documents how firm-level characteristics shape attention and thereby the strength of price and expectation responses to policy (Yang, 2022, Afrouzi, 2024, Wu, 2024). We provide a household-level counterpart along both dimensions, showing that pre-determined attentiveness gates the pass-through of policy shocks to individual beliefs and that households with greater economic exposure account for a disproportionate share of belief transmission.

The paper also bridges theories of inattentive expectations with empirical evidence on monetary policy communication. On the theoretical side, our framework nests classic information frictions—

sticky information and rational inattention (Mankiw and Reis, 2002, Sims, 2003, Maćkowiak and Wiederholt, 2009, Afrouzi and Yang, 2021)—within the behavioral expectations operator of Gabaix (2020), and relates to broader bounded-rationality approaches (Angeletos and Lian, 2018, Bordalo, Gennaioli and Shleifer, 2018), generating sharp predictions about when policy news affects beliefs. On the empirical side, we build on work documenting limited information and learning among households and firms (Coibion and Gorodnichenko, 2015a, Candia, Coibion and Gorodnichenko, 2024, Afrouzi, Flynn and Yang, 2024), the effects of central bank communication on household beliefs (Carvalho and Nechio, 2014, Lamla and Vinogradov, 2019, Claus and Nguyen, 2020, Kryvtsov and Petersen, 2021, Coibion, Gorodnichenko and Weber, 2022, Bauer, Pflueger and Sunderam, 2024), and experience-based expectation formation (Malmendier and Nagel, 2016, Cavallo, Cruces and Perez-Truglia, 2017, DAcunto, Malmendier, Ospina and Weber, 2021). Our contribution is to link these strands by measuring attentiveness prior to policy announcements and showing that it governs who updates, by how much, and when—yielding a state-dependent expectations multiplier.

Household and firm attentiveness to inflation has been measured in several complementary ways. One strand uses "revealed attention" from search behavior and news supply, such as internet search for inflation-related queries and counts of inflation articles in major outlets (Kumar, Coibion, Afrouzi and Gorodnichenko, 2015, Marcellino and Stevanovic, 2022, Korenok, Munro and Chen, 2023). Pfäuti (2024) infers attention from updating behavior, estimating a time-varying attention parameter from how strongly short-run inflation expectations load on recent inflation, and classifying "high-attention" regimes when this responsiveness exceeds an estimated threshold. Kroner (2025) introduces a complementary pre-announcement index of investor attention around CPI releases, aggregating news coverage, media mentions, and Google search intensity into a CPI-attention measure that predicts market reactions. Micro-based approaches complement these aggregates by inferring attentiveness directly from survey behavior (*e.g.,* Braitsch and Mitchell, 2022, Song and Stern, 2024). In particular, Bracha and Tang (2024) proxy inattention from the MSC's two-step inflation module: among respondents who first say prices will "stay the same," low attention is flagged if they answer "don't know" at the numeric follow-up or, if they give a number, when it departs substantially from contemporaneous inflation. Relative to this work, we provide a direct, policy-linked mapping from pre-determined household attentiveness to externally identified monetary policy shocks, and we show that the pass-through of policy news to beliefs is systematically stronger when attention is higher.

The remainder of the paper proceeds as follows. Section 2 presents the behavioral expectations model and testable implications. Section 3 describes the data and measurement of attentiveness.

Section 4 reports the main empirical results. Section 5 provides robustness checks. Section 6 concludes.

2 Behavioral Expectations with Endogenous Attention

This section develops a minimal framework in which household attention endogenously governs the strength of the expectations channel of monetary policy. We build on the bounded rational expectations operator of Gabaix (2020). Households choose attention prior to the realization of macroeconomic shocks, trading off forecast accuracy against cognitive costs. Attention determines the weight on fully informed forecasts, scaling how strongly beliefs respond to policy news.

The model delivers four testable implications. First, monetary policy affects individual inflation expectations only to the extent that households are attentive, implying that attention gates pass-through at the micro level. Second, aggregating across households yields an expectations multiplier: the time-series response of aggregate inflation beliefs is proportional to the economy's average attentiveness. Third, increases in payoff-relevant uncertainty raise attention and therefore amplify the expectations response to monetary policy, generating endogenous state dependence. Fourth, groups for whom information is more valuable or less costly—such as households with greater financial exposure—exhibit larger belief responses.

2.1. Setup

Timing. At each date t, household i chooses an attention level $m_{i,t} \in [0,1]$ and forms a behavioral one-year-ahead inflation expectation. Attention is chosen *before* the period-$t+1$ shocks realize. Monetary policy and other macroeconomic "news" then arrive *between* the two dates. At date $t+1$, the household forms an updated behavioral expectation, incorporating the intervening news through its predetermined attention weight.

Forecast target and news. Households forecast next-period inflation π_{t+1} (the one-year-ahead rate), and we write $\mathbb{E}_t[\pi_{t+1}]$ for its fully informed (rational) forecast given date-t information. Between dates t and $t+1$, monetary policy and other macroeconomic news arrive and move this forecast; by date $t+1$ the target is realized, so $\mathbb{E}_{t+1}[\pi_{t+1}] = \pi_{t+1}$. The change in the fully informed forecast across dates therefore equals the intervening news, which we record as a *news identity*:

$$\mathbb{E}_{t+1}[\pi_{t+1}] - \mathbb{E}_t[\pi_{t+1}] \;=\; \theta\,\varepsilon^{mp}_{t+1} + \Gamma'\varepsilon^{o}_{t+1},$$

where ε^{mp}_{t+1} is the monetary policy surprise and $\varepsilon^{o}_{t+1} \in \mathbb{R}^K$ stacks the other contemporaneous disturbances (*e.g.*, markup, energy/import prices, wage growth, commodity, tax changes). The scalar θ

and vector $\Gamma = (\gamma_1, \ldots, \gamma_K)'$ are *semi-elasticities* mapping standardized innovations into the forecast; we adopt the sign convention that a contractionary monetary policy surprise lowers expected inflation, so that $\theta < 0$. The main analysis uses only this reduced-form news process.

We normalize the innovations to be mean-zero Gaussian and i.i.d. across periods:

$$\varepsilon^{mp}_{t+1} \sim \mathcal{N}(0,1), \qquad \varepsilon^{o}_{t+1} \sim \mathcal{N}\big(0,\, \Sigma_{o,t}\big),$$

where $\Sigma_{o,t}$ is a $K \times K$ positive semidefinite covariance matrix with ones on the diagonal that summarizes the macro-uncertainty regime prevailing at date t. Unless stated otherwise, we assume $\mathrm{Cov}_t(\varepsilon^{mp}_{t+1}, \varepsilon^{o}_{t+1}) = 0$ within the identification window; off-diagonal elements of $\Sigma_{o,t}$ allow contemporaneous correlation among non-MP shocks.[1]

Behavioral expectations and attention choice. Household i forms a behavioral expectation by blending a coarse long-run anchor $\bar{\pi}$ (the steady-state inflation rate, a costless default) with the fully informed forecast:

$$\mathbb{E}^{B}_{i,t}\big[\pi_{t+1}\big] \;=\; (1 - m_{i,t})\,\bar{\pi} \;+\; m_{i,t}\,\mathbb{E}_t\big[\pi_{t+1}\big],$$

where $\mathbb{E}_t[\cdot]$ is the full-information conditional expectation.[2] Placing weight $1 - m_{i,t}$ on the anchor rather than the fully informed forecast leaves household i with the forecast error

$$\mathbb{E}^{B}_{i,t}\big[\pi_{t+1}\big] - \mathbb{E}_t\big[\pi_{t+1}\big] \;=\; (1 - m_{i,t})\big(\bar{\pi} - \mathbb{E}_t[\pi_{t+1}]\big),$$

whose square is $(1 - m_{i,t})^2\, U_t$, where

$$U_t \;\equiv\; \big(\mathbb{E}_t[\pi_{t+1}] - \bar{\pi}\big)^2$$

is the squared deviation of the fully informed forecast from the anchor.[3] Because the household's spending and saving decisions depend on this forecast, a forecast error reduces welfare; approximating that welfare loss to second order in the forecast error makes it quadratic, with curvature $\omega_i > 0$ measuring the benefit of being informed. Using the forecast error above, the welfare loss from inattention is $\frac{1}{2}\,\omega_i\,(1 - m_{i,t})^2\, U_t$—the benefit-weighted counterpart of the square derived

[1] Any unconditional variances can be absorbed into (θ, Γ). Time variation in $\Sigma_{o,t}$ captures changing macro uncertainty across states of the world.

[2] For simplicity, we model the long-run anchor $\bar{\pi}$ as fixed. This assumption could be relaxed to a time-varying anchor, $\bar{\pi}_t$, to account for potential shifts in the inflation regime. Our model's core mechanism remains unchanged, as the household's behavioral expectation in Equation (2.1) would simply become $\mathbb{E}^{B}_{i,t}\big[\pi_{t+1}\big] \;=\; (1 - m_{i,t})\,\bar{\pi}_t \;+\; m_{i,t}\,\mathbb{E}_t\big[\pi_{t+1}\big]$. The key prediction—that the pass-through of a shock ε^{mp}_{t+1}, which represents news relative to the current anchor, is scaled by attention $m_{i,t}$—is robust to this extension.

[3] Attention is chosen before the next-period shock ε^{mp}_{t+1} realizes but with knowledge of the date-t forecast $\mathbb{E}_t[\pi_{t+1}]$, so the deviation $\mathbb{E}_t[\pi_{t+1}] - \bar{\pi}$ is predetermined when $m_{i,t}$ is chosen. Defining U_t as this squared deviation—rather than as a variance—thus requires neither a conditioning convention nor a zero-mean assumption, and the forecast-error loss below involves no further expectation.

above. Weighing this against a convex cost of attention, which we take to be quadratic ($\frac{\kappa_i}{2}\, m_{i,t}^2$), the household chooses $m_{i,t}$ to minimize their sum:[4]

$$m_{i,t} \;=\; \arg\min_{m\in[0,1]} \frac{1}{2}\,\omega_i\, U_t\,(1-m)^2 + \frac{\kappa_i}{2}\, m^2,$$

with closed-form solution

$$m_{i,t}^*(U_t) \;=\; \frac{\omega_i\, U_t}{\omega_i\, U_t + \kappa_i} \;\in\; [0,1].$$

The payoff-relevant term U_t summarizes macroeconomic uncertainty: it is larger when the fully informed forecast strays further from the anchor, which occurs when macroeconomic news is more volatile (of order $\theta^2 + \Gamma'\Sigma_{o,t}\Gamma$).[5]

Optimal attention $m_{i,t}^*$ rises when macroeconomic conditions are more volatile, so the fully informed forecast strays further from the anchor (larger U_t, $\partial m_{i,t}^*/\partial U_t > 0$), when attention is more valuable for the household (higher ω_i, $\partial m_{i,t}^*/\partial \omega_i > 0$), and falls when attention is more costly (higher κ_i, $\partial m_{i,t}^*/\partial \kappa_i < 0$).

2.2. Testable Implications

We now characterize individual and aggregate responses to a contractionary MP surprise ($\varepsilon_{t+1}^{mp} > 0$ with $\theta < 0$). Proofs are deferred to Appendix A.

Proposition 1 (Attention Gates Individual Pass-Through)**.** *Consider the revision in household i's behavioral expectation between the two dates. The revision attributable to the news arriving in between is*

$$\Delta\pi_{i,t+1}^e \;\equiv\; \mathbb{E}_{i,t+1}^B[\pi_{t+1}] - \mathbb{E}_{i,t}^B[\pi_{t+1}] \;=\; m_{i,t}^*(U_t)\big(\theta\,\varepsilon_{t+1}^{mp} + \Gamma'\varepsilon_{t+1}^o\big),$$

so that the marginal effect of the monetary policy surprise on the revision is

$$\frac{\partial\,\Delta\pi_{i,t+1}^e}{\partial\,\varepsilon_{t+1}^{mp}} \;=\; \theta\, m_{i,t}^*(U_t).$$

Proof. See Appendix A.1. ∎

Proposition 1 establishes the micro-level foundation of the expectations multiplier: the response

[4]The payoff parameter ω_i can be micro-founded by linking it to household economic decisions. For instance, in a consumption-saving problem with utility depending on the perceived real interest rate, the loss from mis-forecasting inflation is larger for households with nominally exposed balance sheets (*e.g.*, net nominal assets or mortgage debt), making ω_i an endogenous function of those exposures. For parsimony, we treat ω_i as a reduced-form parameter, which is sufficient for our comparative statics and testable implications.

[5]Let inflation follow the stationary AR(1) process, $\pi_{s+1} = \bar\pi + \rho(\pi_s - \bar\pi) + \theta\,\varepsilon_{s+1}^{mp} + \Gamma'\varepsilon_{s+1}^o$, with persistence $\rho \in [0,1)$ and $\mathrm{Cov}(\varepsilon^{mp}, \varepsilon^o) = 0$. Then $\mathbb{E}_t[\pi_{t+1}] - \bar\pi = \rho(\pi_t - \bar\pi)$, so $U_t = \rho^2(\pi_t - \bar\pi)^2$, whose typical magnitude $\mathbb{E}[U_t] = \rho^2\,\mathrm{Var}(\pi_t - \bar\pi) = \frac{\rho^2}{1-\rho^2}\big(\theta^2 + \Gamma'\Sigma_{o,t}\Gamma\big)$ is increasing in the volatility of both monetary and non-monetary shocks. Our empirical proxies (recessions, financial-market volatility, macro-uncertainty indices) track this conditional scale.

of an individual's inflation expectation to a monetary policy shock is proportional to that household's level of attention. Households with $m^*_{i,t} \approx 0$ are anchored and barely revise in response to policy news, while attentive households revise in proportion to $\theta\, m^*_{i,t}$.

Proposition 2 (The Aggregate Expectations Multiplier)**.** *Let* $\Delta\pi^e_{t+1} \equiv \mathbb{E}_i[\Delta\pi^e_{i,t+1}]$ *denote the cross-sectional mean revision in inflation expectations. Aggregating Equation* (2.1) *across households yields*

$$\Delta\pi^e_{t+1} = \underbrace{\Lambda_t}_{\in[0,1]} \theta\, \varepsilon^{mp}_{t+1} + \Lambda_t \Gamma' \varepsilon^o_{t+1}, \qquad \Lambda_t \equiv \int m^*_{i,t} di,$$

where Λ_t *is the* average attentiveness *in the economy.*

Proof. See Appendix A.2. ■

Proposition 2 formalizes an expectations multiplier: the aggregate response of inflation beliefs to a monetary policy shock is proportional to the economy's average attentiveness. When a larger share of households is attentive, the same policy surprise generates a substantially larger revision in aggregate expectations. We state the exact aggregation identity for the cross-sectional mean; analogous median-based specifications require an additional linearization but preserve the same sign predictions under monotone shifts.

Proposition 3 (Uncertainty-Driven Endogenous Amplification)**.** *Let* $b_{i,t}(U_t) \equiv \partial \Delta\pi^e_{i,t+1}/\partial \varepsilon^{mp}_{t+1} = \theta\, m^*_{i,t}(U_t)$ *denote household i's pass-through coefficient. For* $\theta < 0$,

$$\frac{\partial b_{i,t}(U_t)}{\partial U_t} = \theta\, \frac{m^*_{i,t}(U_t)\big(1 - m^*_{i,t}(U_t)\big)}{U_t} \;<\; 0,$$

so higher U_t *makes the response to a given contractionary shock more negative. If group A is more attentive than group I at each* U_t *(i.e.,* $m_A(U_t) > m_I(U_t)$*), then*

$$\Big|\partial\big(m_A(U_t)\theta\big)/\partial U_t\Big| \;>\; \Big|\partial\big(m_I(U_t)\theta\big)/\partial U_t\Big|$$

whenever $m_A(U_t)\big(1 - m_A(U_t)\big) > m_I(U_t)\big(1 - m_I(U_t)\big)$. *A simple sufficient condition is if both groups' attention is below this peak, i.e.,* $0 \le m_I < m_A \le \frac{1}{2}$.

Proof. See Appendix A.3. ■

Proposition 3 establishes an endogenous amplification mechanism: increases in payoff-relevant uncertainty raise household attention, which in turn magnifies the expectations response to monetary policy shocks. In high-uncertainty states, contractionary policy generates disproportionately larger downward revisions in inflation expectations, particularly among already-attentive agents. The cross-group comparison depends on the shape of $m(1-m)$, so stronger uncertainty sensitivity for more-attentive groups requires the stated condition rather than following mechanically from higher attention alone. This provides a micro-founded channel through which uncertainty endogenously amplifies monetary transmission. Unlike models in which state dependence arises from exogenous changes in price rigidity or shock volatility, here amplification emerges endogenously through households' information acquisition decisions. We emphasize that this is a *cross-regime* comparison: $m^*_{i,t}$ is constant within a stable regime (since U, ω_i, κ_i are time-invariant there), and state dependence arises through shifts in the regime U rather than through within-regime time variation in attention. Empirically, regimes are indexed by lagged indicators of the macro environment, so the prediction is that pass-through is larger in magnitude in high-uncertainty regimes than in low-uncertainty ones.

Proposition 4 (Economic Exposure and Heterogeneous Transmission)**.** *Fix $U_t > 0$. Let households differ only in (ω_i, κ_i) in Equation (2.1)–Equation (2.1). Then:*

1. ***Attention ordering.*** *$m^*_{i,t}(U_t)$ is strictly increasing in ω_i and strictly decreasing in κ_i (i.e., $\partial m^*_{i,t}/\partial\omega_i > 0$ and $\partial m^*_{i,t}/\partial\kappa_i < 0$).*

2. ***Pass-through ordering.*** *The individual MP pass-through magnitude,*
$$\left|\frac{\partial \Delta\pi^e_{i,t+1}}{\partial \varepsilon^{mp}_{t+1}}\right| = |\theta|\, m^*_{i,t}(U_t),$$
is strictly increasing in ω_i and strictly decreasing in κ_i.

3. ***Selection into attentive.*** *For any threshold $\tau \in (0,1)$, the classification indicator $A_{i,t} = \mathbf{1}\{m^*_{i,t} \geq \tau\}$ is weakly increasing in ω_i and weakly decreasing in κ_i.*

Proof. See Appendix A.4. ■

Groups for whom reducing forecast errors is more valuable (higher ω_i) or less costly (lower κ_i) choose higher attention, are more likely to be classified as attentive under any fixed threshold, and display larger MP pass-through. In Section 4.4, we treat homeowners, stockholders, prime-age, and higher-income households as empirical counterparts of higher-ω (and/or lower-κ) groups, and test the corresponding cross-sectional predictions.

Summary and discussion. In sum, the simple behavioral expectations model delivers four testable implications: (i) *attention gates* the impact of MP shocks on individual expectations; (ii) aggregate MP pass-through scales with the economy's *average attentiveness*; (iii) higher payoff-relevant uncertainty strengthens pass-through—especially for already-attentive agents; and (iv) groups with higher payoff from information (larger ω_i) or lower attention costs (smaller κ_i) choose more attention and exhibit larger pass-through.[6]

These core predictions hold under more general conditions. As shown in Appendix B.1, the results do not rely on a linear attention weight or quadratic attention costs; they require only that higher attention places more weight on the fully informed forecast and that the mental cost of attention is convex. Furthermore, the conclusions are robust to introducing a common, noisy public signal about future inflation that arrives before attention is chosen (Appendix B.2). While such a signal can synchronize attention choices and micro-found time variation in aggregate attentiveness, it does not alter the four main implications regarding individual gating, aggregate scaling, uncertainty amplification, or payoff heterogeneity. Because the signal is common, its level effect can be absorbed by time controls in empirical specifications that include them.

3 Data

This section describes the datasets and the construction of our empirical *attentiveness proxy*, which we will take to the tests implied by Section 2. We first outline sources and sample definitions, then construct an individual-level *accuracy* indicator (our proxy for attention in the model), and finally define an aggregate attentiveness index used in our time-series exercises. Section 4 will bring these measures to the micro and aggregate regressions implied by Propositions 1–4.

3.1. Sources and Samples

In this section, we describe the data sources for our main analysis. Appendix C provides further details about the data used for our analysis.

Micro survey and demographics. Our micro data come from the *Michigan Survey of Consumers* (MSC), which interviews a nationally representative sample monthly and re-interviews a rotating panel of respondents roughly six months later. We use the rotating-panel structure to construct revisions in expectations at the individual level and to control for observed heterogeneity (age, income, education, homeownership, stock ownership, gender, region). The MSC provides one-year-ahead inflation expectations and a rich set of qualitative questions on recent business conditions. We focus on the one-year horizon because it is standard for near-term transmission, aligns with

[6] The Gabaix (2020) behavior expectations operator in Equation (2.1) is best read as a steady-state, reduced-form approximation to a dynamic rational-inattention problem in which the household chooses the precision of a noisy signal about the state and updates by Bayes' rule. In that microfoundation the attention weight $m_{i,t}$ is replaced by the steady-state Kalman gain $K_i \in [0,1]$, which plays the identical gating role and inherits the same comparative statics in uncertainty, stakes, and costs. Appendix B.3 develops this noisy-signal/rational-inattention version explicitly and shows it delivers the same four propositions.

our six-month panel and identification window, and is the measure most responsive to contemporaneous macro and policy news in household data (*e.g.*, Cavallo et al., 2017, Coibion et al., 2022, DAcunto, Malmendier and Weber, 2023). Our baseline micro sample spans September 1998 to March 2020, which is the intersection of MSC availability for the necessary items and the availability of our high-frequency monetary policy shocks.[7]

Monetary policy shocks. Our baseline monetary policy surprise (MPS) measure is the series constructed by Bauer, Lakdawala and Mueller (2022). Following the high-frequency identification logic of Nakamura and Steinsson (2018), these surprises capture revisions to the expected policy rate path around FOMC announcements using money market futures data. Specifically, Bauer et al. (2022) construct MPS as the first principal component of daily changes in fed funds and Eurodollar futures rates, with contracts expiring up to one year ahead of the FOMC meeting. We adopt this series as our baseline because it provides a parsimonious summary of changes in the expected near-term policy path and allows for precise alignment of policy news with the timing of household survey interviews. A potential concern with high-frequency futures-based measures is that they may reflect not only unexpected policy actions but also information revealed by the Federal Reserve. To address this concern, we confirm the robustness of our results using alternative monetary policy shock series designed to mitigate such effects. These include the unified monetary policy shocks of Bu, Rogers and Wu (2021), which exploit the full yield curve and heteroskedasticity-based identification, and the reassessed high-frequency shocks of Bauer and Swanson (2023), which orthogonalize standard surprises with respect to pre-announcement macroeconomic information. For longer-run time-series analysis over the Great Moderation (Section 4.2), we additionally use the narrative monetary policy shocks of Romer and Romer (2004).

To ensure consistent interpretation across all specifications, we normalize the shock series. First, we set the sign so that a positive value always represents a contractionary surprise. Second, we scale the series so that a one-unit change corresponds to a one-percentage-point (100 basis point) tightening. This normalization allows our reported regression coefficients to be interpreted directly as the percentage-point response of inflation expectations to a one-percentage-point policy shock.[8] Our analysis uses all identified surprises, both contractionary and expansionary.

Other macro series. We obtain our macroeconomic data from the St. Louis Federal Reserve's FRED database. We use the unemployment rate, Industrial Production (IP), inflation, and the

[7]We drop November 2002 and May 2003 due to missing stock-ownership information. Following Bachmann, Berg and Sims (2015), we trim observations with absolute one-year inflation expectations above 20% to mitigate outliers.

[8]In panel specifications we cumulate the announcement-window shocks from t to $t+5$ to match the six-month interview horizon.

National Financial Conditions Index (NFCI) as either benchmarks for our attentiveness proxy or as contemporaneous controls. For our state-dependence analysis, we use the NBER-dated recession indicator and the CBOE Volatility Index (VIX).

3.2. Measuring Attentiveness: An Accuracy Proxy

Section 2 formalizes attention as a latent weight $m^*_{i,t} \in [0,1]$. In the data, we proxy attentiveness with a *pre-determined* indicator based on each respondent's qualitative assessment of recent business conditions, recorded at the first interview.

Step 1: Perceived business conditions (favorable / unfavorable / no news). At the first interview in month t, each respondent is asked the Michigan Survey's qualitative question on business-conditions news (Question A6): *"During the last few months, have you heard of any favorable or unfavorable changes in business conditions?"* Depending on whether the respondent reports favorable news, unfavorable news, or no news, we code a three-way categorical variable

$$\text{News}_{i,t} \in \{\text{Fav},\ \text{Unfav},\ \text{Haven't heard}\},$$

which records the *sign* of the respondent's perceived business news at time t (or lack of exposure).

Step 2: Benchmark for business conditions. To construct our accuracy benchmark, we seek a macroeconomic indicator that is canonical, widely reported, and maps closely to the survey's phrasing of "changes in business conditions." The unemployment rate is arguably the most salient and easily understood measure of real economic health for the general public. Specifically, we compare perceived favorability with the three-month change in the unemployment rate ($\Delta\text{Unrate}_t \equiv \text{Unrate}_t - \text{Unrate}_{t-3}$) to smooth out high-frequency noise while still capturing the recent economic developments respondents were asked about. While we view this as a natural benchmark, our main findings are robust to using alternative horizons for this change, such as a one-month ($\text{Unrate}_t - \text{Unrate}_{t-1}$) or six-month ($\text{Unrate}_t - \text{Unrate}_{t-6}$), and to using alternative real and financial indicators, as shown in Section 5.

Accuracy classification. We define three mutually exclusive groups at the first interview date t:

$$\text{Accuracy}_{i,t} = \begin{cases} \text{Accurate} & \text{if Fav \& } \Delta\text{Unrate}_t < 0,\ \text{ or Unfav \& } \Delta\text{Unrate}_t \geq 0, \\ \text{Inaccurate} & \text{if Unfav \& } \Delta\text{Unrate}_t < 0,\ \text{ or Fav \& } \Delta\text{Unrate}_t \geq 0, \\ \text{Haven't heard} & \text{otherwise.} \end{cases}$$

For estimation, we encode attentiveness using a *three-way* set of mutually exclusive indicators,

$$\mathbf{A}_{i,t} = \left(\mathbf{1}\{\text{Accurate}_{i,t}\},\ \mathbf{1}\{\text{Inaccurate}_{i,t}\},\ \mathbf{1}\{\text{Haven't heard}_{i,t}\}\right),$$

and use the corresponding group dummies in our specifications.

Timing and identification. Crucially, the attentiveness indicators, $\mathbf{A}_{i,t}$, are measured at the *first* month-t interview, *prior* to the FOMC announcement window that defines the monetary policy surprise ε_t^{mp}. Hence they are pre-determined with respect to the shock. Under our high-frequency identification,

$$\mathbb{E}\left[\varepsilon_t^{mp} \,\middle|\, \mathbf{A}_{i,t}, \mathbf{X}_{i,t}, \alpha_t\right] = 0,$$

where $\mathbf{X}_{i,t}$ collects observed covariates (age bins, education, income, homeownership, stockholding, gender, region, marital status, and survey-mode controls) and α_t are month-year fixed effects that absorb common macro/news variation. This timing, combined with the exogeneity of high-frequency surprises, forms our key identifying assumption, allowing us to interpret the coefficients on the interaction terms as the differential pass-through of policy news, ruling out reverse causality or within-month information acquisition. Although attention is endogenous to macroeconomic conditions in the model, identification exploits the *cross-sectional* difference in $\mathbf{A}_{i,t}$ within the same month: all respondents interviewed in month t face the same aggregate state, and what differs is which households acquired information—a decision made before the policy shock is realized.

Descriptive statistics by accuracy group. Table 1 reports respondent characteristics across the three groups. The groups are balanced in the sample (Accurate: 37.2%, Inaccurate: 29.7%, Haven't heard: 33.1%). *Accurate* and *Inaccurate* respondents look strikingly similar on observables: homeownership (83.2% vs. 81.9%), stockholding (76.4% vs. 75.6%), education (about 56% vs. 55% with a college degree), age (35-64: 63.4% vs. 61.7%; 65+: 22.0% vs. 23.3%), gender, region, marital status, and average income (both ~ \$94k); the two groups are also balanced across partisan categories.[9] By contrast, the *Haven't heard* group differs systematically: lower homeownership (76.9%), lower stockholding (60.8%), lower educational attainment (36.4% with a college degree; 5.6% less than high school), younger on average (18-34: 22.8%), less likely to be married/partnered (60.0%), and lower average income (\$71.2k).

These patterns are consistent with interpreting our accuracy indicator $\mathbf{A}_{i,t}$ as an *attentiveness* proxy rather than a proxy for fixed traits; observable composition differences are concentrated in the *Haven't heard* category, while *Accurate* and *Inaccurate* respondents are similar on observables. Additional diagnostics reinforce this interpretation. As shown in Appendix Table D.7, the *Haven't*

[9] We classify political stance relative to the sitting U.S. president at the time of the first interview. *Supporters* are respondents who self-identify with the president's party; *Opponents* identify with the out-party; *Independents* include self-reported independents, other parties, and no preference. Among *Accurate* respondents, 32.5% are supporters, 30.3% opponents, and 36.7% independents, with similar shares for the *Inaccurate* group.

Table 1: Demographic and Socioeconomic Characteristics by Attentiveness Group

	Accurate	Inaccurate	Haven't Heard
Panel A: Homeownership			
(1) Homeowner (%)	83.2	81.9	76.9
(2) Renter (%)	16.8	18.1	23.1
Panel B: Stockownership			
(3) Stockholder (%)	76.4	75.6	60.8
(4) Non-stockholder (%)	23.6	24.4	39.2
Panel C: Education level			
(5) Grade 0-8 no hs diploma (%)	0.5	0.6	1.5
(6) Grade 9-12 no hs diploma (%)	1.6	1.3	4.1
(7) Grade 0-12 w/ hs diploma (%)	16.1	16.3	28.0
(8) Grade 13-17 no col degree (%)	25.7	26.7	29.8
(9) Grade 13-16 w/col degree (%)	30.7	29.3	22.8
(10) Grade 17 w/ col degree (%)	25.2	25.5	13.6
Panel D: Age			
(11) 18-34 (%)	14.4	14.8	22.8
(12) 35-64 (%)	63.4	61.7	53.3
(13) 65+ (%)	22.0	23.3	23.7
Panel E: Gender			
(14) Male (%)	56.3	56.2	53.1
(15) Female (%)	43.6	43.7	46.8
Panel F: Region			
(16) West (%)	22.3	22.2	20.5
(17) North Central (%)	27.0	27.0	27.8
(18) Northeast (%)	17.4	17.4	16.3
(19) South (%)	33.2	33.2	35.2
Panel G: Marital status			
(20) Married/partner (%)	67.2	67.0	60.0
(21) Divorced (%)	13.5	13.5	13.9
(22) Widowed (%)	6.3	6.3	8.4
(23) Never married (%)	12.8	13.0	17.4
Panel H: Average income			
(24) Average income	93,911.9	93,886.0	71177.6
Total (%)	37.2	29.7	33.1

Notes: This table reports respondent characteristics by attentiveness group (Accurate, Inaccurate, Haven't Heard). All entries are column percentages unless otherwise noted; "Average income" is mean nominal household income (USD). Demographic categories include housing tenure, stockholding, education, age, gender, region, marital status, and income. Sample covers first interviews from 1998m09–2020m03. See Section 3 for the construction of the attentiveness measure and variable definitions.

heard category is substantially more persistent over the six-month panel than the *Inaccurate* category: about 54.3% of households initially classified as *Haven't heard* remain there at the second interview, compared with 36.0% of initially *Inaccurate* households remaining *Inaccurate*. A binary logit that compares *Haven't heard* directly to *Inaccurate* also shows that the former are systematically younger, lower-income, less-educated, and more likely to be non-stockholders. We therefore interpret *Haven't heard* as a distinct low-engagement group rather than as a noisy relabeling of explicit misperception.

3.3. Discussion of the Accuracy Proxy

Our accuracy-based indicator is a noisy empirical proxy for the latent attention variable $m_{i,t}$ in our model, not a literal observation of $m^*_{i,t}$. This subsection sets out how we interpret it and the empirical disciplines that support reading it as attention rather than as a confound.

3.3.1. Interpretation: a noisy, conservative proxy for an attention state. The identifying claim is narrow: respondents who correctly perceive the sign of recent business conditions are more likely, on average, to have attended to the relevant news flow before the policy shock arrives. Proposition 4 (part 3) provides the model-side mapping through the threshold-crossing rule $A_{i,t} = \mathbf{1}\{m^*_{i,t} \geq \tau\}$. Higher attention raises the probability of a correct classification but does not guarantee it: attentive households occasionally misinterpret signals, and inattentive households sometimes guess correctly. The *Inaccurate* category is therefore best viewed as a mixture of low-attention households and households that attended to the relevant news flow but decoded it incorrectly.

Two features make the resulting estimates conservative. First, this misclassification is classical, weakening the separation between attentive and inattentive groups and biasing the estimated coefficient toward zero, so the estimates in Section 4 are likely to be lower bounds on the true effect. Second, because the model predicts that attention varies endogenously with macroeconomic uncertainty (U_t), the classification changes across survey waves even for the same household—a feature we document in the rotating-panel diagnostics (Appendix Table D.7) and one that is systematically predicted by changes in financial conditions between interviews (Appendix Table D.8). This time variation is a prediction of the model and provides a second, independent source of attenuation that reinforces the lower-bound interpretation. We quantify these effects and assess sensitivity to the accuracy threshold in Section 5.

In sum, we read the indicator as a noisy but conservative measure of a household's transient *attention state*, not a fixed respondent trait. Because *Accurate* and *Inaccurate* households are observationally balanced (Table 1), comparing them isolates attention from fixed characteristics: any differential response to monetary policy (Section 4) is then attributable to how much attention households pay rather than to who they are.

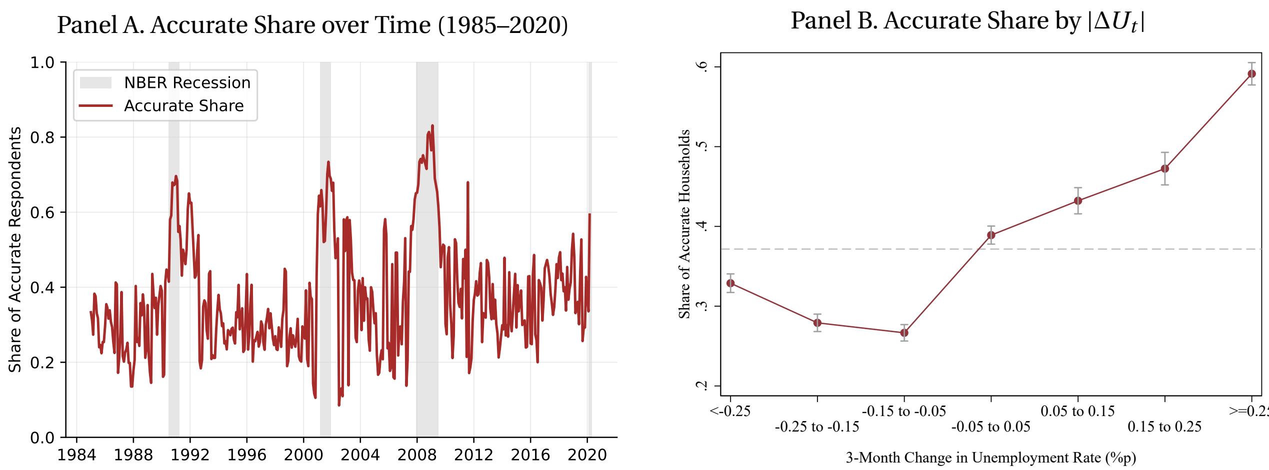

Figure 1: The Accurate Share over Time and by the Magnitude of Unemployment Changes

Notes: Both panels classify a household's first-interview assessment of recent business conditions as *Accurate* when its direction matches the sign of the three-month change in the unemployment rate. Panel A plots the monthly aggregate *Accurate* share from 1985 to 2020, which is strongly countercyclical, rising in recessions. Panel B plots the *Accurate* share conditional on the three-month change in the unemployment rate; vertical lines are 95% confidence intervals and the horizontal dashed line is the unconditional average share. The share is relatively flat for negative or small changes but rises sharply when the unemployment rate increases substantially.

3.3.2. Accuracy tracks the aggregate state, not personal circumstance. If the proxy captures attention, it should respond to the aggregate state as the model predicts and should not merely reflect a respondent's personal situation. Figure 1 shows two patterns in the aggregate share of accurate households. Over time (left panel) the share is strongly countercyclical, rising in recessions; and across the magnitude of unemployment changes (right panel) it rises sharply when unemployment increases substantially but is comparatively flat when unemployment falls or moves little. Both patterns are consistent with endogenous-attention theories in which households devote more attention when real conditions deteriorate and the information becomes more salient or payoff-relevant ($\partial m^* / \partial U > 0$).[10]

The most direct discipline asks what *predicts* being Accurate. Table 2 reports linear-probability regressions of an indicator for *Accurate* versus *Inaccurate* on three successive blocks: standard demographics; the respondent's *own* economic experience and expectations (the personal-finance situation, real-income expectations, and the business-conditions assessments); and the contemporaneous *aggregate* macro state (the absolute three-month change in unemployment $|\Delta U_t|$, inflation

[10] Our accuracy measure is distinct from the recent measure of inflation attention by Bracha and Tang (2024), which also uses the Michigan Survey. While their measure focuses on the stock of knowledge about *current inflation*, our proxy captures the active acquisition of news regarding *changing business conditions*. As shown in Appendix D.1, these two measures are weakly correlated ($\rho \approx 0.03$) and respond to different economic signals: theirs to high inflation, and ours to deteriorating real activity (unemployment).

Table 2: What Predicts Being Accurate? Determinants of Attentiveness

	(1) Demog.	(2) + Personal	(3) + Macro	(4) + Fin. stress
Own financial situation worse vs. a year ago	—	0.002	−0.008	−0.012
		(0.25)	(−0.91)	(−1.44)
Expect own financial situation worse	—	−0.001	−0.002	−0.002
		(−0.05)	(−0.17)	(−0.15)
Business condition worse vs. a year ago	—	0.079^{***}	0.050^{***}	0.038^{***}
		(6.39)	(3.98)	(3.04)
$\|\Delta U_t\|$	—	—	0.216^{***}	0.062^{***}
			(18.30)	(4.19)
St.Louis Financial Stress Index	—	—	—	0.068^{***}
				(18.92)
Demographics	Yes	Yes	Yes	Yes
R^2	0.002	0.009	0.030	0.042
N	25,043	23,991	23,991	23,991

Notes: Linear probability models for **1**{Accurate} among news-reporters (*Accurate* or *Inaccurate*). Personal-experience items enter as "better"/"worse" dummies ("same" omitted). All columns include demographic controls (age, age^2, log income, income quartile, education, gender, homeownership, stockholding, region, marital status). Robust t-statistics in parentheses. The full table is Appendix Table D.2. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

and industrial-production changes, and the St. Louis Fed Financial Stress Index).[11] The pattern is sharp. Demographics alone explain essentially none of the variation in accuracy ($R^2 = 0.002$). A respondent's *own* financial situation is a *null* predictor: "own situation worse" and "expect own situation worse" are economically tiny and statistically insignificant throughout. What predicts accuracy is engagement with the *aggregate* economy: the assessment of *business* conditions loads strongly, and adding the aggregate macro state—$|\Delta U_t|$ and the financial-stress index—raises the R^2 to 0.042. Households perceive recent conditions correctly precisely when the aggregate signal is large and financial stress is high, consistent with the model's prediction that attention is endogenous to payoff-relevant uncertainty ($\partial m^* / \partial U > 0$). This directly rules out the leading alternative that "Accuracy" reflects stable personal pessimism or private-outlook projection: personal mood does not predict it, the aggregate state does.[12]

3.3.3. Independent corroboration: news content and external salience. Independent measures—one inside the survey, others outside it—corroborate the attention interpretation. Within the survey, the MSC records the type of business news each respondent mentions first: among respondents who report hearing business news, *Accurate* households are disproportionately concentrated in

[11] The full determinants specification is in Appendix Table D.2.

[12] That said, in case any concern remains that personal sentiment or mood shapes the belief revision directly, our robustness analysis augments the gating regression with the MSC's consumer-sentiment indices and macroeconomic-expectations variables (Section 5).

labor-market and real-activity categories (52.4% versus 46.6% for *Inaccurate*), and the cleanest attentive-versus-inaccurate pass-through wedge appears precisely in that labor-news subsample (Appendix Table D.1). Outside the survey, the monthly aggregate attention share comoves positively with independent public-salience measures: it is roughly 30 percentage points higher in NBER recessions ($t = 11.93$) and rises with VIX, EPU, and EMV (Appendix Table D.9). It also correlates with a range of proxies outside the Michigan Survey, including the St. Louis Fed Financial Stress Index ($\rho = 0.54$), Google Trends search volume for "interest rates" ($\rho = 0.36$), and newspaper-based EMV sub-indices tracking monetary-policy coverage ($\rho = 0.22$; Appendix Table D.10). These time-varying correlations are difficult to reconcile with a purely fixed-trait interpretation and instead support the view that the proxy captures endogenous information acquisition.

Moreover, the evidence above disposes of a distinct concern: because accuracy is read from a single survey response, the *Accurate* label might reflect a lucky one-shot signal draw rather than attention. Under pure luck, the classification would be random and the two groups interchangeable—but they are not. Movements into and out of the *Accurate* state are systematically predicted by changes in financial conditions rather than occurring at random (Appendix Table D.8); the news content cited differs by group (Appendix Table D.1); and what predicts accuracy is the aggregate macro state, not personal circumstance. None of these patterns could be generated by pure noise. A label-randomization test confirms the point: permuting the accuracy labels across respondents 1,000 times places the actual attentive-group estimate in the tail of the permutation distribution ($p = 0.047$; Appendix Table D.9). And because the two groups are observationally balanced yet update their beliefs very differently in response to the same policy shock—as we show in Section 4—the differential cannot be a matter of luck. Any residual classification noise is classical, attenuating the estimates rather than producing them. Taken together, these considerations support interpreting the accuracy indicator as capturing economically meaningful variation in attention that governs the strength of monetary policy transmission through expectations.

3.4. Aggregate Attentiveness Index

To test Proposition 2 in time series, we construct an aggregate attentiveness measure as the cross-sectional share of attentive respondents at the first interview date t:

$$\mathbf{A}_t^{\text{agg}} \equiv \frac{1}{N_t} \sum_{i=1}^{N_t} \mathbf{A}_{i,t} \in [0,1],$$

where N_t is the number of respondents with non-missing $\mathbf{A}_{i,t}$. We use $\mathbf{A}_t^{\text{agg}}$ directly as a continuous index and, for regime analyses, define *high-attentive* months as the upper quantile of $\mathbf{A}_t^{\text{agg}}$ (*e.g.*, top

30% in the Great Moderation subsample) and *low-attentive* months as the complement. Under the threshold-crossing interpretation in Proposition 4, $\mathbf{A}_t^{\text{agg}}$ is a monotone empirical proxy for the model's average attention $\Lambda_t = \mathbb{E}_i[m_{i,t}^*]$ in Proposition 2, rather than a literal observation of Λ_t itself. The cyclical movement in $\mathbf{A}_t^{\text{agg}}$ operates mostly on the extensive margin of engagement: Appendix Table D.6 decomposes it across the three groups and shows that the *Accurate* share is countercyclical while *both* non-attentive shares fall in recessions. In quiet periods households disengage from macro news entirely; as the aggregate signal becomes salient they move into the attentive state.

3.5. Variable Alignment

For individual i, we compute the revision in one-year-ahead inflation expectations over the six-month panel window, aligning the timing so that the first interview (where $A_{i,t}$ is measured) precedes the MP shock and the second interview falls at $t+h$ (typically $h = 6$ months). For the aggregate time-series regressions in Section 4.2, the dependent variable $\Delta\pi_{t+6}^e$ is the six-month change in the published median one-year-ahead inflation expectation from the Michigan Survey: $\Delta\pi_{t+6}^e \equiv \text{MICH}_{t+6} - \text{MICH}_t$. When used, contemporaneous macro controls are measured between the first and second interviews (*e.g.*, ΔIP and $\Delta\pi$ from t to $t+h$). Monetary shocks are cumulated from t through $t+h-1$ to match the survey horizon when appropriate; Section 4 reports the exact horizon choice and robustness to alternatives.

4 Empirical Results

This section tests the model's central prediction that the expectations channel of monetary policy is governed by household attentiveness and therefore varies systematically across states of the economy. Using externally identified monetary policy shocks and a pre-determined measure of household attention, we provide micro and aggregate evidence for a state-dependent expectations multiplier. We proceed in four steps. First, we show at the individual level that monetary policy shocks affect inflation expectations only among households who are attentive prior to the policy announcement, establishing that attention gates pass-through on impact. Second, aggregating across households, we demonstrate that the time-series response of inflation expectations scales with economy-wide attentiveness: in high-attention regimes, contractionary policy generates large downward revisions in beliefs, while in low-attention regimes the response is negligible. Third, we show that this expectations multiplier is itself state dependent, strengthening sharply during periods of elevated macroeconomic uncertainty. Finally, consistent with the model's payoff-based mechanism, we document larger responses among households with greater financial exposure.

Throughout, identification exploits high-frequency monetary policy surprises and the fact that attentiveness is measured before policy news arrives. Together, these results establish that the expectations channel of monetary policy is inherently unstable and becomes most powerful precisely in high-attention, high-uncertainty environments.

4.1. Attention Gates Monetary Policy Pass-Through

We begin by testing Proposition 1 in the micro data, which provides the individual-level foundation for the expectations multiplier: monetary policy shocks should affect inflation expectations only to the extent that households are attentive at the time of the shock. Identification rests on two timing features. First, attentiveness is measured at the *first* interview in month t and is therefore predetermined with respect to the FOMC announcement window that generates the monetary policy surprise in month t. Second, the MP shock is measured in high frequency around the announcement and then *cumulated* from t to $t+5$ so that the information set between the two interviews (typically six months apart) aligns with the survey horizon. Under this timing, and conditional on observables, the surprise component of MPS_t is orthogonal to respondents' pre-shock attentiveness and demographics, so the interaction coefficients below identify differential expectations responses rather than reverse causality or within-month information acquisition.

Our baseline specification, adapted from Coibion and Gorodnichenko (2015b), tests this attention-gating mechanism by interacting the policy shock with our attentiveness indicators:

$$\Delta\pi^{e}_{i,t+6} = \alpha + \beta'_{M,A}\big(MPS_t \times \mathbf{A}_{i,t}\big) + \beta'_{Z,A}\big(\mathbf{Z}_t \times \mathbf{A}_{i,t}\big) + \Gamma'\mathbf{X}_{i,t} + \varepsilon_{i,t},$$

where $\Delta\pi^{e}_{i,t+6}$ is the change in a household's one-year-ahead inflation expectation between the two survey interviews, MPS_t is the normalized cumulative MP shock from t to $t+5$, $\mathbf{A}_{i,t}$ is our three-way vector of attentiveness indicators (Accurate / Inaccurate / Haven't heard), $\mathbf{Z}_t$ contains concurrent macro changes between interviews (IP growth and inflation), and $\mathbf{X}_{i,t}$ includes standard demographic controls including age and age^2, income, income quartiles, education, gender, home-ownership, stockholding, marital status, region, and survey-mode controls. Coefficients in $\beta_{M,A}$ are the group-specific pass-through slopes implied by Proposition 1.

Table 3 reports the estimates. The results line up closely with the gating prediction. For the *Accurate* group, a 1 pp tightening in the shadow policy rate lowers one-year-ahead expected inflation by -0.360 percentage points ($t = -4.56$). For the *Inaccurate* group, the slope is small and statistically indistinguishable from zero (0.088, $t = 0.81$). The *Haven't heard* group shows a modest negative and only marginally significant coefficient (-0.155, $t = -1.66$), an effect much smaller in magnitude

Table 3: Attention Shapes Monetary Policy Effects on Inflation Expectations

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
(1) MPS_t	-0.360***	0.088	-0.155*
	(-4.56)	(0.81)	(-1.66)
(2) ΔIP_t	0.060***	-0.008	0.013
	(3.60)	(-0.49)	(0.70)
(3)$\Delta\pi_t$	0.370***	0.272***	0.325***
	(9.81)	(6.41)	(7.21)
Controls		Yes	
Observations		37,445	
R^2		0.0138	

Notes: This table shows the baseline regression results of Equation (4.1). Dependent variable is the revision in one-year-ahead inflation expectations between the first and second MSC interviews (t to t+6). MPS_t is the high-frequency monetary policy surprise cumulated from t to t+5 and normalized so that one unit corresponds to a 1 pp change in the shadow policy rate over that window. ΔIP_t is the log change in industrial production and $\Delta\pi_t$ is the change in inflation. Columns report coefficients from interactions with the three attentiveness groups (Accurate, Inaccurate, Haven't Heard) defined at the first interview in month t. All specifications include individual controls (age and age^2, income level and quartiles, education, gender, homeownership, stockholding, marital status, region, and an indicator for reporting unfavorable news). Robust standard errors; t-statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

than that of the Accurate group.[13] Quantitatively, the Accurate–Inaccurate wedge is large: accurate respondents revise down by roughly one third of a percentage point per 1 pp tightening, while inaccurate respondents do not react on impact. This pattern is exactly what Proposition 1 implies.

Beyond statistical significance, the magnitudes imply economically meaningful variation in real-rate transmission across households. For attentive ("Accurate") individuals, a standard 25-basis-point contractionary policy surprise lowers one-year-ahead inflation expectations by approximately 9 basis points. For a given nominal policy path, this revision translates directly into a higher perceived short-term real interest rate, amplifying the contractionary impulse for this group. In contrast, inattentive households exhibit little to no immediate revision in inflation expectations, implying essentially no real-rate adjustment on impact.

The controls behave sensibly. IP growth between interviews is positively associated with revisions only for the *Accurate* group, consistent with real-side news being processed primarily by attentive respondents, while contemporaneous inflation changes load positively and significantly for all groups, reflecting the salience of price changes in household belief formation. Crucially,

[13] One possible interpretation is that this group—which, as shown in Table 1, is observationally distinct—may engage in indirect or passive belief updating. For example, they might react to highly salient signals like changes in gasoline prices or absorb broad economic sentiment from media headlines, even if they do not follow specific news about business conditions.

the primary empirical support for our mechanism comes from the sharp contrast between the "Accurate" and "Inaccurate" groups. As shown in Table 1, these two groups are nearly identical across a wide range of demographic and socioeconomic characteristics, yet respond very differently to monetary policy news. Their divergent expectation updates therefore cannot be readily attributed to observable heterogeneity, pointing instead to differences in ex ante information processing. Taken together, these results establish that attention gates the micro-level pass-through of monetary policy to inflation expectations.

This differential pass-through is also difficult to reproduce with placebo timing. Shifting the monetary policy shock six months forward, the *Accurate* coefficient collapses to 0.08 ($t = 0.86$); a six-month backward shift is also small and insignificant (0.14, $t = 1.52$). A permutation placebo reassigning monthly shocks yields an average absolute attentive-household coefficient of about 0.14, placing the baseline estimate near the upper tail of the placebo distribution (Appendix Table D.9). These exercises confirm the attentive-household response is specific to the timing of policy news.

A simpler two-group specification reinforces the core message. Pooling *Inaccurate* and *Haven't heard* into a single *Not Accurate* category, the attentive-group coefficient remains strongly negative (-0.35, $t = -4.39$), while the *Not Accurate* coefficient is close to zero (-0.06, $t = -0.85$); see Appendix Table D.11. The gating result is robust to distinctions within the non-attentive categories.

Having established that attention gates micro-level pass-through, we now ask whether this mechanism aggregates: does the time-series response of inflation expectations scale with economy-wide attentiveness?

4.2. Aggregate Pass-Through Scales with Attentiveness

We now test Proposition 2, which characterizes the expectations multiplier at the aggregate level: the response of aggregate inflation expectations to a conventional monetary policy shock is proportional to the economy's average attentiveness, Λ_t. This implies that the expectations channel of monetary policy is inherently unstable over time, strengthening when a larger share of households is attentive and weakening when attention is low.

To estimate this relationship in time series, we focus on the Great Moderation period (1985m1–2007m12), which allows us to isolate conventional monetary policy actions prior to the zero lower bound. We use the narrative-based monetary policy shocks of Romer and Romer (2004), denoted $RRshock_t$. We construct an aggregate attentiveness index, $\mathbf{A}_t^{\text{agg}}$, as the cross-sectional share of respondents classified as *Accurate* at the first interview (Section 3.4), which serves as the empirical counterpart to the model's average attention Λ_t.

Table 4: Aggregate Pass-Through Scales with Attentiveness

	Regime Split		Continuous
	(1) High	(2) Low	(3)
(1) $RRshock_t$	-0.504**	-0.015	-0.191**
	(-2.41)	(-0.15)	(-2.05)
(2) $RRshock_t \times \widetilde{\mathbf{A}}^{\text{agg}}_{t-1}$			-0.442***
			(-2.72)
(3) ΔIP_t	0.189***	-0.030	0.049**
	(2.90)	(-1.50)	(2.04)
(4) $\Delta\pi_t$	0.295***	0.227***	0.251***
	(2.81)	(4.58)	(4.80)
Observations	271		271
R^2	0.354		0.418

Notes: Dependent variable is the six-month change in the median one-year-ahead inflation expectation from the Michigan Survey. $RRshock_t$ is the cumulative Romer and Romer (2004) monetary policy shock from period t to $t+5$. ΔIP_t is the log change in industrial production and $\Delta\pi_t$ is the change in inflation. Columns (1)–(2) report regime-specific coefficients where high-attentive months are those with the aggregate attentiveness index $\mathbf{A}^{\text{agg}}_{t-1}$ in the top 30% of its 1985m1–2007m6 distribution and low-attentive months are the complement. Column (3) interacts the policy shock with the standardized continuous attention index $\widetilde{\mathbf{A}}^{\text{agg}}_{t-1}$; control interactions with attention are included but not reported. t-statistics with Newey-West standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

To capture regime variation in the expectations multiplier, we classify months as "high-attention" if $\mathbf{A}^{\text{agg}}_t$ falls in the top 30 percent of its historical distribution. This cutoff is standard in regime-based analyses, and the qualitative results are robust to alternative thresholds such as quartiles or terciles (Appendix Table D.9). Proposition 2 predicts a substantially larger (more negative) policy slope in high-attention regimes.

Our time-series regression mirrors the micro design but uses the six-month change in the published median inflation expectation as the dependent variable, and splits months by $I^A_{t-1} = \mathbf{1}\{\mathbf{A}^{\text{agg}}_{t-1} \text{ in top 30\%}\}$:

$$\Delta\pi^e_{t+6} = \alpha + \beta_M \left(RRshock_t \times I^A_{t-1}\right) + \beta'_{Z,A}\left(\mathbf{Z}_t \times I^A_{t-1}\right) + \varepsilon_t,$$

where $\mathbf{Z}_t$ contains contemporaneous IP growth and changes in inflation rates.

Table 4 reports the results. In high-attention regimes, a one percentage point contractionary policy shock lowers one-year-ahead median inflation expectations by approximately 0.50 percentage points, while in low-attention regimes the estimated response is close to zero and statistically insignificant. This sharp contrast provides direct evidence of a state-dependent expectations multiplier predicted by Proposition 2: identical policy shocks generate large belief revisions when household attention is high and virtually no response when attention is low. Quantitatively, the estimates imply that in high-attention states, a standard 25-basis-point tightening reduces median inflation expectations by roughly 13 basis points. For a given nominal policy path, this corresponds to an amplification of perceived short-term real interest rates on the order of 50 percent. By contrast, in low-attention states the expectations channel is largely dormant, implying little immediate real-rate adjustment following policy shocks.[14]

Column (3) of Table 4 provides a sharper test that avoids discrete regime classification altogether. We interact the policy shock with the standardized attention index $\widetilde{\mathbf{A}}^{\text{agg}}_{t-1}$. The interaction term is negative and significant (-0.44, $t = -2.72$), showing that aggregate pass-through strengthens monotonically as attention rises. Together, these results establish time-series evidence for the expectations multiplier: aggregate belief pass-through varies systematically with independently measured household attentiveness, complementing the micro-level gating results in Section 4.1.

4.3. State Dependence: Uncertainty Amplifies Expectation Responses

Proposition 3 predicts that increases in payoff-relevant uncertainty raise household attention and thereby amplify the expectations response to monetary policy shocks. As a result, the expectations multiplier itself becomes state dependent: contractionary policy generates disproportionately larger downward revisions in inflation expectations in high-uncertainty environments, particularly among already-attentive households.

We bring this prediction to the data by interacting monetary policy surprises with (i) our attentiveness indicators and (ii) proxies for uncertainty measured at $t-1$: NBER recessions, the real-uncertainty index of Ludvigson, Ma and Ng (2021) (LMN), and financial-market volatility (VIX). These measures span business-cycle, real, and financial uncertainty, ensuring our findings are not specific to any domain. For the LMN and VIX indices, we define high-uncertainty states using their cyclical components to capture deviations from recent trends, likely more salient to households than absolute levels. The estimating equation extends Equation (4.1) with a triple interaction:

$$\Delta\pi^e_{i,t+6} = \alpha + \beta'_{M,A,C}\big(MPS_t \times \mathbf{A}_{i,t} \times \mathsf{State}_{t-1}\big) + \beta'_{Z,A,C}\big(\mathbf{Z}_t \times \mathbf{A}_{i,t} \times \mathsf{State}_{t-1}\big) + \Gamma'\mathbf{X}_{i,t} + \varepsilon_{i,t},$$

where $\mathsf{State}_{t-1} \in$ {Recession, High LMN, High VIX}; coefficients on $MPS_t \times \mathbf{A}_{i,t} \times \mathsf{State}_{t-1}$ recover how the policy slope varies with uncertainty for the attentive group, while the corresponding "Inaccurate" terms benchmark the inattention case.

[14] The regime result is not sensitive to the threshold choice. Panel C of Appendix Table D.9 shows that a stricter top-25% cutoff leaves the high-attention slope strongly negative (-0.65, $t = -1.89$) while the low-attention slope remains near zero; a looser median cutoff attenuates the wedge but preserves the ordering.

Table 5: Uncertainty Raises Attention and Amplifies Expectation Responses

	(1) Accurate	(2) Inaccurate	(3) Accurate	(4) Inaccurate	(5) Accurate	(6) Inaccurate
Panel A: NBER						
(1) $Recession \times MPS_t$	-1.701***	-1.125				
	(-4.01)	(-1.00)				
(2) $Normal \times MPS_t$	-0.040	0.116				
	(-0.49)	(1.12)				
Panel B: LMN Real Uncertainty						
(3) $High \times MPS_t$			-0.539***	0.049		
			(-5.51)	(0.35)		
(4) $Low \times MPS_t$			-0.270*	0.250		
			(-1.77)	(1.33)		
Panel C: VIX						
(5) $High \times MPS_t$					-0.456***	0.041
					(-4.06)	(0.22)
(6) $Low \times MPS_t$					-0.008	0.101
					(-0.07)	(0.79)
Controls	Yes		Yes		Yes	
Observations	37,445		37,445		37,445	
R^2	0.0170		0.0146		0.0182	

Notes: This table shows regime- and group-specific policy coefficients from the triple-interaction regression in Equation (4.3). The dependent variable is the revision in 1-year-ahead inflation expectations between interviews, $\Delta\pi^e_{i,t+6}$. MPS_t is the normalized cumulative monetary policy shock from t to t+5. $\mathbf{A}_{i,t}$ is the three-way accuracy indicator (Accurate / Inaccurate / Haven't heard) measured at the first interview in month t. State_{t-1} is (i) the NBER recession dummy (Panel A); (ii) High LMN real-uncertainty (Panel B) and (iii) High VIX financial volatility (Panel C), each defined at $t-1$; "Normal/Low" are the complementary regimes. We include concurrent IP growth and inflation changes between t and t+6. We use individual information about age, income level and quartiles, homeownership, stockownership, gender, education level, region, marital status and an indicator for reporting unfavorable news as controls. Robust standard errors are used for the inference; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results, reported in Table 5, provide strong evidence of endogenous amplification.[15] During NBER recessions, attentive ("Accurate") respondents revise inflation expectations sharply downward on impact (approximately −1.70 percentage points per one percentage point tightening), while inattentive respondents exhibit little response.[16] A similar pattern emerges in high-LMN and

[15] All regression coefficients are reported in Appendix Tables D.16–D.17 in Appendix D.12.

[16] The estimated effect for accurate respondents during NBER-dated recessions is economically very large. This substantial magnitude may reflect the nature of recessions as periods of heightened macro-financial risk and policy scrutiny. During such critical periods, attentive households may become hyper-responsive to Fed actions, perceiving them as crucial signals about the future state of the economy. In *level* terms the implied revision is nonetheless modest: because the coefficients are normalized per one-percentage-point surprise and a typical FOMC surprise is on the order of 5–15 basis points, the predicted recession-state revision for an attentive household is roughly $-1.70 \times 0.10 \approx -0.17$ percentage points, well within the range of ordinary survey revisions.

high-VIX states: attentive households lower expected inflation by roughly 0.5 percentage points, whereas inattentive households again show no statistically significant reaction.[17] In low-uncertainty or normal states, the attentive-group coefficients are much smaller than in recessions or high-volatility episodes and are often statistically indistinguishable from zero; when they remain mildly negative in some specifications, the economically large amplification is still concentrated in the high-uncertainty states highlighted by the model.[18]

These cross-state contrasts are the empirical counterpart of

$$\frac{\partial}{\partial U_t}\big(m^*_{i,t}(U_t)\theta\big) = \theta\frac{m^*_{i,t}(1-m^*_{i,t})}{U_t} < 0 \quad \text{(for contractionary MP shocks)},$$

and reflect precisely the mechanism in Proposition 3: uncertainty raises attention, and higher attention endogenously amplifies the expectations response to monetary policy shocks.

Two features of the design isolate the attention channel from a pure second-moment (volatility) confound. First, the cross-sectional gating result holds aggregate uncertainty *fixed*: all respondents interviewed in a given month face the same volatility and the same policy surprise, so a within-month comparison of *Accurate* versus *Inaccurate* households—who are observationally identical on demographics—cannot be driven by the level of volatility, which is differenced out. Second, the *Inaccurate* triple-interaction acts as a built-in placebo for non-attention nonlinearities: if elevated uncertainty amplified pass-through through some channel other than attention (financial frictions, non-linear pricing, or binding constraints), it should amplify the response of *both* groups in high-uncertainty states. Instead, the amplification concentrates in the *Accurate* group while the *Inaccurate* interaction is statistically indistinguishable from zero across all three uncertainty proxies—exactly what the attention mechanism predicts and a generic volatility channel does not.

These findings complement evidence on state-dependent monetary transmission from prices and quantities. While Vavra (2014) emphasizes time-varying price adjustment, our mechanism operates through expectations: heightened uncertainty raises household attention, which magnifies belief revisions following policy news. This channel generates endogenous amplification precisely in periods of elevated macroeconomic risk. Two opposing forces are at work. On the one hand, increased attention reduces informational frictions, which in standard sticky-information models could attenuate real effects by facilitating faster adjustment. On the other hand, the sharp downward revision in inflation expectations implies a substantially larger increase in perceived short-term real

[17] This core finding—that amplification is concentrated among the attentive—also holds when using a broad, text-based measure of Economic Policy Uncertainty, as shown in Section 5.

[18] The state-dependent amplification also survives the two-group classification that pools *Inaccurate* and *Haven't heard* into *Not Accurate*. See Appendix Table D.11.

interest rates for attentive households. Through standard IS-curve logic, this real-rate amplification strengthens the contractionary impulse on demand. As we show in Section 4.5, attentive households also downgrade their durable-goods purchasing outlook following policy shocks, consistent with this mechanism operating through household spending expectations.

4.4. Payoff Heterogeneity and Accuracy: Who Reacts to Policy News?

Guided by Proposition 4, we next examine how heterogeneity in households' economic exposure shapes the strength of the expectations multiplier. The model predicts that groups for whom being informed is more valuable (higher ω) or less costly (lower κ) should exhibit larger belief responses to monetary policy shocks when attentive, and therefore account for a disproportionate share of aggregate expectations adjustment.

It is worth being explicit about why heterogeneity should appear *within* the attentive group. If latent attention $m^*_{i,t}$ were observed continuously, then conditional on $m^*_{i,t}$ the pass-through would be exactly $\theta m^*_{i,t}$ and demographics would carry no additional information. Our proxy, however, is a coarse binary indicator: the *Accurate* bin pools every household with latent attention above the threshold τ, and within that bin higher-ω households optimally choose larger $m^*_{i,t}$ and therefore transmit more. The within-*Accurate* demographic gradient documented below is thus a *consequence* of the binary proxy rather than a departure from the model, and it is direct evidence that the proxy does not wash out the continuous attention variation the theory predicts.

We proxy these high-payoff groups using three characteristics. First, we consider asset exposure—stockholding and homeownership—since monetary policy directly affects portfolio values and mortgage financing. Second, we examine age, reflecting life-cycle differences in macroeconomic-risk exposure: younger households' lifetime earnings are more sensitive to the business cycle, while prime-age households (35–64) carry the largest balance-sheet exposure through housing and financial assets. Third, we use income, which correlates with asset ownership and information use.

Empirically, we extend Equation (4.1) by interacting MPS_t with the accuracy indicators and each demographic partition, controlling for group means and the full set of covariates. Let $\boldsymbol{D}_{i,t}$ be a mutually exclusive demographic partition (*e.g.*, Stockholder/Non-stockholder; Homeowner/Renter; Young/Middle/Old; Income quartiles), with one category omitted in estimation. Our general specification replaces the demographic block as needed:

$$\Delta\pi^e_{i,t+6} = \alpha + \underbrace{\beta'_{M,A,D}\big(MPS_t \times \mathbf{A}_{i,t} \times \boldsymbol{D}_{i,t}\big)}_{\text{group- and accuracy-specific MP pass-through}} + \beta'_{Z,A,D}\big(\mathbf{Z}_t \times \mathbf{A}_{i,t} \times \boldsymbol{D}_{i,t}\big) + \Gamma'\mathbf{X}_{i,t} + \varepsilon_{i,t},$$

where MPS_t is the normalized cumulative MP surprise between interviews, $\mathbf{Z}_t$ collects concurrent macro changes (IP growth, inflation) between the two interviews, and $\mathbf{X}_{i,t}$ includes the full set of demographics and survey controls. The coefficients in $\beta_{M,A,D}$ deliver the impact slopes by *accuracy* × *demographic* cell. For contractionary shocks, the model predicts large negative slopes for *Accurate* × (high-ω/low-κ) groups (*e.g.*, stockholders, homeowners, prime-age, higher-income) and slopes near zero for *Inaccurate* cells. We estimate Equation (4.4) separately for each partition $\boldsymbol{D}_{i,t}$ and Table 6 reports the $\beta_{M,A,D}$ blocks.[19]

Stockholding. Panel A of Table 6 estimates Equation (4.4) with interactions between monetary policy shocks, pre-determined attentiveness, and stockholding status. The results show that belief responses are concentrated among *attentive stockholders*: a one percentage point contractionary policy surprise lowers their one-year-ahead inflation expectations by approximately 0.41 percentage points on impact ($t = -4.57$). In contrast, attentive non-stockholders exhibit a smaller and statistically insignificant response, and all inattentive groups show coefficients close to zero. This pattern indicates that monetary transmission through expectations is jointly shaped by attention and economic exposure. Even among stockholders, policy shocks have little effect on beliefs in the absence of attention, while attentive stockholders account for a large share of aggregate pass-through. These findings highlight stockholders as key carriers of the expectations multiplier.

Ahn and Xie (2024) independently document that stock-market participation is associated with greater household attentiveness and more accurate inflation beliefs. Using MSC micro data, they show that stockholders update more to macro news, with the attention gap widening in periods of elevated uncertainty. Our results complement theirs along the monetary policy dimension: conditioning on pre-determined attentiveness, the impact of conventional policy surprises is concentrated among *attentive* stockholders, while inattentive stockholders do not react. Quantitatively, this generates a substantially steeper policy slope within the Accurate group—an empirical counterpart to Proposition 4 and further evidence that households with larger financial stakes disproportionately transmit policy into expectations.

Homeownership. Homeownership represents another key margin of economic exposure to monetary policy. Interest rate movements directly affect homeowners through mortgage payments, refinancing opportunities, and housing wealth, implying larger real rate sensitivity and a higher payoff to processing policy news. This channel complements existing evidence that homeowners are particularly responsive to interest rate changes via refinancing and payment adjustments (*e.g.*, Ahn, Xie and Yang, 2024).

[19] All regression coefficients are reported in Appendix Tables D.18–D.20 in Appendix D.12.

Table 6: Attention and Demographic Heterogeneity in Monetary Policy Pass-Through

	(1) Accurate	(2) Inaccurate	(3) Accurate	(4) Inaccurate	(5) Accurate	(6) Inaccurate
Panel A: Stockholding						
(1) $Stock \times MPS_t$	-0.410***	0.150				
	(-4.57)	(1.20)				
(2) $NonStock \times MPS_t$	-0.228	-0.047				
	(-1.42)	(-0.21)				
Panel B: Homeownership						
(1) $Homeowner \times MPS_t$			-0.436***	0.063		
			(-5.08)	(0.54)		
(2) $Renter \times MPS_t$			0.026	0.214		
			(0.13)	(0.76)		
Panel C: Age						
(1) $Young \times MPS_t$					-0.613***	0.260
					(-3.22)	(0.82)
(2) $Middle \times MPS_t$					-0.350***	0.140
					(-3.68)	(1.12)
(3) $Old \times MPS_t$					-0.234	-0.264
					(-1.22)	(-0.98)
Interaction	Stockownership		Homeownership		Age Group	
Controls	Yes		Yes		Yes	
Observations	37,445		37,445		37,445	
R^2	0.0142		0.0144		0.0150	

Notes: This table reports group- and accuracy-specific policy coefficients from the interacted specification in Equation (4.4). The dependent variable is the revision in 1-year-ahead inflation expectations between interviews, $\Delta\pi^e_{i,t+6}$. MPS_t is the normalized cumulative monetary policy surprise from t to t+5 (mapped to a 1 pp change in the shadow rate). $\mathbf{A}_{i,t}$ is the three-way accuracy indicator (Accurate / Inaccurate / Haven't heard) measured at the first interview in month t. $\boldsymbol{D}_{i,t}$ denotes the demographic partition used in each panel: (A) Stockholder vs. Non-stockholder; (B) Homeowner vs. Renter; (C) Age groups (Young 18-34, Middle 35-64, Old 65+). We include concurrent macro changes between interviews (IP growth and inflation) as well as the full set of demographics and survey controls. All lower-order terms and group means are included. Reported coefficients are on $MPS_t \times \mathbf{A}_{i,t} \times \boldsymbol{D}_{i,t}$. Robust standard errors are used for the inference; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimating Equation (4.4) with interactions between monetary policy shocks, pre-determined attentiveness, and homeownership status confirms that homeowners account for a disproportionate share of the expectations multiplier. Panel B of Table 6 shows that among attentive respondents, a one percentage point contractionary policy shock lowers one-year-ahead inflation expectations by approximately 0.43 percentage points for homeowners ($t = -5.08$), while attentive renters exhibit no detectable impact response. For inattentive households, coefficients are small and statistically indistinguishable from zero across both tenure groups. This pattern indicates that monetary transmission through expectations depends jointly on attention and economic exposure. Even among homeowners, policy shocks have little effect on beliefs in the absence of attention, while attentive homeowners show a strong pass-through.

In magnitude, the homeowner effect is comparable to that for stockholders in Panel A of Table 6, highlighting two complementary channels—portfolio exposure and mortgage-linked exposure—through which monetary policy affects household expectations. Crucially, pre-shock attentiveness remains central: without attention, neither homeowners nor renters transmit policy news into expected inflation on impact.

Age Group. Age provides a natural partition of households' exposure to monetary policy. Younger and prime-age households typically face greater labor-market risk and make more frequent economic decisions, implying larger sensitivity of lifetime income and consumption to policy-induced fluctuations. These life-cycle differences suggest that monetary transmission through expectations should be strongest among younger and working-age households. In addition, the personal-experience framework of Malmendier and Nagel (2016) implies that younger individuals place greater weight on recent macroeconomic information, while older individuals rely more on accumulated experience and update less on impact.

Estimating Equation (4.4) with interactions between monetary policy surprises, our pre-determined accuracy indicators, and age-group status (Young 18-34, Middle 35-64, Old 65+) yields a clear gradient within the *accurate* group (Panel C of Table 6). Accurate *young* respondents revise one-year-ahead inflation expectations the most after a 1 pp contractionary MP surprise (-0.613, $t = -3.22$), accurate *middle*-aged respondents respond less but still significantly (-0.350, $t = -3.68$), and accurate *older* respondents show a smaller, insignificant coefficient (-0.234, $t = -1.22$). For *inaccurate* respondents, coefficients are small and indistinguishable from zero across age groups.

These patterns indicate that monetary transmission through expectations varies systematically over the life cycle. Attention remains a prerequisite for belief updating, but conditional on attention, households with greater exposure to labor-market risk and future income—particularly younger and prime-age households—account for a larger share of the expectations multiplier.

Income Quartile. Income provides a further dimension of heterogeneity in households' exposure to monetary policy. Relative to the bottom quartile, middle- and higher-income households typically face greater sensitivity of labor income, asset valuations, and borrowing conditions to policy shocks, implying stronger real-rate transmission and a higher payoff to incorporating policy news.

Using the MSC income quartiles, we estimate Equation (4.4) with the demographic partition $\boldsymbol{D}_{i,t} = \{\text{YTL1}, \ldots, \text{YTL4}\}$ and report results in Table 7. The *accuracy prerequisite* remains first-order:

Table 7: Attention and Income Quartile in Monetary Policy Pass-Through

	(1) Accurate	(2) Inaccurate
(1) $YTL1 \times MPS_t$	0.048	-0.192
	(0.18)	(-0.58)
(2) $YTL2 \times MPS_t$	-0.669***	0.046
	(-3.90)	(0.19)
(3) $YTL3 \times MPS_t$	-0.361***	0.201
	(-2.58)	(1.04)
(4) $YTL4 \times MPS_t$	-0.298**	0.132
	(-2.47)	(0.77)
Interaction	Income Quartile	
Controls	Yes	
Observations	37,445	
R^2	0.0153	

Notes: This table reports group- and accuracy-specific policy coefficients from the interacted specification in Equation (4.4). The dependent variable is the revision in 1-year-ahead inflation expectations between interviews, $\Delta\pi^e_{i,t+6}$. MPS_t is the normalized cumulative monetary policy surprise from t to t+5 (mapped to a 1 pp change in the shadow rate). $\mathbf{A}_{i,t}$ is the three-way accuracy indicator (Accurate / Inaccurate / Haven't heard) measured at the first interview in month t. The demographic partition used in this table is income level. We use YTL4 variable from MSC to define consumers' income quartile. We include concurrent macro changes between interviews (IP growth and inflation) as well as the full set of demographics and survey controls. All lower-order terms and group means are included. Reported coefficients are on $MPS_t \times \mathbf{A}_{i,t} \times \boldsymbol{D}_{i,t}$. Robust standard errors are used for the inference; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

across all quartiles, inaccurate respondents do not react on impact. Within the *accurate* group, we find a clear gradient: middle-income households (YTL2, YTL3) display the largest and most precisely estimated declines in 1-year-ahead expectations after a 1 pp contractionary MP surprise (-0.669 and -0.361, respectively), high-income households (YTL4) react moderately (-0.298), and the lowest-income quartile (YTL1) shows no detectable impact response. This pattern indicates that monetary transmission through expectations is strongest among households with intermediate-to-high economic exposure and weakest among those with limited financial or labor-market stakes. The results align with Proposition 1 and Proposition 4, identifying income as another dimension along which the expectations multiplier operates unevenly.

4.5. The Expectations Multiplier Extends to Real Activity

So far, we have shown that household attention governs the strength of the expectations multiplier, generating large state-dependent responses of inflation beliefs to monetary policy shocks. A natural next question is whether these belief revisions reflect a broader understanding of monetary transmission, or whether attentive households merely adjust inflation expectations in isolation. If monetary policy operates through standard macroeconomic channels, contractionary shocks that lower inflation expectations should also lead households to anticipate weaker real activity and less favorable conditions for discretionary spending. In this section, we test whether the same households that revise inflation expectations also update their real-side outlook in a manner consistent with New Keynesian transmission logic.

To assess this, we examine two additional outcomes from the MSC: (i) expectations about unemployment over the next 12 months, and (ii) attitudes toward vehicle purchasing conditions. These measures capture households' perceptions of labor market slack and near-term consumption environments, providing direct evidence on whether monetary policy affects real-side expectations alongside inflation beliefs.

For unemployment expectations, we estimate our baseline specification (Equation 4.1). We define a categorical variable $D_{i,t}^{Unemp}$ which takes the value of 1 if respondent i expects "more unemployment" over the next year, 0 if "about the same," and −1 if "less unemployment."[20] This variable directly probes whether households understand the Phillips curve trade-off that disinflationary policy comes at the cost of real economic slack.

For vehicle purchasing attitudes, we utilize the following question in the MSC:

> "*Thinking now of the automobile market — do you think the next 12 months or so will be a good time or a bad time to buy a new vehicle...?*"

We construct a variable $I_{i,t+6}^{Vehicle}$ taking the value 1 for "good time," 0 for "uncertain," and −1 for "bad time." Unlike unemployment expectations, where we focus on belief revisions, purchasing attitudes exhibit significant persistence. Therefore, we adopt a level specification that controls for the respondent's initial sentiment to isolate the effect of the shock on the change in attitude:

$$I_{i,t+6}^{Vehicle} = \alpha + \beta_A (MPS_t \times \mathbf{A}_{i,t}) + \gamma I_{i,t}^{Vehicle} + \Gamma X_{i,t} + \epsilon_{i,t},$$

where we include the respondent's initial sentiment $I_{i,t}^{Vehicle}$ to account for persistence in individual attitudes. The coefficient of interest, β_A, captures how the monetary policy shock affects the likelihood of considering it a "good time" to buy, conditional on the household's pre-shock attentiveness. If attentive households correctly anticipate the contractionary effects of policy (*e.g.,* higher rates and/or lower income growth), they should view the environment as less favorable for large discretionary purchases, resulting in a negative β_A.

[20] Since the dependent variable is categorical/ordinal, we estimate linear probability models for simplicity. The coefficients can be interpreted as the change in the net likelihood of holding a pessimistic view (expecting higher unemployment) in response to the shock.

Table 8: Impact of Monetary Policy on Consistent Macroeconomic Expectations

Dependent Variables	Unemployment (Expect "More")	Vehicle Purchasing Attitude		
		Full Sample	Homeowners	Top 50% Income
	(1)	(2)	(3)	(4)
Accurate $\times MPS_t$	0.039***	-0.038***	-0.042***	-0.053***
	(3.09)	(-2.83)	(-2.72)	(-2.89)
Inaccurate $\times MPS_t$	0.018	-0.014	-0.010	-0.002
	(0.91)	(-0.68)	(-0.46)	(-0.08)
Haven't Heard $\times MPS_t$	-0.007	0.011	-0.001	-0.028
	(-0.53)	(0.71)	(-0.05)	(-1.08)
Controls	Yes	Yes	Yes	Yes
Observations	37,160	35,324	28,817	16,914
R^2	0.0123	0.234	0.238	0.225

Notes: Column (1) reports estimates of the baseline specification Equation (4.1) using expectations of unemployment over the next 12 months as the dependent variable. Columns (2)-(4) report estimates of Equation (4.5) using the vehicle purchasing sentiment index at $t+6$, controlling for the sentiment at time t. MPS_t is the normalized cumulative high-frequency monetary policy shock. All specifications include standard demographic controls. Robust t-statistics are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8 reveals a strikingly consistent pattern. Column (1) reports the response of unemployment expectations. Among attentive ("Accurate") households, contractionary monetary policy shocks significantly increase the likelihood of expecting higher unemployment, consistent with the standard view that policy tightening cools labor market conditions. In contrast, inattentive households exhibit no meaningful response, indicating a failure to connect policy news to real economic outcomes.

A similar pattern emerges for durable-goods purchasing attitudes. Column (2) shows that attentive households significantly downgrade their assessment of vehicle buying conditions following a contractionary shock, even after controlling for the prior vehicle-buying attitude. This response is consistent with households internalizing both higher financing costs and weaker income prospects. Again, inattentive households display no comparable adjustment.

Together, these results indicate that the expectations multiplier documented in earlier sections operates through a broader revision of households' macroeconomic outlook. Attentive households simultaneously update beliefs about inflation, labor market conditions, and consumption environments, while inattentive households adjust none of these dimensions.[21]

[21] The attention channel operates primarily through near-term beliefs. Applying the baseline specification to five-year-ahead inflation expectations yields coefficients that are economically small and statistically indistinguishable from zero for all three groups (for the *Accurate* group, about -0.03 with $t = -0.70$); see Appendix Table D.12. This suggests that the mechanism governs the transmission of policy news into near-term beliefs and real-side outlook rather than de-anchoring long-run inflation expectations.

One potential concern is that the muted response of inattentive households reflects financial constraints rather than information frictions. To address this, we re-estimate Equation (4.5) on two unconstrained subsamples: homeowners (Column 3) and households in the top half of the income distribution (Column 4). The results decisively reject this explanation. Even among wealthy and asset-holding households, contractionary shocks reduce purchasing sentiment only for attentive respondents, while inattentive households again show no response. Financial capacity alone is therefore insufficient for transmission: without attention to policy signals, even unconstrained households fail to adjust their economic outlook.

In sum, endogenous attention not only governs inflation belief updating but also shapes households' expectations about real economic conditions and spending environments. In high-attention states, the transmission mechanism becomes fully operative: monetary policy simultaneously lowers inflation expectations and worsens perceived labor market and consumption prospects, amplifying contractionary effects through both real-rate and income-expectations channels. When attention is low, these mechanisms largely shut down. The expectations multiplier therefore reflects a genuine propagation channel, not merely a survey-based artifact.

5 Robustness

We assess the robustness of our findings along several dimensions, beginning with the most substantive threats to identification—omitted variables and measurement error—and then turning to alternative measurement choices. Full details are in Appendix D.

Coefficient Stability. A natural concern is that unobserved confounders correlated with accuracy could drive the baseline coefficient. We address this using the coefficient-stability test of Oster (2019), which asks how large selection on unobservables would need to be, relative to selection on observables, to explain away the result. Comparing the "short" regression (only MPS–attention interactions, no demographic controls) with the "full" specification (all controls), the Oster delta for *mps_accurate* is *negative*: $\hat{\delta} = -0.21$ under $R_{\max} = 1.3\bar{R}^2$. A negative delta means that adding observable controls *strengthens* the coefficient, moving it further from zero. For unobservables to explain away the finding, they would therefore need to work in the *opposite* direction from observables—an economically implausible scenario. The identified set under $R_{\max} = 1.3\bar{R}^2$ is $[-0.53, -0.36]$, which excludes zero; the set remains bounded away from zero under more aggressive assumptions ($R_{\max} = 2.2\bar{R}^2$ and even the knife-edge $R_{\max} = \bar{R}^2$). By contrast, the coefficient for *mps_inaccurate* remains small and positive under all assumptions: under $R_{\max} = 2.2\bar{R}^2$ the identified set is $[0.03, 0.09]$, economically negligible and of the opposite sign to the attentive response, consistent with the baseline finding that inattentive households do not revise beliefs downward in response to policy news.[22]

Richer Controls from the Augmented Sample. Our baseline specification controls for standard demographics but does not include the rich set of sentiment and expectations variables available in the MSC microdata. To check whether the attention-gating result is driven by omitted consumer-confidence or expectations channels, we augment the baseline with five blocks of additional controls drawn from the augmented MSC sample: the consumer sentiment index (ICS), macroeconomic perceptions (interest-rate expectations, unemployment outlook), durable-goods and housing-market attitudes, gasoline-price expectations, and income/business expectations. In every specification, the *mps_accurate* coefficient remains strongly significant, and the kitchen-sink specification that includes all blocks simultaneously yields a coefficient of -0.62 ($t = -5.10$). Adding sentiment controls slightly *increases* the magnitude, reinforcing the Oster result that controls work in the direction of strengthening the finding rather than attenuating it. These results, reported in Appendix Table D.14, rule out the concern that the baseline result is an artifact of omitted consumer sentiment or macroeconomic expectations.

Measurement Error, Attention Dynamics, and Threshold Sensitivity. Because the accuracy proxy is an imperfect indicator of latent attention, misclassification compresses the estimated gap between the attentive and inattentive groups toward zero, analogous to classical measurement-error attenuation. Two independent mechanisms drive this compression. First, static misclassification at t: some truly attentive households are classified as *Inaccurate* (pulling that coefficient toward the attentive response) and vice versa. Second, attention dynamics within the six-month window: because the baseline specification cumulates shocks from t to $t+5$ while attention is measured only at t, households that are correctly classified as *Accurate* at t but rationally disengage as conditions evolve will transmit only part of the cumulative shock, diluting the estimated slope. Both effects work in the same direction—they attenuate $|\hat{\beta}_{\text{Accurate}}|$ relative to the true instantaneous gating effect in Proposition 1—so the baseline estimates are conservative lower bounds on the magnitude of attention-dependent pass-through.[23] The result is also robust to the precise threshold used to

[22] Full results are reported in Appendix Table D.13.

[23] Using the survey's rotating panel, about 45.7% of re-interviewed respondents receive the same three-way accuracy classification in both waves (Appendix Table D.7, Panel A). If all cross-wave disagreement is attributed to noise (ignoring the genuine dynamics component), the implied reliability ratio is about 0.27—an attenuation factor of $1/0.27 \approx 3.6$—so the corrected coefficient would be roughly 3.6 times the OLS estimate. Because some instability reflects endogenous attention adjustment rather than noise, the true noise share is smaller and the specific attenuation factor is an upper bound. The qualitative conclusion—that OLS understates the gating effect—holds regardless of how the instability is decomposed between the two sources.

define accuracy: varying the unemployment-change cutoff from −0.3 to +0.3 percentage points preserves the sign, significance, and group ordering of the baseline specification throughout; see Appendix D.11.

Alternative Monetary Policy Shocks. A potential concern is that high-frequency monetary policy surprises may partially reflect information revealed by the Federal Reserve rather than pure policy innovations. To address this, we re-estimate our baseline specifications using the unified monetary policy shocks of Bu et al. (2021) ("BRW"), which exploit the full yield curve and heteroskedasticity-based identification. The results, reported in Panel A of Table 9, closely mirror our baseline findings: belief responses remain concentrated among attentive households, preserving both the cross-group ordering and statistical significance. Estimated magnitudes are somewhat larger, consistent with the construction of the BRW shocks, which are scaled to move two-year yields nearly one-for-one and therefore represent a more persistent policy signal.

We further verify robustness using the reassessed high-frequency shocks of Bauer and Swanson (2023), which explicitly purge pre-announcement macroeconomic information. As shown in Panel B of Table 9, the *Accurate* group again exhibits the largest expectations response. While the *Haven't Heard* group also displays a significant negative coefficient in this specification, the magnitude remains smaller than for the *Accurate* group, and the *Inaccurate* group continues to show little to no response. This pattern is consistent with our interpretation of *Haven't Heard* as an intermediate-attention category that may absorb policy signals passively, whereas explicit misperception (*Inaccurate*) is associated with minimal transmission.

Taken together, these exercises show that the expectations multiplier and its heterogeneity across attention groups are robust to alternative monetary policy shock constructions, including measures designed to mitigate Fed information effects. The central result—that monetary policy affects expectations primarily through attentive households—does not depend on a particular identification strategy for monetary policy shocks.

Alternative Benchmark Measures for Business Conditions. We next examine whether our results depend on the specific construction of the attentiveness proxy. Our benchmark measure classifies households as *Accurate* based on the three-month change in the unemployment rate, chosen to smooth high-frequency noise while matching the survey's reference to "the last few months." Appendix Table D.21 shows that using one-month or six-month differencing horizons yields nearly identical results, indicating our findings are robust to the precise timing window.

To further assess robustness, we redefine *Accuracy* using alternative aggregate indicators capturing different dimensions of the macro environment: Industrial Production (IP), which proxies real

Table 9: Alternative Monetary Shock Measure

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard	(4) Accurate	(5) Inaccurate	(6) Haven't Heard
Panel A: Bu et al. (2021)						
(1) MPS_t	-1.411***	-0.256	-0.505**			
	(-6.66)	(-1.19)	(-2.14)			
(2) ΔIP_t	0.043***	-0.001	0.004			
	(2.77)	(-0.10)	(0.24)			
(3)$\Delta\pi_t$	0.349***	0.268***	0.319***			
	(9.29)	(6.33)	(7.04)			
Panel B: Bauer and Swanson (2023)						
(1) MPS_t				-1.343***	-0.535	-0.898***
				(-4.46)	(-1.57)	(-2.79)
(2) ΔIP_t				0.079***	0.056**	0.056***
				(3.97)	(1.97)	(2.16)
(3)$\Delta\pi_t$				0.275***	0.268***	0.275***
				(7.45)	(6.32)	(6.11)
Controls		Yes			Yes	
Observations		37,445			35,592	
R^2		0.0148			0.0168	

Notes: This table replaces the high-frequency MPS_t series with the Bu et al. (2021) monetary policy shocks (Panel A) and Bauer and Swanson (2023) (Panel B) and re-estimates the baseline micro specification Equation (4.1). The dependent variable is the revision in one-year-ahead inflation expectations. Shocks are cumulated from t to $t+5$ to align with the six-month survey horizon. Accuracy is measured at the first interview. We include contemporaneous IP growth and inflation changes between interviews; demographics and survey controls are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

activity; the National Financial Conditions Index (NFCI), which captures financial conditions; and the Aruoba-Diebold-Scotti (ADS) Business Conditions Index (Aruoba, Diebold and Scotti, 2009), which provides a composite real-time measure of economic activity. For the ADS benchmark, we compute the change in the monthly average of the daily index between $t-1$ and $t-4$, paralleling our three-month differencing window. Across all three alternatives, the core gating pattern remains intact: belief responses to monetary policy shocks are concentrated among attentive households, while inattentive households exhibit little reaction (Appendix Table D.22). Moreover, heterogeneity with respect to asset exposure and income persists across these definitions, reinforcing the interpretation that households with greater economic stakes account for a disproportionate share of the expectations multiplier (Appendix Tables D.23–D.26).

Two further cross-validation exercises reinforce this reading. First, scoring the *same* news responses against the unemployment, IP, and NFCI benchmarks shows that the three agree on the direction of the economy only in a subset of months, and the gating effect is concentrated precisely in the "all-agree" months when the aggregate signal is unambiguous (−1.03 versus near zero in mixed months), which suggests a signal-clarity pattern (Appendix Table D.4). Second, an independent accuracy measure built from the "business conditions versus a year ago" question in the MSC reproduces the gating (−0.25, $t = -3.7$), and splitting by the topic of news cited shows the attentive-versus-inaccurate wedge is sharpest for labor and real-activity content (−0.59) and weakest for government/policy news (Appendix Table D.5). The proxy thus captures general macro attention, not a single benchmark or question form.

We also consider a more parsimonious proxy for attention based solely on whether respondents report having heard any news about business conditions, without benchmarking against external macro data. Re-estimating Equation (4.1) using "Have Heard" versus "Haven't Heard" indicators (Appendix Table D.27) yields a significantly weaker and less sharply differentiated response pattern. While the "Have Heard" group exhibits a negative reaction, its magnitude is substantially smaller than for the baseline *Accurate* group, and the "Haven't Heard" group also shows a marginal response. This contrast highlights the value of the accuracy-based measure: distinguishing between correct and incorrect perceptions delivers a cleaner separation between households that transmit policy news into expectations and those that do not.

Signal Strength: Dropping Small Unemployment Changes. A potential concern with our baseline attentiveness measure is that it classifies households based on the *direction* of unemployment changes, treating marginal fluctuations (*e.g.*, a 0.1 percentage point change) the same as large economic movements. If households rationally disregard negligible changes, classifying responses to such periods could introduce measurement noise. We re-estimate our main specifications after excluding observations in which the absolute three-month change in the unemployment rate is smaller than 0.1 percentage point ($|U_t - U_{t-3}| < 0.1$). The results, reported in Appendix Tables D.28–D.32, are virtually unchanged, confirming that our findings are not driven by classification noise during quiet periods.

Taken together, these exercises confirm that the expectations multiplier does not hinge on specific measurement or identification choices. The Oster bounds and richer controls rule out omitted-variable explanations; the measurement-error analysis establishes that the baseline estimates are attenuated lower bounds; and the results survive alternative shock constructions, attentiveness benchmarks, sample restrictions, and control specifications.

6 Conclusion

We develop a minimal framework in which households endogenously choose attention to macroeconomic information and test its implications using a pre-determined measure of attentiveness and externally identified monetary policy shocks. The evidence points to a simple mechanism—an expectations multiplier: monetary policy moves beliefs primarily through attentive households. At the micro level, inflation expectations respond to policy news almost entirely within the *Accurate* group, and pooling non-attentive categories leaves a response close to zero. In the aggregate, pass-through scales with economy-wide attentiveness, strengthens in recessions and high-uncertainty environments, and is largest among households with greater economic exposure. Importantly, attentive households revise not only inflation beliefs but also their labor-market and durable-spending outlook, so the multiplier reflects a genuine propagation channel—amplifying the perceived real interest rate and contractionary impulse on demand—rather than a survey artifact.

These findings carry three implications for monetary policy. First, the expectations channel is inherently state dependent: identical policy actions generate very different belief responses depending on how attentive households are, so communication tools such as forward guidance should be most potent precisely when attention is already high—during periods of elevated uncertainty or economic stress. Second, belief transmission is uneven across households; groups with larger financial stakes, such as homeowners and stockholders, account for a disproportionate share of aggregate adjustment, raising distributional considerations for both communication strategy and policy evaluation. Third, because the expectations response is attention-weighted, our reduced-form estimates yield sharp, testable predictions for information-provision experiments pioneered by Coibion et al. (2022): treatment effects should be *larger* in high-uncertainty periods and for high-stakes households, and *smaller* for the persistently disengaged "Haven't heard" group, giving experimenters a structured benchmark for whom to target and when effects should be largest.

More broadly, our findings highlight endogenous attention as a key source of nonlinear monetary transmission. By shaping when and for whom expectations respond to policy, attention links macroeconomic uncertainty to real-rate dynamics and demand fluctuations. Future research could explore how central banks might strategically influence attention, how belief responses propagate over longer horizons, and how time-varying attention inequality interacts with optimal policy in heterogeneous-agent settings.

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APPENDIX

A Proofs

This appendix provides the formal mathematical derivations for the four main propositions presented in the theoretical framework of Section 2. It details the steps for deriving the impact of monetary policy on individual expectations (Proposition 1) , the scaling of aggregate pass-through with attention (Proposition 2), the state-dependent nature of the response to uncertainty (Proposition 3), and the cross-sectional predictions based on payoff heterogeneity (Proposition 4) .

A.1. Proof of Proposition 1.

Proof. Because attention is chosen at the first interview, before the between-interview news arrives, $m^*_{i,t}(U_t)$ is predetermined with respect to that news. Apply the behavioral operator Equation (2.1) at the two interviews. At the first interview (date t), the household reports

$$\mathbb{E}^B_{i,t}[\pi_{t+1}] = (1 - m^*_{i,t})\bar{\pi} + m^*_{i,t}\mathbb{E}_t[\pi_{t+1}].$$

At the second interview (date $t+1$), after the news has arrived, it reports

$$\mathbb{E}^B_{i,t+1}[\pi_{t+1}] = (1 - m^*_{i,t})\bar{\pi} + m^*_{i,t}\mathbb{E}_{t+1}[\pi_{t+1}].$$

Both reports use the same (time-invariant) attention weight $m^*_{i,t}$. Subtracting, and using the news identity Equation (2.1), $\mathbb{E}_{t+1}[\pi_{t+1}] - \mathbb{E}_t[\pi_{t+1}] = \theta\,\varepsilon^{mp}_{t+1} + \Gamma'\varepsilon^o_{t+1}$, the wave-to-wave revision is

$$\Delta\pi^e_{i,t+1} = \mathbb{E}^B_{i,t+1}[\pi_{t+1}] - \mathbb{E}^B_{i,t}[\pi_{t+1}] = m^*_{i,t}\big(\theta\,\varepsilon^{mp}_{t+1} + \Gamma'\varepsilon^o_{t+1}\big).$$

Differentiating with respect to the policy surprise gives $\partial\Delta\pi^e_{i,t+1}/\partial\varepsilon^{mp}_{t+1} = \theta\, m^*_{i,t}$. Since $m^*_{i,t}$ is pinned down by $(U_t, \omega_i, \kappa_i)$, it is independent of ε^{mp}_{t+1}. ■

A.2. Proof of Proposition 2.

Proof. Start from the individual wave-to-wave revision established in Proposition 1,

$$\Delta\pi^e_{i,t+1} := m^*_{i,t}(U_t)\big(\theta\,\varepsilon^{mp}_{t+1} + \Gamma'\varepsilon^o_{t+1}\big).$$

Taking the cross-sectional mean and using that all terms are linear in $m^*_{i,t}$ gives

$$\Delta\pi^e_{t+1} \equiv \mathbb{E}_i[\Delta\pi^e_{i,t+1}] := \underbrace{\mathbb{E}_i[m^*_{i,t}(U_t)]}_{\Lambda_t}\,\theta\,\varepsilon^{mp}_{t+1} \;+\; \Lambda_t\Gamma'\varepsilon^o_{t+1}.$$

By construction $\Lambda_t \in [0,1]$, so the exact aggregation identity is Equation (2). In empirical specifications that use lower-frequency revisions, the other-news term and measurement error enter the residual; under the baseline orthogonality condition and predetermined attention, the corresponding MP slope remains $\theta\Lambda_t$. ■

A.3. Proof of Proposition 3.

Proof. From Equation (2.1),

$$\frac{\partial m^*_{i,t}(U_t)}{\partial U_t} = \frac{\omega_i \kappa_i}{(\omega_i U_t + \kappa_i)^2} = \frac{m^*_{i,t}(U_t)\big(1 - m^*_{i,t}(U_t)\big)}{U_t} > 0.$$

Hence the impact pass-through coefficient $b_{i,t}(U_t) = \theta m^*_{i,t}(U_t)$ satisfies

$$\frac{\partial b_{i,t}(U_t)}{\partial U_t} = \theta \frac{m^*_{i,t}(U_t)\big(1 - m^*_{i,t}(U_t)\big)}{U_t} < 0$$

for $\theta < 0$. For group g, let $S_g(U) \equiv \partial[m_g(U)\theta]/\partial U = \theta \cdot \frac{m_g(U)[1-m_g(U)]}{U}$. If $m_A(1-m_A) > m_I(1-m_I)$, then $|S_A(U)| > |S_I(U)|$ because $|\theta|$ and U cancel in the comparison. A sufficient condition is that both groups' attention levels lie on the increasing portion of the sensitivity curve $f(m) = m(1-m)$, *i.e.*, $0 \le m_I < m_A \le \frac{1}{2}$. Since $f(m)$ is strictly increasing on $[0, \frac{1}{2}]$, this inequality implies $m_A(1-m_A) > m_I(1-m_I)$, ensuring that the sensitivity to uncertainty is strictly higher for the attentive group $(|S_A(U)| > |S_I(U)|)$. ■

A.4. Proof of Proposition 4.

Proof. Fix $U_t > 0$. From Equation (2.1),

$$m^*_{i,t}(U_t) = \frac{\omega_i U_t}{\omega_i U_t + \kappa_i}.$$

(i) Attention ordering. A direct calculation gives $\partial m^*_{i,t}/\partial \omega_i = \frac{U_t \kappa_i}{(\omega_i U_t + \kappa_i)^2} > 0$ and $\partial m^*_{i,t}/\partial \kappa_i = -\frac{\omega_i U_t}{(\omega_i U_t + \kappa_i)^2} < 0$, so $m^*_{i,t}$ is strictly increasing in ω_i and strictly decreasing in κ_i.

(ii) Pass-through ordering. The individual MP pass-through magnitude is $\left|\partial \Delta \pi^e_{i,t+1}/\partial \varepsilon^{mp}_{t+1}\right| = |\theta|\, m^*_{i,t}(U_t)$ by Proposition 1. Monotonicity then follows from part (i).

(iii) Selection into "attentive/accurate". For any threshold $\tau \in (0,1)$, $A_{i,t} = \mathbf{1}\{m^*_{i,t} \ge \tau\}$ is nondecreasing in ω_i and nonincreasing in κ_i because $m^*_{i,t}$ is monotone in those parameters. ■

B Model Extension

This appendix shows that our four testable implications do not rely on the baseline choice of a linear attention weight or quadratic attention costs. The first subsection establishes a general comparative-statics result (Lemma B.1) for an arbitrary increasing attention mapping $\phi(\cdot)$ and strictly convex cost $\psi_i(\cdot)$: the optimal attention m^*_i is unique, increases with payoff-relevant uncertainty U_t and stakes ω_i, and decreases with costs κ_i. The linear/quadratic specification follows as a corollary. The second subsection introduces a common noisy public signal observed before attention is chosen and shows that it synchronizes attention choices—micro-founding time variation in the aggregate attentiveness index—while leaving the individual gating, aggregate scaling, uncertainty amplification, and payoff-heterogeneity predictions unchanged. The third subsection (Appendix B.3) develops a noisy-signal/rational-inattention microfoundation in which the steady-state Kalman gain K_i replaces the attention weight m_i and delivers the same four propositions.

B.1. General attention mapping and convex costs

We show that the main comparative statics do not rely on a linear attention weight or quadratic costs.

Assumption 1 (Information and costs)**.** *The expectations operator is* $E_i^B = \bar{\pi} + \phi(m_i)(E^* - \bar{\pi})$ *with* $\phi : [0,1] \to [0,1]$*,* $\phi \in C^2$*,* $\phi(0) = 0$*,* $\phi(1) = 1$*,* $\phi'(m) > 0$*, and* $\phi''(m) \leq 0$*. The attention cost is* $\psi_i(m)$*, where* $\psi_i \in C^2$ *is strictly convex on* $[0,1]$ *with* $\psi_i'(0) = 0$ *and* $\psi_i''(m) > 0$*. Benefits are scaled by* $\omega_i > 0$*, costs by* $\kappa_i > 0$ *(possibly via* $\psi_i(m) = \kappa_i \tilde{\psi}(m)$ *with* $\tilde{\psi}'(m) > 0$*). Let* U_t *denote the payoff-relevant variance from Equation* (2.1)*.*

With mean-squared forecast loss, the per-period objective can be written (up to a positive multiplicative constant) as

$$\mathcal{L}_i(m; U_t, \omega_i, \kappa_i) \;=\; \frac{1}{2}\omega_i U_t [1 - \phi(m)]^2 \;+\; \psi_i(m),$$

and any interior optimum $m_i^* \in (0,1)$ solves the first-order condition

$$\omega_i U_t \big(1 - \phi(m_i^*)\big)\phi'(m_i^*) \;=\; \psi_i'(m_i^*).$$

Lemma B.1 (Comparative statics under general ϕ and ψ)**.** *Under Assumption 1, there is a unique minimizer* $m_i^*(U_t, \omega_i, \kappa_i) \in (0,1)$ *satisfying Equation* (B.1)*. Moreover,*

$$\frac{\partial m_i^*}{\partial U_t} > 0, \qquad \frac{\partial m_i^*}{\partial \omega_i} > 0, \qquad \textit{and if } \psi_i(m) = \kappa_i \tilde{\psi}(m) \textit{ with } \tilde{\psi}'(m) > 0, \textit{ then } \frac{\partial m_i^*}{\partial \kappa_i} < 0.$$

Proof. Define $F(m; U, \omega, \kappa) \equiv \omega U (1 - \phi(m))\phi'(m) - \psi_i'(m)$. By Assumption 1,

$$F(0) = \omega U \phi'(0) > 0, \qquad F(1) = -\psi_i'(1) < 0,$$

where $\psi_i'(1) > 0$ follows from strict convexity and $\psi_i'(0) = 0$. Therefore an interior solution exists. Moreover,

$$F_m = \omega U \{-(\phi'(m))^2 + (1 - \phi(m))\phi''(m)\} - \psi_i''(m) < 0$$

because $\phi' > 0$, $\phi'' \leq 0$, $1 - \phi \geq 0$, and $\psi_i'' > 0$. Hence F is strictly decreasing in m, so the interior solution is unique. By the implicit function theorem,

$$\frac{\partial m^*}{\partial U} = -\frac{F_U}{F_m} = -\frac{\omega(1-\phi)\phi'}{F_m} > 0$$
$$\frac{\partial m^*}{\partial \omega} = -\frac{F_\omega}{F_m} = -\frac{U(1-\phi)\phi'}{F_m} > 0.$$

If $\psi_i(m) = \kappa_i \tilde{\psi}(m)$ with $\tilde{\psi}'(m) > 0$, then $F_\kappa = -\tilde{\psi}'(m) < 0$, so $\partial m^* / \partial \kappa = -F_\kappa / F_m < 0$. ■

Corollary B.1 (Linear weight/quadratic cost)**.** *If* $\phi(m) = m$ *and* $\psi(m) = \frac{1}{2}\kappa m^2$*, then Equation* (B.1) *reduces to* $\omega U(1-m^*) = \kappa m^*$*, hence the closed form*

$$m^*(U,\omega,\kappa) = \frac{\omega U}{\omega U+\kappa}, \qquad \phi(m^*) = m^*.$$

All four testable implications in the main text follow immediately.

Proof. We substitute the specific functional forms $\phi(m) = m$ and $\psi(m) = \frac{1}{2}\kappa m^2$. The derivatives are $\phi'(m) = 1$ and $\psi'(m) = \kappa m$. Plugging these into the FOC gives: $\omega U_t(1-m^*)(1) = \kappa m^*$. Rearranging yields $m^* = \frac{\omega U_t}{\omega U_t+\kappa}$. ■

Implications. Replacing m_i^* by $\phi(m_i^*)$ in the impact coefficient delivers the same four predictions: (i) individual *gating* (only attentive types load on policy news), (ii) aggregate scaling by $E[\phi(m_i^*)]$, (iii) amplification when U_t is higher, and (iv) larger pass-through for high-ω_i/low-κ_i groups.

B.2. Public Signal Extension

This appendix shows that introducing a common, noisy public signal s_t that arrives *before* attention choices does not alter the four testable implications in the main text: (i) individual gating; (ii) aggregate scaling; (iii) uncertainty amplification; and (iv) payoff heterogeneity. The public signal provides a simple micro-foundation for time-variation in aggregate attentiveness by synchronizing attention choices across households.

Suppose the public signal is about next-period inflation π_{t+1} (*e.g.*, a highly publicized data release or headline), observed before attention choice. Let s_t be informative about π_{t+1} so that the posterior variance

$$U_t^{\text{post}} \equiv Var(\pi_{t+1} \mid s_t)$$

is (weakly) smaller than the prior forecast variance $\text{Var}(\pi_{t+1})$ and (weakly) decreasing in the signal's precision. In this Bayesian extension the payoff-relevant uncertainty is the genuine forecast variance, which plays the role of the reduced-form U_t from the main text. Under quadratic forecast loss, the relevant loss component scales with U_t^{post}.

Assumption 2. *The public signal about the inflation level yields a posterior variance* $U_t^{post} = H(U_t,\tau_s)$ *with* $H_U > 0$ *and* $H_{\tau_s} < 0$*, where* τ_s *is the signal precision. (For Gaussian-normal conjugacy,* $U_t^{post} = (U_t^{-1}+\tau_s)^{-1}$*, which does not depend on the realization of* s_t*.)*

Given s_t (and τ_s), the household chooses attention m to minimize the objective function based on the posterior variance U_t^{post}, using the same structure as in Appendix B.1:

$$\mathscr{L}_i(m; U_t^{\text{post}}, \omega_i, \kappa_i) = \frac{1}{2}\omega_i U_t^{\text{post}}[1-\phi(m)]^2 + \psi_i(m),$$

where $\phi(m)$ is the attention mapping and $\psi_i(m)$ is the cost function (incorporating κ_i), satisfying Assumption 1.

Lemma B.2 (Optimal attention with a public level signal)**.** *Under Assumption 2 and the properties of* ϕ *and* ψ_i *from Assumption 1, the unique optimal attention* $m_i^* = m_i^*(U_t^{post}, \omega_i, \kappa_i)$ *is (weakly)* *increasing in U_t^{post} and in ω_i, and (weakly) decreasing in κ_i and in the signal precision τ_s (via U_t^{post}).*

Proof. The FOC defining the unique optimum $m_i^* = m_i^*(U_t^{\text{post}}, \omega_i, \kappa_i)$ is:

$$\omega_i U_t^{\text{post}}(1-\phi(m_i^*))\phi'(m_i^*) = \psi'_i(m_i^*)$$

This FOC has the exact same structure as Equation (B.1), with U_t^{post} replacing U_t. The comparative statics with respect to U_t^{post}, ω_i, and κ_i therefore follow directly from the proof of Lemma B.1. For the effect of signal precision τ_s, we use Assumption 2 ($U_t^{\text{post}} = H(U_t, \tau_s)$ with $\frac{\partial H}{\partial \tau_s} < 0$) and the chain rule:

$$\frac{\partial m_i^*}{\partial \tau_s} = \frac{\partial m_i^*}{\partial U_t^{\text{post}}} \frac{\partial U_t^{\text{post}}}{\partial \tau_s} = \frac{\partial m_i^*}{\partial U_t^{\text{post}}} \frac{\partial H}{\partial \tau_s} < 0$$

since $\frac{\partial m_i^*}{\partial U_t^{\text{post}}} > 0$ and $\frac{\partial H}{\partial \tau_s} < 0$. ■

Proposition B.1 (Robustness of implications: level signal)**.** *Replacing U_t by U_t^{post} leaves all four implications intact:*

1. ***Individual gating**: the impact coefficient remains proportional to $\phi(m_i^*)\,\theta$ with $m_i^* = m_i^*(U_t^{post}, \omega_i, \kappa_i)$.*

2. ***Aggregate scaling**: $\beta_t^{agg} = \theta\, E[\phi(m_i^*)]$ scales with average attention; a more precise public signal reduces U_t^{post} and thus lowers average attention, but does not alter the gating logic.*

3. ***Uncertainty amplification**: when residual uncertainty U_t^{post} is higher (e.g., the public signal is imprecise or absent), optimal attention is higher and pass-through is stronger.*

4. ***Payoff heterogeneity**: for any U_t^{post}, higher ω_i / lower κ_i types choose more attention and exhibit larger pass-through.*

Proof. The logic follows directly from substituting U_t^{post} for U_t in the derivations underpinning the four main implications and using the comparative statics established in Lemma B.2.

1. **Individual Gating**: The pass-through coefficient is $\frac{\partial \Delta \pi^e_{i,t+1}}{\partial \varepsilon^{mp}_{t+1}} = \phi(m_i^*)\theta$, where m_i^* now depends on U_t^{post}. The form is identical.

2. **Aggregate Scaling**: The aggregate coefficient is $\Lambda_t^{\text{post}}\theta$, where $\Lambda_t^{\text{post}} = \mathbb{E}_i[\phi(m_i^*(U_t^{\text{post}}))]$. It still scales with the (now potentially lower) average attention.

3. **Uncertainty Amplification**: The sensitivity to uncertainty is $\frac{\partial(\phi(m_i^*)\theta)}{\partial U_t^{\text{post}}} = \theta\phi'(m_i^*)\frac{\partial m_i^*}{\partial U_t^{\text{post}}} < 0$. Higher U_t^{post} implies stronger pass-through.

4. **Payoff Heterogeneity**: Since $\frac{\partial m_i^*}{\partial \omega_i} > 0$, $\frac{\partial m_i^*}{\partial \kappa_i} < 0$, and $\phi' > 0$, the pass-through magnitude $|\phi(m_i^*)\theta|$ remains larger for higher ω_i / lower κ_i types, given U_t^{post}.

■

Discussion. A *level* signal reduces residual uncertainty and thereby lowers the marginal value of costly attention, but conditional on the chosen attention, the pass-through of monetary policy news is still multiplied by the attention weight. Since s_t is common, its *level* effect on beliefs is absorbed by time variation (*e.g.*, month fixed effects) in empirical specifications that include them; under that design, the slope with respect to policy surprises is pinned down by the attention-weighted pass-through term.

B.3. Noisy-Signal / Rational-Inattention Microfoundation

The behavioral expectations model in the main text treats attention as a weight $m_i \in [0,1]$ that blends a rational forecast with an anchor. This subsection develops an alternative microfoundation based on *noisy information acquisition.* Instead of directly choosing a blending weight, the household chooses the *precision* of a noisy signal about the current state of inflation; higher precision is more costly. The household then uses Bayesian updating (Kalman filtering) to form beliefs about the true state and forecasts future inflation. The key question is whether this alternative yields the same testable implications for the expectation revision $\Delta\pi_i^e$.

B.3.1. Setup. *Inflation process.* As in the main text, the household is observed at two consecutive dates t and $t+1$ and forecasts next-period inflation π_{t+1}; inflation follows an AR(1),

$$\pi_{s+1} = \bar{\pi} + \rho(\pi_s - \bar{\pi}) + \varepsilon_{s+1},$$

where ε_{s+1} is a composite shock with $\varepsilon_{s+1} \sim \mathcal{N}(0,\sigma_\varepsilon^2)$. For the MP-specific analysis we decompose $\varepsilon_{s+1} = \theta\varepsilon_{s+1}^{mp} + \Gamma'\varepsilon_{s+1}^{o}$, but for the signal-extraction problem we work with the composite.

Information structure. The household does *not* observe π_s directly. Instead, at date s, household i receives a noisy signal

$$x_{i,s} = \pi_s + \eta_{i,s}, \qquad \eta_{i,s} \sim \mathcal{N}(0,\sigma_{\eta,i}^2),$$

where $\eta_{i,s}$ is independent of π_s, ε_s, and $\eta_{j,s}$ for $j \neq i$. The signal-noise variance $\sigma_{\eta,i}^2$ is chosen by the household (time-invariant in steady state), with higher precision being more costly. Timing within a period: (1) ε_s realizes and determines π_s; (2) the household receives $x_{i,s}$; (3) it updates beliefs by Bayes' rule (Kalman); (4) if interviewed, it reports a forecast.

Bayesian updating. At the start of period s, the prior about π_s propagates the previous posterior through the AR(1):

$$\hat{\pi}_{i,s}^- = \bar{\pi} + \rho(\hat{\pi}_{i,s-1} - \bar{\pi}),$$
$$P_{i,s}^- = \rho^2 P_{i,s-1} + \sigma_\varepsilon^2,$$

where $(\hat{\pi}_{i,s-1}, P_{i,s-1})$ is the previous-period posterior. After receiving $x_{i,s}$,

$$\hat{\pi}_{i,s} = \hat{\pi}_{i,s}^- + K_{i,s}\big(x_{i,s} - \hat{\pi}_{i,s}^-\big),$$
$$P_{i,s} = (1 - K_{i,s})\, P_{i,s}^-,$$

with Kalman gain

$$K_{i,s} = \frac{P^-_{i,s}}{P^-_{i,s} + \sigma^2_{\eta,i}} \in [0,1].$$

The gain $K_{i,s}$ plays the same role as the attention weight $m_{i,s}$ in the behavioral model: Equation (B.3.1) is a weighted average of the prior ("anchor") and the signal ("data"), just like $\mathbb{E}^B = (1-m)\bar{\pi} + m\mathbb{E}[\pi_{t+1}]$. When $\sigma^2_{\eta,i}$ is small, $K \to 1$ and the posterior tracks the signal; when $\sigma^2_{\eta,i} \to \infty$, $K \to 0$ and the household sticks with the prior.

Steady-state Kalman filter and endogenous precision. With constant parameters, the variance recursion converges to a steady state in which $P_{i,s} = P_i$, $P^-_{i,s} = P^-_i$, and $K_{i,s} = K_i$. Setting $P^-_i = \rho^2 P_i + \sigma^2_\varepsilon$ and $P_i = (1-K_i)P^-_i$ yields the steady-state Riccati equation

$$P^-_i = \rho^2(1-K_i)\,P^-_i + \sigma^2_\varepsilon \qquad \Longrightarrow \qquad P^-_i = \frac{\sigma^2_\varepsilon}{1-\rho^2(1-K_i)}.$$

The household chooses signal precision $\tau_i \equiv 1/\sigma^2_{\eta,i}$ to minimize

$$\min_{\tau\geq 0} \frac{\omega_i}{2} P_i(\tau) + \frac{\kappa_i}{2}\tau,$$

where $P_i(\tau) = (1-K_i)P^-_i$ and $K_i = P^-_i\tau/(1+P^-_i\tau)$. Taking P^-_i as approximately determined by exogenous parameters (exact when $\rho = 0$, and a good approximation when $\frac{\rho^2 K_i}{1-\rho^2} \ll 1$, i.e., when the endogenous feedback from attention to steady-state prior uncertainty is small), the problem becomes

$$\min_{\tau\geq 0} \frac{\omega_i}{2} \cdot \frac{P^-_i}{1+P^-_i\tau} + \frac{\kappa_i}{2}\tau,$$

with first-order condition

$$-\frac{\omega_i}{2} \cdot \frac{(P^-_i)^2}{(1+P^-_i\tau)^2} + \frac{\kappa_i}{2} = 0 \quad \Longrightarrow \quad \tau^*_i = \left(\sqrt{\frac{\omega_i}{\kappa_i}} - \frac{1}{P^-_i}\right)^+,$$

so the household acquires no information when $P^-_i < \sqrt{\kappa_i/\omega_i}$. Substituting into Equation (B.3.1) gives the steady-state Kalman gain

$$K^*_i = 1 - \sqrt{\frac{\kappa_i}{\omega_i}}\frac{1}{P^-_i} \qquad \text{when } \tau^*_i > 0; \qquad K^*_i = 0 \text{ otherwise.}$$

Because K^*_i is *time-invariant* in steady state, we drop the time subscript. It depends on P^-_i (driven by aggregate shock volatility σ^2_ε and persistence ρ; higher σ^2_ε raises P^-_i and K^*_i) and on the cost-benefit ratio ω_i/κ_i.

Comparative statics (all time-invariant). (i) $\partial K^*_i/\partial\sigma^2_\varepsilon > 0$: a more volatile economy raises P^-_i and hence K^*_i, paralleling $\partial m^*/\partial U > 0$. (ii) $\partial K^*_i/\partial\omega_i > 0$: higher stakes raise attention. (iii) $\partial K^*_i/\partial\kappa_i < 0$: higher costs reduce attention. These match the behavioral model in direction.

State dependence. While K^*_i is constant *within* a regime, it shifts *across* regimes when σ^2_ε changes. Indexing regimes by $r \in \{H, L\}$ with $\sigma^2_{\varepsilon,H} > \sigma^2_{\varepsilon,L}$, $P^-_{i,H} > P^-_{i,L} \Rightarrow K^*_{i,H} > K^*_{i,L}$: households in the high-volatility regime are more attentive and respond more strongly to MP shocks, consistent with Proposition 3.

B.3.2. Forecasts and expectation revision. In steady state (with $K_i \equiv K_i^*$), the household forecasts one-year-ahead inflation π_{t+1}. At the first interview the date-$(t+1)$ shock has not yet realized, so the forecast iterates the AR(1) one step from the current-state belief $\hat{\pi}_{i,t}$,

$$\mathbb{E}_{i,t}[\pi_{t+1}] = \bar{\pi} + \rho(\hat{\pi}_{i,t} - \bar{\pi}).$$

At the second interview π_{t+1} is the current state, observed through a noisy signal, so the household's expectation of π_{t+1} is its updated posterior, $\mathbb{E}_{i,t+1}[\pi_{t+1}] = \hat{\pi}_{i,t+1}$. The expectation revision is therefore

$$\Delta\pi_i^e \equiv \mathbb{E}_{i,t+1}[\pi_{t+1}] - \mathbb{E}_{i,t}[\pi_{t+1}] = \hat{\pi}_{i,t+1} - \left[\bar{\pi} + \rho(\hat{\pi}_{i,t} - \bar{\pi})\right].$$

The bracketed term is exactly the second-interview prior for π_{t+1}, namely $\hat{\pi}^-_{i,t+1} = \bar{\pi} + \rho(\hat{\pi}_{i,t} - \bar{\pi})$, so the revision equals the Kalman correction, $\Delta\pi_i^e = K_i\left(x_{i,t+1} - \hat{\pi}^-_{i,t+1}\right)$. Substituting $\pi_{t+1} = \bar{\pi} + \rho(\pi_t - \bar{\pi}) + \varepsilon_{t+1}$, the innovation is $x_{i,t+1} - \hat{\pi}^-_{i,t+1} = \rho\, e_{i,t} + \varepsilon_{t+1} + \eta_{i,t+1}$, where $e_{i,t} \equiv \pi_t - \hat{\pi}_{i,t}$ is the date-t estimation error. The revision therefore decomposes as

$$\Delta\pi_i^e = \underbrace{K_i\,\rho\,e_{i,t}}_{\textbf{(I): learning about past error}} + \underbrace{K_i\,\varepsilon_{t+1}}_{\textbf{(II): response to new shock}} + \underbrace{K_i\,\eta_{i,t+1}}_{\textbf{(III): signal noise}}.$$

Decomposing $\varepsilon_{t+1} = \theta\varepsilon^{mp}_{t+1} + \Gamma'\varepsilon^{o}_{t+1}$, the MP component of the revision is

$$\frac{\partial\,\Delta\pi_i^e}{\partial\,\varepsilon^{mp}_{t+1}} = \theta\,K_i,$$

which is exactly the behavioral gain $\theta m_{i,t}$ with the Kalman gain K_i in place of the attention weight. Term (I) captures "catching up" on past misperceptions; since $e_{i,t}$ is predetermined with respect to ε^{mp}_{t+1}, it enters the residual. Term (III) is pure signal noise, mean zero and orthogonal to everything.

B.3.3. Comparison: behavioral vs. noisy-signal model.

	Behavioral	**Noisy Signal**
Attention parameter	$m_{i,t} \in [0,1]$	$K_i \in [0,1]$ (steady-state Kalman gain)
Choice variable	Weight on rational forecast	Signal precision $\tau_i = 1/\sigma^2_{\eta,i}$
Time dependence	$m_{i,t}$ chosen each period	K_i time-invariant in steady state
MP pass-through	$\theta\,m_{i,t}$	$\theta\,K_i$
Extra residual terms	None (revision = $m_{i,t}\times$ news)	Past-error learning + signal noise
$\uparrow$ uncertainty $\Rightarrow$	$\uparrow m^* \Rightarrow \uparrow$ pass-through	$\uparrow K^* \Rightarrow \uparrow$ pass-through (across regimes)
$\uparrow \omega_i \Rightarrow$	$\uparrow m^* \Rightarrow \uparrow$ pass-through	$\uparrow K^* \Rightarrow \uparrow$ pass-through
$\uparrow \kappa_i \Rightarrow$	$\downarrow m^* \Rightarrow \downarrow$ pass-through	$\downarrow K^* \Rightarrow \downarrow$ pass-through

Both models give MP pass-through $\theta\times$ attention—$\theta m_{i,t}$ in the behavioral model and θK_i in the noisy-signal model. They differ in three ways: the steady-state K_i is time-invariant within a regime (sharper than per-period re-optimization); the noisy-signal model generates a "catch-up" term (I) absent from the behavioral model; and it generates idiosyncratic signal noise (III) that raises residual variance (lowering R^2) without biasing coefficients. It also micro-founds belief heterogeneity: agents hold heterogeneous beliefs $\hat{\pi}_{i,t}$ about the current state, generating cross-sectional dispersion in expectations even absent new shocks.

Proposition B.2 (The four implications in the noisy-signal model)**.** *With the steady-state Kalman gain K_i in place of the behavioral attention weight, the noisy-signal model delivers the same four implications:*

1. ***Individual gating**: $\partial\Delta\pi_i^e/\partial\varepsilon_{t+1}^{mp} = \theta\, K_i$, so households with low K_i (noisy signals) do not revise while those with high K_i (precise signals) revise strongly.*

2. ***Aggregate scaling**: the cross-sectional mean revision loads on ε_{t+1}^{mp} with coefficient $\theta\,\bar{K}$, where $\bar{K} = \int K_i\, di$ is the economy's average steady-state Kalman gain.*

3. ***Uncertainty amplification**: across volatility regimes, higher σ_ε^2 raises K_i^*, so MP pass-through is larger in magnitude in high-uncertainty states; within a stable regime K_i is constant.*

4. ***Payoff heterogeneity**: K_i is increasing in ω_i and decreasing in κ_i, so households with higher stakes (or lower costs) acquire more precise signals and exhibit larger pass-through.*

Proof.

1. **Individual gating**: in the revision decomposition Equation (B.3.2), ε_{t+1}^{mp} enters only through the new-shock term $K_i\varepsilon_{t+1}$; with $\varepsilon_{t+1} = \theta\varepsilon_{t+1}^{mp} + \Gamma'\varepsilon_{t+1}^{o}$ its coefficient is θK_i. Since K_i is pinned down by $(\sigma_\varepsilon^2, \omega_i, \kappa_i)$, it is independent of ε_{t+1}^{mp}.

2. **Aggregate scaling**: every term of Equation (B.3.2) is linear in K_i, so taking the cross-sectional mean replaces K_i by $\bar{K}$ and the MP slope by $\theta\bar{K}$.

3. **Uncertainty amplification**: a higher σ_ε^2 raises the steady-state prior variance P_i^- through the Riccati equation Equation (B.3.1) and hence the gain K_i^* in Equation (B.3.1), so $|\theta K_i^*|$ rises. Within a regime σ_ε^2 is fixed, so K_i does not vary over time.

4. **Payoff heterogeneity**: from Equation (B.3.1), $\partial K_i^*/\partial\omega_i > 0$ and $\partial K_i^*/\partial\kappa_i < 0$, and the pass-through magnitude $|\theta K_i^*|$ inherits these signs.

■

The two models are *observationally equivalent* for the regression-based tests in the paper: both predict pass-through $\propto$ attention, attention endogenous to uncertainty/stakes/costs, and all four propositions with the same sign and ordering. The noisy-signal model additionally micro-founds belief heterogeneity, connects to the rational-inattention literature (Sims, 2003, Maćkowiak and Wiederholt, 2009), generates "catching-up" dynamics, and matches the way the noisy-signal critique is often phrased (whether a household drew a good or a bad signal). The behavioral model has the advantages of analytical simplicity, a direct link to Gabaix (2020), and a transparent attention weight. For the paper, the behavioral model is the primary specification because it is simpler and delivers the four propositions without additional structure; the noisy-signal model serves as a robustness/microfoundation check showing the predictions are not artifacts of the particular information-processing assumption.

C Data Description and Sources

In this appendix, we detail the data we used in our empirical exercises.

Michigan Survey of Consumers. Our micro-level analysis relies on the University of Michigan Survey of Consumers (MSC), a monthly telephone survey that re-interviews a portion of respondents six months later, creating a rotating panel structure. To construct our baseline attentiveness proxy, we utilize the question on news heard about business conditions (Question A6):

> *"During the last few months, have you heard of any favorable or unfavorable changes in business conditions?"*

Responses are coded based on whether the respondent reports hearing favorable news, unfavorable news, or no news at all.

Our primary dependent variable, the revision in inflation expectations, is constructed from the one-year-ahead inflation expectation questions (Questions A12/A12b):

> *"During the next 12 months, do you think that prices in general will go up, go down, or stay where they are now?"*
>
> *"By about what percent do you expect prices to go up/down on the average, during the next 12 months?"*

To test the coherence of beliefs, we employ questions regarding unemployment expectations (Question A10):

> *"How about people out of work during the coming 12 months — do you think that there will be more unemployment than now, about the same, or less unemployment than now?"*

and vehicle purchasing attitudes (Question A18):

> *"Thinking now of the automobile market — do you think the next 12 months or so will be a good time or a bad time to buy a new vehicle, such as a car, pickup, van or sport utility vehicle?"*

For demographic heterogeneity and control variables, we use the following survey items: homeownership status (Question A24) *"Do you (and your family living there) own the home that you live in?"*; stock market participation (Question A28) *"Do you (and your family living there) have any investments in the stock market, including any publicly traded stock that is directly owned, stocks in mutual funds, or stocks in any of your retirement accounts...?"*; age (Question A29); gender (Question A30); marital status (Question A31); education level (Question A32); and total family income (Question A23) which is used to construct income quartiles.

For the five-year expectations analysis (Appendix Table D.12), we additionally use the five-year-ahead inflation expectation questions (Questions A12c/A12d), which follow the same format as the one-year questions but ask respondents about expected price changes "during the next 5 to 10 years." The revision is constructed analogously as the change in the five-year expectation between the first and second interviews, with values trimmed at ±20%.

Beyond the favorable/unfavorable/no-news split, the MSC records the content of the first reported business-news mention in a coded variable (`NEWS1`). Following the official MSC codebook, we group these content codes into broad topical categories: government and policy (codes 10–19, 50–59), labor and real activity (codes 20–24, 27–28, 45–46, 60–64, 67–68, 85–86), prices/rates/credit (codes 30–33, 37, 39, 71–73, 77, 79), profits and financial markets (codes 25, 35–36, 38, 65, 74–76, 78), and general sentiment (codes 40–44, 47–49, 80–84, 87–89). These categories are used in the content validation exercise (Appendix Table D.1).

For the richer-controls robustness exercise (Appendix Table D.14), we draw on additional MSC survey items beyond the standard demographic and financial controls. These include the three composite sentiment indices published by the MSC—the Index of Consumer Sentiment (ICS), the Index of Current Economic Conditions (ICC), and the Index of Consumer Expectations (ICE)—as well as individual survey items on government economic policy (GOVT, Question A11) *"Speaking of the economic policy of the government—would you say the government is doing a good job, only fair, or a poor job?"*; expected interest rate changes (RATEX, Question A15) *"No one can say for sure, but what do you think will happen to interest rates for borrowing money during the next 12 months?"*; expected unemployment (UNEMP, Question A10); buying conditions for large household durables (DUR, Question A5) *"About the big things people buy for their homes—do you think now is a good or a bad time to buy?"*; vehicles (CAR, Question A18); and houses (HOM, Question A19) *"Do you think now is a good time or a bad time to buy a house?"*. Additional items include expected gasoline price changes (GAS1, Question A14a; available for a subset of months beginning in 2006), expected change in real family income (RINC, Question A3) *"During the next year or two, do you expect that your income will go up more than prices, about the same, or less than prices?"*; personal financial expectations (PEXP, Question A4) *"Now looking ahead—do you think that a year from now you will be better off financially, or worse off, or just about the same?"*; and expected business conditions over the next year (BEXP, Question A8) *"Now turning to business conditions in the country as a whole—do you think that during the next twelve months we'll have good times financially, or bad times, or what?"*.

To assess the persistence of accuracy classifications (Appendix Table D.7), we exploit the MSC rotating panel structure by reconstructing the accuracy classification at the second interview. This requires the news response (`NEWS1`) and the unemployment benchmark at the second-interview date, from which we apply the same classification rule used at the first interview.

Aggregate Inflation Expectations. For the aggregate time-series regressions, the dependent variable is the six-month change in the median one-year-ahead inflation expectation published by the University of Michigan Survey of Consumers (FRED code: `MICH`). Because this series is computed from the full monthly cross-section of respondents (approximately 500 per month), it is not restricted to the rotating-panel subsample used in the micro analysis. At the micro level, the dependent variable is the individual-level revision $\pi^e_{i,t+6} - \pi^e_{i,t}$ for panel respondents reinterviewed after six months.

Macroeconomic Variables. We source aggregate macroeconomic time series primarily from the Federal Reserve Economic Data (FRED) database. To construct the accuracy benchmark for our attentiveness measure, we use the civilian unemployment rate (FRED code: `UNRATE`) and calculate its three-month change. For robustness checks regarding the accuracy definition and as contemporaneous controls, we utilize the Industrial Production Index (FRED code: `INDPRO`) and the Consumer Price Index for All Urban Consumers: All Items (FRED code: `CPIAUCSL`). Financial conditions are proxied by the National Financial Conditions Index (FRED code: `NFCI`).

To capture the salience of gas prices, we use the U.S. Regular All Formulations Gas Price (FRED code: `GASREGCOVW`), aggregated to monthly frequency. For our state-dependence analysis, we define recessions using the NBER-based Recession Indicators for the United States (FRED code: `USREC`) and financial volatility using the CBOE Volatility Index (FRED code: `VIXCLS`). Additionally, we employ the Economic Policy Uncertainty index (EPU), based on newspaper coverage, to capture broader policy-related risks. We also use the Equity Market Volatility (EMV) tracker, a newspaper-based index that measures the frequency of articles discussing stock-market volatility alongside economic fundamentals. All three salience proxies—VIX, EPU, and EMV—are log-transformed and standardized within the sample before use in the attention-validation regressions (Appendix Table D.9, Panel B).

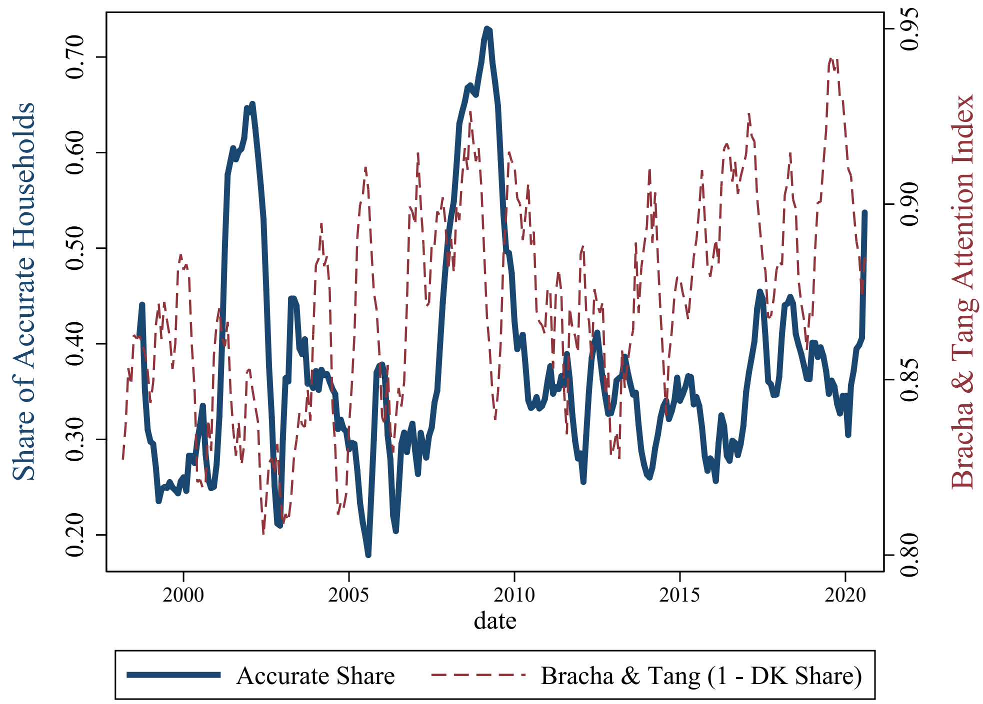
For the broader external validation of the attention proxy (Appendix Table D.10), we draw on additional sources. From FRED, we use the St. Louis Fed Financial Stress Index (FRED code: `STLFSI2`), which aggregates 18 weekly financial-market series spanning interest rates, yield spreads, and volatility into a single stress measure, and the effective federal funds rate (FRED code: `FEDFUNDS`), from which we compute the absolute monthly change. We also use the 10-Year minus

2-Year Treasury constant-maturity spread (FRED code: T10Y2Y). From Google Trends, we obtain the monthly Search Volume Index for U.S. searches of "interest rates," "Federal Reserve," and "inflation" (available from January 2004). From the Equity Market Volatility tracker of Baker, Bloom, Davis, and Kost (available at policyuncertainty.com), we use the monetary-policy and inflation sub-indices, which count newspaper articles mentioning those topics alongside equity-market volatility. All external proxies are standardized within the relevant sample window before use in regressions.

Monetary Policy Shocks. Our baseline measure of monetary policy surprises utilizes the high-frequency shock series from Nakamura and Steinsson (2018), as extended by Bauer et al. (2022). These shocks are identified from changes in interest rate futures (such as federal funds futures and Eurodollar futures) within a narrow 30-minute window around FOMC announcements, capturing the "policy news" component that encompasses both the target rate change and forward guidance. While this high-frequency identification is standard, we address potential concerns about exogeneity and information effects using two alternative measures. First, to mitigate the concern that the Fed possesses private information or that surprises correlate with publicly available data (the "Fed response to news" channel), we employ the orthogonalized shock series from Bauer and Swanson (2023). They construct this measure by regressing raw high-frequency surprises on a comprehensive set of pre-announcement macroeconomic and financial news, thereby purging the predictable component. Second, to address the "information effect" where policy tightening might be interpreted as a signal of strong economic fundamentals, we use the shock series from Bu et al. (2021). Their measure applies a Partial Least Squares approach to the full maturity spectrum of interest rates, effectively separating the pure monetary policy shock from the central bank information shock, which is identified by the comovement of interest rates and stock prices. Finally, for the time-series analysis of the Great Moderation, we use the narrative-based shocks from Romer and Romer (2004).

D Robustness

This appendix supports the robustness analysis discussed in Section 5. It is organized as follows. We first present supplementary material for the data section: a comparison with the Bracha and Tang (2024) attention measure, content-code diagnostics for the accuracy proxy, additional diagnostics for the non-attentive groups, and external validation of the attention proxy against financial stress indices, Google Trends, and newspaper-based salience measures. We then report the full regression output for the state-dependence and demographic heterogeneity analyses. The remaining subsections mirror the order of the main-text robustness discussion, beginning with the most substantive identification checks—coefficient-stability bounds (Oster, 2019), richer survey controls, and measurement-error quantification—followed by alternative monetary policy shock measures, alternative attentiveness benchmarks (differencing horizons, Industrial Production, NFCI, and a "heard news" proxy), sample restrictions, and additional specification checks.



Appendix Figure D.1: Comparison of Attention Measures

Notes: The figure plots the six-month moving averages of two aggregate attentiveness measures. The solid blue line (left axis) represents our baseline share of *Accurate* households, defined as respondents whose perception of business news matches the direction of unemployment changes. The dashed red line (right axis) represents the attention index from Bracha and Tang (2024), calculated as 1 minus the share of respondents who answered "Don't know" regarding the current inflation rate.

D.1. Comparison with Bracha and Tang (2024)

We compare our attentiveness proxy with the inflation inattention measures developed by Bracha and Tang (2024), who also utilize the Michigan Survey. Their primary measure identifies inattention based on respondents who believe prices will stay the same but cannot provide a numeric inflation estimate ("Same-DK") or provide inaccurate estimates of *current* inflation. Appendix Figure D.1 plots the six-month moving averages of our share of accurate households and Bracha and Tang's attention measure. Visually, the two series exhibit distinct dynamics. Consistent with these visual differences, the simple correlation between the two series is weak with correlation coefficient $\rho \approx 0.03$.

This low correlation reflects two fundamental distinctions. First, it captures the difference between the *stock* of economic knowledge and the *flow* of economic news. Bracha and Tang (2024)'s measure proxies for the accumulated stock of knowledge regarding current price levels, whereas our measure tracks the active acquisition of new information about changing business conditions. Second, the economic fundamentals driving attention differ across the measures. Bracha and Tang (2024) and recent work by Pfäuti (2024) focus specifically on attention to *inflation*, documenting that attention surges when inflation acts as a salient signal (*e.g.*, exceeding a certain threshold). In contrast, our measure captures attention to *general business conditions*, which we show in Figure 1, is highly sensitive to labor market deterioration (rising unemployment). Because high inflation and rising unemployment do not necessarily coincide—and indeed often move inversely over the business cycle—these distinct triggers naturally lead to divergent regimes of attentiveness. Thus, the orthogonality of these measures suggests that our findings capture a distinct channel of information processing—attention to cyclical news—that is not subsumed by inflation-specific attention.

D.2. Content of Reported Business News

The MSC business-conditions module contains more information than the simple favorable/unfavorable/no-news split used in the baseline proxy. Although the checked-in files do not include raw A6a verbatim text, they do retain the official coded first mention variable (`NEWS1`), which classifies the content of the reported news. We group these codes into broad topical buckets: labor and real activity, government and policy, prices/rates/credit, profits/financial markets, and generic macro sentiment. Appendix Table D.1 reports three facts. First, among respondents who report hearing any business news, *Accurate* households are disproportionately concentrated in labor-market and real-activity content, whereas *Inaccurate* households are relatively more concentrated in government/policy mentions. Second, the accuracy share is highest in the labor/real-activity category. Third, when the baseline micro pass-through regression is re-estimated within topical subsamples, the cleanest attentive-versus-inaccurate wedge appears precisely for labor/real-activity news. These patterns support the interpretation that the proxy is strongest when respondents report concrete cyclical information rather than vague sentiment.

D.3. Determinants of Attentiveness

Appendix Table D.2 reports the full version of the determinants regression summarized in Table 2. The dependent variable is an indicator for *Accurate* among news-reporters (*Accurate* or *Inaccurate*). Column (1) includes demographics only; column (2) adds the respondent's own economic experience and expectations, entered as "better"/"worse" dummies relative to an omitted "same" category (the personal-finance items PAGO and PEXP, real-income expectations RINC, and the business-conditions assessments BAGO and BEXP); column (3) adds the contemporaneous aggregate macro state; and column (4) adds the St. Louis Fed Financial Stress Index. The own-experience items (PAGO, PEXP, RINC) are uniformly small and, for the personal-finance variables, statistically indistinguishable from zero, whereas the *business-conditions* assessments (BAGO, BEXP) and the aggregate macro state load strongly. The result is that attentiveness is governed by engagement with the aggregate economy rather than by personal circumstance, exactly as the endogenous-attention mechanism predicts.

Appendix Table D.1: Validation from the Content of Reported Business News

Category / statistic	Accurate	Inaccurate	Haven't heard	Interpretation
Panel A: Share of topic within initial category				
Labor / real activity	52.4%	46.6%	–	More prevalent among *Accurate*
Government / policy	16.0%	20.8%	–	Relatively more common among *Inaccurate*
General sentiment	6.3%	7.4%	–	Small share of heard-news responses
No news	–	–	100.0%	*Haven't heard* is mechanically a no-news group
Panel B: Accuracy share among heard-news respondents by topic				
Labor / real activity	58.4%	41.6%	12,470 obs.	Highest accuracy share among major topics
Government / policy	49.1%	50.9%	4,540 obs.	Lowest accuracy share among major topics
Prices / rates / credit	53.9%	46.1%	2,696 obs.	Intermediate accuracy share
General sentiment	51.6%	48.4%	1,697 obs.	Similar split in a smaller topical bucket
Panel C: Topic-specific policy pass-through in heard-news subsamples				
Labor news: Accurate $\times MPS_t$	-0.22 ($t = -2.00$)		12,470 obs.	Clear attentive-household response
Labor news: Inaccurate $\times MPS_t$	0.23 ($t = 1.32$)		12,470 obs.	Essentially zero inaccurate-household response
Gov./policy: Accurate $\times MPS_t$	-0.49 ($t = -1.66$)		4,540 obs.	Significant attentive-household response
Gov./policy: Inaccurate $\times MPS_t$	-0.12 ($t = -0.36$)		4,540 obs.	No clear inaccurate-household response

Notes: `NEWS1` is the coded first mention from the MSC business-conditions question (A6/A6a). The checked-in files retain the official content codes but not the raw verbatim text. For this validation exercise, we group the official codes into broad topic buckets using the MSC codebook: labor/real activity, government/policy, prices/rates/credit, profits/financial markets, and general sentiment. Panels A and B summarize the topic distribution of heard-news responses. Panel C re-estimates the normalized baseline micro specification within topical heard-news subsamples and reports the coefficients on the attentive and inaccurate interaction terms.

D.4. Lived-Experience Falsification

A natural concern is that "Accuracy" merely reflects whether a respondent's *personal* circumstances happen to coincide with the aggregate state, rather than active processing of aggregate news. We test this directly. Among news-reporters (*Accurate* or *Inaccurate*), we split the sample by whether the respondent's own financial direction (PAGO) *contradicts* the aggregate sign of ΔU_t—for example,

Appendix Table D.2: Determinants of Attentiveness (Full Specification)

	(1) Demog.	(2) + Personal exp.	(3) + Macro state	(4) + STLFSI2
PAGO: own situation better	—	0.004	0.007	0.007
		(0.53)	(0.83)	(0.80)
PAGO: own situation worse	—	0.002	−0.008	−0.012
		(0.25)	(−0.91)	(−1.44)
PEXP: expect own better	—	−0.001	0.001	0.002
		(−0.17)	(0.15)	(0.30)
PEXP: expect own worse	—	−0.001	−0.002	−0.002
		(−0.05)	(−0.17)	(−0.15)
RINC: real income better	—	0.006	0.009	0.009
		(0.65)	(0.96)	(0.94)
RINC: real income worse	—	-0.022^{***}	-0.019^{**}	-0.014^{*}
		(−2.86)	(−2.42)	(−1.79)
BAGO: business better vs. yr ago	—	0.063^{***}	0.070^{***}	0.078^{***}
		(5.05)	(5.61)	(6.23)
BAGO: business worse vs. yr ago	—	0.079^{***}	0.050^{***}	0.038^{***}
		(6.39)	(3.98)	(3.04)
BEXP: expect business better	—	0.049^{***}	0.039^{***}	0.036^{***}
		(6.27)	(5.06)	(4.66)
BEXP: expect business worse	—	-0.045^{***}	-0.040^{***}	-0.032^{***}
		(−5.16)	(−4.61)	(−3.80)
$\lvert\Delta U_t\rvert$	—	—	0.216^{***}	0.062^{***}
			(18.30)	(4.19)
$\lvert\Delta \pi_t\rvert$	—	—	0.023^{***}	0.020^{***}
			(6.34)	(5.63)
$\Delta \log \text{IP}_t$	—	—	-0.008^{***}	−0.002
			(−7.73)	(−1.50)
STLFSI2 (financial stress)	—	—	—	0.068^{***}
				(18.92)
Demographics	Yes	Yes	Yes	Yes
R^2	0.002	0.009	0.030	0.042
N	25,043	23,991	23,991	23,991

Notes: Linear probability models for $\mathbf{1}\{\text{Accurate}\}$ among news-reporters (*Accurate* or *Inaccurate*). Personal-experience items enter as "better"/"worse" dummies ("same" omitted). All columns include demographic controls (age, age^2, log income, income quartile, education, gender, homeownership, stockholding, region, marital status). Robust t-statistics in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

a respondent who reports that their own situation improved during a period of rising aggregate unemployment—and re-estimate the baseline gating regression within each subsample. Under the lived-experience-projection alternative, the gating effect should be *weakest* precisely in the "contradicts" subsample, where personal experience points the wrong way. Appendix Table D.3 shows the opposite: the gating coefficient in the contradicting subsample (−0.414, $t = -3.40$) is *larger* in magnitude than the full-sample baseline (−0.285). Households whose personal experience pushes against the aggregate signal, yet who still classify as *Accurate*, are processing aggregate macro news despite contrary personal information—the signature of attention, not projection.

Appendix Table D.3: Lived-Experience Falsification: Gating by Personal-vs-Aggregate Alignment

PAGO subsample	$\hat{\beta}(MPS_t \times \text{Accurate})$	N
Aligned (personal matches aggregate)	-0.185 ($t = -1.36$)	14,587
"Same" (own situation unchanged)	-0.258^{*} ($t = -1.73$)	10,065
Contradicts (own *opposite* aggregate)	$\mathbf{-0.414}^{***}$ ($t = \mathbf{-3.40}$)	12,768
Full-sample baseline	-0.285^{***} ($t = -3.66$)	37,420

Notes: Baseline micro gating regression (Eq. 4.1) re-estimated within PAGO-defined subsamples of news-reporters. "Contradicts" denotes respondents whose own financial direction is opposite to the aggregate sign of ΔU_t. Robust t-statistics. $^{*}p < 0.10$, $^{***}p < 0.01$.

D.5. Cross-Benchmark Concordance and Signal Clarity

The baseline proxy benchmarks the perceived direction of business conditions against the three-month change in unemployment. To assess whether the classification is specific to unemployment, we re-score the same favorable/unfavorable responses against two alternative benchmarks—the three-month change in industrial production and in the Chicago Fed National Financial Conditions Index (NFCI)—and compute the concordance of the resulting classifications among news-reporters (*Accurate* or *Inaccurate*). Appendix Table D.4 (Panel A) shows moderate pairwise concordance (49–62%), reflecting that the three benchmarks themselves agree on the direction of the economy only in a subset of months. The right reading is one of *signal clarity* rather than a cross-sectional quality gradient: when all three benchmarks point the same way (81 of 257 months), the gating coefficient is large (-1.03, $t = -7.1$); in "mixed" months it is essentially zero (Panel B). The proxy bites hardest precisely when the aggregate signal is unambiguous.

Appendix Table D.4: Cross-Benchmark Concordance and Signal Clarity

Panel A: Pairwise concordance among news-reporters

Benchmark pair	Concordance	Cohen's κ
Unemployment vs. IP	62.2%	0.244
Unemployment vs. NFCI	49.4%	-0.016
IP vs. NFCI	51.8%	0.036

Panel B: Unemployment-based gating by month type

Subsample	$\hat{\beta}(MPS_t \times \text{Acc.})$	t	N
Full sample	-0.362^{***}	-4.59	37,445
All-agree months (81)	-1.031^{***}	-7.14	11,703
Mixed months (176)	-0.054	-0.57	25,742

Notes: Accuracy re-scored under each benchmark's three-month directional change. "All-agree" months are those in which the unemployment, IP, and NFCI benchmarks point the same direction. Gating uses the shadow-normalized shock; robust standard errors. $^{***}p < 0.01$.

D.6. Cross-Domain Robustness: A Parallel BAGO Measure and Topic-Specific Gating

We construct a second, independent accuracy measure from the MSC question on business conditions relative to a year ago (BAGO), benchmarked against the twelve-month unemployment change to match its "year ago" framing. The BAGO- and NEWS1-based classifications agree for 62.5% of news-reporters (Cohen's $\kappa = 0.22$)—related but not redundant. Re-running the baseline gating regression with the BAGO-based measure preserves the result (-0.253, $t = -3.7$; Appendix Table D.5), so the gating is not an artifact of the particular survey question used to form the proxy. Splitting NEWS1 reporters by the topic of news cited, the gating is sharpest for labor and real-activity news (-0.59, $t = -4.0$) and weaker for government/policy news (-0.23, $t = -1.9$), reinforcing the content-validation evidence in Appendix D.2.

Appendix Table D.5: Cross-Domain Robustness: BAGO-Based Measure and Topic-Specific Gating

Accuracy measure / subsample	$\hat{\beta}(MPS_t \times \text{Acc.})$	t	N
NEWS1-based (baseline)	-0.377^{***}	-4.54	22,826
BAGO-based (alternative)	-0.253^{***}	-3.72	33,457
NEWS1 topic restriction:			
Labor / real-activity news	-0.592^{***}	-3.96	6,043
Prices / rates / credit news	-0.289	-1.62	4,932
Government / policy news	-0.226^{*}	-1.88	9,528
General sentiment news	-0.494^{*}	-1.66	4,540

Notes: BAGO-based accuracy benchmarks the "business conditions vs. a year ago" response against the twelve-month unemployment change. Gating on the shadow-rate–normalized cumulative shock (MPS scaled so one unit = a 1 pp shadow-rate change, as in the baseline specification of Table 3); robust standard errors. $^{*}p < 0.10$, $^{***}p < 0.01$.

D.7. Cyclicality of the Three Attentiveness Groups

Section 3.4 notes that the aggregate *Accurate* share is strongly countercyclical. Appendix Table D.6 decomposes this across the three groups. The *Accurate* share rises sharply in recessions (from 0.34 in expansions to 0.65) and with the magnitude of the unemployment change, while *both* the *Inaccurate* and *Haven't heard* shares fall. Measured against the signal-strength metric $|\Delta U_t|$, the *Haven't heard* share is the most countercyclical (correlation -0.42), consistent with the extensive-margin reading: in quiet, low-stakes periods households disengage from macro news, and the recession increase in the *Accurate* share reflects movement out of *both* non-attentive states as the aggregate signal becomes salient.

D.8. Additional Diagnostics for the Non-Attentive Groups

The transition matrix in Appendix Table D.7 serves a dual purpose. First, the moderate persistence of the *Accurate* classification (45.6% remain *Accurate* six months later) is consistent with the model's prediction that attention is endogenous to the macroeconomic state U_t. As conditions change

Appendix Table D.6: Cyclicality of the Three Attentiveness-Group Shares

Monthly share	Accurate	Inaccurate	Haven't heard
Full sample	0.375	0.292	0.332
NBER recession	0.646	0.104	0.250
NBER expansion	0.344	0.314	0.342
High VIX	0.409	0.270	0.322
Low VIX	0.353	0.307	0.340
Correlation of monthly share with:			
NBER recession	0.634	−0.533	−0.323
$\|\Delta U_t\|$ (3-month)	0.460	−0.253	−0.420

Notes: Unweighted monthly means of the three-way classification over 1998m09–2020m03 (257 months). High VIX denotes HP-filtered log VIX above trend. $|\Delta U_t|$ is the three-month unemployment change.

Appendix Table D.7: Diagnostics for the Non-Attentive Groups

Panel A: Persistence of Accuracy Classifications			
	Accurate at t+6	Inaccurate at t+6	Haven't heard at t+6
Accurate at t	45.6%	32.0%	22.4%
Inaccurate at t	38.9%	36.0%	25.1%
Haven't heard at t	26.1%	19.6%	54.3%
Panel B: Selected logit coefficients: Haven't heard vs. Inaccurate			
Variable	Coefficient	Odds ratio	t-statistic
Age	-0.068	0.934	-12.85
Log income	-0.367	0.692	-9.06
Non-stockholder	0.277	1.320	8.18
College degree (EDUC 5)	-0.719	0.487	-4.94
Post-college (EDUC 6)	-0.969	0.379	-6.60

Notes: Panel A reports six-month transition shares in the rotating MSC panel, where the second-interview category is reconstructed using the same benchmark rule as in the baseline measure and the corresponding unemployment change. Panel B reports selected coefficients from a binary logit that compares *Haven't heard* to *Inaccurate* respondents. Positive coefficients indicate a higher likelihood of being classified as *Haven't heard*.

between interviews—recessions give way to expansions, or volatility subsides—households optimally adjust their information acquisition, so transitions between groups are expected even absent measurement noise. If the proxy instead captured a fixed trait such as cognitive ability, persistence would be much higher. The observed transition rates thus support the endogenous-attention interpretation.

We test this interpretation directly using linear probability models that regress a household's accuracy classification at the second interview (t+6) on the change in financial conditions between interviews, interacted with the household's initial classification at t. Appendix Table D.8 reports the results for three proxies: the change in log VIX, the change in the Chicago Fed National Financial Conditions Index (NFCI), and the change in the St. Louis Fed Financial Stress Index (STLFSI2), all measured as six-month level changes between the two interview dates. In both the two-way specification (Accurate vs. Not Accurate, Panel A) and the three-way specification (Panel B), worsening financial conditions—rising VIX, tightening NFCI, or rising financial stress—significantly increase the probability that a household is classified as *Accurate* at t+6. The pattern varies by proxy: for VIX, rising volatility selectively reinforces the initially Accurate ($p < 0.001$), consistent with financial-market attention having the strongest retention effect among the already-attentive. For NFCI, the effect is uniformly broad-based ($p = 0.97$), indicating that tightening credit conditions push households toward attention regardless of their initial classification. For STLFSI2, the differential is moderate ($p = 0.12$). In the three-way specification (Panel B), the *Haven't heard* group consistently shows strong transitions toward accuracy across all three proxies, confirming that deteriorating conditions draw even the initially disengaged into active information acquisition. These results confirm that individual-level transitions between accuracy groups reflect systematic responses to changing macroeconomic conditions, as the model predicts.

Second, the transition matrix reveals that the three groups are not interchangeable. The *Haven't heard* group is not simply a noisy relabeling of *Inaccurate*. About 54.3% of households initially classified as *Haven't heard* remain there six months later, whereas only 36.0% of initially *Inaccurate* households remain *Inaccurate*. Transition rates into *Accurate* are also lower for *Haven't heard* (26.1%) than for *Inaccurate* (38.9%), consistent with *Haven't heard* reflecting a more persistent low-engagement state.

Third, a binary logit that compares *Haven't heard* directly to *Inaccurate* shows that the former group is systematically younger, lower-income, less-educated, and more likely to be non-stockholders. The education gradient is particularly strong: relative to respondents without a high-school degree, college-educated households are much less likely to fall into the *Haven't heard* category. Together with the descriptive evidence in Section 3, these diagnostics support interpreting *Haven't heard* as a distinct low-attention group rather than as a small perturbation around explicit misperception.

A complementary two-group robustness check reaches the same substantive conclusion from the opposite direction. Pooling *Inaccurate* and *Haven't heard* into a single *Not Accurate* category yields a sharper version of the main gating result: in the baseline micro specification, the *Accurate* coefficient remains strongly negative (-0.35, $t = -4.39$), while the *Not Accurate* coefficient is small and statistically indistinguishable from zero (-0.06, $t = -0.85$). The same pattern survives in the state-dependent analysis: during NBER recessions, the attentive-group coefficient is about -0.91 ($t = -6.07$) versus -0.65 ($t = -2.19$) for *Not Accurate*, and in high-VIX states it is about -0.49 ($t = -5.12$) versus -0.14 ($t = -1.43$). These exercises show that the core attention-gating result does not rely on a knife-edge separation between *Inaccurate* and *Haven't heard*; if anything, pooling the two non-attentive groups makes the main prediction cleaner, as reported in Appendix Table D.11.

Appendix Table D.9 validates the attention proxy against VIX, EPU, EMV, and NBER recessions.

Appendix Table D.8: Individual Attention Transitions and Macroeconomic Conditions

Panel A: Two-way LPM — Pr(Accurate at t+6)			
	Δ log VIX	Δ NFCI	Δ STLFSI2
$\Delta X \times$ Accurate$_t$	0.024***	0.018***	0.018***
	(5.79)	(5.01)	(4.84)
$\Delta X \times$ Not Accurate$_t$	0.005	0.018***	0.010**
	(1.57)	(4.38)	(2.51)
Accurate$_t$	0.117***	0.118***	0.118***
	(21.65)	(21.91)	(21.84)
H_0: $\beta_{\text{Acc}} = \beta_{\text{NotAcc}}$ (F)	13.61	0.00	2.37
p-value	0.000	0.972	0.124

Panel B: Three-way LPM — Pr(Accurate at t+6)			
	Δ log VIX	Δ NFCI	Δ STLFSI2
$\Delta X \times$ Accurate$_t$	0.024***	0.018***	0.018***
	(5.80)	(5.03)	(4.86)
$\Delta X \times$ Inaccurate$_t$	−0.006	0.014**	−0.006
	(−1.32)	(2.06)	(−1.02)
$\Delta X \times$ Haven't heard$_t$	0.018***	0.024***	0.022***
	(4.17)	(4.71)	(4.36)
Accurate$_t$	0.167***	0.169***	0.168***
	(27.25)	(27.58)	(27.47)
Inaccurate$_t$	0.100***	0.102***	0.101***
	(15.71)	(15.91)	(15.82)
N	35,604	35,604	35,604

Notes: Linear probability models estimated on the MSC rotating panel (N = 35,604 re-interviewed households). The dependent variable is $\mathbf{1}\{\text{Accurate at } t+6\}$, where accuracy at the second interview is reconstructed using the same benchmark rule as in the baseline measure. All ΔX proxies are standardized six-month changes in levels between interview dates: $\Delta \log \text{VIX} = \log \text{VIX}_{t+6} - \log \text{VIX}_t$; $\Delta\text{NFCI} = \text{NFCI}_{t+6} - \text{NFCI}_t$ (Chicago Fed National Financial Conditions Index; positive = tightening); $\Delta\text{STLFSI2} = \text{STLFSI2}_{t+6} - \text{STLFSI2}_t$ (St. Louis Fed Financial Stress Index; positive = more stress). Panel A interacts each proxy with Accurate$_t$ and Not Accurate$_t$ (Inaccurate + Haven't heard). Panel B interacts with all three initial groups separately (Haven't heard omitted as reference for intercepts). Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A natural question is whether the proxy also tracks independently constructed salience measures from outside the Michigan Survey. Appendix Table D.10 reports correlations between the monthly aggregate attention share and a broader set of external proxies over the baseline regression sample. The strongest comovement is with the St. Louis Fed Financial Stress Index (STLFSI2, ρ = 0.57), which aggregates a wide array of financial-market stress indicators and is entirely external to the household survey. Google Trends search volume for "interest rates" (ρ = 0.35) and "Federal Reserve" (ρ = 0.22) also correlate positively with the attention share, as do newspaper-based EMV sub-indices tracking monetary-policy and inflation coverage. These patterns confirm that the accuracy-based

Appendix Table D.9: Identification, Proxy Validation, and Aggregate Attention Checks

Specification	Coefficient	t-statistic	Interpretation
Panel A: Timing and permutation placebos			
Future placebo, Accurate	0.083	0.86	Six-month forward shift of the normalized policy shock
Past placebo, Accurate	0.138	1.52	Six-month backward shift of the normalized policy shock
Permutation mean $\lvert\beta_{Acc}\rvert$	0.142	–	Monthly-shock randomization benchmark
Permutation p-value	0.047	–	Share of draws with $\lvert\beta_{Acc}^{perm}\rvert \geq \lvert\beta_{Acc}^{actual}\rvert$
Panel B: Validation of the attention proxy			
Attention on $z\log$(VIX)	0.088	2.26	One-standard-deviation increase in financial volatility
Attention on $z\log$(EPU)	0.090	3.52	One-standard-deviation increase in policy uncertainty
Attention on $z\log$(EMV)	0.070	2.05	One-standard-deviation increase in macro volatility news
Attention on recession indicator	0.525	7.01	NBER recession dummy
Panel C: Aggregate attention robustness			
Top-30% high-attention slope	-0.504	-2.41	Baseline regime split
Top-30% low-attention slope	-0.015	-0.15	Baseline regime split
Top-25% high-attention slope	-0.645	-1.89	Stricter high-attention cutoff
Top-25% low-attention slope	-0.040	-0.40	Stricter high-attention cutoff

Notes: Panel A summarizes the placebo timing and permutation exercises from the baseline micro regression. Panel B reports HAC regressions of the monthly aggregate attention share on standardized public-salience proxies and the NBER recession indicator. Panel C reports alternative regime cutoffs for the aggregate attention specification over the Great Moderation sample.

proxy captures genuine variation in public engagement with macroeconomic and policy news, not merely survey-specific noise.

Panel B of Appendix Table D.10 provides a more demanding test: using the STLFSI2 median to split months into high- and low-stress regimes and re-estimating the aggregate pass-through regression from Table 4. The high-stress coefficient is -0.42 ($t = -2.25$), while the low-stress coefficient is near zero (0.10, $t = 0.66$). This pattern closely mirrors the attention-based regime split in the main text and establishes that the state-dependent expectations multiplier is not an artifact of our particular proxy construction: an entirely external financial-conditions indicator generates the same asymmetry.

D.9. Coefficient Stability: Oster (2019) Bounds

To assess the threat of omitted-variable bias, we apply the Oster (2019) bounding procedure to the baseline micro specification. The method compares the coefficient movement from a "short" regression (only the accuracy interactions and the monetary policy shock, $\bar{R}^2_{\text{short}} = 0.0004$) to the

Appendix Table D.10: External Validation of the Attention Proxy

Proxy	Correlation	t-stat	N	Source
Panel A: Correlation with aggregate attention share				
St. Louis Financial Stress (STLFSI2)	0.572	8.17	259	FRED
$\lvert\Delta$Fed Funds$\rvert$	0.353	—	259	FRED
Google Trends: "interest rates"	0.350	2.35	195	Google Trends
Google Trends: "Federal Reserve"	0.219	1.21	195	Google Trends
Google Trends: "inflation"	0.230	—	195	Google Trends
EMV: Monetary Policy	0.233	2.73	259	BBD
EMV: Inflation	0.257	—	259	BBD
10Y–2Y Spread	0.258	—	259	FRED
Panel B: Aggregate pass-through by STLFSI2 regime				
High-stress $\times RRshock_t$	-0.420^{**}	−2.25	164	Median-split regime
Low-stress $\times RRshock_t$	0.103	0.66	164	Median-split regime

Notes: Panel A reports Pearson correlations between the monthly aggregate attention share $\mathbf{A}_t^{\text{agg}}$ and external salience proxies over the baseline regression sample (1998m9–2020m3; Google Trends available from 2004m1). The t-statistics in Panel A are from univariate HAC(6) regressions of $\mathbf{A}_t^{\text{agg}}$ on the standardized proxy; where only the correlation is reported (—), the HAC regression was not separately estimated. STLFSI2 is the St. Louis Fed Financial Stress Index. $\lvert\Delta$Fed Funds$\rvert$ is the absolute monthly change in the effective federal funds rate. Google Trends series are the monthly search volume index for the indicated term (U.S.). EMV sub-indices are from the Equity Market Volatility tracker of Baker, Bloom, Davis, and Kost (available at policyuncertainty.com) and track newspaper articles mentioning monetary policy or inflation alongside equity-market volatility. Panel B splits Great Moderation months (1985m1–2007m12) at the median of STLFSI2 and estimates the aggregate pass-through regression with Newey-West standard errors (6 lags). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.11: Two-Group Robustness: Accurate vs. Not Accurate

Specification	Accurate	t-stat	Not Accurate	t-stat
Baseline micro pass-through	-0.346	-4.39	-0.061	-0.85
NBER recession state	-0.908	-6.07	-0.653	-2.19
NBER normal state	-0.076	-0.82	-0.053	-0.72
High VIX state	-0.485	-5.12	-0.137	-1.43
Low VIX state	-0.043	-0.33	-0.032	-0.27

Notes: This table pools *Inaccurate* and *Haven't heard* into a single *Not Accurate* category and re-estimates the baseline and state-dependent specifications on the original pre-2020 normalized micro sample. The table reports the coefficients on the monetary policy shock interactions.

"full" regression with demographic controls ($\bar{R}^2_{\text{full}} = 0.014$) and asks: how much additional selection on unobservables, relative to observables, would be needed to drive the coefficient to zero?

Appendix Table D.13 reports the results. For *mps_accurate*, the Oster δ is *negative* ($\hat{\delta} = -0.21$ under $R_{\max} = 1.3\bar{R}^2$), meaning the coefficient *strengthens* as controls are added. Unobservables would need to have the *opposite* selection pattern from observables to explain away the result—an implausible scenario. The identified set under $R_{\max} = 1.3\bar{R}^2$ is $[-0.53, -0.36]$, which comfortably excludes zero.

Appendix Table D.12: Five-Year Inflation Expectations

Variable	Coefficient	t-statistic	Observations
Accurate $\times MPS_t$	-0.036	-0.74	37,404
Inaccurate $\times MPS_t$	0.079	1.09	37,404
Haven't heard $\times MPS_t$	-0.031	-0.48	37,404

Notes: This table applies the baseline micro specification in Equation (4.1) to the revision in five-year-ahead inflation expectations over the same six-month panel window. The coefficients are economically small and statistically indistinguishable from zero for all three groups, suggesting that the attention channel identified in the main text primarily operates through near-term beliefs rather than long-run inflation anchors.

Appendix Table D.13: Oster (2019) Coefficient Stability Bounds

Variable	$\hat{\beta}_{\text{short}}$	$\hat{\beta}_{\text{full}}$	$\hat{\delta}$	$R_{\max}$	Identified set	Excludes 0?
Panel A: $R_{\max} = 1.3\tilde{R}^2$						
$MPS \times$ Accurate	0.182	−0.360	−0.205	0.018	[−0.528, −0.360]	Yes
$MPS \times$ Inaccurate	0.134	0.088	0.601	0.018	[0.074, 0.088]	Yes
$MPS \times$ Haven't Heard	0.098	−0.156	−0.189	0.018	[−0.234, −0.156]	Yes
Panel B: $R_{\max} = 2.2\tilde{R}^2$						
$MPS \times$ Accurate	0.182	−0.360	−0.821	0.030	[−1.030, −0.360]	Yes
$MPS \times$ Inaccurate	0.134	0.088	2.406	0.030	[0.032, 0.088]	Yes
$MPS \times$ Haven't Heard	0.098	−0.156	−0.758	0.030	[−0.469, −0.156]	Yes

Notes: This table applies the Oster (2019) procedure to the three accuracy-group interaction coefficients. "Short" is the regression with only the three accuracy × MPS interactions (no demographic controls); "Full" is the baseline specification with the full set of controls. $\hat{\delta}$ is the proportional selection ratio: values $|\delta| > 1$ indicate that unobservables would need to be more important than observables to explain the result. Negative δ means the coefficient strengthens with controls, implying unobservables would need opposite-signed selection. $R_{\max}$ is the hypothetical maximum R^2 if all relevant unobservables were included. $N = 37{,}445$ in all specifications. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

D.10. Richer Controls from the Augmented Sample

The MSC contains additional survey items beyond the standard demographic and financial controls used in the baseline specification. We systematically add blocks of these richer controls to assess whether the accuracy-based gating result is driven by omitted attitudes or expectations. Appendix Table D.14 reports the coefficient on *mps_accurate* as each block is added to the baseline.

D.11. Measurement Error and Attenuation Bias

Because accuracy is measured with error—some genuinely attentive households may be misclassified and vice versa—the OLS coefficient on *Accurate* understates the true attention-gating effect. As discussed in Section 5, two independent mechanisms drive this attenuation: static misclassification at the time of measurement, and endogenous attention dynamics over the six-month regression window. We quantify the combined effect in two ways.

First, we exploit the panel structure of the MSC. Among the roughly 250 respondents re-interviewed six months later, 45.7% receive the same three-way accuracy classification in both

Appendix Table D.14: Robustness to Richer Survey Controls

Specification	$\hat{\beta}_{\text{Accurate}}$	SE	t-stat	N	Extra controls
Baseline	-0.360^{***}	0.079	−4.56	37,445	—
+ Sentiment indices	-0.389^{***}	0.079	−4.91	37,445	ICS
+ Macro perceptions	-0.302^{***}	0.080	−3.78	37,032	RATEX, UNEMP
+ Durable/housing	-0.387^{***}	0.082	−4.74	34,849	DUR, CAR, HOM
+ Gas expectations	-0.535^{***}	0.116	−4.63	25,856	GAS1
+ Income expectations	-0.332^{***}	0.080	−4.14	36,109	RINC, PEXP, BEXP
Kitchen sink (all blocks)	-0.620^{***}	0.122	−5.10	23,232	All of the above

Notes: Each row adds one block of additional survey controls to the baseline micro specification and reports the coefficient on $MPS_t \times$ Accurate. "Sentiment indices" is the aggregate Index of Consumer Sentiment (ICS). "Macro perceptions" are interest rate expectations (RATEX) and unemployment expectations (UNEMP). All MSC attitudinal items are entered after dropping "Don't Know"/"No Answer" codes, so blocks containing them have slightly smaller N. "Durable/housing" are buying conditions for durables (DUR), vehicles (CAR), and homes (HOM). "Gas expectations" is the expected change in gas prices (GAS1; available for a subset of months, reducing N). "Income expectations" include expected change in real income (RINC), personal financial expectations (PEXP), and business conditions expectations (BEXP). "Kitchen sink" includes all blocks simultaneously. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.15: Sensitivity to Alternative Accuracy Thresholds

Threshold	$\hat{\beta}_{\text{Accurate}}$	$\hat{\beta}_{\text{Inaccurate}}$	$\hat{\beta}_{\text{Haven't Heard}}$	N_{Acc}	N_{Inacc}	N_{HH}
−0.3 pp	-0.327^{***}	0.128	-0.156^{*}	15,301	9,742	12,402
	(−4.32)	(1.12)	(−1.67)			
−0.1 pp	-0.333^{***}	0.102	-0.156^{*}	14,448	10,595	12,402
	(−4.24)	(0.93)	(−1.67)			
0.0 pp (baseline)	-0.352^{***}	0.062	-0.156^{*}	13,315	11,728	12,402
	(−4.23)	(0.60)	(−1.66)			
+0.1 pp	-0.290^{***}	−0.068	-0.156^{*}	12,722	12,321	12,402
	(−3.32)	(−0.67)	(−1.66)			
+0.3 pp	-0.320^{***}	−0.109	-0.156^{*}	11,371	13,672	12,402
	(−2.75)	(−1.33)	(−1.67)			

Notes: Each row re-estimates the baseline micro specification after redefining the accuracy threshold. "Threshold" is the minimum three-month change in the unemployment rate (percentage points) required for a correctly-directioned response to be classified as *Accurate*. A negative threshold is more lenient (more respondents classified as Accurate); a positive threshold is more restrictive. The baseline threshold is zero. $N = 37{,}445$ in all rows. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

waves. If all cross-wave disagreement is attributed to noise, the implied reliability ratio is low, suggesting a substantial attenuation factor. However, as the transition matrix in Appendix Table D.7 shows, some of this instability reflects genuine attention adjustment to changing macroeconomic conditions rather than pure measurement error. The noise-only attenuation factor is therefore an upper bound; the true noise share is smaller, but the endogenous-dynamics component provides a separate source of dilution that also pushes OLS below the instantaneous gating effect. On net, with a cross-wave reliability ratio of about 0.27, the noise-only correction implies an attenuation factor of $1/0.27 \approx 3.6$, so the corrected effect of attention on monetary-policy pass-through is likely on the order of 3.6 times the OLS point estimate, though the precise magnitude depends on the decomposition between the two sources.

Second, we verify robustness to alternative accuracy thresholds. The baseline classification assigns *Accurate* when the respondent's reported unemployment direction matches the actual three-month change (threshold = 0). Appendix Table D.15 reports the coefficient on *mps_accurate* under thresholds ranging from -0.3 to $+0.3$ percentage points. The coefficient is negative and significant across all thresholds, though the point estimate is somewhat smaller at extreme thresholds where the Accurate/Inaccurate split becomes more uneven.

D.12. Full Reports: State Dependent Analysis and Demographic Heterogeneity

This section provides the complete regression output for the state-dependence analysis presented in Section 4.3 (Appendix Tables D.16 and D.17). The tables report the full set of coefficients, including those for the "Haven't Heard" group and contemporaneous macro controls, for specifications using NBER recessions, the LMN real uncertainty index , and the VIX to define high- and low-uncertainty states.

We also present the full regression tables corresponding to the demographic heterogeneity analysis in Section 4.4 (Appendix Tables D.18–D.20). Each table details the complete set of interaction coefficients for the partitions based on stockholding, homeownership, age group, and income quartile, including results for all three accuracy groups and macro control variables.

Appendix Table D.16: Attention with NBER Business Cycle Indicator

NBER Recession	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: NBER Recession			
(1) $Recession \times MPS_t$	-1.701***	-1.125	-0.989
	(-4.01)	(-1.00)	(-1.29)
(2) $Recession \times \Delta \text{IP}_t$	0.200***	0.084	0.121*
	(5.19)	(0.87)	(1.68)
(3) $Recession \times \Delta \pi_t$	0.304***	0.259	0.244
	(3.29)	(1.32)	(1.52)
Panel B: Normal			
(4) $Normal \times MPS_t$	-0.040	0.116	-0.123
	(-0.49)	(1.12)	(-1.43)
(5) $Normal \times \Delta \text{IP}_t$	-0.028	-0.018	-0.022
	(-1.41)	(-1.00)	(-1.09)
(6) $Normal \times \Delta \pi_t$	0.332***	0.269***	0.276***
	(8.17)	(6.17)	(5.86)
Interaction		NBER	
Controls		Yes	
Observations		37,445	
R^2		0.0170	

Notes: This table reports the state-dependent regression in Equation (4.3) using the NBER recession indicator as State_{t-1}. The dependent variable is the revision in one-year-ahead inflation expectations between interviews. MPS_t denotes the normalized cumulative high-frequency monetary policy shocks from month t to $t+5$. $\mathbf{A}_{i,t}$ is the three-way accuracy vector (Accurate / Inaccurate / Haven't heard) measured at the first interview. We include contemporaneous Industrial Production growth and inflation changes between interviews; all lower-order terms and the full set of demographics and survey controls are included. Robust standard errors are reported; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.17: Attention with Real Uncertainty Indicator

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: High Uncertainty (LMN Real Uncertainty)						
$High \times MPS_t$	-0.539***	0.049	-0.138			
	(-5.51)	(0.35)	(-1.18)			
$High \times \Delta \text{IP}_t$	0.130***	-0.003	0.022			
	(4.37)	(-0.09)	(0.66)			
$High \times \Delta \pi_t$	0.305***	0.137*	0.325***			
	(5.29)	(1.95)	(4.38)			
Panel B: Low Uncertainty (LMN Real Uncertainty)						
$Low \times MPS_t$	-0.270*	0.250	-0.248			
	(-1.77)	(1.33)	(-1.49)			
$Low \times \Delta \text{IP}_t$	0.034	-0.012	0.009			
	(1.62)	(-0.63)	(0.37)			
$Low \times \Delta \pi_t$	0.381***	0.397***	0.310***			
	(7.54)	(7.73)	(5.76)			
Panel C: High Volatility (VIX)						
$High \times MPS_t$				-0.456***	0.041	-0.098
				(-4.06)	(0.22)	(-0.70)
$High \times \Delta \text{IP}_t$				0.079***	0.074**	-0.000
				(2.73)	(1.96)	(-0.01)
$High \times \Delta \pi_t$				-0.011	0.142*	0.138*
				(-0.17)	(1.92)	(1.83)
Panel D: Low Volatility (VIX)						
$Low \times MPS_t$				-0.008	0.101	-0.180
				(-0.07)	(0.79)	(-1.45)
$Low \times \Delta \text{IP}_t$				0.021	-0.028	0.008
				(0.97)	(-1.40)	(0.35)
$Low \times \Delta \pi_t$				0.539***	0.338***	0.414***
				(11.82)	(6.51)	(7.37)
Interaction		LMN real uncertainty			VIX	
Controls		Yes			Yes	
Observations		37,445			37,445	
R^2		0.0146			0.0182	

Notes: This table reports the state-dependent regression in Equation (4.3). Panels A and B use the Ludvigson et al. (2021) real uncertainty index (LMN) and Panels C and D use financial-market volatility (VIX) to define State_{t-1} ("High" when the HP-detrended index is above trend at $t-1$). The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative high-frequency monetary policy shocks from t to $t+5$. Accuracy is measured at the first interview; We include contemporaneous Industrial Production growth and inflation changes between interviews. All lower-order interactions, demographics, and survey controls are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.18: Full Reports for Stockholding and Homeownership

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard	(4) Accurate	(5) Inaccurate	(6) Haven't Heard
Panel A: Stockholding						
$Stock \times MPS_t$	-0.410***	0.150	-0.299**			
	(-4.57)	(1.20)	(-2.38)			
$NonStock \times MPS_t$	-0.228	-0.047	0.034			
	(-1.42)	(-0.21)	(0.24)			
$Stock \times \Delta \text{IP}_t$	0.053***	0.001	0.020			
	(2.84)	(0.09)	(0.91)			
$NonStock \times \Delta \text{IP}_t$	0.090***	-0.049	0.004			
	(2.35)	(-1.22)	(0.11)			
$Stock \times \Delta \pi_t$	0.394***	0.258***	0.377***			
	(9.67)	(5.92)	(6.89)			
$NonStock \times \Delta \pi_t$	0.292***	0.318***	0.241***			
	(3.18)	(2.86)	(3.06)			
Panel B: Homeownership						
$Homeowner \times MPS_t$				-0.436***	0.063	-0.148
				(-5.08)	(0.54)	(-1.40)
$Renter \times MPS_t$				0.026	0.214	-0.181
				(0.13)	(0.76)	(-0.91)
$Homeowner \times \Delta \text{IP}_t$				0.057***	-0.0000	0.027
				(3.11)	(-0.00)	(1.22)
$Renter \times \Delta \text{IP}_t$				0.075*	-0.032	-0.024
				(1.88)	(-0.81)	(-0.67)
$Homeowner \times \Delta \pi_t$				0.386***	0.286***	0.334***
				(9.61)	(6.43)	(6.61)
$Renter \times \Delta \pi_t$				0.297***	0.166	0.276***
				(2.67)	(1.30)	(2.73)
Interaction		Stockholding			Homeownership	
Controls		Yes			Yes	
Observations		37,445			37,445	
R^2		0.0142			0.0144	

Notes: This table reports the full set of coefficients for the stockholding specification (Panel A) and the homeownership specification (Panel B) of Equation (4.4). The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t denotes the normalized cumulative high-frequency monetary policy shocks from t to $t+5$. We interact MPS_t with the three-way accuracy vector (measured at the first interview) and homeownership status (Homeowner / Renter). We include contemporaneous Industrial Production growth and inflation changes between interviews; We include age and age^2, income level and quartiles, education, gender, homeownership, stockholding, marital status, region, and an indicator for reporting unfavorable news as controls; all lower-order terms are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.19: Full Reports for Age Group

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
(1) $[18-34] \times MPS_t$	-0.613***	0.260	-0.006
	(-3.22)	(0.82)	(-0.03)
(2) $[35-64] \times MPS_t$	-0.350***	0.140	-0.145
	(-3.68)	(1.12)	(-1.16)
(3) $[65+] \times MPS_t$	-0.234	-0.264	-0.338*
	(-1.22)	(-0.98)	(-1.66)
(4) $[18-34] \times \Delta \text{IP}_t$	0.110**	0.031	0.004
	(2.43)	(0.72)	(0.10)
(5) $[35-64] \times \Delta \text{IP}_t$	0.059***	-0.019	0.015
	(2.95)	(-0.84)	(0.57)
(6) $[65+] \times \Delta \text{IP}_t$	0.038	0.004	0.022
	(1.01)	(0.13)	(0.60)
(7) $[18-34] \times \Delta \pi_t$	0.381***	0.033	0.206**
	(3.68)	(0.27)	(2.07)
(8) $[35-64] \times \Delta \pi_t$	0.435***	0.328***	0.373***
	(9.41)	(6.17)	(5.94)
(9) $[65+] \times \Delta \pi_t$	0.179**	0.243***	0.311***
	(2.20)	(2.92)	(3.66)
Interaction		Age Group	
Controls		Yes	
Observations		37,445	
R^2		0.0150	

Notes: This table reports the full set of coefficients for the age-group specification of Equation (4.4). The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative high-frequency monetary policy shocks from t to $t+5$. We interact MPS_t with the three-way accuracy vector and age groups (Young 18-34, Middle 35-64, Old 65+). We include contemporaneous Industrial Production growth and inflation changes between interviews; We include age and age^2, income level and quartiles, education, gender, homeownership, stockholding, marital status, region, and an indicator for reporting unfavorable news as controls; all lower-order terms are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.20: Full Reports for Income Quartile

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
(1) $YTL1 \times MPS_t$	0.048	-0.192	0.149
	(0.18)	(-0.58)	(0.71)
(2) $YTL2 \times MPS_t$	-0.669***	0.046	-0.212
	(-3.90)	(0.19)	(-1.12)
(3) $YTL3 \times MPS_t$	-0.361***	0.201	-0.119
	(-2.58)	(1.04)	(-0.77)
(4) $YTL4 \times MPS_t$	-0.298**	0.132	-0.416**
	(-2.47)	(0.77)	(-2.07)
(5) $YTL1 \times \Delta\mathrm{IP}_t$	0.024	-0.043	-0.059
	(0.48)	(-0.93)	(-1.37)
(6) $YTL2 \times \Delta\mathrm{IP}_t$	0.142***	-0.062	0.017
	(3.43)	(-1.49)	(0.49)
(7) $YTL3 \times \Delta\mathrm{IP}_t$	0.024	-0.018	0.020
	(0.82)	(-0.59)	(0.56)
(8) $YTL4 \times \Delta\mathrm{IP}_t$	0.054**	0.047*	0.069*
	(2.26)	(1.80)	(1.93)
(9) $YTL1 \times \Delta\pi_t$	0.427***	0.220	0.282***
	(3.30)	(1.47)	(2.68)
(10) $YTL2 \times \Delta\pi_t$	0.265***	0.313***	0.271***
	(3.16)	(3.12)	(3.12)
(11) $YTL3 \times \Delta\pi_t$	0.438***	0.307***	0.414***
	(6.51)	(4.35)	(5.37)
(12) $YTL4 \times \Delta\pi_t$	0.352***	0.238***	0.329***
	(6.16)	(3.88)	(3.51)
Interaction		Income Quartile	
Controls		Yes	
Observations		37,445	
R^2		0.0153	

Notes: This table reports the full set of coefficients for the income-quartile specification of Equation (4.4). The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t denotes the normalized cumulative high-frequency monetary policy shocks from t to $t+5$. We interact MPS_t with the three-way accuracy vector and income quartiles (YTL1–YTL4). We include contemporaneous Industrial Production growth and inflation changes between interviews; We include age and age^2, income level and quartiles, education, gender, homeownership, stockholding, marital status, region, and an indicator for reporting unfavorable news as controls; all lower-order terms are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.13. Alternative Measures

This section tests the robustness of our baseline Accuracy proxy to its precise construction. First, we re-estimate our baseline using 1-month and 6-month differencing horizons for the unemployment rate to show the results are not sensitive to the 3-month window. Second, we replace the unemployment benchmark entirely with three alternative indicators—industrial production, national financial index, and business conditions indicator—to demonstrate that our results are robust to the choice of macroeconomic data series.

D.13.1. Alternative Differencing Horizons. We reconstruct the attention measure using the one-month and six-month changes in unemployment rate, instead of the three-month change in unemployment rate.

Appendix Table D.21: Alternative Differencing Horizons for Unemployment

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard	(4) Accurate	(5) Inaccurate	(6) Haven't Heard
Panel A: 1 month ($\Delta\text{Unrate}_t \equiv \text{Unrate}_t - \text{Unrate}_{t-1}$)						
(1) MPS_t	-0.360***	0.094	-0.156*			
	(-4.48)	(0.90)	(-1.67)			
(2) ΔIP_t	0.049***	0.001	0.013			
	(3.17)	(0.07)	(0.71)			
(3)$\Delta\pi_t$	0.376***	0.282***	0.325***			
	(9.95)	(6.69)	(7.21)			
Panel B: 6 months ($\Delta\text{Unrate}_t \equiv \text{Unrate}_t - \text{Unrate}_{t-6}$)						
(1) MPS_t				-0.336***	0.090	-0.155***
				(-4.05)	(0.86)	(-1.66)
(2) ΔIP_t				0.068***	-0.026	0.013
				(4.36)	(-1.42)	(0.70)
(3)$\Delta\pi_t$				0.338***	0.328***	0.325***
				(8.87)	(7.79)	(7.21)
Controls		Yes			Yes	
Observations		37,445			37,445	
R^2		0.0136			0.0139	

Notes: This table replaces the differencing horizons in unemployment rate that is used to define *Accuracy* measure with 1 month (Panel A) and 6 months (Panel B) and re-estimates the baseline micro specification Equation (4.1). The dependent variable is the revision in one-year-ahead inflation expectations. Shocks are cumulated from t to $t+5$ to align with the six-month survey horizon. Accuracy is measured at the first interview. We include contemporaneous IP growth and inflation changes between interviews; demographics and survey controls are included. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.22: Attention Gating with Alternative Accuracy Benchmarks

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: Industrial Production (IP)			
(1) MPS_t	-0.334***	-0.044	-0.156*
	(-3.84)	(-0.47)	(-1.67)
(2) ΔIP_t	0.053***	-0.003	0.013
	(3.32)	(-0.16)	(0.71)
(3)$\Delta \pi_t$	0.336***	0.351***	0.325***
	(8.26)	(8.90)	(7.20)
Observations		37,445	
R^2		0.0135	
Panel B: National Financial Conditions Index (NFCI)			
(4) MPS_t	-0.336***	-0.002	-0.155*
	(-3.88)	(-0.03)	(-1.66)
(5) ΔIP_t	0.039**	0.021	0.013
	(2.46)	(1.14)	(0.70)
(6)$\Delta \pi_t$	0.315***	0.375***	0.325***
	(7.98)	(8.95)	(7.21)
Observations		37,445	
R^2		0.0135	
Panel C: ADS Business Conditions Index (Aruoba et al.)			
(7) MPS_t	-0.271***	-0.135	-0.156*
	(-3.29)	(-1.37)	(-1.67)
(8) ΔIP_t	0.028*	0.039**	0.014
	(1.81)	(2.00)	(0.71)
(9)$\Delta \pi_t$	0.434***	0.237***	0.325***
	(10.83)	(5.75)	(7.20)
Observations		37,445	
R^2		0.0136	

Notes: This table re-estimates the baseline specification Equation (4.1) using three alternative accuracy benchmarks. Panel A uses the three-month change in Industrial Production; Panel B uses the three-month change in the NFCI (rising = unfavorable); Panel C uses the change in the monthly average of the daily ADS index between $t-1$ and $t-4$. In each panel, "Accurate" means the respondent's reported business condition news matches the sign of the benchmark change. The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative monetary policy shock from t to $t+5$. All specifications include the full set of demographic and survey controls. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.13.2. Accuracy Measure with Alternative Benchmarks. Tables D.22–D.26 in this section show the robustness of our findings to alternative definitions of the accuracy proxy. We reconstruct the "Accurate" and "Inaccurate" classifications using three alternative benchmarks: (A) the three-month change in Industrial Production (IP), a standard measure of real activity; (B) the three-month change in the National Financial Conditions Index (NFCI), where a rising index signals tighter (unfavorable) financial conditions; and (C) the change in the monthly average of the daily Aruoba et

Appendix Table D.23: Stockholding Heterogeneity with Alternative Accuracy Benchmarks

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: Industrial Production (IP)			
(1) $Stock \times MPS_t$	-0.398***	-0.002	-0.299**
	(-4.02)	(-0.02)	(-2.38)
(2) $NonStock \times MPS_t$	-0.158	-0.154	0.033
	(-0.89)	(-0.80)	(0.23)
Observations		37,445	
R^2		0.0138	
Panel B: National Financial Conditions Index (NFCI)			
(3) $Stock \times MPS_t$	-0.340***	-0.028	-0.299**
	(-3.51)	(-0.26)	(-2.38)
(4) $NonStock \times MPS_t$	-0.336*	0.069	0.033
	(-1.74)	(0.40)	(0.24)
Observations		37,445	
R^2		0.0137	
Panel C: ADS Business Conditions Index (Aruoba et al.)			
(5) $Stock \times MPS_t$	-0.272***	-0.169	-0.299**
	(-2.93)	(-1.46)	(-2.38)
(6) $NonStock \times MPS_t$	-0.282	-0.047	0.033
	(-1.57)	(-0.25)	(0.24)
Observations		37,445	
R^2		0.0142	

Notes: This table re-estimates Equation (4.4) with the Stockholding partition (Stockholder/Non-stockholder) under three alternative accuracy benchmarks. Only the MPS interaction coefficients are reported; interactions with ΔIP_t and $\Delta\pi_t$, group×demographic dummies, and all lower-order terms are included but not shown. The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative monetary policy shock from t to $t+5$. All specifications include the full set of demographic and survey controls. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

al. (2009) Business Conditions Index between months $t-1$ and $t-4$, a composite real-time measure of economic activity. In each case, a respondent is classified as "Accurate" if the sign of their reported business condition news aligns with the sign of the benchmark change, "Inaccurate" if it does not, and "Haven't heard" otherwise. The classification rules parallel our baseline specification.

Appendix Table D.24: Homeownership Heterogeneity with Alternative Accuracy Benchmarks

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: Industrial Production (IP)			
(1) $Homeowner \times MPS_t$	-0.361***	-0.186*	-0.149
	(-3.79)	(-1.83)	(-1.40)
(2) $Renter \times MPS_t$	-0.185	0.660**	-0.182
	(-0.89)	(2.56)	(-0.91)
Observations		37,445	
R^2		0.0142	
Panel B: National Financial Conditions Index (NFCI)			
(3) $Homeowner \times MPS_t$	-0.430***	-0.020	-0.148
	(-4.65)	(-0.20)	(-1.40)
(4) $Renter \times MPS_t$	0.170	0.086	-0.181
	(0.71)	(0.43)	(-0.91)
Observations		37,445	
R^2		0.0141	
Panel C: ADS Business Conditions Index (Aruoba et al.)			
(5) $Homeowner \times MPS_t$	-0.333***	-0.201*	-0.149
	(-3.74)	(-1.84)	(-1.40)
(6) $Renter \times MPS_t$	0.038	0.196	-0.182
	(0.18)	(0.84)	(-0.91)
Observations		37,445	
R^2		0.0141	

Notes: This table re-estimates Equation (4.4) with the Homeownership partition (Homeowner/Renter) under three alternative accuracy benchmarks. Only the MPS interaction coefficients are reported; interactions with ΔIP_t and $\Delta\pi_t$, group×demographic dummies, and all lower-order terms are included but not shown. The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative monetary policy shock from t to $t+5$. All specifications include the full set of demographic and survey controls. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.25: Age Group Heterogeneity with Alternative Accuracy Benchmarks

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: Industrial Production (IP)			
(1) [18-34] × MPS_t	-0.439**	-0.236	-0.007
	(-1.99)	(-0.95)	(-0.04)
(2) [35-64] × MPS_t	-0.327***	0.003	-0.145
	(-3.14)	(0.03)	(-1.16)
(3) [65+] × MPS_t	-0.322	-0.032	-0.338*
	(-1.53)	(-0.14)	(-1.66)
Observations		37,445	
R^2		0.0144	
Panel B: National Financial Conditions Index (NFCI)			
(4) [18-34] × MPS_t	-0.167	-0.379*	-0.006
	(-0.68)	(-1.83)	(-0.04)
(5) [35-64] × MPS_t	-0.363***	0.068	-0.145
	(-3.50)	(0.34)	(-1.16)
(6) [65+] × MPS_t	-0.372*	0.079	-0.338*
	(-1.81)	(0.34)	(-1.66)
Observations		37,445	
R^2		0.0147	
Panel C: ADS Business Conditions Index (Aruoba et al.)			
(7) [18-34] × MPS_t	-0.221	-0.505**	-0.007
	(-0.97)	(-2.17)	(-0.04)
(8) [35-64] × MPS_t	-0.284***	-0.068	-0.145
	(-2.86)	(-0.57)	(-1.17)
(9) [65+] × MPS_t	-0.294	0.004	-0.340*
	(-1.51)	(0.01)	(-1.67)
Observations		37,445	
R^2		0.0145	

Notes: This table re-estimates Equation (4.4) with the Age partition (Young 18-34, Middle 35-64, Old 65+) under three alternative accuracy benchmarks. Only the MPS interaction coefficients are reported; interactions with $\Delta \mathrm{IP}_t$ and $\Delta \pi_t$, group×demographic dummies, and all lower-order terms are included but not shown. The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative monetary policy shock from t to $t+5$. All specifications include the full set of demographic and survey controls. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.26: Income Quartile Heterogeneity with Alternative Accuracy Benchmarks

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
Panel A: Industrial Production (IP)			
(1) $YTL1 \times MPS_t$	0.009	0.018	0.148
	(0.03)	(0.06)	(0.71)
(2) $YTL2 \times MPS_t$	-0.595***	-0.227	-0.213
	(-3.18)	(-1.03)	(-1.13)
(3) $YTL3 \times MPS_t$	-0.248	-0.145	-0.119
	(-1.59)	(-0.88)	(-0.77)
(4) $YTL4 \times MPS_t$	-0.358***	0.131	-0.416**
	(-2.69)	(0.89)	(-2.08)
Observations		37,445	
R^2		0.0148	
Panel B: National Financial Conditions Index (NFCI)			
(5) $YTL1 \times MPS_t$	0.118	-0.155	0.148
	(0.40)	(-0.54)	(0.71)
(6) $YTL2 \times MPS_t$	-0.563***	-0.271	-0.212
	(-2.81)	(-1.40)	(-1.13)
(7) $YTL3 \times MPS_t$	-0.418***	0.240	-0.119
	(-2.75)	(1.44)	(-0.77)
(8) $YTL4 \times MPS_t$	-0.271**	0.024	-0.416**
	(-2.03)	(0.17)	(-2.07)
Observations		37,445	
R^2		0.0151	
Panel C: ADS Business Conditions Index (Aruoba et al.)			
(9) $YTL1 \times MPS_t$	0.078	-0.186	0.148
	(0.27)	(-0.64)	(0.71)
(10) $YTL2 \times MPS_t$	-0.581***	-0.330	-0.212
	(-3.13)	(-1.52)	(-1.13)
(11) $YTL3 \times MPS_t$	-0.376***	0.170	-0.119
	(-2.58)	(0.94)	(-0.77)
(12) $YTL4 \times MPS_t$	-0.112	-0.245	-0.416**
	(-0.88)	(-1.54)	(-2.08)
Observations		37,445	
R^2		0.0152	

Notes: This table re-estimates Equation (4.4) with the Income Quartile partition (YTL1–YTL4) under three alternative accuracy benchmarks. Only the MPS interaction coefficients are reported; interactions with ΔIP_t and $\Delta\pi_t$, group×income dummies, and all lower-order terms are included but not shown. The dependent variable is the revision in one-year-ahead inflation expectations. MPS_t is the normalized cumulative monetary policy shock from t to $t+5$. All specifications include the full set of demographic and survey controls. Robust standard errors; t-statistics in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.13.3. Accuracy measure with "Heard News" proxy.

We re-estimate our baseline using a simpler "Heard News" proxy to evaluate the specific contribution of accuracy to our attention measure.

Appendix Table D.27: Classification Based on News Heard

	(1) Have Heard	(2) Haven't Heard
(1) MPS_t	-0.208***	-0.156*
	(-3.26)	(-1.67)
(2) $\Delta \mathrm{IP}_t$	0.032**	0.013
	(2.68)	(0.71)
(3) $\Delta \pi_t$	0.342***	0.325***
	(11.97)	(7.21)
Controls	Yes	
Observations	37,445	
R^2	0.0131	

Notes: This table shows regression results of Equation (4.1) with replacing *Accuracy* measure with *News Heard* measure. Dependent variable is the revision in one-year-ahead inflation expectations between the first and second MSC interviews (t to t+6). MPS_t is the high-frequency monetary policy surprise cumulated from t to t+5 and normalized so that one unit corresponds to a 1 pp change in the shadow policy rate over that window. $\Delta \mathrm{IP}_t$ is the log change in industrial production and $\Delta \pi_t$ is the change in inflation. Columns report coefficients from interactions with the two attentiveness groups (Have Heard and Haven't Heard) defined at the first interview in month t. All specifications include individual controls (age and age^2, income level and quartiles, education, gender, homeownership, stockholding, marital status, region, and an indicator for reporting unfavorable news). Robust standard errors; t-statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D.14. Dropping Small Unemployment Changes Sample

A potential critique of our baseline attentiveness measure is that it defines accuracy based solely on the *direction* of unemployment changes. This approach treats marginal fluctuations (*e.g.,* a 0.1 percentage point change) the same as substantial economic shifts. If households rationally ignore negligible changes due to optimization frictions, classifying them as "Inaccurate" based on these small movements could introduce measurement error or noise. To address this concern and ensure our results capture genuine attention to salient economic signals, in this section, we conduct a robustness check by restricting our sample. We re-estimate our main specifications after dropping observations where the absolute three-month change in the unemployment rate is less than 0.1 percentage points ($|U_t - U_{t-3}| < 0.1$). This filter removes periods of economic stability where the "correct" answer is ambiguous, focusing the analysis on periods with clearer signals.

Appendix Table D.28: Attention Gating (Dropping Small Unemployment Changes)

	(1) Accurate	(2) Inaccurate	(3) Haven't Heard
(1) MPS_t	-0.364***	0.088	-0.151
	(-4.01)	(0.71)	(-1.46)
(2) ΔIP_t	0.060***	-0.009	0.020
	(3.33)	(-0.51)	(0.96)
(3)$\Delta\pi_t$	0.366***	0.273***	0.307***
	(8.72)	(5.88)	(6.22)
Controls		Yes	
Observations		30,321	
R^2		0.0144	

Notes: Dependent variable is the revision of 1-year ahead expected inflation. MPS_t denotes the normalized cumulative monetary policy shocks between period $t+5$ and t to change the shadow rate by 1 pp between period $t+5$ and $t-1$. ΔIP_t denotes the log difference of Industrial Production between period $t+6$ and t. $\Delta\pi_t$ denotes the inflation rate changes between period $t+6$ and t. Each number of the panel comes from the interaction term. Robust standard errors are used in calculation. We use the individual information about age, income level and quartiles, homeownership, stockownership, gender, education level, region, marital status and an indicator for reporting unfavorable news as controls. t statistics in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendix Table D.29: Aggregate Pass-Through (Dropping Small Unemployment Changes)

Accuracy Regime	(1) High	(2) Low
(1) MPS_t	-0.956***	-0.057
	(-4.23)	(-0.63)
(2) ΔIP_t	0.248***	-0.032
	(3.83)	(-1.54)
(3) $\Delta\pi_t$	0.216**	0.204***
	(2.09)	(4.16)
Observations	168	
R^2	0.443	

Notes: Dependent variable is the revision of 1-year ahead expected inflation. MPS_t denotes the cumulative Romer and Romer (2004) monetary policy shocks from period t to $t+5$. ΔIP_t denotes the log difference of Industrial Production between period $t+6$ and t. $\Delta\pi_t$ denotes the inflation rate changes between period $t+6$ and t. Sample period spans from 1985m1 to 2007m12. Newey-West standard errors with 6 lags are used for the inference. t statistics in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendix Table D.30: Uncertainty Amplifies Expectation Responses (Dropping Small Unemployment Changes)

	(1) Accurate	(2) Inaccurate	(3) Accurate	(4) Inaccurate	(5) Accurate	(6) Inaccurate
Panel A: NBER						
(1) Recession	-1.693***	-1.235				
	(-3.97)	(-1.08)				
(2) Normal	-0.016	0.106				
	(-0.17)	(0.90)				
Panel B: LMN Real(1)						
(3) High Uncertainty			-0.574***	0.025		
			(-5.03)	(0.16)		
(4) Low Uncertainty			-0.300*	0.304		
			(-1.81)	(1.43)		
Panel C: VIX						
(5) High Volatility					-0.441***	0.032
					(-3.57)	(0.16)
(6) Low Volatility					0.040	0.066
					(0.33)	(0.45)
R^2	0.0182		0.0156		0.0196	
Controls	Yes		Yes		Yes	
Observations	30,321		30,321		30,321	

Notes: Dependent variable is the revision of 1-year ahead expected inflation. MPS_t denotes the normalized cumulative monetary policy shocks between period $t+5$ and t to change the shadow rate by 1%p between period $t+5$ and $t-1$. Business cycle dummies are constructed by NBER recession indicator, LMN real (1) and VIX for each panel respectively. Each number of the panel comes from the interaction term. Robust standard errors are used in calculation. We use the individual information about age, income level and quartiles, homeownership, stockownership, gender, education level, region , marital status and an indicator for reporting unfavorable news as controls. t statistics in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Appendix Table D.31: Accuracy with Stockholding, Homeownership, and Age (Dropping Small Unemployment Changes)

	(1) Accurate	(2) Inaccurate	(3) Accurate	(4) Inaccurate	(5) Accurate	(6) Inaccurate
Panel A: Stock-holding						
$Stock \times MPS_t$	-0.423***	0.217				
	(-4.18)	(1.55)				
$NonStock \times MPS_t$	-0.191	-0.222				
	(-1.03)	(-0.90)				
Panel B: Homeownership						
$Homeowner \times MPS_t$			-0.451***	0.042		
			(-4.65)	(0.32)		
$Renter \times MPS_t$			0.099	0.334		
			(0.44)	(1.04)		
Panel C: Age						
$Young \times MPS_t$					-0.562***	0.353
					(-2.59)	(0.98)
$Middle \times MPS_t$					-0.371***	0.132
					(-3.46)	(0.94)
$Old \times MPS_t$					-0.240	-0.275
					(-1.12)	(-0.95)
Interaction	Stockownership		Homeownership		Age Group	
Controls	Yes		Yes		Yes	
Observations	30,321		30,321		30,321	
R^2	0.0151		0.0151		0.0160	

Notes: Dependent variable is the revision of 1-year ahead expected inflation. Each number of the panel comes from the interaction term. Robust standard errors are used in calculation. We use the individual information about age, income, homeownership, stockownership, gender, education level, region , marital status and an indicator for reporting unfavorable news as controls. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table D.32: Accuracy with Income Quartile (Dropping Small Unemployment Changes)

	(1) Accurate	(2) Inaccurate
(1) $YTL1 \times MPS_t$	-0.074	-0.273
	(-0.26)	(-0.75)
(2) $YTL2 \times MPS_t$	-0.623***	-0.034
	(-3.28)	(-0.12)
(3) $YTL3 \times MPS_t$	-0.318**	0.296
	(-2.04)	(1.37)
(4) $YTL4 \times MPS_t$	-0.333**	0.140
	(-2.44)	(0.71)
Interaction	Income Quartile	
Controls	Yes	
Observations	30,321	
R^2	0.0164	

Notes: Dependent variable is the revision of 1-year ahead expected inflation. We use YTL4 variable from MSC to define consumers' income group. Each number of the panel comes from the interaction term. Robust standard errors are used in calculation. We use the individual information about age, income level and quartiles, homeownership, stockownership, gender, education level, region , marital status and an indicator for reporting unfavorable news as controls. t statistics in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.