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Comments

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The Conservatism Principle and Asymmetric Preferences Over Reporting Errors

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ABSTRACT: At present, accounting conservatism is generally viewed from a measurement or reporting perspective. In contrast, we consider whether it relates to a moral rule of conduct. Conservatism has been described as deriving from a preference for reporting errors to be in the direction of understatement rather than overstatement. We experimentally pair Reporters who provide information with Users who rely on the information. We posit that under misaligned incentives that motivate aggressive reporting, Users view an aggressive report as reflecting Reporters' exploitative intent and expect that a social norm prohibiting aggressive reporting applies. We predict that Users use noisy reporting errors to gauge Reporters' norm compliance. Consistent with this we find that, *ceteris paribus*, Users prefer not to be paired with Reporters who produce overstatement errors that are likely to reflect aggressive reporting. This preference, revealed through Users' incentivized actions, is both inconsistent with neoclassical economic models and cannot be explained by loss aversion. Alternatively, when Reporters' motives are aligned with Users', we find no such preference. While our evidence is indirect, it opens the possibility that conservatism emerged from a norm that enhances trust and cooperation among economic agents. We believe this insight can open new possibilities for conservatism research.

Keywords: accounting conservatism; experimental economics; intentions; moral hazard
JEL Codes: B52, D81, D82, M41

1. INTRODUCTION

Societies adapt practices such as honesty, integrity, and reliability into informal rules – social norms – that restrict the flexibility of individual choices in order to promote cooperative activity and increase economic efficiency (North 2005). While conservatism has been a feature of accounting systems for at least five hundred years (Littleton 1941), scholars' current explanations for its presence largely require features of modern economies such as formal contracts and government regulation (Watts 2003a). In contrast, we consider whether conservatism emerged from the practices societies adopted to promote cooperative activity in the period before the development of formal institutions (Smith 2008; Dickhaut et al. 2010).

Imagine a nascent relationship between a farmer and a baker in which the farmer reports their estimated crop yield to the baker, who then plans their productive capacity. When realized, the farmer sells as much of their yield to the baker as the baker can use. While the farmer prefers that the baker build greater capacity, the baker incurs costs for both insufficient and unused capacity. Say that in one possible instance the farmer reports seven bushels and the actual yield is five bushels – while in another instance the farmer reports three bushels, and the yield is five bushels. Even if *ex ante* honest estimates of seven bushels and three bushels are equally likely to result in a yield of five bushels, given the underlying moral hazard – the farmer's incentive to overstate their report to try to induce the baker to build greater capacity – *ex post*, it is more likely the baker was exploited if the report was seven bushels than three. Sanders et al. (1938) describe conservatism by stating that "[t]here is a prevalent assumption that, while overstatement…is a major fault, understatement…may be a positive virtue." We propose that this relative preference for understatement versus overstatement arises when people are vulnerable to exploitation via aggressive reporting, as the baker is vulnerable to the farmer.

We design an experiment that tests for this preference and which isolates our proposed 'moral hazard' mechanism. In our setting, a Reporter develops an informed, private estimate of their outcome for the period and sends a report to the User, who takes an action that relies on the report. The realized outcome is then publicly revealed, and each party's payoffs are disclosed. Similar to the farmer (Reporter) communicating his expected yield to the baker (User), the User's payoff is maximized when their action (e.g., capacity choice) exactly meets the realized outcome (e.g., farmer's yield), and the User incurs symmetric costs for actions greater than and less than the outcome. We manipulate the Reporter's payoff in a 1 x 2 between-subjects design. In the *Aligned* treatment, the Reporter's payoff is also maximized when the User's action is equal to the realized outcome. In the *Misaligned* treatment, the Reporter's payoff is maximized when the User's action is greater than the outcome – so while Reporters are motivated to report aggressively to try to deceive the User and induce greater action (e.g., to build larger capacity), Users are motivated to anticipate and avoid such exploitation. The Reporter's 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 reported outcomes in the presence of various agency conflicts (Kothari et al. 2010).

We group six Reporters and six Users who interact in pairs for ten periods. At the start of each period, we provide Users with graphs that display each Reporters' full history of reporting errors, and we ask Users to rank the Reporters in the order they prefer to be paired. We then randomly select Users and pair each with their highest-ranked Reporter that has yet to be paired. As the rankings drive next period's pairings, they reveal Users' preferences over Reporters.

Consider the *Misaligned* treatment, which is characterized by (i) information asymmetry, (ii) measurement uncertainty, and (iii) misaligned incentives that motivate aggressive reporting.

The first two characteristics create a focus on the Reporters' *ex post* reporting error (i.e., whether the report over- or understated the realization), as this is all the Users observe. Since Users can strategically discount any predictable bias in the Reporter's history, Users should prefer consistent reporting errors. Further, if they are solely motivated by their own payoff, they 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 (e.g., McCabe et al. 2003; Sutter 2007). In light of the Reporter's motive, from the perspective of the User the greater the reporting error, the more likely it is that the Reporter had exploitative intent. We posit that Users view exploitation via aggressive reporting as violating an informal rule and, as such, their trust in a Reporter corresponds to their assessed probability of being exploited (Yamagishi 1998). We therefore predict that, *ceteris paribus*, Users disprefer (i.e., prefer not to be paired with) Reporters who produce errors that are likely to reflect aggressive reporting.

We test this hypothesis by regressing the Users' preference rankings in the *Misaligned* treatment on attributes of each Reporter's error history in an ordered logit model. We expect and find that Users prefer Reporters with consistent and accurate reporting errors. Controlling for these factors, the results support our predictions. First, the decline in Users' rankings is larger for Reporters who produce a large overstatement error compared to those who produce an understatement error of equal magnitude, indicating that Users disprefer Reporters who are most likely to have reported aggressively. Second, the decline in Users' rankings is larger for Reporters who produce an incremental overstatement error than an incremental understatement error. This indicates that, in the presence of measurement error, Reporters are likely to face greater sanctioning for an aggressive report than for a conservative one.

Next, we predict that Users' apparent dispreference for aggressive reporting is driven by the Reporters' motivation to report aggressively. We test this hypothesis in an ordered logit model with data from both treatments, and test for difference-in-differences: that Users' asymmetric preferences for reporting errors are larger when incentives are misaligned than when aligned. The results support our predictions, as we do not observe asymmetric preferences in the *Aligned* treatment as we do in the *Misaligned* treatment. For example, after controlling for the effect errors have on the consistency and accuracy of reports, we find that an incremental error – both over- and understated – has no significant effect on Users' rankings in the *Aligned* treatment. Similarly, an incremental understatement error has no significant effect on rankings in the *Misaligned* treatment. That is, in those circumstances there are no factors outside of consistency and accuracy that lead Users to disprefer larger magnitude errors. But when Reporters are motivated to report aggressively, an incremental overstatement error is associated with a significant decline in their average rank. That is, there is some additional factor leading Users to disprefer larger magnitude overstatement errors when incentives are misaligned.

After the final round of the experiment, we ask Users to identify each Reporter as one they either "trust" or "don't trust." We then ask Users to predict which Reporters *other* Users said they trust, and we provide a payment for each correct prediction. Analysis from a logit regression indicates that, *ceteris paribus*, Users in the *Misaligned* treatment are more than twice as likely to indicate trust in a Reporter whose aggregate reporting error is negative, and are significantly more likely to report trust in a Reporter whose aggregate reporting error is in the bottom quartile than the top quartile. Further, Users are significantly more likely to believe that other Users report more trust in 'understated' than 'overstated' Reporters. This is consistent with

Users perceiving a shared understanding regarding the link between the direction of reporting errors and Reporters' underlying intentions in light of their motive to report aggressively.

In a setting featuring three realistic characteristics, we demonstrate that Users disprefer Reporters who produce reporting errors that are likely to reflect aggressive reporting. The preference is inconsistent with strategic self-interested behavior derived from neoclassical economic theory, however, consistent with prior research we infer the preference reflects Users' expectations that aggressive reporting violates an informal rule (e.g., Hobson et al. 2011). While this is a prerequisite for a social norm, we do not observe the activation of a norm (Bicchieri 2006) because a social norm requires agreement between Users' normative expectations and Reporters' beliefs about them (Bicchieri and Chavez 2010, 166). However, in Users' normative expectations we demonstrate a necessary initial condition. As such, we explore how reporting might evolve in the presence of this condition.

Consider a social exchange economy in which Users view aggressive reporting as exploitative. Through communication and repeat interaction within the society, a common understanding can emerge that aggressive reporting violates a social norm. If so, then a Reporter who has produced overstatement errors can develop a reputation for exploitation and, through direct and indirect reciprocity, may lose subsequent exchange opportunities. By choosing to report more conservatively, the Reporter can expect to reduce their overstatement errors. Doing so can help indicate their good intentions, which, over time, can repair their reputation, strengthen relationships with exchange partners, and facilitate ongoing exchange. That is, conservatism can emerge as an *informal* bonding cost, borne by the Reporter, 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).

Importantly, while contracting-based conservatism studies generally assume that external parties have asymmetric payoffs to the firm's performance – that they face greater exposure to downside risk (e.g., Hui et al. 2012) – our mechanism does not require this feature. As such, we employ experimental methodology to provide a novel mechanism from which the contracting basis for conservatism can emerge, thereby connecting our findings with the broader empirical literature (Watts 2003a, 2003b). Further, because Reporters in our setting do not face good news or bad news conditions and do not report gains or losses, our findings can be generalized to settings beyond those that are the primary focus of our current literature (Basu 1997).

We do not suggest that our moral hazard mechanism excludes other fundamental explanations (e.g., Basu and Waymire 2017), but rather that it adds another element of an evolutionary process. However, one distinguishing feature of ours relative to some others (e.g., Hirshleifer and Teoh 2009; Braun 2016) is that the preferences revealed in our study are not driven by loss aversion (Kahneman and Tversky 1979). That said, we believe that the moral hazard and loss aversion mechanisms supplement each other in driving these preferences in the real world as, in many common instances, a User will view an *ex post* overstatement error as a loss (an 'under-realization') and an understatement error as a gain (an 'over-realization').

In summary, we believe our research adds to traditional explanations of conservatism. The Accounting Principles Board described conservatism as deriving from a general preference for reporting errors to be in the direction of understatement rather than overstatement (APB 1970, para 171). We propose that this behavior may have evolved from practices that enhance trust and cooperation within social exchange economies. As this regards prehistoric human behavior, we employ experimental economics methodology to test the idea in an abstract, low-context setting. Sterling (1967, 110) describes conservatism as "the most ancient and probably the most pervasive principle of accounting valuation." Others 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. We believe this can open interesting new possibilities for conservatism research.

2. Background and Hypotheses

2.1 Literature review

The Financial Accounting Standards Board (FASB) has defined conservatism as "a prudent reaction to uncertainty…. if two estimates…are equally likely, conservatism dictates using the less optimistic estimate" (FASB 1980, para 95). Indeed, conservatism evolved from the prudence concept, originally considered a moral virtue conducive to honesty and competence in business (Maltby 2000).[1] 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. Conservatism has been observed in emergent accounting systems from Germany (Harris et al. 1994) to Japan (Someya 1996). Scholars' most common explanations for its presence are that it: (i) addresses moral hazard concerns of external (contracting) parties; (ii) reduces shareholder litigation; (iii) reduces the costs of taxation; and (iv) reduces the political costs imposed on accounting regulators (Watts 2003a). While the latter three explanations require features of modern economies – e.g., government structures that allow for corporations, taxation, and regulators – the contracting explanation may have historical roots.

[1] In *The Theory of Moral Sentiments*, Adam Smith states that the prudent person "is always sincere, and feels horror at the very thought of exposing himself to the disgrace which attends upon the detection of falsehood" (Smith 1790/2011, 215). To the extent *ex post* overstatement errors are viewed as a reflection of *ex ante* dishonesty, this description indicates that a prudent Reporter will report conservatively in the presence of measurement error.

Studies supporting the contracting explanation often focus on counterparties' asymmetric payoffs with respect to firm financial performance, as strong performance does not increase their payoff while weak performance decreases it (Ruch and Taylor 2015). For example, Qiang (2007) finds that firms facing higher contracting costs choose conditionally rather than unconditionally conservative accounting.[2] Focusing on debt contracting, Ahmed et al. (2002) and Zhang (2008) find that conservatism reduces firms' debt costs, while Wittenberg-Moerman (2008) finds evidence consistent with it decreasing a borrower's information asymmetry.

Ball et al. (2000) find that conservatism is greater in common-law countries, concluding that information asymmetry is more likely resolved in code-law countries by other institutional features. Ball and Shivakumar (2005) find that UK private firms, who are more likely to resolve information asymmetry through an 'insider access' model, report less conservatively than do public firms. LaFond and Watts (2008) focus on the manager's asymmetric payoffs to reporting (stronger incentives to over- than understate performance) and find evidence consistent with conservatism decreasing information asymmetry between a firm and its outside equity investors.

Analytical models have also shown possible reasons why conservative accounting measurement affects labor or debt contracting (e.g., Gigler and Hemmer 2001; Bagnoli and Watts 2005; Gigler et al. 2009; Göx and Wagenhofer 2009; Kirschenheiter and Ramakrishnan 2012; Gao 2013; Nagar et al. 2018). In these models, a conservative choice pertains to the initial design of the accounting measurement system – that is, it is a property of the measurement system rather than of the preparer's reporting choices.

[2] Conditional conservatism occurs when negative economic news is recognized in accounting earnings in a timelier manner than positive economic news, while unconditional conservatism occurs through the consistent under-recognition of accounting net assets (Ruch and Taylor 2015).

In sum, historical evidence suggests that there must be fundamental reasons behind the development of accounting conservatism, while the empirical literature highlights some core factors that might contribute to such development, including (i) information asymmetry between preparers and users of reports and (ii) preparers' incentives to overstate reports. Further, whereas the theoretical literature has primarily focused on conservatism as a measurement issue, we consider whether conservatism was adapted from a rule of conduct.

2.2 Background

In early civilizations, during the transition from "maker to manufacturer" (Rosenberg 1994), humans 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 that 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. If, for instance, enough individuals within a society believe that some behavior is inappropriate, those refraining from the conduct face lower transaction costs than those continuing it and, over time, the emergent practice becomes entrenched as a social norm (Smith 2003, 484). 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 small-scale social economies.[3]

2.3 Setting

Imagine a social exchange economy comprised of Reporters and Users who can choose to enter into informal agreements to exchange with one another. If they agree to exchange, the Reporter develops an informed estimate of their demand for the period and sends a report to the User, who takes an action that relies on the report. Inherent to this process is information asymmetry, as the Reporter's best estimate reflects their private information, and measurement uncertainty, because they cannot perfectly estimate future demand. One example of this process is that customers communicate nonbinding expectations about demand to suppliers, who can plan their productive capacity based upon these reports (Özer et al. 2011).

Once realized, the demand is publicly known. The User's payoff is maximized when their action equals the realized demand, and they incur symmetric costs if their action overstates or understates the realization. However, the Reporter's payoff is maximized when the User's action exceeds the demand; as such, if the User naively trusts the report, the Reporter has short-term incentives to report aggressively to encourage greater action.[4] For example, to ensure abundant supply a customer may inflate their estimated demand to try to induce their supplier to build larger capacity, while the supplier faces costs for both insufficient and unused capacity.

Another factor inherent to this setting is ambiguity (i.e., Knightian uncertainty), as neither party has complete knowledge of the degree of measurement uncertainty. That is, there is no

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[3] Our focus is on social exchange because, while organisms likely evolved to respond 'conservatively' to stimuli before humans engaged in 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).

[4] 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 can impose costs on others.

verification mechanism, as there are no combinations of report and realization for which the User can be *certain* the Reporter did not provide their best estimate. Indeed, even if a Reporter provides a report that understates their best estimate, they can still overestimate the realized demand – so, while overstatement errors may be the result of aggressive reporting, the User 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.

As the only evidence available to guide future interactions is the *ex post* reporting error (i.e., the report minus the realized demand), the parties' focus is on this figure. Communication within the economy can create common knowledge about each Reporter's history of reporting errors. To the extent Users have normative expectations regarding Reporters' behavior, they can use their common knowledge to enact positive and negative sanctions (reward and punishment) against Reporters based on their norm compliance (Bicchieri 2006).

2.4 Hypotheses

Our expectation is that Users will examine each Reporter's behavior for signs that they are trustworthy. Trust arises when there is interdependence between parties (Rousseau et al. 1998). In our setting, the User must rely on the Reporter's report to guide their chosen action, which directly affects both parties' 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). The latter is created in our setting by measurement uncertainty. The former is created in part by information asymmetry and enhanced by the incentive misalignment inherent in the payoff structures, which creates substantial uncertainty about the Reporter's intentions.

Rabin (1993) highlighted the role of intentions in shaping attitudes – that people judge others not only on the consequences of their actions, but also according to their motives.[5] In our setting, to the extent aggressive reporting leads to aggressive action (i.e., the User is deceived), an aggressive report, in expectation, both effects a wealth transfer from User to Reporter and reduces social welfare. As social norms generally apply to situations in which there is a conflict between selfish and pro-social incentives, the payoff structures may serve as a cue that the situation is one in which a norm applies (Bicchieri 2006). To the extent Users consider Reporters' motives, they may view aggressive reporting as an exploitation attempt intended to achieve a selfish outcome. Therefore, we posit that Users will perceive a social norm to apply in this situation: an informal rule prohibiting aggressive reporting.

If Users view aggressive reporting as violating a social norm, how can they gauge Reporter's compliance? Because the User does not observe the Reporter's private signal, they cannot determine whether a given report is aggressively biased (i.e., whether the report is greater than the signal). Rather, the User only observes reporting errors when the state of nature is revealed at the end of the period, after they have chosen their action. Therefore, we posit that Users will use noisy reporting errors to gauge Reporters' compliance with the social norm.

> Hypothesis 1: *Ceteris paribus*, Users disprefer Reporters producing reporting errors that are likely to reflect aggressive reporting.

> Hypothesis 2: Users' dispreference for reporting errors likely to reflect aggressive reporting is driven by the Reporters' motivation to report aggressively.

[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 participants' preferences over outcomes (e.g., Levine 1998; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). In our setting, the Reporter's report does not directly affect the User's payoff, which is a function of the User's action and a state of nature. Therefore, any User's preference over reporting outcomes must necessarily relate to their reading of the Reporter's intention rather than to the report itself.

3. Experimental Design

3.1 Overview of experiment

We construct a multi-period information transmission game that pairs Reporters and Users in communities that consist of six Reporters and six Users apiece. They interact in pairs for ten periods, with new pairings each period. The Reporter receives a noisy private signal of the state of nature and prepares a report for the User. That the signal is noisy and private operationalizes measurement uncertainty and information asymmetry, respectively.[6] While the Reporter prepares their report, the User decides whether they want to interact with the paired Reporter in that period. If the User opts out, both parties receive a fixed payment. If the User opts in, they receive the report and choose their action. The state of nature, action, and realized payoffs are then revealed to both agents, and the period ends.

The report, action, and state of nature are integers with range one to ten. The signal is an integer between three through eight, each with equal probability. The state of nature 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, as the expected value of the absolute value of the error equals one. Notably, we inform the parties only that (i) the signal is equal to the expected value of the state of nature in each period and (ii) the state of nature is equally likely to be greater than or less than the signal. That is, to operationalize ambiguity we do not inform them of any other details of either distribution, thus eliminating the User's ability to verify – even *ex post* – whether any report was biased.

[6] The Accounting Principles Board description of conservatism, provided on page six, 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)

Reporter-User pairings in the first period are determined at random. Starting with the second period, each period Users review Reporters' histories of reporting errors and rank every Reporter in the order they prefer to be paired. We then randomly select Users and pair each with their highest ranked unpaired Reporter – that is, the User selecting first is paired with their most preferred Reporter, the User selecting second is paired with their highest-ranking unpaired Reporter, and so on, until the User selecting sixth is paired with the remaining unpaired Reporter. To operationalize sanctioning, Reporters receive a bonus adjustment to their payoff based on how early they are selected, as follows: #1 (+$4), #2 (+$3), #3 ($0), #4 ($0), #5 (–$3), #6 (–$4). We then notify each Reporter of both their selection position and their average ranking in the period among all Users. Each period, we also notify Users of where they ranked the Reporter they are paired with and we ask them if they want to interact with the Reporter or be self-sufficient. If they choose self-sufficiency, both parties receive a payoff of $5.

Lastly, we use these rankings to measure Users' preferences over Reporters for our hypothesis tests. The rankings reveal User preferences because they directly determine the next period's pairings (i.e., how Users 'acquire' their preferred Reporter), and Users cannot strategically "game" the process by not telling the truth (i.e., the mechanism is incentive-compatible).

3.2 Misaligned treatment: Hypothesis 1

The payoffs for each party in the *Misaligned* treatment are presented in Panel A of Table 1. The payoffs 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). While the User's payoff is maximized when the action equals the state of nature and they incur a symmetric cost if their action is greater than or less than the state of nature, the Reporter's payoff is maximized when the action exceeds the state of nature by 3.[7]

[insert Table 1 about here]

Because both parties' payoffs are driven by the difference between the action and the realized state of nature – and not by the absolute level of the realized state – signals contain neither 'good' nor 'bad' news, and reports represent neither gains nor losses.[8] Further, when the action is equal to the signal in the *Misaligned* treatment, the payoffs provide for (i) an expected Users' payoff equal to the expected Reporters' payoff and (ii) maximization of the total expected payoff. As such, an aggressive report that induces an aggressive action will, in expectation, both transfer wealth from the User to the Reporter and reduce total welfare. The welfare loss arises because the wealth transferred from the User ($5 per unit) significantly exceeds the wealth transferred to the Reporter ($1 per unit).

We test hypothesis 1 within the *Misaligned* treatment. Hypothesis 1 predicts that Users disprefer Reporters producing reporting errors that are likely to reflect aggressive reporting. If so, they will disprefer a large overstatement to a large understatement error of equal magnitude. In addition, Users will view each incremental overstatement error – but not each incremental understatement error – as indicating a higher probability of noncompliance. Therefore, we test H1 via two discrete predictions: that, *ceteris paribus*, Users disprefer to be paired with Reporters

[7] Users likely view the report – the number on which they base their action – as a reference point against which they compare the realized state. However, because of the nature of their payoffs, Users experience only neutral changes (when the state equals the report/reference point) and losses (when the state differs from the report, causing their payoff to decrease). That is, when comparing the realized state to the report, they do not experience any gains. As such, there is no apparent role for loss aversion (Kahneman and Tversky 1979) to affect User preferences.

[8] Because we employ an abstract, low-context setting in line with the experimental economics paradigm, we employ a parsimonious design that focuses on the core elements inherent to our setting and we do not operationalize every facet (Davis and Holt 1993). For example, in our conceptualized setting (section 2.3) Reporters and Users engage in economic exchange. We simplify this, so the state of nature is a number and the action is a guess about the number. Further, we simplify the Reporter's payoff (so it is driven by a constant difference between the action and the state of nature) to focus on the core element that the Reporter prefers a greater action than the User.

who produce (H1a) a large overstatement error than an understatement error of equal magnitude (our 'level of error' prediction) and (H1b) an incremental overstatement error than an incremental understatement error (our 'change in error' prediction).

3.3 Aligned treatment: Hypothesis 2

To test hypothesis 2, we utilize a between-subjects design and manipulate the Reporter's incentive to overstate their report.[9] Specifically, we create an *Aligned* treatment in which both parties' payoff is maximized when the action equals the state of nature. The payoffs for the *Aligned* treatment are included in Panel B of Table 1. A comparison of Panels A and B reveals that we merely shift the Reporter's payoff structure by three units across treatments. This represents the only factor that we manipulate between treatments.

We test hypothesis 2 by measuring preferences of Users in both treatments. Hypothesis 2 predicts that Users' apparent dispreference for aggressive reporting is driven by the Reporters' motivation to report aggressively. Following H1, we test H2 via two discrete predictions: that, *ceteris paribus*, Users' dispreference to be paired with Reporters who produce (H2a) a large overstatement error than understatement error of equal magnitude and (H2b) an incremental overstatement than understatement error is larger in the *Misaligned* than the *Aligned* treatment.

3.4 Experimental procedures

The experiment was conducted at a medium-sized private university in a dedicated experimental economics laboratory. A total of 192 participants were recruited from a participant pool consisting primarily of undergraduate students with each being randomly assigned to a

[9] We manipulate only one of the three key characteristics of our setting – the Reporter's motive – as we view the effects of the others to be self-evident. In the absence of measurement uncertainty, *ex post* Users can perfectly infer any bias in the report. In the absence of information asymmetry, the User can plan their action based on the signal itself, so the report is unnecessary. While different *levels* of these parameters may have less-clear effects (e.g., see Abdel-Rahim and Stevens (2018) regarding a reduction in the level of information asymmetry), we leave these considerations for future research.

single session. There were four sessions of both treatments, with each session containing 24 participants. Each session was performed with two 'communities' in the room at the same time, providing a sample of 48 Reporters and 48 Users in each treatment.[10] In all treatments the experiment lasted ten periods and participants were aware of this in advance.[11] While Reporter-User pairs in each community received different stochastically-generated signals and states of nature, we used the same set of signals and states for each treatment so that variation in outcomes is due to variation in participant behavior.

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 *Misaligned* treatment. Participants then answered quiz questions on their computer about the instructions (see Appendix 2 for the quiz for the *Misaligned* treatment), where each question was repeated until the participants selected the correct answer. The experimenter privately answered any questions regarding the experimental procedures. This phase took approximately half an hour. In the second phase each participant was assigned a role (labeled as 'sender' or 'receiver') and remained in that role for the entire experiment. We described the roles and actions to participants using neutral terms to minimize the possibility of subjects making implicit assumptions about behavior (Haynes and Kachelmeier 1998).

[10] The authors' institutions obtained Internal Review Board (IRB) approval for this experiment, which requires that they not use deception.

[11] As Users do not rank Reporters after the tenth period, our hypothesis tests are unaffected by end-game effects. In untabulated results we do find end-game effects in the reports and actions in the *Misaligned* treatment. Specifically, reports in the final period are more aggressive and, anticipating this, Users discount the reports to a greater degree, so the net result is that investment efficiency (i.e., action minus the signal) in the final period is unchanged.

Each participant was paid a $7 participation fee in addition to their payoffs from (i) one randomly selected period, (ii) an incentivized belief elicitation task on Reporter trustworthiness (see section 4.4), and (iii) 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 participants 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). Further, this reporting bias is significantly larger in the *Misaligned* than *Aligned* treatment, thereby validating our manipulation. Users effectively discount most of this bias in their action (see *Action Bias* and *Action Efficiency*). The self-sufficiency option is rarely used in either treatment (under 3% in both Panels A and B); further, no individual User selected self-sufficiency more than twice and no Reporter had the option imposed on them more than twice (untabulated). Reporters out-earn Users in both treatments, and both parties' payoff is maximized when their incentives are aligned (Panel B). We find no significant correlation between the untabulated quiz score, age, gender, risk attitudes, or the SD3 personality trait and these measures of Reporter or User behavior. Regarding risk attitudes, we note recent arguments that raise serious doubts about such measures (Friedman et al. 2014).

[insert Tables 2 and 3 about here]

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 when incentives are aligned Reporters produce slightly fewer overstatement errors (35%) than understatement errors (41%), and that errors are smaller in both magnitude and variance (see |*Error*| × *Neg* and |*Error*| × *Pos*). Further, reports under aligned incentives are both more consistent and accurate than under misaligned incentives (as *Consistency* and *Accuracy* are configured so larger values represent more consistent and accurate reports, respectively).

4.2 Hypothesis 1: Users disprefer Reporters likely to have reported aggressively

4.2.1 Testing hypothesis 1

We test hypothesis 1 by estimating equation (1) for the *Misaligned* treatment:

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

Where *Rank* is the User's ranking position of a Reporter. *Rank* 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. |*Error*| is the absolute value of the current period's reporting error (i.e., the report minus the state of nature). 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. We multiply both values by negative-one so that larger numbers reflect more consistent and accurate reports, respectively. We estimate equation (1) in a random-effects ordered logistic regression – where the User identifies the panels, and each ranking decision (six per period) identifies the trials within panels – with robust errors clustered by User.

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 Users' rankings is larger for a large overstatement error than an understatement error of equal magnitude (H1a), then the difference in fitted values $[(\alpha_1 + \beta_1 \times |Error|) - (\alpha_2 + \beta_2 \times |Error|)]$ will be positive for errors of large magnitude. If the decline in rankings is larger for an incremental overstatement error than an incremental understatement error (H1b), then the difference $\beta_1 – \beta_2$ will be positive.

Note, however, that while Users observe Reporters' full history of reporting errors each period, our model in equation (1) measures the effect of over- and understatement errors for only the most recent period. As such, our empirical tests for H1b will include models that incorporate one and two lagged errors in addition to the default model outlined in equation (1).

4.2.2 Results for hypothesis 1

Results for the estimation of equation (1) are in 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-User pair to control for possible instances where a User 'locks-in' to a Reporter for reasons other than their reporting errors.

[insert Table 4 and Figure 1 about here]

Figure 1 illustrates the effect of each reporting error on rankings and helps to provide intuition for the regression results in Table 4. There appears to be a fixed 'punishment' for understatement errors (coefficient on *Neg* is significantly negative), and the slope on understatement errors appears to be flat (coefficient on |*Error*| × *Neg* is insignificant in all specifications with control variables).[12] On the other hand, there appears to be a large downward slope on overstatement errors (coefficient on |*Error*| × *Pos* is consistently negative and highly significant across all specifications), and the best-fit line appears to provide a slightly positive intercept (coefficient on *Pos* is positive, though not significantly so).

As expected, Table 4 reports that control variables *Consistency* and *Accuracy* (*OthersConsistency* and *OthersAccuracy*) are positive (negative), indicating that Users prefer Reporters who are more consistent (Koonce and Lipe 2010) 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 A of Table 5 provides the estimates for the difference in fitted values of over- and understatement errors of equal magnitude, our test of H1a. 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 (which ranged from –2 to +2), perhaps indicating that Users were able to successfully intuit when Reporters attempted to exploit them via aggressive reporting. This evidence provides clear support for hypothesis 1a that the decline in Users' rankings is larger for a large overstatement than understatement error of equal magnitude.

[insert Table 5 about here]

Panel B provides estimates for β_1–β_2, i.e., our test of H1b. The coefficient of interest is positive and significant for the current period's reporting error in all five columns, as well as the

[12] If there were no factors outside of consistency and accuracy to lead Users to disprefer understatement errors, one would expect the coefficient on the 'fixed effect' *Neg* to also be insignificant. We speculate that it is consistently negative across models because of a recency effect where User assessments of Reporter consistency and accuracy place disproportionate weight on the most recent period's reporting error.

coefficient on the first and second lagged error when included. Table 4 indicates the robust effect of incremental overstatement errors, as the coefficients are significantly negative (at 1%) in all columns and for all lags. This evidence provides clear support in favor of hypothesis 1b that the decline in Users' rankings is larger for an incremental overstatement error than an incremental understatement error.[13] Taken together, our results indicate that Users disprefer Reporters whose reporting errors likely reflect aggressive reporting.

4.3 Hypothesis 2: Users' dispreference for aggressive reporting is driven by Reporters' motives

4.3.1 Testing hypothesis 2

We test hypothesis 2 by estimating equation (2) with data from both the *Misaligned* and *Aligned* treatments.

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

Where *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* treatment. 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. As before, our empirical tests for H2b will include models that incorporate one and two lagged errors in addition to the default model outlined in equation (2). We estimate equation (2) in a random-effects ordered logistic regression with standard errors clustered by User.

[13] In untabulated tests, we measure the effect of socially aversive traits (the Dark Triad) on User preferences. We use a median split to divide the population in the *Misaligned* treatment into low and high SD3 groups and find no significant difference in preferences for over- and understatement errors between groups. Indeed, while socially aversive individuals are less likely to be trustworthy (which would affect the Reporter's actions in our setting), it is less clear how they differ in their perception of others' trustworthiness (Smith 2008, 250-256; Furnham et al. 2013).

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 fitted values $[(\alpha_4 + \beta_3 \times |Error|) – (\alpha_5 + \beta_4 \times |Error|)]$ will be positive for errors of large magnitude.[14] If our prediction from H1b is driven by Reporters' motivation to report aggressively (H2b), then the difference-in-differences in slopes ($\beta_3–\beta_4$) will be positive; this prediction is illustrated in Panel A of Figure 2.

[insert Figure 2 and Table 6 about here]

4.3.2 Results for hypothesis 2

Results for the estimation of equation (2) are in Table 6. 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-User pair. Control variables *Consistency* and *Accuracy* (*OthersConsistency* and *OthersAccuracy*) are positive (negative) in the *Aligned* treatment – consistent with the *Misaligned* results from Table 4 – although there appear to be some differences in magnitude between treatments, as some coefficients on the *Misaligned* interactions are significant.

[14] The difference in fitted values for under- versus overstatement errors in the *Misaligned* versus *Aligned* treatments: $[(\alpha_1+\alpha_3+\alpha_4+\beta_1\times|Error|+\beta_3\times|Error|) – (\alpha_2+\alpha_3+\alpha_5+\beta_2\times|Error|+\beta_4\times|Error|)] – [(\alpha_1+ \beta_1\times|Error|) – (\alpha_2+ \beta_2\times|Error|)] = [(\alpha_4 + \beta_3\times|Error|) – (\alpha_5 + \beta_4\times|Error|)]$. As with H1b, our empirical tests for H2b will include models that incorporate one and two lagged errors in addition to the default model outlined in equation (2).

Panel A of Table 7 provides the estimates for the difference-in-differences in fitted values, our test of H2a. In Panel A of Table 5 we found that the decline in Users' 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. This evidence provides clear support for hypothesis 2a.

[insert Table 7 about here]

Panel B provides estimates for β_3–β_4, identifying whether the difference-in-differences in slopes is significant, our test of H2b. The coefficient of interest is positive and significant for the current period's reporting error in all five columns. The coefficient on the first lag is positive but insignificant in columns (4) and (5), while the coefficient on the second lagged error is significant in column (5). The estimates in column (2) are used to illustrate our results in Panel B of Figure 2, which visually demonstrates that the results conform to our directional predictions. This evidence provides support for hypothesis 2b. Taken together, our results indicate that Users' dispreference for aggressive reporting is driven by Reporters' motivation to report aggressively.

4.4 Robustness test: Measures of trust as alternate dependent variables

After the last period – when Users typically rank Reporters for the next period's pairings – we instead ask all participants (both Users and Reporters) to identify each Reporter as one they either "trust" or "don't trust" (first-order reports). We then ask all participants to select which Reporters the Users said they trust (second-order reports), and we provide a payment for each correct selection. These data allow us to supplement our hypothesis 1 findings on User preferences in the *Misaligned* treatment, which we posit are driven by Users' trust in Reporters, and to confirm the universality of this link via Users' second-order reports.

We analyze the trust dependent variables in the *Misaligned* treatment within logit regressions, using dummy variables for Reporters in the bottom (*Understated*) and top (*Overstated*) quartiles of aggregate errors, as well as control variables *Consistency* and *Accuracy* from our prior analyses; *Understated* Reporters were those with negative aggregate reporting errors. The results (untabulated) indicate that Users' reports on trust are broadly consistent with their period-by-period preference rankings. Results from Users' first-order reports indicate that, *ceteris paribus*, they are significantly more likely to trust an *Understated* than *Overstated* Reporter. Importantly, Users' second-order reports confirm this result, providing some evidence that Users perceive a shared understanding regarding the link between reporting errors and Reporters' underlying intentions in light of their motive to report aggressively.

4.5 Additional robustness tests

We perform additional untabulated tests to demonstrate the robustness of our results (i) across different participant 'communities' and (ii) across periods within the game. First, we re-estimate results and re-perform hypothesis tests after successively removing each community in our sample (a *Misaligned* community for H1a/H1b and an *Aligned* community for H2a/H2b). For H1a [H2a] we re-estimate the value for ±5 in Panel A of Table 5 [Table 7], and for H1b [H2b] we re-estimate the value for column (2) in Panel B of Table 5 [Table 7]. All coefficients for H1a, H1b, and H2a remain significant at the 10% level, while seven of eight coefficients for H2b remain significant at the 10% level (the excepted p-value = 0.135). This indicates that our findings are not driven by behavior within a single outlier community of participants.

We also re-estimate results for column (2) of Table 4 for each successive three-period subsample (i.e., periods 2–4, 3–5, … 7–9). Due to the significant loss of power in these results, each of which are based on only three-eighths of our full *Misaligned* sample, we focus on the most core element of our study: that, *ceteris paribus*, Users disprefer Reporters who produce larger overstatement errors. We find that for all successive three-period subsamples, an incremental overstatement error is associated with a decline in User rankings at a 5% level of statistical significance. Moreover, a review of period-by-period results shows no clear pattern of these results either monotonically strengthening or weakening as the game proceeds, as the coefficient on an incremental overstatement error is negative and significant at a 1% level in periods 3, 4, 5, 8, and 9. This suggests that our findings are not driven by early-period behavior that dissipates over time as participants gain experience in our setting.

4.6 Analysis of responses to post-experimental questionnaire in the Misaligned treatment

We administered a short, non-incentivized survey after completion of the experiment and the incentivized "trust/don't trust" task. We ask all participants about appropriate reporting rules for the Reporter and how they knew whether the rule was followed, as well as about their strategy and whether and why it might have changed during the experiment.[15]

When asked what reporting rule Reporters should follow, Users' responses describe a primary preference for accuracy (e.g., "Report the [signal]"). Reporters' responses are split between promoting accuracy versus aggressive reporting. A few oppose aggressive reporting, with one saying: "Consider that only you will make more money if you report a higher number. Reporting a number similar to the [signal] gives the [User] a better chance at making money."[16]

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[15] To maintain consistency, we replace participants' references to the experimental labels 'Sender' and 'Receiver' with our conceptual labels 'Reporter' and 'User', respectively, e.g., "Sender" will be replaced with "[Reporter]."
[16] This Reporter's comment that "*only you* will make more money" (emphasis added) implies a consideration of total social welfare, as an aggressive report that induces an aggressive action results in 'us' earning less money.

Both Reporters and Users state that they use the reporting error charts to monitor Reporters' rule compliance and read their intentions. A User states: "If the square more often than not hits the 0 it's likely the [Reporter] is using that rule. If it more often than not appears (slightly) above he's trying to squeeze a little bit extra at the expense of the [User]. If it is consistently below the [Reporter] is incompetent." On overstatement errors, one Reporter states "they were probably playing to make the most out of their own profit instead of mutual profit."

When asked about their strategy, some Users describe an aversion to overstatement errors. One User describes that if a Reporter's "reports differed on average over 2 away from the numbers I wouldn't trust them to give me an accurate clue. …I would know they were just trying to make more money for themselves." Another User concisely summarizes their view of trust:

> As described above the [Reporter] with the most and recent "0"s was the most trustworthy. If a [Reporter] had more lowballs I would trust him faster than one with more highballs as it would be less in the favor of the [Reporter] to give lower numbers. In that case I would ascribe it to chance. With highballs it was more likely the [Reporter] tried to get that extra little money.

Lastly, a few Reporters link aggressive reporting to lower rankings from Users, with one claiming "I tried to become more trustworthy by reporting the [signal] to get the bonus" and another changing strategy after reporting aggressively "because people were giving me bad ratings and i (*sic*) was earning less money."

5. Discussion and Conclusion

5.1 Contrasting social norm predictions with economic theory predictions

We follow the recommendations of Brown et al. (2009) and Stevens (2019) and contrast our predictions with those derived from neoclassical economic theory. To do so, we derive the theoretical equilibrium for our game; see Appendix 4. Given our simplifying assumptions, when the Reporter is motivated to report aggressively in the *Misaligned* treatment, the Nash equilibrium is 'babbling' and the Reporter's message contains no information – for example, they randomize their reports. On the other hand, a 'truth-telling' equilibrium can emerge in the *Aligned* treatment. In both cases, Users expect that all Reporters play identical strategies, so they are indifferent between Reporters.[17] As such, the prediction from economic theory is that Users' preferences are unassociated with both reporting errors and the Reporter's motivation, which contrast with hypotheses 1 and 2, respectively. These predictions are unchanged after considering the effect of the payoff adjustments driven by Users' rankings.

Economic theory, therefore, establishes null hypotheses which clearly contrast with our social norm hypotheses. In line with prior research (e.g., see Hobson et al. 2011), because User preferences in the *Misaligned* treatment deviate from their economic self-interest in the hypothesized direction, we infer that it reflects Users' expectation that a social norm applies in that situation.[18] Indeed, because Users in the *Misaligned* treatment are able to effectively discount Reporters' bias, their earnings are similar to those under *Aligned* incentives (see Table 2), further emphasizing that their preferences are driven by something other than economic self-interest. As we fail to find these preferences in the *Aligned* treatment, we infer that it reflects a behavioral preference driven by how Users view Reporters' intentions in light of their motives.

5.2 Identifying a social norm

To place our findings in context, we translate Bicchieri (2006, 11)'s conditions for a social norm to our experimental setting, where we posit that aggressive reporting violates a norm. If so, a necessary initial condition is that enough Users expect that the behavioral rule

[17] Prior experiments show that while Reporters tend to adopt deceptive reporting practices, they transmit more information than predicted (Blume et al. 1998; Forsythe et al. 1999; Dickhaut et al. 2003; Cai and Wang 2006). There is also evidence that some Reporters reject economic incentives to mislead in favor of truth-telling (Sánchez-Pagés and Vorsatz 2007; Hurkens and Kartik 2009; Gneezy et al. 2013; Sheremeta and Shields 2013).

[18] By bringing their sociality into the laboratory and not viewing the interaction as strictly an economic game (Smith 2008), the subjects experience a social welfare gain. In the *Misaligned* treatment, the per-period payoffs predicted by economic theory in Appendix 4 are \$11.15 for the Reporter and \$11.31 for the User, while the actual payoffs (per Panel B of Table 2) are \$14.75 and \$12.55, respectively.

exists and that it applies. A norm would then exist if enough Reporters: (i) know that the rule exists and that it applies, and (ii) prefer to conform to the rule under the condition that (a) they believe enough other Reporters conform and, either (b) they believe enough members in the community expect them to conform, or (c) they believe enough members in the community expect and prefer for them to conform and that Users may sanction them for not doing so.[19]

Our experiment is designed to establish Users' normative expectations – the necessary initial condition above – and likely did not allow for sufficient communication of Users' expectations to Reporters.[20] Participants interacted with up to six different partners for only ten periods in total, and Reporters had to infer all of the Users' preferences from only their own ranking each period, without the benefit of direct communication, and with no knowledge about any prior history of similar interactions. As such, future research may identify an alternate design to test whether our setting satisfies the remaining necessary conditions for a social norm.

5.3 Future research

We offer additional 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). Second, in our setting, neither party's payoff is increasing in the magnitude of the state of nature. As such, a signal greater than (lesser than) the historical average does not reflect 'good' ('bad') news, so we cannot identify whether Users prefer reports that incorporate bad news in the signal more completely than good news (Basu 1997). Future research can extend our

[19] Whereas in conditions (a) and (b) a Reporter will conform due to intrinsic norm-following utility, condition (c) allows a norm to exist where Reporters have no norm-following utility and conform in order to avoid sanctioning.
[20] Results from our incentivized "trust/don't trust" task (section 4.4) indicate a significant mismatch in the parties' beliefs about which actions build trust, as results from Reporters' second-order reports indicate their belief that, *ceteris paribus*, Users are significantly *less likely* to trust an *Understated* Reporter. Further, Reporters wrongly believe that User trust is not significantly associated with *Consistency*. These results are consistent with the notion that our experimental setting did not allow Users to effectively communicate their expectations to Reporters.

findings to a setting where this can be tested. Third, in our robustness tests we find some evidence that Users disprefer overstatement errors as early as period 3 and as late as period 9. Future research might teach us more about the innateness of this preference and whether and how it evolves with experience. Fourth, it would be useful to find empirical evidence from real-world settings to determine whether and how the preferences we observe in social exchange extend to personal and impersonal market exchange (Smith and Wilson 2019). Lastly, future research might provide evidence on the nature of the norm, e.g. whether it contains elements of an honesty (e.g., Douthit and Stevens 2014) or fairness norm (e.g., Bicchieri and Chavez 2010).

5.4 Conclusion

We briefly discuss potential implications from our study. First, we present standard-setters with a view of accounting that reflects rules of conduct designed to restrain unsocial behavior and arbitrate conflicts of interest, rather than rules of measurement designed to accurately communicate the complex results of economic events (Solomons 1986, 243).[21] Along these lines, one key takeaway from our study is that Users incorporate Reporters' motives 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). At a more detailed level, 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).

[21] We attribute this distinction and description to a speech given by FASB member David Mosso, as reported in the January 26, 1978 issue of *FASB Viewpoints*, quoted in Solomons (1986).

Lastly, we provide evidence consistent with a latent demand for conservatism in a setting where the signal contains neither 'good news' nor 'bad news' and in which neither gains nor losses are reported, i.e., in a setting where conditional conservatism cannot exist. We further note that a real-world agent seeking to minimize overstatement of net income would naturally record only highly verifiable gains while recording less verifiable losses. That is, through our 'moral hazard' lens, gains and losses are treated *consistently* under conditional conservatism, as both are treated in the fashion necessary to minimize *ex post* overstatement errors.

Following the influential Basu (1997) study, a large stream of research has focused on the differential treatment of accounting 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 the differential timeliness for gains versus losses that we observe today may be a consequence of humans' normative aversion to overstatement errors that are likely to reflect aggressive reporting. That is, we propose the possibility that conservatism arose not only from an asymmetry between *ex ante* gains and losses, but also from an asymmetry in human reactions to *ex post* overstatement errors and understatement errors.

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APPENDIX 1. Instructions for participants in the *Misaligned* treatment

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.

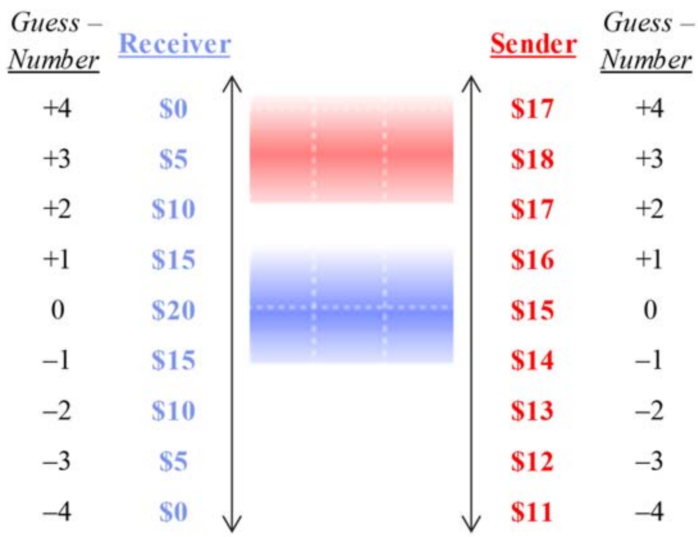
On average, the clue equals the number. The clue can be different from the number, and, when it is different, there is an equal chance it is less than or more than the clue. After observing the clue, the Sender submits a report for the Receiver about the number expected that period. The report is a single 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.

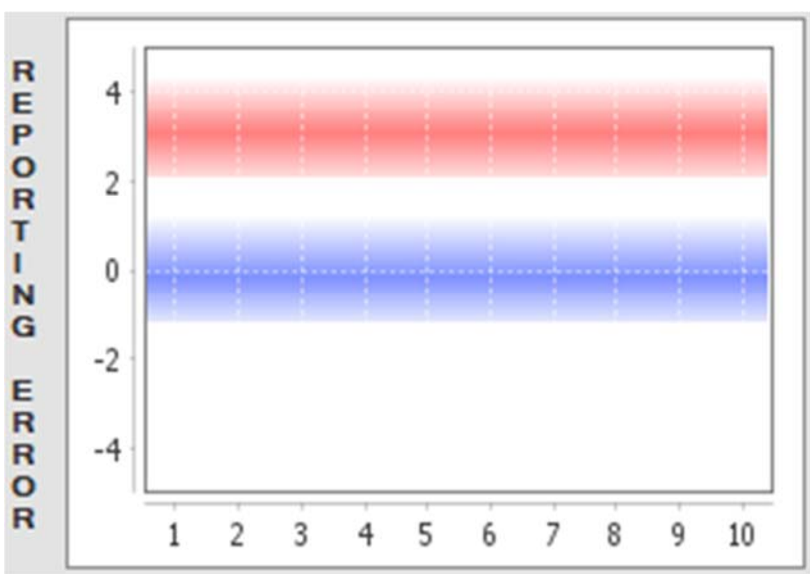


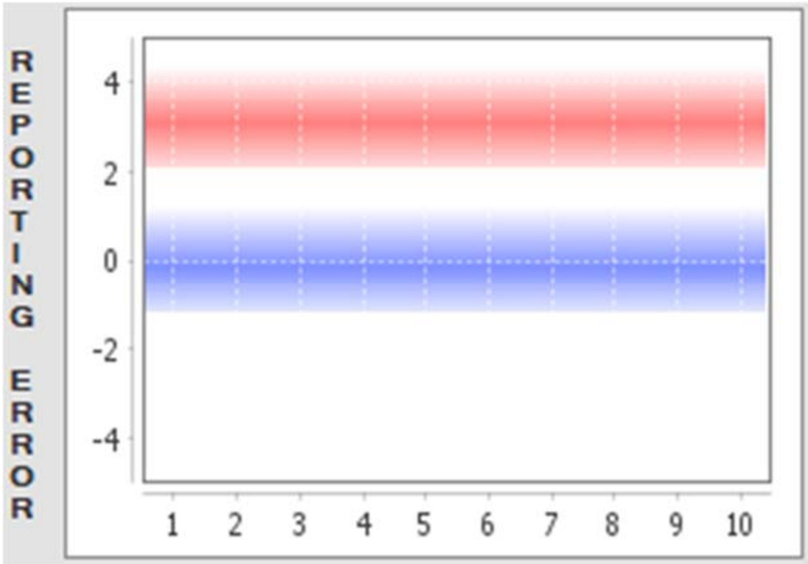
So, for example, if the Receiver's guess was 3 more than the number, the Receiver earns $5 and the Sender earns $18. If the guess was exactly equal to the number, the Receiver earns $20 and the Sender earns $15. If the guess was 3 less than the number, the Receiver earns $5 and the Sender earns $12.

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

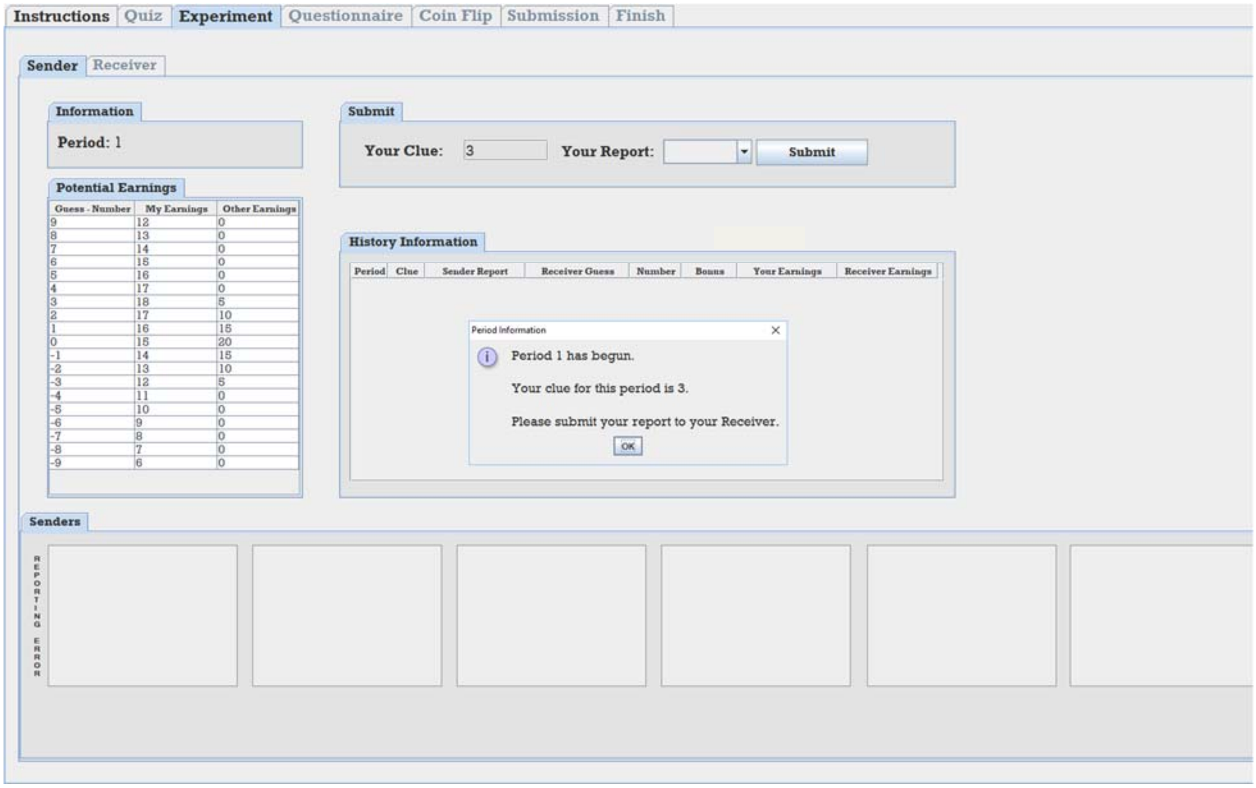
If the Receiver chooses to collaborate in the period:

- 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:

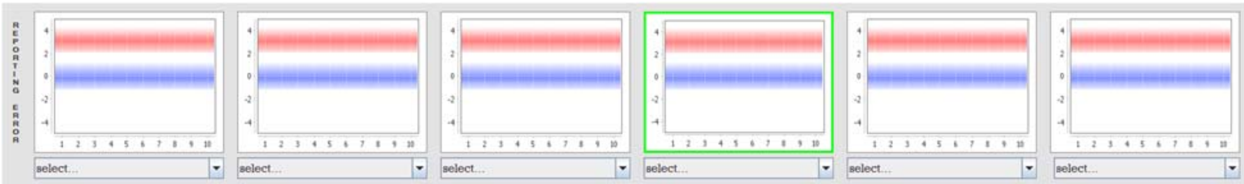


Your private clue about the number that period is shown in the 'Submit' window as "Your Clue."

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.

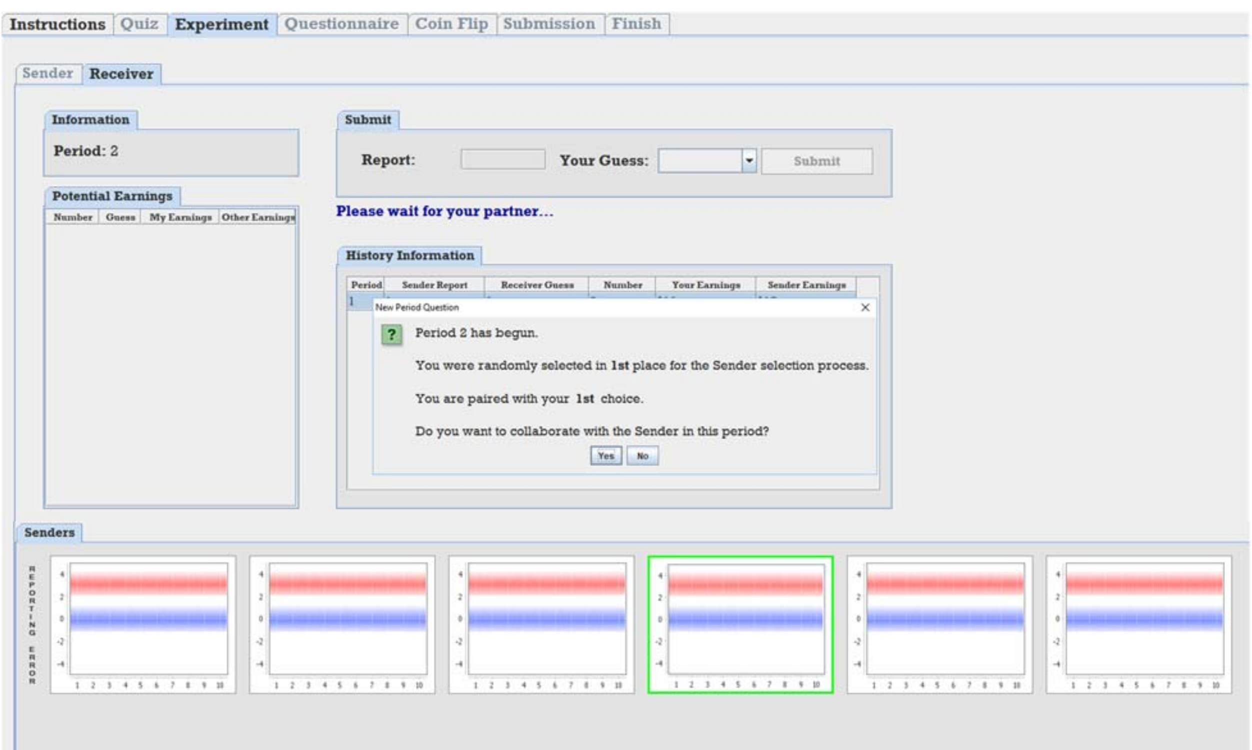
The black squares that indicate reporting errors are omitted in the above image, but they will appear on your screen during the experiment.

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.

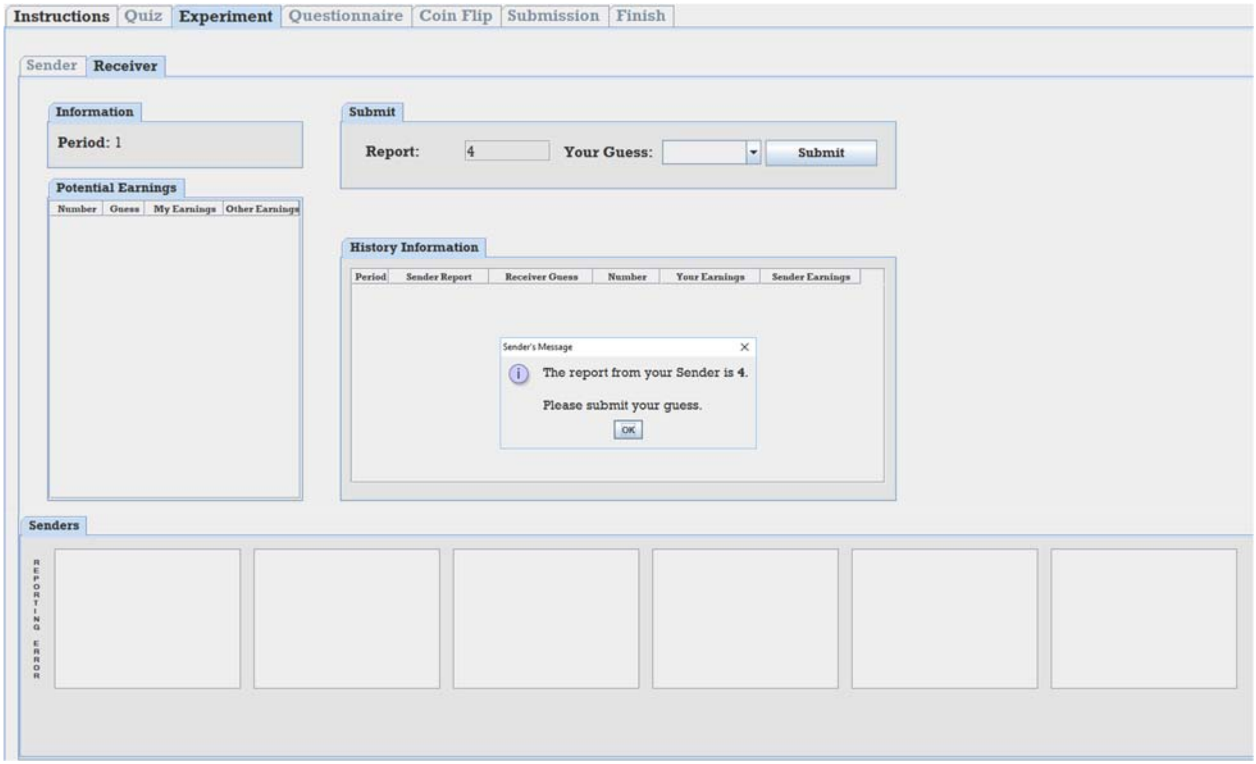
Starting with the second period, once the pairings are announced, each Receiver decides whether to collaborate with their paired Sender. If the Receiver chooses not to collaborate, both the Receiver and Sender earn $5 instead of having the opportunity to earn the amounts listed on the first page. If 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 the *Misaligned* treatment

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's (i.e., Reporter's) payoffs for the quiz provided to participants in the *Aligned* treatment.

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%

APPENDIX 4. Derivation of theoretical equilibrium and alternate predictions

In this Appendix we derive the equilibrium predictions for our game in order to provide formal benchmarks for our hypotheses. We assume that Reporters (senders) and Users (receivers) have no intrinsic utility from norm following.

The setup and payoffs in our experiment closely resemble the analytical model of Crawford and Sobel (1982). In that model, homogenous agents have a common prior on the states of nature, know the signal structure of the Reporter's information, and seek to maximize their own payoffs. When Reporters are not motivated to report aggressively – that is, when there are no conflicts of interest between the action the Reporter and the User desire – truthful and complete disclosure is possible. However, Crawford and Sobel predict that larger conflict of interests lead Reporters to suppress information. Given sufficiently large conflicts of interest, all Nash equilibrium will be in "partition equilibrium" where adjacent private signals are pooled together, masking information, where in the limit Reporters reveal no information. Crawford and Sobel prove there can be multiple equilibria, including a "babbling equilibrium" where Users glean no information from the Reporter's message, and others in which information can be transmitted from the Reporter to the User.

To derive the theoretical equilibrium in our game, we assume all modeled agents are risk neutral, have utility only from their own earnings, and that they have common priors and common knowledge of the states of nature and the Reporter's private signal structure (i.e., the 'noise'). (We employ these latter assumptions for tractability and acknowledge that our game incorporates ambiguity over the states of nature and the Reporter's private signal.) Further, for the time being we will ignore the effect of the payoff adjustments driven by Users' rankings.

In each period, the Reporter's private signal is a discrete integer value $y \sim$ Uniform$\{3,8\}$ and he knows the state of nature $s \in \{y-2, y-1, y, y+1, y+2\}$ with probability $\left\{\frac{2}{14}, \frac{3}{14}, \frac{4}{14}, \frac{3}{14}, \frac{2}{14}\right\}$, respectively. The Reporter reports a value $m \in \{1,2, \dots, 9,10\}$. After seeing the report, the User chooses an action $i \in \{1,2, \dots, 9,10\}$. Thereafter the state of nature, action, and realized payoffs are revealed to both agents. Payoffs are determined as follows:

$$\pi_{Reporter}(i, s, b) = 18 - |i - s - b|$$
$$\pi_{User}(i, s) = max(0, 20 - 5 \times |i - s|)$$

where the bias b represents the conflict of interest between the Reporter and User, is common knowledge, and is manipulated across treatments.

A reporting strategy for the Reporter maps private signals to messages $\alpha: Y \rightarrow M$, and an action strategy for the User maps messages to action $\beta: M \rightarrow I$. The Nash equilibrium is a pair of strategies, denoted as (α^*, β^*), where in expectation each strategy is a best-response to the other. Formally, this means for action strategies and messages used in equilibrium:

$$E[\, \pi_{Reporter}(\beta^*(\alpha^*(y)), s, b)] \geq E\big[\, \pi_{Reporter}(\beta^*(m), s, b)\big] \; \forall \, m \in M \qquad \text{(A1)}$$

$$E[\pi_{User}(\beta^*(m), s)] \geq E[\pi_{User}(i, s)] \; \forall \; i \in I \quad \text{(A2)}$$

The most informative reporting equilibrium strategies, satisfying equations (A1) and (A2) above, are shown in the table below.

Table A1. Nash Equilibrium for different conflicts of interest

Conflicts of interest	Reporting strategies/ Expected Reporter payoff	Action strategies/ Expected User payoff
$b = 0$ *Aligned*	{3}, {4}, {5}, {6}, {7}, {8} $E[\pi_{Reporter}(.)] = \$17$	{3}, {4}, {5}, {6}, {7}, {8} $E[\pi_{User}(.)] = \$15$
$b = 3$ *Misaligned*	{3, 4, 5, 6, 7, 8} $E[\pi_{Reporter}(.)] \cong \$11.15$	{5 or 6} $E[\pi_{User}(.)] \cong \$11.31$

When the conflicts of interest are at $b = 3$, then messages have no information in equilibrium and the User is indifferent between choosing an action of 5 or 6. That is, when Reporters are sufficiently motivated to report aggressively, Users expect that all Reporters are "babbling" and they are indifferent between them. Therefore, their preferences will be unassociated with Reporters' reporting error histories. *As such, for hypothesis 1 economic theory predicts that Users' preferences are unassociated with reporting errors*.

When there are no conflicts of interests ($b = 0$) – that is, where Reporters are not motivated to report aggressively – a truthful reporting strategy can emerge in equilibrium, where the Reporter reveals their private signal $y \in \{3, 4, 5, 6, 7, 8\}$ and the User chooses an action equal to the message value. As Users expect that all Reporters are truth-telling, they are indifferent between Reporters and view the resulting reporting errors as random noise. *As such, for hypothesis 2 economic theory predicts that Users' preferences are unassociated with the Reporters' motivation*.

Lastly, we consider the effect of the payoff adjustments driven by Users' ranking of Reporters. Consider the last period of our game. Since there is no payoff adjustment after the last period, Reporters who may have otherwise moderated their reporting strategy due to the adjustment will revert to maximizing their own payoff. Anticipating this, Users will be indifferent to which Reporter they are paired with in the last period, so the Reporters' history of reporting errors will have no effect on their rankings at the end of the next-to-last period. Knowing that the Users' ranking at the end of the penultimate period is meaningless, Reporters will again revert to maximizing their own payoff in this period, as in expectation the realized reporting errors will have no bearing on their payoff adjustment. Anticipating this, Users will again be indifferent to which Reporter they are paired with in the *next-to-last* period. By backwards induction, this is true of the first period's reporting strategies and the rankings at the end of the first period. That is, to the extent agents are focused on the payoff adjustments, the effect is that throughout the entire game Reporters will seek to maximize their payoffs and Users will be indifferent between Reporters, resulting in preferences that are unassociated with Reporters' reporting error histories. *As such, the predictions derived above are unchanged after considering the possible effect of the payoff adjustments*.

FIGURE 1. Effect of reporting errors in the *Misaligned* treatment on next period's rankings

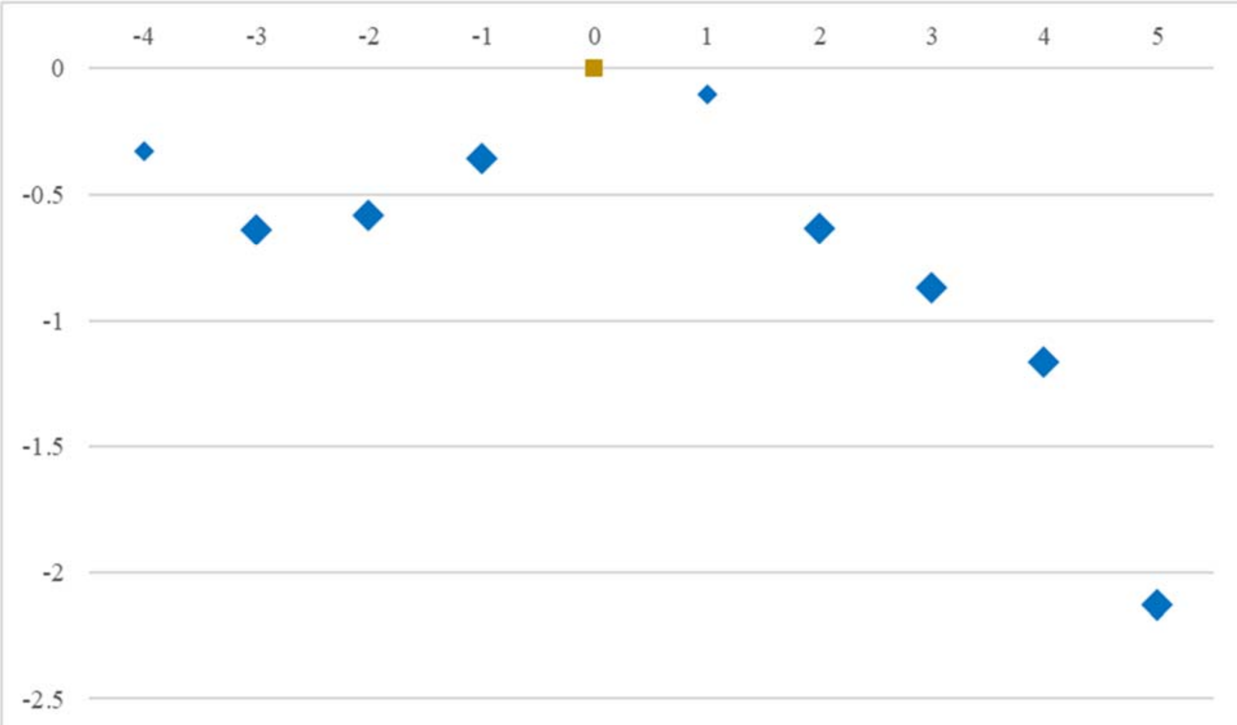
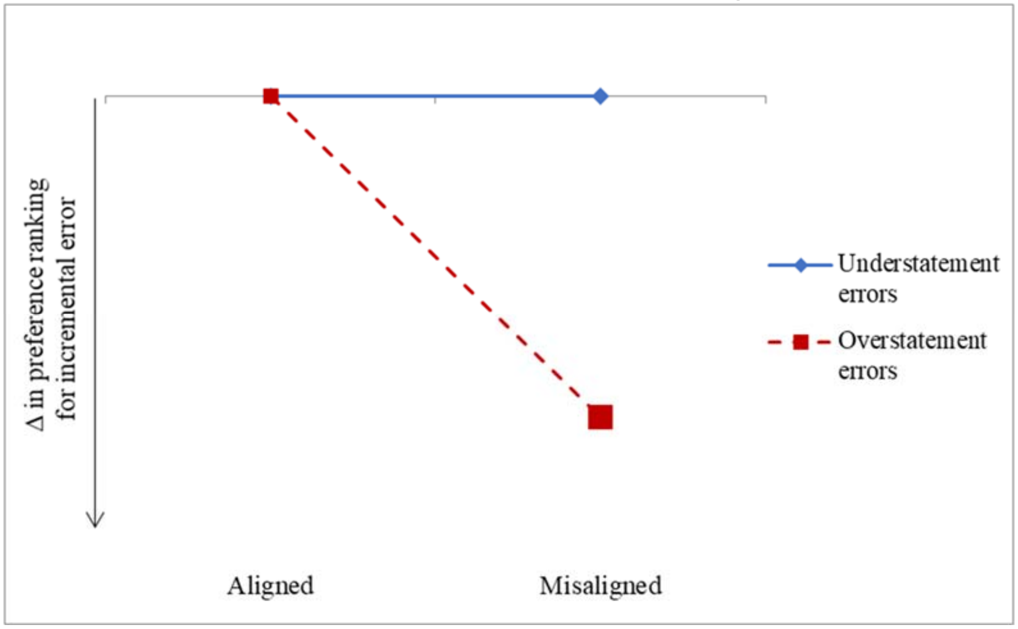


Figure 1 illustrates the *ceteris paribus* effect of a Reporter's reporting errors on their ranking in the next period in the *Misaligned* treatment. The estimates are from a regression of *Rank* on (i) indicator variables for each observed reporting error value (except zero) and (ii) control variables *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 User.

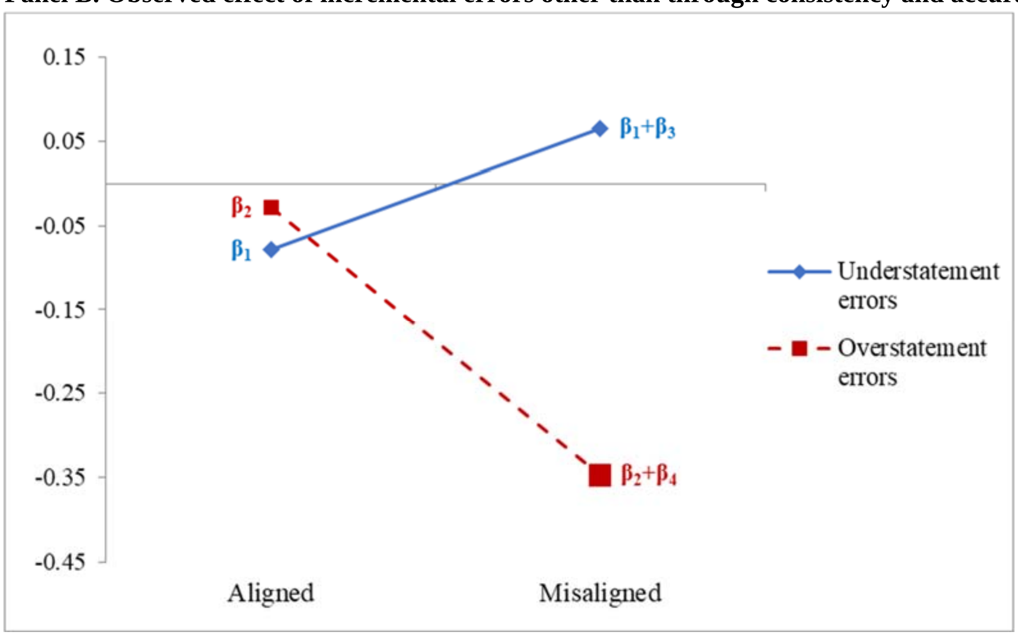
Coefficient estimates that are significantly different from zero at the 5% level are shown with a larger marker. The square at the origin indicates that an error of zero was excluded from the regression, so coefficients for all other errors are measured relative to an error of zero. Only error values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

FIGURE 2. Effect of incremental errors on next period's rankings

Panel A. Predicted effect of incremental errors other than through consistency and accuracy channels



Panel B. Observed effect of incremental errors other than through consistency and accuracy channels



In Figure 2, Panel A illustrates our predictions that the decline in Users' rankings is larger for an incremental overstatement error than an incremental understatement error in the *Misaligned* treatment (H1a), and that this difference is larger than in the *Aligned* treatment (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.

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

TABLE 1. Reporter and User payoffs in the *Misaligned* and *Aligned* treatments

Panel A. Payoffs in the *Misaligned* treatment

Action – State	Reporter	User	Total
9	$12	$ 0	$12
8	$13	$ 0	$13
7	$14	$ 0	$14
6	$15	$ 0	$15
5	$16	$ 0	$16
4	$17	$ 0	$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	$ 0	$11
–5	$10	$ 0	$10
–6	$ 9	$ 0	$ 9
–7	$ 8	$ 0	$ 8
–8	$ 7	$ 0	$ 7
–9	$ 6	$ 0	$ 6

Panel B. Payoffs in the *Aligned* treatment

Action – State	Reporter	User	Total
9	$ 9	$ 0	$ 9
8	$10	$ 0	$10
7	$11	$ 0	$11
6	$12	$ 0	$12
5	$13	$ 0	$13
4	$14	$ 0	$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	$ 0	$14
–5	$13	$ 0	$13
–6	$12	$ 0	$12
–7	$11	$ 0	$11
–8	$10	$ 0	$10
–9	$ 9	$ 0	$ 9

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

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

Panel A. *Misaligned* treatment

Variable	Obs	Mean	SEM	25th Perc	Median	75th Perc
Reporting Bias	48	0.47***	0.13	0.00	0.35	1.40
Action Bias	48	–0.39***	0.08	–0.63	–0.40	–0.15
Action Efficiency	48	0.08	0.07	–0.25	0.00	0.37
Self-Sufficient	48	2.08%	0.66%	–	–	–
Reporter Earnings	48	$14.75***	$0.30	$13.80	$15.00	$16.25
User Earnings	48	$12.55	$0.30	$11.25	$12.75	$13.50

Panel B. *Aligned* treatment

Variable	Obs	Mean	SEM	25th Perc	Median	75th Perc
Reporting Bias	48	0.00	0.04	–0.20	0.00	0.10
Action Bias	48	0.01	0.09	–0.20	0.00	0.32
Action Efficiency	48	0.00	0.08	–0.26	0.00	0.30
Self-Sufficient	48	2.71%	0.77%	–	–	–
Reporter Earnings	48	$16.22	$0.33	$14.25	$16.45	$18.25
User Earnings	48	$12.64	$0.25	$11.50	$12.50	$14.00

Panel A (B) of Table 2 presents summary statistics (mean, standard error of the mean, and quartiles) for the overall game results for the *Misaligned* (*Aligned*) treatment.

Reporting Bias is the average difference between the report and the signal for each Reporter. *Action Bias* (*Action Efficiency*) is the average difference between the action and the report (the average difference between the action and the signal) for each User. *Self-Sufficient* represents the average frequency each User chose not to collaborate, i.e., chose self-sufficiency. *Reporter Earnings* (*User Earnings*) is the average earnings for the Reporter (User) in each period. When the action equals the signal, both parties' expected earnings is $15.00 in the *Misaligned* treatment, while the Reporter's expected earnings rises to $17.00 in the *Aligned* treatment.

Significance levels for differences in means between treatments based on two-tailed p-values using Wilcoxon signed-rank test: (*) 10% level, (**) 5% level, (***) 1% level

TABLE 3. Summary statistics: Reporting characteristics per period

Panel A. Reporting characteristics: *Misaligned* treatment

Variable	Obs	Mean	SEM	25th Perc	Median	75th Perc
Neg	480	0.30	0.02	–	–	–
Pos	480	0.49	0.02	–	–	–
|*Error*| × *Neg*	146	1.86	0.10	1.00	2.00	2.00
|*Error*| × *Pos*	234	2.02	0.08	1.00	2.00	2.00
Consistency	432	–1.29	0.03	–1.56	–1.18	–0.88
Accuracy	480	–1.47	0.04	–1.89	–1.33	–1.00

Panel B. Reporting characteristics: *Aligned* treatment

Variable	Obs	Mean	SEM	25th Perc	Median	75th Perc
Neg	480	0.41	0.02	–	–	–
Pos	480	0.35	0.02	–	–	–
|*Error*| × *Neg*	195	1.71	0.07	1.00	2.00	2.00
|*Error*| × *Pos*	167	1.84	0.09	1.00	2.00	2.00
Consistency	432	–1.20	0.04	–1.50	–1.10	–0.80
Accuracy	480	–1.37	0.04	–1.67	–1.21	–1.00

Panel A (B) of Table 3 presents summary statistics (mean, standard error of the mean, and quartiles) for reporting characteristics for the *Misaligned* (*Aligned*) treatment.

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 3 provides reporting behavior for all ten periods. In order to control for repeated measures within reporters, we test for significant differences between treatments in our regression analysis; see Table 6.

TABLE 4. Revealed User preferences: *Misaligned* treatment

DV = Rank	(1) Periods 1–9	(2) Periods 2–9	(3) Periods 2–9	(4) Periods 2–9	(5) Periods 3–9
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)
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)
Fixed effects?	None	None	Pair	None	None
Total Users	48	48	48	48	48
Total Observations	2,592	2,304	2,304	2,304	2,016
Log pseudolikelihood	–4,362	–3,543	–2,968	–3,532	–3,088
Akaike information criterion	8,743	7,113	5,964	7,099	6,217
Bayesian information criterion	8,795	7,187	6,044	7,197	6,335
Wald chi-square	182.19	125.55	.	179.6	158.36

Table 4 provides the results for the estimation of equation (1). Each observation reflects data on one User'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 User. 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). *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. *OthersConsistency* and *OthersAccuracy* are the average *Consistency* and *Accuracy* of the other five Reporters.

Z-statistics are in brackets underneath each coefficient. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level

TABLE 5. Tests of hypothesis 1: Users disprefer Reporters likely to have reported aggressively

Panel A. Difference between understatement and overstatement errors of equal magnitude

Fitted values from column (2) of Table 4	H1a	±1	±2	±3	±4	±5
$(\alpha_1 + \beta_1 \times \|Error\|) - (\alpha_2 + \beta_2 \times \|Error\|)$	+	–0.33††	0.08	0.48**	0.89***	1.29***
		[5.91]	[0.35]	[5.52]	[8.45]	[9.82]

Panel B. Difference between an incremental understatement and an incremental overstatement error

Differences in coefficients from Table 4	H1b	(1)	(2)	(3)	(4)	(5)
$\|Error\| \times Neg - \|Error\| \times Pos$	+	0.17*	0.41***	0.35***	0.39***	0.43***
		[2.82]	[12.63]	[7.96]	[13.50]	[14.15]
$\|Lag1Error\| \times Lag1Neg -$ $\|Lag1Error\| \times Lag1Pos$	+				0.25***	0.23***
					[8.25]	[6.65]
$\|Lag2Error\| \times Lag2Neg -$ $\|Lag2Error\| \times Lag2Pos$	+					0.34***
						[13.70]

Panels A and B of Table 5 provide results for our tests of hypothesis 1, based on the results for the estimation of equation (1) in Table 4.

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

Panel A provides the fitted values from column (2) of Table 4 for the difference in effect between errors of equal magnitude from ±1 to ±5, shown in each column, on the Reporter's ranking position. Panel B reports the difference between the marginal effect of an incremental understatement error and overstatement error based on estimates from each column of Table 4, as shown. Columns (4) and (5) include one and two additional lagged errors, respectively.

Chi-square statistics are in square brackets underneath each coefficient. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level. In the direction opposite of prediction: (†) 10% level, (††) 5% level, (†††) 1% level

TABLE 6. Revealed User preferences: Both *Misaligned* and *Aligned* treatments

DV = Rank	(1) Periods 1–9	(2) Periods 2–9	(3) Periods 2–9	(4) Periods 2–9	(5) Periods 3–9
Neg	0.88*** (5.76)	–0.14 (–0.82)	–0.11 (–0.59)	–0.13 (–0.77)	–0.34* (–1.86)
Pos	0.03 (0.27)	–0.27 (–1.44)	–0.41** (–2.21)	–0.26 (–1.42)	–0.30* (–1.44)
Misaligned	0.22* (1.83)	–1.28*** (–4.48)	–1.19* (–1.67)	–1.25*** (–4.31)	–1.14*** (–3.05)
Neg × *Misaligned*	–1.35*** (–6.01)	–0.43 (–1.60)	–0.30 (–0.98)	–0.48* (–1.74)	–0.31 (–1.02)
Pos × *Misaligned*	0.19 (0.84)	0.45* (1.78)	0.61** (2.32)	0.43* (1.67)	0.47* (1.66)
|*Error*| × *Neg*	–1.10*** (–11.56)	–0.08 (–0.57)	–0.14 (–0.88)	–0.09 (–0.63)	-0.00 (-0.01)
|*Error*| × *Pos*	–0.71*** (–9.49)	–0.03 (–0.25)	–0.04 (–0.30)	–0.03 (–0.28)	0.00 (0.07)
|*Error*| × *Neg* × *Misaligned*	0.54*** (4.47)	0.14 (0.85)	0.16 (0.85)	0.10 (0.60)	–0.01 (0.08)
|*Error*| × *Pos* × *Misaligned*	–0.02 (–0.24)	–0.32** (–2.26)	–0.29** (–1.98)	–0.35** (–2.46)	–0.43*** (–2.84)
Lag1Neg				–0.20 (–1.14)	–0.18 (–1.07)
Lag1Pos				-0.03 (-0.19)	-0.15 (-0.87)
Lag1Neg × *Misaligned*				-.020 (-0.82)	-0.22 (-0.94)
Lag1Pos × *Misaligned*				0.14 (0.59)	0.27 (1.14)
|*Lag1Error*| × *Lag1Neg*				–0.09 (–0.74)	–0.02 (–0.17)
|*Lag1Error*| × *Lag1Pos*				–0.05 (–0.60)	–0.02 (–0.26)
|*Lag1Error*| × *Lag1Neg* × *Misaligned*				-0.05 (-0.34)	0.02 (0.13)
|*Lag1Error*| × *Lag1Pos* × *Misaligned*				–0.16 (–1.46)	–0.21* (–1.77)
Lag2Neg					0.40* (1.91)
Lag2Pos					0.17 (0.87)
Lag2Neg × *Misaligned*					-0.90*** (-3.44)
Lag2Pos × *Misaligned*					0.02 (0.08)
|*Lag2Error*| × *Lag2Neg*					-0.25** (–1.98)

	(1)	(2)	(3)	(4)	(5)
|Lag2Error| × Lag2Pos					–0.06
					(–0.62)
|Lag2Error| × Lag2Neg × Misaligned					0.37***
					(2.61)
|Lag2Error| × Lag2Pos × Misaligned					–0.15
					(–1.21)
Consistency		1.28***	1.50***	1.28***	1.78***
		(7.62)	(8.62)	(7.62)	(5.34)
Consistency × Misaligned		–0.31	–0.53	–0.31	–0.68
		(–1.10)	(–1.50)	(–1.07)	(–1.49)
Accuracy		1.55***	1.83***	1.54***	1.08***
		(6.20)	(5.08)	(5.65)	(2.59)
Accuracy × Misaligned		–0.59*	0.34	–0.76**	–0.54
		(–1.84)	(0.64)	(–2.14)	(–0.99)
OthersConsistency		–1.00***	–1.67***	–1.01***	–1.26***
		(–4.62)	(–7.52)	(–4.64)	(–3.40)
OthersConsistency × Misaligned		0.22	0.76*	0.21	0.29
		(0.69)	(1.69)	(0.64)	(0.60)
OthersAccuracy		–0.73***	–1.68***	–0.75***	–0.57*
		(–3.56)	(–4.05)	(–3.72)	(–1.85)
OthersAccuracy × Misaligned		–0.50*	–0.97*	–0.48	–0.50
		(–1.66)	(–1.78)	(–1.59)	(–1.15)
Fixed effects?	None	None	Pair	None	None
Total Users	96	96	96	96	96
Total Observations	5,184	4,608	4,608	4,608	4,032
Log pseudolikelihood	–8,739	–6,999	–5,874	–6,986	–6,126
Akaike information criterion	17,506	14,041	11,882	14,032	12,328
Bayesian information criterion	17,597	14,183	12,060	14,225	12,568
Wald chi-square	392.16	335.41	.	444.79	398.84

Table 6 provides the results for the estimation of equation (2). Each observation reflects data on one User'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 User. 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). *Neg* identifies understatement errors, *Pos* identifies overstatement errors, and *|Error|* is the absolute value of the reporting error. *Misaligned* identifies pairings in the *Misaligned* treatment. *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.

Z-statistics are in brackets underneath each coefficient in Panel A. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level

TABLE 7. Tests of hypothesis 2: Users' dispreference for aggressive reporting is driven by Reporters' motives

Panel A. Difference-in-differences: Under- and overstatement errors of equal magnitude by treatment

Fitted values from column (2) of Table 6	H2a	±1	±2	±3	±4	±5
$(\alpha_4 + \beta_3 \times \|Error\|) - (\alpha_5 + \beta_4 \times \|Error\|)$	+	–0.41[††]	0.05	0.51*	0.97**	1.43**
		[6.17]	[0.08]	[2.74]	[4.17]	[4.85]

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

Differences in coefficients from Table 6	H2b	(1)	(2)	(3)	(4)	(5)
|*Error*| × *Neg* × *Misaligned* –	+	0.56***	0.46**	0.46**	0.45**	0.42**
|*Error*| × *Pos* × *Misaligned*		[15.19]	[6.51]	[4.49]	[6.57]	[6.04]
|*Lag1Error*| × *Lag1Neg* × *Misaligned* –	+				0.11	0.23
|*Lag1Error*| × *Lag1Pos* × *Misaligned*					[0.49]	[2.42]
|*Lag2Error*| × *Lag2Neg* × *Misaligned* –	+					0.51***
|*Lag2Error*| × *Lag2Pos* × *Misaligned*						[11.66]

Panels A and B of Table 7 provide the results for our tests of hypothesis 2, based on the results for the estimation of equation (2) in Table 6.

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

Panel A provides the fitted values from column (2) of Table 6 for the difference-in-differences in the effect of errors of equal magnitude from ±1 to ±5, shown in each column, and between the *Misaligned* and *Aligned* treatments, on the Reporter's ranking position. Panel B reports the difference-in-differences in the marginal effect of an incremental understatement error and overstatement error and between the *Misaligned* and *Aligned* treatments, based on estimates from each column of Table 6, as shown. Columns (4) and (5) include one and two additional lagged errors, respectively.

Chi-square statistics are in square brackets underneath each coefficient. Significance levels based on two-tailed p-values: (*) 10% level, (**) 5% level, (***) 1% level. In the direction opposite of prediction: (†) 10% level, (††) 5% level, (†††) 1% level