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Disclosing Conflicts of Interest – Do Experience and Reputation Matter?

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Abstract

In a controlled laboratory experiment, we investigate the effects of disclosing conflicts of interest on the reporting behaviour of information providers. First, we replicate the findings of Cain, Loewenstein, and Moore (2005) that such disclosure can trigger more biased reporting, since it removes moral concerns. Second, we show that this effect diminishes or even reverts with experience and reputation. Third, we observe that non-disclosure can have the positive effect of facilitating the formation of reputation.

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Introduction

The risk of conflicts of interest is present when information providers, who have a binding obligation to tell the truth, simultaneously have incentives to send biased reports to information users. In the context of financial reporting, the relevant information providers include managers and auditors, who jointly produce financial reports (Antle & Nalebuff, 1991), and financial analysts, who process this information. Incentives for biased reporting may arise for managers when bonuses are linked to performance (Watts & Zimmerman, 1978), for auditors when financial incentives are linked to retention (DeAngelo, 1981; Magee & Tseng, 1990), and for financial analysts when contracts are assigned by the covered firm (Cowen, Groysberg & Healy, 2006; Ljungqvist, Marston, Starks, Wei & Yan, 2007).

In the U.S., financial market regulators recently required all relevant groups of financial information providers to disclose financial matters that potentially present conflicts of interest. Managers must disclose their entire compensation package (SEC, 2006); financial analysts must disclose compensation linked to a specific recommendation (SEC, 2003a); and auditors must disclose separately audit fees, audit-related fees, tax fees, and all other fees for past two fiscal years (SEC, 2000; 2003b). The EC Directive of 2006 follows U.S. regulation and requires the disclosure of auditor fees in all member states of the European Union.

Disclosure requirements are part of a multi-pronged approach to ameliorate the effects of conflicts of interest. For example, auditors also have a legal duty to maintain "total independence from the client at all times" *United States v. Arthur Young & Co.*, 465 U.S. 805, 818 (1984), and face litigation risk when they do not comply with rules regarding independence (Palmrose, 1991). Further mechanisms were recently added that limit the joint provision of audit and nonaudit services (SEC, 2000), and require audit committee

preapproval for nonaudit services (SEC, 2003b). The disclosure of auditor fees should complement these approaches by helping investors "evaluate for themselves whether the proportion of fees for audit and nonaudit services causes them to question the auditor's independence" (SEC, 2000). This, in turn, should motivate auditors to stop providing services that raise doubts about their independence.

In our study, we examine the direct effects of disclosing conflicts of interest on the reporting behaviour of information providers. Sender-receiver models suggest positive effects. While information providers have incentives to send biased reports in order to exploit potentially credulous investors when conflicts of interest are not disclosed (Sobel, 1985), they are less motivated to do so when their incentives are disclosed, because information users who are aware of the misalignment of incentives will not trust them (Crawford & Sobel, 1982). In contrast, the moral licensing theory of Cain, Loewenstein and Moore (2005) (hereinafter "CLM") postulates the negative effect of disclosing conflicts of interest. While information providers feel morally bound to accurately report to information users who are unaware of a potential conflict of interest, this moral limit is removed when information users are informed about the conflicts of interest.

Our research question concerns the incremental effects that disclosure of conflicts of interest has on the behaviour of information providers in a scenario where those conflicts are present. This tests the validity of predictions of sender-receiver models and moral licensing theory. Furthermore, we investigate whether the effects of disclosing conflicts of interest change with experience and with reputation by introducing these two factors stepwise. Because experience and reputation are two important characteristics of the audit environment (e.g., Bonner & Lewis, 1990; Wilson Jr. & Grimlund, 1990), we use the typology of auditing.[1]

[1] In our instructions, we labelled auditors as "type A" and investors as "type B". CLM use the denotations "advisor" and "estimator" instead, Crawford and Sobel (1982) use the denotations "sender" and "receiver".

Our experimental design is based on the underlying structure of the Crawford and Sobel (1982) model. This model employs an abstract setting that captures the basic structure of the general relationship between a better-informed information provider (auditor) and a less-informed information user (investor). In our experiment, the auditor has private information and transmits a message to an investor who has the task of estimating the value of a quantity. Both the auditor's payoffs and the investor's payoffs depend solely on the investor's estimates. In all settings, investor payoffs are higher the more accurate his estimates are. In the treatment settings of misaligned incentives, auditors receive higher payoffs when investor estimates are too high. We also construct baseline settings of aligned incentives where auditors face the same payoff structure as investors in order to infer auditor behaviour under partially aligned incentives. We manipulate, first, whether the investor is informed about auditor incentives. Second, we investigate the effects of experience within-subjects by repeating the task for nine periods and giving subjects detailed feedback after each period. Third, the opportunity to establish reputation is manipulated by letting auditors and investors interact either once (no reputation condition) or repeatedly (reputation condition).

Our findings provide new evidence for the validity of the moral licensing theory of CLM, but also highlight experience and reputation as important boundary conditions. First, we replicate CLM's result that auditor reporting is more biased when conflicts of interest are disclosed. In addition, we provide further evidence that this behaviour can be attributed to moral licensing, because auditors self-report to feel guilty in case of misreporting unless their incentives are disclosed. Second, the behaviour of the auditor changes with experience, and auditor reporting is more biased in the no disclosure condition, which is consistent with predictions of sender-receiver models (Crawford & Sobel, 1982). Third, with reputation, no difference in reporting bias occurs, but some evidence that reputation formation only happens in the no disclosure condition is found.

While our study focuses on the direct effects of disclosing conflicts of interest on auditor independence, prior research has focused on indirect effects. As an important indirect effect, disclosure could enhance auditor independence by motivating auditors not to offer activities that potentially induce conflicts of interest.[2] Consistent with this reasoning is the empirical observation that auditors reduced the provision of nonaudit services after disclosure of nonaudit service fees was mandated (Omer, Bedard & Falsetta, 2006). This is supported by the results of an experimental study that found that audit committee members become reluctant to pre-approve nonaudit services when these services have to be disclosed (Gaynor, McDaniel & Neal, 2006). Meanwhile, the effectiveness of the requirement to disclose auditor fees could be undermined by the discretion offered in the application of disclosure rules that allows auditors and audited firms to minimize the amount of nonaudit fees disclosed (Asthana & Krishnan, 2006).

Our study also contributes to experimental economics research in auditing. First, by investigating the incremental effects of reputation in both disclosure conditions, we expand prior research that has identified several other factors as potential impediments to reputation formation, including the amount of costs investors must bear when misreporting occurs (King, 1996), ambiguity of accounting standards (Mayhew, Schatzberg & Sevcik, 2001), timeliness (Mayhew, 2001), and precision of feedback (Schwartz & Young, 2002). Second, we also connect to prior research that has examined the effects of disclosing auditor fees on investor behaviour. Findings are that investors react to this disclosure with a persistent belief that the provision of nonaudit services is associated with impaired auditor independence (Davis & Hollie, 2008; Dopuch, King & Schwartz, 2001).

[2] Empirical research has shown that providing non-audit services negatively affects investors' perception of auditors' independence (e.g., Higgs & Skantz, 2006; Khurana & Raman, 2006; Mishra, Raghunandan & Rama, 2005), but has provided mixed evidence whether it reduces auditors' independence in fact (Frankel, Johnson & Nelson, 2002; Ashbaugh, LaFond & Mayhew, 2003).

The second section explains the theory and develops the hypotheses. The third section describes the experimental settings. The fourth section presents the results. The final section offers conclusions and implications for auditing regulation and addresses limitations.

Theory and hypotheses

Effects of disclosure by level of experience

Crawford and Sobel (1982) model one-time interaction between better-informed auditors and less-informed investors with disclosure of conflicts of interest. In this model, auditors only fully reveal private information to investors when the incentives for both parties are completely aligned. When the incentives are only partly aligned, a partial pooling equilibrium in which auditors send coarse information to investors who rely on this report to some degree is possible. The impact of disclosure on auditor and investor behaviour grows monotonically with the severity of the conflicts of interest: The more misaligned the incentives, the coarser auditor reports and the lower investor reliance becomes. When auditor and investor incentives are completely misaligned, pooling equilibria arise where auditors only send the coarsest information possible to investors who ignore this information. Strategies of coarse reporting include sending the highest possible value or a randomly selected value (Rasmusen, 2007, p. 285).

Sobel (1985) investigates a one-time interaction under the assumption that conflicts of interest are not disclosed. Non-disclosure results in investors forming a belief about the trustworthiness of auditor reporting. The potential presence of investors who believe that auditors are trustworthy provides incentives for auditors to send biased reports in order to exploit investors. Sobel (1985) assumes in his model that half of investors consider auditor reports trustworthy, and that the other half does not. Recent experimental research supports

the assumption that people are often credulous when they have no information about potential conflicts of interest (Gneezy, 2005).

Comparing Crawford and Sobel (1982) and Sobel (1985) yields the following predictions. When incentives are completely misaligned, auditors in the no disclosure condition will always send high values as reports in order to exploit potentially trusting investors, while auditors in the disclosure condition might choose a strategy of randomly selecting a value. Therefore, auditors in the disclosure condition are expected to be less upwardly biased in their reports.

In contrast to these economics-based predictions, CLM argue from a psychological perspective that disclosure of conflicts of interest might have "perverse effects". They postulate that auditors in the no disclosure condition feel morally bound to report in an unbiased manner, because they would consider it unfair to misreport information to investors who are not aware about potential conflicts of interest. Disclosing conflicts of interests to investors would relieve auditors from this moral obligation and would serve as a "moral license" to misreport.

Given these competing predictions from sender-receiver models and moral licensing theory, we distinguish our hypotheses by the level of subject experience. CLM's experiment demonstrates that the effects of moral licensing dominate when subjects cannot acquire experience from feedback. This yields our first hypothesis that auditor reporting will be more biased when auditor incentives are disclosed and subjects are inexperienced (H1). Our second hypothesis is that the effects of disclosure change with experience and that this change is primarily attributable to the no disclosure condition (H2). This is based on recent findings by Gneezy and List (2006) that psychological factors dominate in the beginning, but their impact fades in the long run, where economic factors dominate instead. The dissipation of psychological factors with experience especially affects auditor behaviour in the no disclosure

condition, because they are expected to feel morally bound to correctly report in the beginning. In the disclosure condition, meanwhile, auditors feel morally licensed to misreport from the very beginning – this is why a diminishing influence of psychological factors would have no behavioural consequences.

H1: Without experience, auditor reporting is more upwardly biased in the disclosure condition than in the no disclosure condition, as predicted by moral licensing theory.

H2: With experience, auditor reporting becomes more upwardly biased in the no disclosure condition, while no such effect occurs in the disclosure condition. That is, experience and disclosure show an interaction effect.

Changes in the effects of disclosure with reputation

Without disclosure of conflicts of interest, Sobel (1985) extends the one-time interaction to a repeated game spanning a known number of periods. In this model, investors are not informed about the degree of auditor alignment of incentives, but have an initial belief about it. In the following periods, investors are assumed to adjust their beliefs in accordance with the accuracy of auditor reporting. This adjustment of beliefs allows auditors to establish a reputation for honest reporting. Auditors also have incentives to establish reputation, because it provides the opportunity to exploit investor trust by reporting in a biased manner in a later period. In reality, auditors can also profit from reputation in other ways, e.g., by charging higher fees.

The predictions are different when investors are fully informed about misalignment of incentives among auditors. In this scenario, investors will not form personal beliefs about the degree of misalignment, because they already posses this information. Accordingly, investors will also not update beliefs based on observed reports, and auditors have no opportunity to influence investors' beliefs.

Sobel (1985) provides our third hypothesis that the effects of disclosure depend on opportunities for reputation formation, because we expect that only auditors in the no disclosure condition will attempt to form reputation. The moral licensing theory of CLM (2005) does not contribute to the development of this hypothesis, because it has only been established for environments without opportunities for reputation formation.

H3: With reputation, experienced auditor reports become less upwardly biased in the no disclosure condition, while no such effect occurs in the disclosure condition. That is, disclosure and reputation show an interaction effect.

Experimental method

Task

Our experimental design follows the framework developed by Crawford and Sobel (1982). The auditor receives private information about the value of an asset and sends a report to an investor who has the task of estimating the value of the asset. The underlying scenario in reality is the following. The auditor gathers information about the value of an asset during his audit and issues a report to investors to help them in their investment decisions. The transmission of information from the auditor to the investor can happen both directly via the auditor report and indirectly via proposed audit adjustments and auditor-client negotiations on relevant accounting issues (Antle & Nalebuff, 1991; Gibbins, Salterio & Webb, 2001).

The sender-receiver model of Crawford and Sobel (1982) is a cheap-talk game. Talk is cheap, because auditor reports do not affect any payoffs directly. Instead, both auditor and investor payoffs depend on investor estimates. Direct sanctioning of misreporting is not possible within this framework. Punishment of auditors can happen only indirectly through investor estimates. When sanctions would be introduced into such a model, the equilibrium predicted by sender-receiver models would collapse and the unravelling argument for truth telling would apply (Rasmusen, 2007, p. 286). A scenario without direct sanctions for misreporting maps into auditing when assuming that it is difficult for outsiders to evaluate audit quality (Francis, 2004, p. 352).

Our experimental design is an abstract adaptation of the design used by CLM (see Figure 1 and Table 1). In our implementation of the game, it is common knowledge that the value of a hypothetical asset is randomly drawn from values uniformly distributed between 10.01 and 30.00. The auditor receives private information about the interval in which the true asset value exists. Possible ranges are]10,15],]15,20],]20,25], and]25,30]. This procedure of providing private information is common knowledge. Both the auditor's report and the investor's estimate are point values, and can take any value between 10.01 and 30.00.

--- Insert Figure 1 about here ---

--- Insert Table 1 about here ---

Payoff schemes

In our treatment settings, investors are paid based on the accuracy of their estimates, which is common knowledge. The payoff function is $\Pi_{investor} = \max\{5 - |estimate_{investor} - value|, 0\}$. This means that investors receive the maximum payment of 5 Taler when they estimate the value of the asset correctly. From this maximum payment, the absolute value of the difference

between the estimate and the actual asset value is subtracted. The minimum payment is 0 Taler. This incentive scheme is intended to reflect an environment in which investors benefit from estimating asset values accurately. The auditor's payoff function is $\Pi_{\text{auditor, misaligned}} = \max\{\min\{\text{estimate}_{\text{investor}} - \text{value}, 5\}, 0\}$. The auditor's payment is based on the difference between the investor estimate and the true value of the asset in Taler. They receive the maximum payment of 5 Taler when the investor overestimates the value by 5 units or more, and the minimum payment of 0 Taler when the investor underestimates the value by any amount. This incentive scheme corresponds to an environment in which auditors feel financial pressure to cooperate with the audited company in managing earnings. For these payoff structures, auditor and investor incentives are completely misaligned.[3]

We also construct baseline settings in which auditor and investor incentives are completely aligned: Both investors and auditors have the same payoff function, which is identical to the payoff function of investors in the treatment settings. Therefore, both auditors and investors have incentives to achieve accurate estimates. We expect that auditors will report truthfully and that investors will trust auditors in these settings. For the disclosure conditions, this expectation can be directly derived from Crawford and Sobel (1982). In the no disclosure conditions, investor and auditor behaviour depends critically on investor beliefs concerning the trustworthiness of auditors. If investors trust auditors, and auditors expect this, auditors should report truthfully. The result would be that the behaviour is the same in the no disclosure condition as in the disclosure condition. We expect to observe this outcome,

[3] In the setting of Crawford and Sobel (1982), the auditor has incentives to send noisy, but still informative reports to the investor as long as incentives are partially aligned, and the investor has incentives to make use of this information in his estimates. Such a partial pooling equilibrium arises, because the auditor has incentives to avoid that the investor's estimate is too far away from the true value. Auditor's utility is $0 - (\text{value} - \text{estimate}_{\text{investor}} + b)^2$, where b indicates how much the investor has to overestimate in order that the auditor maximizes her utility. When b becomes too large, only pooling equilibria arise in which the auditor sends uninformative reports which are ignored by the investor. In our setting, auditor's payoff function is $\max\{\min\{\text{estimate}_{\text{investor}} - \text{value}, 5\}, 0\}$. Differences to Crawford and Sobel (1982) are that we assume risk neutrality, pay a base payment of 5, have a lower bound of 0, use a linear instead of a quadratic function, and specify b as 5. One major difference is that auditors' payoffs do not diminish when the investors' estimates are above b (= 5). Therefore, the partial equilibrium is not a solution, since the auditor has no incentives to avoid too high estimates of the investor. Only the pooling equilibria are a solution. This means that incentives are completely misaligned in our setting.

because recent research in economics shows that people trust as a default (e.g., Gneezy, 2005).

Under the assumption that auditor and investor behaviour changes monotonically with variations in the degree of the alignment of incentives, the examination of both settings under a complete misalignment and complete alignment allows for conclusions on behaviour under partially aligned incentives. This assumption is supported by analytical models (Crawford & Sobel, 1982) and experimental studies (Dickhaut, McCabe & Mukherji, 1995).

Treatment variables

Our main manipulation is the disclosure of auditor conflicts of interest to the investor. In the disclosure condition, investors are precisely informed about the incentives of auditors. In the no disclosure condition, the investor receives no information about the auditor's incentive scheme. In both conditions, auditors are aware of the investors' level of information. In reality, the disclosure of auditor fees shall serve as an indicator of auditor conflicts of interest.

We test for the effects of experience by repeating the game for nine periods. After each period, precise feedback about the value of the asset is given to both auditors and investors, which allow both parties to acquire experience. We regard the first period, in which subjects are still inexperienced, as a quasi-replication of CLM (2005) in a more controlled environment. We consider the subjects of CLM (2005) as inexperienced although they participated in a multi-period interaction, because they had no opportunity to acquire experience through feedback.[4] Both inexperienced and experienced subjects are relevant to

[4] CLM subjects' receive in the first periods no feedback and in later periods virtually useless feedback. The feedback was virtually useless to the investor due to the auditor's uncontrolled estimation skills. Investors had the task to estimate the value of a jar of coins without taking a closer look at it. Private information was provided to the auditors by allowing them to closely examine the jar. Since auditors were untrained in estimating the value of jars of coins, they tended to systematically underestimate it. The consequence was that auditors regularly assumed that they were sending reports including estimates higher than true value, even though their advice was actually at or below the true value. This had the effect that investors receiving feedback about the true value often thought erroneously that auditors wanted to provide accurate or downward biased advice. Our design

the auditing context. On one hand, experience acquisition might be possible in circumstances in which audit engagements are regularly renewed. On the other hand, the very limited feedback that auditors receive about the quality of their past actions might make it difficult to acquire relevant experience (Waller & Felix Jr, 1984). Furthermore, the length of auditor-investor interactions is in many cases limited by mandatory or voluntary rotation of auditors (e.g., Dopuch, King & Schwartz, 2003).

Opportunities for reputation are manipulated by the matching of subjects. In the no reputation condition, auditors and investors are rematched after each period and each auditor is ensured to interact with the same investor only once (perfect stranger matching). In the reputation condition, each auditor interacts with the same investor over all nine periods (partner matching). We believe that repeated interaction with the same partner, coupled with precise feedback after each period, provides auditors with the opportunity to build reputation. In contrast to our design, CLM (2005) only investigated a non-perfect stranger matching.[5] Both the reputation and the no reputation conditions are relevant to auditing. On one hand, audit theory emphasises the importance of reputation in auditing, because investors have to rely on reputation because they can rarely directly observe audit quality (Kreps, Milgrom, Roberts & Wilson, 1982; Wilson Jr. & Grimlund, 1990). On the other hand, archival research has often failed to identify reputation concerns as important drivers of audit quality (Khurana & Raman, 2004; Lennox, 1999; Weber, Willenborg & Zhang, 2008; Willenborg, 1999). A recent study shows that even market reactions in response to the failure of Arthur Andersen cannot be clearly attributed to shredded reputation (Nelson, Price & Rountree, 2008).

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circumvents a systematic bias from auditors' homegrown estimation skills by directly inducing auditor information.

[5] This means that the auditor is matched with a random investor in each period without ensuring that the auditor is matched with the same investor twice.

Procedure

The experiment was conducted at the experimental lab of [*deleted in order to ensure a blind review*], working with subjects recruited from the mailing list of the experimental lab. Decision forms were computerised using zTree (Fischbacher, 1999).[6] Altogether, 256 subjects participated in 14 sessions and all completed the session (see Table 2). In each session, 18 to 20 subjects participated, to whom the role of either auditor or investor was randomly assigned in equal proportion. We used review questions to make sure that subjects understood the game and provided a brief post-experimental questionnaire. Each session lasted about 45 minutes, with payoffs for each subject ranging from 2.50 € to 17.50 €. Average payoffs were 10.18 €, which clearly covered the subjects' opportunity costs. To determine payoffs, one of the nine periods was randomly selected in each session. Each earned Taler was converted into 3 Euro.

--- Insert Table 2 about here ---

Statistical modelling and dependent variables

Unless otherwise mentioned, we use general least squares effects modelling with two-tailed tests, except for the replication of CLM, where we use one-tailed tests. Effect coding is used for the categorical treatment variables, because it allows for a meaningful analysis and interpretation of both the main and interaction effects (Pedhazur, 1982, pp. 343 seq.). We control for the individual subject by including it as a random effect in the model.[7] We control for the interval of the asset value by including it as a fixed effect in the model, because the

[6] The program code of the experiment described in this paper is available from the authors upon request.
[7] Roughly speaking, including subject as a random effect implies that for each condition only one mean decision is used for each individual in the statistical tests; more precisely speaking, this has the effect that the variance and the number of degrees of freedoms are adjusted taking into account that the decisions made by the same individual in different periods are not independent.

bias contained in the auditor report and the investor estimate depend on the asset value, by definition.

Our main dependent variable is the reporting bias, which is defined as the difference between the reported value and the expected asset value. The expected asset value is the midpoint of the interval that constitutes the auditor's private information.[8] We also explore investor behaviour by examining the determinants of investor estimate bias. Analogous to the reporting bias, the estimate bias is defined as the difference between the estimate and the expected asset value.

In the post-experimental questionnaire, we collected data for further exploring the validity of the moral licensing theory of CLM (2005). We described a hypothetical scenario in which a type A individual (the auditor) sent an upwardly biased report to a type B individual (the investor) within our experimental scenario. We asked the subjects, using 7-point Likert scales, whether they thought that such behaviour was morally correct and whether they would feel guilty for behaving in the same way.

Results

Baseline settings

First, we establish a baseline of behaviour for aligned incentives. We observe that auditor reporting is truthful and that investor estimates are accurate in all conditions (see Table 3). These findings show that investors trust auditors in both the no disclosure and the disclosure conditions to a similarly high degree, both in the reputation condition (90.7% vs. 86.4%) and the no reputation condition (93.2% vs. 100.0%; untabulated).[9] This is consistent with recent

[8] As the interval of private information is common knowledge, only the intervals of advice and of estimate are decisive when assuming fully rational agents. Under this strong assumption, advice bias and estimate bias would be defined as the difference between the interval of advice and estimate, and the interval of private information. Our results hold qualitatively when defining our dependent variables in that way.

[9] Investors are classified as trusting when their estimates are within the interval of auditors' report.

findings in experimental economics that people who are uninformed about the degree of alignment trust by default (e.g., Gneezy, 2005).

--- Insert Table 3 about here ---

Effects of disclosure without experience and without reputation

After having established the baseline behaviour, we investigate the behaviour in treatment settings where conflicts of interest are present. First, we examine the first period (no experience condition) with perfect stranger matching (no reputation condition) (*setting I*). We expect to replicate CLM's finding that disclosing conflicts of interest will increase auditor reporting bias (H1). Our results confirm the hypothesis, since auditors are significantly more upwardly biased in the disclosure condition than in the no disclosure condition (mean (SE) = 5.17 (1.22) vs. 2.52 (1.67); $p = 0.034$) (see Table 4, Panel A and Table 5). This finding is in line with the moral licensing theory predicting that auditors are morally concerned to send biased information unless their incentives are disclosed. New evidence for the validity of the moral licensing theory is provided by auditors' self-reporting: Auditors in the disclosure condition consider sending upwardly biased reports more often as being morally correct (mean (SE) = 4.72 (0.34) vs. 2.72 (0.52); $p = 0.003$; untabulated) and state that they would feel less guilty about it (mean (SE) = 1.22 (0.10) vs. 2.56 (0.35); $p = 0.002$; untabulated).

Investors informed about the misalignment of incentives are not affected in their estimates by the more biased reporting of auditors in the disclosure condition. Their estimate bias is not significantly different from those of investors in the no disclosure condition (see Table 4, Panel B).

--- Insert Table 4 about here ---

--- Insert Table 5 about here ---

Changes of the effects of disclosure with experience

Second, we include experienced subjects from the condition without opportunities for reputation formation in our analysis (*setting II*). We classify subjects from the second period as experienced, because they receive comprehensive feedback after the first period, which allows them to acquire experience. We hypothesised that experience will change the effects of disclosure on reporting bias (H2). This is supported by a highly significant interaction between disclosure and experience ($p = 0.002$; see Table 5). The interaction between disclosure and experience results in the effects of disclosure reported above for inexperienced subjects reverting with experience. Experienced subjects are marginally significantly less biased in the disclosure condition than in the no disclosure condition (mean (SE) = 4.40 (0.97) vs. 6.28 (0.72); $p = 0.056$; see Table 4, Panel A). This means that the perverse effects of disclosure postulated by CLM cannot be observed for experienced subjects.

We expected that this interaction would be mainly driven by a change in the behaviour of auditors in the no disclosure condition, because moral feelings that influence auditor behaviour in the beginning in this condition will dissipate over time. Consistent with this reasoning, auditors in the no disclosure condition become significantly more biased in their reporting after they have acquired experience (mean (SE) = 2.52 (1.67) vs. 6.28 (0.72); $p < 0.001$; see Table 4, Panel A). In a further analysis, we test whether this change in behaviour can be attributed to a vanishing effect of moral concern. For this purpose, we set up a regression model that includes experience and reported feelings of guilt as independent variables.[10] The direction of the highly significant interaction effect between experience and reported feelings of guilt ($p = 0.011$; untabulated) shows that feeling guilty reduces the

[10] As reported in the method section, the regression analysis also includes subject as a random effect and interval as a control variable.

reporting bias of auditors without experience, but not the reporting bias of auditors with experience.

We expected that the reporting behaviour of auditors in the disclosure condition would not change with experience, because the fading influence of moral factors over time would be irrelevant in this condition where disclosure serves as a moral license for misreporting from the very beginning. The results support our expectations. In the disclosure condition, the reporting bias is not significantly different when comparing the first period to the second through eighth period (mean (SE) = 5.17 (1.22) vs. 4.40 (0.97); $p = 0.209$; see Table 4, Panel A).

We also explore investor behaviour (see Table 4, Panel B). We find that the estimates of experienced investors in the no disclosure condition are upwardly biased (mean (SE) = 2.78 (0.56)), because investors frequently trust the biased reports of their auditors (56%). Experienced investors in the no disclosure condition trust their auditors less often (29%), which results in unbiased estimates (mean (SE) = -0.64 (0.52)). This effect of disclosure on the estimate bias is highly significant ($p < 0.001$).

--- Insert Figure 2 about here ---

--- Insert Figure 3 about here ---

Changes of the effects of disclosure with reputation

Third, we examine the behaviour of experienced subjects in the condition where reputation formation is possible (*setting III*). When comparing the disclosure and no disclosure conditions, one finds that the difference in reporting bias is not significant (mean (SE) = 3.80 (0.88) vs. 4.18 (0.48); $p = 0.768$, see Table 4, Panel A). Furthermore, no significant

differences in auditor self-reported feelings of guilt in case of misreporting appear.[11] These findings imply that the negative effects of disclosure postulated by CLM are not present in environments where reputation formation is possible.

We also investigate our third hypothesis that the availability of opportunities for reputation formation has different effects on the behaviour of experienced subjects in the disclosure and the no disclosure condition (*setting II and III*). The results of a regression analysis shows that the interaction effect between reputation and disclosure is marginally significant (p = 0.088, see Table 5). Analyses of subsamples illustrate this interaction effect. In the no disclosure condition, auditors attempt to establish a reputation for unbiased reporting. They are significantly less upwardly biased in the reputation condition compared to the no reputation condition (mean (SE) = 4.18 (0.48) vs. 6.28 (0.72); p < 0.001; see Table 4, Panel A). Contrary, in the no disclosure condition, no significant effect of the availability of opportunities for reputation formation can be observed (mean (SE) = 3.80 (0.88) vs. 4.40 (0.97); p = 0.582; see Table 4, Panel A). Further evidence for reputation formation in the no disclosure condition, but not in the disclosure condition, can be derived when plotting auditor reports by interval of private information. These plots reveal that patterns of auditor reporting within the reputation condition differ across disclosure conditions (see Figure 3). In contrast to auditors in the disclosure condition, auditors in the no disclosure condition adjust their report in response to private information. This is reflected by a lower absolute value of auditor reporting bias in the no disclosure condition (mean (SE) = 5.03 (0.39) vs. 6.51 (0.49); p = 0.013; untabulated). Furthermore, the interaction between disclosure and private information is highly significant in a regression analysis on the reporting bias ($p < 0.001$, untabulated). For lower intervals of the value of the asset, auditors in the no disclosure condition seem to be

[11] Auditors in the disclosure condition report about the same feelings of guilt as auditors in the no disclosure condition (mean (SE) = 3.00 (0.42) vs. 3.06 (0.41), p = 0.773; untabulated) and evaluate misreporting more often as morally correct (mean (SE) = 2.95 (0.42) vs. 2.28 (0.43); p = 0.004; untabulated).

more concerned about their reputation and are thus less biased in their reporting compared to auditors in the disclosure condition.

Investors reward attempts at reputation formation by auditors in the no disclosure condition by showing with their estimates a higher level of trust in auditor reporting (71% vs. 41%). As a result, investors in the no disclosure condition are more biased (mean (SE) = 2.16 (0.37) vs. -0.67 (0.59), $p = 0.006$; see Table 4, Panel B). However, they also deviate less from the true values of the assets in their estimates, which is reflected by a significantly lower level of the absolute value of estimate bias (mean (SE) = 3.47 (0.32) vs. 5.89 (0.41); $p < 0.001$; untabulated).

Overall regression

We set up a meta-regression analysis to include all data from the treatment settings of misaligned incentives in order to explore determinants of auditor and investor decision-making (see Table 6). We include our treatments as effects, and control for potential path dependence of the reporting bias and estimate.

The overall analysis reveals that disclosure has no significant main effect on reporting bias, but a significant interaction effect with experience and a significant three-way interaction with experience and reputation. This is consistent with our result that the perverse effects of disclosure predicted by the moral licensing theory are restricted to an environment where there are no opportunities to acquire experience or to form reputation.

Significantly positive effects of opportunities for reputation formation are indicated by a significant main effect of this variable. While the two-way interaction between reputation and disclosure is not significant, one can infer from the three-way interaction between reputation, disclosure, and interval that auditors whose incentives are not disclosed make a stronger

attempt to form reputation compared to auditors whose incentives are disclosed, because the auditors in the no disclosure condition are less biased at lower intervals.

Disclosure reduces the bias in investor estimates. Meanwhile, the interaction between disclosure and interval indicates that this main effect is limited to higher intervals of private information, while the effects revert for lower intervals. This effect is even more pronounced in the reputation condition, as indicated by the highly significant three-way interaction between disclosure, reputation, and interval. Overall, these overall results are consistent with subsample results in which investors are generally more biased under no disclosure, but can also profit from non-disclosure by being less biased in their estimates for lower intervals, especially in the reputation condition.

--- Insert Table 6 about here ---

Conclusions

Mandatory disclosure of audit and nonaudit fees has been revived in the U.S. in 2001 (SEC, 2000; 2003b) and has recently been introduced in the European Union (European Union, 2006). CLM conducted an innovative study that experimentally tests for potentially unintended effects of disclosing conflicts of interest on the behaviour of information providers. We advance this research by using a controlled experimental design, and extend it by testing for the effects of experience and reputation – two potentially important characteristics of the audit environment.

The contribution of our study is, first, to show that the "perverse effects" of disclosing conflicts of interest predicted by CLM's moral licensing theory can be replicated in a more controlled experiment. Second, we find evidence for the validity of this theory is restricted to a scenario where there are no opportunities to establish reputation and no opportunities to

acquire experience through feedback. For experienced subjects in the no reputation condition, disclosure even reduces bias. This behaviour is consistent with economic sender-receiver models (Crawford & Sobel, 1982; Sobel, 1985). Third, we observe that only auditors in the no disclosure condition use opportunities to establish reputation for credible reporting. Overall, our findings suggest that while disclosure of conflict of interest can have positive effects in some scenarios, regulators should also be aware of disclosure's potential detrimental effect on auditor moral reasoning and auditor reputation.

Our experimental settings include three main assumptions as limitations. First, the framework of Crawford and Sobel (1982) is a cheap-talk game where auditor reporting has no direct payoff consequences. The downside of this framework is that it does not allow for direct punishment of biased reporting, which could happen in reality, e.g., through private lawsuits or public oversight. We decided to use this framework nevertheless, because it allows for investigation of the incremental effects of disclosing conflicts of interest on auditor behaviour. Furthermore, one could argue that it also often reflects reality, because direct mechanisms for punishing an impairment of auditor independence are difficult to implement. Audit quality is difficult to observe (Francis, 2004, p. 352), and increasing penalties for ensuring independence cannot be unlimited, because it can threaten the sustainability of the auditing profession (Levitt Jr. & Nicolaisen, 2008). These factors also explain why regulators follow a multi-pronged approach for ensuring auditor independence and do not rely solely on sanctions.

Second, we only investigate two extreme cases of complete alignment and complete misalignment of incentives in order to facilitate the development of testable hypotheses and to ensure sufficient statistical power for testing. Critical for generalising the results of our experiment to settings of partially misaligned incentives is the assumption that auditor and investor behaviour change monotonically with varying degrees of alignment of incentives.

This assumption is supported by economic models (Crawford & Sobel, 1982) and experimental evidence (Dickhaut et al., 1995).

A third limitation arises from the experimental design feature that the degree of misalignment of auditor and investor incentives cannot be influenced by the parties and remains constant over time. The reason for this design choice is that we aimed to provide evidence of the incremental direct effect of disclosing conflicts of interest on auditor behaviour. Thus, our experiment does not investigate potentially positive indirect effects of disclosure, e.g., discouraging auditors to undertake activities associated with conflicts of interest.

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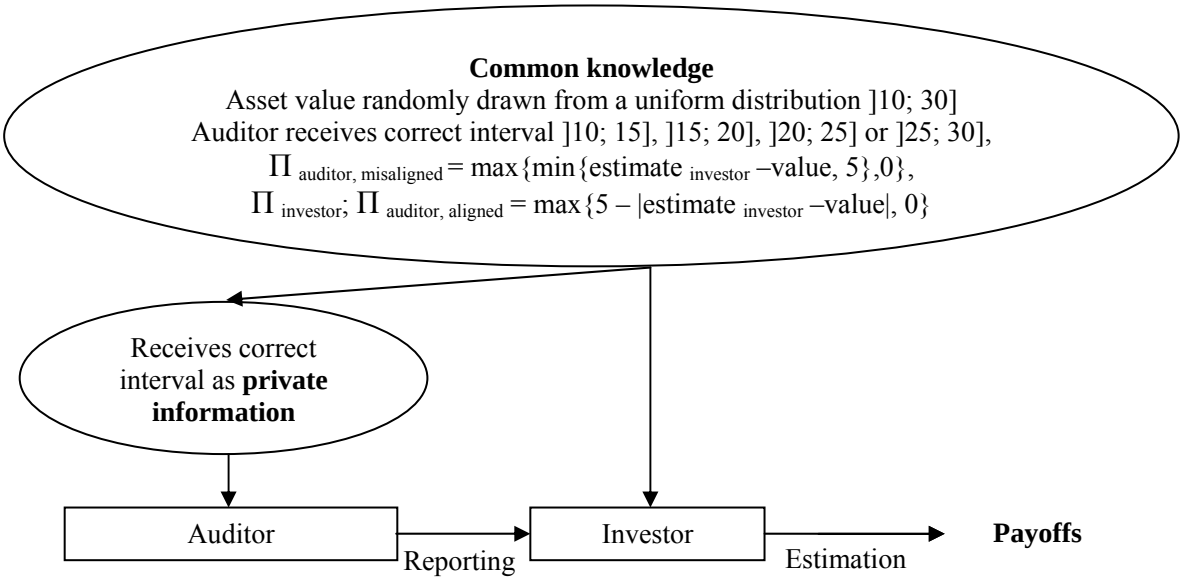
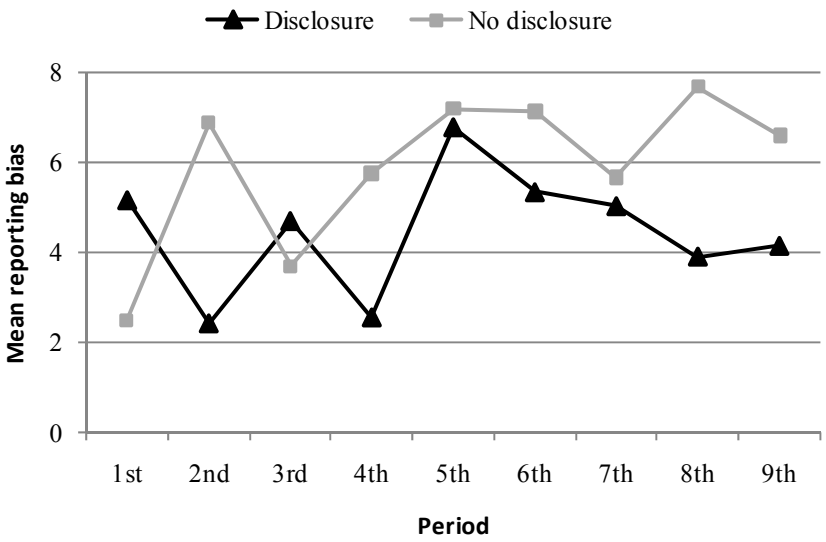
Figure 1
Auditor-investor game



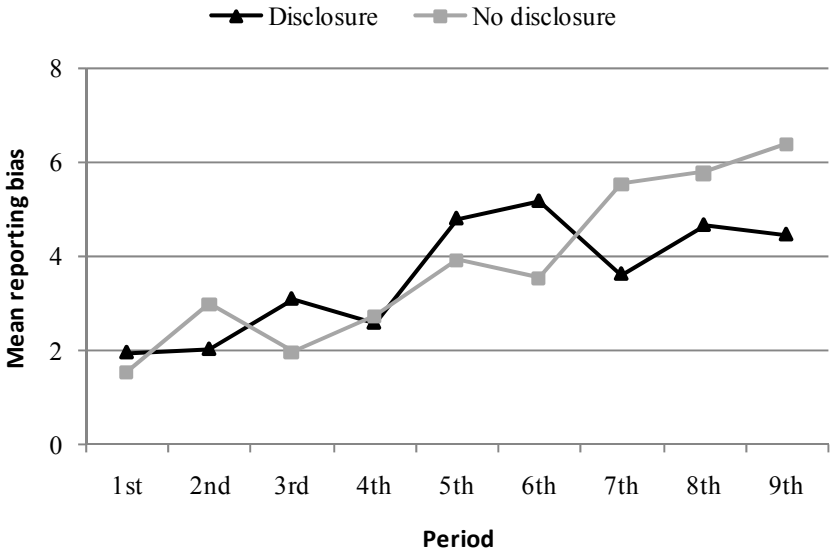
This figure illustrates the disclosure condition of the auditor-investor game. In the no disclosure condition, the investor is not informed about auditor's payoff function ($\Pi_{auditor}$).

Figure 2
Auditors' reporting bias by period (treatment settings)

Panel A: No reputation condition

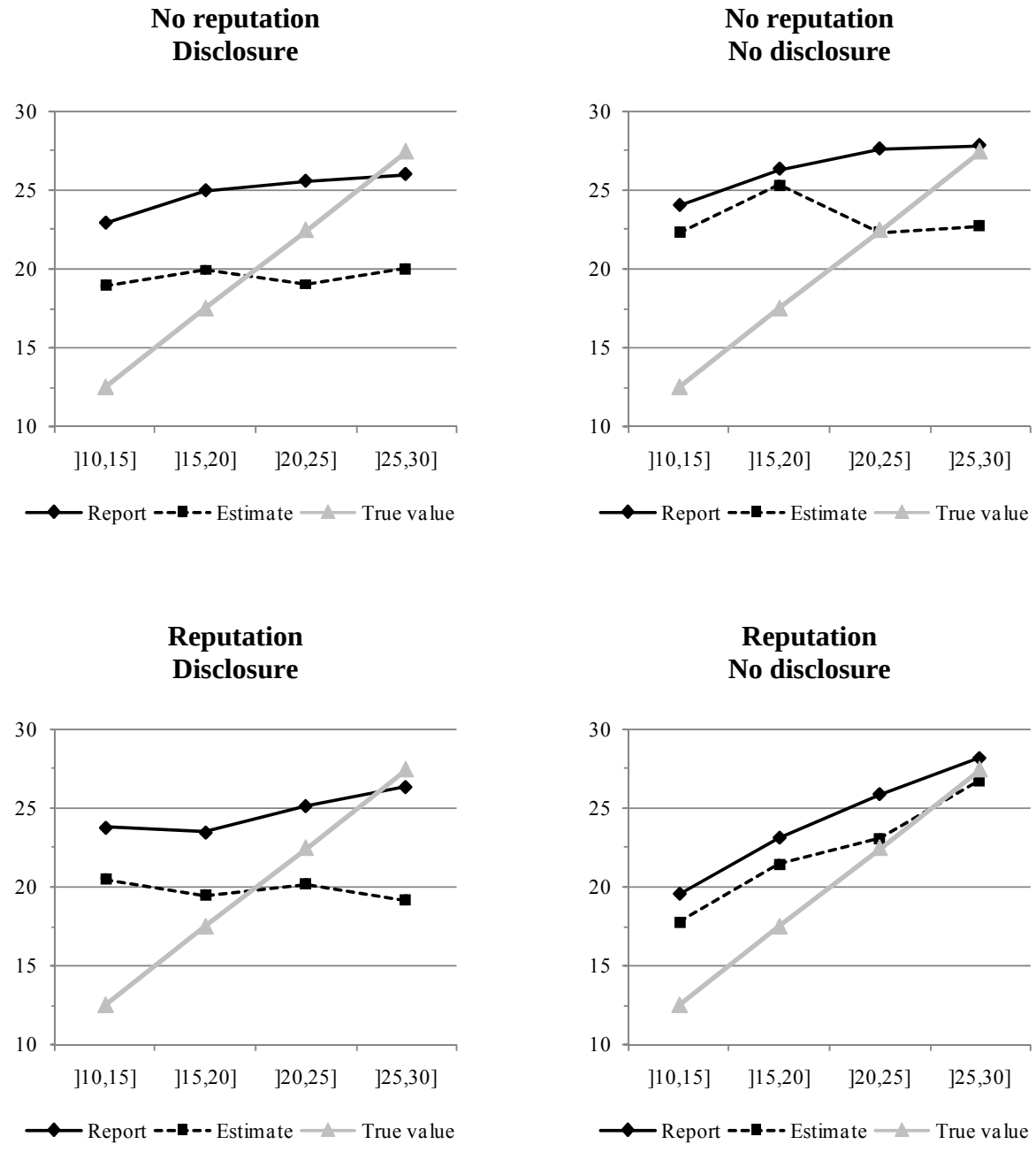


Panel B: Reputation condition



Aggregated descriptive and test statistics are reported in Table 4, Panel A.
Panel A of this figure (no reputation setting) shows that the reporting bias is higher in the disclosure condition in the first period, but is generally lower in the following periods.
Panel B of this figure (reputation setting) shows that the reporting bias is about the same in both disclosure conditions.

Figure 3
Mean values of auditors' reports and investors' estimates by auditors' private information (treatment settings)



The figures show the behaviour of experienced subjects (2nd to 8th period) by reputation and disclosure condition. 'True value' is defined as the middle of the interval which the auditor receives as private information.

Table 1
Comparison of the experimental design of this study with CLM

	Cain, Loewenstein, and Moore (2005)	**This study**
Investors' task	Estimating the value of a jar of coins.	Estimating the value of an asset.
Determination of the asset value	Six jars with values ranging from \$10 to \$30.	Random value from a uniform distribution ranging from 10.01 to 30.00.
Auditor's payoff function (treatment settings)	$\Pi_{\text{auditor, misaligned}}$ [in \$] = $\begin{cases} 0 & \text{estimate - value} < 0.5 \\ 1.00 & 0.5 \le \text{estimate - value} \le 1 \\ 1.90 & 1.01 \le \text{estimate - value} \le 1.50 \\ \dots & \dots \\ 5.50 & \text{estimate - value} \ge 5.01 \end{cases}$	$\Pi_{\text{auditor, misaligned}}$ [in Taler, 1 Taler = 3 €] = $\max\{\min\{\text{estimate–value}, 5\},0\}$
Investor's payoff function (all settings) & Auditors' payoff function (baseline settings)	$\Pi_{\text{investor}} = \Pi_{\text{auditor, aligned}}$ [in \$] = $\begin{cases} 5.00 & \lvert\text{estimate - value}\rvert \le 0.50 \\ 4.50 & 0.51 < \lvert\text{estimate - value}\rvert \le 1.00 \\ 4.00 & 1.01 < \lvert\text{estimate - value}\rvert \le 1.50 \\ \dots & \dots \\ 0.00 & \lvert\text{estimate - value}\rvert \ge 5.01 \end{cases}$	$\Pi_{\text{investor}} = \Pi_{\text{auditor, aligned}}$ [in Taler, 1 Taler = 3 €] = $\max\{5 - \lvert\text{estimate–value}\rvert, 0\}$
Review questions	No	Yes
Disclosure of conflicts of interest	„Auditor is paid on how high you estimate".	Detailed information about auditors' incentive scheme; also checked in review questions.
Auditors' information	Opportunity to observe closely the jar of coins.	Information about the correct interval of the asset value:]10,15];]15,20];]20,25];]25,30].
Investors' information	Opportunity to observe from a distance the jar of coins.	Information about the range and distribution of possible asset values]10,30]; information that auditors receive true interval.
Matching of auditors and investors	Shuffling of advice forms; 6 periods with 3-5 auditors.	Randomized matching by computer: I. 9 periods perfect stranger matching. II. 9 periods partner matching.
Feedback	No feedback in periods 1-3. Very limited feedback in periods 4-6.	Precise feedback about the value of the asset and the realized payoffs after each period.

Table 2
Experimental settings

Panel A: Baseline settings

Treatment	**No reputation**	**Reputation**
Disclosure	1 sessions = 18 subjects	1 sessions = 18 subjects
No disclosure	2 sessions = 36 subjects	2 sessions = 36 subjects

Panel B: Treatment settings

Settings	**(I)**	**(II)**	**(III)**
Treatment	**No reputation No experience**	**No reputation Experience**	**Reputation Experience**
Disclosure	2 sessions = 36 subjects		2 sessions = 40 subjects
No disclosure	2 sessions = 36 subjects		2 sessions = 36 subjects

Settings: **Baseline settings:** Auditors and investors have incentives that investors estimate accurately. **Treatment settings:** Investors have incentives to be accurate in their estimates; auditors have incentives that investors are upwardly biased in their estimates.

Treatments: **Disclosure** (categorical) = 1 for disclosure, -1 for no disclosure of auditors' incentives. **Experience** (categorical) = 1 for experience (2^{nd}-8^{th} period),-1 for no experience (1^{st} period). **Reputation** (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).

Table 3
Descriptive and test statistics on main dependent variables (baseline settings)

Panel A: Auditors' reporting bias

Treatments	**No reputation**	**Reputation**
	Mean (SE)	Mean (SE)
Disclosure	0.03 (0.08)	-0.10 (0.12)
No disclosure	0.15 (0.10)	0.15 (0.22)
p-value (Disclosure vs. no disclosure)	n.s.	n.s.

Panel B: Investors' estimate bias

Treatments	**No reputation**	**Reputation**
	Mean (SE)	Mean (SE)
Disclosure	-0.01 (0.04)	0.13 (0.14)
No disclosure	0.14 (0.12)	0.19 (0.16)
p-value (Disclosure vs. no disclosure)	n.s.	n.s.

Model:	General Least Squares (GLS) effects model with two-tailed tests
Dependent var.:	**Reporting bias** = auditors' report – middle of the interval of auditors' private information. **Estimate bias** = investors' estimate – middle of the interval of auditors' private information.
Treatments:	**Disclosure** (categorical) = 1 for disclosure, -1 for no disclosure of auditors' incentives. **Reputation** (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).
Control var.: (not reported)	**Interval** (ordinal) = interval of the asset value which the auditor receives as private information, 1 =]10,15], 2 =]15,20], 3 =]20,25], 4 =]25,30]. **Subject** (random effect)

Table 4
Descriptive and test statistics on main dependent variables (treatment settings)

Panel A: Auditors' reporting bias

Settings **Treatments**	**(I)** **No rep.** **No exp.**	**(II)** **No rep.** **Exp.**	**(III)** **Rep.** **Exp.**	**(I & II)** **No rep.** **No exp & Exp.**	**(II & III)** **No rep. & Rep.** **Exp.**
	Mean (SE)	Mean (SE)	Mean (SE)	p-value (No exp. vs. exp.)	p-value (No rep. vs. rep.)
Disclosure	5.17 (1.22)	4.40 (0.97)	3.80 (0.88)	n.s.	n.s.
No disclosure	2.52 (1.67)	6.28 (0.72)	4.18 (0.48)	<0.001	<0.001
p-value (Discl. vs. no discl.)	0.034^{+}	0.056	n.s.		

Panel B: Investors' estimate bias

Settings **Treatments**	**(I)** **No rep.** **No exp.**	**(II)** **No rep.** **Exp.**	**(III)** **Rep.** **Exp.**
	Mean (SE)	Mean (SE)	Mean (SE)
Disclosure	-1.31 (1.41)	-0.64 (0.52)	-0.67 (0.59)
No disclosure	1.53 (1.68)	2.78 (0.56)	2.16 (0.37)
p-value (Discl. vs. no discl.)	n.s.	<0.001	0.006

Model:	General Least Squares (GLS) effects model with two-tailed tests, $^{+}$one-tailed test
Dependent var.:	**Reporting bias** = auditors' report – middle of the interval of auditors' private information. **Estimate bias** = investors' estimate – middle of the interval of auditors' private information.
Treatments:	**Disclosure** (categorical) = 1 for disclosure of auditors' incentives, -1 for no disclosure. **Experience** (categorical) = 1 for experience (2nd-8th period), -1 for no experience (1st period). **Reputation** (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).
Control var.: (not reported)	**Interval** (ordinal) = interval of the asset value which the auditor receives as private information, 1 =]10,15], 2 =]15,20], 3 =]20,25], 4 =]25,30]. **Subject** (random effect)

Table 5
Regression analysis of subsamples on auditors' reporting bias (treatment settings)

Settings	**(I)**	**(I & II)**	**(II & III)**
Treatments	**No reputation** **No experience**	**No reputation** **No experience & Experience**	**No reputation & Reputation** **Experience**
	Mean (SE) [p-value]	Mean (SE) [p-value]	Mean (SE) [p-value]
Disclosure	**1.71** **(0.90)** **[0.034]**$^{+}$	0.23 (0.52) [n.s.]	-0.32 (0.34) [n.s.]
Experience		0.47 (0.36) [n.s.]	
Disclosure * Experience		**-1.13** **(0.36)** **[0.002]**	
Reputation			-0.87 (0.34) [0.012]
Reputation * Disclosure			**0.59** **(0.34)** **[0.088]**
Adjusted R^2	0.335	0.585	0.588
Observations	36	288	518
Subjects	36	36	74

Model:	General Least Squares (GLS) effects model with two-tailed tests; $^{+}$one-tailed test. The effects that test the respective hypothesis are in bold.
Dependent var.:	**Reporting bias** = auditors' report – middle of the interval of auditors' private information.
Treatments:	**Disclosure** (categorical) = 1 for disclosure of auditors' incentives, -1 for no disclosure. **Experience** (categorical) = 1 for experience (2^{nd}-8^{th} period), -1 for no experience (1^{st} period). **Reputation** (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).
Control var.: (not reported)	**Interval** (ordinal) = interval of the asset value which the auditor receives as private information, 1 =]10,15], 2 =]15,20], 3 =]20,25], 4 =]25,30]. **Subject** (random effect)

Table 6
Overall regression analysis for auditors' reports and investors' estimates (treatment setting)

Dependent variable	**Reporting bias**	**Estimate bias**
Coefficients	Mean (SE)	Mean (SE)
Intercept	3.82 *** (0.38)	1.09 *** (0.34)
Interval	-2.78 *** (0.24)	-3.65 *** (0.25)
Disclosure	0.23 (0.38)	-1.26 *** (0.34)
Experience	0.85 *** (0.26)	0.18 (0.27)
Reputation	-0.97 ** (0.38)	-0.12 (0.34)
Disclosure * Interval	-0.58 *** (0.16)	-0.66 *** (0.16)
Disclosure * Experience	-0.59 ** (0.26)	- 0.30 (0.27)
Disclosure * Reputation	-0.07 (0.38)	0.28 (0.34)
Experience * Interval	-0.68 *** (0.24)	-0.71 *** (0.25)
Experience * Reputation	0.27 (0.26)	-0.06 (0.27)
Reputation * Interval	0.53 *** (0.16)	0.58 *** (0.16)
Disclosure * Experience * Reputation	0.55 ** (0.26)	0.08 (0.27)
Disclosure * Reputation * Interval	-0.33** (0.16)	-0.62*** (0.16)
Estimate bias$_{-1}$	0.02 (0.03)	
Reporting bias$_{-1}$		-0.13 *** (0.03)
Adjusted R^2	0.587	0.623
Observations	666	666
Subjects	74	74

Model: General Least Squares (GLS) effects model with two-tailed tests; $^{+}$one-tailed test.
*(**)[***] significant at the 10% (5%) [1%]-level

Dependent var.: **Reporting bias** = auditors' report – middle of the interval of auditors' private information.
Estimate bias = investors' estimate – middle of the interval of auditors' private information.

Treatments: **Disclosure** (categorical) = 1 for disclosure of auditors' incentives, -1 for no disclosure.
Experience (categorical) = 1 for experience (2^{nd}-9^{th} period), -1 for no experience (1^{st} period).
Reputation (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).

Control var.: **Estimate bias**$_{-1}$ = Bias of the investor in the prior period with whom the auditor was matched.
Reporting bias$_{-1}$ = Bias of the auditor in the prior period with whom the investor was matched-
Interval (continuous) = interval of the asset value which the auditor receives as private information, 1 =]10,15], 2 =]15,20], 3 =]20,25], 4 =]25,30]; centred around 0.
Subject (random effect)

Appendix A. Summary table for the reported regression analyses (treatment setting)

Panel A: Regression analyses on the reporting bias (details for Table 4, Panel A)

Settings	**(I)**	**(II)**	**(III)**
Treatments	**No rep. No exp.**	**No rep. Exp.**	**Rep. Exp.**
Disclosure	0.034[+]	0.056	0.652
Interval	0.002	<0.001	<0.001
Adjusted R^2	0.335	0.679	0.482
Observations	36	252	266
Subjects	36	36	38

[+] one-sided test

Panel B: Regression analyses on the estimate bias (details for Table 4, Panel B)

Settings	**(I)**	**(II)**	**(III)**
Treatments	**No rep. No exp.**	**No rep. Exp.**	**Rep. Exp.**
Disclosure	0.254	<0.001	0.006
Interval	<0.001	<0.001	<0.001
Adjusted R^2	0.566	0.706	0.460
Observations	36	252	266
Subjects	36	36	38

Panel C: Further regression analyses on the reporting bias (reported in the text)

Settings	**(I & II)**	**(III)**
Treatments	**No rep. No exp. & Exp.**	**Rep. Exp.**
Disclosure		0.008
Experience	0.197	
Guilt	0.302	
Guilt * Experience	0.010	
Disclosure * Interval		0.007
Interval	<0.001	<0.001
Adjusted R^2	0.646	0.488
Observations	288	266
Subjects	36	38

These tables show the p-values for the variables of the regression analyses reported in the result section.

Model: General Least Squares (GLS) effects model with two-tailed tests; [+]one-tailed test.

Dependent var.: **Reporting bias** = auditors' report – middle of the interval of auditors' private information.
Estimate bias = investors' estimate – middle of the interval of auditors' private information.

Treatments: **Disclosure** (categorical) = 1 for disclosure of auditors' incentives, -1 for no disclosure.
Experience (categorical) = 1 for experience (2^{nd}-8^{th} period), -1 for no experience (1^{st} period).
Reputation (categorical) = 1 for reputation (partner matching), -1 for no reputation (stranger matching).
Guilt (categorical) = 1 if reported feeling of guilt >=4 (on a 7-point Likert scale), -1 otherwise

Control var.: **Interval** (ordinal) = interval of the value of the asset which the auditor receives as private information, 1 =]10,15], 2 =]15,20], 3 =]20,25], 4 =]25,30]
Subject (random effect)

Appendix B. Research instrument

Instructions

Welcome to our experiment. Please read the following two pages of the instructions carefully. [Disclosure: Instructions are for all participants the same.] [No disclosure: Instructions are different for participant of type A and B.] During the whole experiment we ask you to stay quiet and not to talk to your neighbour. Please turn off your mobile phone. In the case that you have any questions please raise your hand and one of the experimenters will come to you.

In the experiment you can earn money. The amount you are going to earn depends on your own decisions and on the decisions of the other participants.

Altogether, there are 18 subjects participating in this session. 9 subjects decide as type A and 9 subjects decide as type B. If you decide as type A or as type B was decided by the random draw of the seat. During the whole experiment you are of type [Type A: A][Type B:B]

The experiment lasts 9 periods. In each period, one type A player interacts with one type B player. During the 9 periods you interact [Reputation: always with the same participant.] [Experience: with no participant a second time.]

Order of play

At the beginning of each round one of the intervals [10.01,15.00], [15.01,20.00], [20.01,25.00], [25.01,30.00] will be drawn. The value of the good will be drawn from the selected interval. All values in this interval are equally likely to occur. Thus, the value of the good can be in the range of 10.01 to 30.00 Taler.

A receives the information of the selected interval. B does not receive this information.

A then gives an advice about the value of the good to B. The advice will contain a number in the interval 10.01 to 30.00 Taler.

B receives the advice from A. B is supposed to provide an estimation of the value of the good. The estimate consists of a number in the interval 10.01 to 30.00 Taler.

Meanwhile A answers the following question: What is your expectation of B's estimate?

When finished, A and B will receive feedback on the value of the good and their own payoff. [Disclosure: and the payoff of the other player.]

Payoff

Each subject receives a show-up fee of 2.50 Euro. The show-up fee does not depend on the decisions during the experiment.

At the end of the experiment the computer will draw one of the 9 rounds randomly. This round will be paid off for all subjects in this session. Amounts during the experiment will be displayed in "Taler". The payoff in the selected round will be converted in € and paid out in cash. One Taler will be converted in 3 €.

[Disclosure type A and type B, No-Disclosure type A: The payoff of A depends on B's estimate of the value of the good. In case the estimate is higher than the value of the good, A's payoff is the difference between B's estimate and the value of the good. That means that for one Taler overestimation of B, type A player will be paid out one Taler. At a **maximum** A receives a payoff of 5 Taler. In case B's estimate is lower or equal the value of the good the payoff of A is 0 Taler.]

[No Disclosure type B: The calculation of type A's payoff is not known by the participants of type B. Type A participants are informed about this.]

B's payoff depends on his own estimation of the value. Is the estimation is exactly the value of the good, B receives a payoff of 5 Taler. For one Taler deviation of type B's estimation the payoff of B will decrease of one Taler. It is not important whether the estimation is lower or higher than the value of the good. If B deviates 5 or more Taler from the value of the good, he receives zero payoff.

Example. A receives the information that the value of the good is between 25.01 and 30.00 Taler. A provides B with an advice of 29.00 Taler. B estimates the value of the good at 28.50 Taler. The value of the good is 26.00 Taler. The payoff of [Disclosure type A and type B, No disclosure type A: A is 2.50 Taler and] of B is 2.50 Taler.

In the case that the period described above will be selected for payoff, both subjects will receive at the end of the experiment 2.50 Euro show-up fee and 3 x 2.50 Taler = 7.50 Euro from the period, altogether 10 Euro.

Control questions

The following questions should test whether you have understood the rules mentioned above. Please answer the questions carefully. Before starting the experiment, we will check whether you have answered the questions correctly.

Question 1. Imagine the following situation:

A receives the information that the value is between 20.01 and 25.00 Taler.
A gives B the advice that the value of the good is 24.00 Taler.
B estimates that the value of the good is 23.00 Taler.
The true value of the good is 23.00 Taler.

What is A's and B's payoff?

[Disclosure type A and B, No Disclosure type A: A receives _Taler] B receives _Taler

Question 2. Imagine the following situation:

A receives the information that the value is between 15.01 and 20.00 Taler.
A gives B the advice that the value of the good is 28.00 Taler.
B estimates that the value of the good is 17.00 Taler.
The true value of the good is 20.00 Taler.

What is A's and B's payoff?

[Disclosure type A and B, No Disclosure type A: A receives _Taler] B receives _Taler

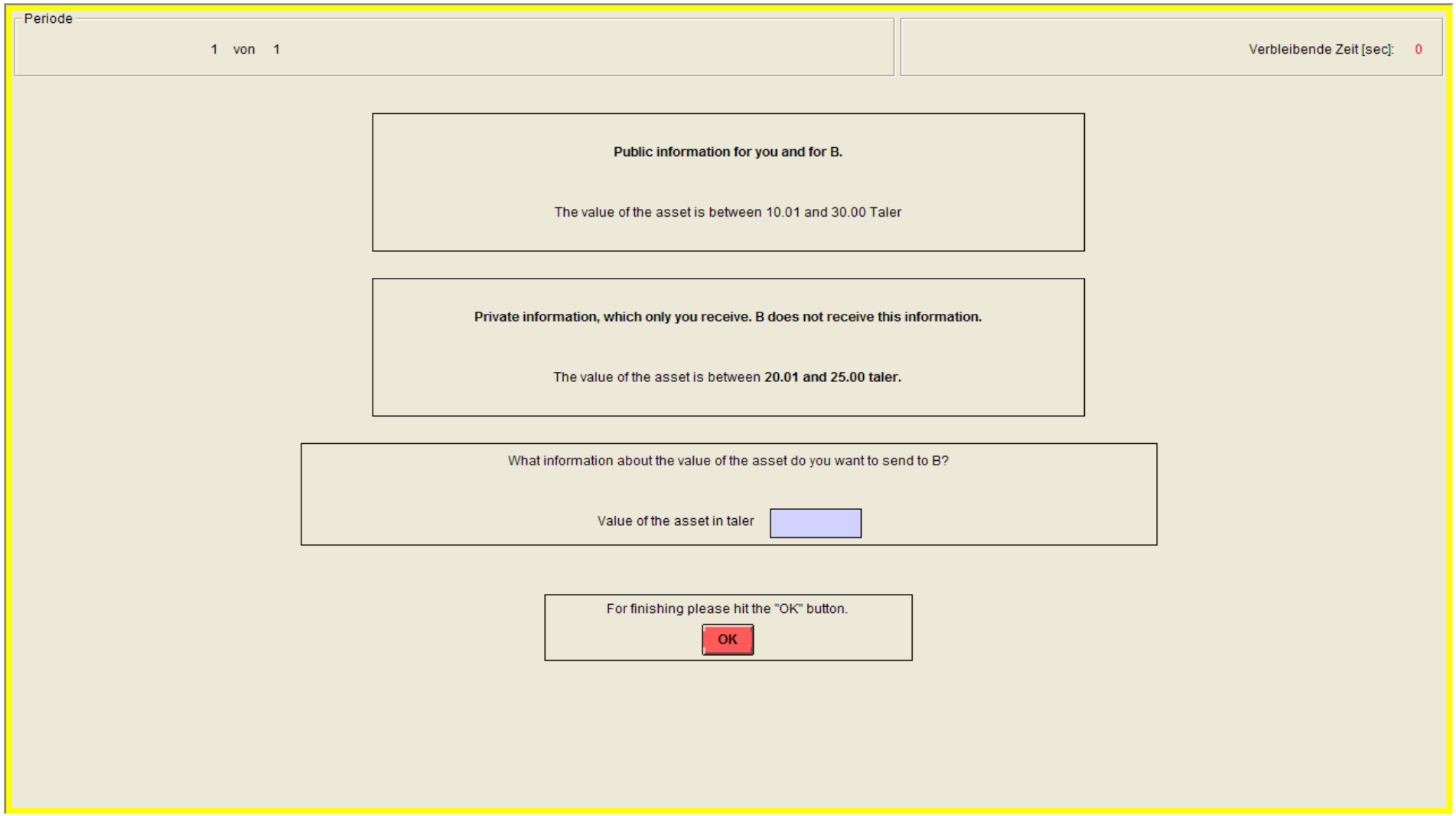
Question 3. Imagine the following situation:

A receives the information that the value is between 10.01 and 15.00 Taler.
A gives B the advice that the value of the good is 12.00 Taler.
B estimates that the value of the good is 25.50 Taler.
The true value of the good is 11.50 Taler.

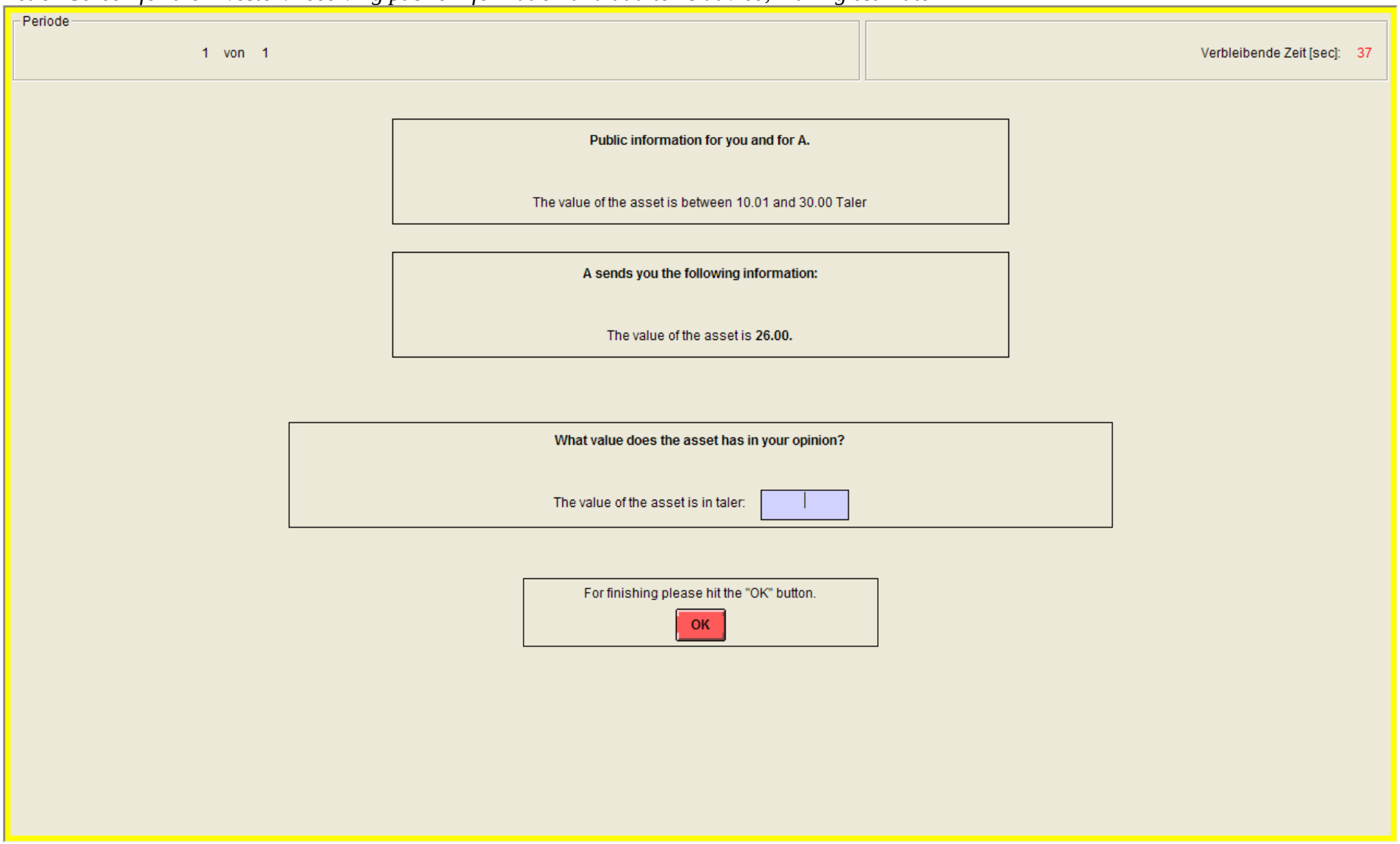
What is A's and B's payoff?

[Disclosure type A and B, No Disclosure type A: A receives _Taler] B receives _Taler

Action Screen for the auditor: receiving public and private information; sending advice to the investor.



Action Screen for the investor: receiving public information and auditor´s advice; making estimate



Payoff Screen for the auditor

Payoff Screen for the investor

Questionnaire: Evaluation of a hypothetical misreporting of the auditor: moral correctness, feeling guilty

Evaluation of A (Sender of Information)

Please imagine, that the following situation would have just happened in the experiment:

B is not informed about the payoff structure of A.

A received the information that the value of the asset is between 15.01 and 20.00 Taler.

A then gave B the advice that the value of the asset is 28 Taler.

Do you think that the behaviour of A is morally correct?

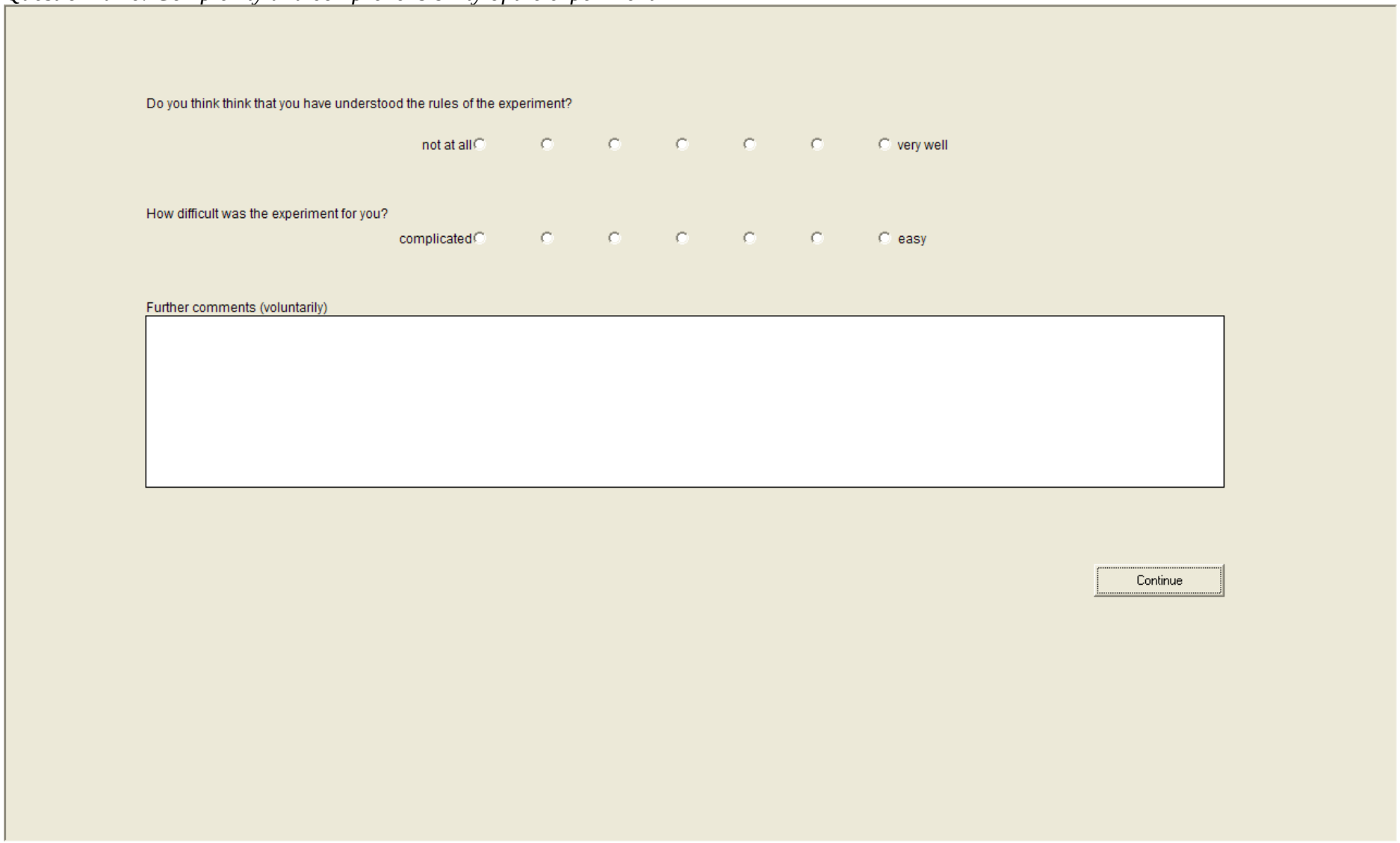
not at all correct ○ ○ ○ ○ ○ ○ ○ fully correct

Imagine that you would have behaved in the same way as A just behaved. Would you feel guilty?

not guilty at all ○ ○ ○ ○ ○ ○ ○ very guilty

Continue

Questionnaire: Complexity and comprehensibility of the experiment



Questionnaire: Personal data of the participants