Narrative Persuasion - Preregistration

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1 Introduction

Our experimental design takes inspiration from the ideas discussed in Schwartzstein and Sunderam (2021), however the primary objective of the experiment is not to test the Schwartzstein and Sunderam (2021) model. Rather, we aim to shed light on when and why persuasion using models is likely to occur and what factors can help to protect individuals from being persuaded in this way. We do this by testing the set of hypotheses described below using comparative static comparisons using the exogenous variation generated by our treatment conditions as well as the additional variation created by the experimental design.

Following Schwartzstein and Sunderam (2021) (S&S), we will consider a strategic setting in which there is a persuader / advisor (narrative-sender) and a receiver / investor (narrative-recipient). The receiver has access to data that is informative about the true underlying model. The persuader's objective is to propose a model to the receiver that guides the receiver in interpreting this data. The receiver then takes an action that influences the payoffs of both the persuader and the receiver. Importantly, the persuader's incentives may be either aligned or misaligned with the receiver's – i.e., the persuader might attempt to convince the receiver to take an action that does not serve her own best interests.

In this setting, we will investigate which factors influence the effectiveness of persuasion using models. Specifically, we will ask questions such as the following: *(1) Are receivers worse off when the sender's incentives are misaligned? (2) Does knowing the sender's incentives make receivers skeptical? (3) Does access to private data protect receivers? (Alternatively: Are persuaders less effective when they cannot construct ex post models that fit the receiver's available data?) (4) Are receivers better off if they are encouraged to make sense of the evidence themselves before they receive the sender's message? (5) Does the empirical plausibility of the sender's proposed model affect receiver's trust in the model?*

2 Experimental Design

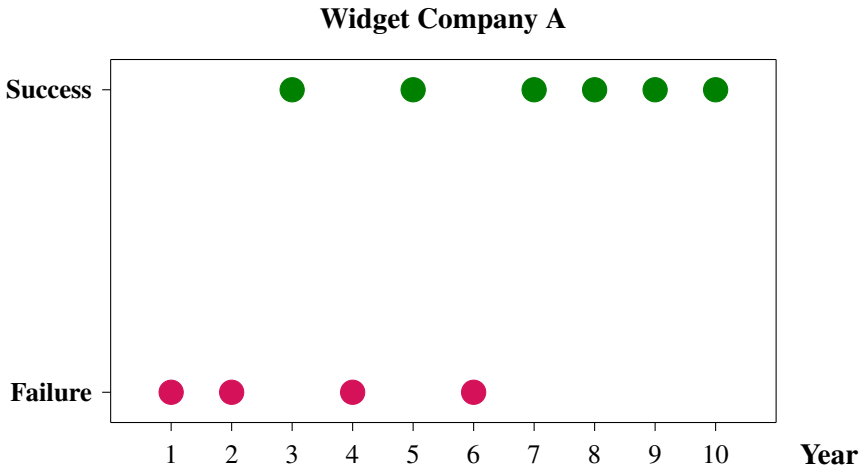
In our experiment, we consider a two-player game where one player takes the role of *sender* and the other takes on the role of *receiver*. We frame our experiment using an investment game, such that the receiver is an *investor* whose objective is to assess the likelihood that a fictitious company will be successful (as opposed to unsuccessful) in the coming year. The experiment labels the coming year as "Year 11". The sender is an *advisor* to the investor, and will provide advice about the fictitious company.

In each round of the experiment, the investor's objective is, therefore, to correctly assess the underlying state of the world (i.e., the likelihood of the company being successful in Year 11).

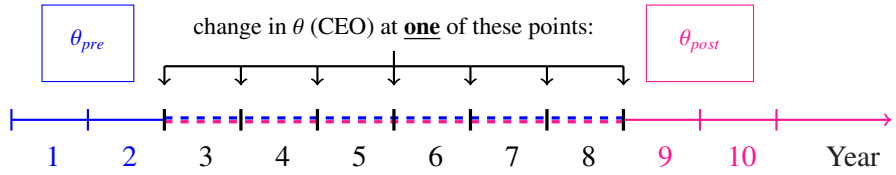
To do this, the investor can draw on the information she observes about the history of success of the company.

However, prior to the investor reporting their assessment of the company's likelihood of success, the advisor sends a message to the investor. The advisor always knows the true model generating the data. In addition, in most treatment conditions, the advisor also observes the data that the receiver has access to. The advisor may use this message to try to persuade the investor to hold a belief that is biased in a certain direction by distorting the investor's interpretation of the data containing the history of past outcomes.

The Data Generating Process: The history of past outcomes consists of the past ten periods (years) of the company's performance. This data shows whether the company was "successful" or "unsuccessful" in each of the past ten years (i.e., from Year 1 to Year 10). The following provides an illustrative example of how one particular history could be represented.



In each year, the probability of the company being successful is determined by an underlying fundamental, θ. This fundamental changes exactly once during the ten years. More specifically, it is common knowledge that θ_{pre} is drawn from $U[0,1]$ prior to Year 1, and then is redrawn once at some point after Year 2 and before Year 9, denoted by $\theta_{post} \sim U[0,1]$. Therefore, the probability of success in each of the ten years is governed by $(\theta_{pre}, \theta_{post})$. In the experiment, we frame the change in the fundamental state as a change in the CEO of the company. The value of θ_{pre} then summarizes the probability of success for the period before the CEO changed (the pre period) and θ_{post} denotes the probability of success for the period after the CEO changed (the post period). The following figure illustrates the structure of the historical data.



Consequently, the last two periods in the historical dataset are commonly known to be (*i*) governed by a different probability of success to the first two periods, and (*ii*) informative about the success probability of the company in Year 11.

To formalise the setup, let $c \in \{2, 3, 4, 5, 6, 7, 8\}$ denote the period before the the structural change (i.e., if $c = 2$, then the structural change occurred at the end of year 2, or equivalently, at the beginning of year 3). We specify a data generating process where c is uniformly distributed, which we also disclose to participants.[1] The variable c summarises the true model: it specifies that the last $10 - c$ years of data are relevant for whether the company is successful under the new CEO. Therefore, θ_{pre} denotes the realised probability of success up to and including year c and θ_{post} denotes the realised probability of success after year c.

The Advisor's Additional Information: The advisor is fully informed about the underlying data generating model—i.e. the advisor knows the true values of the three fundamental parameters: $(c^T, \theta^T_{pre}, \theta^T_{post})$. The investor knows that the advisor has this additional information.

The Advisor's Message: The advisor sends three pieces of information to the investor: (*i*) an estimate $c^S \in \{2, 3, 4, 5, 6, 7, 8\}$ of the year when there was a structural change, and (*ii*) estimates $\theta^S_{pre} \in [0, 1]$ and $\theta^S_{post} \in [0, 1]$ of the success probability prior to and after the structural change, respectively.

The Investor's Decision: The investor observes the advisor's report $(c^S, \theta^S_{pre}, \theta^S_{post})$ and then submits her own estimate of θ^R_{post}.

The Investor's Incentives: The investor is incentivised to estimate θ_{post} as close as possible to θ^T_{post}. We will use the binarized scoring rule (Hossain and Okui, 2013) to ensure that the investor's payment will be maximized (in expectation) if she reports her true belief about θ_{post}.

[1] In other words, participants know that c is randomly drawn from a uniform distribution over $\{2, 3, 4, 5, 6, 7, 8\}$. (In the experiment, we frame this as being at the beginning of years 3 to 9, rather than at the end of years 2 to 8.) Participants also know that both θ_{pre} and θ_{post} are independently drawn from uniform distributions, $U[0, 1]$. This is done independently for each of the ten companies (i.e., for each of the ten rounds of the experiment).

The Advisor's Incentives: Participants in the experiment who are assigned the role of advisor will be randomly assigned into one of three incentive conditions. In all three conditions, the advisor's payment will be a function of their matched investor's θ_{post}-report. Under the three conditions, the advisor's payment is either: (a) increasing in the investor's estimate of θ_{post}, (b) decreasing in the investor's estimate of θ_{post}, or (c) increasing in the accuracy of the investor's estimate of θ_{post}. Each advisor keeps the same incentives for the duration of the experiment.

This will be incentivized using an strategic version of the binarized scoring rule (BSR), where there are two key differences from the standard BSR. First, the belief report that is relevant for determining the probability of receiving the bonus payment is made by *another individual*, not oneself. Second, in incentive conditions (a) and (b), the θ^S_{post} reported by the investor is compared to extreme θ_{post} values, $\theta_{post} = 1$ or $\theta_{post} = 0$ respectively, rather than being compared to the true θ_{post} to determine the advisor's payment. In incentive condition (c), the advisor's payoff is calculated in the same way as the investor's payoff (i.e., their incentives are perfectly aligned).

Strategic Information about Incentives: Investors are told about the different types of advisors that they may face. Specifically, they are told about the distribution of advisors with each of the three incentive types, namely that the probability of being matched with each advisor type in each round is one-third. In treatment Skepticism, investors will additionally be informed about the incentives of their specific matched advisor in each round (more details below).

Advisors know the incentives of investors. In all treatment conditions, advisors are also always told that investors may or may not know their matched advisor's incentives.

General Comments about the Design: The basic idea of this design is that the advisor (in contrast to the investor) knows the underlying DGP $(c^T, \theta^T_{pre}, \theta^T_{post})$, which provides an opportunity for gains from communication between both players, since the advisor is more informed but the advisor's payoff depends on the investor's action. Depending on advisor's incentives, the advisor might sometimes try to deceive the investor into reporting an overly optimistic or pessimistic belief about θ_{post}. Specifically, the advisor can use the other dimensions of the report, (c^S, θ^S_{pre}), as supporting evidence for trying to shift this belief about θ_{post}.

We have chosen to deviate from S&S in that we usually do not elicit the investor's prior beliefs about the model (i.e., before persuasion); that is, her prior beliefs about $(c, \theta_{pre}, \theta_{post})$.

The reason for this is two-fold. First, we wish to study situations in which senders (advisors) present data to receivers (investors) at the same time as they communicate their theory explaining the data, as opposed to the receiver first constructing their own personal theory of the data. This conjunction of receiving the data and a potential theory at the same time reflects many real-world

situations. Second, we wish to explicitly study whether being encouraged to form a personal theory of the data *prior* to receiving a potential theory from an advisor has a protective function in helping to insulate receivers (investors) from persuasion.

2.1 Treatment Conditions:

To address our research questions, we will consider four core between-subject treatment conditions.

BASELINE: Our BASELINE treatment follows the structure described above. The other three treatments involve small deviations from the BASELINE condition in which we exogenously vary one specific feature of the decision environment.

PRIVATEDATA: To investigate whether having access to private data serves a protective role against persuasion, we vary whether the advisor observes the historical performance dataset. In particular, it is common knowledge in this treatment that the advisor does not observe the historical performance dataset when choosing their message. The advisor, therefore, knows the true underlying parameters of the data generating process, and is still able to try to persuade the investor by sending an inaccurate message, but is unable to tailor the message to the data that the investor observes. This may make it more difficult for the advisor to send a message that is both deceptive and persuasive.

SKEPTICISM: To investigate whether knowing their specific matched advisor's incentives makes investors skeptical, investors are made aware of the advisor's incentives. Because we are interested in investor behavior, we hold the advisor's information set constant by telling advisors also in this treatment that investors may or may not know their incentives.[2]

SEQUENTIAL: In this treatment, we examine the effect of being encouraged to form a default (or prior) theory about the data generating process *before* entertaining theories received from others. Specifically, instead of receiving the historical data and the advisor's message simultaneously, and then forming a belief about the data generating process, in this treatment investors will first receive only the data. We will then ask them to report their prior belief about the data generating process (i.e., c, θ_{pre}, and θ_{post}). Thereafter, they receive the advisor's message, and we elicit their final assessment of θ_{post}.

[2]We control for investors' higher-order beliefs by informing them that advisors do not know that investors know what their matched advisor's incentives are.

This treatment will allow us to evaluate whether being encouraged to try to make sense of the data oneself first serves a protective function against persuasion using models.[3]

2.2 Procedures:

The experiment will be conducted via the platform, Prolific. Participants will take part in the experiment in groups of 6. Within each group, 3 participants are randomly assigned to the role of the sender (advisor) and 3 are assigned to the role of the receiver (investor). Each advisor is randomly assigned to one of the three incentive conditions (i.e., there will be one advisor from each of the three incentive conditions within each group of 6). Both advisors and investors keep their role for the duration of the experiment; advisors additionally stay within their incentive condition throughout the experiment.

The experiment consists of ten rounds. In each round, each investor is randomly matched with an advisor within their group of six (i.e., the three investors are randomly matched with the three advisors).

Within each of the ten rounds, the true underlying data generating process will be held constant across all matched investor-advisor pairs. Specifically, the triple of fundamentals, $(c^T, \theta^T_{pre}, \theta^T_{post})$, is held constant within a specific round across all subjects.[4] However, conditional on these fundamentals, the observed historical data of success and failure of the company is drawn independently for each matched pair of participants. This provides us with exogenous variation in the data observed by subjects, conditional on a particular set of fundamentals governing the success of a the company in that round.

Participants are paid for one randomly chosen round of the experiment and do not receive any feedback until the end of the experiment. The absence of feedback implies that investor behavior cannot affect advisors. Advisors only influence investors directly through the messages they send. We, therefore, can implement the experiment in a simpler way where we first collect all advisor choices for the ten rounds, and thereafter collect investor choices.

[3] An additional benefit of this treatment is that the reported prior beliefs will provide us with some descriptive information about the types of subjective models that investors construct in the absence of messages from advisors. It also allows us to examine updating of beliefs.

[4] We will therefore randomly draw ten realizations of $(c^T, \theta^T_{pre}, \theta^T_{post})$; one for each round of the experiment. These will apply to all participants in all sessions of the experiment.

3 Hypotheses and Analysis

3.1 Definitions and Measures

Our main hypotheses and analysis relate to the following objects that we collect in each round, for each matched sender-receiver pair:

(i) The sender's message, $(c^S, \theta^S_{pre}, \theta^S_{post})$.

(ii) The receiver's assessment, θ^R_{post}.

(iii) The realized historical dataset of successes and failures, $h = (\omega_1, \ldots, \omega_{10})$.[5]

In addition, we collect the fundamental parameters of the true data generating process, $(c^T, \theta^T_{pre}, \theta^T_{post})$, which vary across rounds, but are held constant across participants within a given round.

In order to organise the discussion of our hypotheses below, it will be useful to define some derivative measures that we can construct from this information. We organise these measures into three categories: (*i*) measures that compare a participant's message or assessment to the truth, (*ii*) measures that compare a participant's message or assessment to the observed historical data, and (*iii*) measures that provide an indication of the degree to which the sender is able to persuade the receiver (i.e., to shift their assessment).

Measures Relative to the Truth: The two measures of primary interest in this class are the distance between sender's message about θ_{post} and the true value, and the distance between the receiver's assessment of θ_{post} and the true value:[6]

(i) Distance between the sender's message and the truth: $D^T(\theta^S_{post}) := |\theta^S_{post} - \theta_{post}|$

(ii) Distance between the receiver's assessment and the truth: $D^T(\theta^R_{post}) := |\theta^R_{post} - \theta_{post}|$

Measures Relative to the Historical Data: For each round and each matched pair of participants, the historical success data comprises ten realizations of the underlying data generating process in that round. It is therefore informative to compare participants' messages and assessments to the information that they observe.

To do this, for each observed dataset, we determine the data-optimal model, namely the model that is most likely to have generated the data, $(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post})$. Following S&S, we take the maximum likelihood estimate of $(c, \theta_{pre}, \theta_{post})$ for a given dataset h as the model that most

[5] Where $\omega_t \in \{0, 1\}$ with $P(\omega_t = 1) = \theta_{pre}$ if $t \leq c$ and $P(\omega_t = 1) = \theta_{post}$ if $t > c$.

[6] In addition, we construct an indicator variable that takes a value of one if the sender lies in their message, and zero if they tell the truth: $\mathbb{I}(\theta^S_{pre} \neq \theta_{pre} \vee c^S \neq c \vee \theta^S_{post} \neq \theta_{post})$

likely generated the data. Given this data-optimal model, we can compare the message and assessment of the sender, $(c^S, \theta^S_{pre}, \theta^S_{post})$, to the optimum. We will do this by constructing an empirical plausibility index (EPI) which takes on values between 0 and 1 and is equal to 1 if the sender's message is equal to the data-optimal model. If the sender's message is equal to the model that is least likely given the data, the EPI will take on a value of 0. Values of the EPI that are strictly between 0 and 1 reflect cases of intermediate plausibility. We use the EPI as a measure of the distance in plausibility between the sender's message and the data-optimal model:

$$\text{EPI}(c^S, \theta^S_{pre}, \theta^S_{post}|h) := \frac{\mathcal{L}(c^S, \theta^S_{pre}, \theta^S_{post}|h)}{\mathcal{L}(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post}|h)}, \tag{1}$$

where $\mathcal{L}(\cdot|h)$ is the likelihood function conditional on the historical data h. In Appendix A.1, we provide further details on the construction of the EPI and discuss its relation to other benchmarks.

Measures of Persuasion: In our analysis, it will be of interest to have measures of the degree to which senders are able to persuade receivers. The measures discussed above already contribute to this by showing how far receivers' assessments are shifted away from the truth, or from the data. However, we also want to construct measures that indicate the degree to which receivers follow the message of the sender.[7] To do this, we construct the following measures:

(i) Distance between the sender's message and the data-optimal model:

$D^{DO}(\theta^S_{post}) := |\theta^S_{post} - \theta^{DO}_{post}|$

(ii) Distance between the sender's message and the receiver's assessment:

$D^S(\theta^R_{post}) := |\theta^R_{post} - \theta^S_{post}|$

(iii) The ratio of the distance that the receiver moves away from the data-optimal point to the distance that the sender *tries* to move the receiver from the data-optimal point:

$T := \frac{\theta^R_{post} - \theta^{DO}_{post}}{\theta^S_{post} - \theta^{DO}_{post}}$

A trust measure of $T = 1$ means the receiver is highly trusting of the sender; a trust value of $T = 0$ means that the receiver is maximally skeptical of the sender. Moreover, $T < 0$ and

[7] For a single receiver in a single round, one can think of this as an indication of the receiver's trust in the sender's report. When considering the average across all rounds for a single receiver, one can think of this as a measure of the receiver's gullibility.

$T > 1$ suggest excessive skepticism and trust, respectively.[8] The following figure illustrates this measure:

Figure 1: Illustration of our measure of trust.

3.2 Hypotheses

Our main hypotheses are stated below. They concentrate on comparing receiver behaviour using two dimensions of exogenous variation: (i) different treatment conditions, and (ii) different sender types (i.e., senders with aligned or misaligned incentives).

When interpreting the hypotheses, and the associated empirical analysis plans, it is important to keep in mind that in three of our treatments (Baseline, Skepticism, and Sequential), we hold the instructions of the senders completely constant. Since senders also receive no feedback between rounds, this implies that sender behavior in these treatments should be approximately balanced on average, which allows a clean comparison of the receiver behavior in response to sender messages across these treatment conditions. In our fourth treatment, PrivateData, both the senders' and the receivers' instructions change in comparison to Baseline, since both learn that the sender will not observe the historical dataset prior to sending a message to the receiver. This implies that a treatment comparison between PrivateData and another treatment (e.g., Baseline) should be interpreted as a change in the equilibrium play of senders and receivers.

With regards to sender types, in the hypothesis section, we will often distinguish between receivers who face a sender with *aligned* versus *misaligned* incentives. A sender has aligned incentives if their payment is maximized when the receiver adopts the true θ_{post} of the data generating model. A sender who is incentivized to induce the receiver to report an estimate of θ_{post} that is shifted towards either 0 or 1 is misaligned. As mentioned above, we introduce exogenous variation in the sender incentives within each of our treatment conditions.

Following the section below in which we describe our main hypothesis, we also discuss a set of secondary hypotheses which focus more on within-treatment variation and sender behaviour.

3.2.1 Main Hypotheses

Influence of persuasion by senders (in Baseline): We study the impact of sender incentives on receiver assessments by comparing the distance of the receiver's assessment to the true model,

[8] We would normally expect to see $T \in [0, 1]$ for each observation (i.e., that the receiver's report is between the data-optimal model and the sender's message). Therefore, checking for violations of this may be used as a form of rationality check on receiver behaviour.

$D^T(\theta^R_{post})$, within the Baseline treatment. Our first hypothesis is that receiver assessments are further from the truth when they face a sender with misaligned incentives. This provides a test of whether senders are able to persuade receivers to shift their beliefs, despite receivers observing objective data.

Hypothesis 1. *In* Baseline*, the distance between the receiver's assessment and the truth is larger when sender incentives are misaligned than when sender incentives are aligned.*

We will test this hypothesis using the following regression model:

$$D^T(\theta^R_{post}) = \beta_0 + \beta_1 \times \mathbb{I}(\text{Misaligned sender}) + \rho_r + \varepsilon$$

and estimating it via OLS. In the equation above, $\mathbb{I}$(Misaligned sender) is an indicator function which takes a value of 1 if sender incentives are misaligned, ρ_r are round fixed effects and ε is an error term.[9] We will account for repeated observations and potential within matching group spillovers by clustering errors at the matching group level.[10] Using the estimates from this equation, we will test whether $\beta_1 > 0$. In addition, we will also present results from a Wilcoxon rank-sum test that tests whether the distributions of $D^T(\theta^R_{post})$ differ by alignment of the sender. When reporting these tests we will again account for repeated measurement and within matching group spillovers by reporting a test statistic for receiver outcomes which adjusts for clustered errors at the matching group level (see, e.g., Rosner et al., 2006).

Comparative statics using between-treatment variation

The following three hypotheses all involve exploiting the variation provided by our treatment conditions. We measure how persuasion changes in the various treatments relative to Baseline using OLS regressions of the following kind:

$$D^T(\theta^R_{post}) = \beta_0 + \beta_1 \times \mathbb{I}(\text{Treatment}) + \rho_r + \varepsilon. \tag{2}$$

As our main persuasion measure to test our hypotheses, we take the distance between the truth and the receiver's assessment, $D^T(\theta^R_{post})$. To augment these results, we will also report the results of similar regressions which use the distance between the receiver's assessment and the sender's message, $D^S(\theta^R_{post})$, as an alternative outcome measure.[11] The regressions will also typically

[9] Since the true model is held constant within each round of the experiment, the ρ_r parameters absorb both round and true model fixed effects.

[10] It is worth noting that since senders receive no feedback at all during the experiment, the within matching group spillovers are more limited in scope than usual in experiments where subjects interact in groups. In our experiment, interaction between players only operates in one direction: from senders to receivers via the messages. Receivers also do not receive any feedback on the outcomes of their decisions prior to the end of the experiment.

[11] It is important to note that the treatment comparisons involving the distance between the receiver's report and the

include round fixed effects (ρ_r). In addition, we will also report nonparametric Wilcoxon rank-sum tests results for each hypothesis comparing the distribution of the outcome variable between two treatments. As before, both for the regression and the nonparametric test, standard errors will be clustered at the matching group level.

Influence of receiver skepticism: We study the impact of receiver skepticism by comparing the distance between the receiver's assessment and the truth when matched to a *misaligned* sender between Baseline and Skepticism. Since the sender's instructions are held constant between Baseline and Skepticism, the treatment comparison holds sender behaviour fixed and only potentially changes receiver behaviour. Our hypothesis is that, when moving from Baseline to Skepticism, this distance between the receivers assessment and the truth will decrease.[12]

Hypothesis 2. *When matched with a sender with misaligned incentives, the distance between the receiver's assessment and the truth is smaller in* Skepticism *than in* Baseline.

We test this hypothesis by estimating the regression specified in equation (2) for the Baseline and Skepticism treatments and testing whether $\beta_1 < 0$.

Influence of the receiver forming their own prior model: Here, we study the impact of encouraging the receiver to form their own personal interpretation of the data before they receive the assessment from the advisor. We do this by comparing the distance between the receiver's assessment and the truth when matched with a misaligned sender between the Baseline and Sequential treatments. Our hypothesis is that, when matched to a misaligned sender, receivers' assessments are closer to the truth in Sequential than in Baseline.

Hypothesis 3. *When matched with a sender with misaligned incentives, the distance between the receiver's assessment and the truth is smaller in* Sequential *than in* Baseline.

We test this hypothesis by estimating the regression specified in equation (2) for the Baseline and Sequential treatments and testing whether $\beta_1 < 0$.

sender's message, $D^S(\theta^R_{post})$, have a simple interpretation when comparing the three treatments in which the sender's information set is held identical (i.e., Baseline, Skepticism, and Sequential). However, treatment comparisons of this object involving the PrivateData treatment are more complicated to interpret, since both sender and receiver behavior may change. It is for this reason that we focus on $D^T(\theta^R_{post})$ as our primary object of interest. This has a clear interpretation across all four treatments.

[12] Our hypothesis here focuses only on misaligned senders. However, when considering aligned senders, it is possible that communication between an aligned sender and receiver improves when moving from Baseline to Skepticism as the receiver knows in Skepticism exactly when their matched sender is aligned. As a corollary to Hypothesis 2, we will also test for this possibility by comparing the distance between the receiver's assessment and the truth when matched to an aligned sender between Baseline and Skepticism.

Influence of receiver private data: We study the protective role of the receiver having access to private data by comparing the distance between the receiver's assessment and the truth when matched to a *misaligned* sender between BASELINE and PRIVATEDATA. We hypothesize that, when matched to a misaligned sender, receivers' assessments are closer to the truth in PRIVATEDATA than in BASELINE. Another interpretation of this hypothesis is that it is a test of whether senders who are able to construct an *ex post* narrative or model that is tailored to the exact historical data series that receivers observe are able to be more persuasive.

Hypothesis 4. *When matched with a sender with misaligned incentives, the distance between the receiver's assessment and the truth is smaller in* PRIVATEDATA *than in* BASELINE.

We test this hypothesis by estimating the regression specified in equation (2) for the BASELINE and PRIVATEDATA treatments and testing whether $\beta_1 < 0$.

3.2.2 Secondary Hypotheses

Our secondary hypothesis are organized to test for certain empirical regularities using within-treatment variation. They mostly follow the theoretical predictions of the framework adapted from S&S to fit our experimental design. Appendix A.3 sets up the adapted framework, presents predictions, and discusses how they can be tested using data from the experiment. A reader interested in more detailed theoretical justifications of the following hypotheses can refer to this Appendix.

Since we use within-treatment variation for our secondary hypotheses, when studying receiver behaviour, we will predominantly focus on the BASELINE treatment in order to hold constant other contextual factors.[13] We will also focus on the subset of rounds where receivers are matched with a *misaligned* sender to study persuasion. For this reason, we will also collect a larger sample size in our BASELINE treatment.[14] When studying sender behavior, we are able to exploit the fact that the senders face an identical choice problem in the BASELINE, SKEPTICISM, and SEQUENTIAL treatments (i.e., senders in these three treatments receive identical instructions—they only differ in the receivers they are matched with, but are not aware of these differences and do not receive feedback from these receivers during the experiment). Therefore, we pool the senders from these three treatments for our within-treatment comparisons for senders.

[13] As a robustness check, we will also report the results for all receivers in the Appendices of the paper, including fixed effects to control for treatment differences, as well as fixed effects that account for potential interactions between the treatment and the alignment of the senders' incentives.

[14] A second reason for collecting a larger sample for our BASELINE treatment is that we use the BASELINE treatment as a comparison group in most of our main hypotheses, which makes it efficient to collect a larger sample for this treatment in comparison to the other treatments.

Secondary Hypotheses Regarding Receiver Behavior:

The influence of the empirical plausibility of narratives on receiver trust: This hypothesis addresses the question: are receivers more willing to follow a message that fits the data well?

We study the impact of the receiver receiving an empirically plausible message (i.e., a message that fits the observed historical data well) by relating the distance between the sender's message and the receiver's assessment, $D^S(\theta^R_{post})$, to the empirical fit of the sender's message, as measured by the Empirical Plausibility Index (EPI). We hypothesize that the better the sender's message fits the observed data, the smaller the distance between the sender's message and the receiver's assessment. Essentially, this says that receivers will be more willing to follow a sender's message if it fits the data they observe well.

Hypothesis 5a. *The distance between the sender's message and the receiver's assessment decreases in the EPI.*

We will test for this hypothesis by running a regression of the following form using data from receivers in the Baseline treatment:

$$D^S(\theta^R_{post}) = \beta_0 + \beta_1 \text{EPI}(c^S, \theta^S_{pre}, \theta^S_{post}|h) + \alpha + \rho_r + \varepsilon$$

and testing whether $\beta_1 < 0$. In the equation above, α denotes the estimated effect of being matched to an aligned sender. The round indicator variable, ρ_r, captures experimental round fixed effects. We will cluster standard errors at the matching group level.

The influence of alternative available models on receiver trust: Here, we introduce a sub-hypothesis that checks for a potential force moderating the relationship between the message's EPI and the receiver's assessment: if there exist different models that fit the observed data comparatively well, does this make it more difficult to persuade the receiver to adapt the sender's model compared to the case where there is a single salient data-optimal model?

We study the impact of the availability of alternative models by examining whether the shape of the EPI function, taken across all possible values of θ_{post}, affects the distance between the sender's message and the receiver's assessment, $D^S(\theta^R_{post})$. The EPI function is single-peaked in cases where the data provides a relatively salient data-optimal model but has multiple peaks when the data provides room for multiple competing explanations. We hypothesize that, if the history of outcomes can be equally well explained by different models, the receiver is less easily swayed by the sender's model (assuming that the receiver has reason to believe that there is at least some chance that the sender does not have aligned incentives, as is the case in our Baseline treatment). The rationale behind this hypothesis is that when the EPI has multiple

peaks, the receiver can more easily entertain alternative models that explain the data similarly well. Therefore, we conjecture that the distance between the sender's message and the receiver's assessment is higher if, among all possible values of θ_{post}, the EPI has multiple local optima.[15] To adjust for possible changes in the sender's message quality across different histories, we condition the hypothesis on the value of the EPI evaluated at the sender's model.

Hypothesis 5b. *Conditional on the value of the EPI evaluated at the sender's model, the distance between the sender's message and the receiver's assessment is smaller if the EPI has a single global optimum than if it has multiple local optima.*

We will test for this hypothesis by running a regression of the following form using data from receivers in the Baseline treatment:

$$\begin{aligned} D^S(\theta^R_{post}) =& \beta_0 + \beta_1 \text{EPI}(c^S, \theta^S_{pre}, \theta^S_{post}|h) + \beta_2 \mathbb{I}(\text{EPI has multiple peaks}) \\ &+ \alpha + \rho_r + \varepsilon \end{aligned}$$

and testing whether $\beta_2 > 0$, where the variable "EPI has multiple peaks" is a binary variable that takes a value of one when the EPI has more than one local maximum. Fixed effects and standard errors are calculated in the same way as in the specification for Hypothesis 5a.

Secondary Hypotheses Regarding Sender Behavior:

To conduct our within-treatment hypothesis tests pertaining to senders, we will pool sender data from all treatments where senders face an identical decision problems (i.e., the Baseline, Skepticism, and Sequential treatments).

The influence of incentives on sender behaviour: In a first comparison, we ask how senders react to different incentives. We will do this by comparing the distance between the sender's message and the truth, $D^T(\theta^S_{post})$, between aligned and misaligned senders. Our hypothesis is that the messages of misaligned senders are further from the truth.

Hypothesis 6a. *The distance between the sender's message and the truth of the post report, $D^T(\theta^S_{post})$, is larger for misaligned senders than for aligned senders.*

Constructing a convincing narrative: A related test of sender strategies concerns the narrative part of the sender's problem: A sender might adjust their choices of c and θ_{pre} to make their

[15] Another way to think about this is that, if the log likelihood function of the model for a given history is relatively flat in θ_{post}, the sender is less swayed by the receiver's message, even if the communicated model has a high EPI because alternative models exist that also have a high EPI. We proxy flatness of the log likelihood function by distinguishing between flat (multiple peaked) and non-flat (single peaked) functions. Figure A.3.1 in Appendix A.3 plots this function for an example history.

report of θ_{post} more convincing.[16] As we show in the Appendix, an upward incentive-biased sender should deviate from reporting the data-optimal year of change c^{DO} only if a different year increases the number of successes or decreases the number of failures in the post period. Conversely, a downward incentive-biased sender should deviate only if a different year decreases the number of successes or increases the number of failures in the post period. We hypothesize that this behaviour leads to a systematic bias in the choice of θ_{pre} away from the true model, which results in upward incentive-biased senders reporting a smaller value than the truth and downward incentive-biased senders reporting a larger value than the truth. In other words, the bias in the choice of θ_{pre} operates in the opposite direction to the choice of θ_{post} for misaligned senders.

Hypothesis 6b. *The distance between the sender's message and the truth of the pre report, $D^T(\theta^S_{pre})$, is larger for misaligned senders than for aligned senders.*

To test the previous two hypotheses, we specify and estimate regressions of the form

$$D^T(\theta^S) = \beta_0 + \beta_1 \mathbb{I}(\text{Misaligned sender}) + \rho_r + \varepsilon, \tag{3}$$

that either use $D^T(\theta^S_{post})$ (Hypothesis 6a) or $D^T(\theta^S_{pre})$ (Hypothesis 6b) as an outcome variable. We will test whether $\beta_1 > 0$. We will take account of repeated measurement by clustering standard errors at the sender level.[17]

Balancing persuasiveness against the truth (Aligned senders): Aligned senders face a trade-off between sending a truthful message and sending a message that *more plausibly induces the truth*. Whether this tension induces the sender to bias their report of θ_{post} away from the data-optimal model may depend on the difference between θ^T_{post} and θ^{DO}_{post}. If this difference is positive (i.e., $\theta^T_{post} > \theta^{DO}_{post}$), an aligned sender has an incentive to bias their report upwards moving it closer to the truth, while they have an incentive to bias it downward towards the truth if the difference is negative (i.e., $\theta^T_{post} < \theta^{DO}_{post}$).[18] This leads us to the following hypothesis which asks whether aligned senders follow such a strategy that involves reporting a weighted average of the truth, θ^T_{post}, and the data-optimal parameter, θ^{DO}_{post}:

[16] This is despite the fact that the sender's incentives depend only on the receiver's θ_{post} report, implying that distortions of c and θ_{pre} serve a pure story-telling role.

[17] When reporting regressions on sender outcomes, we will take a less conservative clustering approach than when reporting on receiver outcomes, since senders do not receive feedback from other participants in their matching group.

[18] Note that this hypothesis could be formulated in two different, but equivalent, ways—either by considering that reports can be biased away from the data-optimal model or that they can be biased away from the true data generating model. Both are captured by the following intuition: We expect aligned senders' reports to reflect a compromise that biases their reports away from the data-optimal model and towards the true model (i.e., we expect that the average aligned sender will choose a report that represents some linear combination of the true θ^T_{post} and the data-optimal θ^{DO}_{post}).

Hypothesis 7a. *The distance between the data-optimal model and the aligned sender's report, $D^{DO}(\theta^{S}_{post})$, increases in the distance between the truth and the data optimal report $|\theta^{T}_{post} - \theta^{DO}_{post}|$.*

We test this hypothesis by estimating the following model for senders from the pooled Baseline, Skepticism, and Sequential treatments:

$$D^{DO}(\theta^{S}_{post}) = \beta_0 + \beta_1 \mathbb{I}(\text{Misaligned}) + (\beta_2 + \beta_3 \mathbb{I}(\text{Misaligned})) \cdot |\theta^{T}_{post} - \theta^{DO}_{post}| + \rho_r + \varepsilon$$

and testing whether $\beta_2 > 0$.

Gravitational pull of the truth is weaker for misaligned senders: A final, related hypothesis is that misaligned senders should be less responsive to the true model than aligned senders. Essentially, the misaligned senders have incentives to persuade the receiver to move away from the truth, and they are constrained only by the receivers information set (i.e., the historical data, which yields the data-optimal model) and their own truth-telling preferences. If misaligned senders have no truth-telling preferences, they will completely disregard the truth and it will play no role in influencing their report. In this hypothesis we check whether misaligned senders: (a) are influenced by the truth, and (b) whether the size of this influence (pull towards the truth) is smaller than it is for aligned senders.

Hypothesis 7b. *The distance between the data-optimal model and the misaligned sender's report is governed to a lesser extent by the size of $|\theta^{T}_{post} - \theta^{DO}_{post}|$ than in the aligned sender's report.*

In the regression model specified above, we test whether: (i) $\beta_2 + \beta_3 > 0$, namely whether misaligned senders are responsive to the truth at all, and (ii) $\beta_3 < 0$, namely whether they are less responsive than aligned senders.

Tentative plans for additional exploratory analysis: In addition to the analysis specified above that is aimed at testing the hypotheses that we have outlined, we plan to also include some more exploratory analyses in the paper. We view it as being potentially useful to provide a description (snapshot) of our tentative plans regarding this exploratory analysis, although we note that this analysis is likely to change in the final version of the paper (in the paper, we will indicate which analyses were pre-registered and which are exploratory). Our tentative plans include the following: We plan to estimate regression models that explain the sender's report as a function of their type, the data optimal model, the period they report on, and the true model. We also plan to measure the percentage of sender messages that are consistent with utility maximization and investigate how messages deviate from the theoretical benchmark. Appendices A.2 and A.3 contain further details.

4 Sample Size

As most of our planned hypothesis tests either: (i) compare the BASELINE treatment to one of our other treatment conditions, or (ii) compare participants within the BASELINE treatment, we will collect more observations for BASELINE than for the other treatments. In particular, we plan to collect data from 360 participants (180 senders and 180 receivers) in BASELINE and from 180 participants in each of the remaining treatments. The sample size gives us 80% power to detect a minimum treatment effect of 2.3 when considering the distance between the receiver's assessment and the truth at the 5%-level.[19] We based the power analysis on data we collected in a pilot of the BASELINE treatment where we found that the distance between the receiver's assessment and the truth, our main outcome variable of interest, had a mean of 17.467 and a standard deviation of 14.094.

Therefore, in total, we will collect approximately 900 observations in these four treatment conditions: 360 in BASELINE, 180 in SKEPTICISM, 180 in SEQUENTIAL and 180 in PRIVATEDATA. Within each treatment, half will be senders and half receivers. Amongst senders, one-third will be randomly assigned to each incentive condition.

[19] In the power analysis, we randomly draw observations from the pilot data and simulate regression results that include round fixed-effects and which cluster standard errors at the matching group level.

A Appendix

A.1 Construction of the empirical plausibility index

In this section, we show how we determine the model that is most likely to have generated a history of outcomes. Possible models consist of parameter combinations $(c, \theta_{pre}, \theta_{post})$. The data set consists of a vector $h = (\omega_1, \omega_2, ..., \omega_{10})$, where $\omega_t \in \{0, 1\}$. An $\omega_t = 1$ denotes "success" and an $\omega_t = 0$ denotes "failure". For each possible parameter combination and data set, we can calculate the empirical likelihood as follows:

$$\begin{aligned}\mathcal{L}(c, \theta_{pre}, \theta_{post}|h) &= \prod_{t=1}^{c}(\theta^S_{pre})^{\omega_t}(1-\theta^S_{pre})^{1-\omega_t} \times \prod_{t=c+1}^{10}(\theta^S_{post})^{\omega_t}(1-\theta^S_{post})^{1-\omega_t}, \\ &= (\theta^S_{pre})^{\omega_1+...+\omega_c}(1-\theta^S_{pre})^{c-(\omega_1+...+\omega_c)} \times (\theta^S_{post})^{\omega_{c+1}+...+\omega_{10}}(1-\theta^S_{post})^{10-c-(\omega_c+...+\omega_{10})}, \\ &= (\theta^S_{pre})^{k_{pre}}(1-\theta^S_{pre})^{c-k_{pre}} \times (\theta^S_{post})^{k_{post}}(1-\theta^S_{post})^{10-c-k_{post}}.\end{aligned} \tag{4}$$

In the equation above, $k_{pre} \equiv \sum_{t=1}^{c}\omega_t$ denotes the number of successes before the structural break and $k_{post} \equiv \sum_{t=c+1}^{10}\omega_t$ denotes the number of successes after the structural break. We further know that, fixing c, the maximum likelihood estimator of θ_{pre} and θ_{post} is equal to $\theta^{DO}_{pre}(c) = k_{pre}/c$ and $\theta^{DO}_{post}(c) = k_{post}/(10-c)$. Therefore, the optimal year of change c^{DO} for a given data set h is equal to $\arg\max_{c\in\{2,3,...,8\}} \mathcal{L}(c, \theta^{DO}_{pre}(c), \theta^{DO}_{post}(c)|h)$.

We evaluate the empirical plausibility index of sender's messages (EPI) for a given data set by comparing the empirical likelihood of the sender's model to the that of the model that is most likely to have generated the data as follows:

$$\text{EPI}(c^S, \theta^S_{pre}, \theta^S_{post}|h) := \frac{\mathcal{L}(c^S, \theta^S_{pre}, \theta^S_{post}|h)}{\max_c \mathcal{L}(c, \theta^{DO}_{pre}(c), \theta^{DO}_{post}(c)|h)}.$$

Since, for any data set, there always exists a model which induces a minimized likelihood value of zero,[20] the empirical plausibility index is scaled to take on values between zero and one. An empirical plausibility index of one suggests that the sender sent the model that is most likely to have generated the data set, while a value of zero suggests that the sender sent the model which is least likely to have generated the data set.

Relation to Schwartzstein and Sunderam (2021) S&S conceptualize an agent who will be

[20]For a given c, if either $k_{pre} < c$ or $k_{post} < 10 - c$, setting $\theta_{pre} = \theta_{post} = 1$ will result in a likelihood value of zero. If the history only consists of successes, so that $k_{pre} = c$ and $k_{post} = 10 - c$, setting $\theta_{pre} = \theta_{post} = 0$ will result in a likelihood value of zero. A model which induces a zero likelihood value thus always exists. Since the likelihood function can never take on negative values, we conclude that its minimum value is zero.

persuaded by a model whenever that model provides a better empirical fit of the data than an initial default model held by the agent. The empirical fit is thereby measured by the likelihood conditional on the data and the agent's prior beliefs. Whenever some model induces a higher EPI than another model, an agent in S&S would prefer the first model. To show this equivalence more precisely, we derive the posterior distribution over $(c, \theta_{pre}, \theta_{post})$ that a Bayesian agent with prior belief $\psi(c, \theta_{pre}, \theta_{post})$ would hold after observing h. Denote this posterior distribution by $f(c, \theta_{pre}, \theta_{post}|h, \psi)$. Using Bayes' rule,

$$f(c, \theta_{pre}, \theta_{post}|h, \psi) = \frac{f(h|c, \theta_{pre}, \theta_{post})\psi(c, \theta_{pre}, \theta_{post})}{\sum_{x=2}^{8} \int_{y\in[0,1]} \int_{z\in[0,1]} f(x, y, z|h)\psi(x, y, z)\,\mathrm{d}y\mathrm{d}z}.$$

Now, $\psi(c, \theta_{pre}, \theta_{post})$ is constant for all potential messages, since we specified a data generating process where all parameters are uniformly distributed and independent of one another. Further, the denominator in the equation above is constant over all potential messages. It follows that the joint distribution is directly proportional to $f(h|c, \theta_{pre}, \theta_{post})$, which is equal to the likelihood function in (4). As a consequence, any message which maximizes the likelihood function also maximizes the joint distribution of parameters. Therefore, a message that suggests a model with EPI = 1 would always (weakly) persuade an agent regardless of the default model in S&S. More generally, if for any two models $(c', \theta'_{pre}, \theta'_{post})$ and $(c'', \theta''_{pre}, \theta''_{post})$ EPI$(c', \theta'_{pre}, \theta'_{post})$ > EPI$(c'', \theta''_{pre}, \theta''_{post})$, an agent in S&S would judge the former model more plausible.

Comparison to the beta-binomial updating formula A different popular belief benchmark in the literature is to compare stated beliefs about certain parameters to their objective Bayesian expected value. We will consider the case where an agent forms a degenerate belief about c and subsequently arrives at non-degenerate beliefs for θ_{pre} and θ_{post} using Bayesian updating. Before seeing any data, agents hold a uniform prior over θ_{pre} and θ_{post}. A uniform distribution on $[0, 1]$ can be represented by a beta distribution with parameters $\alpha = 1$ and $\beta = 1$. The mean of a beta distribution is given by

$$\frac{\alpha}{\alpha + \beta}.$$

Upon seeing n realizations of the state (success/failure), out of which k are successes and l are failures, agents update the parameters of the beta distribution to $\tilde{\alpha} = \alpha + k$ and $\tilde{\beta} = \beta + l$. The posterior *mean* belief is thus equal to

$$\frac{\tilde{\alpha}}{\tilde{\alpha} + \tilde{\beta}} = \frac{\alpha + k}{\alpha + \beta + n}.$$

This is also known as the beta-binomial updating formula. However, the posterior *mode* of a

beta distribution is given by

$$\min\left\{\frac{\tilde{\alpha}-1}{\tilde{\alpha}+\tilde{\beta}-2}, 1\right\} \text{ if } \tilde{\alpha} > 1, \tilde{\beta} \geq 1.$$

Consider the following example of a Bayesian agent who observes $h' = (1, 1, 1, 1, 0, 0, 1, 0, 1, 0)$ and believes that $c = 4$. If non-degenerate, their posterior belief over θ_{pre} is distributed according to a beta distribution with $\alpha = 5$ and $\beta = 1$. Their *mean* belief of θ_{pre} is thus equal to $5/6$. In contrast, an agent in S&S would find an estimate of θ_{pre} more persuasive that maximizes the likelihood function. This estimate is equal to the empirical frequency of successes in periods 1-4; $4/4$. Similarly, the expected value of θ_{post} according the beta-binomial updating formula is $3/8$, whereas the maximum likelihood estimate of θ_{post} for $c = 4$ is $2/6$. These considerations imply that $\text{EPI}(4, 4/4, 2/6|h') > \text{EPI}(4, 5/6, 3/8|h')$. It is straightforward to verify that the maximum likelihood estimates coincide with the mode of the updated beta distribution. Therefore, it is best to think of our EPI measure as quantifying the plausibility of a model under the assumption that agents evaluate the model's likelihood against the historical data and accept the model whenever the likelihood is sufficiently high. When they accept the model, they form a degenerate belief about $(c, \theta_{pre}, \theta_{post})$, which is equal to the parameters of the accepted model.

A.2 Analysis of senders' messages

To gain a more fine-grained insight into sender strategies, we will specify and estimate regression models that explain a sender's report of θ_{pre} and θ_{post} as a function of the sender's type, the empirical data, the period they report on, and the true model. We offer two approaches to specifying such models.

Parametric approach We take the *pre* and *post* report of a sender as the outcome variable (θ_t^S) and specify the regression model

$$\begin{aligned}\theta_t^S =& \beta_0 + \beta_1 \mathbb{I}(\text{t = post}) + \mathbb{I}(\text{type = upward}) \times [\beta_2 + \beta_3 \mathbb{I}(\text{t = post})] \\ &+ \mathbb{I}(\text{type = downward}) \times [\beta_4 + \beta_5 \mathbb{I}(\text{t = post})] \\ &+ \delta_1 \theta_t^T + \delta_2 \theta_t^{DO} + \rho_r + \varepsilon.\end{aligned}$$

We call this the "parametric approach" since we explicitly include θ_t^T and θ_t^{DO} as benchmark controls in the regression. Therefore, estimated effects of the sender type and on the reporting period are relative to these benchmarks. In the later presented semiparametric approach, we instead measure differences in reporting relative to the empirically observed average report. Let us highlight the interpretation of a number of coefficients and their expected signs:

- β_2 and β_3 capture deviations in the reporting behavior of an upward biased sender relative to the behavior of an aligned sender, separately for the *pre* and for the *post* period. We expect $\beta_2 < 0$ and $\beta_3 > 0$ (see hypotheses 6a and 6b).

- β_4 and β_5 capture deviations in the reporting behavior of an downward biased sender relative to the behavior of an aligned sender, separately for the *pre* and for the *post* period. We expect $\beta_4 > 0$ and $\beta_5 < 0$ (see hypotheses 6a and 6b).

To examine aligned senders, notice that they essentially have the same incentives to bias their model away from the data-optimal model as the upward biased sender if $\theta_t^T - \theta_t^{DO} > 0$ and an incentive as the downward biased sender if $\theta_t^T - \theta_t^{DO} < 0$. We introduce the terms

$$\begin{aligned}(\theta_t^T - \theta_t^{DO})\left[\beta_6 + \beta_7 \mathbb{I}(\text{t = post})\right] \\ +\mathbb{I}(\text{type = upward})(\theta_t^T - \theta_t^{DO}) \times \left[\beta_8 + \beta_9 \mathbb{I}(\text{t = post})\right] \\ +\mathbb{I}(\text{type =downward})(\theta_t^T - \theta_t^{DO}) \times \left[\beta_{10} + \beta_{11} \mathbb{I}(\text{t = post})\right]\end{aligned}$$

in the regression above. Here we accordingly expect that $\beta_6 < 0$ and $\beta_7 > 0$.

Semiparametric approach The experiment provides a high degree of variation in the histories that sender-receiver pairs observe. With the semiparametric approach, we will use this feature of the experimental design to maximize what we can learn from the data. The method we will use consists of "mirroring" histories, as described in the following. One can construct a mirror image of any history of past outcomes h that reverses the timing of success and failure. For example, the history $h = (1, 0, 1, 1, 1, 0, 0, 0, 0, 1)$ has a mirror image history $h' = (1, 0, 0, 0, 0, 1, 1, 1, 0, 1)$. More formally, h' is a mirror image of h if $\omega_t = \omega'_{10-(t-1)}$ for all $t \in \{1, \ldots, 10\}$, $\omega_t \in h$ and $\omega'_t \in h'$. Observe that h' is a mirror of h if and only if h is a mirror of h'. We will refer to any two histories (h, h') where h' is a mirror of h as a "mirror pair".[21]

This part of our analysis consists of identifying mirror pairs for which the set of all senders collectively report at least two models (one for each history of the pair) in the experimental data. We then compare the θ^S_{pre} from one history of the pair to the θ^S_{post} from the other history of the pair. This comparison allows us to cleanly identify the directions into which senders bias their reports. A sender who always reports the true data generating model should on average report the same θ_{pre} for history h as θ_{post} for history h' and the same θ_{pre} for history h' as θ_{post} for history h. On the contrary, a sender who exaggerates the post period success probability should report a θ_{post} for history h that is larger than the θ_{pre} for history h', etc. Table 1 presents example of experimental data and the comparisons we will make. To facilitate the analysis we will typically transform the data from a wide format as displayed in table 1 to a long format as displayed in

[21] Note that the definition implies that (h, h) is a mirror pair if h is symmetric.

Table 1: Example of experimental data and planned comparisons

θ^S_{pre}	θ^S_{post}	history	sender type	mirror pair id
0.3	0.6	h	upward biased	12
0.5	0.45	h'	upward biased	12
0.7	0.3	h"	aligned	31
0.3	0.7	h"'	aligned	31

Table 2: The long version of Table 1

θ^S	period	history	sender type	mirror pair id	$\mathbb{I}$(reference history)	$\mathbb{I}$(comparable to reference *pre* report)
0.3	pre	h	upward biased	12	1	1
0.6	post	h	upward biased	12	1	0
0.5	pre	h'	upward biased	12	0	0
0.45	post	h'	upward biased	12	0	1
0.7	pre	h"	aligned	31	1	1
0.3	post	h"	aligned	31	1	0
0.3	pre	h"'	aligned	31	0	0
0.7	post	h"'	aligned	31	0	1

table 2. The long format has only one column for the sender report θ^S which can either denote a report for the *pre* or *post* period probability of success. It doubles the size of the experimental data, as we have two reports (one pre and one post) for each sender and round. We will specify the same models as in the parametric approach but, instead of controlling for $\delta_2\theta_t^{DO}$ we will include mirror pair dummies $\pi_{p,comp}$. The mirror pair fixed effect indicator, $\pi_{p,comp}$, differs by two variables, the pair id p and a binary indicator $comp \in \{0, 1\}$ which varies within mirror pairs. The reason is that not every parameter between two histories is comparable. Instead, we can only compare the *pre* report of a history to the *post* report of the mirror history. For that reason we define for each mirror pair a *reference history* that we use to construct the indicator in the fixed effect to absorb differences in average reporting between the two possible θ reports for each history. For example, in table 2, h is the reference history for mirror pair 12. The pre report of the reference history is comparable to the post report of the nonreference history. Therefore, the indicator $\mathbb{I}$(comparable to reference *pre* report) is 1 in rows 1 and 4 of the table and 0 in rows 2 and 3. Similar comparisons apply to mirror pair 31, where h'' is the reference history.

In comparison to the parametric approach, in the nonparametric approach we do not assume that senders know the data optimal model and choose their message accordingly. Instead, we only assume that, absent any incentives to bias the report away from the data-optimal message and concerns for truth-telling, senders will send a message after seeing history h that mirrors their message after seeing message h' if (h, h') are a mirror pair.

A.3 Theoretical framework

This section sketches a framework that guides our secondary hypotheses. The framework largely follows S&S but is in some ways adjusted to our setting.

Consider a sender whose goal is to persuade a receiver of a certain model. The sender and receiver observe a history of outcomes h. The history records for each of ten years t the success ($\omega_t = 1$) or failure ($\omega_t = 0$) of a company, which is generated by a true data generating model m^T. A model consists of a year of change $c \in \{2, 3, \ldots, 8\}$, a pre-change success probability $\theta_{pre} \in [0, 1]$ and a post-change success probability $\theta_{post} \in [0, 1]$. The company's outcome in each year up to the year of change is drawn from a binomial distribution with success probability θ_{pre}. In years $t > c$, the company's outcome is drawn from a binomial distribution with success probability θ_{post}. We will use "pre period" to describe the range of years up to the year of change and "post period" to describe the range of years after the year of change. The timing of the game is as follows:

(i) Nature draws three parameters $(c^T, \theta^T_{pre}, \theta^T_{post})$ that form the true data generating model. Each of the parameters is drawn from a uniform distribution and is uncorrelated with the other parameters.

(ii) The true data generating model generates a history h.

(iii) The receiver observes h and draws a default model m^D from a distribution function $M(c, \theta_{pre}, \theta_{post}|h)$.

(iv) The sender observes h and sends a model $m^S = (c^S, \theta^S_{pre}, \theta^S_{post})$ to the receiver.

(v) The receiver decides whether to adopt the sender's model. In case the receiver accepts, they make a report θ^S_{post}. Otherwise, they report the value θ^D_{post} of the default model.

(vi) Sender payoffs realize.

Following S&S, we consider the receiver to be a nonstrategic agent who decides as if their objective is to adopt the most compelling model. Models can be evaluated by their fit, which we take to be equal to the value of the log likelihood function evaluated at the model parameters.[22] For a history that is generated as described above, the log likelihood function is

$$ll(c, \theta_{pre}, \theta_{post}) = k_{pre}(c)log(\theta_{pre}) + f_{pre}(c)log(1-\theta_{pre}) + k_{post}(c)log(\theta_{post}) + f_{post}(c)log(1-\theta_{post}).$$[23]

In the equation, $k_{pre}(c) = \sum_{t=1}^{c} \omega_t$ denotes the number of successes and $f_{pre}(c) = c - k_{pre}(c)$

[22] As discussed in section A.1, this is how a Bayesian agent would choose among models in our setting.

[23] Here and in the following, we usually do not condition functions on a particular history h to save notation.

denotes the number of failures in the pre-period. The values $k_{post}(c) = \sum_{t=c+1}^{10} \omega_t$ and $f_{post}(c) = 10 - c - k_{post}(c)$ similarly denote the number of successes and failures in the post period. For a given year of change c, there is always one pair $(\theta_{pre}, \theta_{post})$ that maximizes the log likelihood function. We denote these likelihood maximizers by $\hat{\theta}_{pre}(c)$ and $\hat{\theta}_{post}(c)$. Closed-form solution exist. In period p, the likelihood maximizer given c is equal to the number of successes divided by the total length of the period; $\hat{\theta}_p(c) = k_p(c)/(k_p(c) + f_p(c))$. The following discussion assumes that the log likelihood function has a unique optimum.[24] We call the model that maximizes the log likelihood function the data-optimal model and denote it by $m^{DO} = (c^{DO}, \hat{\theta}^{DO}_{pre}, \hat{\theta}^{DO}_{post})$. Most of the time, the data-optimal model will be different from the true data-generating model.

Receiver types The receiver's type depends on the drawn default model.[25] The distribution of default models implies a distribution of log likelihood function values with c.d.f. $G(\ell)$ and p.d.f. $g(\ell)$. For simplicity, we assume that g has full support over all possible values of the likelihood function, i.e., $g(\ell) > 0$ for all $\ell \in (-\infty, ll(m^{DO})]$. The default model is private information to the receiver, though the sender knows that its log-likelihood value is distributed according to $G(\ell)$.[26]

Sender types The sender can either be aligned, upward biased or downward biased. The receiver's report will determine the sender's payoff in different ways, depending on the sender's type. In particular, the receiver's report θ^R_{post} maps into the sender's payoff according to a scoring rule

$$1 - (\varphi - \theta^R_{post})^2.$$

This rule assigns the sender the maximum score whenever the receiver reports sender's target φ. If the sender is aligned φ is equal to θ^T_{post}, if the sender is upward biased φ is equal to 1, and if the sender is downward biased φ is equal to 0. Since the receiver adopts the sender's model if it provides at least the same fit as the default model, the sender's expected utility from sending a model m^S is

$$\begin{aligned} u(c^S, \theta^S_{pre}, \theta^S_{post}; h, \varphi) = {} & \mathbb{P}(ll(c^S, \theta^S_{pre}, \theta^S_{post}) \geq \ell)[1 - (\varphi - \theta^S_{post})^2] \\ & + \mathbb{P}(ll(c^S, \theta^S_{pre}, \theta^S_{post}) < \ell)\mathbb{E}[1 - (\varphi - \theta^D_{post})^2 | ll(c^S, \theta^S_{pre}, \theta^S_{post}) < \ell]. \end{aligned}$$

In the equation above, $\mathbb{E}[1 - (\varphi - \theta^D_{post})^2 | ll(c^S, \theta^S_{pre}, \theta^S_{post}) < \ell]$ is the sender's expected payoff when the receiver does not adopt the sender's model. We make the simplifying assumption that the sender believes this expectation term to be equal to a value $x \in (0, 1)$, which is independent

[24] This holds for almost all possible histories. Degenerate histories like (1, 1, 1, 1, 1, 1, 1, 1, 1, 1), for which any year can be part of a data optimal model, are the exceptions.

[25] This is a major difference between our framework and S&S, who assume only one type of receiver.

[26] As will become clear later, this assumption could be relaxed by quite a bit without qualitatively changing the results. What is important is that the sender knows that (*i*) the receiver holds a default model that might not be equal to the data-optimal model and that (*ii*) the receiver adopts the sender's model whenever the sender's model has a log likelihood value at least equal to the receiver's default model.

of the sender's message.[27] Plugging in $G(\ell)$ and the sender expectation, the sender's expected utility function is equal to

$$u(c^S, \theta^S_{pre}, \theta^S_{post}; h, \varphi) = G(ll(c^S, \theta^S_{pre}, \theta^S_{post}))[1 - (\varphi - \theta^S_{post})^2] + (1 - G(ll(c^S, \theta^S_{pre}, \theta^S_{post})))x.$$

The maximization problem can then be written as

$$\max_{c, \theta_{pre}, \theta_{post}} G(ll(c, \theta_{pre}, \theta_{post}))(1 - x - (\varphi - \theta_{post})^2).$$

A.3.1 Analysis

We analyse sender behaviour. In the first part of the analysis we focus on a misaligned, i.e., upward or downward biased sender. We extend the results to the aligned sender at the end of the section. Throughout the analysis, we will often benchmark sender strategies by comparing them to the strategy that communicates the data-optimal model m^{DO}. Since $G(ll(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post})) = 1$, the receiver always adopts the data-optimal model upon reception. In the analysis below, we somewhat informally assume that x, the sender's payoff when the receiver does not adopt the sender's model, is close to zero. We do not believe that this is a meaningful restriction: For any x, all the results below would go through under the qualifier that the sender only chooses among models which induce a scoring rule payoff $1 - (\varphi - \theta^R_{post})^2 \geq x$.

The sender faces a conflict between an *accuracy motive* which induces them to communicate a model with a high fit that is likely adopted by the receiver, and a *direction motive* to convince the receiver to report a particular value of θ_{post}. We start with a result that naturally follows from the accuracy-direction tradeoff. The sender only communicates a non data-optimal θ_{post} if it increases the direction motive.

Observation 1. *Consider the choice of the optimal θ^S_{post}:*

(i) An upward biased sender chooses a $\theta^S_{post} \geq \theta^{DO}_{post}$.

(ii) A downward biased sender chooses $\theta^S_{post} \leq \theta^{DO}_{post}$.

Proof. Consider case (i). The data-optimal model dominates the choice of any model $(c', \theta'_{pre}, \theta'_{post})$ with $\theta'_{post} < \theta^{DO}_{post}$ because any such alternative model decreases accuracy and direction motives. Any model $(c'', \theta''_{pre}, \theta''_{post})$ with $\theta''_{post} > \theta^{DO}_{post}$ instead decreases the accuracy motive but (weakly) increases the direction motive. The claim follows. A symmetric argument can be made for case (ii). □

[27] This is a simplifying assumption as, in principle, knowing that the receiver does not adopt a certain model might be informative about the value of θ^D_{post}. While we are aware of this possibility, we regard it as second-order. The assumption above awards us with tractability and allows us to focus on the direct effects of the sender's report.

The direction motive only applies to θ_{post} but not to θ_{pre}. It follows that any sender, regardless of type, will always communicate the likelihood maximizer of θ_{pre} conditional on the year of change.

Observation 2. *For any type of sender who chooses any year of change c^S, θ^S_{pre} is equal to $\hat{\theta}_{pre}(c^S)$.*

Proof. The value of θ_{pre} affects the expected utility function only through the effect it has on $ll(\cdot)$ (the accuracy motive). It follows that choosing the value θ_{post} which maximizes the log likelihood is optimal. □

We now turn to the choice of the optimal cutoff c^S. Consider a sender who considers to communicate the data-optimal model with year of change c^{DO}. It turns out that the sender only considers alternative years of change if they can better rationalize a θ_{post} in line with the direction motive.

Observation 3a. *Consider how an upward biased sender chooses the optimal c^S:*

(i) For years $c' > c^{DO}$ the sender prefers a model with year c' over a model with year c^{DO} if and only if:

- *$f_{post}(c') < f_{post}(c^{DO})$ and*
- *$\theta^S_{post} > \tilde{\theta}_2(c')$, where $\tilde{\theta}_2(c')$ is a critical value on $(\hat{\theta}_{post}(c^{DO}), 1)$.*

(ii) For years $c' < c^{DO}$ the sender prefers a model with year c' over a model with year c^{DO} if and only if:

- *$k_{post}(c') > k_{post}(c^{DO})$, $\hat{\theta}_{post}(c') > \hat{\theta}_{post}(c^{DO})$, and*
- *$\theta^S_{post} \in (\tilde{\theta}^L_2(c'), \tilde{\theta}^H_2(c'))$, where $\tilde{\theta}^L_2(c') > \theta^{DO}_{post}$ and $\tilde{\theta}^L_2(c') \leq \tilde{\theta}^H_2(c') \leq 1$ are two critical values.*

Proof. Let us denote the empirical successes and failures implied by the data-optimal model by k^{DO}_j and f^{DO}_j (for $j \in \{pre, post\}$). We compare the data-optimal model to a model $m' = (c', \theta'_{pre}, \theta'_{post})$ with $c' \neq c^{DO}$ and implied empirical successes and failures k'_j and f'_j. Since m^{DO} maximizes the log likelihood function, it follows that $ll(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post}) > ll(c', \theta'_{pre}, \theta'_{post})$ for any m'. Therefore, any model with cutoff c' can only lead to an increase in the sender's expected utility if it increases the direction motive. For an upward biased sender, m' induces a higher direction motive if $\theta'_{post} > \theta^{DO}_{post}$. We show conditions under which the sender prefers to communicate a model $(c', \theta'_{pre}, \theta'_{post})$ over $(c^{DO}, \theta^{DO}_{pre}, \theta'_{post})$. As both models have the same direction motive, the sender prefers the first to the second model only if the log likelihood difference

$ll(c', \theta'_{pre}, \theta'_{post}) - ll(c^{DO}, \theta^{DO}_{pre}, \theta'_{post}) > 0$ is positive. This difference has a number of important properties. First, consider the value of the difference when evaluated at θ^{DO}_{post}. Since m^{DO} maximizes the log likelihood function, it follows that $ll(c', \theta'_{pre}, \theta^{DO}_{post}) - ll(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post}) < 0$. Second, the sign of the derivative of the difference with respect to θ'_{post} when evaluated at θ^{DO}_{post} depends on likelihood maximizer of θ_{post} under the alternative model, $\hat{\theta}_{post}(c')$, as follows:

$$\frac{\partial ll(c', \theta'_{pre}, \theta^{DO}_{post})}{\partial \theta_{post}} - \frac{\partial ll(c^{DO}, \theta^{DO}_{pre}, \theta^{DO}_{post})}{\partial \theta_{post}} \begin{cases} \leq 0 & \text{if } \hat{\theta}_{post}(c') \leq \theta^{DO}_{post} \\ > 0 & \text{if } \hat{\theta}_{post}(c') > \theta^{DO}_{post}. \end{cases}$$

In both cases, the derivative of the log likelihood evaluated at the data-optimal model is zero, since it is evaluated at the optimum. The sign of the difference is then fully determined by the sign of the log likelihood derivative evaluated at the alternative model. It is negative if $\hat{\theta}_{post}(c') < \theta^{DO}_{post}$ (the log likelihood is past its peak) and positive if $\hat{\theta}_{post}(c') > \theta^{DO}_{post}$ (the peak is still to come). Another important property of the log likelihood functions is that they cross at most once for values of $\theta_{post} \in [0, 1]$. We show this by taking the derivative of the log likelihood difference with respect to θ_{post};

$$\frac{\partial ll(c', \theta'_{pre}, \theta_{post})}{\partial \theta_{post}} - \frac{\partial ll(c^{DO}, \theta^{DO}_{pre}, \theta_{post})}{\partial \theta_{post}} = \frac{k'_{post} - k^{DO}_{post}}{\theta_{post}} + \frac{f^{DO}_{post} - f'_{post}}{1 - \theta_{post}}.$$

Note that, if they are nonzero, the two terms on the right hand side always have the opposite sign because either $k^{DO}_{post} \geq k'_{post}$ and $f^{DO}_{post} \geq k'_{post}$ or $k^{DO}_{post} \leq k'_{post}$ and $f^{DO}_{post} \leq k'_{post}$ (in both cases, at least one inequality is strict). Setting the derivative equal to zero and rearranging, we find that

$$\frac{\theta^0_{post}}{1 - \theta^0_{post}} = \frac{k^{DO}_{post} - k'_{post}}{f^{DO}_{post} - f'_{post}},$$

which implies a unique θ^0_{post} as a solution. This value is equal to

$$\theta^0_{post} = \frac{k^{DO}_{post} - k'_{post}}{k^{DO}_{post} - k'_{post} + f^{DO}_{post} - f'_{post}}.$$

The log likelihood difference can thus either increase, decrease, first increase and then decrease or first decrease and then increase for $\theta_{post} \in [0, 1]$. We now distinguish between a number of cases that determine the shape of the log likelihood difference.

Case 1: $k'_{post} = k^{DO}_{post}$. The critical value θ^0_{post} is equal to zero. It directly follows that the likelihood difference is monotone. If $\hat{\theta}_{post}(c') > \theta^{DO}_{post}$ ($f'_{post} < f^{DO}_{post}$) it increases, if $\hat{\theta}_{post}(c') < \theta^{DO}_{post}$ ($f'_{post} > f^{DO}_{post}$) it decreases.

Case 2: $k'_{post} > k^{DO}_{post}$ *and* $f'_{post} > f^{DO}_{post}$. The derivative of the log likelihood difference changes its sign at θ^0_2. We ask whether $\theta^0_2 \gtrless \theta^{DO}_{post}$. Plugging in values, this is equivalent to showing whether

$$\frac{k^{DO}_{post} - k'_{post}}{k^{DO}_{post} - k'_{post} + f^{DO}_{post} - f'_{post}} \gtrless \frac{k^{DO}_2}{k^{DO}_2 + f^{DO}_2}.$$

After rearranging, we find that

$$\theta^0_2 > \theta^{DO}_{post} \text{ if } \hat{\theta}_{post}(c') > \theta^{DO}_{post} \text{ and } \theta^0_2 \leq \theta^{DO}_{post} \text{ if } \hat{\theta}_{post}(c') \leq \theta^{DO}_{post}.$$

Since we know the sign of the derivative at θ^{DO}_{post}, this pins down the whole shape of the derivative; it first increases and then decreases.

The additional cases $f'_{post} = f^{DO}_{post}$ and $k'_{post} < k^{DO}_{post}$ and $f'_{post} < f^{DO}_{post}$ follow in a similar way. We summarize the results in the table below.

Table 3: Shape of the log likelihood difference for different parameter combinations

	$k'_{post} = k^{DO}_{post}$	$k'_{post} > k^{DO}_{post}$ and $f'_{post} > f^{DO}_{post}$	$f'_{post} = f^{DO}_{post}$	$k'_{post} < k^{DO}_{post}$ and $f'_{post} < f^{DO}_{post}$
$\hat{\theta}_{post}(c') > \theta^{DO}_{post}$	Increasing	First increasing, then decreasing Peak at $\theta^0_{post} > \theta^{DO}_{post}$	Increasing	First decreasing, then increasing Minimum at $\theta^0_{post} < \theta^{DO}_{post}$
$\hat{\theta}_{post}(c') \leq \theta^{DO}_{post}$	Decreasing	First increasing, then decreasing Peak at $\theta^0_{post} \leq \theta^{DO}_{post}$	Decreasing	First decreasing, then increasing Minimum at $\theta^0_{post} \geq \theta^{DO}_{post}$

As a final property of the log likelihood difference, when taking the limit of $\theta_{post} \to 1$ we find that

$$\lim_{\theta'_{post} \to 1} \left[ll(c', \theta'_{pre}, \theta'_{post}) - ll(c^{DO}, \theta^{DO}_{pre}, \theta'_{post}) \right] = \lim_{\theta'_{post} \to 1} log(1 - \theta'_{post})(f'_{post} - f^{DO}_{post}) + \kappa, \qquad (5)$$

where κ is a number independent of θ_{post}. Since $\lim_{\theta_{post} \to 1} log(1 - \theta_{post}) = -\infty$, the difference is positive in the limit if $f^{DO}_{post} > f'_{post}$ and negative if $f^{DO}_{post} < f'_{post}$.

This discussion has a number of implications for sender strategies. Consider the first row of table 3 where $\hat{\theta}_{post}(c') > \theta^{DO}_{post}$.

- If $k'_{post} = k^{DO}_{post}$ it must be that $f'_{post} < f^{DO}_{post}$. Since the difference is positive in the limit as θ'_{post} becomes large, there is one value $\tilde{\theta}_2 \in (\theta^{DO}_{post}, 1)$ so that a model that couples $\theta^S_{post} > \tilde{\theta}_{post}$ with c' has a larger likelihood than a model with c^{DO}.
- If $k'_{post} > k^{DO}_{post}$ and $f'_{post} > f^{DO}_{post}$ there might be a range of values between $(\theta^{DO}_{post}, 1)$ for which a model that couples a θ^S_{post} in that range with c' has a larger likelihood than a model with c^{DO}.

- If $f'_{post} = f^{DO}_{post}$ the difference is increasing. From equation (5), a value $\tilde{\theta}_{post} \in (\theta^{DO}_{post}, 1)$ under which a model with c' and $\theta^{S}_{post} > \tilde{\theta}_2$ has a larger likelihood only exists if $\kappa > 0$.

- If $k'_{post} < k^{DO}_{post}$ and $f'_{post} < f^{DO}_{post}$ there is one value $\tilde{\theta}_{post} \in (\theta^{DO}_{post}, 1)$ so that a model that couples $\theta^{S}_{post} > \tilde{\theta}_{post}$ with c' has a larger likelihood than a model with c^{DO}.

Consider the second row of table 3 where $\hat{\theta}_{post}(c') \leq \theta^{DO}_{post}$.

- If $k'_{post} = k^{DO}_{post}$ or $f'_{post} = f^{DO}_{post}$ an upward biased sender would never choose the model c' since its likelihood is lower than that of the data-optimal model for all values $\theta^{S}_{post} \geq \theta^{DO}_{post}$.

- If $k'_{post} > k^{DO}_{post}$ and $f'_{post} > f^{DO}_{post}$ then the difference starts decreasing before θ^{DO}_{post}. Since it is negative at θ^{DO}_{post}, there is no $\theta^{S}_{post} \geq \theta^{DO}_{post}$ where the alternative model has a higher likelihood.

- If $k'_{post} < k^{DO}_{post}$ and $f'_{post} < f^{DO}_{post}$ there is one value $\tilde{\theta}_2 \in (\theta^{DO}_{post}, 1)$ so that a model that couples $\theta^{S}_{post} > \tilde{\theta}_{post}$ with c' has a larger likelihood than a model with c^{DO}.

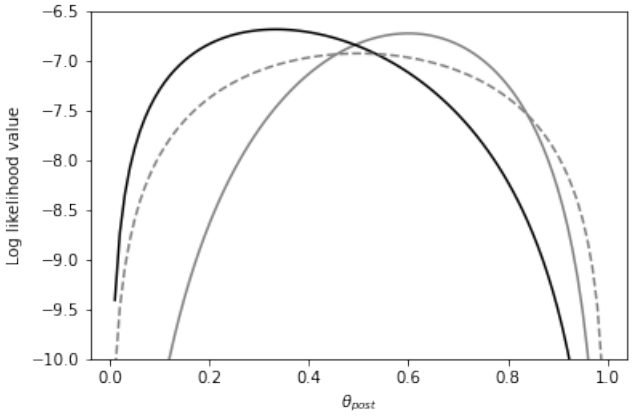
Finally, note that $c' < c^{DO}$ if and only if $k'_{post} \geq k^{DO}_{post}$ and $f'_{post} \geq f^{DO}_{post}$ and that $c' > c^{DO}$ if and only if $k'_{post} \leq k^{DO}_{post}$ and $f'_{post} \leq f^{DO}_{post}$. The above considerations imply the claims in the observation. □

This result puts restrictions on the years of change an upward biased sender is willing to communicate. In words, the observation says that a sender will only choose a later year if the later year implies fewer failures in the post period. Conversely, the sender will only choose an earlier year if the earlier year implies more successes in the post period. Perhaps surprisingly, the sender is slightly more constrained in choosing an earlier than a later year. The reason for this asymmetry seems to be the following: As θ_{post} becomes very large, the log likelihood function puts a strong penalty on any failure in the second period so that this term dominates the function value (intuitively, with a high θ_{post} failures are difficult to explain). Therefore, a later year under which fewer failures happen in the post period becomes more attractive. On the converse, an earlier year does not lower the number of failures, which makes it unattractive for high values of θ_{post}.

Since, for a fixed θ_{post}, the direction motive is held constant for any c, the sender prefers the year of change which maximizes the log likelihood function. The figure below plots log likelihood functions for different values of c and for an example history $h = (0, 1, 1, 0, 0, 1, 1, 0, 1, 0)$. The black line displays the log likelihood function with year of change 7. The figure shows that, for intermediate values of θ_{post}, this cutoff is dominated by a model with $c = 5$ (the gray line) which adds two additional successes to the post period. For very high values of θ_{post} both

models are dominated by a model with a later year of change of $c = 8$, whose log likelihood is displayed by the dashed line. This later year of change minimizes the number of failures in the second period.

Figure 2: Log likelihood functions for different c



Note: The graph plots values of three log likelihood functions for different values of θ_{post} and for history $h = (0, 1, 1, 0, 0, 1, 1, 0, 1, 0)$. The black line plots the log likelihood of model $(7, \hat{\theta}_{pre}(7), \theta_{post})$, the grey line of model $(5, \hat{\theta}_{pre}(5), \theta_{post})$, and the dashed line of model $(8, \hat{\theta}_{pre}(8), \theta_{post})$.

We obtain a similar result for the downward biased sender.

Observation 3b. *Consider how a downward biased sender chooses the optimal c^S:*

(i) For years $c' > c^{DO}$ the sender prefers a model with year c' over a model with year c^{DO} if and only if:

- $k_{post}(c') < k_{post}(c^{DO})$ *and*
- $\theta^S_{post} > \tilde{\theta}_2(c')$, *where $\tilde{\theta}_2(c')$ is a critical value on $(\hat{\theta}_{post}(c^{DO}), 1)$.*

(ii) For years $c' < c^{DO}$ the sender prefers a model with year c' over a model with year c^{DO} if and only if:

- $k_{post}(c') < k_{post}(c^{DO})$, $\hat{\theta}_{post}(c') < \hat{\theta}_{post}(c^{DO})$, *and*
- $\theta^S_{post} \in (\tilde{\theta}^L_2(c'), \tilde{\theta}^H_2(c'))$, *where $\tilde{\theta}^L_2(c') > \theta^{DO}_{post}$ and $\tilde{\theta}^L_2(c') \leq \tilde{\theta}^H_2(c') \leq 1$ are two critical values.*

Having gained insight into the choice of c^S, we close with an observation on the sender's optimal model

Observation 4. *Consider the sender's choice of the optimal model* $(c^S, \theta^S_{pre}, \theta^S_{post})$. *Denote by* c^{max} *the year for which* $\hat{\theta}_{post}(c^{max}) = max\left\{\hat{\theta}_{post}(c)\right\}_{c\in\{2,\dots,8\}}$ *and by* c^{min} *the year for which* $\hat{\theta}_{post}(c^{min}) = min\left\{\hat{\theta}_{post}(c)\right\}_{c\in\{2,\dots,8\}}$.

(i) The upward biased sender chooses a model for which either $\theta^S_{post} > \hat{\theta}_{post}(c^{max})$ *or* $ll(c^S, \hat{\theta}_{pre}(c^S), \theta^S_{post}) \geq ll(c^{max}, \hat{\theta}_{pre}(c^{max}), \hat{\theta}_{post}(c^{max}))$ *holds.*

(ii) The downward biased sender chooses a model for which either $\theta^S_{post} < \hat{\theta}_{post}(c^{min})$ *or* $ll(c^S, \hat{\theta}_{pre}(c^S), \theta^S_{post}) \geq ll(c^{min}, \hat{\theta}_{pre}(c^{min}), \hat{\theta}_{post}(c^{min}))$ *holds.*

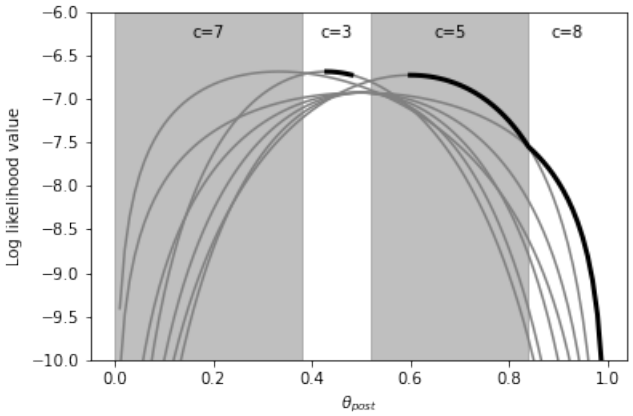
Proof. Consider case (i). Suppose by contradiction that none of the conditions hold. Then the sender could increase the accuracy and the direction motive by transmitting model $(c^{max}, \hat{\theta}_{pre}(c^{max}), \hat{\theta}_{post}(c^{max}))$ instead of model $(c^S, \theta^S_{pre}, \theta^S_{post})$, a contradiction. Starting from a model with $\theta^S_{post} > \hat{\theta}_{post}(c^{max})$, the accuracy motive decreases when moving to model $(c^{max}, \hat{\theta}_{pre}(c^{max}), \hat{\theta}_{post}(c^{max}))$. Starting from a model with $ll(c^S, \hat{\theta}_{pre}(c^S), \theta^S_{post}) \geq ll(c^{max}, \hat{\theta}_{pre}(c^{max}), \hat{\theta}_{post}(c^{max}))$, the direction motive decreases. Therefore, at least one but not both conditions must hold. A symmetric argument can be made to show case (ii). □

Figure 3 plots log likelihood functions of an example history for all possible years of change. It illustrates the upward biased sender's problem to pick among combinations of c and θ_{post}. The black line displays combinations which are consistent with Observation 4. This line has gaps, as some combinations are dominated by other combinations. For example, the data-optimal model in the example has year $c^{DO} = 3$, which makes any θ_{post} to the left of the peak of its likelihood function suboptimal. The c^{max} in this example is equal to 5, which is why the black line continues without gaps vor values of θ_{post} larger than $\hat{\theta}_{post}(5)$.

Aligned sender The direction motive of the aligned sender depends on the true data generating model. For example, if $\theta^T_{post} < \theta^{DO}_{post}$, the aligned sender has an incentive to communicate a θ^S_{post} smaller than the data-optimal value. Whether the aligned sender biases reports upward or downward depends on whether the difference $\theta^T_{post} - \theta^{DO}_{post}$ is smaller or larger than zero.[28] Therefore, the same qualitative theoretical results as for the upward biased sender also hold for the aligned sender when $\theta^T_{post} > \theta^{DO}_{post}$. When instead $\theta^T_{post} < \theta^{DO}_{post}$, the predictions for the aligned sender follow those of the downward biased sender. We however note that the misaligned senders represent extreme cases. Therefore, the predictions for the aligned sender would be quantitatively smaller.

[28] This discussion largely ignores the case where $\theta^T_{post} = \theta^{DO}_{post}$, which is unlikely to ever be exactly true. We note that in this unlikely case, the sender only has an accuracy motive, i.e., the sender will communicate the data-optimal model.

Figure 3: Combinations of c and θ_{post} consistent with utility maximization, upward biased sender



Note: The graph plots values of log likelihood functions for all possible years of change, different values of θ_{post}, and for history $h = (0, 1, 1, 0, 0, 1, 1, 0, 1, 0)$. The years at the top of the figure highlight years that are optimal for values of θ_{post} within the shaded area. The black line highlights values of θ_{post} that are consistent with utility maximization.

Observation 5a. *If $\theta^T_{post} > \theta^{DO}_{post}$, part (i) of Observation 1 and Observation 3a also apply to the aligned sender. If $\theta^T_{post} < \theta^{DO}_{post}$, part (ii) of Observation 1 and Observation 3b also apply to the aligned sender.*

Observation 5b. *Consider the aligned sender's choice of the optimal model $(c^S, \theta^S_{pre}, \theta^S_{post})$. Denote by c^{max} the year for which $\hat{\theta}_{post}(c^{max}) = min\left\{|\theta^T_{post} - \hat{\theta}_{post}(c)|\right\}_{c\in\{2,\dots,8\}}$.*

(i) The aligned sender chooses a model for which either $|\theta^T_{post} - \hat{\theta}_{post}(c)| > |\theta^T_{post} - \hat{\theta}_{post}(c^{max})|$ or $ll(c^S, \hat{\theta}_{pre}(c^S), \theta^S_{post}) \geq ll(c^{max}, \hat{\theta}_{pre}(c^{max}), \hat{\theta}_{post}(c^{max}))$ holds.

A.4 Implications for the empirical analysis

The observations above provide benchmarks for sender behavior. In particular, we can measure the percentage of biased sender messages that are consistent with parts (i) and (ii) of Observation 4 and, using Observation 5b, we can do a similar exercise for aligned senders.

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