Accounting Conservatism as a Social Norm

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ABSTRACT: We consider whether accounting conservatism emerged from a social norm that enhances trust and cooperation among personal exchange partners. We create an exchange setting featuring (i) information asymmetry, (ii) measurement uncertainty, and (iii) senders motivated to report aggressively. We conduct an experiment in which receivers and senders interact in pairs for ten periods, and each period receivers rank each sender in order of preference, which guides the next period's pairings. We posit that receivers perceive a social norm to apply – an informal rule against aggressive reporting – and use noisy reporting errors to gauge senders' compliance. Consistent with this expectation we find that, *ceteris paribus*, the decline in receivers' rankings is larger for senders producing an incremental overstatement than understatement error. We also find that the decline is larger for senders who produce large overstatement than understatement errors of equal magnitude. We then manipulate senders' motives by aligning them with receivers' and find that receivers' asymmetric preferences over errors are driven by senders' motivation to report aggressively. To the best of our knowledge, ours is the first study to document users' aversion to overstatement errors, which opens the possibility that conservatism first emerged as an informal bonding cost during personal exchange. We believe this insight can open interesting new possibilities for conservatism research.

Keywords: Accounting conservatism; Experimental economics; Social norm; Trust
JEL Codes: B52, D81, D82, M41

1. INTRODUCTION

Societies adapt values such as honesty, integrity, and reliability into informal rules – social norms – that restrict the flexibility of choices under uncertainty in order to change the payoffs to cooperative activity (North 2005). We seek to provide initial empirical evidence that, in a similar fashion, accounting conservatism can emerge as a social norm. While conservatism has been a feature of accounting systems for at least five hundred years (Littleton 1941), scholars' current explanations for its presence all require features of modern economies (Watts 2003). In contrast, we consider whether conservatism derives from the class of home- and socially-grown rules of actions, traditions, and moral principles that promotes specialization and exchange in personal markets (Smith 2008; Dickhaut et al. 2010).

We create a personal exchange setting featuring a sender and a receiver that is characterized by (i) information asymmetry, (ii) measurement uncertainty, and (iii) senders who are motivated to report aggressively. The first two characteristics create uncertainty about the sender's intent, as receivers only observe the senders' *ex post* reporting error (i.e., whether they understated or overstated the realized value). In light of the sender's motive, from the perspective of the receiver the greater the value of the reporting error, the greater the probability the sender had exploitative intent. We posit that receivers view exploitation through aggressive reporting as a violation of an informal rule and, as such, that their trust in a sender corresponds to their assessed probability of being exploited (Yamagishi 1998). The Accounting Principles Board describes conservatism as deriving from a general preference for reporting errors to "be in the direction of understatement rather than overstatement" of net income and net assets (APB 1970, para 171). Our study is motivated by the hypothesis that this preference originates from a social norm that serves to enhance trust and cooperation among exchange partners.

In our game, the sender (Reporter) receives a noisy private signal about his expected yield for the period and sends a report to the receiver (Investor), who sets her level of investment. The Investor's payoff is maximized when she invests an amount equal to the realized yield, and her payoff is symmetric in over- and under-investment errors of equal magnitude. The Reporter's payoff is maximized when the investment is greater than the yield, so he is motivated to report aggressively to try to deceive the Investor and induce greater investment. This motivation has many real-world analogues, including the entrepreneur's inherent optimistic bias (e.g., Devine 1963; Sterling 1967) and, in modern times, corporate managers' incentives to overstate reports in the presence of various agency conflicts (Kothari et al. 2010).

We establish a community of six Reporters and six Investors who interact in pairs for ten periods. At the start of each period, we provide Investors with every Reporter's history of reporting errors and ask them to rank each Reporter in order of preference. We randomly assign Investors into a selection order where, starting with the Investor selecting first, each Investor is paired with her highest-ranked available Reporter. The position in which each Reporter is selected affects his payoff for the period, as the payoff increases (decreases) if they are selected first or second (fifth or sixth). The payoff adjustments provide Investors with a mechanism to sanction Reporters – through reward and punishment – based on their compliance. This process is analogous to competition among real-world entrepreneurs who adapt to customer preferences.

Since the Investor can rationally discount any systematic bias evident in the Reporter's history, she should prefer consistent reporting errors. Further, if she is solely motivated by her own payoff, she should have no preference over the sign of the reporting errors. However, prior research shows that people consider how their counterpart's payoffs affect the intentions behind their actions (see e.g., McCabe et al. 2003). To the extent aggressive reporting leads to aggressive investing, an aggressive report, in expectation, both effects a wealth transfer from Investor to Reporter and reduces social welfare. We posit that Investors view aggressive reporting as exploitative and as violating an informal rule, and that they will use noisy reporting errors to gauge the probability the Reporter has complied with the rule. As such, we hypothesize that, *ceteris paribus*, the decline in Investors' rankings is larger for Reporters producing (i) an incremental overstatement error than an incremental understatement error and (ii) large overstatement errors than understatement errors of equal magnitude.

We test these predictions by regressing the Investors' preference rankings on various characteristics of each Reporter's error history in an ordered logit model. We control for the consistency and accuracy of each Reporter's errors, as well as for the average consistency and accuracy of the *other* Reporters' errors. As expected, Investors prefer Reporters whose errors are consistent and accurate. Controlling for these factors, the results support our predictions. First, the decline in Investors' rankings is larger for Reporters producing an incremental overstatement than understatement error. This indicates that, in the presence of measurement error, the potential reputational costs of an aggressive report are larger than for a conservative one. Second, the decline in Investors' rankings is larger for Reporters who produce large overstatement than understatement errors of equal magnitude. Both this and our prior result indicate that Investors 'punish' those Reporters most likely to have reported aggressively.

Our second hypothesis is that Investors' asymmetric preferences for over- and understatement errors are driven by the Reporters' incentive to report aggressively. To test this, we manipulate Reporters' incentives; in addition to our baseline condition described above (*Misaligned*), we create a control condition (*Aligned*) in which both parties' payoff is maximized when the investment is equal to the yield. We test our prediction in an ordered logit model in which we include data from both the baseline and control conditions, and we test for difference-in-differences: that Investors' asymmetric preferences for understatement errors versus overstatement errors are larger when incentives are misaligned than when aligned. The results support our predictions, as Investors do not have asymmetric preferences for over- and understatement errors when the Reporters' incentives are aligned with their own.

We also conduct an incentivized post-experimental task to bring additional light regarding how participants view trust in this setting. In analyzing these results, we noted a significant mismatch between reporting behavior Investors said they trust and behavior Reporters believe Investors trust. One possibility suggested by this evidence is that our experimental setting did not allow Investors to effectively communicate their preferences to Reporters. A social norm would require agreement between Investors' normative expectations and Reporters' beliefs about them (Bicchieri and Chavez 2010, 166). This evidence suggests that while Investors have an aversion to overstatement errors, perhaps our experimental setting did not provide sufficiently strong signals to communicate relevant expectations to Reporters. That is, while these appear to be strong conditions for us to measure an underlying normative preference, they may not be proper conditions for the norm to be activated.

In our setting, participants interacted with up to six different partners for only ten periods in total. Reporters had to infer all of the Investors' various normative expectations from only their own relative ranking each period, without the benefit of direct communication, and with no knowledge about any prior history of similar interactions. In reality, economic institutions evolve over long periods of time through actions taken by agents who have inherited solutions to frequently encountered problems of the past (North 2005). Future research might address this gap and identify conditions under which Reporters have accurate beliefs about Investors' expectations and identify whether reporting behavior changes to adapt to a group norm.

Under a set of three realistic key conditions, we document that receivers of reports have a normative aversion to overstatement errors. We posit that this arises because overstatement errors cause distrust about the sender's intent and we find that Investors are 109% more likely, *ceteris paribus*, to indicate trust in a Reporter whose aggregate reporting error is negative. We propose that these may be the initial conditions from which modern-day accounting conservatism has evolved, as follows. Given an expectation that overstatement errors reflect untrustworthy behavior, an individual in a personal exchange setting may lose access to markets if they develop a reputation for exploitation. By reducing overstatement errors, conservative reporting can help indicate that sender's trustworthiness, strengthening relationships with trading partners, building reputation capital, and facilitating ongoing exchange. That is, conservatism may have emerged as an *informal* bonding cost that arises endogenously through repeat interaction and that serves to minimize 'contracting' losses arising from information asymmetry (Watts and Zimmerman 1986; Ball et al. 2000).

We do not suggest this explanation for the emergence of conservatism to the exclusion of others, but as one of many elements of an evolutionary process. However, one distinguishing feature of ours relative to other fundamental explanations (e.g., Hirshleifer and Teoh 2009; Braun 2016; Basu and Waymire 2017) is that we abstract away from 'good news' or 'bad news' conditions such that reporting is not conditional on gains versus losses (Basu 1997). Indeed, someone seeking to minimize overstatement of net income and net assets would naturally record only highly verifiable gains while recording less verifiable losses. That is, by our construction gains and losses are treated *consistently* under conditional conservatism, as both are treated in the fashion necessary to minimize *ex post* overstatement errors. As such, we propose that a normative aversion to overstatement errors is a cause of conservatism and that the differential timeliness for gains versus losses is a consequence of this preference.

Sterling (1967, 110) describes conservatism as "the most ancient and probably the most pervasive principle of accounting valuation." Other scholars have suggested that conservatism may reflect an 'innate' or 'inborn' tendency (Hill and Gordon 1959, 170; Staubus 1996, 73). We provide evidence consistent with an underlying cause of conservatism that explains its historical roots and helps explain why many scholars have come to view the behavior as innate.

2. Background and Hypotheses

2.1 Overview

Chatfield (1977, 11) notes that the principle of conservatism was reflected in a Greek accounting system in the fifth century BC. Basu (2009, 12) cites fragmentary evidence of conservatism in 60 A.D. Further, it has been observed in emergent accounting systems from Germany (Harris et al. 1994; Ball and Shivakumar 2005) to Japan (Someya 1996). Scholars' most common explanations for conservatism include: it is a means to address moral hazard concerns of external (contracting) parties; it reduces shareholder litigation; it reduces the costs of taxation; and it reduces the political costs imposed on accounting regulators (Watts 2003). While these explanations require features of modern economies, historical evidence suggests that there must be a more fundamental answer.

In early civilizations, individuals faced a choice either to be self-sufficient or to specialize in producing a limited number of goods. Specialization is risky if an individual cannot find and retain trading partners, and relationships with trading partners can fall apart if there are barriers to the emergence of trust (Crockett et al. 2009). In an environment preceding legal protection of property rights – and preceding institutions which enable formal monitoring and bonding activities (Jensen and Meckling 1976) – trust yields implicit 'contracts' that are self-enforced by the parties through repeat interaction (Fehr et al. 1997; Kimbrough et al. 2008).

Social norms are shared, informal understandings about actions that are obligatory, permitted, or forbidden (Ostrom 2000). Smith (2008, 161-163) suggests that long histories of social interaction promote norms that support specialization and enhance wealth. For example, the Golden Rule, "do unto others as you would have others do unto you," encourages cooperation in small-scale social economies. We conjecture that, in a similar fashion, accounting conservatism emerged from a social norm to help build the trust and cooperation necessary to promote specialization and exchange in personal markets.[1]

2.2 Setting

We construct a multi-period game that pairs a Reporter and an Investor each period. The Reporter receives a noisy signal of his expected yield and prepares a report for the Investor. We inform both parties that the signal is equal to the expected value of the yield in each period, and that the yield is equally likely to be greater than or less than the signal; however, to create ambiguity we do not inform them of any other details of either distribution. While the Reporter prepares his report, the Investor decides whether she wants to exchange in that period or be self-sufficient. If the Investor opts out, there is no exchange and both parties receive a fixed autarky payment. If they opt in, the Investor receives the report and determines her level of investment. See Figure 1 for an overview of this timeline.

[1] Our focus is on personal exchange because, while organisms likely evolved to respond 'conservatively' to stimuli before humans engaged in personal exchange (e.g., fleeing from a strong negative signal, even an uncertain one, as a naturally-selected survival instinct), this does not directly speak to modern accounting, whose fundamental demand is to help guide exchange (Waymire 2009).

The Investor's (Reporter's) payoff is maximized when the investment equals (is greater than) the yield. As such, if the Investor naively trusts the report, then the Reporter has short-term incentives to report aggressively to encourage a high level of investment. Meanwhile, the Investor incurs a symmetric cost if she underinvests or overinvests relative to the yield.[2]

Each period, each Investor reviews every Reporter's history of reporting errors and ranks them in order of preference.[3] The six Investors are first placed in a random selection order; then, the Investor selecting first is paired with her preferred Reporter, the Investor selecting second is paired with her highest-ranking unpaired Reporter, and so on, until the Investor selecting sixth is paired with the remaining unpaired Reporter. Reporters selected first and second (fifth and sixth) receive an increase (decrease) to their payoff in that period. This provides Investors with a mechanism to use positive and negative sanctions (reward and punishment) on Reporters based upon their compliance with Investors' expectations.

2.3 Normative expectations

Our expectation is that the most successful reporting strategies will signal that the Reporter is trustworthy. Trust arises when there is interdependence between parties (Rousseau et al. 1998). In our setting, the Investor must rely on the Reporter's report to determine her investment, and this action directly affects the Reporter's payoff. Trust also arises when there is uncertainty regarding (i) the counterpart's intentions (Rousseau et al. 1998) and (ii) the outcome of the interaction (Heimer 2001). We create the latter by incorporating measurement uncertainty

[2] This structure also resembles modern economies, which are effectively a series of cooperative games between firms and their (i) customers, (ii) suppliers, (iii) debtholders, and (iv) equity holders, where the firm faces incentives to report optimistically, and where inaccurate reports impose costs on others.

[3] Starting with the second period. Reporter-Investor pairings in the first period will be determined at random.

via a noisy signal.[4] We create uncertainty about the Reporter's intentions by incorporating information asymmetry, as the Reporter's signal is private. Lastly, the incentive misalignment inherent in the payoff structures creates substantial uncertainty about the Reporter's intentions.

Rabin (1993) highlighted the role of intentions in shaping attitudes about fairness – that people judge others not only based on the consequences of their actions, but also according to their motives.[5] In our setting, to the extent aggressive reporting leads to aggressive investing (i.e., the investor is deceived), an aggressive report, in expectation, both effects a wealth transfer from Investor to Reporter and reduces social welfare. As such, to the extent Investors consider Reporters' motives, they may view aggressive reporting as an exploitation attempt intended to achieve a selfish outcome. Social norms generally apply to situations in which there is a conflict between selfish and pro-social incentives (Bicchieri 2006). We posit that Investors will perceive a social norm to apply in this situation: an informal rule against aggressive reporting.

2.4 Hypotheses

If Investors view aggressive reporting as violating a social norm, how can Reporters demonstrate their compliance? Because the Investor does not observe the Reporter's private signal, she cannot determine whether any report is aggressively biased (i.e. whether the report is greater than the signal). Rather, she only observes reporting errors once exchange concludes and the yield is revealed. Further, due to the noise in the signal, even a negatively biased report can produce an overstatement error – so, while overstatement errors may be the result of aggressive

[4] The Accounting Principles Board description of conservatism, provided on page one, emphasizes the role of measurement uncertainty. Other definitions are similar, e.g. Gilman (1939, 130): "…*in case of doubt* income should be excluded…while *in case of doubt* costs, expenses, or losses should be included." (emphasis added)

[5] Intention-based approaches have since reconciled previously unexplained empirical findings across a large range of economic games (e.g., see McCabe et al. 2003; Falk and Fischbacher 2006; Falk et al. 2008). Other studies have focused on subjects' preferences over outcomes (e.g., Levine 1998; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). In our setting, the Reporter's action does not directly affect the Investor's payoff, which is a function of the Investor's action and a state of nature. Therefore, any Investor preference over reporting outcomes must necessarily relate to their reading of the Reporter's intention rather than to the report itself.

reporting, the Investor cannot be certain this is the case. This ambiguity is a key feature of our setting, as Heiner (1983) notes that social norms arise *because of* uncertainty in distinguishing preferred from less-preferred behavior on each individual action.

We posit that Investors will use noisy reporting errors to gauge Reporters' compliance with the social norm. If so, Investors will view each incremental overstatement error – but not each incremental understatement error – as indicating a higher probability of noncompliance. In addition, Investors will view large overstatement errors as a likely failure to comply, leading them to prefer large understatement to large overstatement errors of equal magnitude. This leads to our hypotheses.

> Hypothesis 1a: *Ceteris paribus*, the decline in Investors' rankings is larger for an incremental overstatement error than understatement error.
>
> Hypothesis 1b**:** *Ceteris paribus*, the decline in Investors' rankings is larger for large overstatement errors than understatement errors of equal magnitude.
>
> Hypothesis 2a (2b)**:** Investors' asymmetric preferences regarding overstatement and understatement errors in H1a (H1b) are driven by Reporters' motivation to report aggressively.[6]

3. Experimental Design

3.1 Baseline condition: Hypotheses 1a and 1b

Each community consists of six Reporters and six Investors, who interact for ten periods, with new pairings each period. The report, investment, and yield are integers with range one to ten. The signal is an integer between three through eight, each with equal probability of

[6] Of the three key characteristics of our setting, we only measure the effects of the Reporters' motivation to report aggressively. We view the effects of the other two characteristics as self-evident. In the absence of measurement uncertainty, Investors have a perfectly accurate *ex post* verification mechanism of any bias in the report. In the absence of information asymmetry, the Investor can plan their level of investment based on the signal itself, so the report is unnecessary.

selection. The yield is equal to the signal plus an error term from the set {–2, –1, 0, 1, 2}, with the following odds: –2 (1/7), –1 (3/14), 0 (2/7), +1 (3/14), +2 (1/7). We choose this distribution because it provides significant measurement uncertainty while maintaining a sufficient degree of information in the signal (the expected value of the absolute value of the error equals one).

The payoffs for each party are shown below and presented in Panel A of Table 1.

$$\pi_I = \max(0, 20 - 5*|Investment - Yield|)$$
$$\pi_R = 18 - |Investment - Yield - 3|$$

These payoff functions are based on Crawford and Sobel (1982), which have been used as the basis for prior theoretical and experimental research (Dickhaut et al. 1995; Cain et al. 2005; Koch and Schmidt 2010; Qu 2013). The 'bias' term in the Reporter's payoff creates misaligned incentives, so the Investor's (Reporter's) payoff is maximized, in expectation, when the investment equals the signal (exceeds the signal by 3). Further, when the investment is equal to the signal, this structure provides for (i) an expected Investors' payoff equal to the expected Reporters' payoff and (ii) maximization of the total expected payoff. As such, an aggressive report that induces an aggressive investment will, in expectation, both transfer wealth from the Investor to the Reporter and reduce social welfare.

Each period, starting with the second period, Investors review Reporters' history of reporting errors and rank every Reporter in order of preference. We use these rankings to measure Investors' relative preference for reporting errors in order to test Hypotheses 1a and 1b. We also use these rankings to pair Investors to their preferred Reporters by placing Investors into a random selection order and pairing each with their highest ranked remaining Reporter. Based on their order of selection, Reporters receive a bonus adjustment to their payoff, as follows: #1 (+4), #2 (+3), #3 (0), #4 (0), #5 (–3), #6 (–4).

Each period, we notify each Reporter both of their overall selection position (that determines the adjustment to their payoff) as well as their average ranking in the period among all Investors. Each period, we notify the Investors of where they had ranked the Reporter they are paired with, and ask the Investors either to choose to exchange or to be self-sufficient in that period. If they choose the latter, both parties receive a payoff of five.

3.2 Control condition: Hypotheses 2a and 2b

To test Hypotheses 2a and 2b, we utilize a between-subjects design and manipulate the Reporter's incentive to overstate their report. Specifically, in addition to our *Misaligned* baseline condition we create an *Aligned* control condition in which both parties' payoff is maximized when the investment equals the yield. As such, *Misaligned* can demonstrate whether Reporters' incentive to overstate reports drives Investors' relative aversion to overstatement errors. The payoffs for the *Aligned* condition are included in Panel B of Table 1.

3.3 Experimental Procedures

The experiment was conducted at Chapman University's Economic Science Institute (ESI). A total of 192 participants were recruited from a participant pool consisting primarily of undergraduate students with each being randomly assigned to a single session. There were four sessions of each of the three conditions, each containing 24 participants, providing a sample of 48 Reporters and 48 Investors in each condition.[7] The authors' institutions obtained Internal Review Board (IRB) approval for this experiment, which requires us to not use deception. In all treatments the experiment lasted ten periods and participants were aware of this. We used the same set of stochastically-generated signals and yields for each treatment so that variation in outcomes is due to variation in behavior.

[7] Experiments were performed with two communities in a room at the same time (24 participants; 12 Reporters and 12 Investors).

Each session lasted approximately one hour and was sequenced as follows. Participants were seated at visually isolated workstations and interacted with each other anonymously over a local computer network. Each participant first read the instructions on their computer. The instructions explain the experimental procedures and payoffs; see Appendix 1 for the instructions for the baseline condition. Participants then answered several quiz questions on their computer to ensure that they understood the instructions; see Appendix 2 for the quiz for the baseline condition. The experimenter privately answered any questions regarding the experimental procedures. Each participant was assigned a role, labeled "Receiver" for the Investor and "Sender" for the Reporter, and remained in that role for the entire experiment.

Each participant was paid a $7 participation fee in addition to payoffs from one randomly selected round, an incentivized belief elicitation task on reporter trustworthiness (see section 4.5), and a risk assessment measure (see Appendix 3). Lastly, while payments were processed participants were asked demographic information (e.g., age, gender) and filled out the Short Dark Triad (SD3) personality trait survey (Jones and Paulhus 2014). On average subjects earned $19.50 in addition to their participation fee.

4. RESULTS

4.1 Summary statistics

Summary statistics for the overall game results are included in Table 2. They demonstrate that under misaligned incentives, on average Reporters aggressively bias their reports (0.47 in Panel A) and Investors discount most of this bias in their investment (see *Investing Bias* and *Investing Efficiency*), although there is significant cross-sectional variation among participants. Reporters out-earn Investors in both conditions, and both parties' payoff is maximized when their incentives are aligned (Panel B). We find no significant correlation of the quiz, gender, risk attitudes, or the SD3 personality trait on Reporter or Investor behavior.

Summary statistics for the periodic results used for hypothesis testing are included in Panels A and B of Table 3. Panel A confirms that under misaligned incentives Reporters produce more overstatement errors (49%) than understatement errors (30%). Panel B demonstrates that errors are smaller in both magnitude and variance (see |*Error*|**Neg* and |*Error*|**Pos*) when incentives are aligned.

4.2 Hypotheses 1a and 1b

4.2.1 Testing hypotheses 1a and 1b

We test hypotheses 1a and 1b by estimating equation 1 for the Baseline condition:

$$Rank = \alpha_0 + \alpha_1 Neg + \alpha_2 Pos + \beta_1 |Error| * Neg + \beta_2 |Error| * Pos + \sum \beta_k Controls \qquad (1)$$

Rank is the ranking position of a Reporter by an Investor; it is inverted from the original rankings (one through six where one is the most preferred) so that higher numbers reflect a better ranking (six through one where six is the most preferred). *Neg* (*Pos*) is a binary variable that identifies whether the current period report produced an understatement (overstatement) error – the excluded term is for an error of zero – while |*Error*| is the absolute value of the current period's reporting error (i.e., the report minus the yield). We utilize four control variables. *Consistency* is the variance in the Reporter's reporting errors multiplied by negative-one, *Accuracy* is the Reporter's average absolute reporting error multiplied by negative-one, while *OthersConsistency* and *OthersAccuracy* are the average *Consistency* and *Accuracy* of the other five Reporters.[8] We estimate equation 1 in a random-effects ordered logistic regression with standard errors clustered by Investor.

[8] We multiply the variance and average absolute errors by negative-one so that larger numbers reflect more consistent and accurate reports, respectively.

The fixed effect of an understatement error (overstatement error) on rankings is measured by α_1 (α_2), while the marginal effect of an incremental understatement error (overstatement error) on rankings is measured by β_1 (β_2). If the decline in Investors' rankings is larger for an incremental overstatement error than an incremental understatement error (H1a), then the difference $\beta_1 – \beta_2$ will be positive. If the decline in rankings is larger for large overstatement errors than understatement errors of equal magnitude (H1b), then the difference in fitted values $[(\alpha_1 + \beta_1 * |Error|) – (\alpha_2 + \beta_2 * |Error|)]$ will be positive for errors of large magnitude.

4.2.2 Results for hypotheses 1a and 1b

Results for the estimation of equation 1 are in Panel A of Table 4. The first column presents results for all periods without control variables. Column (2) estimates the full equation and provides data for the current period's reporting error, while columns (4) and (5) each add one additional lagged error. Column (3) estimates the full equation and includes fixed effects for each Reporter-Investor pair to control for possible instances where an Investor 'locks-in' to a Reporter for reasons other than their reporting errors. As expected, *Consistency* and *Accuracy* (*OthersConsistency* and *OthersAccuracy*) are positive (negative), indicating that Investors prefer Reporters who are more consistent and more accurate. (While *Accuracy* is no longer significant in column (5), likely because the additional lagged |*Error*| terms absorb the effect of prior reporting accuracy, *OthersAccuracy* remains significant).

Panel B provides the estimates for $\beta_1 – \beta_2$, our test of H1a. The coefficient of interest is positive and significant for the current period's reporting error in all five columns, while the coefficient on the first lag is significant in column (5). The coefficient on the second lagged error is not significant. However, the results from Panel A indicate that the coefficient on incremental overstatement errors is significantly negative (at 1%) for all lags. We interpret these results as broadly consistent with hypothesis 1a.

Figure 2 illustrates the effect of each reporting error on rankings and helps to provide intuition for the regression results in column (2) of Table 4. While there is a fixed 'punishment' for understatement errors (coefficient on *Neg* is significantly negative), the slope on understatement errors appears to be flat (coefficient on |*Error*|**Neg* is insignificant). On the other hand, there is a large downward slope on overstatement errors (coefficient on |*Error*|**Pos* is negative and highly significant), and the best-fit line appears to provide a slightly positive intercept (coefficient on *Pos* is positive, though insignificantly so).

Panel C of Table 4 provides the estimates for the difference in fitted values of over- and understatement errors of equal magnitude, our test of H1b. The coefficient of interest is positive and significant for errors of magnitude ±3 or greater. Interestingly, this magnitude lines up perfectly with the error in the signal, perhaps indicating that Investors were able to successfully intuit when Reporters attempted to exploit them via aggressive reporting. This evidence provides clear support in favor of hypothesis 1b.

4.3 Hypotheses 2a and 2b

4.3.1 Testing hypotheses 2a and 2b

We test hypotheses 2a and 2b by estimating equation 2 with data from both the Baseline *Misaligned* condition as well as the control *Aligned* condition.

$$\begin{aligned} Rank = {} & \alpha_0 + \alpha_1 Neg + \alpha_2 Pos + \alpha_3 Misaligned + \alpha_4 Neg{*}Misaligned + \\ & \alpha_5 Pos{*}Misaligned + \beta_1 |Error|{*}Neg + \beta_2 |Error|{*}Pos + \\ & \beta_3 |Error|{*}Neg{*}Misaligned + \beta_4 |Error|{*}Pos{*}Misaligned + \\ & \textstyle\sum \beta_k Controls + \sum \beta_k Controls{*}Misaligned \end{aligned} \tag{2}$$

Rank, *Neg*, *Pos*, and |*Error*|, and control variables *Consistency*, *Accuracy*, *OthersConsistency*, and *OthersAccuracy* are as defined in equation 1. *Misaligned* is a binary variable that identifies pairings in the *Misaligned* condition. As in equation 1, the excluded term is for an error of zero, so the effects of positive and negative errors are measured relative to zero error. We estimate equation 2 in a random-effects ordered logistic regression with standard errors clustered by Investor.

The fixed effect of an understatement error (overstatement error) on rankings in the *Aligned* treatment is measured by α_1 (α_2), while the marginal effect of an incremental understatement error (overstatement error) on rankings is measured by β_1 (β_2). The fixed effect of an understatement error (overstatement error) on rankings in the *Misaligned* treatment is measured by $\alpha_1+\alpha_3+\alpha_4$ ($\alpha_2+\alpha_3+\alpha_5$), while the marginal effect of an incremental understatement error (overstatement error) on rankings is measured by $\beta_1+\beta_3$ ($\beta_2+\beta_4$).

If our prediction from H1a is driven by Reporters' motivation to report aggressively (H2a), then the difference-in-differences in slopes (β_3–β_4) will be positive; this prediction is illustrated in Panel A of Figure 3.[9] If our prediction from H1b is driven by Reporters' motivation to report aggressively (H2b), then the difference-in-differences in fitted values $[(\alpha_4 + \beta_3 * |Error|) - (\alpha_5 + \beta_4 * |Error|)]$ will be positive for errors of large magnitude.[10]

4.3.2 Results for hypotheses 2a and 2b

Results for the estimation of equation 2 are in Panel A of Table 5. The first column presents results for all periods without control variables. Column (2) estimates the full equation and provides data for the current period's reporting error, while columns (4) and (5) each add

[9] We predict the effect of an incremental over- and understatement error in the *Aligned* condition (and an understatement in the *Misaligned* condition) to be zero as we are measuring its effect on rankings that's not already captured by any change in the consistency and accuracy of the reporting history. That is, we predict no effect of these incremental errors on rankings other than through the consistency and accuracy channels.

[10] The difference in fitted values for under- versus overstatement errors in the *Misaligned* versus *Aligned* conditions: $[(\alpha_1+\alpha_3+\alpha_4+\beta_1*|Error|+\beta_3*|Error|) - (\alpha_2+\alpha_3+\alpha_5+\beta_2*|Error|+\beta_4*|Error|)] - [(\alpha_1+ \beta_1*|Error|) - (\alpha_2+ \beta_2*|Error|)] = [(\alpha_4 + \beta_3*|Error|) - (\alpha_5 + \beta_4*|Error|)]$

one additional lagged error. Column (3) estimates the full equation and includes fixed effects for each Reporter-Investor pair. Similar to Table 4, *Consistency* and *Accuracy* (*OthersConsistency* and *OthersAccuracy*) are positive (negative) in the *Aligned* condition, although there appear to be some differences between conditions (as some coefficients on the *Misaligned* interactions are significant).

Panel B provides the estimates for β_3–β_4, identifying whether the difference-in-differences in slopes is significant, our test of H2a. The coefficient of interest is positive and significant for the current period's reporting error in all five columns, while the coefficient on the first lag is significant in column (4). The coefficient on the second lagged error is not significant. The estimates in column (2) are used to illustrate our results in Panel B of Figure 3, which visually demonstrates that the results conform to our directional predictions. We interpret these results as broadly consistent with hypothesis 2a.

Panel C of Table 5 provides the estimates for the difference-in-differences in fitted values, our test of H2b. In Panel C of Table 4 we found that the decline in Investors' rankings is larger for overstatement than understatement errors of magnitude ±3 or greater; here, we confirm that these differences are driven by Reporters' motivation to report aggressively, as the coefficients are slightly larger in magnitude and are all statistically significant. In total, these results provide clear support for hypothesis 2b.

4.4 Economic significance of hypothesis 1a results

Our results in Table 4 take shape in column (2) once we add controls for the consistency and accuracy of the reports, indicating the possibility that over- and understatement errors have a second-order effect in this setting. To interpret the economic significance of our hypothesis 1a results, we measure the magnitude of the effect of a one-standard deviation change in

Consistency and *Accuracy*, as well as of an incremental overstatement error one-standard deviation in magnitude. Results from column (2) indicate that a one-standard deviation decline in *Consistency* (*Accuracy*) decreases a Reporter's ranking by 0.6 (0.7) of a position, while the decline from an incremental overstatement error one standard deviation in magnitude is 0.4 of a position.[11] However, this measures the effect of a *pattern* of consistent or accurate reporting against a *single* overstated or understated report. To address this, we analyze results in column (5), where the current and two lagged |*Error*|**Pos* terms are all significant. These data indicate that, *ceteris paribus*, reporting three consecutive incremental overstatement errors one standard deviation in magnitude decreases the current period's ranking by 1.1 positions.[12]

4.5 Supplemental analysis: Identifying the components of trust

After the last period – when Investors typically rank Reporters for the next period's pairings – we ask Investors to identify each Reporter as one they either "trust" or "don't trust." We then ask the Investors to select which Reporters *other* Investors said they trust, and we provide a payment for each correct selection. At the same time, we ask each Reporter to identify which Reporters they trust, and then ask them to select which Reporters the *Investors* said they trust; we again provide a payment for each correct selection. This allows us to link various reporting characteristics to Investors' beliefs about Reporters' trustworthiness – and to identify whether Investors' expectations align with Reporters' beliefs about them.

Results from the *Misaligned* condition are included in Table 6. The dependent variable *Trust* equals one when the participants indicate "trust" in a Reporter and zero when they indicate "don't trust." Column (1) in both Panels provides the participants' reports on who they trust,

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[11] This is calculated as [coefficient] * [standard deviation of the variable]:
Consistency: 0.95*0.66 = 0.63; *Accuracy*: 0.94*0.79=0.74; |*Error*|**Pos*: 0.34*1.24=0.42.

[12] This is calculated as [coefficient] * [standard deviation of the variable]:
|*Error*|**Pos* + |*Lag1Error*|**Pos* + |*Lag2Error*|**Pos*: (0.42*1.24) + (0.23*1.24) + (0.21*1.24) = 1.07.

while column (2) provides their report on who they believe the Investors said they trust. Each participant provided a response for all six Reporters in their community. Control variables *Consistency* and *Accuracy* return from our prior analyses. *Understated* (*Overstated*) equals one when the Reporter's aggregate reporting error is negative (ten or greater). As each represents 12 out of the total of 48 Reporters, they measure the bottom and top quartiles of aggregate errors.

Results from Panel A of Table 6 indicate that Investors' reports on trust are broadly consistent with their period-by-period preference rankings, as Investors reported significantly higher trust in the Reporters who were consistent and who produced understatement errors. However, while Investors' reports indicate they are 109% more likely to trust a Reporter whose average reporting error is negative, from column (2) of Panel B we see that Reporters do not believe that is the case, as they perceive that Investors are 56% *less likely* to trust a Reporter whose average reporting error is negative. In fact, the differences between Panels A and B indicate a significant mismatch in the parties' beliefs about which actions build trust. This is particularly striking on the issue of consistency: while Investors indicate it is the most important component of trust, Reporters perceive that Investors see it as a relatively insignificant factor.

5. Discussion and Conclusion

5.1 Discussion

We offer some directions for future research. First, future research might identify whether, once conservatism is "codified" as an accounting institution, it supports *impersonal* exchange by promoting reputation formation and facilitating reciprocity (Basu and Waymire 2006, 220). Additionally, in our setting, the receiver's payoff is not increasing in the magnitude of the sender's yield. As such, a signal greater than (lesser than) the historical average does not reflect ‘good’ (‘bad’) news, so we cannot identify whether reports incorporate bad news in the signal more completely than good news (Basu 1997). Future research can extend our findings to a setting where this can be tested. Lastly, we have noted that senders in our setting did not appear to understand receivers’ expectations regarding their reports. Future research might explore alternate conditions under which senders hold accurate beliefs about receivers’ expectations and identify whether a behavioral norm emerges.

5.2 Conclusion

We demonstrate that in the presence of three conditions – information asymmetry, measurement uncertainty, and senders motivated to report aggressively – receivers of reports have a general preference for understatement errors versus overstatement errors. The first two conditions create a verification problem that heightens attention on the *ex post* reporting error. The third condition makes the receiver wary of the senders’ intentions, as reporting errors of greater value indicate a greater likelihood of exploitative intent – and, in fact, we demonstrate that receivers’ aversion to overstatement errors is driven by senders’ motivation to report aggressively. To the best of our knowledge, ours is the first study to document these findings. We briefly discuss two potential implications.

Our study may be useful to standard-setters by identifying those conditions where users are likely to value conservatism – and by identifying conditions where users are less likely to demand conservatism, like when measurement uncertainty is low (e.g., Level 1 assets under U.S. GAAP). More broadly, one key takeaway from our study is that users incorporate senders’ motivations in their preferences over reports. Evolved social norms often incorporate more information than rules-based standards (Sunder 2005), which may be derived from normative models of what standard-setters believe financial statement users should want (Young 2006). To the extent standards are set based on models that exclude considerations of preparers' motives, they are likely to contain some degree of constructivist error (Smith 2008).

Finally, following the seminal Basu (1997) study, a large stream of research has focused on the differential accounting treatment of gains and losses, which compare a reported value to some reference point at the time a report is issued. We encourage an increased focus on reporting errors, which are determined after realized outcomes are observed. Indeed, that *ex post* errors are vital in driving behavior is consistent with the insight that economic action develops through adaptive human response to the success or failure of realized exchange outcomes (Alchian 1950). We propose that that a relative aversion to overstatement errors originated from a social norm promoting trust and cooperation, and that the reinforcement of this fundamental preference, through an adaptive trial-and-error process, produced the array of conservative accounting procedures that we observe in the modern era. That is, we propose the possibility that conservatism arose not from an asymmetry between *ex ante* gains and losses, but from an asymmetry between *ex post* overstatement errors and understatement errors.

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APPENDIX 1. Instructions for participants in baseline condition (*Misaligned*)

INSTRUCTIONS

This is an experiment in the economics of decision-making. Various research agencies have provided funds for this research. The currency used in the experiment is U.S. dollars, expressed with a '$'. At the end of the experiment your earnings will be paid to you in private and in cash. It is very important that you remain silent and do not look at others' monitors. If you have any questions, or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave and you will not be paid. We expect, and appreciate, that you adhere to these policies.

Today's Experiment

During each period of today's experiment, you will be paired with another participant in the room. One of you will be a Sender and the other will be a Receiver. There is an equal chance that you will be a Sender or a Receiver, and you will stay in the same role for the entire experiment.

There will be two communities in the room today. The participants in the first two rows will be a community of 6 Senders and 6 Receivers (the Front community), and the participants in the back two rows will be another community of 6 Senders and 6 Receivers (the Rear community). You will never interact with any member outside of your community. At the conclusion of the experiment, there will be an announcement indicating which community collected the most cash earnings.

Within your community, there will be new Sender-Receiver pairings during each of the ten periods. Each period, the Sender and Receiver are paired together for a collaborative partnership opportunity.

What Is the Opportunity?

Each period, each Sender-Receiver pair receives a number. However, neither the Sender nor the Receiver know the number beforehand. The number is revealed to the pair at the end of each period. The number is an integer from 1 to 10 (i.e., 1, 2, 3, 4, 5, 6, 7, 8, 9, 10). The number in one period does not affect the number in other periods.

Once paired with a Receiver, the Sender gets a private clue about the number. The clue is in the form of a single number. The Receiver does not receive the Sender's clue, and will never see the clue.

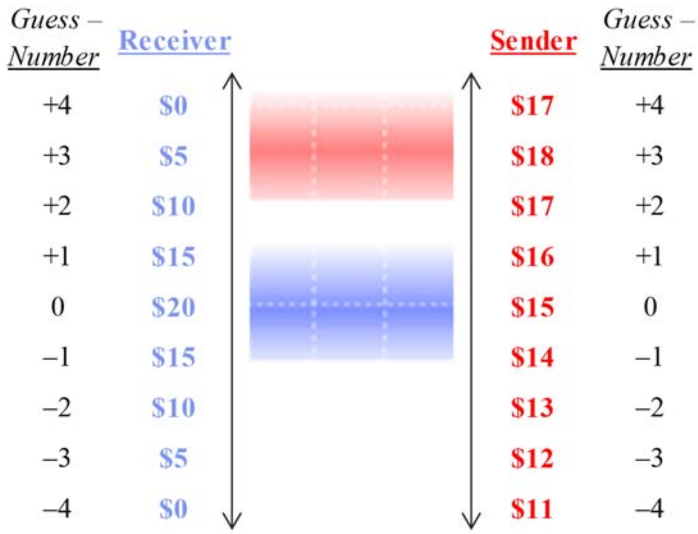
On average, the clue equals the number. The clue can be different from the number, and, when it is different, there is an equal chance it is less than or more than the clue. After observing the clue, the Sender submits a report for the Receiver about the number expected that period. The report is a single number from 1 to 10. The report does not affect what the number is.

After the Receiver sees the report, they make a guess about what the number will be that period. The guess is a single number from 1 to 10. The guess does not affect what the number is.

Last, both the Sender and Receiver are informed of the guess and the number for the period, as well as their respective earnings. The earnings for all differences between the guess and number are shown in the table below. Notice the earnings do *not* depend upon either the Sender's report or their private clue.

	← *Guess is less than the number*										*Guess is more than the number* →								
Guess – Number	*−9*	*−8*	*−7*	*−6*	*−5*	*−4*	*−3*	*−2*	*−1*	*0*	*+1*	*+2*	*+3*	*+4*	*+5*	*+6*	*+7*	*+8*	*+9*
Receiver	$0	$0	$0	$0	$0	$0	$5	$10	$15	$20	$15	$10	$5	$0	$0	$0	$0	$0	$0
Sender	$6	$7	$8	$9	$10	$11	$12	$13	$14	$15	$16	$17	$18	$17	$16	$15	$14	$13	$12
Total	$6	$7	$8	$9	$10	$11	$17	$23	$29	$35	$31	$27	$23	$17	$16	$15	$14	$13	$12

The picture on the next page illustrates these earnings for certain outcomes.

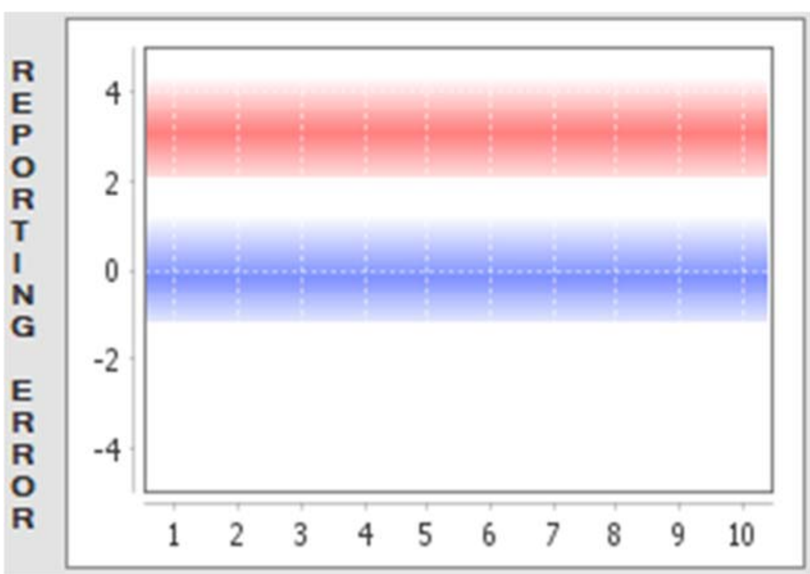


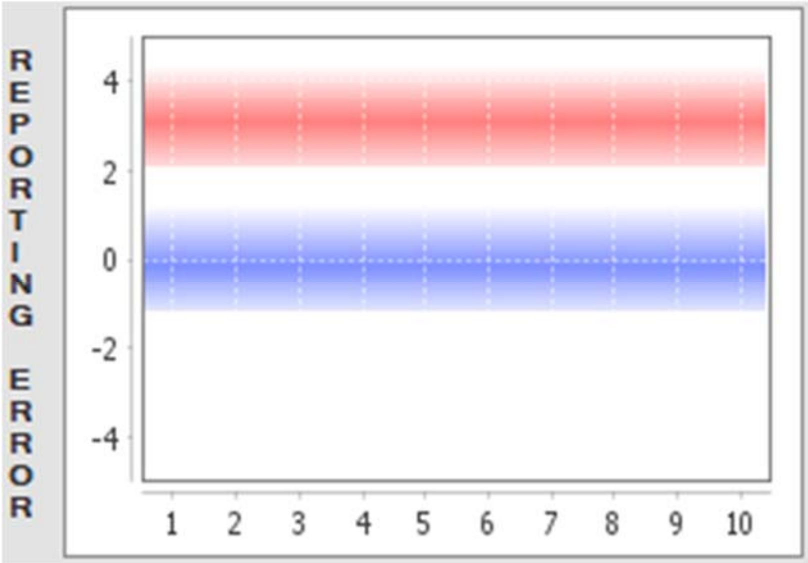
So, for example, if the Receiver's guess was 3 more than the number, the Receiver earns $5 and the Sender earns $18. If the guess was exactly equal to the number, the Receiver earns $20 and the Sender earns $15. If the guess was 3 less than the number, the Receiver earns $5 and the Sender earns $12.

The Receiver's Decision to Collaborate

In the first period, each Sender will be randomly paired with a Receiver and they will collaborate in the earnings opportunity (i.e., the Sender gets a clue and sends a report, and then the Receiver sees the report and makes a guess).

Starting in the 2nd period, the Receiver decides whether to collaborate with the Sender, and this decision determines whether the partnership opportunity takes place. Everyone in the community sees six charts, illustrating each Sender's prior reporting errors – that is, the difference between their report and the number for all past periods. For each period, a black square will show if the Sender's report was bigger than the number, was less than the number, or was equal to the number.





So, for example, if the Sender's report is 2 more than the number, a black square will show at 2 on the vertical axis. If the report is 2 less than the number, a black square will show at –2. This will be the case for reporting errors between 4 and –4.

While reporting errors can possibly go from 9 to –9, in order to fit all six charts on the screen we restrict the vertical area of the chart to show the range from 4 to –4. If the Sender's report is 5 or 6 more than the number, a red triangle pointing upwards will show at "4". If the Sender's report is 5 or 6 less than the number, a blue triangle pointing downwards will show at "–4". If the Sender's report is 7, 8, or 9 more than the number, a larger red triangle pointing upwards will show at "4". If the Sender's report is 7, 8, or 9 less than the number, a larger blue triangle pointing downwards will show at "–4".

The Receiver sees reporting error charts for all 6 Senders, and the chart of the Sender they are paired with in the current period will be highlighted. If the Receiver decides not to collaborate with their paired Sender that period, they will not interact, and both the Receiver and the Sender earn $5 for the period.

While the Receiver is deciding whether to collaborate, the Sender makes their report. Even if the Receiver decides not to collaborate, the Sender's reporting error for that period will be included in the next period's chart.

Sender's Bonus and Pairing

The earnings for the Sender in each period are adjusted by a bonus amount. In the first period the Senders are selected in a random order, and their bonus is based on the position they are randomly selected. If they are selected in the first two positions their earnings will increase, and if they are selected in the last two positions their earnings will decrease, as shown in the table below.

Sender selection	Earnings Adjustment
#1	+$4
#2	+$3
#3	$0
#4	$0
#5	–$3
#6	–$4

Starting with the beginning of the second period, every Receiver ranks every Sender in order of preference, starting with the Sender they most prefer to collaborate with at 1 and the Sender they least prefer to collaborate with at 6. These rankings are used to pair each Receiver with a Sender and to determine the Sender's bonus amount, as follows.

Each period, every Receiver is randomly assigned a selection slot from #1 to #6. The Receiver selecting #1 is paired with their most preferred Sender; the Receiver selecting #2 is paired with their most preferred Sender still available; the Receiver selecting #3 is paired with their most preferred Sender still available; and so forth, until all pairings are determined for the period.

The Sender's bonus is based upon how early they are selected. If they are selected in the first two positions their earnings will increase, and if they are selected in the last two positions their earnings will decrease, as shown in the table above. Through this process, the Sender's bonus depends on where they are ranked in the Receivers' order of preference.

Once the pairings are determined, each party is informed of the outcome. The Receiver is notified which Sender they are paired with (e.g. "You are paired with your 3rd choice"). The Sender is notified of which position they were selected and of their average ranking across all Receivers in that period. For example, if half of the Receivers ranked a particular Sender 3rd and the other half ranked the Sender 4th, then the Sender's average ranking across all Receivers would be 3.5.

The period then continues as described above, as the Receiver decides whether to collaborate and the Sender receives their private clue and submits their report.

Summary of Each Period

The first period:

- Senders and Receivers are randomly paired
- Everyone is informed of the pairings for that period
- Senders see their private clue and submit their report
- Receivers see the report and make their guess

Every period after the first period:

- Receivers see charts of past periods' reporting errors of all Senders
- Receivers ranks each Sender in order of preference
- Senders will be paired based on Receivers' preferences
- Everyone is informed of the pairings for that period
- Senders are informed of their average rank among all Receivers

Every period after the first period, once the pairings are announced:

- Receivers decide whether to collaborate with their paired Sender
- Senders see their private clue and submit their report

If the Receiver chooses to not collaborate in the period:

- The Receiver and the Sender both earn $5 for the period
- The Sender's total earnings include a bonus

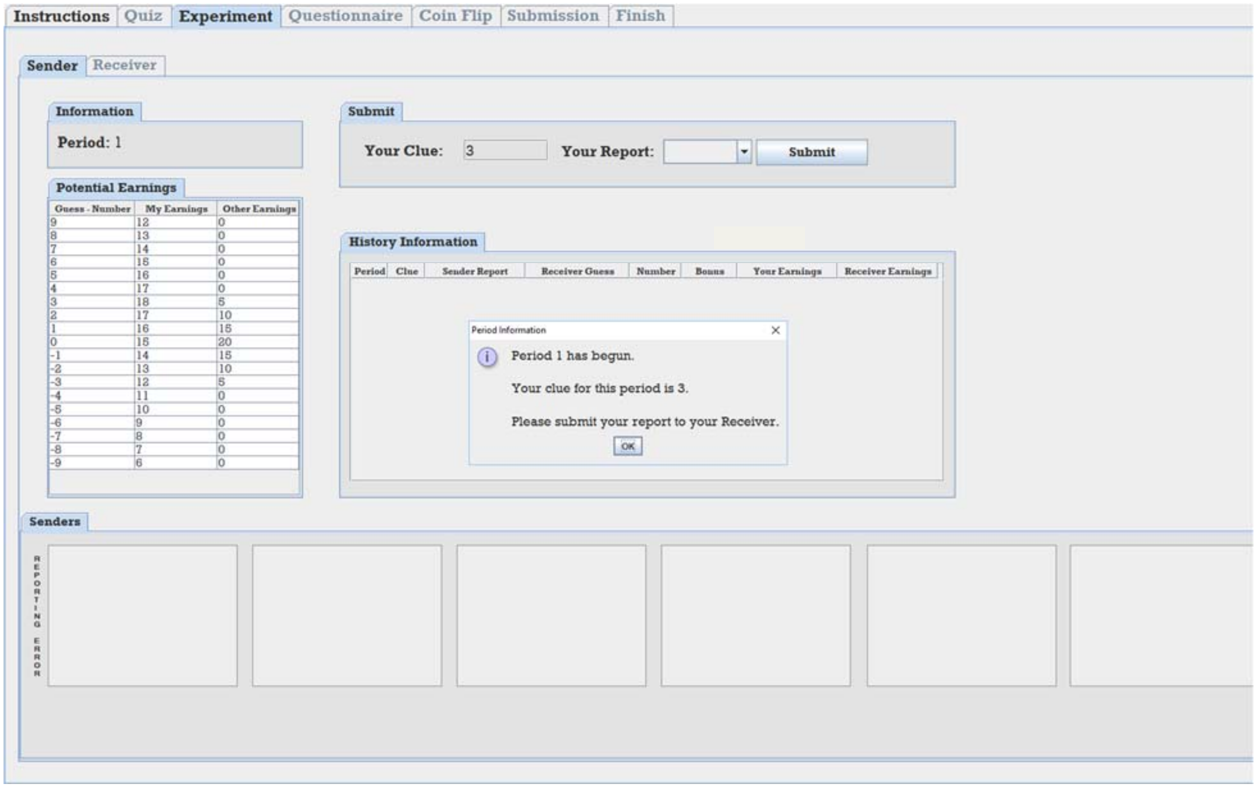
If the Receiver chooses to collaborate in the period:

- The Receiver sees the report and makes their guess
- Both the Receiver and the Sender are informed of the guess, number, and of their earnings
- The Sender's total earnings include a bonus

Now let's review what each period will look like for Receiver and Sender.

Sender: Reporting Decision

If you are a Sender, you will see the following on your computer monitor each period:

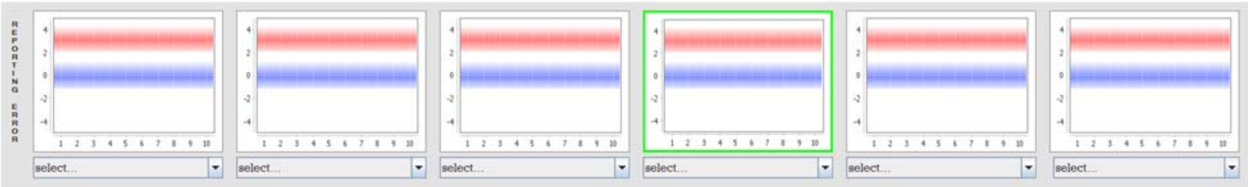


Your private clue about the number that period is shown in the 'Submit' window as "Your Clue."

As a Sender, you must submit a report for the Receiver after seeing your private clue. You can report any value from 1 to 10. Your report does not affect the number. Once you decide what to report, press the "Submit" button.

Receiver: Ranking Senders in Order of Preference

If you are a Receiver, you will see all Senders' prior reporting errors in a chart like this on your computer monitor each period, starting with the second period:



A black square will indicate whether, for each period so far, that Sender's report overstated or understated the number. A square above the middle line means they reported larger than the number, and a square below the middle line means they reported less than the number. A square on the middle line means they reported the exact number.

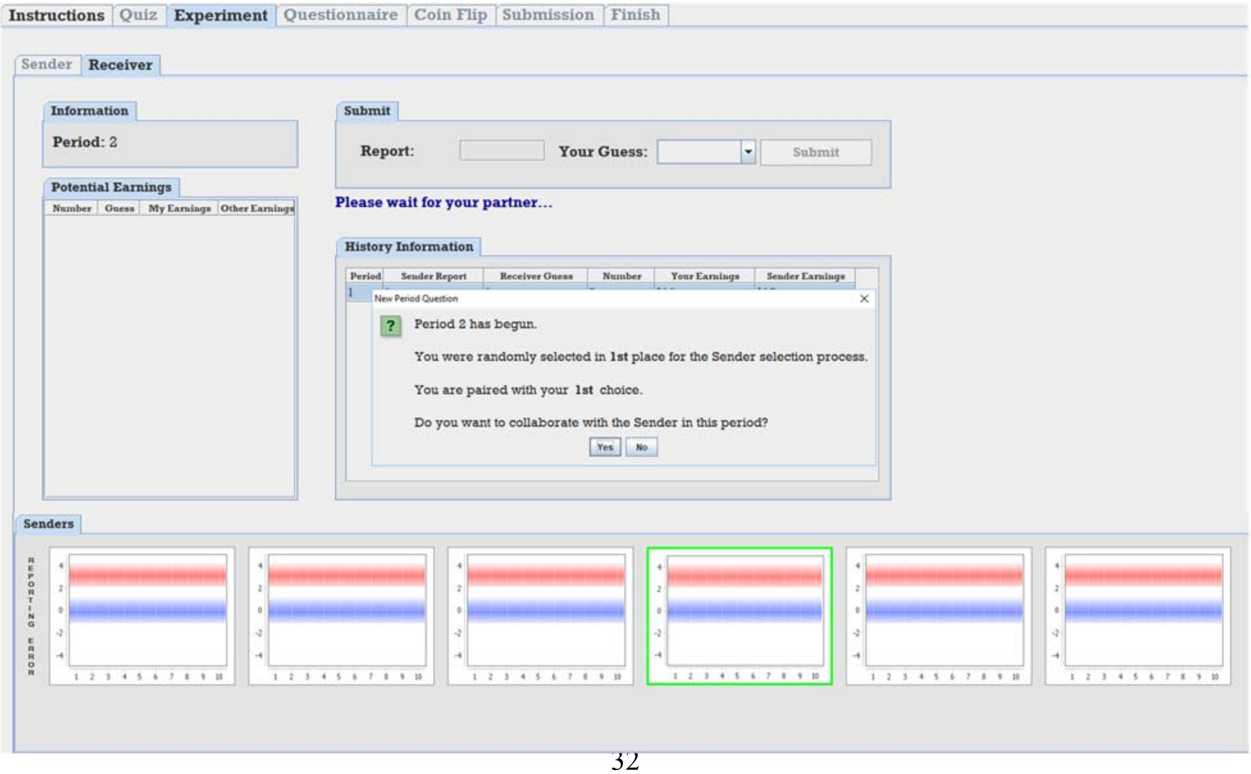
The black squares that indicate reporting errors are omitted in the above image, but they will appear on your screen during the experiment.

If you are a Receiver, you must rank each Sender in order of preference, starting with the Sender you most want to collaborate with at 1 and ending with the Sender you least want to collaborate with at 6.

Receiver: Decision to Collaborate with Sender

In the first period, you will be randomly paired with a Sender and do not have an option to opt out of collaboration.

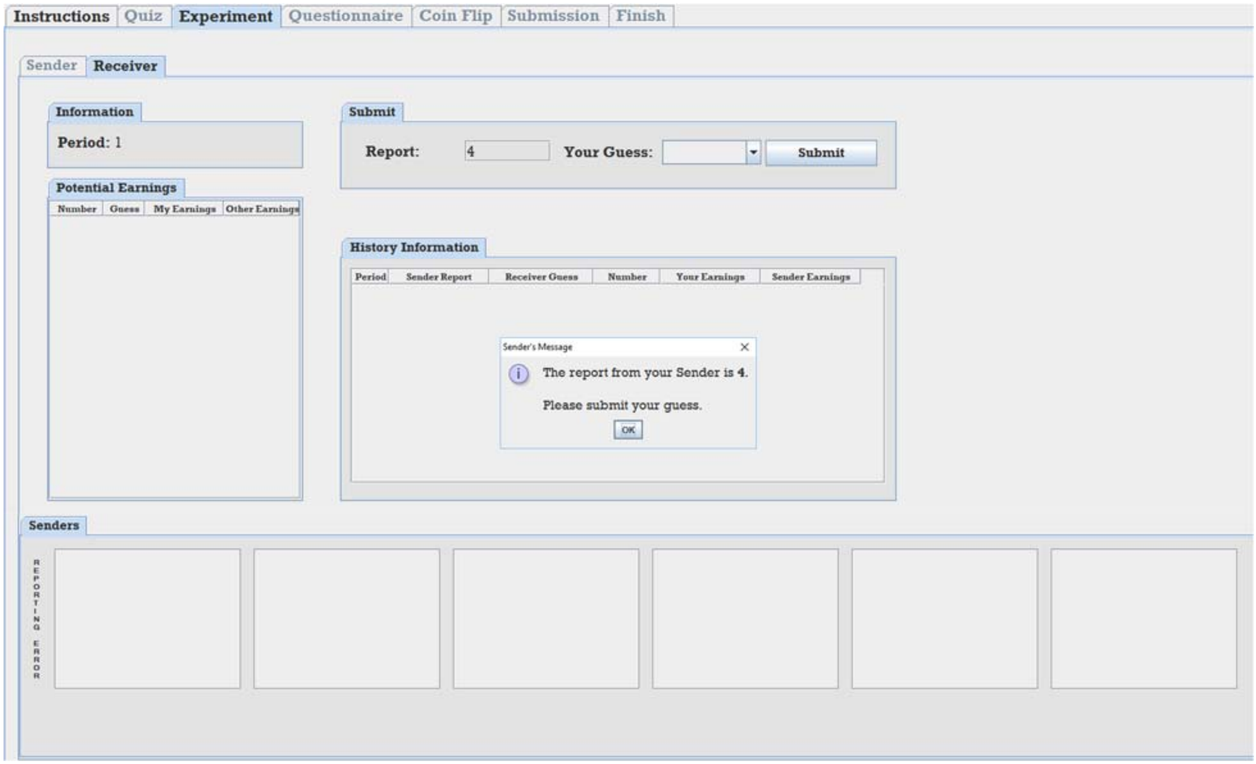
Starting with the second period, once the pairings are announced, each Receiver decides whether to collaborate with their paired Sender. If the Receiver chooses not to collaborate, both the Receiver and Sender earn $5 instead of having the opportunity to earn the amounts listed on the first page. If you are the Receiver, you will see the following on your computer monitor each period:



The black squares that indicate reporting errors are omitted in the screenshot above, but they will appear on your screen during the experiment.

Receiver: Make a Guess

If you are the Receiver and you have chosen to collaborate (or if it is the first period), you will see the following on your computer monitor:



The report from the Sender will be stated in the Submit window as "Report." After the first period, you will also be provided with details about the Sender's prior reporting errors. The chart for the Sender you are paired with will be highlighted. If it is the first period, like in the picture above, the will be no charts.

If you are the Receiver, you must make your guess of what the number will be that period. Your guess must be from 1 to 10. Your guess will not affect what the number is.

Once you enter a guess, the 'Potential Earnings' table will update. The table reports what your earnings and the Sender's earnings will be for each possible number, based upon your current guess. You can change your guess to update the table. Once you decide what number to guess, press the "Submit" button.

Results

If the Receiver chooses not to collaborate in the period, both the Receiver and the Sender are informed of the decision and that their earnings for the period are $5 (before the Sender's bonus adjustment).

If the Receiver chooses to collaborate in the period, both the Receiver and the Sender are informed of both the Receiver's guess and the number for the period, and of their respective earnings for the period.

After reviewing the results, please press the "Continue" button. The period's results will be added to a table displayed on your monitor with all the prior periods' results.

Whether the Receiver chooses to collaborate in the period or chooses not to collaborate, the information on the Sender's report and the number for the period will be incorporated into the Sender's reporting error chart in the next period.

Conclusion

At the end of 10 periods, the experiment is over. You will be asked to complete a short survey and wait until your name is called. When it is, bring your belongings and walk to the cashier's window in the front of the laboratory. You will be paid in cash for the earnings from one of the ten periods, chosen at random.

Sequence of Today's Experiment

1. Quiz
2. 10 periods of Sender-Receiver collaborative opportunities
3. Demographic information
4. Survey while payment is being processed

APPENDIX 2. Quiz questions for participants in baseline condition (*Misaligned*)

You will be a Receiver or Sender and will remain in that role for the entire experiment.

- True
- False

If you are a Receiver, you will definitely be partnered with the same Sender during the entire experiment. If you a Sender, you will definitely be partnered with the same Receiver during the entire experiment.

- True
- False

In each period that the Receiver decides to not collaborate, then, before the bonus adjustment:

- The Receiver and Sender earn $0 apiece
- The Receiver and Sender earn $5 apiece

Every period the Sender gets a private clue and sends a report to the Receiver. The report:

- Must be equal to the clue
- Can be any number between 1 to 10

In each period that the Receiver decides to collaborate, their guess:

- Must be equal to the Sender's report
- Can be any number between 1 to 10

The Sender's earnings (based on the Receiver's guess and the actual number) are adjusted by a bonus:

- True
- False

If a Receiver's guess is 2 more than the actual number, the earnings for the period (ignoring the bonus adjustment) are:[^]

- $10 for the Receiver and $17 for the Sender
- $10 for the Receiver and $13 for the Sender

If the Receiver's guess is 2 less than the actual number, the total for the period (ignoring the bonus adjustment) are:[^]

- $10 for the Receiver and $17 for the Sender
- $10 for the Receiver and $13 for the Sender

The Receiver's earnings are largest when:

- Their guess is 3 larger than the actual number
- Their guess is equal to the number

[^] We modified the Sender payoffs for the quiz provided to subjects in the *Aligned* control condition.

The Sender's earnings (ignoring bonus adjustment) are largest when the Receiver’s guess:

- Is 3 larger than the actual number
- Is equal to the actual number

You will be paid for the earnings from one period, chosen at random.

- True
- False

If the Sender's report is 7, the Receiver's guess is 6, and the number is 5, the Sender's reporting error chart in that period will show +1.

- True
- False

The Receivers' ranking of preferred Senders affects the Senders' bonus adjustment.

- True
- False

APPENDIX 3. Risk assessment measure for all participants

Participants in all treatments viewed on their monitor the following risk attitude measure adapted from Dave et al. (2010).

We want you to select from among six different choices the one choice you would like to be paid for. The six different choices are listed below.

Each choice has two possible payoffs (Heads or Tails) with the indicated probabilities of occurring. Your payoff for this part of the study will be determined by:

- Which of the six choices you select, and
- Which of the two possible payoffs occur, determined by a computer coin flip.

For example, if you select Choice 4 and Tails occurs, you will earn $5.2 If Heads occurs, you earn $1.6.

For every Choice, each flip outcome (Head or Tails) has a 50% chance of occurring.

	Coin Flip	Payoff	Chances
Choice 1	Heads	$2.8	50%
	Tails	$2.8	50%
Choice 2	Heads	$2.4	50%
	Tails	$3.6	50%
Choice 3	Heads	$2.0	50%
	Tails	$4.4	50%
Choice 4	Heads	$1.6	50%
	Tails	$5.2	50%
Choice 5	Heads	$1.2	50%
	Tails	$6.0	50%
Choice 6	Heads	$0.2	50%
	Tails	$7.0	50%

FIGURE 1. Timeline of events

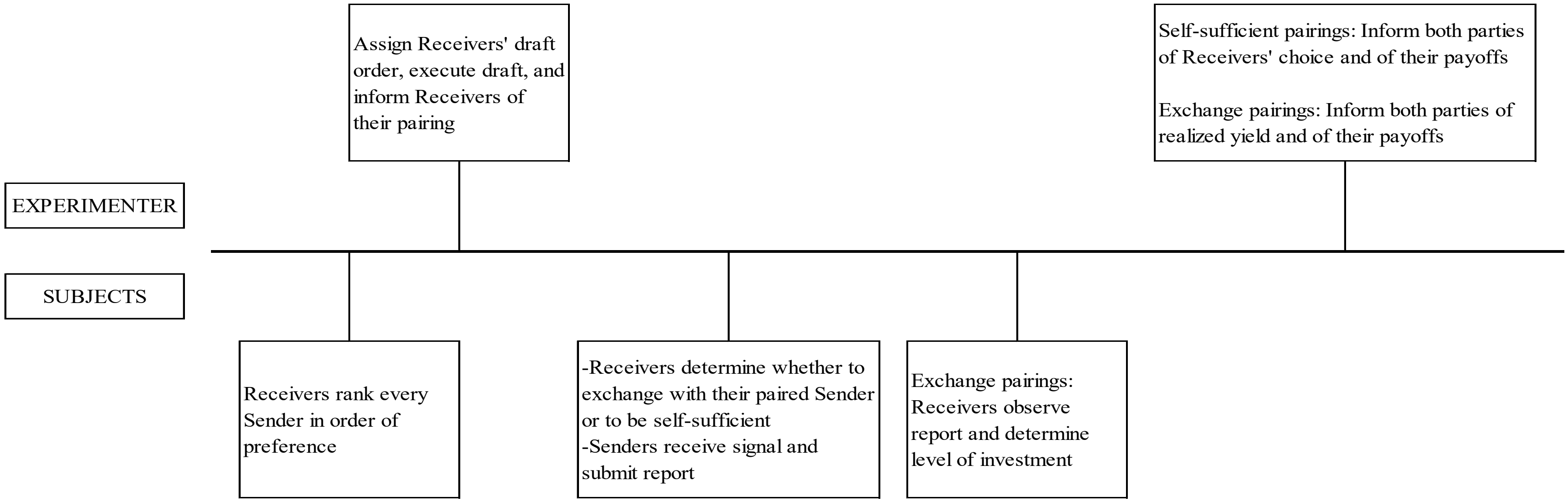


Figure 1 provides a timeline of events in our setting, starting with the second period. The timeline in the first period excludes the Investors' ranking of Reporters and begins with the experimenter assigning each Investor a draft position and determining Reporter-Investor pairings at random.

FIGURE 2. Effect of reporting errors in *Misaligned* condition on next period's rankings

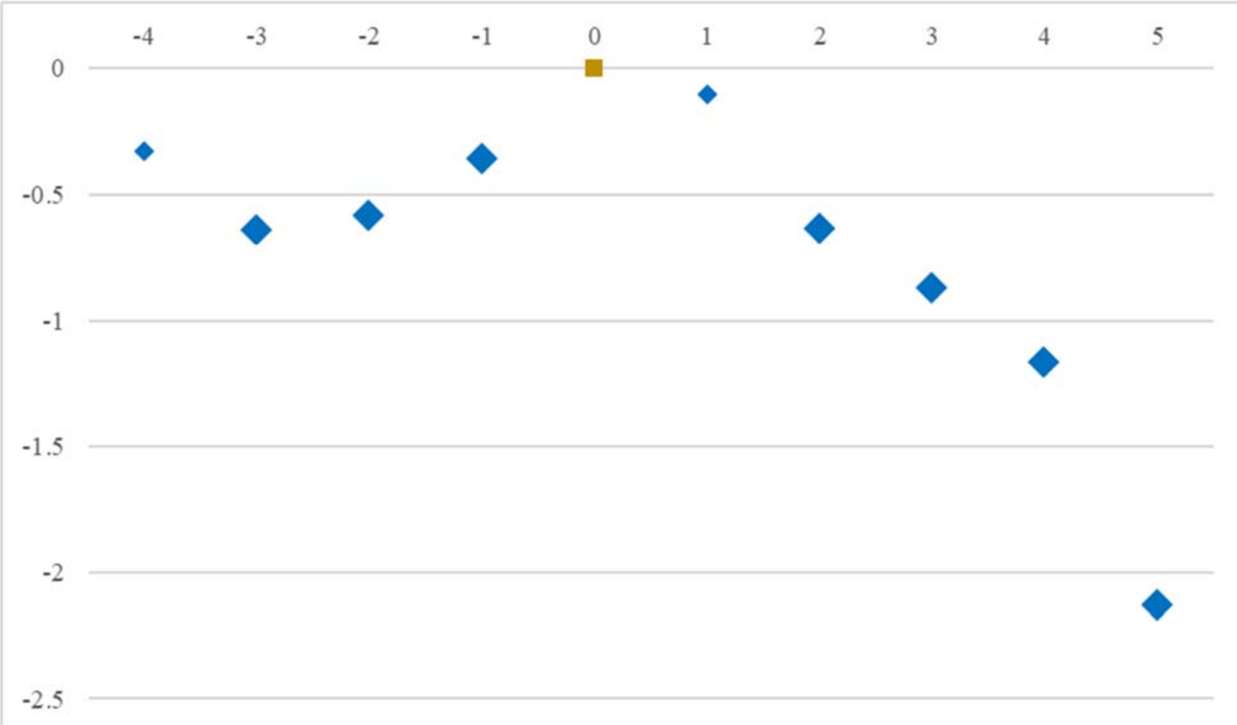
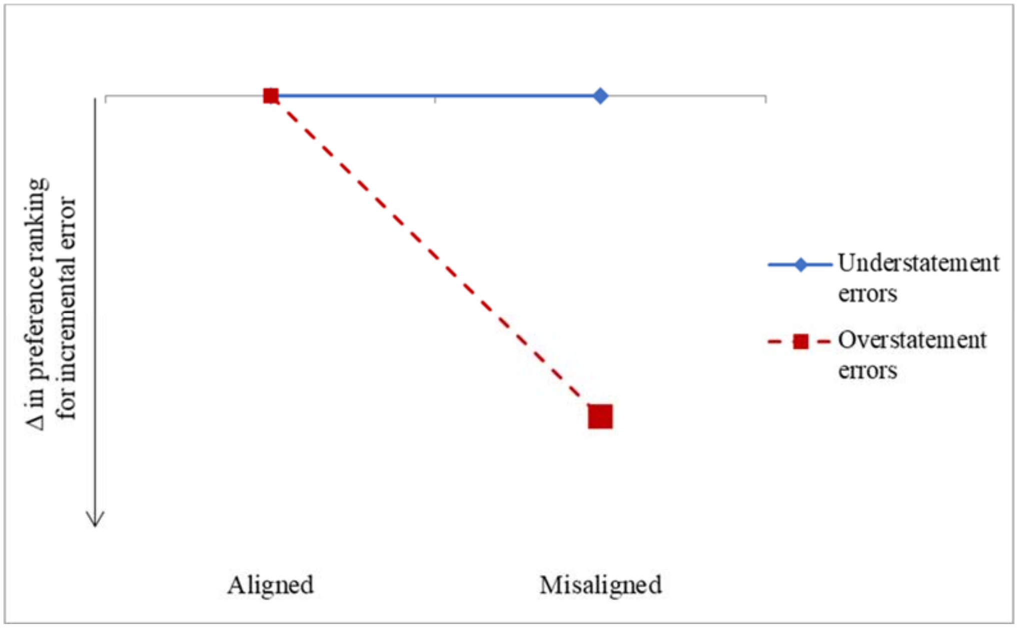


Figure 2 illustrates the *ceteris paribus* effect of a Reporter's reporting errors on their ranking in the next period in the baseline *Misaligned* condition. The estimates are from a regression of *Rank* on (i) indicator variables for each observed reporting error value (except zero) and (ii) control variables *Consistency*, *Accuracy*, *OthersConsistency*, and *OthersAccuracy*. The regression specifications were in-line with those for equation 1: a random-effects ordered logistic regression with standard errors clustered by Investor.

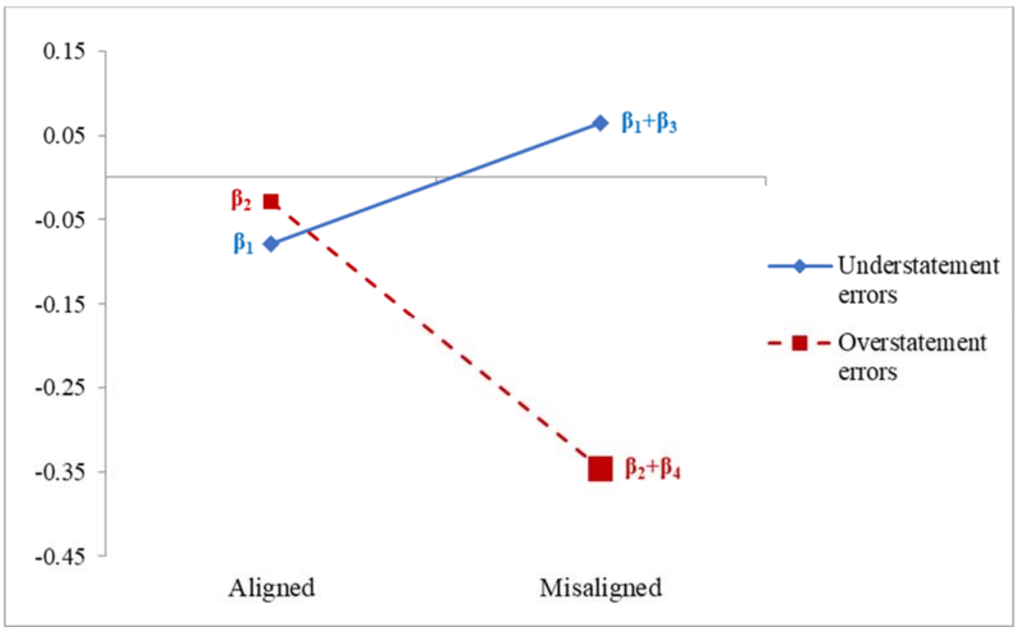
Coefficient estimates that are significantly different from zero at the 5% level are shown with a larger marker. The square at the origin indicates that an error of zero was excluded from the regression, so coefficients for all other errors are measured relative to an error of zero. Only error values with 30 or more observations are shown.

FIGURE 3. Effect of incremental errors other than through consistency and accuracy channels

Panel A. Predicted effect of incremental errors



Panel B. Observed effect of incremental errors



In Figure 3, Panel A illustrates our predictions that the decline in Investors' rankings is larger for an incremental overstatement error than an incremental understatement error in the *Misaligned* condition (H1a), and that this difference is larger than in the control *Aligned* condition (H2a). Panel B presents the observed results using coefficients from column (2) of Table 5, estimating equation 2. Effects predicted to be significantly different from zero (Panel A) and coefficient estimates that are significant at the 5% level (Panel B) are shown with larger markers.

$$Rank = \alpha_0 + \alpha_1 Neg + \alpha_2 Pos + \alpha_3 Misaligned + \alpha_4 Neg*Misaligned + \alpha_5 Pos*Misaligned + \beta_1|Error|*Neg + \beta_2|Error|*Pos + \beta_3|Error|*Neg*Misaligned + \beta_4|Error|*Pos*Misaligned + \sum\beta_k Controls + \sum\beta_k Controls*Misaligned \quad (2)$$

TABLE 1. Reporter and Investor payoffs in the *Misaligned* and *Aligned* conditions

Panel A. Payoffs in the *Misaligned* (baseline) condition

Investment – Yield	π_R	π_I	Total
9	$12	$–	$12
8	$13	$–	$13
7	$14	$–	$14
6	$15	$–	$15
5	$16	$–	$16
4	$17	$–	$17
3	$18	$ 5	$23
2	$17	$10	$27
1	$16	$15	$31
0	$15	$20	$35
–1	$14	$15	$29
–2	$13	$10	$23
–3	$12	$ 5	$17
–4	$11	$–	$11
–5	$10	$–	$10
–6	$ 9	$–	$ 9
–7	$ 8	$–	$ 8
–8	$ 7	$–	$ 7
–9	$ 6	$–	$ 6

Panel B. Payoffs in the *Aligned* (control) condition

Investment – Yield	π_R	π_I	Total
9	$ 9	$–	$ 9
8	$10	$–	$10
7	$11	$–	$11
6	$12	$–	$12
5	$13	$–	$13
4	$14	$–	$14
3	$15	$ 5	$20
2	$16	$10	$26
1	$17	$15	$32
0	$18	$20	$38
–1	$17	$15	$32
–2	$16	$10	$26
–3	$15	$ 5	$20
–4	$14	$–	$14
–5	$13	$–	$13
–6	$12	$–	$12
–7	$11	$–	$11
–8	$10	$–	$10
–9	$ 9	$–	$ 9

Table 1 provides the payoffs for the Reporter and Investor in the *Misaligned* condition (Panel A) and the *Aligned* condition (Panel B) for all possible outcomes. For both Panels, the Reporter payoffs represents the base payoff before the bonus adjustment for the period is applied.

TABLE 2. Summary statistics: Overall results per Reporter/Investor

Panel A. Baseline *Misaligned* condition

Variable	Obs	Mean	S.D.	25th Perc	Median	75th Perc
Reporting Bias	48	0.47	0.92	0.00	0.35	1.40
Investing Bias	48	–0.39	0.56	–0.63	–0.40	–0.15
Investing Efficiency	48	0.08	0.52	–0.25	0.00	0.37
Autarky	48	2.08%	5.04%	–	–	–
Reporter Earnings	48	$14.75	$2.05	$13.80	$15.00	$16.25
Investor Earnings	48	$12.55	$2.10	$11.25	$12.75	$13.50

Panel B. Control *Aligned* condition

Variable	Obs	Mean	S.D.	25th Perc	Median	75th Perc
Reporting Bias	48	0.00	0.31	–0.20	0.00	0.10
Investing Bias	48	0.01	0.59	–0.20	0.00	0.32
Investing Efficiency	48	0.00	0.56	–0.26	0.00	0.30
Autarky	48	2.71%	4.49%	–	–	–
Reporter Earnings	48	$16.22	$2.27	$14.25	$16.45	$18.25
Investor Earnings	48	$12.64	$1.76	$11.50	$12.50	$14.00

Panels A and B of Table 2 present summary statistics regarding the overall game results for both of our conditions.

Reporting Bias is the average difference between the report and the signal for each Reporter. *Investing Bias* (*Investing Efficiency*) is the average difference between the investment and the report (the average difference between the investment and the signal) for each Investor. *Autarky* represents the average frequency each Investor chose self-sufficiency. *Reporter Earnings* (*Investor Earnings*) is the average earnings for the Reporter (Investor) in each period. When the investment equals the signal, both parties' expected earnings is $15.00.

TABLE 3. Summary statistics: Periodic results per Reporter/Investor

Panel A. Reporting characteristics: Baseline *Misaligned* condition

Variable	Obs	Mean	S.D.	25th Perc	Median	75th Perc
Neg	480	0.30	0.46	–	–	–
Pos	480	0.49	0.49	–	–	–
|*Error*|**Neg*	146	1.86	1.24	1.00	2.00	2.00
|*Error*|**Pos*	234	2.02	1.24	1.00	2.00	2.00
Consistency	432	–1.29	0.66	–1.56	–1.18	–0.88
Accuracy	432	–1.46	0.79	–2.00	–1.33	–1.00

Panel B. Reporting characteristics: Control *Aligned* condition

Variable	Obs	Mean	S.D.	25th Perc	Median	75th Perc
Neg	480	0.41	0.49	–	–	–
Pos	480	0.35	0.48	–	–	–
|*Error*|**Neg*	195	1.71	0.96	1.00	2.00	2.00
|*Error*|**Pos*	167	1.84	1.12	1.00	2.00	2.00
Consistency	432	–1.20	0.74	–1.50	–1.10	–0.80
Accuracy	432	–1.37	0.85	–1.67	–1.21	–1.00

Panel A (B) of Table 3 present summary statistics for reporting characteristics for the baseline (control) condition.

Rank is the ranking position and is inverted so higher numbers reflect a better ranking (so six is the most preferred Reporter). *Neg* identifies understatement errors, *Pos* identifies overstatement errors, and |*Error*| is the absolute value of the reporting error. *Consistency* is the variance in the Reporter's reporting errors multiplied by negative-one. *Accuracy* is the Reporter's average absolute reporting error multiplied by negative-one.

TABLE 4. Investor preferences: Baseline *Misaligned* condition

Panel A. Regression results

DV = Rank	(1) Periods 1–9	(2) Periods 2–9	(3) Periods 2–9	(4) Periods 2–9	(5) Periods 3–9
Consistency		0.95***	0.96***	0.96***	1.09***
		(4.06)	(3.05)	(3.95)	(3.47)
Accuracy		0.94***	2.14***	0.77***	0.54
		(3.82)	(5.00)	(2.90)	(1.48)
OthersConsistency		–0.78***	–0.90**	–0.79***	–0.96***
		(–3.36)	(–2.28)	(–3.35)	(–3.10)
OthersAccuracy		–1.21***	–2.61***	–1.21***	–1.05***
		(–5.01)	(–6.87)	(–4.92)	(–3.26)
Neg	–0.47***	–0.56***	–0.41*	–0.61***	–0.65***
	(–2.81)	(–2.75)	(–1.76)	(–2.88)	(–2.67)
Pos	0.23	0.17	0.20	0.15	0.17
	(1.20)	(1.03)	(1.09)	(0.90)	(0.86)
|*Error*|**Neg*	–0.57***	0.06	0.02	0.01	0.01
	(–6.89)	(0.64)	(0.21)	(0.14)	(0.10)
|*Error*|**Pos*	–0.74***	–0.34***	–0.33***	–0.38***	–0.42***
	(–9.29)	(–4.29)	(–4.27)	(–4.54)	(–4.53)
Lag1Neg				–0.40**	–0.40**
				(–2.41)	(–2.43)
Lag1Pos				0.10	0.11
				(0.64)	(0.71)
|*Lag1Error*|**Lag1Neg*				–0.04	0.00
				(–0.62)	(0.03)
|*Lag1Error*|**Lag1Pos*				–0.21***	–0.23***
				(–2.96)	(–2.85)
Lag2Neg					–0.50***
					(–3.23)
Lag2Pos					0.19
					(1.07)
|*Lag2Error*|**Lag2Neg*					–0.12*
					(–1.88)
|*Lag2Error*|**Lag2Pos*					–0.21***
					(–2.93)
Fixed effects?	None	None	Pair	None	None
Observations	2,592	2,304	2,304	2,304	2,016
Log pseudolikelihood	–4,362	–3,543	–2,968	–3,532	–3,088
AIC	8,743	7,113	5,964	7,099	6,217
BIC	8,795	7,187	6,044	7,197	6,335
Wald chi-square	182.19	125.55	.	179.6	158.36

Panel B. Difference in an incremental understatement and overstatement error

DV = Rank	H1a	(1)	(2)	(3)	(4)	(5)
|*Error*|**Neg* – |*Error*|**Pos*	+	0.17*	0.41***	0.35***	0.39***	0.43***
		[2.82]	[12.63]	[7.96]	[13.50]	[14.15]
|*Lag1Error*|**Neg* – |*Lag1Error*|**Pos*	+				0.17	0.23*
					[2.55]	[3.49]
|*Lag2Error*|**Neg* – |*Lag2Error*|**Pos*	+					0.09
						[0.75]

Panel C. Difference in understatement and overstatement errors of equal magnitude

Fitted values from column (2)	H1b	±1	±2	±3	±4	±5
$(\alpha_1 + \beta_1 * \|Error\|) – (\alpha_2 + \beta_2 * \|Error\|)$	+	–0.33**	0.08	0.48**	0.89***	1.29***
		[5.91]	[0.55]	[5.52]	[8.45]	[9.82]

Panel A of Table 4 provides the results for the estimation of equation 1. Panel B provides results for hypothesis 1a, while Panel C provides results for hypothesis 1b.

$$Rank = \alpha_0 + \alpha_1 Neg + \alpha_2 Pos + \beta_1 |Error| * Neg + \beta_2 |Error| * Pos + \sum \beta_k Controls \qquad (1)$$

Each observation in Panel A reflects data on one Investor's ranking of one Reporter in one given period based upon their history of reporting errors. We utilize a random-effects ordered logit regression because the dependent variable consists of six ordered categories. Standard errors are heteroscedasticity-robust and clustered by Investor. Columns (1), (2) and (3) test the most recent reporting error, while Columns (4) and (5) include one and two additional lagged errors, respectively. For brevity, we omit the estimated constants (i.e., the "cut-points").

Rank is the ranking position and is inverted so higher numbers reflect a better ranking (so six is the most preferred Reporter). *Consistency* is the variance in the Reporter's reporting errors multiplied by negative-one. *Accuracy* is the Reporter's average absolute reporting error multiplied by negative-one. *OthersConsistency* and *OthersAccuracy* are the average *Consistency* and *Accuracy* of the other five Reporters. *Neg* identifies understatement errors, *Pos* identifies overstatement errors, and |*Error*| is the absolute value of the reporting error.

Z-statistics [Chi-square statistics] are in brackets underneath each coefficient in Panel A [Panels B and C]. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level

TABLE 5. Investor preferences: Both baseline *Misaligned* and control *Aligned* conditions

Panel A. Regression results

DV = Rank	(1) Periods 1–9	(2) Periods 2–9	(3) Periods 2–9	(4) Periods 2–9	(5) Periods 3–9
Consistency		1.28***	1.50***	1.28***	1.89***
		(7.62)	(8.62)	(7.46)	(5.54)
*Consistency*Misaligned*		–0.31	–0.53	–0.31	–0.79*
		(–1.10)	(–1.50)	(–1.10)	(–1.76)
Accuracy		1.55***	1.83***	1.55***	1.08**
		(6.20)	(5.08)	(5.67)	(2.52)
*Accuracy*Misaligned*		–0.59*	0.34	–0.77**	–0.51
		(–1.84)	(0.64)	(–2.17)	(–0.94)
OthersConsistency		–1.00***	–1.67***	–1.00***	–1.21***
		(–4.62)	(–7.52)	(–4.64)	(–3.38)
*OthersConsistency*Misaligned*		0.22	0.76*	0.19	0.26
		(0.69)	(1.69)	(0.61)	(0.54)
OthersAccuracy		–0.73***	–1.68***	–0.77***	–0.67**
		(–3.56)	(–4.05)	(–3.78)	(–2.14)
*OthersAccuracy*Misaligned*		–0.50*	–0.97*	–0.45	–0.41
		(–1.66)	(–1.78)	(–1.52)	(–0.93)
Neg	0.88***	–0.14	–0.11	–0.13	–0.39**
	(5.76)	(–0.82)	(–0.59)	(–0.78)	(–2.25)
Pos	0.03	–0.27	–0.41**	–0.26	–0.32
	(0.27)	(–1.44)	(–2.21)	(–1.39)	(–1.61)
Misaligned	0.22*	–1.28***	–1.19*	–1.25***	–1.34***
	(1.83)	(–4.48)	(–1.67)	(–4.31)	(–4.50)
*Neg*Misaligned*	–1.35***	–0.43	–0.30	–0.47*	–0.19
	(–6.01)	(–1.60)	(–0.98)	(–1.74)	(–0.67)
*Pos*Misaligned*	0.19	0.45*	0.61**	0.43*	0.52*
	(0.84)	(1.78)	(2.32)	(1.65)	(1.89)
*|Error|*Neg*	–1.10***	–0.08	–0.14	–0.09	0.02
	(–11.56)	(–0.57)	(–0.88)	(–0.61)	(0.12)
*|Error|*Pos*	–0.71***	–0.03	–0.04	–0.03	0.02
	(–9.49)	(–0.25)	(–0.30)	(–0.27)	(0.18)
*|Error|*Neg**Misaligned	0.54***	0.14	0.16	0.10	–0.02
	(4.47)	(0.85)	(0.85)	(0.59)	(–0.09)
*|Error|*Pos*Misaligned*	–0.02	–0.32**	–0.29**	–0.35**	–0.44***
	(–0.24)	(–2.26)	(–1.98)	(–2.43)	(–2.91)
Lag1Neg				–0.33***	–0.32***
				(–2.68)	(–2.66)
Lag1Pos				0.05	0.01
				(0.44)	(0.10)
*|Lag1Error|*Lag1Neg*				–0.15*	–0.06
				(–1.75)	(–0.64)
*|Lag1Error|*Lag1Pos*				–0.09	–0.08
				(–1.25)	(–1.06)

*|Lag1Error|*Lag1Neg*Misaligned*				0.14*	0.08
				(1.73)	(0.74)
*|Lag1Error|*Lag1Pos*Misaligned*				–0.10	–0.10
				(–1.24)	(–1.09)
Lag2Neg					–0.14
					(–1.04)
Lag2Pos					0.20
					(1.53)
*|Lag2Error|*Lag2Neg*					0.00
					(–0.02)
*|Lag2Error|*Lag2Pos*					–0.10
					(–1.17)
*|Lag2Error|*Lag2Neg*Misaligned*					–0.03
					(–0.27)
*|Lag2Error|*Lag2Pos*Misaligned*					–0.08
					(–0.93)
Fixed effects?	None	None	Pair	None	None
Log pseudolikelihood	–8,739	–6,999	–5,874	–6,987	–6,135
AIC	17,506	14,041	11,810	14,030	12,338
BIC	17.597	14,183	12,009	14,210	12,552
Observations	5,184	4,608	4,608	4,608	4,032
Wald chi-square	392.16	335.41	.	435.01	408.62

Panel B. Difference-in-differences: Incremental understatement and overstatement error by condition

DV = Rank	H2a	(1)	(2)	(3)	(4)	(5)
*|Error|*Neg*Misaligned –* *|Error|*Pos*Misaligned*	+	0.56***	0.46**	0.46**	0.45**	0.42**
		[15.19]	[6.51]	[4.49]	[6.57]	[5.49]
*|Lag1Error|*Neg*Misaligned –* *|Lag1Error|*Pos*Misaligned*	+				0.24*	0.18
					[2.73]	[0.94]
*|Lag2Error|*Neg*Misaligned –* *|Lag2Error|*Pos*Misaligned*	+					0.06
						[0.11]

Panel C. Difference-in-differences: Under- and overstatement errors of equal magnitude by condition

Fitted values from column (2)	H2b	±1	±2	±3	±4	±5
$(\alpha_4 + \beta_3 * \lvert Error \rvert) - (\alpha_5 + \beta_4 * \lvert Error \rvert)$	+	–0.41**	0.05	0.51*	0.97**	1.43**
		[6.17]	[0.08]	[2.74]	[4.17]	[4.85]

Panel A of Table 5 provides the results for the estimation of equation 2. Panel B provides results for hypothesis 2a, while Panel C provides results for hypothesis 2b.

$$\begin{aligned} Rank = \alpha_0 + {} & \alpha_1 Neg + \alpha_2 Pos + \alpha_3 Misaligned + \alpha_4 Neg*Misaligned + \alpha_5 Pos*Misaligned + \\ & \beta_1 \lvert Error \rvert * Neg + \beta_2 \lvert Error \rvert * Pos + \beta_3 \lvert Error \rvert * Neg*Misaligned + \\ & \beta_4 \lvert Error \rvert * Pos*Misaligned + \textstyle\sum \beta_k Controls + \sum \beta_k Controls*Misaligned \end{aligned} \quad (2)$$

Each observation in Panel A reflects data on one Investor's ranking of one Reporter in one given period based upon their history of reporting errors. We utilize a random-effects ordered logit regression because the dependent variable consists of six ordered categories. Standard errors are heteroscedasticity-robust and clustered by Investor. Columns (1), (2) and (3) test the most recent reporting error, while Columns (4) and (5) include one and two additional lagged errors, respectively. For brevity, we omit the estimated constants (i.e., the "cut-points").

Rank is the ranking position and is inverted so higher numbers reflect a better ranking (so six is the most preferred Reporter). *Consistency* is the variance in the Reporter's reporting errors multiplied by negative-one. *Accuracy* is the Reporter's average absolute reporting error multiplied by negative-one. *OthersConsistency* and *OthersAccuracy* are the average *Consistency* and *Accuracy* of the other five Reporters. *Neg* identifies understatement errors, *Pos* identifies overstatement errors, and |*Error*| is the absolute value of the reporting error. *Misaligned* identifies pairings in the *Misaligned* condition.

Z-statistics [Chi-square statistics] are in brackets underneath each coefficient in Panel A [Panels B and C]. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level

TABLE 6. Supplemental analysis: Participants' reports on trust

Panel A. Investors in the *Misaligned* condition

DV = Trust	(1) First-order report		(2) Second-order report	
	Mean (z-stat)	% Change in Odds	Mean (z-stat)	% Change in Odds
Consistency	2.27*** (2.82)		1.35* (1.86)	
Accuracy	0.57 (0.95)		1.29** (2.18)	
Understated	0.74* (1.73)	109%	0.55 (1.37)	74%
Overstated	–0.61 (–1.18)	–45%	–0.57 (–1.32)	–43%
Understated – Overstated	1.34** [4.63]		1.12** [4.50]	
Observations	288		288	
Total Reporters	48		48	
Wald chi-square	15.75		18.26	
p > chi-square	0.0034		0.0011	

Panel B. Reporters in the *Misaligned* condition

DV = Trust	(1) First-order report		(2) Second-order report	
	Mean (z-stat)	% Change in Odds	Mean (z-stat)	% Change in Odds
Consistency	0.90 (1.41)		1.09 (1.62)	
Accuracy	1.44** (2.17)		1.62*** (3.01)	
Understated	–0.49 (–1.22)	–39%	–0.83** (–2.10)	–56%
Overstated	–0.95** (–2.40)	–61%	–0.36 (–1.05)	–30%
Understated – Overstated	0.46 [0.57]		–0.47 [0.67]	
Observations	288		288	
Total Reporters	48		48	
Wald chi-square	16.70		18.94	
p > chi-square	0.0022		0.0008	

In Table 6, each observation reflects a Trust/Don't Trust selection on a Reporter based upon their history of reporting errors. We use a logit regression because the dependent variable is binary. The dependent variable in column (1) is the participants' reports on who they trust. The dependent variable in column (2) is the participants' report on who they believe Investors said they trust. The final value within each column provides the percentage change in odds for an increase in *Trust* for a unit increase in the independent variable of interest. This is calculated as [(e^coefficient)–1], for example, in column (1) of Panel A: (e^0.74) – 1 = 2.09 – 1 = 1.09. For brevity, we omit the estimated constants.

Trust equals one when the participants respond "Trust" and zero when they respond, "Don't Trust." *Consistency* is the variance in the Reporter's reporting errors multiplied by negative-one. *Accuracy* is the Reporter's average absolute reporting error multiplied by negative-one. *Understated* (*Overstated*) equals one when the Reporter's aggregate reporting error is negative (ten or greater); both variables represent 12 out of 48 Reporters.

Z-statistics [chi-square statistics] are in brackets underneath each coefficient. Standard errors are heteroscedasticity-robust and clustered by participant – Investors in Panel A and Reporters in Panel B. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level