The Messenger Matters*

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Abstract

Does the messenger matter in communication with the public? For policy communication on monetary, climate, fiscal, or other issues to be impactful, it must successfully reach people and influence their beliefs. I combine novel empirical evidence with theory to study how messenger identity, particularly nationality, influences central bank communication in the Euro area. First, I construct a multilingual dataset of over 8 million tweets and document three novel stylized facts for ingroup audiences: greater salience of policymakers, higher information availability, and stronger belief updating. Second, I design an inflation forecasting experiment, identifying causal evidence that ingroup messengers significantly increase the use of information. Third, I incorporate these effects into a stylized coordination model and demonstrate that delegating communication to multiple heterogeneous messengers maximizes welfare when public information quality is high, while centralizing communication is preferable when it is low. These findings identify the strategic selection of messengers as an additional policy tool, complementing traditional disclosure policies.

Keywords: Central Bank Communication, Inflation Expectations, Ingroup Effects, Coordination Games
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1 Introduction

Communicating a policy effectively to the public is critical for its impact: people will only respond if they receive the information and believe it. This includes health campaigns promoting vaccine uptake during pandemics, subsidies and taxes incentivizing climate-friendly behavior, and monetary policy aimed at influencing consumption and savings decisions. How can policies – whether on climate, health, fiscal, monetary, and other issues – be communicated effectively to the public?

One important dimension that can be varied is the messenger. Communicating with the wider public means the audience is diverse in gender, ethnicity, nationality, and other dimensions. This may raise the importance of the communicating policymaker, given so-called "*ingroup effects*": in interpersonal settings, individuals respond more strongly to information provided by someone who shares their characteristics (i.e., someone from an "*ingroup*") across various contexts such as education (Carrell et al., 2010; Gershenson et al., 2022), health (Alsan et al., 2019), or finance (Stolper and Walter, 2019). Malmendier and Veldkamp (2022) formalize ingroup effects in economic decision-making and show their relevance for occupational choice. Policy communication, however, differs from these interpersonal settings: information is typically received through the media rather than directly from a messenger, and institutional reputation may overshadow the importance of the individual messenger.

In this paper, I combine novel empirical evidence and theoretical methods to address two questions. First, positively, how does messenger identity impact policy communication with the public? Second, normatively, how should policy be communicated to the public in light of these messenger effects? I focus on central bank communication, which has become core component of central banks' monetary policy toolkit (see Blinder et al. (2024) for a recent overview). Specifically, I analyze communication by the European Central Bank (ECB), an international organization that provides a primary example of communicating with diverse audiences. I study nationality – a salient yet understudied characteristic – to investigate ingroup dynamics. To do so, I first create a new multilingual dataset of tweets that provides novel evidence of ingroup effects along two key dimensions: information availability and information processing (belief updating). I then design an inflation forecasting experiment, in which participants of four different nationalities forecast inflation subject to receiving signals from various messengers. The treatment is an isolated variation in messenger nationality (with and without institutional context), allowing me to precisely estimate the causal effect of messenger identity on inflation expectations. With this design, I further confirm the efficacy of two concrete examples of delegating communication: to other board members of the same institution or to other institutions. Finally, based on these reduced-form insights, I develop a stylized coordination model to derive welfare-maximizing communication policies when policymakers and audiences are diverse. I identify the strategic delegation of communication as an additional policy tool that complements the well-studied disclosure policy.

I document three motivating stylized facts showing that messenger nationality impacts the effectiveness of policy communication in the Euro area, using over 8 million multilingual tweets from 2016 to 2022. First, the salience of the messenger (here, the president of the ECB) rises strongly for the ingroup. Second, information availability – measured as the volume of tweets – is higher for the ingroup. And third, after the ECB's press conference, Twitter users update their beliefs more strongly when they are in the ingroup.

Because tweets, while informative on real-world messenger effects, come with data limitations and cannot directly speak to inflation expectations, I design a pre-registered inflation forecasting experiment to estimate causal ingroup effects, underlying mechanisms, and messenger-driven attention to information. In this experiment, participants from Germany, Spain, France, and Italy forecast inflation in six different scenarios. Each time, they receive a signal about future inflation from varying messengers, and then get the chance to update their forecasts. The treatment consists of isolated variation in the nationality and/or institutional affiliation of otherwise uncharacterized messengers. Using a standard Bayesian belief updating framework, I estimate the causal nationality-based ingroup effect on updating inflation expectations, with and without institutional context. The within-subject design allows me to further distinguish between two drivers of ingroup effects: *homophily* (preference for similar others) and *heterophobia* (aversion to different others). In addition, I provide causal evidence for two concrete policy recommendations: delegating communication to diverse messengers within an institution, and delegating communication to other institutions.

Across messenger treatments, participants under-use signals by about 10 percentage points relative to a Bayesian benchmark, consistent with survey evidence of under-reaction to news (Coibion and Gorodnichenko, 2012). However, signals from ingroup messengers are used 5.2 percentage points more than those from outgroup messengers – roughly half of the average information under-use. This constitutes the pure nationality-based ingroup effect on information processing related to inflation expectations. Delegating communication to ECB experts of the same nationality, or to national central banks (NCBs), successfully replicates these ingroup effects, albeit to a lesser extent. This points to two feasible policy interventions within the existing institutional framework: communicating through several board members covering various nationalities or through the Eurosystem of NCBs. The positive ingroup effects on belief updating are due to homophily rather than heterophobia, primarily driven by trust, and associated with self-reported perceived messenger quality.

After the experiment, participants complete a survey designed to directly measure messenger effects on information availability, complementing the Twitter evidence. The results show that individuals receive more news about their ingroup policymakers, indicating broader reach beyond higher news coverage. However, while messengers influence real-world information availability and reach, they do not causally affect attention to information in the experiment. Instead, attention responds causally to the level of inflation and its uncertainty, in line with attention to information being endogenous to the inflationary environment.

My empirical evidence shows that a shared identity with the messenger improves information availability and use. Does this imply that public policy should always be communicated through multiple messengers to raise diversity? This is a normative question on how to design optimal communication policy that maximizes social welfare. To address it, I develop a model with strategic complementarities in the spirit of a "beauty contest" (Morris and Shin, 2002), which captures the dual role of disclosing public information: to inform and to coordinate. Agents care about aligning their actions with both an unknown fundamental and the actions of others, and their in- or outgroup status with the messenger shapes how they receive and process public information. While linked to the empirical findings based on nationality, the model remains abstract, allowing findings to extend to public communication with heterogeneous messengers and receivers across various dimensions (such as gender, ethnicity, age, socioeconomic status, or expertise), and applicable to policy communication beyond monetary issues. The model evaluates two communication policies: a disclosure policy and a delegation policy. The disclosure policy involves setting the precision of public information, while the delegation policy entails choosing the messenger(s) to deliver public information, thereby influencing the share of agents classified as ingroup.

The model delivers a simple policy map: when coordination is weak, delegation is always beneficial, especially when public information is precise relative to private information. By contrast, when coordination is strong and the public signal is noisy, both disclosure and delegation should be restrained; as the signal improves, disclosure should rise while communication should remain centralized; once sufficiently precise, both disclosure and delegation raise welfare. Thus, *strategic delegation* is a powerful policy tool to maximize welfare: diversifying messengers is optimal when the relative quality of public information is high, while centralizing communication can be preferable when it is low, even when disclosure may raise welfare, making it *distinct* from disclosure.

The messenger matters. For effective policy communication, the identities of the messenger and receivers are important. By strategically delegating communication to messengers with diverse characteristics, policymakers can influence how messages are received and processed, and ultimately maximize social welfare through their disclosure of public information.

Related Literature. This paper contributes to the literature on central bank communication with the non-expert public by assessing the importance of the messenger in reaching and influencing non-experts. While communication with financial market participants has long been recognized as a powerful policy tool, targeting the non-expert public is a more recent endeavor. When successful, such communication can influence institutional trust, inflation expectations, and economic volatility following monetary policy decisions. Yet, doubts remain about whether central banks actually reach the public. Some studies find that central bank communication does influence media coverage (Lamla and Vinogradov, 2019; Ter Ellen et al., 2022), and while non-experts react to some communication events, they are generally less responsive than experts (Ehrmann and Wabitsch, 2022). Blinder et al. (2024) provide a comprehensive overview of this literature, building on work such as Blinder et al. (2008), Ehrmann et al. (2013), Binder (2017), Blinder (2018), Haldane and McMahon (2018), and Coibion et al. (2020).

At the core of this paper is an inflation forecasting experiment designed to causally identify how messenger identity shapes inflation expectations, contributing to the growing literature on macroeconomic experiments (see Haaland et al. (2023) for an overview). I vary the messenger across forecasting tasks, extending McMahon and Rholes (2023), who use such tasks to study central bank credibility. For each messenger, I randomize forecaster performance and the inflationary environment, drawing on historical data to mirror credible dynamics. Another key novelty is the causal measurement of attention to information, implemented through interactive features that reveal how messenger identity and the inflationary environment affect information acquisition. My within-subject design, where each participant experiences all treatments, offers greater flexibility than between-subject randomized control trials (RCTs). A thematically close example is the RCT by D'Acunto et al. (2022), which demonstrates the importance of diverse policymakers for central bank communication. My design complements this work by varying only a single attribute at a time – messenger nationality or institutional affiliation – allowing the clean estimation of ingroup effects without confounds from multiple identity dimensions. It further decomposes these effects into homophily and heterophobia, quantifies the roles of perceived quality and trust, and provides causal evidence for concrete policy recommendations. Finally, by presenting multiple inflation scenarios, the experiment mitigates the influence of contemporaneous macroeconomic conditions on treatment effects (Weber et al., 2025).

This paper also ties to the literature on social identity by cleanly identifying ingroup effects in central bank communication and assessing their impact on the social value of public information. The literature documents that ingroup effects improve the use of information in various domains, including education, healthcare, finance, and economics.[1] In central banking, recent evidence suggests policymakers' gender and ethnicity affect perceptions, trust, and influence (D'Acunto et al., 2022; Bodea et al., 2021; Bodea and Kerner, 2022). In the same context, this paper designs an experiment to cleanly identify ingroup effects attributed to an *isolated* characteristic: nationality (outside and within institutional context). Malmendier and Veldkamp (2022) find ingroup effects influence occupational choices, and formalize ingroup-dependent belief formation in economic decision-making more generally. This paper is, to the best of my knowledge, the first to incorporate such ingroup effects into a model of strategic complementarities.

Finally, in doing so, this paper contributes to the literature on optimal disclosure in coordination models. These models are used to evaluate the social value of public information and allow for the assessment of welfare-maximizing communication policy. First explored by Morris and Shin (2002), they have substantially evolved since.[2] My model incorporates the empirically identified messenger effects, distinguishing agents by whether their characteristics align with the messenger of the public signal, which determines an agent's information availability and processing. While Cornand and Heinemann (2008) discusses partial information availability, my model differs by introducing a belief updating bias and endogenizing signal availability to signal size. The latter extends an idea by Nimark and Pitschner (2019) whereby larger signals are more likely reported by the media. The belief updating bias that I introduce is a *resonance weight* to expectation formation (Malmendier and Veldkamp, 2022), complementing behavioral distortions such as myopia, or extra discounting of the future (Gabaix, 2020; Angeletos and Huo, 2021). My model set-up allows me to consider a new communication policy: the *delegation* of communication to other messengers. This policy complements the widely explored disclosure policy (e.g., James and Lawler 2011, Angeletos and Lian 2018, Bassetto 2019, Kohlhas 2020, Kohlhas 2022).

[1] In education, Black teachers positively impact Black students' outcomes, with stronger effects when gender also matches (Gershenson et al., 2022; Price, 2010; Carrell et al., 2010). In healthcare, Alsan et al. (2019) find that Black men are more likely to seek preventative care when interacting with Black male doctors. In financial advising, Stolper and Walter (2019) show that homophily significantly increases clients' likelihood of following advice. The literature on social identity also extends to disciplines outside economics and finance, such as psychology and sociology, which exceed the scope of this paper but provide important foundations.

[2] They show that public information can disproportionately influence agents' actions, potentially leading to welfare-reducing overreactions to noisy public signals. Developments since Morris and Shin (2002)'s seminal work include Svensson (2006) arguing that increased precision of public information is likely beneficial for reasonable parameter values, and Amador and Weill (2010) examining a dynamic setting, highlighting varying welfare outcomes.

2 Three Stylized Facts

This section motivates the core claim that messenger identity matters for policy communication. Using high-frequency data from the Euro area (EA), I document how messenger nationality shapes communication outcomes in a monetary union. Three stylized facts emerge: following policy communication, messenger identity affects (i) the salience of the messenger, (ii) the availability of information, and (iii) belief updating. Together, these patterns suggest that messenger identity plays an important role in the effectiveness of policy communication.

2.1 Twitter Data

Twitter provides high-frequency, unsolicited textual data on how individuals engage with ECB communication across nationalities. Its scale allows for the observation of attention, reach, and belief formation at a granularity that other data sources cannot achieve. To capture these dynamics empirically, I construct a multilingual dataset of publicly available tweets posted between 1 November 2016 and 31 October 2022, using the Academic Twitter API. This period covers three full years under Mario Draghi's presidency (1 November 2016–31 October 2019) and three years under Christine Lagarde (1 November 2019–31 October 2022). Tweets are included if written in German, French, Italian, or Spanish – reflecting the main languages of the four largest EA countries. Tweet language proxies nationality.[3] English tweets are also included for completeness, but are excluded from nationality-specific analyses because they cannot be reliably attributed to a single national audience. Tweets are selected if they contain the abbreviation "ecb," the phrase "European Central Bank," or their language-specific equivalents.[4] The final dataset contains 8,031,937 tweets after extensive cleaning and translation (see Table A6 for a summary by language). Appendix A.1 documents the cleaning process and language-specific descriptives.

Relative to survey or market data, Twitter offers several advantages. Tweets are unsolicited, high-frequency expressions of public opinion, enabling the observation of belief updates among both professionals and non-experts, as well as repeated tracking of the same users over time, thereby facilitating panel-style analysis. Although Twitter users are not representative of the general population, the platform's scale provides a valuable window

[3]Appendix A.1 provides evidence validating tweet language as a proxy for nationality. The experiment later in the paper does not rely on this assumption.

[4]Selection terms follow Ehrmann and Wabitsch (2022), with two key modifications. First, tweets are excluded if they mention "Draghi" or "Lagarde" without reference to the ECB, to avoid capturing content unrelated to ECB communication (e.g., Draghi's role as Italian Prime Minister or Lagarde's prior tenure at the IMF). Second, tweets must contain keywords in the original content, including tweets, replies, quote tweets, and retweets.

into cross-national communication patterns. These descriptive patterns, while not causal, motivate the experimental design and guide the hypotheses tested in the next section.

For the descriptive evidence presented in this section, I define *the ingroup* as users whose tweet language corresponds to the nationality of the ECB president. Thus, Italian-language tweets are ingroup during Draghi's term and French-language tweets during Lagarde's.[5]

2.2 Stylized Fact 1: Greater Messenger Salience

The first stylized fact concerns salience: messenger identity directly influences how prominently individual messengers are reported. This is important because salience-based models predict that salient attributes disproportionately capture attention and influence choices (Bordalo et al., 2012, 2013), and because models of limited attention highlight "coarse thinking," whereby individuals focus on broad categories – such as who the messenger is – rather than processing all available information (Mullainathan et al., 2008).

To document this, I compare how often ECB presidents are mentioned across language-specific Twitter samples. Figure 1 presents word clouds for English, Italian, and French tweets. Although tweet selection is based only on ECB-related keywords and not on policymaker names, "Draghi" ranks in the Italian top 10 but around 50$^{\text{th}}$ in French, while "Lagarde" is in the French top 10 but much lower in Italian. In English, German, and Spanish samples – where neither president is in the ingroup – both names appear with similarly low frequency.

These differences underscore that messengers of policy communication are not perceived uniformly. ECB presidents are mentioned substantially more often in tweets written in their ingroup language, indicating that individual policymakers are indeed noticed and that their identities systematically shape reporting prominence. Establishing this fact is crucial for interpreting subsequent stylized facts on communication reach and influence as potentially driven by messenger identity.

2.3 Stylized Fact 2: Higher Information Availability

The second stylized fact concerns information availability and diffusion: messenger identity is systematically associated with the information available on communication events, suggesting a greater potential reach of policy communication. Such variation in available information is important as it can influence business cycle dynamics and political participation (Chahrour et al., 2021; Gentzkow et al., 2011).

To assess information availability, I compare tweet volume across languages. For each language, I compute the relative share in tweet volume and compare it across presidencies in

[5] The experiment later in the paper generalizes this definition to abstract from individual ECB presidents.

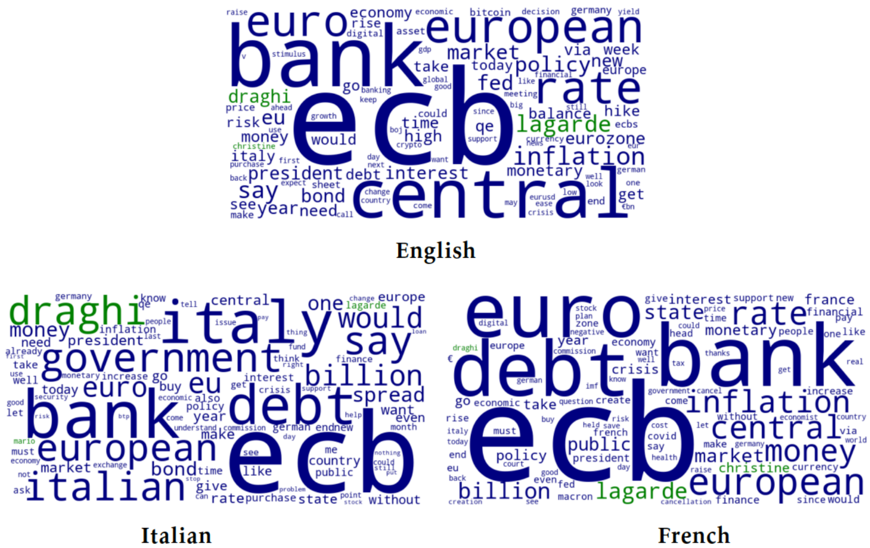


Figure 1: Reporting on Policymakers by Language

Notes: The 100 most frequent words in ECB-related tweets posted in English (top), Italian (bottom left), and French (bottom right) between 2016 and 2022. Words are ranked by frequency after text cleaning, lemmatization, and removal of stopwords. Names of ECB presidents are highlighted in green. See Figure A8 for German and Spanish versions.

Figure 2 (left panel). The sample is split into 6-week cycles corresponding to the interval between ECB press conferences – the main communication event – to mitigate concerns that general Twitter adoption or one-off events drive the results. Spanish users are consistently the most active, accounting for nearly 40% of ECB-related tweets, followed by Italians with about one-third. During Draghi's presidency, the Italian share rises markedly, while French tweet shares increase sharply under Lagarde. By contrast, Spanish and German tweet shares remain relatively stable.[6]

A simple regression estimates that ingroup status increases information availability by 10.5 percentage points (p-value < 0.01) (Table A7), a sizable effect given average tweet shares of 19% in France and 28% in Italy. This effect is not only due to journalists or other professionals: following Ehrmann and Wabitsch (2022), I classify users by their monetary expertise and find that the increase holds across all groups, being strongest among those who are neither strict experts nor complete non-experts.[7]

This pattern is confirmed in traditional print media. Using LexisNexis data for major

[6]The German share rises slightly during Lagarde's term, which may reflect Isabel Schnabel's entry to the ECB Executive Board or the coinciding disproportional growth of German Twitter (see Table A5 in Appendix A.1). These factors highlight that, while patterns suggest ingroup effects on information availability, they remain descriptive and should not be interpreted as causal.

[7]Users are classified as experts if they comment on at least every second press conference; non-experts if they do so only sporadically and predominantly tweet about ECB-unrelated topics; and neither experts nor non-experts if they fall in between.

national newspapers between 2016 and 2022, I apply the same selection criteria as for Twitter (articles mentioning "ECB," "European Central Bank," or their local-language equivalents) and treat the switch in ECB presidency as a pseudo-treatment in a difference-in-differences framework (see Appendix A.2 for details). Controlling for time and country fixed effects, an ingroup president increases national ECB-related coverage by 6.1 percentage points (p-value < 0.01), equivalent to roughly 272 additional articles per year (Table A8). This parallel evidence confirms that information availability rises in both social and traditional media when messengers share the audience's nationality.

These findings highlight that messenger identity systematically shapes the availability of policy information. Section 3.6 and Appendix B.4 complement this evidence by showing that information communicated by ingroup policymakers is not only more available but also more likely to reach individuals.

2.4 Stylized Fact 3: Stronger Belief Updating

The third stylized fact concerns how individuals process information: messenger identity affects how strongly individuals adjust their beliefs in response to new information. In Bayesian models, stronger belief updating to identical messages arises from higher perceived signal accuracy. If ingroup messengers are perceived as more accurate, policy communication becomes more effective within the ingroup.

To document this, I assess how within-user beliefs change following ECB press conferences (the signals). These press conferences are ideal to study because they are the institution's primary event for communicating monetary policy, highly anticipated, and their messages are carefully crafted. They occur every six weeks, yielding 48 observations in my sample.[8]

The beliefs of interest are about the economy and are proxied with sentiment, a widely used measure of shifts in expectations about economic conditions (e.g., Jaimovich and Rebelo 2009; Chahrour et al. 2021; Flynn and Sastry 2025). I measure tweet sentiment using a dictionary-based approach for its simplicity and transparency, which provides a score of how positive or negative a tweet is.[9] Sentiment distributions are highly similar across languages, shown in Appendix A.1 alongside details of measuring sentiment.

Belief updating is defined as the absolute change in a user i's sentiment between their final tweet in the 7-day quiet period before press conference j (the "pre" tweet) and their first tweet within 24 hours after it (the "post" tweet), relative to the average update across all

[8] The monetary policy announcement changed from 13:45 to 14:15 CET after July 21, 2022. Similarly, the starting time of the press conference shifted from 14.30 to 14:45 CET (see here).

[9] Specifically, I use the polarity measure integrated in the Python package *TextBlob*, which is based on the Princeton University's WordNet lexicon (Loria, 2018). Results are robust to alternative natural language processing (NLP) methods such as *BERT*, a large language model (LLM).

N_j users at that conference.[10] Taking the absolute difference prevents positive and negative changes from canceling out, while subtracting the conference average controls for information content, isolating deviations from the common trend.

I find that beliefs shift more strongly following ECB press conferences when the president shares the audience's nationality: Italians update relatively more under Draghi, and the French under Lagarde. By contrast, the Spanish and German samples serve as comparison groups, where updating should not vary across time (see right panel of Figure 2).[11]

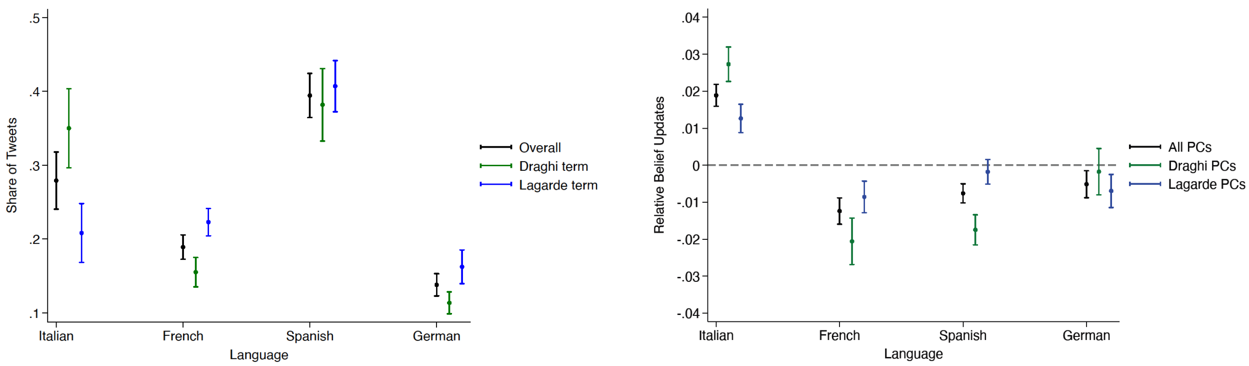


Figure 2: Ingroup Effects on Information Availability and Belief Updating

Notes: Ingroup effects on tweet volume (left panel) and belief updating (right panel). The left panel shows the share of tweets by language across each 6-week press conference cycle, with 95% confidence intervals. The right panel plots absolute belief updates relative to the conference-specific average, calculated as the absolute sentiment change in user tweets before and after press conferences. Green and blue shaded areas indicate Draghi's and Lagarde's presidencies, respectively.

A simple OLS regression estimates an ingroup effect of 0.014 (p-value < 0.01), which is sizable given that average belief updates across conferences lie within ± 0.020. These larger sentiment shifts confirm stronger belief updating for the ingroup. Appendix A.3 further demonstrates that belief updating is directionally aligned with the signal: using the monetary surprise index of Altavilla et al. (2019) confirms that both in- and outgroup users incorporate new information, but the ingroup places more than twice the weight on ECB signals and about half as much on prior beliefs (Table A10). The effects persist across monetary expertise levels, though they are somewhat stronger among non-experts (Figure A12).

In sum, this stylized fact shows that messenger identity shapes not only the salience and

[10] The quiet period refers to the seven days preceding each press conference, during which no major ECB communications occur. The sentiment during this period, $Prior_{i,t-1} \equiv Sentiment_{i,t-1}$, captures users' prior beliefs. This window isolates responses to the press conference itself while limiting confounding from other ECB news. Formally, $RelativeUpdate_{ij} = \Delta Sent_{ij} - \frac{1}{N_j}\sum_{k=1}^{N_j} \Delta Sent_{kj}$, where $\Delta Sent_{ij} = \left|Sent_{i,\text{post}(j)} - Sent_{i,\text{pre}(j)}\right|$.

[11] Germans show no difference in updating behavior across presidents. The stronger Spanish updating under Lagarde may reflect the contemporaneous presence of Spanish Vice President de Guindos, who also participates in ECB press conferences.

availability of policy information but also the way audiences process and internalize it. The stronger belief updating observed for the ingroup suggests that policy communication is more influential when conveyed by an ingroup messenger.

2.5 Discussion of Messenger Effects

Taken together, the three stylized facts delineate the main dimensions of messenger effects: messenger identity shapes how prominently policymakers are reported on, as well as how information spreads and how beliefs adjust. Ingroup messengers therefore improve both the reach and influence of policy communication.

While this descriptive evidence offers valuable insight into real-world communication dynamics, the Twitter-based analysis has several limitations. First, it focuses on two specific policymakers within a particular institutional context, so observed patterns may partly reflect individual characteristics beyond nationality. Second, Twitter activity is shaped by contemporaneous economic news, evolving platform algorithms, and changes in user composition, which complicate causal interpretation. Third, sentiment provides only a noisy proxy for beliefs, capturing expressed tone rather than fully articulated expectations. Finally, language differences may introduce additional measurement noise, though the time-series structure and pseudo–difference-in-differences approach help mitigate such concerns by focusing on within-language variation over time. These limitations underscore that, while informative, the descriptive evidence cannot establish causality, motivating the need for a controlled experiment.

Building on the stylized facts, the experiment exogenously varies messenger identity to identify how messenger characteristics causally shape belief formation about inflation expectations and to investigate the underlying mechanisms through four targeted hypotheses. First, I test whether individuals place greater weight on information from nationality-matched messengers (*pure ingroup effect*). Second, I assess whether such ingroup effects persist within an institutional context, such as the ECB (*institutional context*). Third, I distinguish whether ingroup effects stem from a relative over-use of ingroup signals (*homophily*) or an under-use of outgroup signals (*heterophobia*). Finally, I test whether these dynamics extend to the institutional level by comparing the effectiveness of communication from national central banks and the ECB (*institutional affiliation*).

3 Experiment: Messenger Effects on Expectations

This section presents the pre-registered inflation forecasting experiment. In this individual-choice experiment, participants in Germany, Spain, France, and Italy forecast inflation, receiving forecasts attributed to different messengers (the treatments). The design provides causal evidence on how messenger identity and institutional context shape belief updating, the mechanisms behind these effects, and attention to information.

3.1 Experimental Design

The experiment is designed to isolate the causal effect of messenger identity on belief updating. It varies one messenger characteristic at a time while randomizing inflation conditions and task order, ensuring that observed differences can be attributed to an isolated messenger characteristic. This approach limits confounds such as other traits of real-world policymakers, institutional context, or the inflationary environment, all of which can shape information use (D'Acunto et al., 2022; Weber et al., 2025). The within-subject design builds on McMahon and Rholes (2023), who study forecaster credibility, but extends their approach by systematically varying the identity of the forecaster (the "messenger"), incorporating real-world inflation and forecast data for credible dynamics, and introducing new features to measure attention to information. Figure 3 outlines the sequence of steps in the experiment.[12]

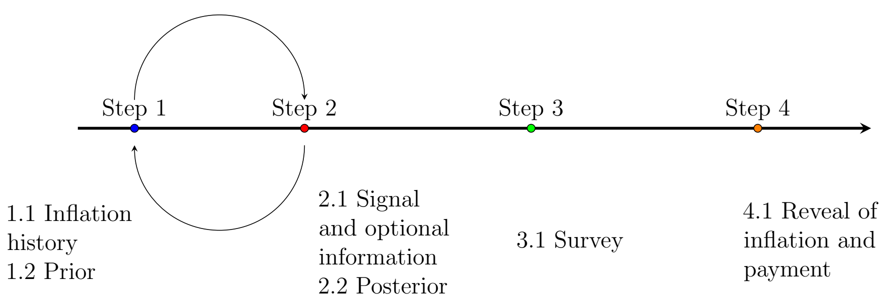


Figure 3: Timeline of Experiment

Notes: The timeline outlines the sequence of steps in tge experiment. Steps 1-2 repeat for all six forecasting tasks; followed by step 3 and 4.

Forecasting Tasks. To study belief updating, participants complete six forecasting tasks designed to elicit prior and posterior beliefs about inflation expectations after receiving a signal. Each task is based on real historical Euro area (EA) inflation data and professional

[12]The experimental interface is coded in oTree (Chen et al., 2016), using interactive tools and visuals to facilitate task comprehension. Appendix B.3 shows the entire experimental interface.

forecasts, ensuring realistic dynamics.[13] In each task, participants first observe ten periods of realized inflation before submitting a point and range forecast for period 11, capturing their prior beliefs and associated precision (i.e., subjective uncertainty; formal precision measures are defined in Section 3.3). They then receive a professional forecast for period 11 (the signal), attributed to a randomly assigned messenger and shown together with the messenger's forecast history from the preceding ten periods. The messenger's historical forecast performance conveys signal precision based on past forecast errors, mirroring how forecast credibility is typically evaluated in practice. Historical inflation and forecasts are displayed visually, with exact values available on hover (Figure A22 shows a screenshot of the interface). Finally, participants re-submit their forecasts for period 11, which captures their posterior beliefs. Each unique combination of inflation realizations and forecasts constitutes a scenario, and six scenarios are randomized across messenger treatments.

Messenger Treatments. Each forecasting task features one of ten messenger treatments. Treatments fall into three categories, as summarized in Table 1 and aligned with the hypotheses outlined in Section 2.5.

Table 1: Overview of Messenger Treatments

Treatment	**Hypotheses**	**Messenger**
1	H1	Expert from Germany
2	H1	Expert from Spain
3	H1	Expert from France
4	H1	Expert from Italy
5	H2, H3	Expert from Germany who represents the **ECB**
6	H2, H3	Expert from Spain who represents the **ECB**
7	H2, H3	Expert from France who represents the **ECB**
8	H2, H3	Expert from Italy who represents the **ECB**
9	H3, H4	Expert who represents the European Central Bank (**ECB**)
10	H4	Expert who represents the National Central Bank (**NCB**)

Notes: Messenger treatments and their mapping to hypotheses H1–H4. Colors are used here only for visualization; in the experiment all text appeared in black.

(i) *Nationality-Only Experts*: These treatments identify the pure ingroup effect by comparing information use from generic experts distinguished *only* by nationality, with no other identifying features. Four nationalities are used, corresponding to participant nationalities. Each participant encounters one expert of their own nationality (the generic ingroup expert)

[13]Appendix B.1 displays the full quarterly data series (Figure A13) and the forecast-realization pairs for the six randomly selected sequences (Figure A14), along with additional details on average forecast precision, mean realized inflation, and the standard deviation of realized inflation for the selected sequences (Table A11).

and one expert of another nationality, randomly selected from the three remaining options (the generic outgroup expert).

(ii) *Nationality-ECB Experts*: These add institutional context where messengers are ECB experts with specified nationalities. One matches the participant's nationality (the ECB ingroup expert), while the other does not (the ECB outgroup expert). For consistency, the outgroup nationality is held constant within each participant and matches that of the generic outgroup expert. This design allows direct comparison of ingroup and outgroup effects across institutional and non-institutional contexts, providing the basis for assessing whether delegating communication to different ECB policymakers impacts effectiveness.

(iii) *Institutional Experts*: These present experts without specified nationalities, attributed either to the ECB (the ECB expert) or to the participant's national central bank (the NCB expert).[14] These treatments test whether delegating communication to NCBs generates effects similar to nationality-based ingroup dynamics. In combination with the ECB in- and outgroup expert treatments in (ii), they also allow me to distinguish whether institutional ingroup effects are driven by homophily (greater use of ingroup signals) or heterophobia (reduced use of outgroup signals).

To limit fatigue, each participant is faces six treatments – two from each category. Treatment order and the allocation of inflation scenarios to messengers are randomized at the participant level, ensuring valid comparisons across messenger treatments.

Attention to Information. In each forecasting task, participants can request additional qualitative information underlying the messenger's forecast, providing a measure of attention to information such as central bank communication. Up to three "Read More" buttons reveal details from historical ECB communications corresponding to the SPF forecasts, ensuring consistency between the qualitative information and the numerical signal.[15] Attention is measured by the number of buttons clicked: information is presented as a flow, and participants decide when to stop, thereby revealing their chosen level of attention. This approach follows the literature on information acquisition, which similarly measures attention by tracking clicks on résumés (Bartoš et al., 2016). It offers a more reliable measure than completion time, a common proxy for attention, since long decision times may reflect slow thinking or unrelated interruptions rather than genuine attention.

Incentives and Survey. Forecasts are incentivized to reduce forecast error, ensuring high-quality decisions (Charness et al., 2016). Following common practices in experimental

[14]This corresponds to the Deutsche Bundesbank for Germans, Banque de France for the French, Banca d'Italia for Italians, and Banco de España for Spaniards.

[15]Appendix B.1 (Table A12) lists all information pieces and their corresponding inflation scenarios.

economics, I use scoring rules that reward the accuracy of both point and range forecasts.[16] The latter are further incentivized for precision, resulting in range forecasts that are plausible and narrow (see Appendix B.1 for details on incentives). After completing all tasks, participants answer a short survey about perceived messenger ability, trust in institutions and real-world policymakers, and their exposure to news about these policymakers.[17] This enables me to test mechanisms behind the observed effects.

Full details on the data, forecasting sequences, incentive schemes, provided information, post-experimental survey are described in Appendix B.1, and screenshots of the experimental interface are shown in Appendix B.3.

3.2 Experimental Sample

The experiment is conducted in Germany, Spain, France, and Italy to ensure that culturally driven effects are representative of the EA.[18] A total of 400 participants (100 per country) complete the experiment online via Prolific. One Spanish outlier with implausibly high forecasts is dropped, leaving 399 participants.[19] A power analysis is reported in Appendix B.2. Participants must pass a comprehension quiz before proceeding.[20] Most data are collected on October 31, 2023, with a few additional responses within 48 hours. Average earnings are £7.58 (£4 participation fee plus £3.58 in bonuses).[21]

While not perfectly representative, the sample broadly matches national demographics for employment, education, and income. Participants are slightly younger than the population average but considerably more diverse than typical lab samples, which usually consist only of students. Nationality and residence are required to coincide, ensuring strong cultural identification and excluding individuals living abroad. Appendix B.2 provides detailed comparisons with national benchmarks.

[16] Similar approaches have been adopted by e.g., Pfajfar and Žakelj 2016, Rholes and Petersen (2021), Mokhtarzadeh and Petersen 2021, Petersen and Rholes 2022, and McMahon and Rholes 2023.

[17] These policymakers are the sitting NCB governors and ECB board members of the four nationalities at the time of the experiment (see Table A13). All survey questions are asked after the forecasting tasks to avoid priming, but before revealing the realized values, preventing task performance from influencing responses. The full survey and screenshots are shown in Figures A24–A29 in Appendix B.3.

[18] This is a between-subject element of the design.

[19] Forecasts consistently exceeded 20%, reaching up to 70%, suggesting disregard for instructions and incentives.

[20] Of 553 participants who began, 153 dropped out after failing the quiz, returning the task, or timing out.

[21] Bonuses are based on forecast accuracy, with a maximum of £6.

3.3 Estimating Belief Updating

I benchmark belief updating against a standard Bayesian framework where posterior beliefs are a weighted average of priors and signals, with weights given by relative precision (e.g., Veldkamp 2011). Appendix B.4 provides details of this common framework.[22] In practice, participants deviate systematically from Bayesian updating, typically under-weighting signals.

To measure deviations from the Bayesian benchmark, I regress posterior beliefs on weighted priors and weighted signals

$$Posterior_i = \gamma \underbrace{\left(\frac{\alpha_i}{\alpha_i + \beta} A_i \right)}_{\text{weighted Prior}} + \delta \underbrace{\left(\frac{\beta}{\alpha_i + \beta} B \right)}_{\text{weighted Signal}} + \epsilon_i, \tag{1}$$

where A_i denotes the prior, B the signal, α_i prior precision, and β signal precision. Prior precision, α_i, is proxied by the inverse of the participant's subjective forecast uncertainty, measured as the width of the elicited forecast range. This assumes narrower ranges reflect higher confidence and therefore higher precision.[23] Signal precision, β, is proxied by the inverse of the historical forecast noise of the professional forecaster, measured as the inverse of the mean absolute forecast error (MAE) over the preceding ten periods – a simple, transparent proxy that aligns with how forecast credibility is typically assessed in practice, even though it differs from the inverse variance assumed in a strict Bayesian framework. The identifying assumption is that participants interpret historical forecast errors as signal noise – or at least apply a consistent rule for inferring signal precision from the provided data across treatments. Signal precision is held fixed within each scenario (see Table A11 for values).

I estimate Eq. (1) by OLS without an intercept. If participants behave as the Bayesian benchmark, both coefficients equal one ($\gamma = \delta = 1$). Empirically, I find $\gamma = 1.21$ (priors over-weighted) and $\delta = 0.90$ (signals under-used). In other words, participants place roughly 10 percentage points less weight on signals than the Bayesian benchmark predicts. This pattern is consistent with experimental evidence of under-inference (Benjamin, 2019) and survey evidence of under-reaction to macroeconomic news (Coibion and Gorodnichenko, 2012). The signal under-use holds across all nationalities (see Figure 4 and Appendix Table A14) and underscores the importance of the central question: can certain messengers increase signal use?

[22] The Bayesian benchmark assumes normally distributed inflation and i.i.d. signals. In reality, EA inflation is non-normal and forecasts show mild autocorrelation, so this benchmark is imperfect and deviations from it should not be interpreted as proof of non-Bayesian behavior. However, what matters is relative updating across messenger treatments, which are equally affected by these data features.

[23] Zero-width ranges are excluded from the main analyses since these result in infinite precision. However, results are robust when setting a small lower bound ϵ to avoid infinite precision, retaining all observations.

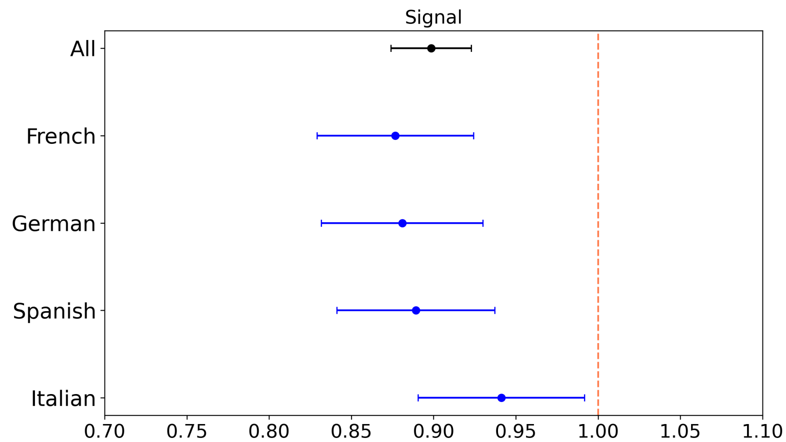


Figure 4: Signal Use Across Treatments by Nationality

Notes: Estimated signal-use coefficients across all messenger treatments, by participant nationality. The benchmark Bayesian weight on the signal is 1 (vertical dashed line). The full sample is shown in black; nationality-specific estimates in blue. Error bars denote 95% confidence intervals.

To test how different messengers affect the use of signals, I extend Eq. (1) by interacting signals with messenger treatment dummies

$$Posterior_{ij} = \gamma \left(\frac{\alpha_{ij}}{\alpha_{ij} + \beta_j} A_{ij} \right) + \sum_{j=1}^{J} \delta_j T_j \left(\frac{\beta_j}{\alpha_{ij} + \beta_j} B_j \right) + \epsilon_{ij}, \tag{2}$$

where T_j is an indicator for messenger treatment $j \in J = \{1, \ldots, 6\}$. Each δ_j captures responsiveness to the signal under treatment j, allowing direct tests of the hypotheses.

3.4 Causal Effects of Messenger Identity on Inflation Expectations

Estimating Eq. (2) isolates the causal effect of messenger identity by comparing information use across treatments that differ only in identity, with inflation scenarios (including each messenger's forecast performance) and task order randomized.[24] I test four hypotheses corresponding to distinct dimensions of messenger identity: nationality, institutional context, the behavioral mechanism (homophily vs. heterophobia), and institutional affiliation. Table 2 reports the estimates for all four hypotheses, where the most conservative specification

[24] Given the relatively small sample size, randomization of task order and inflation scenarios is slightly imbalanced. I therefore include controls for task order and inflation scenario. Because these controls absorb scenario-level variation, I focus on *relative* signal uptake across treatments; absolute Bayesian coefficients are not directly interpretable in this specification.

(column (3)) provides the main effects.[25]

Table 2: Main Effects from Experiment (H1, H2, H3, H4)

	(1)	(2)	(3)
Pure Ingroup Effect (H1):			
Ingroup Expert - Outgroup Expert	0.064**	0.047***	0.052***
	(0.029)	(0.017)	(0.017)
R^2	0.960	0.986	0.994
Observations	795	795	795
Ingroup Effect within Institutional Context (H2):			
ECB Ingroup Expert - ECB Outgroup Expert	-0.014	0.022	0.028*
	(0.029)	(0.018)	(0.017)
R^2	0.960	0.985	0.994
Observations	795	795	795
Homophily (H3):			
Ingroup ECB Expert - Neutral ECB Expert	0.006	0.034*	0.035**
	(0.029)	(0.018)	(0.018)
Observations	794	794	794
R^2	0.961	0.985	0.993
Heterophobia (H3):			
Outgroup ECB Expert - Neutral ECB Expert	0.020	0.014	0.013
	(0.027)	(0.017)	(0.017)
Observations	795	795	795
R^2	0.965	0.986	0.994
Institutional Effect (H4):			
Neutral NCB Expert - Neutral ECB Expert	0.027	0.031*	0.034**
	(0.028)	(0.017)	(0.017)
Observations	795	795	795
R^2	0.965	0.987	0.994
Inflation Scenario		✓	✓
Individual-FE			✓

Notes: Effects compare coefficients from the same OLS regressions. Observations are for forecasting tasks and vary slightly across treatments due to instances of infinite prior precision. *Inflation Scenario* indicates specifications that control for the underlying data of forecasting tasks and messenger order. Column (3) reports the main specification. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Hypothesis 1: Pure Ingroup Effect. *Signals from nationality-matched ingroup experts are used more than signals from outgroup experts.*

I test this using the *Nationality-Only Expert* treatments (1–4), where experts differ only by nationality. This makes nationality salient while holding all other features constant, allowing a clean identification of the pure ingroup effect.

[25] Specifically, these estimates control for inflation scenario, task order, and individual fixed effects.

I find strong support for Hypothesis 1. Participants update significantly more toward ingroup experts, with an estimated effect of 0.052 (p-value < 0.01). While signals from both in- and outgroup experts are under-used relative to the Bayesian benchmark, ingroup signals are used about five percentage points more. This closes roughly half of the sample-average gap to the Bayesian benchmark shown in Section 3.3, making this ingroup effect sizable.

Hypothesis 2: Institutional Context. *Ingroup effects persist within the institutional context of the ECB.*

I test this using the *ECB Expert* treatments (5–8), where messengers are ECB-affiliated experts with nationalities attached. This design extends the pure ingroup effect by adding an institutional dimension while keeping nationality variation constant.

I find support for Hypothesis 2. Participants update significantly more toward ingroup than outgroup ECB experts, but the effect size shrinks to 0.028 (p-value < 0.1) – about a quarter of the average gap to the Bayesian benchmark and roughly half as large as the pure ingroup effect in Hypothesis 1. This suggests that nationality still matters within the ECB, though institutional context dampens its influence.

One interpretation is that institutional reputation reduces the relevance of individual messenger identity. Another possibility is that attaching both the ECB and nationality makes it easier for participants to associate expert treatments with real policymakers, potentially distorting perceived signal precision based on prior knowledge (Esponda et al., 2023). Likewise, other attributes of such associated policymakers may trigger multiple ingroup effects simultaneously (Stolper and Walter, 2019), weakening the identification of nationality-based effects. These considerations help explain why D'Acunto et al. (2022), who vary real policymakers within the same institution, find limited effects on inflation expectations.

Most importantly, however, nationality-based ingroup effects *persist* even within the institutional context. Consistent with this pattern, data from the ECB's cross-country Consumer Expectations Survey (CES) show that during the five years in which the ECB president was French, French households' inflation expectations were most closely aligned with ECB forecasts (see Appendix B.5 for details).

Hypothesis 3: Homophily vs. Heterophobia. *Ingroup effects reflect over-use of ingroup signals (homophily), not under-use of outgroup signals (heterophobia).*

Ingroup effects within institutions can arise in two ways: through a relative over-use of information from ingroup messengers (*homophily*), a relative under-use of information from outgroup messengers (*heterophobia*), or a combination of both.

To distinguish between homophily and heterophobia, I compare ECB ingroup and outgroup experts to a neutral ECB expert without a specified nationality (treatments 5–9). Stronger updating toward ingroup ECB experts relative to the neutral expert indicates homophily, whereas weaker updating toward outgroup ECB experts relative to the neutral expert indicates heterophobia.

I find clear evidence for homophily but not heterophobia. Participants update significantly more toward ingroup ECB experts than the neutral ECB expert (0.035 (p-value < 0.05)), but they do not update significantly less to outgroup ECB experts (0.013). Thus, ingroup effects in the ECB context are driven by homophily – a preference for similar messengers – rather than discrimination against outgroup messengers.

Hypothesis 4: NCB vs. ECB. *Signals from national central banks are used more than those from the ECB.*

Finally, having established nationality-based ingroup effects, I now examine whether these dynamics extend to the institutional level.

To test this hypothesis, I compare signals from national and supranational institutions using the *Institutional Expert* treatments (9–10), in which experts are presented either as the participant's NCB or as the ECB, without nationalities attached. This design isolates the effect of institutional affiliation and tests whether ingroup dynamics persist across institutional contexts.

I find support for Hypothesis 4. Participants update significantly more toward NCB signals than ECB signals (0.034 (p-value < 0.05)), closing roughly a third of the Bayesian gap. The effect is smaller than the pure ingroup effect but larger than the ECB ingroup effect. This highlights the potential effectiveness of delegating communication to NCBs, but it abstracts from the risks of cacophony of voices (Blinder, 2007) and the importance of aligned communication (Tutino, 2016; Do Hwang et al., 2021). Overall, the benefits of delegating to NCBs should be viewed as an upper bound, achievable only if messages remain identical across institutions.

3.5 Mechanisms

Having established the causal ingroup effects, a natural next step is to identify what drives them. Discrimination between messengers may arise subconsciously, through implicit biases, or consciously, reflecting subjective perceptions of messenger quality or institutional trust. I study two such channels – perceived ability and trust – and find that both account for a significant portion of the observed ingroup effects.[26]

Differences in information use may reflect perceived signal quality beyond the signal precision provided in the experiment via the historical forecast performance. Participants may assign higher precision to ingroup messengers' signals because they perceive them as more able. To test this, I interact messenger-dependent signal use with self-reported perceived ability (a 7-point Likert scale rating of each messenger's forecasting and analytical skill). Results show that roughly 80% of the pure ingroup effect is explained by perceived ability (see columns (1) vs. (2) in Table 3). Taking a conservative view, about one-fifth of the ingroup effect (0.010 (p-value < 0.05)) remains beyond perceived quality.[27] Similar reductions occur across all hypotheses: the ECB homophily effect becomes more statistically significant (at 5% rather than 10%) but falls to one-third of its size, while the institutional effect (NCB vs. ECB) shrinks to slightly less than a third but remains significant. Thus, perceived ability explains a decisive amount of the increased signal uptake, although a residual ingroup effect persists.

Trust in real-world institutions and policymakers provides a second channel. Interacting signal use with ex-post reported trust of institutions and representative policymakers shows that for ECB experts, trust explains more than half of the homophily effect but does not eliminate it (see columns (3) vs. (4) in Table 3).[28] By contrast, for institutional experts, the higher signal use for NCBs relative to the ECB disappears once trust is accounted for. Trust renders the institutional ingroup effect statistically indistinguishable from zero, whereas for ECB messengers nationality-based homophily persists – albeit substantially reduced – even after accounting for trust.

[26] One caveat is that perceived ability and trust are self-reported after the experiment, and may therefore partly reflect ex-post rationalization of prior choices. As such, the following findings should not be interpreted causally, and effect sizes should be viewed with caution.

[27] This estimate assumes participants fully incorporate their stated perceived ability on top of the forecast history, making it a lower bound.

[28] Eliciting trust for messenger treatments only makes sense if participants associated expert treatments with actual policymakers. Therefore, trust data was asked for only for experts with institutional affiliation. Furthermore, because trust data are only elicited conditional on participants knowing the representative policymakers or institutions, the sample size drops considerably. In this restricted sample, the ECB ingroup effect (H2) is insignificant. Hence the comparison here focuses on the homophily effect (H3), where sufficient observations remain.

Table 3: Mechanisms: Perceived Quality and Trust

	(1)	(2)	(3)	(4)
Pure Ingroup Effect (H1):				
Ingroup Expert - Outgroup Expert	0.052***	0.010**		
	(0.017)	(0.005)		
Observations	795	795		
R^2	0.994	0.992		
Ingroup Effect within Institutional Context (H2):				
ECB Ingroup Expert - ECB Outgroup Expert	0.028*	0.005	0.053	0.006
	(0.017)	(0.004)	(0.044)	(0.015)
Observations	795	795	315	315
R^2	0.994	0.993	0.998	0.997
Homophily (H3):				
Ingroup ECB Expert - Neutral ECB Expert	0.035**	0.009**	0.051**	0.021***
	(0.018)	(0.004)	(0.022)	(0.007)
Observations	794	794	612	612
R^2	0.993	0.992	0.996	0.995
Heterophobia (H3):				
Outgroup ECB Expert - Neutral ECB Expert	0.013	0.008**	-0.004	0.012
	(0.017)	(0.004)	(0.033)	(0.011)
Observations	795	795	477	477
R^2	0.994	0.993	0.998	0.998
Institutional Effect (H4):				
Neutral NCB Expert - Neutral ECB Expert	0.034**	0.008**	0.033*	0.005
	(0.017)	(0.004)	(0.017)	(0.004)
Observations	795	795	779	779
R^2	0.994	0.994	0.994	0.993
Inflation Scenario	✓	✓	✓	✓
Individual-FE	✓	✓	✓	✓
Perceived Messenger Ability		✓		
Trust				✓
Knowing PMs/Inst			Yes	Yes

Notes: Effects compare coefficients from the same OLS regressions. Specifications control for inflation scenario and task order and include individual fixed effects, as in the main specification. *Perceived Messenger Ability* denotes the interaction with signal use. *Knowing PMs/Inst* indicates whether the participant has heard of the relevant institutions or policymakers; *Trust*, elicited only conditional on knowing them, captures its interaction with signal use. Observations are for forecasting tasks and vary slightly across treatments due to instances of infinite prior precision or unfamiliarity with PMs/Inst. Column (1) reports the main results. Column (2) adds the interaction with perceived ability. Column (3) repeats the baseline on the restricted sample where policymakers and institutions are known, ensuring comparability with column (4), which adds the interaction with trust. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

3.6 Messengers Impact Reach but Not Attention to Information

Messenger effects may operate not only through belief updating but also through reach. Does the greater reporting on ingroup messengers and their communications documented in Section 2 translate into better reach?

Post-experimental survey responses suggest that it does: participants are significantly better informed about ECB policymakers from their ingroup than from their outgroup. Specifically, the probability of knowing these policymakers and receiving news about them is higher by 27% (p-value < 0.01) and 29% (p-value < 0.01), respectively (see Table A17 and Appendix B.4 for more details). Thus, policymakers reach ingroup audiences more effectively. This raises the question whether better reach reflects greater attention to ingroup messengers or simply greater information availability. To test this, I directly measure attention by counting the number of "Read More" buttons clicked to access additional qualitative information.

On average, participants click 1.8 out of 3 buttons, and this measure does not vary significantly across messenger treatments (see Figure 5), indicating that messengers do not have a direct causal effect on attention. Since quantitative forecasts from ingroup messengers are used more without requests for additional qualitative information, this suggests that ingroup dynamics do not generate a greater demand to learn more about the forecast (e.g., as might be encouraged through central bank communication). Instead, information from ingroup messengers appears to be accepted at face value.

This further suggests that better reach arises from the increased information availability (Section 2), not from heightened attention. A natural concern, however, is whether button clicks truly capture attention. To validate this measure, I exploit the randomized inflation scenarios – implemented to prevent inflation data from biasing messenger effects – to test whether attention varies with the economic environment.

While messengers do not affect attention, inflation levels and uncertainty do so strongly and causally (see Figure 5). A one percentage point increase in average historical inflation raises the probability of requesting any information by 32.6 percentage points and increases clicks by almost one (0.921 (p-value < 0.01)), shown in Table A15. Similarly, a one-point increase in the standard deviation of historical inflation over the ten observed periods (i.e., higher inflation uncertainty) raises the probability of requesting any information by 24.3 percentage points and increases clicks by 0.701 (p-value < 0.01). These findings align with Cavallo et al. (2017), who document weaker priors in low-inflation contexts, and Weber et al. (2025), who show that attention is endogenous to inflation levels. My within-subject experimental design contributes to this literature by causally showing the strong reaction of attention to the uncertainty of inflation. Thus, attention is endogenous to the inflationary

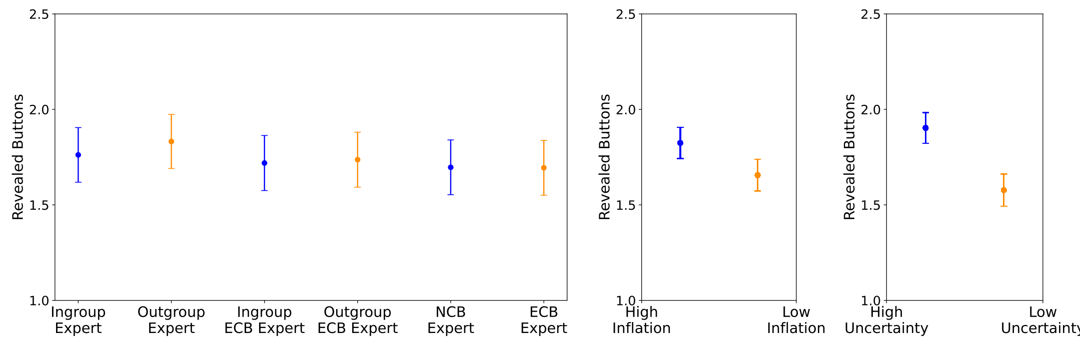


Figure 5: Requests For Additional Information Pieces

Notes: Average number of clicks on buttons by messenger treatment (left panel), by inflation level (middle panel) and by inflation uncertainty (right panel) with 95% confidence intervals. Of the six inflation scenarios, the three scenarios with the highest (lowest) inflation shown as "High (Low) Inflation." Similarly, the three with the highest (lowest) standard deviation of inflation history are shown as "High (Low) Uncertainty."

environment.[29]

A related question may be whether being attentive in the experiment (clicking on buttons) affects measured messenger effects. Ingroup effects emerge primarily among attentive participants, and this is the case across hypotheses, while inattentive participants (no clicks) show none (see column (2) vs. (4) in Table A16). Effects further intensify with the number of buttons clicked. Attention thus moderates treatment effects. Since attention is not randomized, these findings are not strictly causal, but they highlight that treatment effects require participants to actively engage with information. This underscores the importance of treatment compliance in information experiments, as emphasized by Knotek et al. (2024).

3.7 Discussion and Policy Implications

The experimental findings demonstrate positive ingroup effects on information processing, which somewhat diminish within the ECB context. One might wonder how the experimental evidence aligns with real-world behavior on Twitter and what it implies for policy.

Policymakers have many characteristics beyond nationality, which complicates direct comparison with the clean identification in the experiment. A key factor behind the stronger ingroup patterns observed on Twitter seems to be the increased information availability (i.e., news supply), as shown in Section 2.3. Ingroup policymakers receive more media coverage than their outgroup counterparts, amplifying the reach of their communication (Section 3.6).

In sum, shared nationality raises both availability and use of policy communication,

[29] Endogenous attention reflects the costliness of being mistaken about inflation. Note that in the experiment, where payoffs depend only on forecast accuracy, expected forecast errors should not depend on the level of inflation, only its uncertainty.

making messages more effective for ingroup audiences. To benefit from this, the reduced-form evidence makes two concrete suggestions: delegating communication to diverse board members, or delegating communication through the Eurosystem's national central banks.

Motivated by these facts, Section 4 develops a stylized coordination model to evaluate optimal communication strategies under messenger effects, providing welfare-based guidance.

4 Modeling Optimal Communication

In light of the empirical messenger effects, how should policymakers select messengers to communicate with the public? To derive welfare-maximizing communication policies, I develop a model with strategic complementarity. I extend the 'beauty contest' by Morris and Shin (2002) to incorporate messenger identity: agents receive private and public signals, allowing me to capture the dual role of public information disclosure: to inform about an economic fundamental and to coordinate agents with others.[30] Based on whether agents share characteristics with the messenger, they are classified as either in- or outgroup agents, which affects their availability and processing of the public signal.

I study two communication policies: (i) a *disclosure policy* that sets the precision of the public signal, and (ii) a *delegation policy* that selects the messenger(s) and thereby changes the composition of in- and outgroup agents, while keeping a single public signal constant.

I find that *strategic* delegating communication is socially desirable: delegation raises social welfare when public information is precise or under weak coordination motives, while centralization of communication preserves welfare, when strategic complementarities are strong and public information is insufficiently precise. This is due to outgroup agents' lower availability and use of public information, which can mitigate potential welfare losses. Centralization can be optimal when disclosure is either welfare-harming or -improving, making delegation a distinct policy tool that complements disclosure.

The model is kept general to apply to any policy communication with heterogeneous messengers and audiences, including monetary policy as well as climate, health, fiscal, education, etc. While nationality in the EA is the motivating example, the model extends to other dimension of heterogeneity, such as age, gender, ethnicity, socio-economic background, or economic expertise.

[30] Examples of this coordination motive for inflation are firms setting prices considering their competitors' prices, or agents negotiating wages similar to others.

4.1 The Model Set-Up

A continuum of agents $i \in [0,1]$ choose actions with a 'beauty-contest' coordination motive, caring about aligning actions with others' actions in addition to an unknown fundamental. Agents come in two *types* $h \in \{g, o\}$: ingroup (g) if their characteristics match the messenger ($\theta_i = \theta_m$), and outgroup (o) otherwise. These types differ in access to, and processing of, the public signal.

4.1.1 Information Structure

The fundamental $x \sim \mathcal{N}(\mu, \tau_x^{-1})$ represents an unobserved variable, such as future inflation. Each agent observes private and public signals.

$$\text{Private signal:} \quad y_i = x + \epsilon_{iy}, \quad \epsilon_{iy} \sim \mathcal{N}(0, \tau_y^{-1}) \tag{3}$$

$$\text{Public signal:} \quad Y = x + \epsilon_Y = x + \epsilon_z + \epsilon_V, \quad \epsilon_z \sim \mathcal{N}(0, \tau_z^{-1}), \quad \epsilon_V \sim \mathcal{N}(0, \tau_V^{-1}) \tag{4}$$

Policymakers' own information about the fundamental ($z = x + \epsilon_z$) has a fixed noise component ϵ_z. As part of a *disclosure policy*, they decide how much of this information to disclose by controlling the additional noise term ϵ_V, affecting the overall precision of the public signal τ_Y beyond τ_z. Full disclosure corresponds to $\tau_V \to \infty$, while complete opacity is $\tau_V \to 0$, and partial disclosure is any interim case, $\tau_V \in \mathbb{R}^+$. Accounting for both disclosure and noise in the public information, the observed public signal can be written as $Y \sim \mathcal{N}(x, \tau_Y^{-1})$, where $\tau_Y^{-1} = \tau_V^{-1} + \tau_z^{-1}$.[31]

The second decision the policymakers make is on their *delegation policy*. Choosing the messenger(s) of communication sets the share of ingroup agents in the economy, denoted α, while not impacting the underlying public signal Y or its precision τ_Y.[32]

Public Signal Availability. The public signal is not strictly common knowledge.[33] Ingroup agents always receive Y. Outgroup agents receive Y only if its magnitude exceeds their idiosyncratic threshold d_o, where $d_o \sim \mathcal{N}_+(0,1)$ (truncated normal on $[0, \infty)$). This parsimoniously captures newsworthiness thresholds in media coverage (Nimark and Pitschner, 2019). Intuitively, larger signals are more likely to be reported, increasing the probability that outgroup agents receive them. The fraction of informed outgroup agents observing Y is

[31] Note that ϵ_V and ϵ_z are independent of each other, x, and of ϵ_{iy}.

[32] Perceived expertise may vary with the messenger. I study this a caveat in Appendix C.4, making τ_Y (weakly) decreasing in α.

[33] This departs from standard coordination models in the literature. An exception is Cornand and Heinemann (2008), who restrict the availability of public signals but do not allow it to vary by agent type or signal size.

$$A = 2\Phi(|Y|) - 1, \tag{5}$$

where Φ is the standard normal cumulative distribution function. The fraction of outgroup agents who do not observe the signal is $1 - A$. As $|Y|$ becomes very large, A approaches 1, meaning nearly all outgroup agents observe large signals, whether positive or negative.

Belief Updating. Agents share a diffuse normal prior. Private signals are processed in a Bayesian way. Ingroup agents also process the public signal like Bayesians. Informed outgroup agents apply *information resonance* (Malmendier and Veldkamp, 2022), meaning they downweight public signals based on their noise and an additional *resonance weight* ρ_{im}, resulting in a total weight of ω_{im} (the *relevance weight*) applied to the public signal.

$$\omega_{im} \equiv \rho_{im}\tau_Y, \quad \rho_{im} \in (0,1], \quad \rho_{im} = 2 - 2\Phi\big(\chi \, \|\theta_i - \theta_m\|\big), \tag{6}$$

where ρ_{im} defines the resonance that a signal coming from messenger m has for receiving agent i, which is strictly decreasing in $\|\theta_i, \theta_m\|$, the Euclidean distance in characteristics, and depends on $\chi > 0$, the sensitivity to mismatch.[34] The normal cumulative distribution function Φ equals 0.5 at 0, and hence the expression $2 - 2\Phi(\cdot)$ takes on a value of 1 at zero and then declines quickly towards zero.

Information sets (left) and posteriors (right) are

$$\Omega_i = \begin{cases} \{y_i, Y\}, & h = g, \\ \{y_i, Y\}, & h = o \text{ and } |Y| \geq d_o, \\ \{y_i\}, & h = o \text{ and } |Y| < d_o, \end{cases} \quad E_{ih}[x \mid \Omega_i] = \begin{cases} \dfrac{\tau_y y_i + \tau_Y Y}{\tau_y + \tau_Y}, & h = g, \\ \dfrac{\tau_y y_i + \rho_{im}\tau_Y Y}{\tau_y + \rho_{im}\tau_Y}, & h = o, \ |Y| \geq d_o, \\ y_i, & h = o, \ |Y| < d_o. \end{cases} \tag{7}$$

The shares of the population in the ingroup (α), outgroup ($1-\alpha$), and informed outgroup (A) are common knowledge. However, agents assume homogeneous information processing among informed agents, consistent with the assumption of unawareness of biases.[35]

[34] If $\chi = 0$: agents learn from everyone without discounting anyone's information $\rho_{im} = 1$. If χ is very large: agents disregard any information from messengers that do not exactly match their characteristics $\rho_{im} < 1$. $0 < \chi < 1$ represents cases in-between.

[35] Assuming a lack of awareness of biases aligns with the inherent definition of bias and is consistent with its usage in the literature (e.g., Angeletos and Huo 2021, Gust et al. 2025.

4.1.2 Payoffs

Agents $i \in [0,1]$ choose action $a_i \in \mathbb{R}$ to maximize their payoff $u_i \in \mathbb{R}$. Such an action could reflect decisions about consumption, savings or investments. Their payoff depends on their own action, strategic complementarities, and the state of the exogenous fundamental $x \in \mathbb{R}$

$$u_i = -(1-r)(a_i - x)^2 - r(a_i - \bar{a})^2, \quad \bar{a} = \int_0^1 a_i \, di, \quad r \in [0,1), \tag{8}$$

where $-(a_i - x)^2$ (the quadratic loss between the agent's action a_i and the exogenous fundamental state x) reflects the agent's desire to align their action with x. $-(a_i - \bar{a})^2$ (the deviation of agent i's action from the actions of all other agents a_j) captures the "beauty contest" term: agents care about coordinating their actions with the average action of others. r gives the weight on the coordinating, second-guessing motive.

4.1.3 Social Welfare

Social welfare is defined as the (normalized) average of individual utilities

$$W(a,x) \equiv \frac{1}{1-r} \int_0^1 u_i(a,x) \, di = -\int_0^1 (a_i - x)^2 \, di. \tag{9}$$

The social planner cares only about mean-squared distance to the fundamental since the coordination motive introduces a social inefficiency, where agents' desire to align with others distorts individual actions without improving outcomes based on the true fundamental state.

4.1.4 Agents' Actions

Optimal actions satisfy

$$a_{ih} = (1-r)E[x|\Omega_i] + rE[\bar{a}|\Omega_i]. \tag{10}$$

Agents do not observe others' actions. Instead, they form expectations about $\bar{a}$ from beliefs about others' posteriors (i.e., $E_{ih}[y_j|\Omega_i] = E_{ih}[x|\Omega_i]$, with the latter given in Eq. (7).

4.1.5 Timeline

The model consists of two stages. First, policymakers set disclosure τ_V (hence τ_Y) and delegation (the messenger(s), hence α), aiming to maximize expected welfare. Second, agents observe signals, form posteriors, and choose actions, aiming to maximize expected utility using their information sets.

4.2 Equilibrium

In equilibrium, neither policymakers nor agents want to deviate from their strategies.

4.2.1 Derivation of Equilibrium Actions

Suppose that all agents of type h use a linear strategy

$$a_{ih} = \kappa_h y_i + (1 - \kappa_h)Y. \tag{11}$$

In equilibrium, all informed agents of type h choose the same optimal weight κ_h, depending on their expectations about the others' behavior. Recall that all agents know α and the realized A but believe that other agents process public signals as they do, unaware of biases.

Lemma 1. *The optimal signal extraction weights for informed in- and outgroup agents are*

$$\kappa_g^* = \frac{\tau_y q}{\tau_Y + \tau_y q} \quad \text{and} \quad \kappa_o^* = \frac{\tau_y q}{\rho_{im}\tau_Y + \tau_y q}, \tag{12}$$

where $q \equiv 1 - r + r(1 - \alpha)(1 - A)$. *Proof in Appendix C.2.1.*

Equilibrium weights differ only in the perceived precision of Y: ingroup agents use τ_Y, informed outgroup agents use $\rho_{im}\tau_Y$. The term q captures the coordination pull toward private information, heightened compared to Morris and Shin (2002) by the mass $(1-\alpha)(1-A)$ of uninformed outgroup agents who rely solely on y_i. Equilibrium actions of informed agents are

$$a_{ig}(y_i, Y) = \frac{\tau_Y Y + \tau_y q y_i}{\tau_Y + \tau_y q} \quad \text{and} \quad a_{io}(y_i, Y) = \frac{\rho_{im}\tau_Y Y + \tau_y q y_i}{\rho_{im}\tau_Y + \tau_y q}. \tag{13}$$

Uninformed outgroup agents rely only on private information and choose

$$a_{io}(y_i) = y_i. \tag{14}$$

4.2.2 Average Action

The average action in equilibrium is

$$\begin{aligned} \bar{a} &= \alpha a_{ig} + (1-\alpha)A a_{io} + (1-\alpha)(1-A)a_{io}(y_i) \\ &= x\left[\alpha\kappa_g^* + (1-\alpha)A\kappa_o^* + (1-\alpha)(1-A)\right] + Y\left[\alpha(1-\kappa_g^*) + (1-\alpha)A(1-\kappa_o^*)\right]. \end{aligned} \tag{15}$$

4.2.3 Uniqueness of the Linear Equilibrium

Proposition 1. *The equilibrium strategies derived constitute the unique linear equilibrium of the model. Proof in Appendix C.2.2.*

The model nests Morris and Shin (2002) in two cases (i) $\alpha = 1$, so that all agents are ingroup (homogeneous), and (ii) $|Y| \to \infty$ with $\chi = 0$, so that all agents are informed ($A \to 1$) and update without bias ($\rho_{im} = 1$).

4.3 Welfare Implications and Policy Analysis

To evaluate the welfare effects of communication policies, I conduct partial comparative statics on expected social welfare. I condition on realized signal magnitude $|Y|$ and hold the fraction of informed outgroup agents A fixed, while varying $\tau_Y,\ \tau_y,\ r,\ \rho$, and α.

4.3.1 Social Welfare Computation

Having derived the equilibrium in Section 4.2, expected social welfare is given by Eq. 16.

$$
\begin{aligned}
E[W(a,x)|x] &= -E\left[\int_0^1 (a_i - x)^2\, di\right] \\
&= -\alpha \int_0^1 E\left[\left(a_{ig}(y_i, Y) - x\right)^2\right] di - (1-\alpha)A \int_0^1 E\left[\left(a_{io}(y_i, Y) - x\right)^2\right] di \\
&\quad - (1-\alpha)(1-A) \int_0^1 E\left[\left(a_{io}(y_i) - x\right)^2\right] di \\
&= -\alpha\left[\kappa_g^2 E({\epsilon_{iy}}^2) + (1-\kappa_g)^2 E(\epsilon_Y^2)\right] \\
&\quad - (1-\alpha)A\left[\kappa_o^2 E({\epsilon_{iy}}^2) + (1-\kappa_o)^2 E(\epsilon_Y^2)\right] - (1-\alpha)(1-A)E({\epsilon_{iy}}^2) \\
&= -\alpha \frac{\tau_Y + \tau_y q^2}{\left[\tau_Y + \tau_y q\right]^2} - (1-\alpha)A \frac{\rho_{im}^2 \tau_Y + \tau_y q^2}{\left[\rho_{im}\tau_Y + \tau_y q\right]^2} - \frac{(1-\alpha)(1-A)}{\tau_y}.
\end{aligned}
\tag{16}
$$

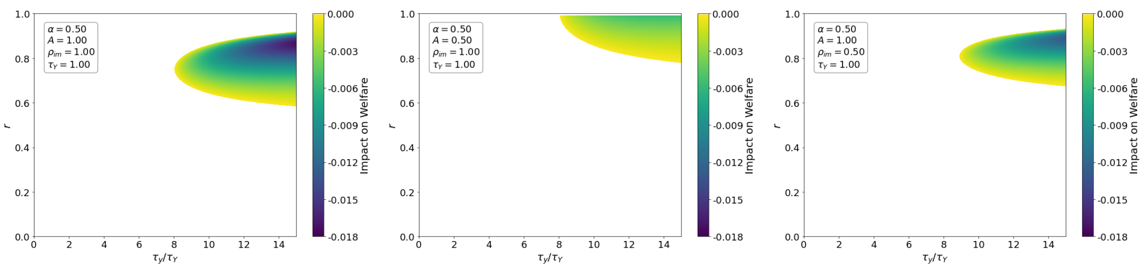
How do disclosure and delegation policies – through τ_Y and α – affect social welfare?

4.3.2 Public Signal Precision – The Disclosure Policy

To start with the traditional disclosure policy in the presence of in- and outgroup agents, recall that disclosure raises τ_Y. Under full disclosure ($\tau_V \to \infty$), the disclosed public signal Y is as precise as its underlying information τ_z. Conversely, under complete opacity ($\tau_Y = 0$ since $\tau_V \to 0$), Y is pure noise without meaningful information.

Proposition 2. *Increasing τ_Y raises welfare only if public information is sufficiently precise relative to private information and r is not too high. Derivation in Appendix C.2.3.*

[Morris and Shin (2002)] show that disclosure can reduce welfare when agents over-coordinate on a noisy public signal, because a coordination motive induces them to neglect more precise private information. Figure 6 reproduces this benchmark in panel (a) and shows how limited signal availability (in (b)) and updating bias (in (c)) each shrink the set of $(r, \tau_y/\tau_Y)$ for which additional disclosure is harmful. Thus, outgroups can buffer welfare losses from disclosure.[36]



(a) Benchmark (b) Restricted Signal Availability (c) Updating Bias

Figure 6: Disclosure's Effect on Social Welfare

Notes: Panels depict the welfare implications of increasing public signal precision ($\frac{\partial E(W|x)}{\partial \tau_Y}$) for the two messenger effects. White: non-negative marginal effects, color: negative. Panel (a): all are informed, no bias ($A = 1, \rho_{im} = 1$, replicating Morris and Shin (2002)). Panel (b): a fixed fraction of outgroup are informed ($A = 0.5$), no bias. Panel (c): all are informed but outgroup strongly under-weight public information ($\rho_{im} = 0.5$). In all panels $\tau_Y = 1$ for scaling; the vertical axis reports r, and the horizontal axis $\frac{\tau_y}{\tau_Y}$.

Corollary 1. *The presence of outgroup agents can mitigate potential welfare losses from increased disclosure when public information is noisy.*

This raises the question of the optimal composition of in- and outgroup agents.

4.3.3 In- and Outgroup Composition – The Delegation Policy

Public policy communication can be delegated to messengers to repeat the same public signal and whose characteristics raise α. Reduced-form evidence in this paper suggests two concrete implementations for central bank communication: (i) using other board members, or (ii) using NCB governors in the Eurosystem. What, then, determines the optimal share α^*?

Proposition 3. *An optimal share of ingroup agents (α^*) exists and depends on $r, \tau_y, \tau_Y, A, \rho_{im}$. Derivation in Appendix C.3.1.*

[36] For completeness, Appendix C.2.4 analyzes private signal precision. Heterogeneity in processing biases implies that increasing private signal precision can reduce welfare under strong coordination and sizable bias.

An explicit closed form for α^* is not feasible because of the updating bias ($\rho_{im} < 1$).[37] Numerical methods show that α^* is jointly shaped by four factors: (i) the public–private precision ratio (τ_Y/τ_y), (ii) the strength of coordination r, (iii) the degree of updating bias ρ_{im}, and (iv) realized signal size $|Y|$ (availability A). Delegation (high α^*) is optimal when the public signal is sufficiently precise; centralization (lower α^*) is more likely optimal under strong coordination, noisy public information – especially when many outgroup agents are informed and biased. Appendix C.3 details these mechanisms and provides numerical visualization across coordination r and public signal precision τ_Y, holding $\tau_y = 1$ (see Figure A34).

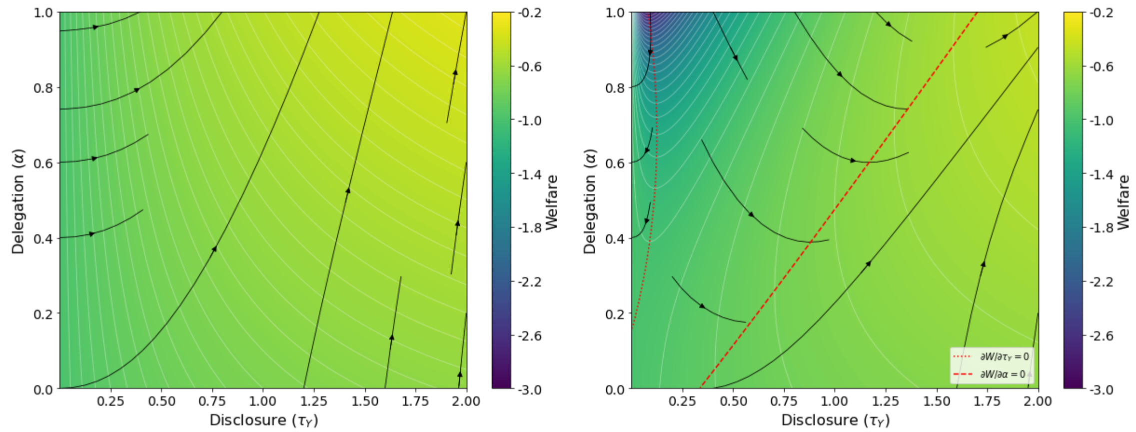
A Caveat: Expertise Loss under Delegation. Delegating away from the (presumably) most expert messenger may lower public signal precision. If τ_Y declines with α at rate $k \equiv -\frac{d\tau_Y}{d\alpha} > 0$, then delegation is locally welfare-improving only if $\frac{\partial E[W|x]}{\partial \alpha} > k(\alpha)\frac{\partial E[W|x]}{\partial \tau_Y}$. Two implications follow: *(i)* When $\frac{\partial E[W|x]}{\partial \tau_Y} > 0$ (disclosure is beneficial), delegation must overcome the positive marginal cost of expertise loss $k(\alpha)\frac{\partial E[W|x]}{\partial \tau_Y}$. *(ii)* When $\frac{\partial E[W|x]}{\partial \tau_Y} < 0$ (disclosure is harmful), expertise loss helps of reducing over-coordination – yet, so does centralized communication (Section 4.4). The trade-off is tighter under strong coordination and with noisy public signals (see Appendix C.4).

4.4 Policy Implications: When to Delegate?

Since both disclosure and delegation have been shown to affect welfare, how should optimal policy communication balance delegation and disclosure policies to maximize the social value of information? Figure 7 plots welfare contours in (τ_Y, α) for low and high coordination, fixing $A = 0.5$ (i.e., conditioning on the realized $|Y|$) and a modest under-weighting $\rho_{im} = 0.95$ (consistent with the empirical ingroup effect).

1. *Low coordination: delegate and disclose always.* If $\tau_Y > \tau_y$, the corner $\alpha^* = 1$ is optimal and gains from delegation are large; full disclosure is also optimal. If $\tau_Y < \tau_y$, gains from delegation are smaller but still positive – larger increases in α are needed for noticeable welfare improvements. Intuitively, when public information is relatively noisy, the marginal benefit of shifting agents toward the public signal is lower (see Figure 7a).

[37] With only restricted availability but no updating bias: $\alpha^* = \min\left\{1, \frac{\tau_Y + \tau_y}{3r\,\tau_y}\right\}$. Thus $\alpha^* < 1$ whenever $\frac{\tau_Y}{\tau_y} < 3r - 1$ (requiring $r > 1/3$). $r < \frac{1}{3}$ always results in the corner solution of only ingroup agents ($\alpha^* = 1$), in line with the optimal publicity result of Cornand and Heinemann (2008). Intuitively, when coordination motives are sufficiently strong and the public signal is relatively noisy, keeping some outgroup agents (who miss small signals) can offset over-reaction to the public signal. By contrast, with weak complementarities, broad delegation is optimal, ensuring homogeneous ingroup agents fully use the public signal.



(a) Low Coordination (r=0.10)

(b) High Coordination (r=0.90)

Figure 7: Social Welfare Contours

Notes: Welfare ($E[W|x]$) over disclosure (τ_Y) and delegation (α), conditional on a "moderate" signal ($A = 0.5$). Parameters: $\tau_y = 1$, $\rho_{im} = 0.95$. Shading indicates welfare: lighter = higher (less negative) welfare. Thin white lines are iso-welfare (contour) levels. Black stream arrows show the local welfare-ascent direction. Panel (a): low coordination $r = 0.10$. Panel (b): high coordination $r = 0.90$. In (b): red *dotted* line: $\partial W/\partial \tau_Y = 0$ (disclosure boundary); red *dashed* line: $\partial W/\partial \alpha = 0$ (delegation boundary), where W is expected welfare.

2. *High coordination, noisy public signals: disclose and delegate strategically.* When strategic complementarities are strong, three cases emerge (see Figure 7b):

   (i) *Restrict and centralize*: When public information is noisy, and disclosure would harm welfare, centralizing communication (to reduce α) preserves welfare ($\partial W/\partial \tau_Y < 0$ and $\partial W/\partial \alpha < 0$). Thus, limiting the number or diversity of messengers emerges as an *additional* policy tool, particularly valuable when policymakers cannot limit disclosure (e.g., because of periodic announcements). This reflects the finding discussed in Section 4.3.2, and occurs for parameter combinations left of the red dotted line.

   (ii) *Disclose and centralize*: As the quality of public information improves, disclosure improves welfare, while delegation still hurts ($\partial W/\partial \tau_Y > 0$ and $\partial W/\partial \alpha < 0$). The policymaker should increase disclosure but centralize communication. This identifies delegation not just as an additional policy tool, but also a *distinct* one. This arises because outgroup agents place relatively more weight on private signals (and expect others to do the same), which buffers inefficient coordination on a still-imperfect public signal.[38] This occurs between the two red lines (dotted and dashed).

   (iii) *Disclose and delegate*: When public information is sufficiently precise, disclosure and delegation both raise welfare ($\partial W/\partial \tau_Y > 0$ and $\partial W/\partial \alpha > 0$). This occurs to the right of the red dashed line.

[38] Homogeneous outgroup interpretation of the public signal can further reinforce coordination benefits.

Disclosure and delegation thus jointly determine the social value of public information. Maximizing welfare requires strategic delegation, accounting for the realized signal size, relative precisions, potential expertise losses, and the strength of strategic complementarities.

5 Conclusion

The messenger matters. This paper shows empirically that public communication is more effective both in reach and in influence, when messenger and audience identities align. Based on these facts, a coordination model identifies the selection of messenger(s) as an additional policy tool to optimize public policy communication.

Motivating evidence shows that ingroup policymakers are more salient in reporting, raise information availability, and shift beliefs more strongly towards their message. Controlled experimental evidence then estimates that participants under-use information by about 10 percentage points relative to a Bayesian benchmark, but they place 5.2 percentage points more weight on the same quantitative forecast when it comes from an ingroup messenger of shared nationality. This ingroup effect attenuates yet persists within the ECB context and extends to institutions, with NCB signals used more than ECB signals. Ingroup effects are driven by perceived messenger ability and trust, and are due to homophily rather than heterophobia. While attention does not vary by messenger, it rises with the level and uncertainty of inflation, reflecting that attention is endogenous to the inflationary environment. Thus, messenger effects work through exposure and credibility, not through greater processing effort.

In light of this evidence, a stylized coordination model incorporates these messenger effects on information availability and processing. Assessing optimal communication policy identifies a novel delegation policy alongside the well-studied disclosure policy as welfare-relevant. The analysis yields three regimes under strong strategic complementarities, each prescribing a distinct mix of disclosure and delegation: restrict and centralize, disclose and centralize, and disclose and delegate. While increasing the share of ingroup agents via delegation improves welfare when public information is precise, the presence of outgroup agents can buffer potential welfare losses from disclosing noisy public information, beyond what restricting disclosure alone can achieve. Thus, delegation complements disclosure in maximizing the social value of public information.

Two limitations merit emphasis. First, the experiment purposefully isolates identity along a single dimension, yet policymakers embody many traits such as age, gender, ethnicity, socio-economic background, or expertise. Natural extensions include studying interactions among characteristics and assessing their relative importance. Second, I abstract from "cacophony of voices"; quantifying the costs of the many-voices issue is an interesting next step.

To conclude, the messenger can serve as an additional, powerful policy tool. Strategically selecting who communicates public policy allows the maximization of social welfare, and therefore the optimization of communication. This is true for policy communication with diverse public audiences beyond monetary policy, including climate or fiscal policy.

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Appendix

A Details on Twitter Data and Additional Evidence

A.1 Twitter Data: Descriptive Analysis

Data Cleaning I collected roughly 11 million tweets via the Academic API between November 2022 and March 2023. After retrieval, I drop ECB-unrelated tweets using language-specific filters (e.g., excluding the English Cricket Board), exclude "Draghi"/"Lagarde" mentions without ECB references, lowercase and de-noise (URLs/emojis/handles, except "lagarde"/"ecb"), translate all text to English using Google Translate's Neural Machine Translation to standardize dictionary processing and eradicate any spelling mistakes, and fully spell out contracted words (e.g., "can't" to "cannot" or "we'll" to "we will"). The final dataset has 8,031,937 tweets (Table A4). An audit of frequent words using word clouds confirms the cleanliness of the samples. Figure A8 shows these word clouds indicate that the final sample indeed contains at a decisive majority tweets about central banking. In addition, the figure visualizes how different languages vary in their focus of discussions on Twitter, as discussed in the main part of the paper.

Table A4: Peaks in Tweet Volume by Language

Language	Peak Volume	Peak Date	Event
English	25,624	21 Jul 2022	ECB raising rates for first time in 11 years
German	7,098	05 May 2020	German constitutional court ruling
Spanish	26,599	19 Feb 2018	Eurogroup's support for Luis de Guindos
French	11,905	03 Jul 2019	Announcement of Lagarde as incoming ECB president
Italian	22,050	19 Mar 2020	Day after PEPP announcement

Notes: Daily tweet volume based on CET time zone, corresponding to headquarter of the ECB.

Tweet Volume Daily tweet volumes vary over time and by languages (Figure A9). Patterns in the time series across all samples generally relate to a decision by the ECB or an announcement of new ECB board members, further confirming successful data cleaning. The sample of English tweets stands out, making up almost half of the entire sample (46%). This is unsurprising, as English is the official language of the ECB, and the lingua franca.

Idiosyncrasies of language samples and the validity of the proxy for nationality The daily tweet volume maxima (i.e., peaks) of tweet samples split by language coincides strongly with events of severe national interest (Table A4). This is evidence in favor of the

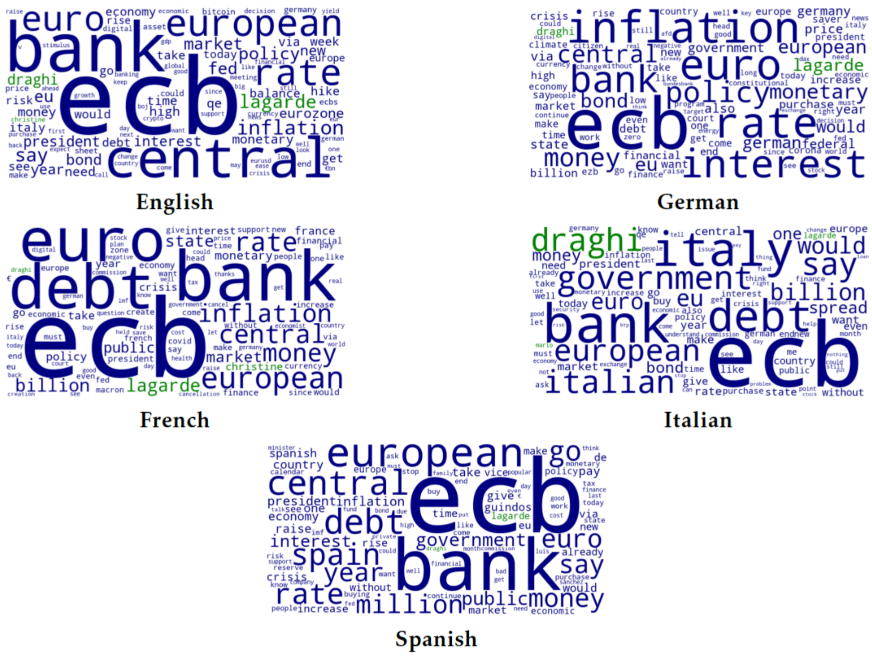


Figure A8: Word Clouds by Language

Notes: The 100 most frequent words between 2016-2022. Samples are split by original tweet language. Word size indicates word frequency. The following terms are highlighted in green for better visibility: "christine," "lagarde," "mario" and "draghi." Word cloud is based on cleaned, translated and lemmatized tweets. Stopwords are removed. No ngrams are included.

validity of proxying nationality with tweet language. Interestingly, the peaks of the time series are different for each language, and coincide with matters that have particular importance to different countries. The French peak in July 2019 reflects Lagarde's nomination as incoming ECB president, while the Spanish peak in February 2018 followed Eurogroup support for de Guindos. The German peak in May 2020 coincides with constitutional judges ruling parts of the ECB's asset purchase program unconstitutional. The Italian peak in March 2020 occurred a day after the PEPP announcement, which sharply lowered Italy's borrowing costs amid the early Covid-19 outbreak. The English peak in July 2022 marks the ECB's first rate hike in 11 years, suggesting English tweets focus more on EA-wide monetary policy, while other languages emphasize country-specific news. This underscores the importance of non-English tweets for understanding the general dynamics of central bank communication, while making it impossible to infer any nationality from them.

Time trends Tweet volumes and Twitter users rise over time i.e., between Draghi and Lagarde. Growth in tweets is largest for French and smallest for Italian (Table A5). These descriptives may reflect ingroup effects as well as other platform growth that is unrelated to ECB presidency, and should therefore not be interpreted causally.

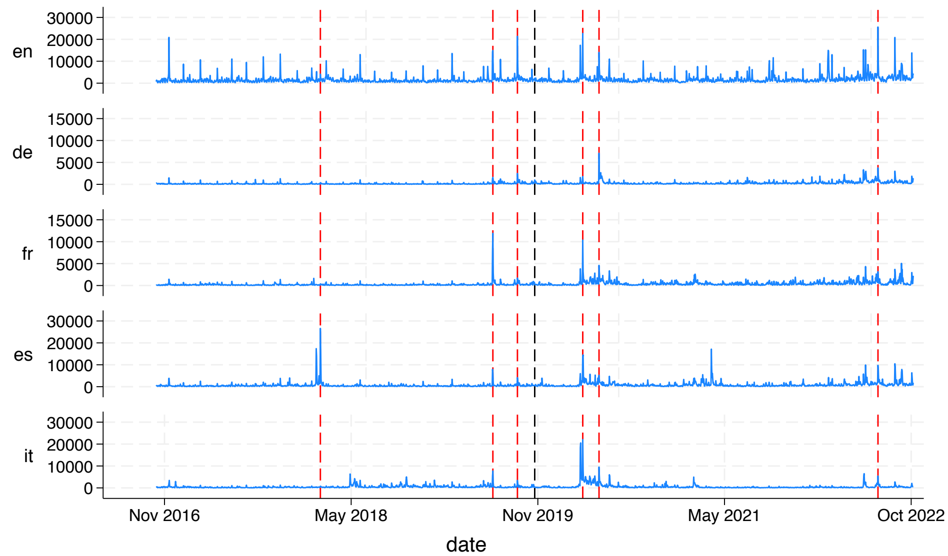


Figure A9: Daily Tweet Volumes by Language.

Notes: Daily volume of tweets (using CET as this is the time zone corresponding to the headquarter of the ECB). The dashed grey vertical line indicates the changeover in presidency (November 1, 2019). Dashed red vertical lines indicate the following events (from left to right): Eurogroup's support for de Guindos (February 19, 2018), announcement of Lagarde as incoming ECB president (July 3, 2019), the day after PEPP announcement (March 19, 2020), German constitutional court ruling (May 5, 2020), ECB raising rates for the first time in 11 years (July 21, 2022).

Table A5: Number of Tweets and distinct Twitter Users by Language and Presidency

	Draghi (Twitter Users)	Lagarde (Twitter Users)	Tweet Growth (User Growth)
English	1,595,810 (279,482)	2,102,124 (499,940)	31.7% (78.9%)
German	168,006 (29,903)	397,848 (67,829)	136.8% (126.8%)
Spanish	623,046 (153,419)	1,106,234 (257,135)	77.6% (67.6%)
French	238,455 (62,737)	585,257 (120,942)	145.4% (92.8%)
Italian	543,504 (60,403)	671,653 (78,873)	23.6% (30.6%)

Notes: Number of tweets and distinct Twitter users (in brackets) by language and presidency, where "Draghi" refers to tweets posted during 2016–2019 until the switchover in presidency in November 2019, and "Lagarde" refers to tweets posted afterwards (2019–2022). Languages are ranked by the number of distinct Twitter users (ranking remains the same across presidencies). The blue font indicates ingroup periods.

Sentiment Measure Sentiment of tweet content is measured by a dictionary-based method that provides a continuous estimate of how positive or negative a text snippet is. A sentiment score is computed for each tweet using the polarity measure of the Python package *TextBlob*, which is based on the Princeton University's WordNet lexicon (Loria, 2018). Sentiment scores are estimated on tweets that are cleaned and translated (and thus spell-checked). This ensures that the same (English) dictionary can be used to create the sentiment scores. I normalize this sentiment indicator to assign values between 0 (most negative) and 1 (most positive) to each tweet, where neutral text gets a value of 0.5. The original polarity sentiment indicator returns a value between -1 and 1, where the most positive (negative) text obtains a value of 1 (-1) and neutral text gets a value of 0.

Sentiment across languages To be able to make meaningful comparisons of beliefs across languages, it is important that no language shows idiosyncratic differences in the measure of tweet sentiment. Given the strong similarity in the distribution tweet sentiment, this seems to be indeed the case. An overview of tweet sentiment across languages can be found in Table A6. The average and standard deviation of tweet sentiment is remarkable similar across languages.[39]

Table A6: Sentiment of Tweets by Language

Language	N	Mean	Std. dev.	p10	p90
English	3,697,934	0.53	0.10	0.45	0.65
German	565,854	0.52	0.10	0.42	0.63
Spanish	1,729,280	0.53	0.10	0.44	0.65
French	823,712	0.53	0.10	0.44	0.64
Italian	1,215,157	0.52	0.11	0.42	0.65

Notes: Sentiment ranges from 0 to 1, with more positive (negative) tweets closer to 1 (0) and neutral tweets at 0.5. Means and standard deviations are computed over all available tweets by language during 2016–2022.

A.2 Information Availability in Twitter and Newspapers

Information Availability Rises on Twitter Ingroup status significantly affects information availability on Twitter (Table A7). Across level of expertise, tweet volume rises for the ingroup, with the largest rise for users that are neither strict experts nor non-experts.

[39] Average tweet sentiment is close to neutral between 0.52 and 0.53, and the standard deviation is either 0.10 or 0.11. Even the 10^{th} and the 90^{th} percentiles of tweet sentiment do not vary considerably across countries, with values for the 10^{th} percentile ranging between 0.42 and 0.45, and for the 90^{th} between 0.63 and 0.65, respectively. It is noteworthy, however, that across languages most tweets have rather neutral sentiment around 0.5, and that tweets are slightly more positive than negative.

Table A7: Ingroup Effect on Information Availability on Twitter

	(1) All	(2) Experts	(3) Non-Experts
Ingroup	0.105***	0.0244**	0.0778***
	(0.0179)	(0.0103)	(0.0190)
Spanish	0.256***	0.129***	0.243***
	(0.0179)	(0.0103)	(0.0190)
French	-0.00138	-0.0944***	0.0195
	(0.0200)	(0.0115)	(0.0212)
Italian	0.0887***	0.0923***	0.0903***
	(0.0200)	(0.0115)	(0.0212)
Constant	0.138***	0.212***	0.142***
	(0.0127)	(0.00725)	(0.0134)
Observations	200	200	200
R^2	0.582	0.738	0.505

Notes: OLS regressions of ingroup status with the ECB president on the share of tweets by language per 6-week press conference (PC) cycle. German is the baseline language. Observations cover 4 languages and 49 press conferences, with one cycle overlapping Draghi's and Lagarde's presidencies, yielding 50 president–PC cycle combinations. Expert classification follows Ehrmann and Wabitsch (2022). Standard errors in parentheses.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Does Print Media Confirm Increased Information Availability? Twitter covers information availability beyond mere newspaper coverage, as it better reflects how exposed individuals are on average to given news story. However, to preclude doubts that information availability via print media provides a different picture to the results from Twitter, I analyze a sample of newspaper articles sourced from LexisNexis. The selection criteria mirror the Twitter sample: articles are included in the sample if they contain the keywords "ECB," "European Central Bank" or the translated equivalents in the respective languages. For each nationality, the source of newspaper articles must be in the respective country and the language of the article must be the local language. The sample is restricted to only newspaper articles and the sample spans from November 1, 2016 to October 31, 2022, which reflects exactly three years of presidency for both Mario Draghi and Christine Lagarde. To avoid sample fluctuations over time that are due to LexisNexis-related data availability rather than reflecting actual changes in newspaper articles, I limit my sample to a few major newspapers for each country that are consistently available for the entirety of the sample, while ensuring political perspectives of selected newspapers are balanced. For Italian news, I include "Corriere della Sera" and "La Nazione." The French sample is comprised of "Le Figaro," "La Tribune," and "La Croix." The German sample contains "BILD" (incl. Sunday and regional editions), "Süddeutsche Zeitung" (incl. regional editions), and "Der Tagesspiegel." Finally, the included Spanish newspapers are "El Mundo" and "El País."

The presidency switch serves as a pseudo-treatment to assess how a policymaker's nationality affects national news using a difference-in-difference approach. Newspaper volume remains stable in Germany and Spain but declines in Italy under Lagarde, while it rises in France. Figure A10 visualizes these trends. Table A8 shows the estimated effect, controlling for time- and country-specific effects. The ECB president's nationality significantly increases the share of their corresponding national newspaper volume by 6.1 percentage points (or by roughly 272 articles).

In sum, echoing my social media results, I find that the ECB president's nationality raises traditional national newspapers reporting. This confirms the positive ingroup effects on information availability.

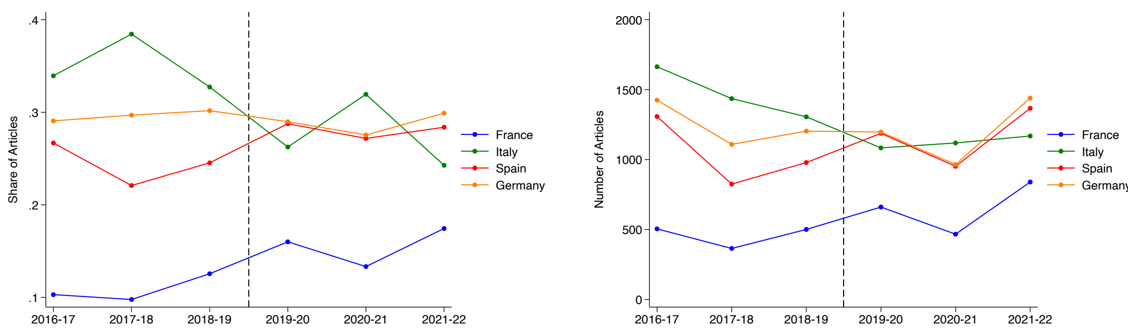


Figure A10: National Newspaper Articles Over Time

Notes: The left panel plots countries' share of articles each year, while the right panel shows the volume of national newspaper articles over time. Each point is based on the volume of newspaper articles over 12 months (from November 1 until October 31 of the following year). The vertical grey dashed line indicates the switch in presidents.

Table A8: Ingroup Effect on Information Availability on Newspaper Volume

	(1) Share Of Articles	(2) Share Of Articles	(3) Number of Articles	(4) Number of Articles
Ingroup	0.061***	0.061**	271.8***	271.8**
	(0.018)	(0.021)	(68.1)	(71.6)
2017-2018	−0.000	0.020	−291.8***	−184.0
	(0.019)	(0.028)	(78.2)	(97.3)
2018-2019	−0.000	0.005	−228.0***	−181.0
	(0.012)	(0.025)	(67.2)	(155.4)
2019-2020	−0.000	−0.010	−193.0***	−212.0*
	(0.011)	(0.011)	(53.8)	(94.3)
2020-2021	−0.000	0.005	−349.8***	−291.5
	(0.017)	(0.041)	(66.0)	(144.3)
2021-2022	0.000	−0.013	−21.5	−80.0
	(0.015)	(0.031)	(73.2)	(124.0)
Spanish	−0.030**		−120.2*	
	(0.013)		(56.8)	
French	−0.191***		−802.8***	
	(0.013)		(67.0)	
Italian	−0.010	0.180***	−62.9	739.8***
	(0.020)	(0.021)	(76.6)	(71.6)
Constant	0.292***	0.100***	1404.0***	578.7***
	(0.007)	(0.013)	(42.2)	(104.7)
Observations	24	12	24	12
R^2	0.925	0.952	0.941	0.970

Notes: OLS regressions. Column (1) reports the effect of ingroup status with the ECB president on the share of newspaper articles, controlling for time and country fixed effects. Column (2) restricts the sample to countries that switch ingroup status (France and Italy). Columns (3) and (4) repeat the analysis using article counts instead of shares. All regressions are estimated by OLS. *Ingroup* equals 1 for the ECB president's country of origin. Baseline categories: outgroup, years 2016–2017, and Germany or France for countries. Robust standard errors in parentheses.$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

A.3 Empirical Estimation of Belief Updating with Tweets

Priors and Posteriors I compare tweet sentiment before and after ECB press conferences. For users who tweet both before and after a conference, the posterior is the sentiment of the first post-conference tweet, $Posterior_{i,t} \equiv Sentiment_{i,t}$, and the prior is the sentiment of the user's last tweet during the quiet period (7 days before the press conference), $Prior_{i,t-1} \equiv Sentiment_{i,t-1}$. This isolates responses to the press conference while avoiding other ECB communications during the quiet period (Fig. A11).

Estimation I first estimate updating with press-conference fixed effects:

$$\text{Posterior}_{i,t} \;=\; \beta_1\, \text{Prior}_{i,t} \;+\; \delta' D_t \;+\; \epsilon_{i,t}, \tag{17}$$

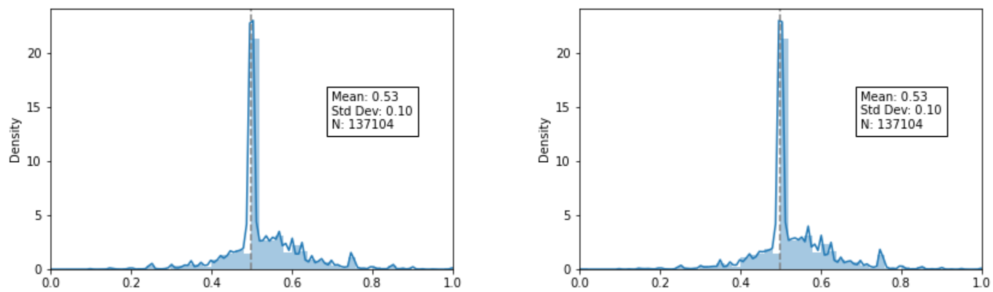


Figure A11: Distributions of Priors and Posteriors

Notes: Histograms with kernel densities for priors (left) and posteriors (right). The dotted vertical line marks 0.5 (neutral). Boxes report means, SDs, and N. Values are normalized to $[0, 1]$.

where D_t are press conference dummies. Next, I allow updates to differ by ingroup status,

$$\text{Posterior}_{i,t} = \beta_1 \text{Prior}_{i,t} + \beta_2 \left(\text{Prior}_{i,t} \times \text{Ingroup}_{i,t}\right) + \beta_3 \text{Ingroup}_{i,t} + \delta' D_t + \epsilon_{i,t}, \quad (18)$$

so that a negative β_2 indicates lower prior reliance (greater responsiveness) for the ingroup, while limiting assumptions about the signal, similar to Coibion et al. (2021). For horizon definitions, I estimate (i) quiet periods (QP: last 7 days prior to the press conference), (ii) outside quiet periods (NQP: weeks excluding QP and the 24h press conference window), and (iii) the 24h press conference window. To be included in (N)QP, users are required to tweet at least twice within the same (N)QP; PC updates require a prior in the most recent QP and a posterior within 24 hours after the monetary announcement.

Avoiding signal quantification allows flexible horizon definitions. During QP, when the ECB restricts communication, priors should weigh more heavily, so prior coefficients are expected to be higher. During NQP, when communication resumes, prior coefficients should be lower. For PC updates, I require a posterior within 24h of the announcement and a prior from the most recent QP to ensure responses reflect only press conference information. Whether updating is stronger or weaker than in NQP depends on whether belief shifts stem mainly from monetary policy news or other ECB-related events (e.g., personnel changes).

Table A9 reports β_1 across horizons: prior reliance is highest for QP and lower for PC, indicating stronger belief updating when the ECB communicates than when it does not. It is lowest for NQP, consistent with stronger updates from unanticipated events. Across all horizons, ingroup individuals rely about a third less on their priors than outgroup individuals. Lower prior reliance implies higher responsiveness to new information, making central bank signals more effective in shaping beliefs.

Table A9: Belief Updates at Different Points in Time

	Quiet Period (QP)	Outside QP (NQP)	Press Conferences (PCs)
Dep. Var.: Posterior	**(1)**	**(2)**	**(3)**
Prior	0.511***	0.339***	0.492***
	(0.002)	(0.001)	(0.004)
Ingroup*Prior	-0.403***	-0.285***	-0.455***
	(0.005)	(0.002)	(0.010)
Ingroup	0.224***	0.154***	0.255***
	(0.003)	(0.001)	(0.005)
Outgroup Prior	0.511***	0.339***	0.492***
	(0.002)	(0.001)	(0.004)
Ingroup Prior	0.331***	0.208***	0.292***
	(0.002)	(0.001)	(0.005)
Fixed Effects	QP	NQP	PC
Observations	228,733	2,009,176	63,757
R^2	0.958	0.958	0.954

Notes: OLS regressions. Horizons: QP = 7 days pre–press conference; NQP = outside QP and the 24h PC window; PC window = prior from the most recent QP and posterior within 24h post-announcement. Inclusion requires at least 2 tweets within the interval. "Ingroup Prior" is a linear combination of the relevant coefficients. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Signals Finally, I replace press conference fixed effects with press conference signals to measure responsiveness to news:

$$\begin{aligned} Posterior_{i,t} = \beta_1 Prior_{i,t} + \beta_2 Prior_{i,t} * Ingroup_{i,t} + \beta_3 Ingroup_{i,t} \\ + \beta_4 Signal_t + \beta_5 Signal_t * Ingroup_{i,t} + \epsilon_{i,t}, \end{aligned} \tag{19}$$

where $Signal_t$ is the monetary policy surprises from the Euro Area Monetary Policy Event-Study Database (EA-MPD) (Altavilla et al., 2019).[40] This identifies prior reliance (β_1, β_2) and signal use (β_4, β_5) by group.

Table A10 shows that ingroup users rely less on priors and react more to signals. Results are remarkably similar across expert and non-expert subsamples. Figure A12 illustrates these patterns.

[40]This database is provided by the ECB and contains high-frequency asset price changes for the time window of the announcement of monetary policy decisions on press conference days. I use the volatility indicators for the 2-year Overnight Index Swaps (OIS-2Y) to capture the signal of monetary policy news, including any informational effects of Forward Guidance.

Table A10: Belief Updating after Press Conferences

Dep. Var.: Posterior	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
	All	All	All	Non-Experts	Experts
Prior	0.492***	0.835***	0.811***	0.851***	0.818***
	(0.004)	(0.002)	(0.002)	(0.007)	(0.006)
Ingroup*Prior	-0.455***	-0.806***	-0.782***	-0.795***	-0.701***
	(0.010)	(0.010)	(0.010)	(0.037)	(0.026)
Signal		0.166***	0.197***	0.163***	0.188***
		(0.003)	(0.003)	(0.009)	(0.006)
Ingroup*Signal			-0.164***	-0.147***	-0.169***
			(0.003)	(0.022)	(0.016)
Ingroup	0.255***	0.440***	0.498***	0.487***	0.457***
	(0.005)	(0.006)	(0.006)	(0.021)	(0.015)
Outgroup Prior	0.492***	0.835***	0.811***	0.851***	0.818***
	(0.004)	(0.002)	(0.002)	(0.007)	(0.006)
Ingroup Prior	0.292***	0.469***	0.527***	0.543***	0.574***
	(0.005)	(0.005)	(0.006)	(0.020)	(0.014)
Outgroup Signal			0.197***	0.163***	0.188***
			(0.003)	(0.009)	(0.006)
Ingroup Signal			0.532***	0.503***	0.475***
			(0.006)	(0.023)	(0.016)
Fixed Effects	PC	-	-	-	-
Observations	63,757	63,757	63,757	5,509	11,478
R^2	0.954	0.946	0.946	0.944	0.957

Notes: OLS regressions. Columns (2)–(5) exclude the constant. Signal: EA-MPD OIS-2Y. Columns (4)–(5) restrict to non-experts/experts (following Ehrmann and Wabitsch (2022)). "Ingroup Prior/Signal" are linear combinations of the relevant coefficients. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

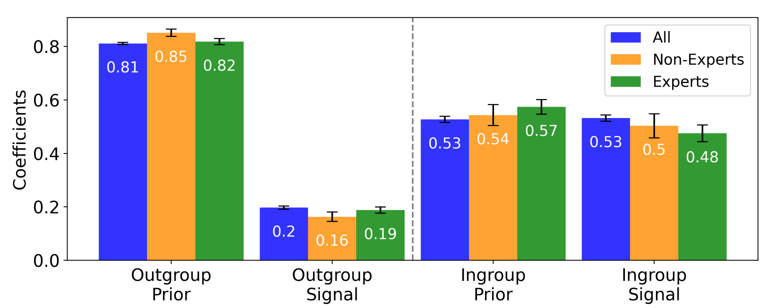


Figure A12: Belief Updating After Press Conferences by Expertise

Notes: Bars plot summed OLS regression coefficients (base + interaction) with 95% confidence intervals for priors (signals) in the left (right) panel for either individuals of the out- or the ingroup. Blue bars: entire sample; orange: non-experts; green: experts. The expert classification follows Ehrmann and Wabitsch (2022).

B Experimental Design, Data, and Results

This appendix documents the full experimental protocol. It expands Section 3 by detailing: data in forecasting tasks and information treatments, the post-experimental survey, additional results, and the complete interface of the experiment.

B.1 Details on the Experimental Design

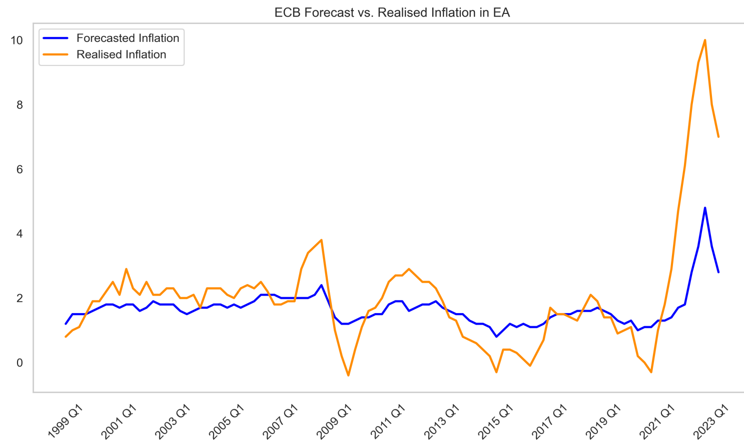
This appendix provides the full technical details of the experimental design. It expands on the condensed description in Section 3.1 by documenting (i) the data used in forecasting tasks, (ii) the incentive schemes, (iii) the additional information treatments, and (iv) the post-experimental survey. Figures of the experimental interface are provided separately in Appendix B.3.

Data in Forecasting Tasks. Data for the forecasting tasks are based on actual annual inflation realizations in the EA between 1990Q1 and 2023Q1 (at quarterly frequency), and real-world quarterly forecasts of EA annual inflation from the ECB Survey of Professional Forecasters (SPF).[41] Using real data ensures credible economic dynamics. To emphasize this, participants are explicitly informed that both inflation dynamics and the forecasts correspond to the real EA economy and that forecasts are real historical expert forecasts. The large amount of data points (97 quarters) makes it unlikely that participants would recognize a historical pattern. Figure A13 shows the full quarterly series of realized EA inflation, SPF forecasts, and forecast errors.

From this, six forecasting sequences of 11 periods each are randomly selected – one for each forecasting task. Participants observe the first ten periods before making their prior forecast for period 11. Figure A14 illustrates the selected sequences, and Table A11 reports summary statistics (mean inflation, forecast precision, and volatility) for each. Sequences are randomized across treatments to avoid confounding messenger effects with scenario-specific differences.

[41] The SPF is a quarterly survey of expectations for the rates of inflation and other variables. Respondents of the SPF are experts affiliated with financial or non-financial institutions based within the European Union. More information on the SPF can be found here.

Figure A13: Quarterly EA Inflation, SPF Forecasts, and SPF Forecast Errors



Notes: Realized quarterly EA HICP inflation and one-year-ahead forecasts from the ECB Survey of Professional Forecasters.

Table A11: Overview of Randomly Selected Sequences

10-Period Starting Date	10-Period Average of Forecasting Precision	10-Period Average of Inflation	10-Period STD of Inflation	Next-Period Signal	Next-Period Realization
2006Q3	1.6	2.6	0.8	1.4	1
2008Q1	1.1	1.7	1.5	1.5	1.7
2004Q1	2.4	2.2	0.2	2.1	2.2
2011Q4	1.7	1.9	0.8	1.2	0.6
2001Q2	1.9	2.3	0.3	1.6	2.1
2017Q3	4.8	1.5	0.4	1.3	1.1

Notes: Average forecast precision, mean realized inflation, and the standard deviation of realized inflation for the six randomly selected sequences. The indicated date marks the beginning of a 10-quarter sequence. "Next-Period Signal" and "Realization" refer to the forecast and realization of inflation, respectively, in the 11^{th} quarter. Data for realized EA inflation and SPF forecasts are taken from the ECB Statistical Data Warehouse.

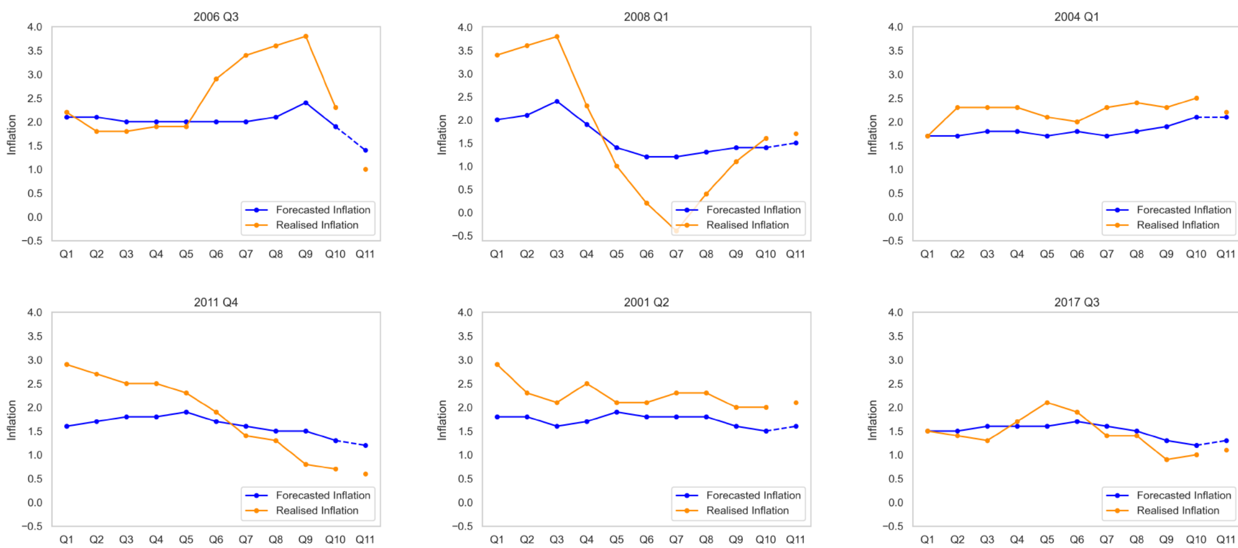


Figure A14: Inflation Sequences

Notes: Sequences are randomly selected from real EA HICP and SPF one-year-ahead forecast data. The solid line shows the history of realized inflation and forecasts presented to participants. The dashed line shows the forecast leading to the signal, and the unconnected final dot shows the value participants are asked to forecast. Dates are omitted in the experiment. Sequences are randomized across treatments.

Details on Forecast Incentives. Point forecasts and range forecasts are incentivized using scoring rules standard in the experimental literature. For *point forecasts*, participants receive a bonus that decreases with the absolute forecast error. A perfect forecast yields the maximum bonus, which diminishes proportionally with each percentage point of error. The point forecast score $F_{i,t}$ is given by

$$F_{i,t} = 6 \cdot 3^{-|\mathbb{E}_{i,t-1}\{\pi_t\} - \pi_t|}, \tag{20}$$

where π_t is realized inflation at time t, and $\mathbb{E}_{i,t-1}\{\pi_t\}$ is participant i's forecast made at $t-1$. A perfect forecast yields $F_{i,t} = 6$; the score declines by two-thirds for each percentage point of error.

For *range forecasts*, participant i submits at $t-1$ a range $[\underline{u}_{i,t-1}, \overline{u}_{i,t-1}]$ for π_t. Let $r_{i,t-1} = \overline{u}_{i,t-1} - \underline{u}_{i,t-1} \geq 0$ denote the width. The score realized at t is

$$U_{i,t} = \begin{cases} 0, & \pi_t \notin [\underline{u}_{i,t-1}, \overline{u}_{i,t-1}], \\ \dfrac{\phi}{1 + r_{i,t-1}}, & \pi_t \in [\underline{u}_{i,t-1}, \overline{u}_{i,t-1}], \end{cases} \qquad \text{with } \phi = 6.$$

To avoid learning effects, and any impact from forecast performance on survey answers, realized inflation values are not shown until the very end of the experiment. Participants then receive feedback on all 12 forecasts (six priors and six posteriors) and learn which of the 24 decisions (12 point forecasts and 12 ranges) is randomly selected for payment. Randomization ensures equal effort is applied to all forecasts.

Information Treatments. In addition to the numerical forecast (the signal), participants may request up to three qualitative pieces of information linked to the same inflation scenario. These are based on ECB press conferences held at approximately the same time as the corresponding SPF forecast, ensuring consistency between numerical and qualitative information. Table A12 lists all information pieces revealed by clicking "Read More" by inflation scenario. Attention to information is measured by the number of "Read More" buttons clicked.

Table A12: Additional Information Pieces and Corresponding Inflation Scenarios

Inflation Scenario	Information Pieces
2006 Q3	1) Overall, the evidence that inflationary pressures are diminishing further has increased and, looking forward, inflation rates are expected to be in line with price stability, supporting the purchasing power of incomes and savings. 2) The recent decline in headline inflation mainly reflects the considerable easing in global commodity prices over the past few months, which more than offsets the impact of the sharp rise in unit labour costs in the first half of this year. Lower commodity prices and weakening demand should lead to inflationary pressures diminishing further. 3) The large declines in commodity prices and the impact of weakening demand on price developments strongly impacted this forecasts.
2008 Q1	1) Price developments are expected to remain moderate over the medium-term horizon. Global inflationary pressures may persist, while domestic price pressures are expected to remain low. 2) The still weak annual growth rate of bank loans to the private sector conceals the fact that monthly flows have now been positive for a number of months. At the same time, these aggregate developments continue to reflect mainly an ongoing strengthening in the annual growth of loans to households, while the annual growth of loans to non-financial corporations has remained negative. 3) Overall, price stability is expected to be maintained over the medium term, thereby supporting the purchasing power of Euro area households. Inflation expectations remain firmly anchored.
2004 Q1	1) The inflation rates are forecasted to remain elevated. Factors contributing to this include higher energy prices, indirect effects of past oil price increases, and potential stronger-than-expected wage developments. 2) The strength of monetary and credit growth, coupled with ample liquidity, pose inflationary risks over the medium to longer term. 3) The rapid rate of monetary growth is primarily driven by the stimulative impact of low-interest rates.
2011 Q4	1) The risks to the economic outlook for the Euro area are considered to be broadly balanced over the medium term, both for upside and downside risks to price developments. 2) The monetary analysis confirms subdued underlying growth in broad money and credit, and the annual growth rate of loans to the private sector continues to contract. 3) Progress has been made in improving the funding situation of banks, but further steps are needed to strengthen the resilience of banks and reduce the fragmentation of Euro area credit markets.
2001 Q2	1) Real economic activity in the Euro area was weak recently, but survey data and recent financial market developments indicate a gradual upturn in economic activity. 2) The expected pick-up in activity is supported by both external factors, such as global recovery, and domestic factors, such as ongoing adjustment efforts by companies to enhance competitiveness and profitability. 3) Downside risks to the main scenario for economic growth have declined, but they have not disappeared. Macroeconomic imbalances in some regions and high oil prices are mentioned as potential risks to economic activity in the Euro area.
2017 Q3	1) While there are some initial signs of stabilization in the growth slowdown and a mild increase in underlying inflation, overall inflation remains low. 2) Ongoing employment growth and increasing wages are underpinning the resilience of the Euro area economy. 3) Inflation expectations are at low levels, and measures of underlying inflation have remained generally muted. Inflation is expected to increase over the medium term, supported by monetary policy measures, economic expansion, and solid wage growth.

Notes: Information pieces are linked to inflation scenarios (each scenario's first quarter shown in the first column). Each inflation scenario includes three buttons, each displaying one piece of information. Information pieces are summarized statements based on ECB press conferences, which can be found in full here.

Post-Experimental Survey. After completing the forecasting tasks, participants answer survey questions designed to measure mechanisms behind messenger effects. They rate each messenger's *perceived ability* (7-point Likert scale), and report their *trust* and *exposure* to monetary institutions (the ECB and their NCB) and to policymakers representative of the messenger treatments. These policymakers are the sitting ECB board members and NCB governors of the four nationalities at the time of the experiment (see Table A13). Participants also report monetary policy expertise, associations with messengers of treatments, and complete attention and experimenter demand checks. All survey questions are fully shown in Appendix B.3.

Table A13: Representative Policymakers by Nationality

	ECB Board Member	NCB Governor
Germany	Isabel Schnabel	Joachim Nagel
Spain	Luis de Guindos	Pablo Hernández de Cos
France	Christine Lagarde	François Villeroy de Galhau
Italy	Fabio Panetta	Ignazio Visco

Notes: ECB board members and NCB governors shown were in office at the time of the experiment and are used as representative policymakers in the post-experimental survey. Policymakers count as ingroup if their nationality matches the participant's, and as outgroup otherwise.

B.2 Experimental Sample

Prolific provides background data on participants before the study, including age, sex, income, marital and employment status, student status, ethnicity, country of birth and residence, years in current country, childhood residence, financial decision-making, languages, education, migration history, investments, property ownership, and cultural and linguistic background. Figure A15 summarizes key participant characteristics.

While not designed to be nationally representative, the sample is far more diverse than typical lab experiments, which usually consist of students only. Employment rates align closely with national figures (67.7% in the experiment compared to 68.1% in the population) (Eurostat, 2023a). The unemployment rate is somewhat higher in the experiment (14.7% versus 7.6%) (Eurostat, 2023e), but cross-country differences follow population patterns, with higher unemployment in Spain and Italy than in France and Germany. This discrepancy may be driven by students on Prolific classifying themselves as unemployed, a view supported by the relatively low combined shares of respondents reporting being "not in paid work" (5.0%) or "other" (11.7%) compared to the inactive share of the population (26.3%) (Eurostat, 2023c). It could also partly reflect a lower proportion of retirees, as participants are younger on average: across all countries, the median age in the experiment was 30, compared to a population median of about 45 in 2023 (Eurostat, 2023d). Educational attainment is, on average, somewhat higher than in the population, which is the case across all four countries and might simply reflect the lower age of participants. The French sample is the most

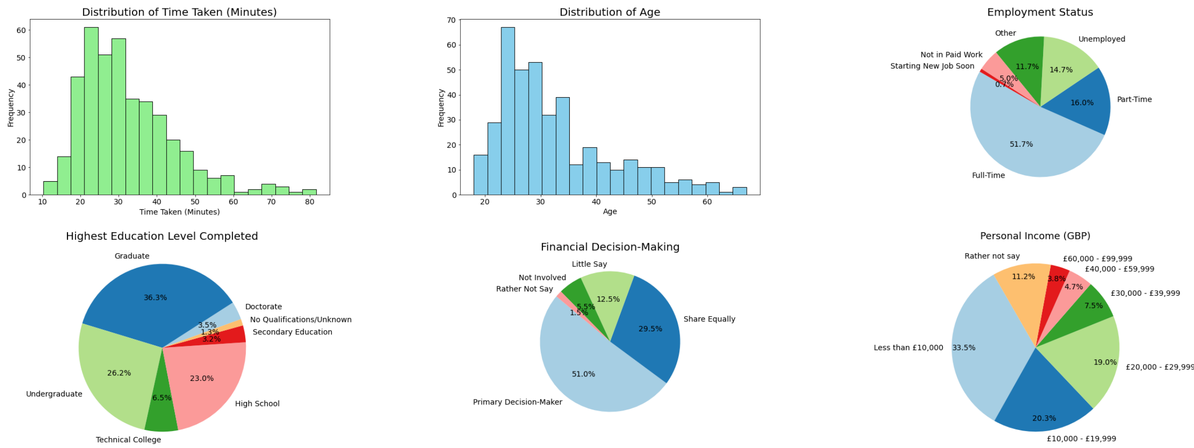


Figure A15: Descriptive Evidence of Experiment Participants

Notes: Panels show key characteristics of participants. The top-left histogram reports completion times. Median completion time is around 30 minutes; most finish within an hour. Prolific automatically disqualifies participants who take longer than 90 minutes.

educated, in line with population patterns (Eurostat, 2023b). Further, reported household income is consistent with the population average.[42]

Power Analysis. A power analysis determines the required sample size to avoid insignificant results caused by insufficient power (see Cohen (2013)). My experiment uses a within-subjects design, so power calculations are based on comparisons of dependent means.

Based on observational results, pilot data, and related findings (e.g., D'Acunto et al. 2022), the expected effect size is approximately 0.1, with plausible values up to 0.3. Figure A16 shows the required sample size across power and significance levels. The conventional specification of 0.8 power at $\alpha = 0.05$ is represented by the dark blue line. This suggests an ideal sample size just below 800, sufficient to detect an effect size of 0.1. Due to budget constraints, the actual sample size is 400 participants (100 per nationality), which ensures sufficient power to detect effects of about 0.14 and larger. Smaller effects, if statistically insignificant, may therefore reflect limited power.

[42]Average personal income after taxes of participants is just over 17k GBP, using the midpoints of each bracket. This method might produce slightly higher values if those in the 60–99k GBP bracket cluster closer to the lower bound. OECD (2023) reports annual gross wages, which, after accounting for taxation, amount to roughly 31k EUR in France and Germany and around 23k EUR in Italy and Spain.

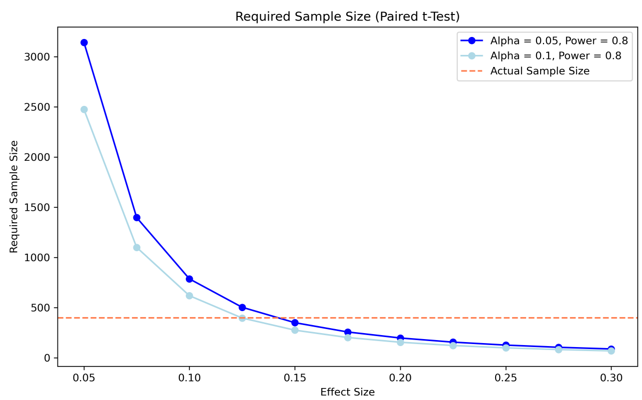


Figure A16: Power Analysis

Notes: Required sample size for paired t-tests at different effect sizes, power levels, and significance thresholds. The red dashed line indicates the actual sample size of 400.

B.3 Interface of the Experiment

Welcome!

Name of Study: Forecasting Inflation

Description: The aim of this experiment is to better understand macroeconomic expectations. No prior knowledge is necessary to participate in the experiment. Your task will be to forecast inflation in different economic scenarios. After these forecasting tasks, we will ask you a few short questions about your views related to the economy.

How will I get remunerated for my time? We highly value your participation in this 25-minute experiment, which is why we would like to remunerate you fairly. You will get £4 for participating in this survey. In addition, you have the chance to get up to £6 added to your payment. The more accurately you forecast, the more of the additional £6 you will take home. The maximum amount of money you could get is thus £10, and the minimum is £4. We will reward you as quickly as possible. Typically, you can expect to get rewarded within 1-3 business days after submission.

How will my data be used? Your answers will be completely anonymous, and we will keep them confidential. Your data will be stored in a password-protected file and may be used in academic publications. Your IP address will never be stored. Research data will be stored for a minimum of five years after publication or public release. By submitting your personal data, you agree to this transfer, storing or processing.

Who will have access to my data? Data are collected for the sole purpose of research and processed by researchers at the University of Oxford. Any personal information that could identify you will be removed or changed before files are shared with other researchers or results are made public. Responsible members of the University of Oxford and funders may be given access to data for monitoring and/or audit of the study to ensure we are complying with guidelines.

Do I have to take part? No. Please note that your participation is voluntary. You may withdraw at any point during the experiment for any reason, before submitting your answers, by closing the browser. However, we are only able to reimburse participants who complete the full survey.

Who has reviewed this project? This project has been reviewed by, and received ethics clearance through, the University of Oxford's Economics Department's Research Ethics Committee [reference number: ECONCIA21-22-24].

What if there is a problem? If you have a concern about any aspect of this project, please speak to Alena Wabitsch (alena.wabitsch@economics.ox.ac.uk), who will try to answer your query. The researcher should acknowledge your concern within 5 working days and give you an indication of how they intend to deal with it. If you remain unhappy or wish to make a formal complaint, please contact the relevant Chair of the Research Ethics Committee at the University of Oxford:

The Department of Economics Departmental Research Ethics Committee can be contacted at:
Email: ethics@economics.ox.ac.uk
Address: Department of Economics
Manor Road 10
Oxford 0X1 3UQ
United Kingdom

The University of Oxford's Data Protection Officer can be contacted at:
Email address: data.protection@admin.ox.ac.uk
Telephone number: + 44 1865 280 199
Address: Planning and Council Secretariat
University Offices
Wellington Square
Oxford OX1 2JD
United Kingdom

If you have read and understood the information above, and agree to participate with the understanding that the data (including any personal data) you submit will be processed accordingly, please check the relevant box below to get started.

Yes, I have understood the above information and agree to take part.

Please provide your Prolific ID:

Click *Next* to start the experiment.

Next

Figure A17: Welcome Page

Instructions

Please read these instructions carefully to make good decisions during the experiment. The quality of your decisions directly impact your bonus payment, which can be up to £6, in addition to the £4 participation payment. There will be a quiz on these instructions on the next page. If you answer at least one question incorrectly three times, the experiment will be ended early.

Throughout the experiment, you can always access these instructions by clicking the *"Show Instructions"* button at the bottom of any page.

Your Goal in the Experiment:
Your task is to **accurately forecast the annual inflation rate in the Euro area**. This means, you will be predicting the percentage increase or decrease in prices in the entire Euro area between a specific point in time and exactly 1 year earlier. Your goal is to **minimise forecast error**, which is the difference between your forecasted value of annual inflation and the realised annual inflation.

Note: The more accurate your inflation forecasts, the lower your forecast error, the ***more bonus payment*** *you get!*

Whenever we talk about inflation in this experiment, what we mean is the *annual* inflation rate (i.e., the percentage change in prices of goods and services over the course of exactly 1 year.) Data for inflation reflect real economic scenarios of the Euro area. Sometimes you will be shown professional forecasts, which are based on analyses of real European experts for the given, real economic scenarios.

The Experiment:

You will make two types of inflation forecasts:

- **Point Forecast:** Your 'Point Forecast' of inflation is your best guess of the exact value of inflation for the next unknown period.
- **Range Forecast:** Your 'Range Forecast' of inflation is the range of plausible values that you think will almost certainly contain the realised value of inflation in the next period (i.e. the upper and the lower inflation bounds of inflation).

The closer your point forecast is to the realised inflation, the more money you get. Similarly, the smaller your indicated range of inflation, the more money you get (as long as the realised inflation rate is inside your forecasted range).

What to Expect:
You will face 6 **distinct** forecasting tasks. Each task is independent and unrelated to the others. In each task, you will first make your inflation forecasts (*'Initial Forecasts'*). You will then receive additional information about the next period's inflation rate and have a chance to adjust your forecasts (*'Updated Forecasts'*). After all 6 forecasting tasks, we will ask you a few questions, and then unveil the realised inflation rates and your performance.

What to do in each Forecasting Task:

1. Examine the given 10 periods of inflation history.
2. Make your Initial Forecasts for the next period:
   - A point forecast of inflation
   - The corresponding range forecast of inflation
3. You will be provided professional inflation forecasts, which are based on real historic forecasts of European experts for the respective scenarios. *Note: The provided historic differences between the realised inflation and the forecasted inflation represent the experts' historic forecast errors.*
4. Make your Updated Forecasts for the next period:
   - As before, provide a point forecast, and the corresponding range forecast.
   - *Note: Your Updated Forecasts can be the same as your Initial Forecasts, use some of the same values, or use completely different values.*
   - We will remind you of the values of your Initial Forecasts, when you are making your Updated Forecasts.

Overview of all the steps of a forecasting task:

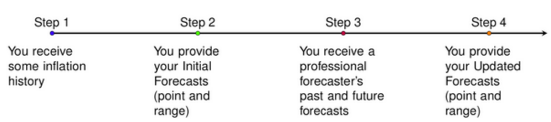


Figure A18: Instructions (1)

Interacting with the data:
Historical data, professional forecasting history, and your point and range forecasts will be visualised in an interactive chart.

- Hover over chart dots for values.
- Type all your forecasts in percentages (up to 1 decimal) into the clearly labelled input text boxes.
- You may submit positive values (prices are going up), negative values (prices are going down), or a value of zero (prices are staying the same).
- Your forecast range can be as big or small as you prefer.
- Your point forecast doesn't have to be the mid-point of your range forecast. Your point forecast may be closer or farther from the upper than the lower bound.
- Your point forecast must always lie within your forecasted range - the software will prevent other inputs.
- Your point inflation forecasts will be visualised with green squares, and the corresponding range forecasts with green triangles.
- Submit your forecasts by clicking the 'Next' button. Note: You won't be able to change your answers after submitting them.

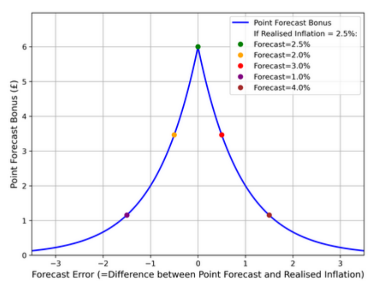
How your decisions get you bonus payment:

You can earn up to £6 in bonus payment for the accuracy of your point and range forecasts. The full £6 bonus requires your point forecast to match realised inflation (down to 1 decimal), and your range forecast to have the upper and lower bound on the value of the realised inflation (also down to 1 decimal).

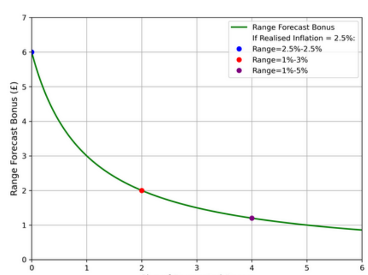
*Your bonus will be awarded for **either** your point forecast accuracy **or** your range forecast accuracy in **one** set of forecasts (Initial **or** Updated) in **one of the 6** forecasting tasks.* Here's how it works:
- Our software *randomly* selects one of your six forecasting tasks.
- For that forecasting task, the software *randomly* chooses either your Initial Forecasts or your Updated Forecasts.
- Within these selected Forecasts, the software *randomly* picks either your point forecast or your range forecast.

*Note: This means that to get a high bonus payment, you must take **both** the point and range forecasts in **all** Initial and Updated Forecasts across **all** forecasting tasks equally seriously. This is because you will only be paid for **one** randomly selected decision across the entire experiment.*

Point forecast bonus:
- Your exact forecast payment is computed like this: P = 6 * $3^{-|\text{Forecast Error}|}$
- The larger your forecast error (distance of forecasted inflation above or below the realised inflation), the less you earn.
- The figure below shows how your point forecast bonus falls, the further away your forecast is from the realised inflation value.
- The colourful dots in the figure visualise the following example: Suppose that realised inflation turns out to be **2.5%**:
  - If your point forecast of inflation was 2.5%, you would earn P = 6 * 3^{-0} = £6.00
  - If your point forecast of inflation was 2.0%, you would earn P = 6 * $3^{-0.5}$ = £3.46
  - If your point forecast of inflation was 3.0%, you would earn P = 6 * $3^{-0.5}$ = £3.46
  - If your point forecast of inflation was 1.0%, you would earn P = 6 * $3^{-1.5}$ = £1.15
  - If your point forecast of inflation was 4.0%, you would earn P = 6 * $3^{-1.5}$ = £1.15



Range forecast bonus:
- If realised inflation is inside your range forecast, you score R = 6 * [1 / (1 + total range)]
- The total range of your forecast is given by the gap between the upper bound of the range forecast and the lower bound of the range forecast.
- The larger the range you create, the less money you earn, when inflation is within your range forecast.
- If inflation falls **outside** your forecast range, you earn **nothing**
- The figure below shows how your range forecast bonus falls, the larger your forecasted range is (as long as the realised value of inflation falls within that range).
- The colourful dots in the figure visualise the following example: Suppose that realised inflation turns out to be **2.5%**:
  - If your forecasted range was between 2.5% and 2.5%, then you would earn R = 6 * [1 / (1 + 0)] = £6.00
  - If your forecasted range was between 1% and 3%, you would earn R = 6 * [1 / (1 + 2)] = £2.00
  - If your forecasted range was between 1% and 5%, you would earn R = 6 * [1 / (1 + 4)] = £1.20
  - If your forecasted range was between 0% and 2%, you would earn nothing because realised inflation is not within your range.



Note that the *"Next"* button may appear with a delay on certain pages. Similarly, parts of certain pages will be displayed only after a few seconds. Go through the experiment at your own pace and don't rush any decision.

Figure A19: Instructions (2)

Comprehension Quiz

Before starting with the first forecasting tasks, we ask that you successfully complete the following comprehension quiz. You are always allowed to consult the instructions whenever you need to (see button at the bottom of the page).

If you get an answer wrong, you will be told so, and will have the opportunity to correct your answer. If you submit the quiz three times with at least one wrong answer, we will end the experiment early.

Your performance on this quiz does not affect your payment in any way, but you will not be able to proceed to the forecasting tasks until you have correctly answered all of the quiz questions.

1. True or False? Your goal in the experiment is to forecast inflation as accurately as possible.

○ True ○ False

2. True or False? For your bonus payment the software randomly selects a single forecasting task, of which it further randomly selects the Initial Forecasts or the Updated Forecasts, and finally it randomly selects either your point or your range forecast.

○ True ○ False

3. If inflation turns out to be 5.0% in the period selected for bonus payment, and your forecasted range is between -0.2% and 4.7%, how much extra payment will you get for your range?

-------- ⌄

4. True or False? If inflation turns out to be 2.2%, and your point forecast (that was selected for bonus payment) is 1.0%, your bonus payment is less than if you had forecasted 0.3%.

○ True ○ False

5. How many forecasting tasks will you have to do?

-------- ⌄

Next

Show instructions

Figure A20: Comprehension Quiz

Notes: Participants must answer all questions correctly within three attempts to proceed. Those who fail are excluded from the experiment.

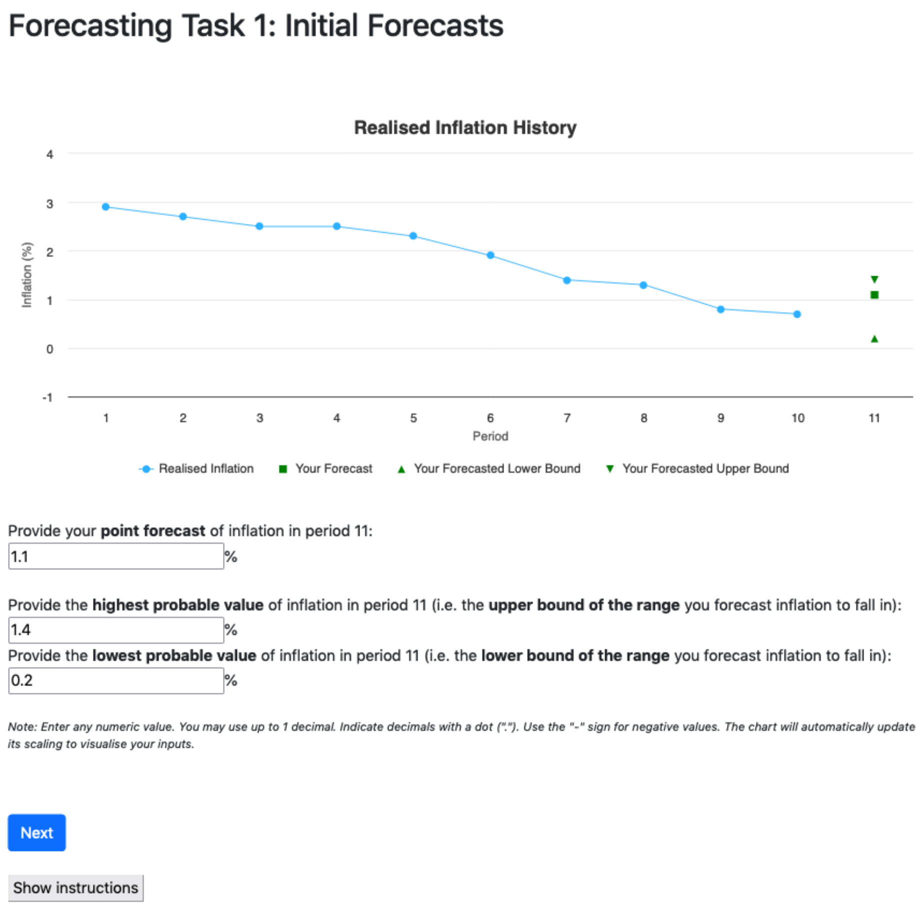


Figure A21: Prior elicitation

Notes: Participants are required to spend at least ten seconds on this page to reduce the risk of rushing without processing the information.

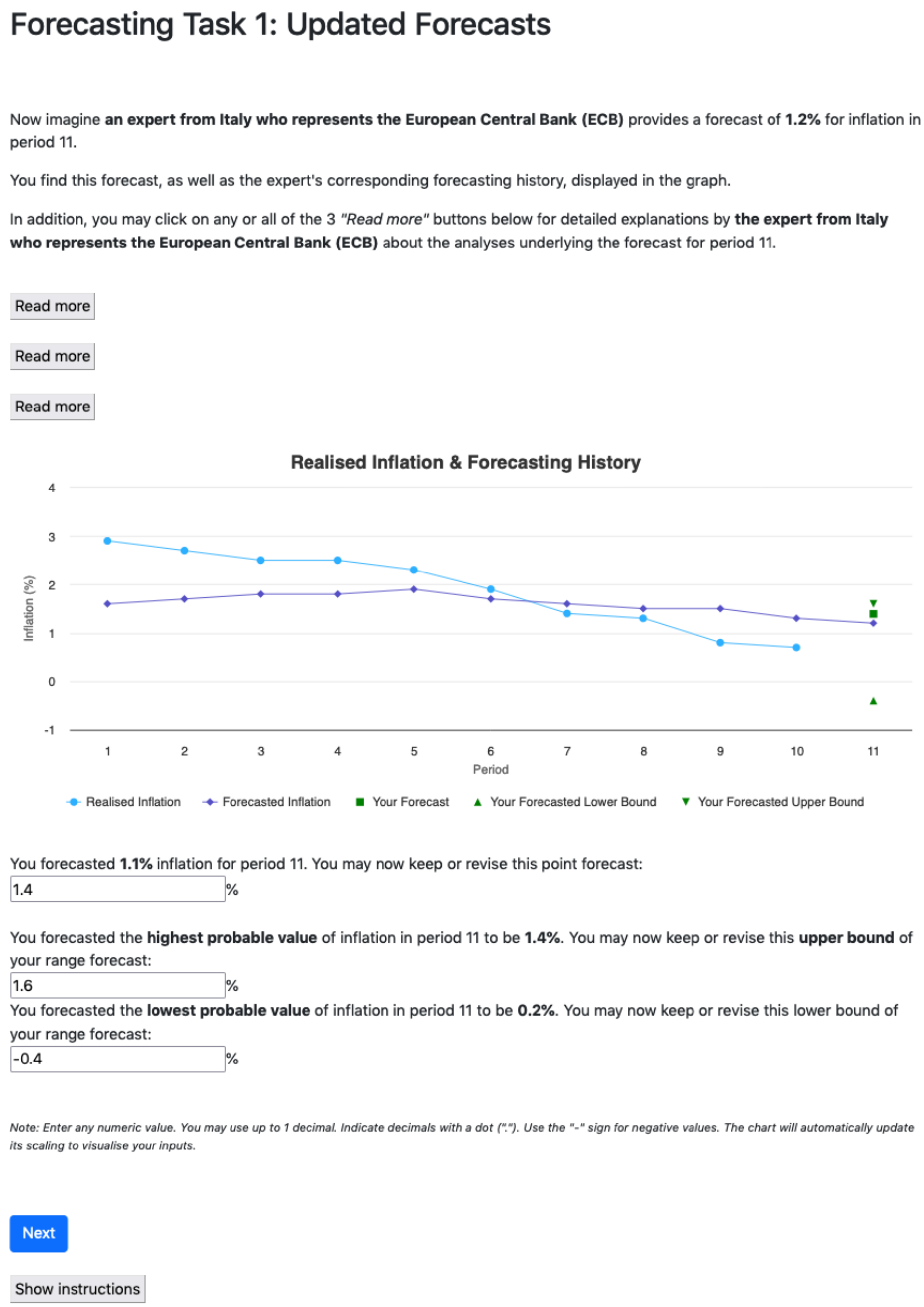


Figure A22: Posterior elicitation

Notes: The first sentence of this page is shown in isolation for 5 seconds before the rest of the page appears to ensure participants attend to the messenger and the signal. Participants are then required to spend at least another 10 seconds on this page to reduce the risk of rushing decisions without paying attention to the provided information. Hovering over the dots in the graph reveals exact values of realized inflation and inflation forecasts.

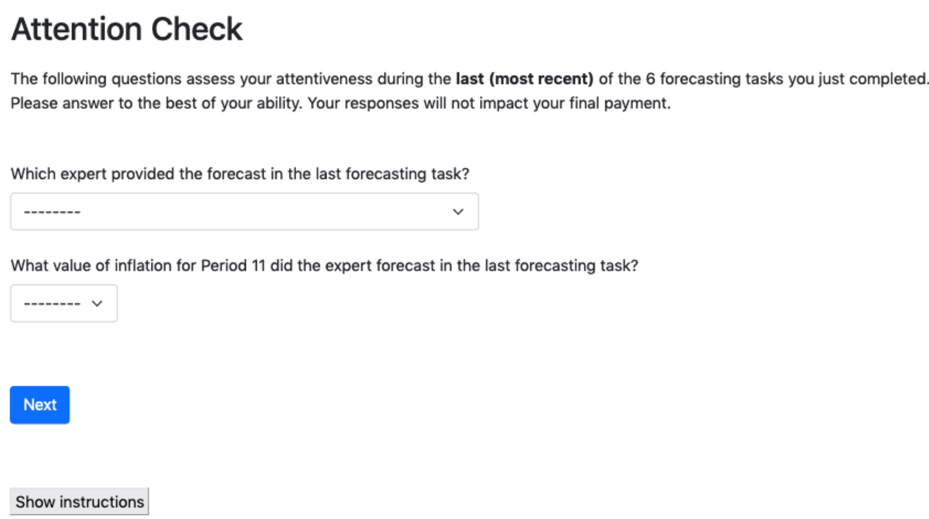


Figure A23: Attention Check

Notes: The order of answer options for experts is randomized. The options for period 11 values reflect actual signal values of the six forecasting tasks.

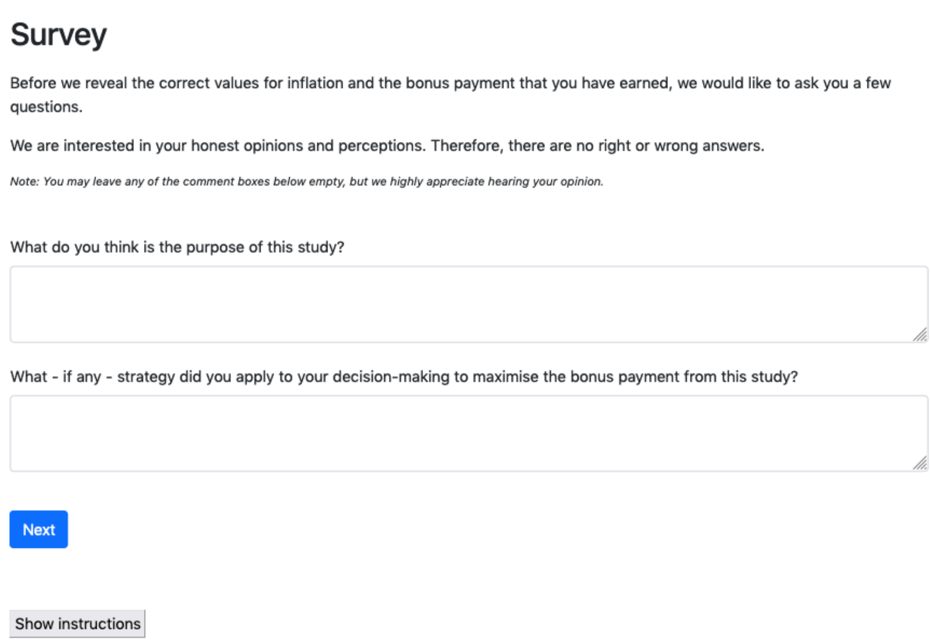
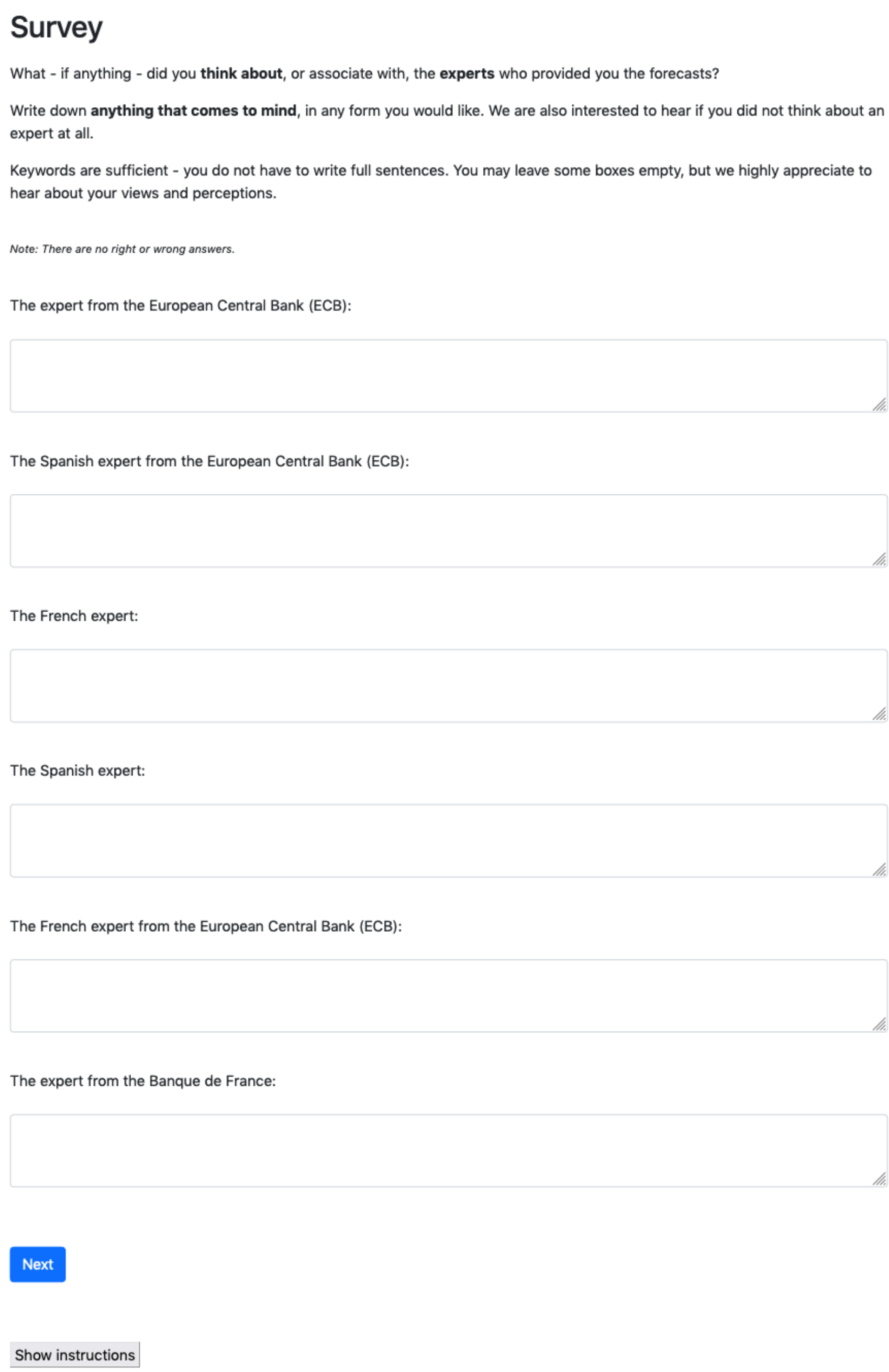


Figure A24: Survey: Purpose of Experiment & Strategy

Notes: Participants are allowed to skip any or all of these questions entirely.



Survey

What - if anything - did you **think about**, or associate with, the **experts** who provided you the forecasts?

Write down **anything that comes to mind**, in any form you would like. We are also interested to hear if you did not think about an expert at all.

Keywords are sufficient - you do not have to write full sentences. You may leave some boxes empty, but we highly appreciate to hear about your views and perceptions.

Note: There are no right or wrong answers.

The expert from the European Central Bank (ECB):

The Spanish expert from the European Central Bank (ECB):

The French expert:

The Spanish expert:

The French expert from the European Central Bank (ECB):

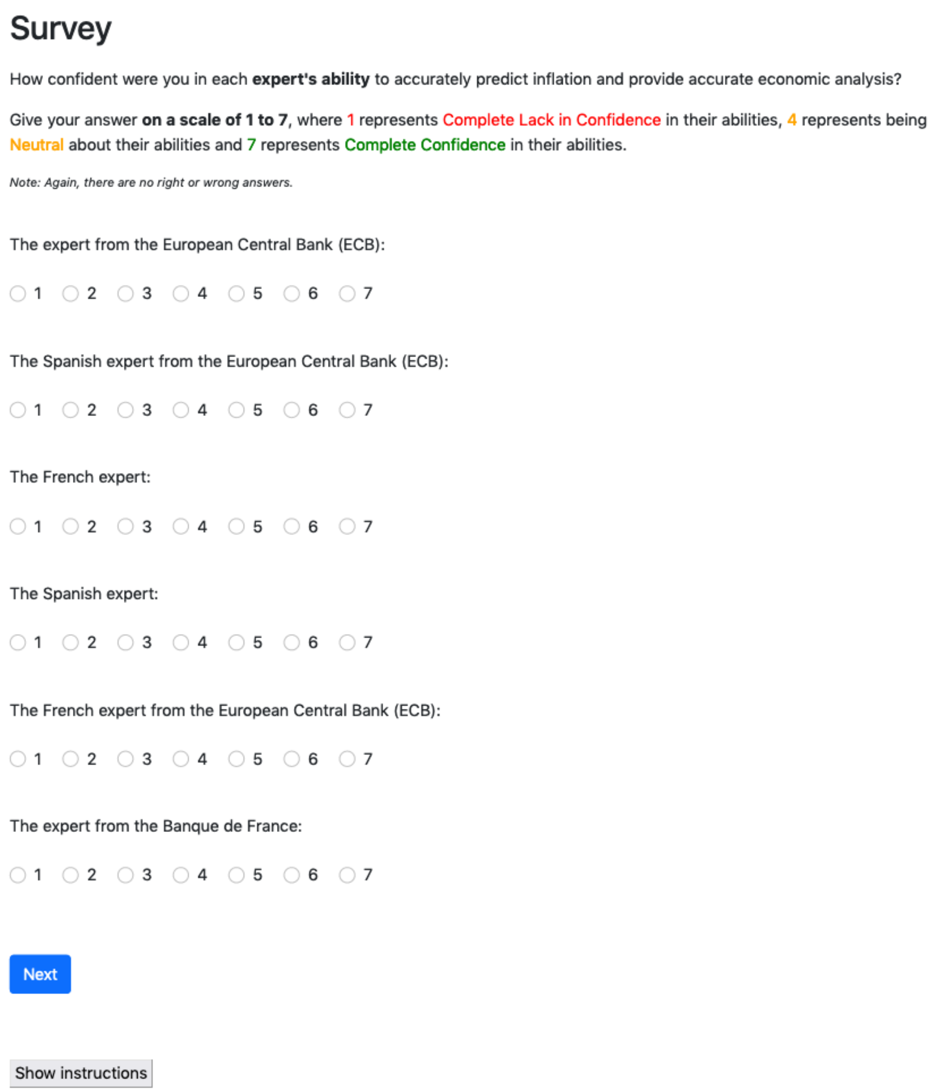
The expert from the Banque de France:

Next

Show instructions

Figure A25: Survey: Association

Notes: Participants are allowed to skip any or all of these questions entirely. To make it easier for participants to recall, the order of experts represents the occurrence of experts in forecasting tasks (i.e., randomized across individuals).



Survey

How confident were you in each **expert's ability** to accurately predict inflation and provide accurate economic analysis?

Give your answer **on a scale of 1 to 7**, where 1 represents Complete Lack in Confidence in their abilities, 4 represents being Neutral about their abilities and 7 represents Complete Confidence in their abilities.

Note: Again, there are no right or wrong answers.

The expert from the European Central Bank (ECB):

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The Spanish expert from the European Central Bank (ECB):

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The French expert:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The Spanish expert:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The French expert from the European Central Bank (ECB):

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The expert from the Banque de France:

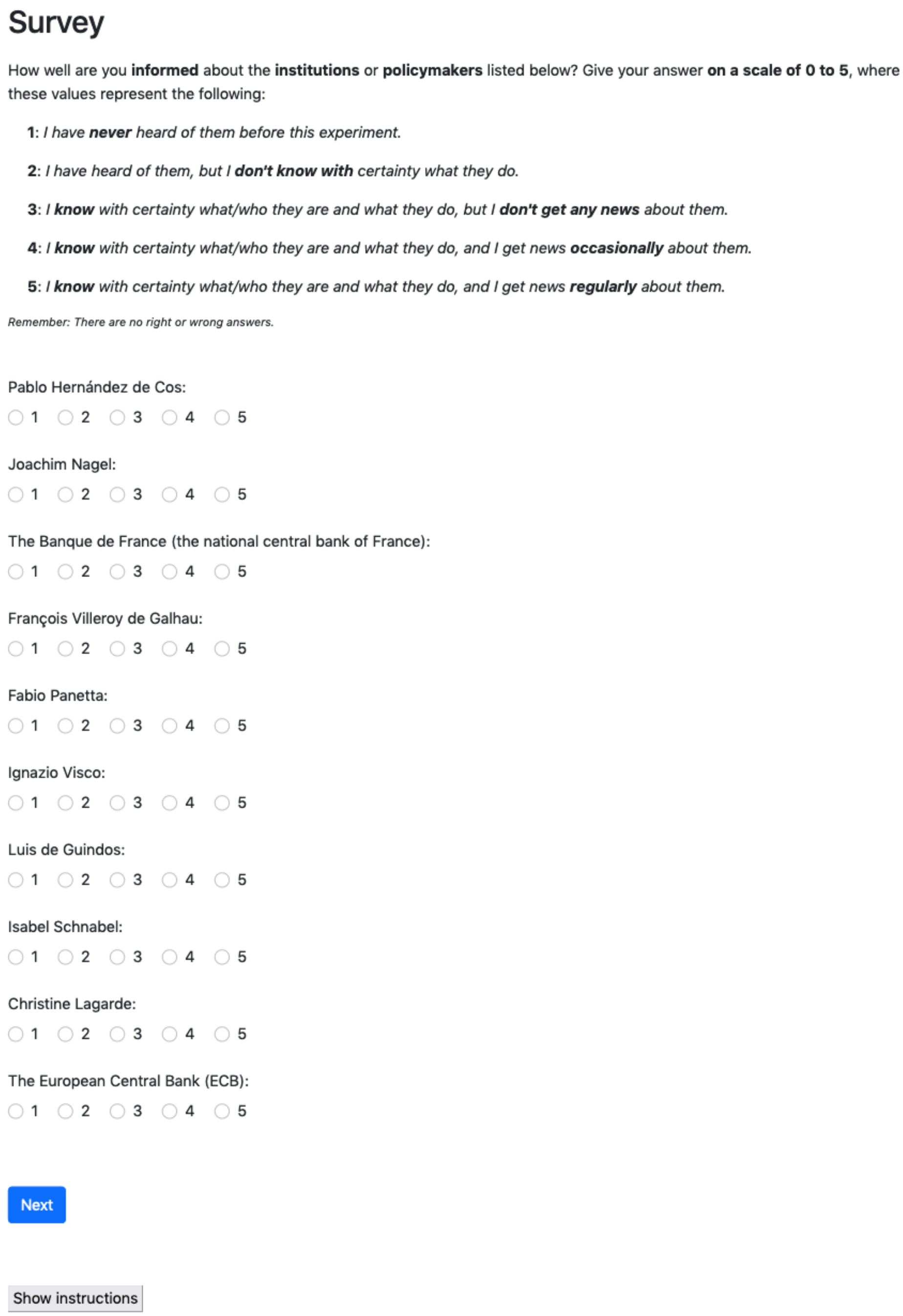
○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Next

Show instructions

Figure A26: Survey: Perceived Ability

Notes: Order of experts represents occurrence of experts in forecasting tasks (i.e., randomized across individuals).



Survey

How well are you **informed** about the **institutions** or **policymakers** listed below? Give your answer **on a scale of 0 to 5**, where these values represent the following:

1: *I have* ***never*** *heard of them before this experiment.*

2: *I have heard of them, but I* ***don't know with*** *certainty what they do.*

3: *I* ***know*** *with certainty what/who they are and what they do, but I* ***don't get any news*** *about them.*

4: *I* ***know*** *with certainty what/who they are and what they do, and I get news* ***occasionally*** *about them.*

5: *I* ***know*** *with certainty what/who they are and what they do, and I get news* ***regularly*** *about them.*

Remember: There are no right or wrong answers.

Pablo Hernández de Cos:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Joachim Nagel:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

The Banque de France (the national central bank of France):
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

François Villeroy de Galhau:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Fabio Panetta:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Ignazio Visco:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Luis de Guindos:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Isabel Schnabel:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

Christine Lagarde:
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

The European Central Bank (ECB):
○ 1 ○ 2 ○ 3 ○ 4 ○ 5

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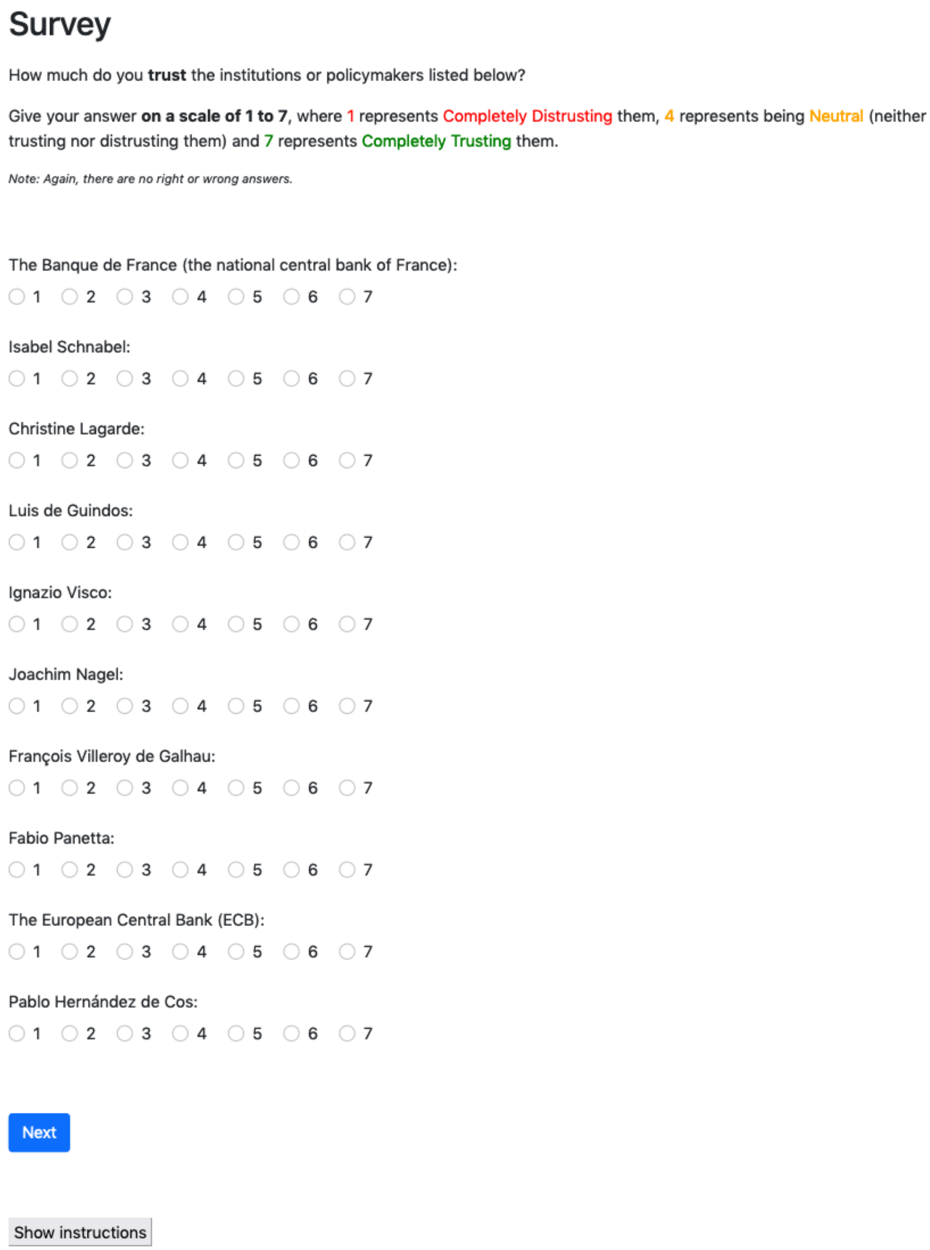
Figure A27: Survey: Exposure

Notes: The order in which institutions and policymakers appear is randomized at the participant level.



Survey

How much do you **trust** the institutions or policymakers listed below?

Give your answer **on a scale of 1 to 7**, where 1 represents Completely Distrusting them, 4 represents being Neutral (neither trusting nor distrusting them) and 7 represents Completely Trusting them.

Note: Again, there are no right or wrong answers.

The Banque de France (the national central bank of France):

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Isabel Schnabel:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Christine Lagarde:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Luis de Guindos:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Ignazio Visco:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Joachim Nagel:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

François Villeroy de Galhau:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Fabio Panetta:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

The European Central Bank (ECB):

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

Pablo Hernández de Cos:

○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7

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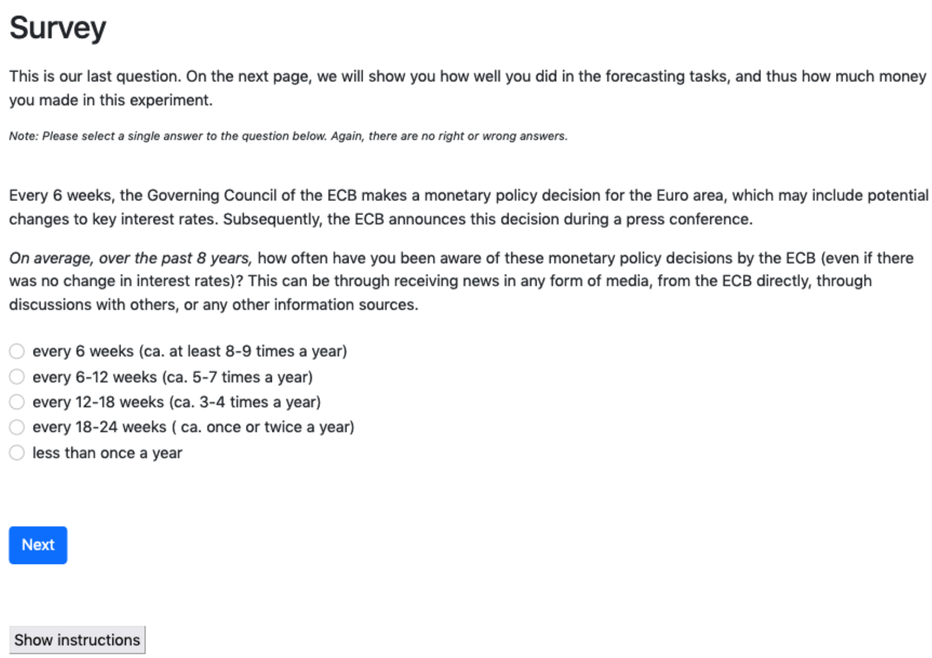
Figure A28: Survey: Trust

Notes: This page is only shown to participants who indicate knowing at least one institution or policymaker. Only institutions and policymakers that the participant indicates knowing appear in this list, since indicated trust is only meaningful if participants know the institutions and policymakers. The order in which institutions and policymakers appear is randomized at the participant level.



Survey

This is our last question. On the next page, we will show you how well you did in the forecasting tasks, and thus how much money you made in this experiment.

Note: Please select a single answer to the question below. Again, there are no right or wrong answers.

Every 6 weeks, the Governing Council of the ECB makes a monetary policy decision for the Euro area, which may include potential changes to key interest rates. Subsequently, the ECB announces this decision during a press conference.

On average, over the past 8 years, how often have you been aware of these monetary policy decisions by the ECB (even if there was no change in interest rates)? This can be through receiving news in any form of media, from the ECB directly, through discussions with others, or any other information sources.

- every 6 weeks (ca. at least 8-9 times a year)
- every 6-12 weeks (ca. 5-7 times a year)
- every 12-18 weeks (ca. 3-4 times a year)
- every 18-24 weeks (ca. once or twice a year)
- less than once a year

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Figure A29: Survey: Monetary Policy Expertise

Notes: The question allows for potential comparison of experimental and Twitter results, distinguishing between experts and non-experts. With some caveats, answers to this question can be seen as an imperfect proxy for monetary policy expertise.

Realised Inflation & Your Performance

We now reveal the realised values for inflation in period 11, your corresponding forecasts, and the bonus payment that you will earn, if the software selects a forecast.

Forecasting Task 1: The realised inflation was 2.1%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.0%	**£1.79**	1.0%	**£1.79**
***Range** Forecasts*	-0.2%-1.4%	**£0.0**	1.0%-2.0%	**£0.0**

Forecasting Task 2: The realised inflation was 2.2%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.1%	**£1.79**	1.0%	**£1.61**
***Range** Forecasts*	1.1%-2.9%	**£2.14**	-124.0%-1.0%	**£0.0**

Forecasting Task 3: The realised inflation was 1.0%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.1%	**£5.38**	1.0%	**£6.0**
***Range** Forecasts*	0.0%-2.9%	**£1.54**	1.0%-1.0%	**£6.0**

Forecasting Task 4: The realised inflation was 1.1%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.1%	**£6.0**	1.0%	**£5.38**
***Range** Forecasts*	-0.2%-2.2%	**£1.76**	1.0%-1.0%	**£0.0**

Forecasting Task 5: The realised inflation was 1.7%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.6%	**£5.38**	1.6%	**£5.38**
***Range** Forecasts*	0.0%-2.9%	**£1.54**	0.4%-2.5%	**£1.94**

Forecasting Task 6: The realised inflation was 0.6%.

	Initial Forecasts		**Updated** Forecasts	
	Your Forecasts	*Bonus Payments*	*Your Forecasts*	*Bonus Payments*
***Point** Forecasts*	1.0%	**£3.87**	1.0%	**£3.87**
***Range** Forecasts*	1.0%-1.0%	**£0.0**	1.0%-1.0%	**£0.0**

Click the *"Next"* button below for the software to randomly select the forecast for your bonus payment.

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Figure A30: Payoff Overview

Notes: Realized inflation values for period 11 and the corresponding performance of participants are only shown at the very end of the experiment and after all survey questions to avoid learning effects between forecasting tasks and performance-driven biases to survey answers asked after the experiment.

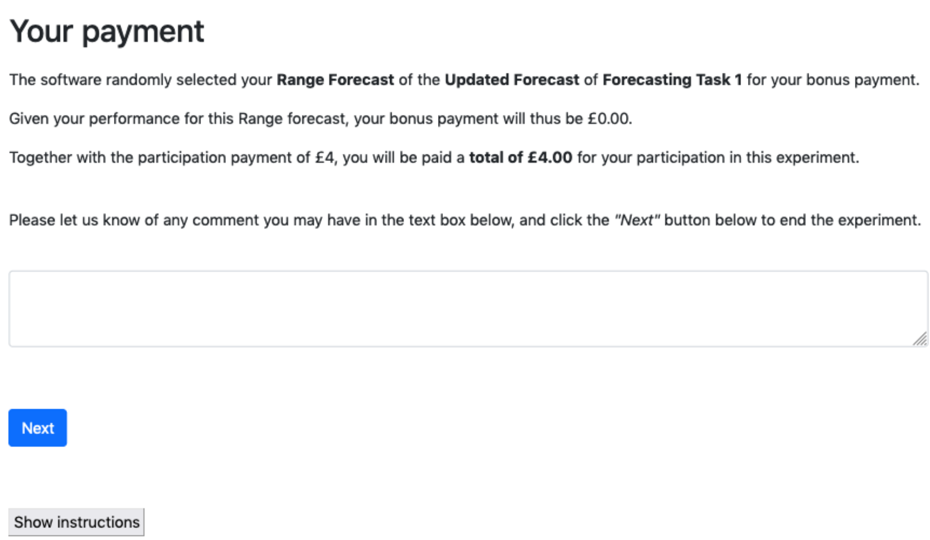


Figure A31: Payoff Reveal

Notes: The software reveals the randomly selected decision for each participant's bonus payment.

B.4 Details on Estimation and Results

Bayesian Belief Updating Framework. To assess how agents update their inflation expectations following new forecast information, I use a standard Bayesian belief updating framework. This framework estimates how agents balance novel information (the signal) against their existing beliefs (priors). In the Bayesian case, agents weigh information and priors equally when both have the same precision.

Formally, let participant i's prior belief about inflation x be $x \sim \mathcal{N}(A_i, \alpha_i^{-1})$. The signal is $B = x + e$, with $e \sim \mathcal{N}(0, \beta^{-1})$. Bayes' law implies

$$E_i[x|B] = \frac{\alpha_i A_i + \beta B}{\alpha_i + \beta}, \tag{21}$$

i.e., the posterior is a precision-weighted average of prior and signal. As discussed in the main text, EA inflation is non-normal and forecast errors show mild autocorrelation, so the Bayesian case serves as a benchmark; deviations are interpreted comparatively across treatments.

Average signal use. Table A14 shows that participants in all countries under-use the signal compared to a Bayesian benchmark across treatments.

Table A14: Average Signal Use by Nationality

	(1) Overall	(2) Germany	(3) Spain	(4) France	(5) Italy
Signal	0.899***	0.881***	0.889***	0.877***	0.941***
	(0.012)	(0.025)	(0.025)	(0.024)	(0.026)
Prior	1.206***	1.234***	1.198***	1.262***	1.139***
	(0.012)	(0.025)	(0.022)	(0.025)	(0.025)
Observations	2,385	600	594	593	598
R^2	0.961	0.961	0.963	0.964	0.959

Notes: OLS regressions without an intercept (estimating Eq. (1)). Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table A15: Endogenous Attention

Dep.:	OLS Attention (Continuous)	Probit Attention (Binary)	Marginal Effects Attention (Binary)
Inflation Level	0.921***	0.864***	0.326***
	(0.090)	(0.136)	(0.050)
Inflation Uncertainty	0.701***	0.644***	0.243***
	(0.077)	(0.114)	(0.042)
Signal Precision	0.179***	0.164***	0.062***
	(0.033)	(0.048)	(0.018)
Messenger Treatments			
Outgroup Expert	0.059	0.047	
	(0.063)	(0.091)	
ECB Ingroup Expert	-0.053	-0.063	
	(0.063)	(0.091)	
ECB Outgroup Expert	-0.029	-0.042	
	(0.063)	(0.091)	
ECB Expert	-0.064	-0.050	
	(0.063)	(0.091)	
NCB Expert	-0.055	-0.040	
	(0.063)	(0.091)	
Constant	-1.117**	-2.193***	
	(0.467)	(0.439)	
Treatment Order	✓	✓	✓
Individual-FE	✓		
Observations	2,394	2,394	2,394
R^2	0.692		
Pseudo R^2		0.023	

Notes: OLS regression in column (1) with the number of additional information pieces requested (buttons clicked) as dependent variable. Probit regression in column (2) with a binary outcome equal to 1 if at least one information piece is requested and 0 otherwise as dependent variable. Marginal effects corresponding to probit in column (3). Inflation level is the average inflation over the ten periods in a scenario; inflation uncertainty is the standard deviation over those periods. The reference category for messenger treatments is the *Nationality-Only Ingroup Expert*. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Attention and Ingroup Effects. When participants request additional information, they show stronger ingroup effects across hypotheses. I attribute this to these participants being attentive. In fact, ingroup effects disappear when limiting the sample to forecasting tasks where no buttons are revealed (i.e., "no attention"), highlighting that treatments might not have been salient to these inattentive participants. In turn, effects become stronger the more attention individuals pay (i.e., the more buttons they reveal). Note that since attention is not exogenous in my experiment, these effects are not strictly causal. For instance, a lack of attention could also represent low effort exertion, resulting in missing the treatment variation.

Two robustness exercises show that it is indeed attention – not the additional information – that drives ingroup effects on signal use. Column (2) in Table A16 compares only forecasting tasks where all information is retrieved and confirms effects (except that the ingroup effect within ECB context loses significance). Similarly, column (3) confirms robustness of all effects with the full sample, controlling for revealed buttons (for given inflation scenarios).

Table A16: The Role of Attention and Being Informed

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure Ingroup Effect (H1):								
Ingroup Expert - Outgroup Expert	0.052***	0.066***	0.056***	0.057**	0.039			
	(0.017)	(0.025)	(0.018)	(0.024)	(0.036)			
Observations	795	463	795	488	307			
R^2	0.994	0.995	0.994	0.995	0.995			
Ingroup Effect Within Institutional Context (H2):								
ECB Ingroup Expert - ECB Outgroup Expert	0.028*	0.029	0.029*	0.018	0.033	0.053	-0.006	-0.037
	(0.017)	(0.025)	(0.017)	(0.027)	(0.035)	(0.044)	(0.064)	(0.107)
Observations	795	446	795	465	330	315	193	122
R^2	0.994	0.996	0.994	0.995	0.995	0.998	0.999	0.999
Homophily (H3):								
Ingroup ECB Expert - Neutral ECB Expert	0.035**	0.083***	0.040**	0.081***	-0.007	0.051**	0.110***	-0.030
	(0.018)	(0.026)	(0.018)	(0.025)	(0.032)	(0.022)	(0.031)	(0.042)
Observations	794	437	794	462	332	612	361	251
R^2	0.993	0.995	0.994	0.995	0.995	0.996	0.997	0.998
Heterophobia (H3):								
Outgroup ECB Expert - Neutral ECB Expert	0.013	0.059**	0.007	0.057**	-0.032	-0.004	0.047	-0.251**
	(0.017)	(0.023)	(0.017)	(0.022)	(0.036)	(0.033)	(0.040)	(0.097)
Observations	795	441	795	465	330	477	284	193
R^2	0.994	0.996	0.994	0.996	0.995	0.998	0.999	1.000
Institutional Effect (H4):								
Neutral NCB Expert - Neutral ECB Expert	0.034**	0.058**	0.038**	0.057**	0.005	0.033*	0.052**	0.006
	(0.017)	(0.025)	(0.018)	(0.024)	(0.034)	(0.017)	(0.024)	(0.035)
Observations	795	433	795	464	331	779	455	324
R^2	0.994	0.995	0.994	0.995	0.995	0.994	0.996	0.995
Inflation Scenario	✓	✓	✓	✓	✓	✓	✓	✓
Individual-FE	✓	✓	✓	✓	✓	✓	✓	✓
Attention		Full	Controlled	Yes	No		Yes	No
Knowing PMs/Inst						Yes	Yes	Yes

Notes: Effects compare coefficients within the same OLS regressions. *Inflation Scenario* denotes controls for scenario and messenger order; all specifications include individual fixed effects, as in the main specification. *Attention–Full* restricts to tasks with all information revealed; *Attention–Controlled* keeps the full sample and controls for revealed buttons; *Attention–Yes* denotes the subsample where at least one button is clicked; *Attention–No* denotes the subsample without any revealed information. *Knowing PMs/Inst* indicates at least having heard of the relevant institutions or policymakers. Observations are for forecasting tasks and vary slightly across treatments due to instances of infinite prior precision or unfamiliarity with PMs/Inst. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Interestingly, attention further highlights another potential explanation of ingroup effects within the institutional context. When representative ECB policymakers are known but attention is low, there is substantial heterophobia (-0.251 (p-value < 0.05)) This suggests that nationality may be used as a shortcut. Conversely, homophily is very large (0.110 (p-value < 0.01)) when ECB policymakers are known and participants are attentive (see columns (6)-(8) in Table A16).

Information Availability: Exposure to News in the Media. Survey responses indicate greater reach for ingroup policymakers. Table A17 shows higher probabilities of having heard of, knowing, and following news about ingroup policymakers compared to similar outgroup policymakers.[43] The likelihood of knowing policymakers increases by roughly a third if they are in the ingroup rather than outgroup. Participants are 29% (p-value < 0.01) more likely to follow news of a given policymaker if they are in the ingroup.

Table A17: Information Reach: Survey Evidence

	(1) Heard Of	(2) Know	(3) Informed
Ingroup	0.080**	0.271***	0.286***
	(0.035)	(0.031)	(0.024)
Constant	0.489***	0.175***	0.020
	(0.025)	(0.022)	(0.017)
Observations	798	798	798
R^2	0.01	0.09	0.15

Notes: OLS regressions. Representative in- and outgroup policymakers are ECB board members (Lagarde, de Guindos, Schnabel, Panetta); results for NCB governors (Villeroy de Galhau, de Cos, Nagel, Visco) are similar. The *Ingroup* dummy captures the probability for ingroup relative to outgroup policymakers. Data come from the survey question shown in Figure A27. Standard errors in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Figure A32 plots this by participant nationality. Effects for ECB ingroup policymakers seem to be driven by the French and Spanish – coinciding with the nationalities of the ECB president and vice president. This suggests that nationality strongly matters, especially for these two ECB Board positions.

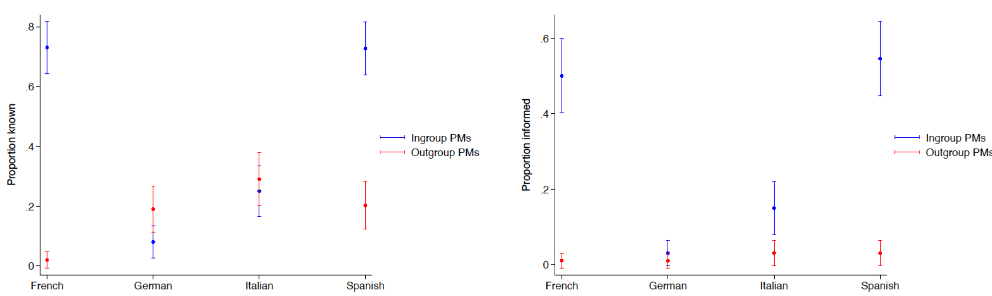


Figure A32: Knowledge of Policymakers and News Exposure

Notes: Shares knowing (left) and receiving news (right) for ingroup vs. outgroup ECB board members. The average response across all three outgroup members is used. Plots for NCB governors are similar.

[43] As described earlier in the paper, the policymakers used to proxy for messengers are NCB governors (Villeroy de Galhau, de Cos, Nagel, Visco) and ECB board members (Lagarde, de Guindos, Schnabel, Panetta).

B.5 Real-world Forecasting

The experimental findings suggest that ingroup agents update more strongly to a signal; thus their inflation forecasts should – ceteris paribus – more closely resemble the forecasts by the ECB. To check whether this is indeed the case, I compare the distance between the ECB's 12 months-ahead inflation forecasts to forecasts of households in the EA. Values for the former are taken from the ECB's Macroeconomic Projection Database (MPD). The MPD is published four times a year (in March, June, September and December).[44] The latter come from the ECB's Consumer Expectations Survey (CES), which is a representative survey of consumers in selected EA countries started in 2020. I use their aggregated monthly mean and median indicators of quantitative household inflation expectations over the next 12 months.[45]

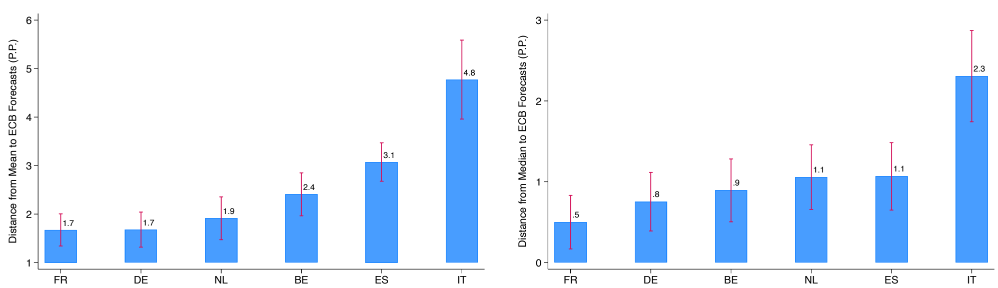


Figure A33: Comparing Household Inflation Forecasts to ECB Projections

Notes: The left (right) panel shows the difference between mean (median) household twelve months-ahead inflation forecasts and ECB projection (lagged by 1 month) with 95% confidence intervals. I consider fifteen survey waves for six countries, whereby data for Belgium are missing for the first wave (April 2020).

Figure A33 shows that forecasts of 1 year-ahead inflation by households in France indeed most closely resembled ECB's forecasts (lagged by 1 month).[46] Available data spans from April 2020 until December 2023, when the ECB's president was French (Christine Lagarde). During this time, the mean forecasts for French households are on average 1.7 percentage points above the ECB's projections, the lowest distance across countries (together with German households). Across countries, mean household forecasts are on average 2.6 percentage points above ECB forecasts. Similarly, French median household forecasts are most similar to ECB forecasts (0.5 percentage point distance), with a cross-country average distance of 1.1 percentage points. While these findings are correlations rather than causal effects, the trend is in line with ingroup agents updating more closely to the signal.

[44] The MPD contains information on the outlook for the EA and contributes to the ECB Governing Council's assessment of economic developments and risks to price stability. The June and December projections are conducted by Eurosystem staff, while the March and September projections are conducted by ECB staff.

[45] The survey provides mean and median values that are population-weighted and winsorised at the 2nd and 98th percentiles of the weighted distribution of responses for each survey wave and country.

[46] Not lagging ECB's forecast results in very similar findings.

C Model Details

C.1 Threshold for Public Signal Availability

To model the variability in agents' thresholds, I use the tractable and convenient truncated normal distribution. Each outgroup agent's threshold d_o follows a truncated normal distribution $\mathcal{N}_+(0,1)$ on $[0,\infty)$, which ensures thresholds are non-negative. The probability density function (PDF) of d_o is:

$$f_{d_o}(d) = \frac{\phi(d)}{\Phi(\infty) - \Phi(0)} = 2\phi(d), \quad \text{for } d \geq 0, \tag{22}$$

where $\phi(d)$ and $\Phi(d)$ are the standard normal PDF and cumulative distribution function (CDF), respectively. The cumulative distribution function (CDF) of d_o is

$$F_{d_o}(d) = \int_0^d f_{d_o}(z)\, dz = 2\Phi(d) - 1. \tag{23}$$

This CDF determines the fraction of outgroup agents who observe Y based on their individual thresholds.

C.2 Proofs and Derivations

C.2.1 Proof of Lemma 1: Optimal Signal Extraction Weights

Lemma 1 claims that the optimal weights κ_h for ingroup and informed outgroup agents are given by

$$\kappa_g^* = \frac{\tau_y q}{\tau_Y + \tau_y q} \quad \text{and} \quad \kappa_o^* = \frac{\tau_y q}{\rho_{im}\tau_Y + \tau_y q},$$

where $q = (1 - r + r(1-\alpha)(1-A))$. Below is the corresponding proof of the optimal signal extraction weights by agent type.

Uninformed Outgroup Agents An uninformed outgroup agent simply chooses $a_{io}(y_i) = y_i$. Since Y is unobserved by this agent, he relies on $E[Y] = x$, so he uses $E_{io}[x|y_i] = y_i$ for any Y. To derive the action of informed agents, suppose that the population, consisting of the different agent types h, follows a linear strategy of the form

$$a_{jh} = \kappa_h y_j + (1 - \kappa_h)Y. \tag{24}$$

Ingroup Agents An ingroup agent g, who always receives the public signal, updates without bias and expects all agents in the economy to do the same. She assigns the same signal extraction weight κ_g to both ingroup agents and informed outgroup agents who receive Y, assuming they process the signal without bias and unaware of heterogeneity in information processing. Knowing the fraction of outgroup agents A that receive the public signal and the overall share of outgroup agents $(1-\alpha)$, her expected average action of the population is

$$\begin{aligned}
E_{ig}[\bar{a}|y_i, Y] &= [\alpha\kappa_g + (1-\alpha)A\kappa_g + (1-\alpha)(1-A)]\, E_{ig}[y_j|y_i, Y] \\
&+ [\alpha(1-\kappa_g) + (1-\alpha)A(1-\kappa_g)]\, Y \\
&= [\alpha\kappa_g + (1-\alpha)A\kappa_g + (1-\alpha)(1-A)]\, \frac{\tau_y y_i + \tau_Y Y}{\tau_y + \tau_Y} \\
&+ [\alpha(1-\kappa_g) + (1-\alpha)A(1-\kappa_g)]\, Y \\
&= Y\left[(1-\kappa_g)\,[\alpha + (1-\alpha)A] + \frac{\tau_Y}{\tau_y + \tau_Y}\,[\kappa_g\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right] \\
&+ y_i\left[\frac{\tau_y}{\tau_y + \tau_Y}\,[\kappa_g\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right].
\end{aligned} \tag{25}$$

Thus, the ingroup agent g's optimal action is given by

$$\begin{aligned}
a_{ig}(y_i, Y) &= (1-r)E_{ig}[x|y_i, Y] + rE_{ig}[\bar{a}|y_i, Y] \\
&= r\left\{Y\left[(1-\kappa_g)\,(\alpha + (1-\alpha)A) + \frac{\tau_Y}{\tau_y + \tau_Y}\,[\kappa_g\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right]\right. \\
&+ \left. y_i\left[\frac{\tau_y}{\tau_y + \tau_Y}\,[\kappa_g\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right]\right\} + (1-r)\frac{\tau_y y_i + \tau_Y Y}{\tau_y + \tau_Y} \\
&= \left[\frac{\tau_y}{\tau_y + \tau_Y}(1 - r + r[\kappa_g(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right] y_i \\
&+ \left[1 + \frac{\tau_y}{\tau_y + \tau_Y}\,[r[-\kappa_g(\alpha + (1-\alpha)A) + \alpha + (1-\alpha)A] - 1]\right] Y.
\end{aligned} \tag{26}$$

Comparing coefficients with the population's strategy (Eq. (24)) gives

$$\kappa_g = \frac{\tau_y}{\tau_y + \tau_Y}\,[1 - r + r\,[\kappa_g\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]], \tag{27}$$

from which I can solve for κ_g

$$\kappa_g{*} = \frac{\tau_y q}{\tau_Y + \tau_y q}. \tag{28}$$

Informed Outgroup Agents The informed outgroup agent o – who receives both signals but updates with a bias – looks very similar to the ingroup agent but down-weights the public signal by the resonance weight ρ_{im}. His expected average action of the population is

$$
\begin{aligned}
E_{io}[\overline{a}|y_i, Y] &= [\alpha\kappa_o + (1-\alpha)A\kappa_o + (1-\alpha)(1-A)]\, E_{io}[y_j|y_i, Y] \\
&+ [\alpha(1-\kappa_o) + (1-\alpha)A(1-\kappa_o)]\, Y \\
&= \alpha[\kappa_o E_{io}[y_j|y_i, Y] + (1-\kappa_o)Y] + (1-\alpha)A[\kappa_o E_{io}[y_j|y_i, Y] + (1-\kappa_o)Y] \\
&+ (1-\alpha)(1-A)E_{io}[y_j|y_i, Y] \\
&= Y\left[(1-\kappa_o)\,[\alpha + (1-\alpha)A] + \frac{\rho_{im}\tau_Y}{\tau_y + \rho_{im}\tau_Y}\,[\kappa_o\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right] \\
&+ y_i\left[\frac{\tau_y}{\tau_y + \rho_{im}\tau_Y}\,[\kappa_o\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right].
\end{aligned}
\tag{29}
$$

Thus, the informed outgroup agent o's optimal action is given by

$$
\begin{aligned}
a_{io}(y_i, Y) &= (1-r)E_{io}[x|y_i, Y] + rE_{io}[\overline{a}|y_i, Y] \\
&= \left[\frac{\tau_y}{\tau_y + \rho_{im}\tau_Y}(1 - r + r[\kappa_o(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]\right] y_i \\
&+ \left[1 + \frac{\tau_y}{\tau_y + \rho_{im}\tau_Y}\,[r[-\kappa_o(\alpha + (1-\alpha)A) + \alpha + (1-\alpha)A] - 1]\right] Y.
\end{aligned}
\tag{30}
$$

Comparing coefficients with the population's strategy (Eq. (24)), I get for the informed outgroup agent

$$
\kappa_o = \frac{\tau_y}{\tau_y + \rho_{im}\tau_Y}\,[1 - r + r\,[\kappa_o\,(\alpha + (1-\alpha)A) + (1-\alpha)(1-A)]]\,, \tag{31}
$$

from which I can solve for κ_o

$$
\kappa_o* = \frac{\tau_y q}{\rho_{im}\tau_Y + \tau_y q}. \tag{32}
$$

Therefore, the optimal weights κ_h for ingroup and informed outgroup agents are as stated in Lemma 1.

C.2.2 Proof of Proposition 1: Uniqueness of the Linear Equilibrium

Proposition 1 claims that the equilibrium strategies derived constitute the unique linear equilibrium of the model. *Proof:* To prove that the equilibrium strategy is indeed a linear combination of the available signals, and that the equilibrium is therefore unique, I extend the proof by Morris and Shin (2002) to accommodate heterogeneous agent types with biased information processing. The optimal action of agents is given by Eq. (10), and the average action is given in Eq. (15), which can also be written as

$$
\begin{aligned}
\bar{a} &= \int_0^\alpha a_{ig}(y_i, Y)\, di + A\int_\alpha^1 a_{io}(y_i, Y)\, di + (1-A)\int_\alpha^1 a_{io}(y_i)\, di \\
&= \alpha\bar{a}_{ig}(x, Y) + (1-\alpha)A\bar{a}_{io}(x, Y) + (1-\alpha)(1-A)x.
\end{aligned}
\tag{33}
$$

Integrating over the interval $[0, \alpha]$ for ingroup agents and $[\alpha, 1]$ for outgroup agents returns the average action as a weighted sum of the expected actions of each group, given the symmetry and independence of agents within each group. The integrals over outgroup agents are weighted by A and $(1 - A)$, which represent the fractions of informed and uninformed agents, respectively. Remember that agents expect other informed agents to update and act just like themselves, as agents are naive about their own biases and those of others. This means that ingroup agents assume the average action of informed agents is $\bar{a}_i(x, Y) = \bar{a}_{ig}(x, Y)$, while informed outgroup agents assume $\bar{a}_i(x, Y) = \bar{a}_{io}(x, Y)$ across all informed agents. The average action of uninformed outgroup agents is known by all and is $\bar{a}_{io}(x) = x$, as defined earlier. Hence, the optimal strategy of any informed agent of type h is

$$\begin{aligned} a_{ih}(y_i, Y) &= qE_{ih}[x|y_i, Y] + [r(\alpha + (1-\alpha)A)]E_{ih}[\bar{a}|y_i, Y] \\ &= qE_{ih}[x|y_i, Y] + [r(\alpha + (1-\alpha)A)]qE_{ih}[\bar{E}(x)] + [r(\alpha + (1-\alpha)A)]^2 E_{ih}[\bar{E}[\bar{a}(x, Y)]], \quad j \neq i \\ &= q\sum_{s=0}^{\infty} (r(\alpha + (1-\alpha)A))^s E_{ih}\left[\bar{E}_i^s[x|y_i, Y]\right]. \end{aligned} \tag{34}$$

$\bar{E}_i^s[x]$ denotes the average expectation of the average expectation (s-times) of x, and $\bar{E}_i^s[\bar{a}]$ is the average expectation of the average expectation (s-times) of the average action, where all averages concern informed agents only and are dependent on agent type h. The infinite sum captures the recursiveness of higher-order beliefs, where agents form expectations about the expectations of others in this strategic interaction setting. $E_{ih}\left[\bar{E}_i^s[x|y_i, Y]\right]$ is a linear combination of y_i and Y.[47] Thus, $a_{ih}(y_i, Y)$ is also a linear combination of these signals. By establishing that the optimal actions of agents are linear combinations of their signals, and that these strategies lead to consistent expectations about others' actions, it is shown that the linear solution constitutes the unique equilibrium.

C.2.3 Derivation for Proposition 2: Optimal Disclosure

Taking the derivative of $E[W(a, x)|x]$ with respect to τ_Y shows that $\frac{\partial E[W|x]}{\partial \tau_Y} \gtrless 0$ and depends on the particular parameter values.

$$\begin{aligned} \frac{\partial E(W|x)}{\partial \tau_Y} = &\frac{2\alpha\left(\tau_Y + \tau_y q^2\right)}{\left(\tau_Y + \tau_y q\right)^3} - \frac{\alpha}{\left(\tau_Y + \tau_y q\right)^2} \\ &+ \frac{2A(1-\alpha)\rho_{im}\left(\rho_{im}^2 \tau_Y + \tau_y q^2\right)}{\left(\rho_{im}\tau_Y + \tau_y q\right)^3} - \frac{A(1-\alpha)\rho_{im}^2}{\left(\rho_{im}\tau_Y + \tau_y q\right)^2} \end{aligned} \tag{35}$$

[47] The proof by Morris and Shin (2002) is directly applicable to ingroup agents. It extends to expectation formation with the resonance weight ρ_{im} of informed outgroup agents since the recursive structure and the linearity of the expectation function are preserved. The updating bias merely reduces the influence of the public signal, and thus shifts the coefficients in the explicit linear expression slightly.

C.2.4 Raising Private Signal Precision May Harm Welfare

Proposition 4. *The effect of increasing the precision of private signals (τ_y) on expected welfare is ambiguous and depends on the coordination parameter r and the presence of outgroup agents ($\frac{\partial E(W|x)}{\partial \tau_y} \gtrless 0$).*

Derivation: Taking the derivative of $E[W(a,x)|x]$ with respect to τ_y shows that $\frac{\partial E[W|x]}{\partial \tau_y} \gtrless 0$ and depends on the particular parameter values.

$$\begin{aligned} \frac{\partial E(W|x)}{\partial \tau_y} = &-\frac{\alpha q^2}{(\tau_Y + \tau_y q)^2} - \frac{(1-\alpha)Aq^2}{(\rho_{im}\tau_Y + \tau_y q)^2} \\ &+ \frac{2Aq(1-\alpha)\left(\tau_y q^2 + \rho_{im}^2 \tau_Y\right)}{(\rho_{im}\tau_Y + \tau_y q)^3} \\ &+ \frac{2q\alpha((\tau_y q)^2 + \tau_Y)}{(\tau_Y + \tau_y q)^3} + \frac{(1-\alpha)(1-A)}{\tau_y^2} \end{aligned} \tag{36}$$

At first glance, more precise private signals improve alignment with the fundamental and thus welfare. However, the sign depends on $(r, \alpha, A, \rho_{im}, \tau_Y/\tau_y)$: with strong coordination and a sizable, biased informed outgroup, increasing τ_y can lower welfare. This occurs because of projection: informed outgroup agents underweight public information and expect others to do the same; under high r, they tilt actions toward private signals to coordinate on what they believe others will do, increasing action dispersion. Negative effects require both sufficiently strong strategic complementarities and sufficiently large bias and informed outgroup. The resonance weight needed for negative effects is much lower than what I find empirically.[48]

This contrasts with Morris and Shin (2002) and Cornand and Heinemann (2008), and aligns with the broader insights in Hellwig (2005) and Angeletos and Pavan (2007): the welfare effect of higher private precision depends on the trade-off between coordination on public signals and dispersion from idiosyncratic ones. With strong coordination motives and sizable processing biases, greater reliance on private signals raises dispersion and can reduce welfare; otherwise, higher private precision improves welfare.

Corollary 2. *When there are significant biases in outgroup agents' information processing, increasing τ_y may reduce social welfare.*

[48]Specifically, $\rho_{im} \lesssim 0.3$ is needed for negative effects, given $r \gtrsim 0.5$, roughly equal public and private precision, and at least half the population being outgroup and informed. For milder biases or smaller informed outgroups, the effect is nonnegative.

C.3 Optimal Delegation

C.3.1 Derivation for Proposition 3: Optimal Delegation

The optimal α^* is found by setting $\frac{\partial E[W|x]}{\partial \alpha} = 0$ and solving for α. Complex non-linearities makes an explicit analytical solution for α^* infeasible. The Weierstrass Extreme Value Theorem guarantees the existence of a welfare-maximizing α^*, given that the expected welfare function $E[W(a,x)|x]$ is continuous in α and the domain $[0,1]$ is compact. α^* is either a corner solution (0 or 1) or is obtained by setting the first-order condition to zero:

$$\begin{aligned}\frac{\partial E(W|x)}{\partial \alpha} &= \frac{\alpha 2r\tau_y(1-A)\,q}{(\tau_Y+\tau_y q)^2} - \frac{\alpha 2r\tau_y(1-A)\left(\tau_Y+\tau_y q^2\right)}{(\tau_Y+\tau_y q)^3} + \frac{(1-\alpha)\,2A\,(1-A)\,r\tau_y q}{(\rho_{im}\tau_Y+\tau_y q)^2} \\ &\quad - \frac{(1-\alpha)\,2A\,(1-A)\,r\tau_y\left(\tau_y q^2+\rho_{im}^2\tau_Y\right)}{(\rho_{im}\tau_Y+\tau_y q)^3} + \frac{A\left(\tau_y q^2+\rho_{im}^2\tau_Y\right)}{(\rho_{im}\tau_Y+\tau_y q)^2} - \frac{\tau_Y+\tau_y q^2}{(\tau_Y+\tau_y q)^2} + \frac{1-A}{\tau_y} = 0.\end{aligned} \tag{37}$$

While an analytical solution is infeasible, Figure A34 reports α^* numerically across coordination r and public signal precision τ_Y, holding $\tau_y = 1$. Panels vary (i) signal size via availability A (top: large signal, $A = 0.8$; bottom: small signal, $A = 0.2$) and (ii) updating bias (left: $\rho_{im} = 1$; right: $\rho_{im} = 0.5$). Four forces determine optimal delegation:

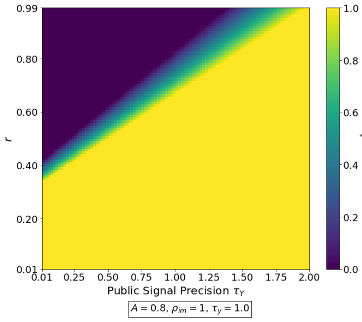
(i) α^* is (weakly) increasing in relative *public signal precision* τ_Y/τ_y: when the public signal is precise, delegation is desirable ($\alpha^* \to 1$); when it is noisy, α^* falls: when $\tau_Y < \tau_y$ in high coordination environments, raising α can reduce welfare unless public precision is sufficiently high.[49]

(ii) α^* is decreasing in the *coordination motive* r whenever the public signal is insufficiently precise: stronger complementarities raise the cost of coordinating on a noisy public signal, making it optimal to retain some outgroup ($\alpha^* < 1$).

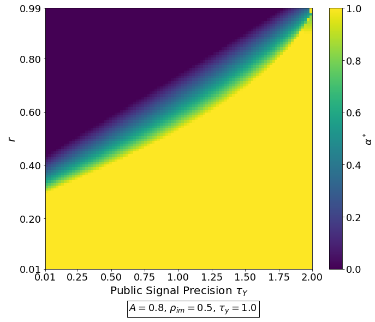
(iii) α^* is shifted down with *bias in updating* ($\rho_{im} < 1$): relative to the unbiased case, the region with $\alpha^* < 1$ expands, especially when τ_Y is large (see Figures A34a vs. A34b). This is because biased outgroup agents under-weight the public signal, providing a buffer against over-coordination on noisy public signals, and increasing ingroup agents removes this buffer.

(iv) α^* decreases with *signal size* $|Y|$ *(i.e., availability* A*)*. Large signals make the outgroup buffer operative: more outgroup agents actually receive Y, so their bias reduces α^* more strongly (see Figures A34b vs. A34d). With small signals (low A), many outgroup agents miss Y, so bias has little additional effect and the surface resembles the no-bias case (see Figures A34c vs. A34d).

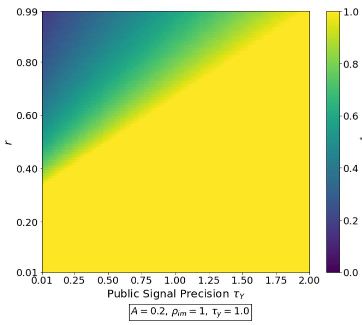
[49] The sufficient level of strategic complementarity depends on updating bias and availability. It is $r > 1/3$ when outgroup agents update without bias (regardless of availability), and lower when updating is biased and signals are widely available. For example, with large signals ($A \to 1$) and a small updating bias $\rho_{im} = 0.95$, $r > 0.156$ suffices. Larger signals lower this threshold; stronger biases raise it.



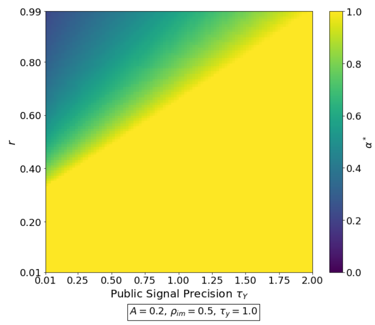
(a) Large Signal - No Bias



(b) Large Signal - Bias



(c) Small Signal - No Bias



(d) Small Signal - Bias

Figure A34: Optimal Share of Ingroup Agents

Notes: Welfare-maximizing α^* as a function of (r, τ_Y) with $\tau_y = 1$. Top: large signal (holding $A = 0.8$ fixed); bottom: small signal ($A = 0.2$). Left: no updating bias ($\rho_{im} = 1$); right: outgroup strongly under-weights the public signal ($\rho_{im} = 0.5$).

C.4 Delegation With Expertise Loss

Proposition 5. *If delegation to increase α lowers public signal precision, $\tau_Y = \tau_Y(\alpha)$ with $\tau_Y'(\alpha) \leq 0$, then delegation is locally welfare-improving iff*

$$\frac{d\, E[W|x]}{d\alpha} > 0 \iff \frac{\partial E[W|x]}{\partial \alpha} > k(\alpha)\, \frac{\partial E[W|x]}{\partial \tau_Y}, \tag{38}$$

where $k(\alpha) \equiv -\tau_Y'(\alpha) \geq 0$ is the marginal expertise loss (precision drop) per unit increase in α.

Proof sketch. Delegation changes α directly and τ_Y indirectly. By the chain rule,

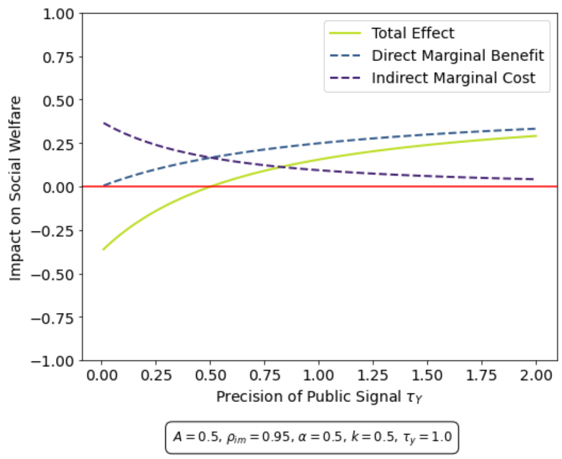
$$\frac{d\, E[W|x]}{d\alpha} = \frac{\partial E[W|x]}{\partial \alpha} + \frac{\partial E[W|x]}{\partial \tau_Y}\, \tau_Y'(\alpha) = \frac{\partial E[W|x]}{\partial \alpha} - k(\alpha)\, \frac{\partial E[W|x]}{\partial \tau_Y}. \tag{39}$$

Local welfare improves iff $\frac{dE[W|x]}{d\alpha} > 0$, which yields Eq. (38). For exposition, let $\tau_Y(\alpha) =$

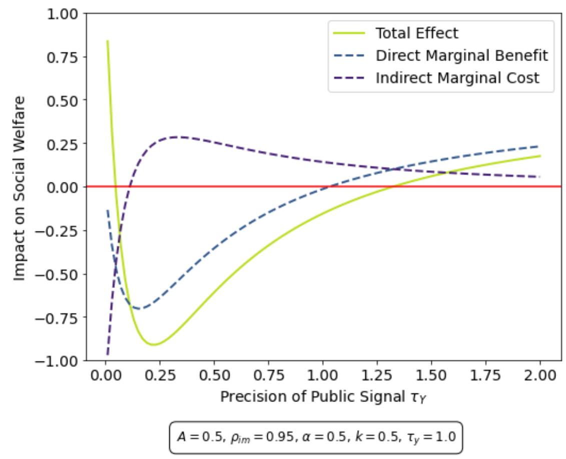
$\tau_Y^0 - k\alpha$ with $k > 0$. Then $\tau_Y'(\alpha) = -k$ and (39) becomes

$$\frac{d\,E[W|x]}{d\alpha} = \frac{\partial E[W|x]}{\partial \alpha} - k\,\frac{\partial E[W|x]}{\partial \tau_Y}. \tag{40}$$

The delegation–expertise trade-off is exactly Eq. (38) with $k(\alpha) \equiv k$. Therefore, delegation raises welfare only when the marginal benefit of raising α exceeds the marginal cost from the induced loss in public signal precision. Figure A35 visualizes this tradeoff.



(a) Low Coordination (r=0.10)



(b) High Coordination (r=0.90)

Figure A35: Social Welfare Impact of Delegation With Expertise Loss

Notes: Welfare effects of increasing (α) via delegation, accounting for reduced public signal precision from lower messenger expertise. Green: total effect; blue: marginal benefit; purple dashed: marginal cost. Parameters: $\tau_y = 1$, $A = k = \alpha_0 = 0.5$. Arrows indicate welfare ascent. Panel (a): low strategic complementarities ($r = 0.10$). Panel (b): high complementarities ($r = 0.90$).

Notably, when $\frac{\partial E}{\partial \tau_Y} > 0$ (disclosure is beneficial), expertise loss imposes a positive marginal cost $k(\alpha)\,\frac{\partial E}{\partial \tau_Y}$ that delegation must overcome; when $\frac{\partial E}{\partial \tau_Y} < 0$ (disclosure is harmful), expertise loss could help at the margin – yet so would centralization as shown in Section 4.4.