SONDERFORSCHUNGSBEREICH 504

Rationalitätskonzepte,
Entscheidungsverhalten und
ökonomische Modellierung

No. 06-10

Disclosing Conflict of Interest - Does Experience and Reputation Matter?

Christopher Koch*
and Carsten Schmidt**

September 2006

Financial support from the Deutsche Forschungsgemeinschaft, SFB 504, at the University of Mannheim, is gratefully acknowledged. The authors would like to thank W. Robert Knechel, Hansrudi Lenz, Reiner Quick for valuable comments on earlier versions of the paper.

*Sonderforschungsbereich 504, email: chrkoch@sfb504.uni-mannheim.de

**Sonderforschungsbereich 504, email: cschmidt@sfb504.uni-mannheim.de

Universität Mannheim
L 13,15
68131 Mannheim

Disclosing Conflict of Interest – Does Experience and Reputation Matter?

Christopher Koch*, Carsten Schmidt[+]

*University of Mannheim, Mannheim Business School,
Sonderforschungsbereich 504, chrkoch@sfb504.uni-mannheim.de
[+] University of Mannheim, Sonderforschungsbereich 504,
cschmidt@sfb504.uni-mannheim.de

September 4, 2006

Abstract

Disclosure of conflict of interest is currently seen as an effective tool for reducing threats to auditor independence. Cain, Loewenstein, and Moore (2005) provide evidence for perverse effects of disclosing conflict of interest. Using a controlled laboratory experiment, we replicate their finding that such a disclosure can cause an impairment of auditor independence. However, as subjects gain experience we find that these results revert and auditors give less biased advice. Our results imply that the perverse effects noted in the literature might be an artifact of an environment with inexperienced subjects and of less relevance for the audit environment where main actors are experienced. To the contrary, disclosure of conflict of interest can even improve auditor independence by fostering fairness. Furthermore, we find that disclosure of conflict of interests disturbs reputation building.

JEL: C91, D82, G28, M42

Keywords: *Auditor Independence, Audit Fees, Nonaudit Fees, Experimental Economics*

---

*Corresponding author: Christopher Koch, University of Mannheim, Schloss Ostflügel, 68131 Mannheim, Tel. +49(621)-1813443, Fax: +49(621)1811694, Email: ChrKoch@sfb504.uni-mannheim.de. The financial support of the German Research Foundation (DFG Grant SFB504) is gratefully acknowledged. The authors would like to thank W. Robert Knechel, Hansrudi Lenz and Reiner Quick for valuable comments on earlier versions of the paper.

1. Introduction

Disclosure of fees paid to auditors is mandatory in the US since February 2001 (SEC, 2000) and will be mandatory in all countries of the EU from 2008 on (EU, 2006). The aim is to ameliorate the potential negative effects of high audit and especially nonaudit fees on auditor independence. Empirical evidence on the effects of high fees on auditor independence are mixed (e.g. Frankel, Johnson and Nelson, 2002; Ashbaugh, LaFond and Mayhew, 2003), while some evidence exists that it lowers the perceived auditor independence (e.g. Brandon, Crabtree and Maher, 2004; Mishra, Raghunandan and Rama, 2005; Krishnan, Sami and Zhang, 2005). Disclosure is considered by legislators as a solution as it presumably bears little costs and has the potential benefit of better informed decision making by investors (SEC, 2000; SEC, 2003a). This intuition is well described by the famous quote of the US judge Brandeis that "sunlight is the best disinfectant" (Brandeis, 1967).

However, disclosing the conflict of interest of auditors by disclosing their fees might be problematic if it sends wrong signals to investors. Investors could falsely believe that auditor independence is impaired by high fees even if it is not. Such false beliefs might be even relevant for markets as experimental evidence suggest that they are very persistent (Dopuch, King and Schwartz, 2003).

Conflict of interest is a common phenomenon in professional advisor-client relationships. For example, financial analysts may face the conflict between following their professional obligation to express their personal view about stocks and following their personal interest by issuing favorable analysis of client companies. Therefore analysts are required to disclose direct or indirect compensations relating to specific recommendations (SEC, 2003b). Other examples include physicians receiving presents from pharmaceutical companies (Kassirer, 2001), researchers receiving funds (Wagner and Michaels, 2004), real estate agents selling houses too cheap and too fast (Levitt and Syverson, 2005), and insurance agents or salesmen receiving commissions (Shapiro, 2003). More generally, the theory of conflicts of interest can also be applied to general economic questions like the behavior of competitors in oligopoly markets (Crawford and Sobel, 1982).

Cain, Loewenstein and Moore (2005) (hereinafter called CLM) provide experimental evidence that disclosing conflict of interest might have perverse effects even if it signals the

correct level of auditor independence to investors.[1] They find that auditors impair their independence to a higher degree when their conflict of interest is disclosed. According to them, this change in the behavior of auditors might be on the one hand of strategic nature as auditors might accept even more biased financial reports in order to counteract skepticism by the investors who are informed about potential threats to independence. On the other hand auditors might feel morally licensed to comprise their independence if investors are informed about the incentives of the auditor. This change in the behavior of auditors is not fully anticipated by investors. With the disclosure of conflict of interest, investors are therefore worse off in the setting of CLM.

We set up an experiment to test for these potential perverse effects of disclosure of conflict of interest. We reduce the complexity of the experiment designed by CLM to construct a more controlled environment. In our setting, investors have the task to estimate the randomly drawn, uniformly distributed values of assets. Investors are paid based on the accuracy of their estimates. They receive information about the value of assets from a better-informed auditor. This shall compare to a situation where the auditor provides information to investors by certifying financial statements. Auditors are in our experiment provided with incentives to send biased information to the investor as the payoffs of auditors rise with higher estimates of the investors. Auditors have therefore a strong incentive to impair their independence. As an experimental treatment, the disclosure of conflict of interest is manipulated: Investors are either informed about the incentives of the auditors or receive no information thereof.

We extend the experiment of CLM by testing for the influence of experience and reputation, two factors which are often considered to be the main characteristics of the auditing environment (Chaney and Philipich, 2002; Wilson Jr. and Grimlund, 1990; Bonner and Lewis, 1990). We introduce experience by having the subjects performing the task over nine periods and providing them with detailed feedback after each period. We investigate reputation by manipulating the way auditors and investors interact: In the experience condition, each auditor interacts with a different investor in each period, whereas in the reputation condition each auditor interacts with the same investor over all periods.

[1] We denote the better informed party who sends information as "auditor" and the worse informed party who receives the information as "investor". In our instructions we labelled auditors as "type A" and investors as "type B". CLM use instead the denotations "advisor" and "estimator", Crawford and Sobel (1982) use the denotations "sender" and "receiver".

We derive our hypothesis for the different settings by considering the psychological effects found to be of relevance in that setting by CLM. We further draw from conventional economic analysis of auditor independence which generally assumes that incentives of auditors to impair their independence are not disclosed (Magee and Tseng, 1990). We complement this analysis by considering economic analysis of sender-receiver games where incentives of both players are common knowledge (Crawford and Sobel, 1982). Fairness effects found to be important by the research strand of experimental economics are also considered (Güth, Huck and Ockenfels, 1996).

We contribute to experimental economics research on auditor independence (Schatzberg, 1990; Dopuch and King, 1991; Schatzberg and Sevcik, 1994; Calegari, Schatzberg and Sevcik, 1998; Dopuch, King and Schwartz, 2001). More specifically we connect to the research of Dopuch, King and Schwartz (2003) and Davis and Hollie (2005). They test for the effect of disclosure of disclosing fees on the efficiency of decision-making of investors. We also research investors' behavior, but test additionally for add to their research by testing for the effects of disclosure of disclosing fees on auditor independence by letting investors interact with human auditors instead of robot auditors as in the earlier studies. We are not aware of any study in the accounting literature who has examined experimentally this effect yet.

On the other hand, we contribute to the literature on reputation formation of auditors (King, 1996; Mayhew, 2001; Mayhew, Schatzberg and Sevcik, 2001) as we examine disclosure of conflict of interest as a potential influencing factor.

We use a stylized experimental design in order to derive policy implication on the question of the economic usefulness of mandating a disclosure of auditors' fees (Kachelmeier and King, 2002). Archival testing of the effects on disclosing fees on auditor independence is problematic as auditors' fees are very difficult to obtain in situations where fees are not disclosed, but should compulsorily be considered as an influencing factor (DeAngelo, 1981; Frankel, Johnson and Nelson, 2002; Brandon, Crabtree and Maher, 2004). The stylized experiment allows us as well to create an environment with completely misaligned incentives which legislators might fear to exist. Compared to that the field provides evidence of auditors and investors with all kind of incentive alignments and thus makes it difficult to disentangle between the effects of disclosure and the incentive structure.

Our main findings of the experiment are the following. We replicate the perverse effect of disclosing conflict of interest noted by CLM. Even in our more controlled experimental setting, auditors impair their independence to a higher degree in the first round with the disclosure of conflict of interest. However, we find that this effect disappears and even reverts for experienced subjects from the second period on. Disclosing fees improves in these periods auditor independence. We find some evidence that this happens as the influence of moral licensing diminishes and fairness effects become more important with experience and the provision of feedback. Disclosure of conflict of interest also helps investors to give unbiased advice while investors in the no disclosure condition still give upward biased advice even with experience. Furthermore, we find evidence that disclosure of conflict of interest might disturb reputation building. This is expressed by less valuable information passed on by the auditors and worse calibrated estimates of investors when auditors' incentives are disclosed.

The remainder of this paper proceeds as follows. In the next section we discuss the reasons for mandating disclosure of audit fees and present economic and psychological theories for the behavior of auditors and investors when incentives of auditors are disclosed and when they are not disclosed. In section III we describe the research design, before developing our hypotheses in section IV. In section V we report the results of our replication of the experiment by CLM and the effects of experience and reputation. We conclude the paper by summarizing our results and discussing possible implications for the auditing environment.

2. Economic and Psychological Effects of Disclosing Auditors' Fees

2.1. Reasons of legislators for mandating disclosure of auditors' fees

Auditors have a "public watchdog" function as they have the duty to serve the creditors and investors of the audited company as well as the investing public in general (United States v. Arthur Young & Co. et al., 465 U.S. 805, 817–818). They have the professional obligation to be independent in their judgment from the client. However, auditors have sometimes incentives to please the audited company by issuing favorable audit opinions. These incentives may arise from audit and nonaudit fees which the auditor receives (Magee and Tseng, 1990). The high audit and nonaudit fees that audit companies received from

companies involved in accounting scandals like Enron or Worldcom might be considered as supporting this claim (Coffee, 2004).

Legislators have reacted to this potential threat to auditor independence by both forbidding certain nonaudit services and mandating the disclosure of audit and nonaudit fees thereby following a "two-pronged approach" (SEC, 2000). Companies have to disclose separately the audit fees, audit-related fees, tax fees, and all other fees for the two most recent fiscal years (SEC, 2003a). The aim of that disclosure is to "shed light on the independence of public companies' auditors [to] assist investors in making investment […] decisions" (SEC, 2003a).

2.2. Empirical evidence of the influence of auditors' fees on auditors' independence

Empirical studies measuring the effects of nonaudit fees on proxies for auditor independence, e. g. the size of discretionary accruals, the proportion of going-concern opinions or the extent of reliance on internal controls, find mixed results. While Frankel, Johnson and Nelson (2002), DeFond, Raghunandan, and Subramanyam (2002) and Felix Jr., Gramling and Maletta (2005) find a positive association, Larcker and Richardson (2005) show that this may hold only for some companies and many other studies do not find a significant relationship (Antle, et al., 2004; Ashbaugh, LaFond and Mayhew, 2003; Chung and Kallapur, 2003; Kinney Jr., Palmrose and Scholz, 2004; Reynolds, Deis and Francis, 2004; Higgs and Skantz, 2006). The results for the influence of audit fees on auditor independence might be similar as audit fees and nonaudit fees are jointly determined (Whisenant, Sankaraguruswamy and Raghunandan, 2003; Simunic, 1984; Craswell, 1999).

Some empirical studies indicate that auditor independence in appearance might be endangered when the audit company receives a high proportion of nonaudit fees (Shockley, 1981; Pany and Reckers, 1987; Brandon, Crabtree and Maher, 2004; Mishra, Raghunandan and Rama, 2005; Higgs and Skantz, 2006) and from audit services (Gul, 1991).

2.3. Economic theory for settings without disclosure of conflict of interest

Economic theory suggests that the fees received from the audited company can be considered as incentives for the auditor to impair her independence (Watts and Zimmerman, 1981). One necessary condition is that these fees can be seen as a proxy for future rents from

the audited company. Rents from an audit engagement can arise even in competitive settings when transaction costs of switching the auditor exist (DeAngelo, 1981). A further necessary condition is that the management can credibly threaten the auditor with the deprivation of these rents, e. g. by arranging the dismissal of the auditor (Magee and Tseng, 1990). As rents from nonaudit services are supposed to be higher than for audit services (SEC, 2000) and as it is probably easier for the management to credible threaten the auditor with a deprivation of those rents, nonaudit fees might have a bigger effect on auditor independence.

One of the most important incentives to withstand such an impairment of auditor independence is a concern for reputation (Watts and Zimmerman, 1986). A loss of reputation may lead to lower audit fees (Beatty, 1993; Craswell, Francis and Taylor, 1995; Francis, Reichelt and Wang, 2005) or even to a loss of audit clients (Chaney and Philipich, 2002; Krishnamurthy, Zhou and Zhou, 2006). However, it is not easy to predict under what circumstances reputation building will happen as it might be very context specific (Davis and Holt, 1993), p. 396). Auditing research has found that the cost that investors have to bear (King, 1996), the timeliness of feedback about the performance of auditors (Mayhew, 2001) and the ambiguousness of accounting standards (Mayhew, Schatzberg and Sevcik, 2001) may matter.

When audit fees are not disclosed, investors have limited information to judge the auditor independence. Recent research has found that investors who have no information about a potential misreporting generally have trust in the reports as a default (Gneezy, 2005). Investors can however obtain noisy information about a potential impairment of independence over time by comparing actual outcomes with earlier reports. This can be compared to strategic learning (Camerer, Ho and Chong, 2003).

2.4. Economic theory for settings with disclosure of conflict of interest

Crawford and Sobel (1982) describe a theoretical framework for analyzing relationships in which a sender (the auditor) who has private information transmits a message to a receiver (the investor) who has the task to estimate the value of a quantity. The incentives of the rational and selfish sender and receiver may be misaligned and are common knowledge. The theory can be used to predict the level of truthfulness in auditors' reports and the level of trust of investors when conflict of interest is disclosed. This framework has been used in accounting contexts to examine the role of multiple users of the message (Newman

and Sansing, 1993), proprietary costs of private information (Gigler, 1994) and imperfect private information (Fischer and Stocken, 2001). Probably due to the complexity, the role of non-disclosure of the incentives has not been analyzed in this framework.

The model of Crawford and Sobel predicts for one-time interactions that if incentives of auditors and investors are aligned below a certain degree, auditors should only send uninformative signals to investors which should consequently be ignored by investors. With an increase of the alignment of incentives between auditors and investors from that degree on, the informativeness of auditors' report and investors' reliance thereon should generally increase monotonically (Dickhaut, McCabe and Mukherji, 1995). In case of fully aligned incentives, auditors will report their information truthfully and investors will fully rely on it.

2.5. Psychological effects of disclosure of conflict of interest

CLM argue that disclosure of conflict of interest might have psychological "perverse effects" on the behavior of auditors and investors. Auditors whose incentives to impair independence are disclosed might feel morally licensed to do so. They might consider impairing independence as "fair play" as the auditor is informed about it. The psychological cost of lying (Gneezy, 2005) in the form of accepting earnings management is removed when the investor is fully informed about the incentives of the auditor.

Auditors might also allow more aggressive earnings management in the disclosure condition to counteract higher discounting of more skeptic investors. They might feel the need to shout even louder if investors who are informed by the incentives of the auditors cover their ears. This strategic effect can be considered as a psychological effect as there is no reason why auditors should not impair their independence to the same degree in the no disclosure condition.

Ethical reasons speak for a lower impairment of auditor independence in the disclosure condition, because people avoid being regarded as unfair and such an unfair behavior can be much easier detected by an investor who is perfectly informed about the incentives of the auditor. Therefore it can be expected that people behave fairer in the disclosure condition (Güth, Huck and Ockenfels, 1996).

Investors on the other hand should ignore the information passed on by the auditor when they are informed that the incentives of the auditors are misaligned to their own

incentives above a certain degree (Crawford and Sobel, 1982). But results from psychology and experimental economics show that people can suffer from a curse of knowledge bias (Camerer, Loewenstein and Weber, 1989). Investors receiving information from the auditor might therefore be unable to ignore it even if they should. This could make investors trust the information from the auditor to some degree even in the disclosure condition as it was the case in CLM. They might also follow the advice of the auditor when audit and nonaudit fees are disclosed as investors might even think that such a disclosure improves auditor independence (Gaynor, McDaniel and Neal, 2005).

3. Experimental Design

3.1. Experimental procedure

In our laboratory setting we incorporate main features of an audit environment with misaligned incentives. The environment which we try to reconstruct consists of an auditor who receives rather precise information about the value of an asset through her audit procedures.[2] The auditor passes on her information to an investor by certifying the financial statement in which a specific value is assigned to the asset.[3] The investor has very little own information about the value of the asset except for the information certified by the auditor. Based on this information, the investor forms a belief about the value of the asset on which he might base his decision to buy or sell shares in the company. In the tradition of established methods of experimental economics, we use a generic frame to improve the internal validity of the experiment, e. g. the auditors are called "type A" and the investors are called "type B" in our instructions. We hereby want to keep out confounding effects of different interpretations of the term "auditor" by subjects as we want to focus on the strategic interaction in dependence on the disclosure of conflict of interest.

The structure and details of our experimental design are shown in figure 1 and table 1. At the beginning of each of a total number of nine periods, the auditor receives private information about the value of an asset and has then to decide what information she sends to the investor. The value of the asset is randomly drawn from values uniformly distributed between 10.01 and 30.00. The private information which the auditor receives is the interval in which the value of the asset is located. Intervals are restricted to]10,15],]15,20],]20,25], and

[2] For a higher readability, we refer to the auditor as she and to the investor as he in the following.
[3] The information passed on by the auditor in the form of the certification of financial statements is in the following labeled advice. Hereby, we want to distinguish it from other kinds of information used in the experiment (e.g. public information, private information).

]25,30] and this is common knowledge. She then decides which information about the value of the asset she sends to the investor as advice. She is free to choose any value between 10.01 and 30.00.

The investor receives the public information about the range and distribution of possible values of the asset and the information sent to him by the auditor. His task is then to estimate the value of the asset based on this information. While this happens, the auditor is asked to give her guess what value of the asset the investor will estimate.

The investor is paid based on the accuracy of his estimate. He receives the maximum payment of 5 taler (1 taler = 3 €) if he estimates the value of the asset correctly. If he estimates the value too high or too low, the difference between his estimate and the value of the asset is subtracted from the maximum payment. The minimum payment is 0 taler. This incentive scheme compares to an environment where investors do better in their investment decisions the more accurate they estimate the value of assets.

The auditor is paid on how much the investor overestimates the value of the asset. She receives the difference between the estimate of the investor and the value of the asset in taler. Her maximum payment is 5 taler and the minimum payment is 0 taler. We offer therefore incentives to the auditor to send too high information to the investor in order to mislead him. This compares to an audit environment where the auditor has an incentive to impair her independence and accepts earning manipulations in exchange for higher audit or nonaudit fees. We want to examine this environment as this is the environment which legislators mandating disclosure of fees fear to exist.

--- Insert figure 1 about here ---

--- Insert table 1 about here ---

The incentives of the auditor and the investor are fully misaligned in our experimental design. This might be criticized as being unrealistic in an auditing context. However, Crawford and Sobel (1982) show that the behavior of auditors and investors should be the same in settings of a higher alignment of incentives as long as the degree of alignment is below a certain level. Furthermore, if the degree of alignment increases from that level on, the tendency of experimental results should also hold as truth telling by the auditor should

generally increase monotonically (Dickhaut, McCabe and Mukherji, 1995). Finally, we can construct theoretically the behavior of auditors and investors in settings where their incentives are perfectly aligned. In such settings, auditors should report truthfully and investors should fully rely on this report. In our opinion, there is no reason to believe that such a behavior should not arise in experiments using settings of full alignment of incentives which is why we do not examine such a baseline experimentally.

3.2. Experimental manipulation

The first experimental manipulation is the disclosure of auditors' conflict of interest to the investor. In one condition of this treatment, the investor is precisely informed about the incentives of the auditor (disclosure condition) and his comprehension thereof is ensured by control questions. In the other condition of the treatment, the investor receives no information at all about the incentive scheme of the auditor (no disclosure condition). The auditor is in both conditions informed how much knowledge the investor has about her incentives. The no disclosure condition is hereby meant to be indicative for the former situation where disclosure of audit and nonaudit fees was not mandated, whereas the condition of full disclosure of auditor's incentives stands for the current situation were fees paid to the auditor has to be disclosed.

The second experimental manipulation is the way auditors and investors are matched. Note that in all conditions, one auditor interacts with one investor. In the first condition, perfect stranger matching is used which means that each auditor interacts only for one of the nine periods with the same investor. In the second condition, partner matching is implemented where the auditor interacts with the same investor over all nine periods. We fix the length of the experiment and do not determine it randomly in order to ensure comparability to the non-reputation setting. We believe that the setting of interaction with the same partner over all periods coupled with precise feedback provides ample opportunities for auditors to build up reputation. We believe so as research in experimental economics has shown that in general subjects do not induce backwards more than one or two steps (Camerer, Ho and Chong, 2004). Furthermore, choosing an alternative design as determining the last period randomly or not informing the participants about the number of periods also has its disadvantages (Boatsman and Grasso, 1992).

The first period of the perfect stranger matching is a quasi-replication of the experiment of CLM in a more controlled environment. We consider only the first period as a replication as CLM conducted a quasi one-shot experiment: Though it lasted six periods, the participants were provided with no feedback over the first three periods and with only very limited feedback over the last three periods. As this feedback was in addition rather uninformative for investors due to an underestimation bias of the auditors, their setting hardly provides any opportunities for gaining experience. We consider our design as more clear-cut since consistent underestimation is ruled out by design, understanding of the setting by all subjects is ensured by control questionnaires, and detailed feedback is given after each round.

We classify the 2nd to 8th period of the perfect stranger matching as the experience setting. We already consider subjects interacting for the second time as experienced as our experimental setting is rather simple, control questions are used and detailed feedback is provided. This is also supported by the results reported below which change significantly already from the first to the second period. We exclude the ninth period to ensure comparability to the reputation setting where we exclude the last period to eliminate possible last round effects.

We classify the 2nd to 8th period of the partner matching treatment as the reputation setting. We eliminate the first round to ensure comparability to the experience setting and eliminate the ninth round to eliminate possible last round effects.

--- Insert table 2 about here ---

The experiment was conducted at the experimental lab of the national research center on concepts of rationality, decision-making and economic modeling (Sonderforschungsbereich 504) at the University of Mannheim, Germany, using subjects recruited from the mailing list of the experimental lab where mostly students from the University of Mannheim are inscribed. The decisions forms were computerized using zTree (Fischbacher, 1999). We conducted 8 sessions in a 2 x 2 between-subject design, with two sessions for each of the four treatment conditions (see table 2). In each session 18 to 20 subjects participated to whom either the role of an auditor or of an investor was randomly assigned in equal proportion. All together, 148 subjects participated and all of them

completed the session. Each session lasted about 40 to 50 minutes with payoffs for each subject ranging from 2.50 € (3.00 $) to 17.50 € (21.00 $) and averaging 8.30 € (10.00 $). For determining payoffs, one period of the nine periods was randomly selected in each session.

4. Hypotheses

4.1. Stranger interaction without feedback: replication of Cain, Loewenstein, and Moore (2005)

We classify the first period of the perfect stranger matching as the replication of the experiment of CLM.

According to CLM, auditors might feel reluctant to follow only their self-interest when their conflict of interest is not disclosed to investors. With the disclosure of the conflict of interest, these moral concerns could at least partly be removed. Auditors might consider it to some degree fair play and feel "morally licensed" to follow only their self-interest when investors are informed about it. Connected to this argument are studies initiated by (Gneezy, 2005) which show that lying can bear psychological costs which might be removed through the disclosure of conflict of interest. In addition to this, auditors might also be motivated to pursue stronger their self-interest in the disclosure condition in an effort to counteract increased skepticism by the other party who is informed about the conflict of interest.

Investors should ignore reports by auditors when they are informed that the incentives of the auditors are as misaligned as in our experiment. However, they might suffer the curse-of-knowledge bias (Tversky and Kahneman, 1974; Camerer, Loewenstein and Weber, 1989) and anchor too much on the higher advice of the auditor in the disclosure condition.

Hypothesis 1: The advice bias[4] is larger in the disclosure condition than in the no disclosure condition.

Hypothesis 2: The estimate bias[5] is larger in the disclosure condition than in the no disclosure condition.

[4] Advice bias is hereby defined as the difference between the advice and the expected value of the asset (= middle of the interval of the private information of the auditor).
[5] Estimate bias is defined as the difference between the estimate and the expected value of the asset (= middle of the interval of the private information of the auditor).

4.2. Stranger interaction with feedback: the experience setting

Our first extension of the experiment of CLM is that we do not use a quasi-one-shot design, but let auditors interact with investors over nine periods with detailed feedback after each round. We hereby test for effects of experience and strategic learning.

In the no disclosure condition, the auditor has only incentives to give biased advice. Incentives for truthful reporting do not exist as reputation building is not possible due to the perfect stranger matching. Therefore the auditor should always send the highest possible value of the asset to the investor (e. g. Magee and Tseng 1990).

In the disclosure condition, the auditor has theoretically no incentives to give upward biased advice as investors should ignore her advice according to economic theory. The auditor should therefore send uninformative advice (Crawford and Sobel 1982). However, it might be plausible that investors nevertheless rely to some degree on auditors' advice. Even if this is the case, we predict that advice of the auditor will be less biased in the disclosure condition as the auditor might feel more strongly the need to appear fair towards the investor as the intentions of her behavior are best observable for the investor in the disclosure condition with feedback (Güth, Huck and Ockenfels, 1996).

We hypothesize that the psychological effects found to be of relevance by CLM are of less importance for experienced subjects. Auditors might feel morally licensed to give biased advice also in the disclosure condition after they experience that they need to lie in order to receive higher payments. Differences in auditors' behavior in dependence on the disclosure condition might also diminish after subjects in the disclosure condition experience that trying to counteract skepticism of investors by giving higher advice is not successful.

For investors we hypothesize that they give more accurate and less biased advice in the disclosure condition as they are better informed about the incentives of auditors and can therefore better discount their advice. We expect anchoring effects to disappear as investors gain experience. We expect that investors in the no disclosure condition are misled by the biased advice of the auditor though some strategic learning about the incentives of the auditors might happen over the time.

We would like to emphasize that the following hypotheses for the experience setting are just opposite to our hypotheses for the replication of CLM and to the results of CLM.

Hypothesis 3: The advice bias is lower in the disclosure condition than in the no disclosure condition.

Hypothesis 4: The estimate bias is lower in the disclosure condition than in the no disclosure condition.

4.3. Partner interaction with feedback: the reputation setting

Our second extension of the experiment of CLM is that we manipulate the matching of auditors and investors. We test for reputation effects by conducting a partner matching in which the same auditor interacts with the same investor over all nine periods.

Economic theory does not provide for hypothesis what the effects of disclosure will be in a repeated setting. To explore possible effects of disclosure, we state therefore the null hypothesis that it will have no effects.

Hypothesis 5: The advice bias is the same in both disclosure conditions.

Hypothesis 6: The estimate bias is the same in both disclosure conditions.

We examine also the effects of reputation by comparing the results of the reputation setting to those of the experience setting for each of the two disclosure conditions. In both conditions, the potential loss of auditor reputation should serve as an incentive for the auditor to report truthfully. The bias in the report of the auditor should therefore be smaller in the reputation setting compared to the experience setting. Investors should profit from this by giving less biased and more accurate estimates.

Hypothesis 7: In the no disclosure condition of the reputation setting, the advice bias and the estimate bias are lower than in the no disclosure condition of the experience setting.

Hypothesis 8: In the disclosure condition of the reputation setting, the advice bias and the estimate bias are lower as in the disclosure condition of the experience setting.

5. Results

5.1. Statistical modeling

If not otherwise mentioned we use parametric tests which control for the individual subject and the interval of the value of the asset in our statistical analyses. We control for the individual subject by including it as a random effect in the model as the decisions of a single individual in different periods cannot be considered as being independent.[6] Since standard t-test might be biased, we control for the interval of the value of the asset by including it as a fixed effect in the model as the bias of the advice and the estimate depend on it by definition. We classify hereby the interval as a continuous variable suspecting an approximate linear trend. We report among other things the means estimated by the statistical model, The so-called LS-Means presented for one factor are adjusted for the effects of other factors. The statistical analysis was done using JMP version 5.0.1.

5.2. Results for the replication of Cain, Loewenstein, and Moore (2005)

We label the first period of the perfect stranger matching as the replication of the experiment of CLM. The descriptive data summarized in table 3 show that the mean value of the advice of the auditors is higher in the disclosure condition than in the no disclosure condition (26.28 vs. 23.35) and more biased (5.17 vs. 2.52). Despite the higher advice in the disclosure condition, auditors do not expect investors to give higher estimates (22.61 vs. 22.72). Actually, investors in the disclosure condition even estimate the value of the asset to be lower than investors in the no disclosure condition (19.81 vs. 22.36) and are less upward biased (-1.31 vs. 1.53).

Hypothesis 1 is marginally confirmed as the advice bias is border line significantly higher in the disclosure condition than in the no disclosure condition ($p=0.101$). Hypothesis 2 stated that estimates of investors are more biased in the disclosure condition. This hypothesis cannot be confirmed. Instead we find marginal support for the opposite as the estimate bias is border line significantly lower in the disclosure condition ($p=0.095$). The results of the test statistics are graphically presented in figure 2.

[6] We are aware of the fact that one could argue we should even further aggregate the values of all individuals in a perfect stranger matching session to one single value as there are indirect interactions between all of them. However, we believe that this would go too far as the subjects do not directly interact with each other as opposed to a market setting where direct interaction between all subjects happens. We also find that our results hold when controlling for carry over effects from the preceding period.

--- Insert figure 2 about here ---

We are therefore able to replicate the "perverse effects of disclosure of conflict of interest" of CLM on auditor independence for a setting where auditors and investors are inexperienced and have not received any feedback yet. However, we do not find evidence that investors suffer from the curse-of-knowledge bias as they are not misled by the higher advice in the disclosure condition into giving higher estimates. The reason for this might be that our subjects were well informed about the experimental environment due to control questions. Interestingly, they are even too skeptic and discount the advice too much in the disclosure condition.

5.3. Results for the experience setting

We classify the 2nd to 8th period of the perfect stranger matching as the experience setting. Qualitative results do not change when including the last round in the analysis. The descriptive results (see table 3) show that the advice of experienced auditors is lower (24.72 vs. 26.44) and less biased (4.40 vs. 6.28) with the disclosure of conflict of interest compared to the situation without this disclosure. Experienced investors are still more biased (2.78 vs. -0.65) and give higher estimates (22.94 vs. 19.67) in the no disclosure condition.

Hypothesis 3 can be marginally supported as the advice bias for experienced auditors is lower when their incentives to impair independence are disclosed ($p=0.056$). Hypothesis 4 is confirmed as the estimate bias of the investors is highly significantly lower in the disclosure condition ($p=0.001$). The results of the test statistics are graphically presented in figure 3.

--- Insert figure 3 about here ---

We find therefore that the perverse effects of disclosure of conflict of interest on auditors' behavior do not hold from the second period on and even revert ($p=0.001$ for the effects of the interaction between disclosure and first period on the advice bias). Auditor independence improves with the disclosure of their incentives instead of being more impaired as they gain experience.

We further explored reasons for this change in behavior and find some evidence that moral licensing and the psychological costs of lying diminish after the first period. For doing so, we asked all subjects whether they consider it morally correct to give upward biased advice in the experiment. We find that auditors who are morally concerned show a lower advice bias in the first period. This effect however disappears from the second period on as shown in figure 4 ($p=0.029$ for the effects of the interaction between the classification of giving upward biased advice as a lie and the first period on the advice bias).

--- Insert figure 4 about here ---

Experienced investors can profit from the higher independence of experienced auditors insofar as their estimates are less biased. However, as their estimates are even downward biased, they cannot profit from it through higher payoffs which stay about the same when compared to the replication setting of inexperienced subjects (1.23 vs. 1.20). These findings are in accordance with Dopuch, King and Schwartz who also find that the quality of decision-making of investors do not significantly improve with the disclosure of fees even if it signals the correct level of auditor independence.

The downward bias in the estimates affects also the payoffs of auditors negatively which are significantly lower in the disclosure condition ($p=0.001$). The higher independence of experienced auditors in the disclosure condition is therefore not rewarded which confirms the results of King (1996) who also finds that truthful reporting was not rewarded by investors.

--- Insert table 4 about here ---

5.4. Results for the reputation setting

We classify the 2nd to 8th period of the partner matching treatment as the reputation setting. Again, qualitative results do not change when including additionally the data of the 1st or 9th period. The descriptive results (see table 3) show that the advice and the advice bias is for both disclosure conditions on a similar level (24.24 and 3.71 vs. 23.39 and 3.87).

Estimates and estimate biases are however smaller in the disclosure condition (19.87 and -0.67 vs. 21.68 and 2.16).

Hypothesis 5 stated that there will be no difference in the advice bias for both conditions. As the difference is indeed not significant, this hypothesis cannot be rejected (p=0.64). Hypothesis 6 predicted the same for the estimate bias and can be rejected as the estimate bias is significantly smaller in the disclosure condition (p=0.004). The results of the test statistics are graphically presented in figure 2.

--- Insert figure 5 about here ---

Though the advice bias is about the same in both disclosure conditions, this does not mean that the mean behavior of auditors is the same regarding the private information. Indeed the interaction effect of disclosure and the interval of the asset on the advice is highly significant (p=0.001). While auditors in the disclosure condition give about the same advice for each interval (average difference in advice is 1.14 per interval), they differentiate much more in the no disclosure condition as shown in figure 6 (average difference in advice is 3.13 per interval). Therefore, it can be concluded that auditors in the no disclosure condition try to form some reputation by giving more informative advice. This does not happen in the disclosure condition.

--- Insert figure 6 about here ---

Such an interaction between the disclosure of incentives and the interval of the asset exists also for the estimate (p=0.001). While investors give about the same estimate regardless of the true value of the asset (average difference in estimate of -0.24 per interval), they are able to give higher estimates for higher true values of the asset in the no disclosure condition as shown in figure 7 (average difference in estimate of 3.06 per interval). Hints for a better calibration of the estimates of investors in the no disclosure condition are also the higher payoffs for the investors in this condition (1.48 vs. 2.10; p=0.009; see table 4).

--- Insert figure 7 about here ---

Hypothesis 7 can be confirmed for the most part as the advice bias is significantly lower (p=0.001) and less biased (p=0.13) in the no disclosure condition of the reputation setting. This is in accordance with the results above that reputation formation happens in this setting. For the disclosure condition we find no evidence for reputation formation when testing hypothesis 8 as the advice bias is not significantly lower in the reputation setting (p=0.63) and the estimate bias is about the same in both conditions (-0.67 vs. -0.65; p=0.80).

In summary, we find that reputation formation is hindered by the disclosure of conflict of interest. Auditors provide less valuable advice in the disclosure condition which also affects the decision-quality of investors negatively. They send uninformative advice and are less trusted by investors.

5.5. Overall robustness of results: A regression analysis and payoffs

To check for the overall robustness of our main results, we fit a general linear mixed model employing all data. We are interested in the determinants of auditors' advice bias and investors' estimate bias. To account for individual characteristics of participants an error components econometric model with the individual as the random component has been used (see table 5).

Beside subject specific effects, which are controlled for by the specified model, other factors might drive the results as well. In the previous analyses we found the advice bias strongly influenced by the interval of the chosen value. Therefore we control for the interval and find a higher advice bias in lower intervals. This observation is more pronounced in the disclosure condition and less pronounced with reputation. A time trend can be found in the data as well. Advice bias in later periods increases slightly with this effect more pronounced in the reputation treatment. We cannot find evidence that auditors systematically react to the estimate bias of the paired investor in the former period.

Regarding the bias of advice, disclosure of conflict of interest has no overall significant effect which can be partly traced back to the marginally significant interaction with the first period. This interaction provides a consistency check of the perverse effects first pointed out by CLM. Additionally, disclosure interacts highly significantly with the interval

of the asset. This means that the advice bias of the auditor is lower only for low asset values. These low asset values are especially interesting as problems of auditor independence mainly relate to accepting earning overstatements. Therefore it can be stated that disclosure of conflict of interest improves auditor independence.

Disclosure of conflict of interest has a highly significant influence on the estimate bias. The data suggests that disclosure of conflict of interest might be helpful to reduce the prediction bias of investors. Reputation has a highly significant influence in interaction with the interval of the asset. Thus repeated interaction with the same auditor is helpful for auditors for low asset values.

When looking at investors' and auditors' payoffs it turns out that the lower estimate bias in all disclosure treatments does not translate into higher payoffs of investors when compared to no disclosure. One explanation is that payoffs are driven by coordination effects. Since we designed the game as a fixed pie distribution game when the estimate is above the value and with decreasing pie the estimate being below the value, the sum of investor and auditor payoffs is higher in treatments where investors consistently overestimate the value. This is the case in the no disclosure treatment and thus the sum of auditors' and investors' payoff is significantly higher (see Table 4, coordination) . When investigating the no disclosure treatment we find without repeated interaction auditors to receive a relatively larger part of the pie and with repeated interaction the pie almost distributed evenly between both types. This observation confirms that reputation has an effect in the no-disclosure treatment and plays a minor role in the disclosure treatment.

Auditors' payoffs are significantly lower in the disclosure condition for both the experience (1.66 vs. 2.65; p=0.001) and the reputation setting (1.60 vs. 2.18; p=0.017; see table 4). Since auditors send less informative advice, investors are less calibrated. Disclosure of conflict of interest is in our experiment therefore comparable to a sanction for the activity disclosed.

6. Conclusions

Mandatory disclosure of audit and nonaudit fees has recently been revived in the U.S. and will soon be prescribed in all countries of the European Union (EC 2005). It is hoped that disclosure of conflict of interest can ameliorate problems of threats to auditor independence

by enabling investors to make better investment decisions. However, effects of disclosure of fees on auditor independence in fact have hardly been researched yet. A reason might be that such effects are difficult to examine empirically due to missing data about auditors' fees before their disclosure was mandatory. Some research has examined the effects of disclosure of fees on market efficiency experimentally (Dopuch, King and Schwartz, 2003; Davis and Hollie, 2005). To our knowledge, the first paper that examines experimentally the effects of disclosure of fees on auditor independence is that of CLM. We advance their research by using a more controlled experimental design and extending the setting by testing for effects of experience and reputation – two main characteristics of the auditing environment.

Legislators prescribing the disclosure of fees implicitly assume that high audit and nonaudit fees might impair auditor independence. Economic theory and some empirical evidence also indicate that the higher the fees from a certain client are, the higher is the danger that the auditors impair their independence. We therefore assume for our experimental setting that the disclosure of fees is comparable to a disclosure of conflict of interest.

We derive the hypotheses for the different settings of our experiment by combining conventional economic models of auditor independence (DeAngelo, 1981; Magee and Tseng, 1990) with economic theory predicting the degree of independence in reporting when incentives of auditors and investors diverge and are common knowledge (Crawford and Sobel, 1982). We consider also psychological effects found to be relevant in settings of disclosure of conflict of interest by CLM and apply results from experimental economics concerning fairness (Güth, Huck and Ockenfels, 1996).

The first main result of our experiment is that the perverse effects of disclosure of conflict of interest on auditor independence reported by CLM can be replicated in a more controlled experiment as long as subjects are inexperienced. Auditors whose incentives are disclosed give more biased advice probably to counteract their lower credibility. Investors are however able to discount the advice of the auditor in the disclosure condition and are not prone to anchoring effects. They even show some degree of overskepticism as their estimates are somewhat downward biased.

Our second main result is that the disclosure of conflict of interest can have positive effects if subjects are experienced. Experienced auditors give less biased advice in the disclosure condition. The change in behavior of the auditors in the disclosure condition can be

explained by a diminished influence of moral licensing with experience and increased importance of fairness effects with the provision of feedback.

Our third main result is that disclosure of conflict of interest has however negative effects in the sense that it hinders reputation formation of auditors and leads to more distrust by investors. While auditors in the disclosure condition cannot build up reputation and instead send only uninformative advice, auditors in the no disclosure condition give advice that is more strongly associated with the true value of the asset in the reputation setting. Their information is also more informative as it is more closely aligned to the true values of the asset. This allows investors to give better calibrated estimates though they are still upward biased.

Our fourth main results is that disclosure of conflict of interest might be an essential tool for eliminating biases in the estimates of investors in circumstances where auditor independence is impaired. Without such a disclosure, investors' estimate bias is upward biased over all settings. It is not mitigated either by experience nor reputation.

As a first implication of our experiment for the audit environment, we want to stress the positive effects of disclosure of conflict of interest for experienced subjects. Disclosure lowers the impairment of auditor independence and allows investors to make unbiased investment decisions. It may even support ethical behavior of auditors by giving auditors the incentive to appear fair. Though we replicate the perverse effects of disclosure reported by CLM for inexperienced subjects, we find no evidence of their appearance for experienced subjects. The effects found by CLM might therefore be of little relevance for the auditing environment.

As a second implication, we would like to point out potential negative effects of disclosure of conflict of interest. Disclosure hinders reputation formation and leads to more distrust by investors which has the effect of lower informativeness of audit reports and lower quality of decision-making. It leads therefore to a situation where both auditors and investors are worse off than before. Disclosure of conflict has therefore a sanctioning effect on the activity disclosed.

Audit companies who nevertheless want to stay engaged in the non-audit services should be aware of this negative effect and take measure to counteract potential reputation losses. This could be done for example by complementing the disclosure of fees by

communicating the reasons for providing non-audit services to investors or by describing implemented safe-guards for ensuring auditor independence.

Legislators should be careful in prescribing the disclosure of conflict of interest. It shall be remembered that one of the reasons why the SEC stopped mandating disclosure of fees in 1982 after its introduction in 1978 was, that it was worried of an "unwarranted curtailment of nonaudit services" (SEC, 1982, p. 3810). Such a sanctioning effect for nonaudit fees seems to be wanted to some degree by legislators (SEC, 2000). Sanctioning is however certainly not intended for regular audit services as high audit fees can also be a sign of the provision of high audit quality. Mandating the disclosure of audit fees might then unnecessarily and unintentionally destroy reputation of audit companies and lower audit quality.

References

Antle, R., E. A. Gordon, G. Narayanamoorthy and L. Zhou. 2004. The Joint Determination of Audit Fees, Non-Audit Fees, and Abnormal Accruals. Working Paper. Yale ICF Working Paper No. 02-21, available at SSRN: http://ssrn.com/abstract=318943.

Ashbaugh, H., R. LaFond and B. W. Mayhew. Do Nonaudit Services Compromise Auditor Independence? Further Evidence. *Accounting Review*. 78 (3): 611-639.

Beatty, R. P. The Economic Determinants of Auditor Compensation in the Initial Public Offerings Market. *Journal of Accounting Research*. 31 (2): 294-302.

Boatsman, J. R. and L. P. Grasso. A Perspective on the Use of Laboratory Market Experimentation in Auditing Research. *Accounting Review*. 67 (1): 148-156.

Bonner, S. E. and B. L. Lewis. Determinants of Auditor Expertise. *Journal of Accounting Research*. 28 (3): 1-28.

Brandeis, L. D. 1967. *Other people's money and how the bankers use it*. New York: Harper & Row.

Brandon, D. M., A. D. Crabtree and J. J. Maher. Nonaudit Fees, Auditor Independence, and Bond Ratings. *Auditing*. 23 (2): 91-105.

Cain, D. M., G. Loewenstein and D. A. Moore. The Dirt on Coming Clean: Perverse Effects of Disclosing Conflicts of Interest. *Journal of Legal Studies*. 34 (1): 1-25.

Calegari, M. J., J. W. Schatzberg and G. R. Sevcik. Experimental evidence of differential auditor pricing and reporting strategies. *Accounting Review*. 73 (2): 255-275.

Camerer, C., T. Ho and K. Chong. Models of Thinking, Learning, and Teaching in Games. *American Economic Review*. 93 (2): 192-195.

Camerer, C., G. Loewenstein and M. Weber. The Curse of Knowledge in Economic Settings: An Experimental Analysis. *Journal of Political Economy*. 97 (5): 1232-1254.

Camerer, C. F., T.-H. Ho and J.-K. Chong. A Cognitive Hierarchy Model of Games. *Quarterly Journal of Economics*. 119 (3): 861-898.

Chaney, P. K. and K. L. Philipich. Shredded Reputation: The Cost of Audit Failure. *Journal of Accounting Research*. 40 (4): 1221-1245.

Chung, H. and S. Kallapur. Client Importance, Nonaudit Services, and Abnormal Accruals. *Accounting Review*. 78 (4): 931-955.

Coffee, J. C. J. What Caused Enron? A Capsule Social and Economic History of the 1990s. *Cornell Law Review*. 89 269-309.

Craswell, A. T. Does the Provision of Non-Audit Service Impair Independence. *International Journal of Auditing*. 3 (1): 29-40.

Craswell, A. T., J. R. Francis and S. L. Taylor. Auditor Brand Name Reputations and Industry Specializations. *Journal of Accounting & Economics*. 20 (3): 297-322.

Crawford, V. P. and J. Sobel. Strategic Information Transmission. *Econometrica*. 50 (6): 1431-1451.

Davis, D. D. and C. A. Holt. 1993. *Experimental economics*. Princeton, NJ: Princeton University Press.

Davis, S. M. and D. Y. Hollie. 2005. An Experimental Investigation of Non-Audit Service Fees and Investors' Perceptions of Auditor Independence: Post-Enron Era, Working Paper, Georgia State University & University of Houston.

DeAngelo, L. E. Auditor Independence, "Low Balling", and Disclosure Regulation. *Journal of Accounting & Economics*. 3 (2): 112-127.

DeFond, M. L., K. Raghunandan and K. R. Subramanyam. Do Non-Audit Service Fees Impair Auditor Independence? Evidence from Going Concern Audit Opinions. *Journal of Accounting Research*. 40 (4): 1247-1274.

Dickhaut, J. W., K. A. McCabe and A. Mukherji. An experimental study of strategic information transmission. *Economic Theory*. 6 (3): 389.

Dopuch, N. and R. R. King. The Impact of MAS on Auditors' Independence: An Experimental Markets Study. *Journal of Accounting Research*. 29 (3): 60-98.

Dopuch, N., R. R. King and R. Schwartz. Independence in Appearance and in Fact: An Experimental Investigation. *Contemporary Accounting Research*. 20 (1): 79-114.

Dopuch, N., R. R. King and R. Schwartz. An Experimental Investigation of Retention and Rotation Requirements. *Journal of Accounting Research*. 39 (1): 93-117.

EU. 2006. Directive 2006/43/EC on statutory audit of annual accounts and consolidated accounts and amending Council Directives 78/660/EEC and 83/349/EEC. Official Journal of the European Union L 157 Pages.

Felix Jr., W. L., A. A. Gramling and M. J. Maletta. The Influence of Nonaudit Service Revenues and Client Pressure on External Auditors' Decisions to Rely on Internal Audit. *Contemporary Accounting Research*. 22 (1): 31-53.

Fischbacher, U. 1999. z-Tree - Zurich Toolbox for Readymade Economic Experiments - Experimenter's Manual, Working Paper, University of Zurich.

Fischer, P. E. and P. C. Stocken. Imperfect Information and Credible Communication. *Journal of Accounting Research*. 39 (1): 119-134.

Francis, J. R., K. Reichelt and D. Wang. The Pricing of National and City-Specific Reputations for Industry Expertise in the U.S. Audit Market. *Accounting Review*. 80 (1): 113-136.

Frankel, R. M., M. F. Johnson and K. K. Nelson. The Relation between Auditors' Fees for Nonaudit Services and Earnings Management. *Accounting Review*. 77 (4): 71-105.

Gaynor, L. M., L. S. McDaniel and T. L. Neal. 2005. The Effects of Joint Provision and Disclosure of Non-Audit Services on Audit Committee Members' Decisions and Investors' Preferences. Working Paper. Georgetown University, University of Kentucky & University of Tenessee, available at SSRN: http://ssrn.com/abstract=564946.

Gigler, F. Self-Enforcing Voluntary Disclosures. *Journal of Accounting Research*. 32 (2): 224-240.

Gneezy, U. Deception: The Role of Consequences. *American Economic Review*. 95 (1): 384-395.

Gul, F. A. Size of Audit Fees and Perceptions of Auditors' Ability to Resist Management Pressure in Audit Conflict Situations. *Abacus*. 27 (2): 162-172.

Güth, W., S. Huck and P. Ockenfels. Two-Level Ultimatum Bargaining with Incomplete Information: An Experimental Study. *The Economic Journal*. 106 (436): 593-604.

Higgs, J. L. and T. H. Skantz. Audit and Nonaudit Fees and the Market's Reaction to Earnings Announcements. *Auditing*. 25 (1): 1-26.

Kachelmeier, S. J. and R. R. King. Using Laboratory Experiments to Evaluate Accounting Policy Issues. *Accounting Horizons*. 16 (3): 219-232.

Kassirer, J. P. Financial Conflict of Interest: An Unresolved Ethical Frontier. *American Journal of Law and Medicine*. 27 (2&3): 149-162.

King, R. R. Reputation Formation for Reliable Reporting: An Experimental Investigation. *Accounting Review*. 71 (3): 375-396.

Kinney Jr., W. R., Z.-V. Palmrose and S. Scholz. Auditor Independence, Non-Audit Services, and Restatements: Was the U.S. Government Right? *Journal of Accounting Research*. 42 (3): 561-588.

Krishnamurthy, S., J. Zhou and N. Zhou. Auditor Reputation, Auditor Independence, and the Stock-Market Impact of Andersen's Indictment on Its Client Firms. *Contemporary Accounting Research*. 23 (2): 465-490.

Krishnan, J., H. Sami and Y. Zhang. Does the Provision of Nonaudit Services Affect Investor Perceptions of Auditor Independence? *Auditing*. 24 (2): 111-135.

Larcker, D. F. and S. A. Richardson. Fees Paid to Audit Firms, Accrual Choices, and Corporate Governance. *Journal of Accounting Research*. 42 (3): 625-658.

Levitt, S. D. and C. Syverson. 2005. Market Distortions when Agents are Better Informed: The Value of Information in Real Estate Transactions, NBER Working Paper No. 11053,

Magee, R. P. and M.-C. Tseng. Audit Pricing and Independence. *Accounting Review*. 65 (2): 315-336.

Mayhew, B. W. Auditor Reputation Building. *Journal of Accounting Research*. 39 (3): 599-617.

Mayhew, B. W., J. W. Schatzberg and G. R. Sevcik. The Effect of Accounting Uncertainty and Auditor Reputation on Auditor Objectivity. *Auditing*. 20 (2): 49-70.

Mishra, S., K. Raghunandan and D. V. Rama. Do Investors' Perceptions Vary with Types of Nonaudit Fees? Evidence from Auditor Ratification Voting. *Auditing*. 24 (2): 9-25.

Newman, P. and R. Sansing. Disclosure Policies with Multiple Users. *Journal of Accounting Research*. 31 (1): 92-112.

Pany, K. and P. M. J. Reckers. Within- Vs. Between-Subjects Experimental Designs: A Study of Demand Effects. *Auditing*. 7 (1): 39-53.

Reynolds, J. K., J. D. R. Deis and J. R. Francis. Professional Service Fees and Auditor Objectivity. *Auditing*. 23 (1): 29-52.

Schatzberg, J. W. A Laboratory Market Investigation of Low Balling in Audit Pricing. *Accounting Review*. 65 (2): 337-362.

Schatzberg, J. W. and G. R. Sevcik. A Multiperiod Model and Experimental Evidence of Independence and "Lowballing". *Contemporary Accounting Research*. 11 (1): 137-174.

SEC. 1982. *SEC Accounting Rules*. Chicago: Commerce Clearing House.

SEC: Final Rule: Revision of the Commission's Auditor Independence Requirements, FRR No. 56, SEC Washington, DC, 2000.

SEC: Final Rule: Strengthening the Commission's Requirements Regarding Auditor Independence, FRR No. 68, SEC Washington DC, 2003a.

SEC: Regulation Analyst Certification, 17 CFR 242, SEC Washington DC, 2003b.

Shapiro, S. P. Bushwhacking the Ethical High Road: Conflict of Interest in the Practice of Law and Real Life. *Law & Social Inquiry*. 28 (1): 87-268.

Shockley, R. A. Perceptions of Auditors' Independence: An Empirical Analysis. *Accounting Review*. 56 (4): 785-800.

Simunic, D. A. Auditing, Consulting, and Auditor Independence. *Journal of Accounting Research*. 22 (2): 679-702.

Tversky, A. and D. Kahneman. Judgment under Uncertainty: Heuristics and Biases. *Science*. 185 (4157): 1124-1130.

Wagner, W. and D. Michaels. Equal Treatment for Regulatory Science: Extending the Controls Governing the Quality of Public Research to Privat Research. *American Journal of Law and Medicine*. 30 (2&3): 119-154.

Watts, R. L. and J. L. Zimmerman. 1981. The Market for Independence and Independent Auditors, Working Paper, University of Rochester.

Watts, R. L. and J. L. Zimmerman. 1986. *Positive Accounting Theory*. Englewood Cliffs, NJ: Prentice Hall.

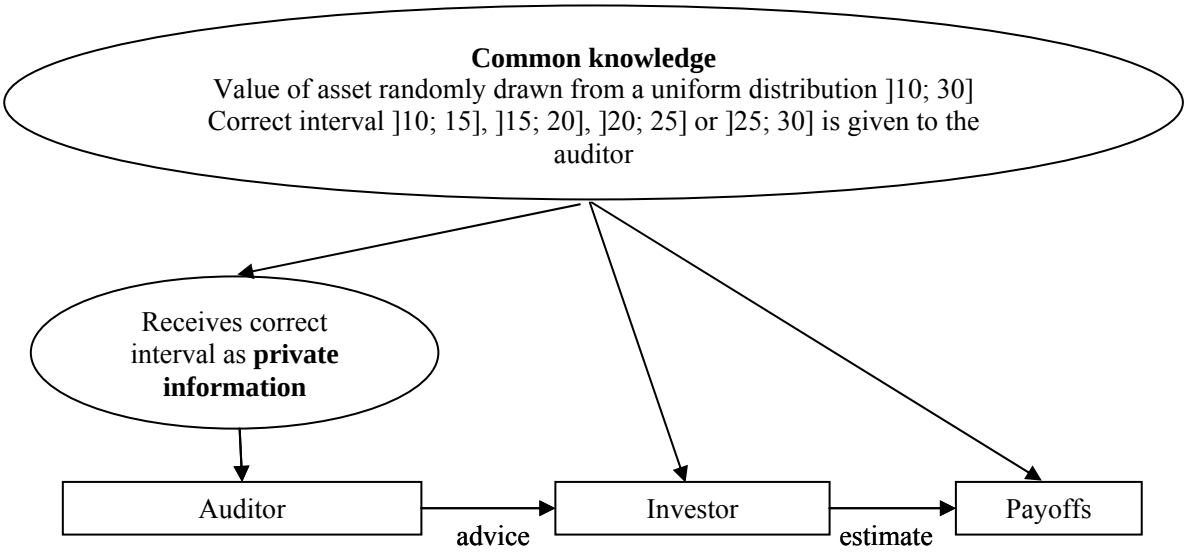
Whisenant, S., S. Sankaraguruswamy and K. Raghunandan. Evidence on the Joint Determination of Audit and Non-Audit Fees. *Journal of Accounting Research*. 41 (4): 721-744.

Wilson Jr., T. E. and R. A. Grimlund. An Examination of the Importance of an Auditor's Reputation. *Auditing*. 9 (2): 43-59.

Figure 1
Auditor-Investor-Game

The value of the asset is randomly distributed between 10.01 and 30.00. The auditor receives private information about the interval in which the value of the asset is located. The auditor is free to send any advice in the possible range of values of the asset to the investor. The investor has the task to estimate the value of the asset having information about the distribution of possible asset values and the advice of the auditor. The investor is paid based on the accuracy of his estimate. The auditor is paid based on the overestimation of the investor. The minimum payment for investors and auditors is 0 taler and the maximum payment is 5 taler. At the end of the session, one of the nine periods is randomly drawn as the payoff period. One taler is then converted into 3.00 Euro and a show-up fee of 2.50 Euro is added to compute the total payoff.

$\Pi_{auditor}$ and $\Pi_{investor}$ show the respective formulas for calculating the payoff of the auditor and the investor.



$\Pi_{auditor} = \max\{\min\{estimate_{investor} - value, 5\}, 0\}$ $\Pi_{investor} = \max\{5 - |estimate_{investor} - value|, 0\}$

Figure 2

Replication of Cain, Loewenstein, and Moore (2005)

The replication of CLM is defined as the first period of the stranger matching.

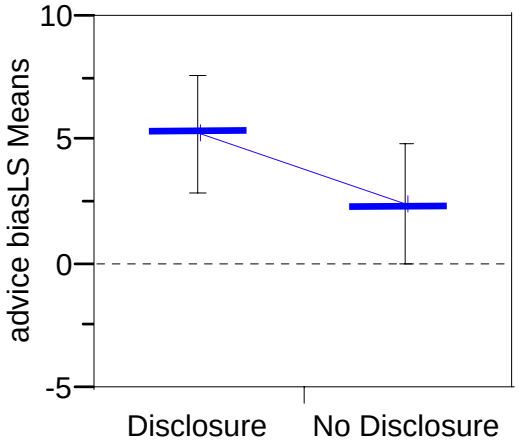
Expected value of the asset = mean of the interval provided to the auditor as private information = mean of the interval from which the value of the asset was drawn.

Advice bias = advice of auditor – expected value of the asset

Estimate bias = estimate of investor – expected value of the asset

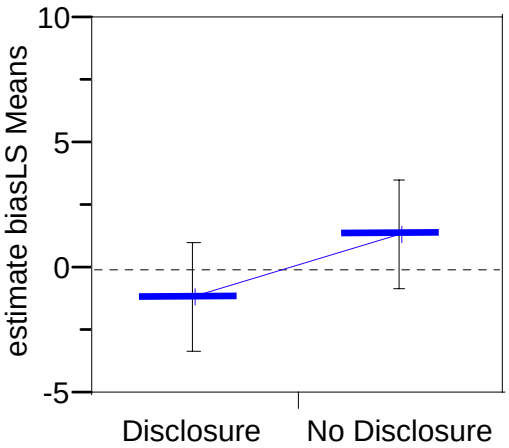
The respective LS means are shown by horizontal bars, the 95% confidence intervals are shown as vertical bars and the p-values controlled for the interval of the asset are reported.

Advice bias



p = 0.101

Estimate bias



p = 0.095

Figure 3

Experience setting

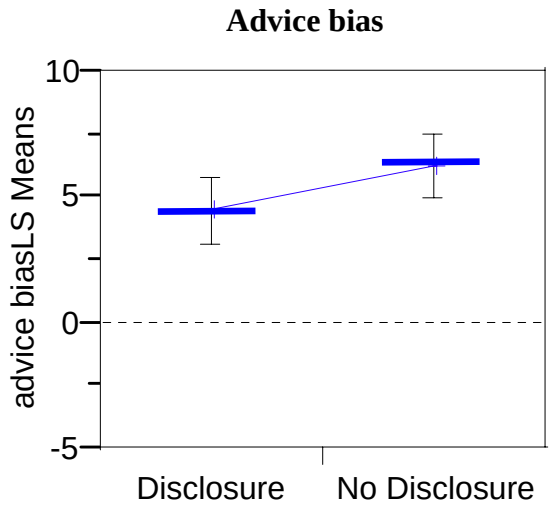
The experience setting is defined as the 2nd to 8th period of the stranger matching.

Expected value of the asset = mean of the interval provided to the auditor as private information = mean of the interval from which the value of the asset was drawn.

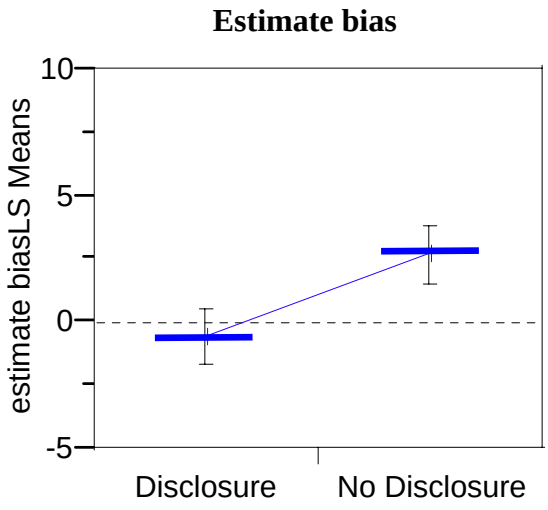
Advice bias = advice of auditor – expected value of the asset

Estimate bias = estimate of investor – expected value of the asset

The respective LS means are shown by horizontal bars, the 95% confidence intervals are shown as vertical bars and the p-values controlled for the interval of the asset are reported.



p = 0.056



p = 0.001

Figure 4
Effect of moral judgment on advice bias

The interaction of period and the moral judgment of the auditors are plotted. To extract the moral judgment, auditors were asked whether they think that giving upward biased advice is morally correct on a 7-step Likert scale. Answers from 1-3 were classified as "morally unconcerned", answers from 5-7 were classified as "morally concerned". Undecided subjects (answer=4) were excluded from this analysis.

Response variable is advice bias defined as advice minus expected value of asset according to private information of auditor. As giving upward biased advice is theoretically not possible if the value of the asset is in the highest interval, this interval was excluded for this analysis.

In the model it is controlled for the main effects of the concern about lying and disclosure. Additionally it is controlled for the interval of the asset (continuous), period of the game and the interactions of disclosure with the period of the game and the concern about lying (nominal).

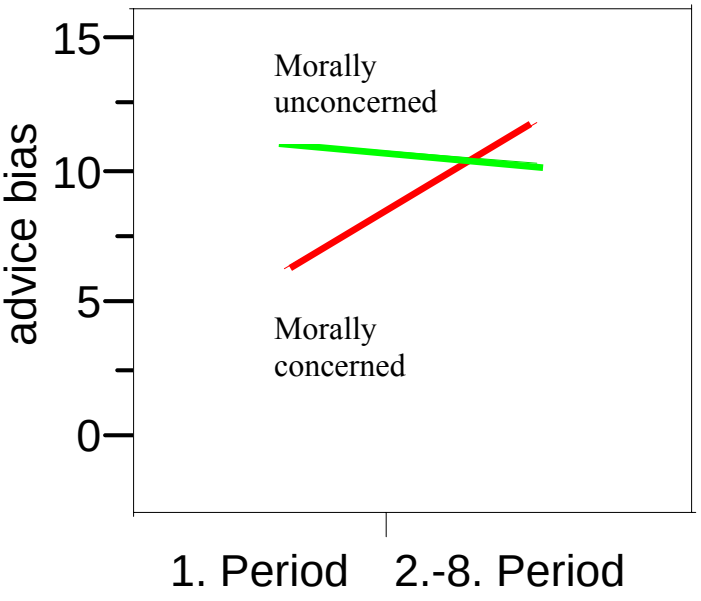


Figure 5

Reputation setting

The reputation setting is defined as the 2nd to 8th period of the partner matching.

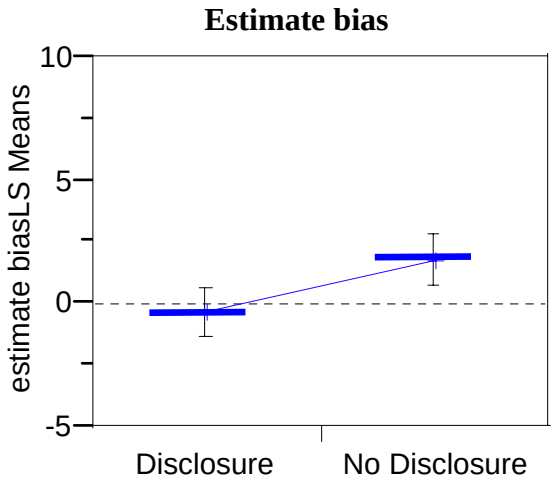
Expected value of the asset = mean of the interval provided to the auditor as private information = mean of the interval from which the value of the asset was drawn.

Advice bias = advice of auditor – expected value of the asset

Estimate bias = estimate of investor – expected value of the asset

The respective LS means are shown by horizontal bars, the 95% confidence intervals are shown as vertical bars and the p-values controlled for the interval of the asset are reported.

p = 0.640



p = 0.004

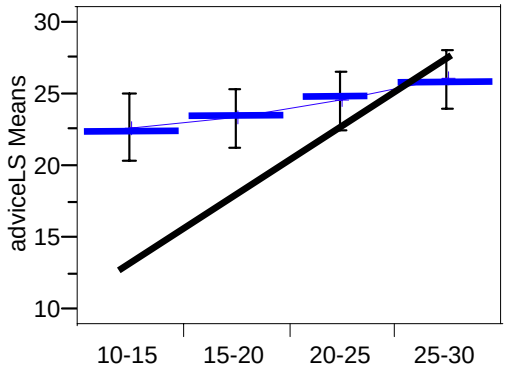
Figure 6

Advice for each interval of the value of the asset (reputation setting)

The means of advice controlled for subjects as a random effect are plotted for each of the four possible intervals of the value of the asset. The solid line indicates the true values (slope of this line equals the length of interval (=5)).

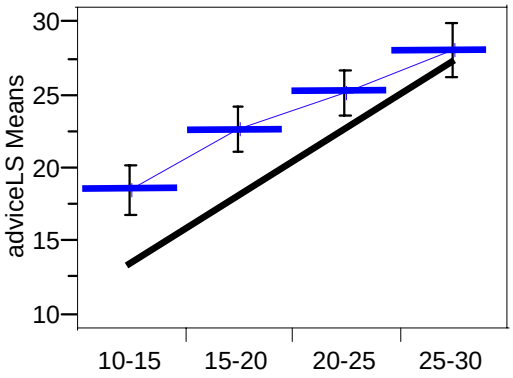
The slopes in the no disclosure are significantly higher ($p<0.001$) which indicates a higher informativeness of the advice in the no disclosure condition

Disclosure



slope = 1.14 (SD = 0.39)

No Disclosure



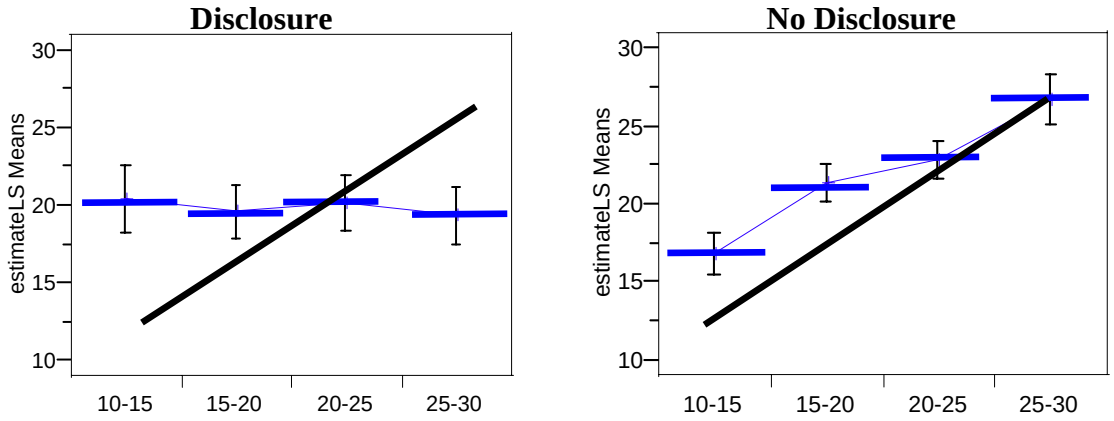
slope = 3.13 (SD = 0.37)

Figure 7

Estimate for each interval of the value of the asset (reputation setting)

The means of estimate controlled for subjects as a random effect are plotted for each of the four possible intervals of the value of the asset. The solid line indicates the true values (slope of this line equals the length of interval (=5)).

The slopes in the no disclosure are significantly higher ($p<0.001$) which indicates a better calibration of the estimate in the no disclosure condition.



slope = -0.24 (SD = 0.41)

slope = 3.06 (SD = 0.33)

Table 1
Comparison of experimental design

	Cain, Loewenstein, and Moore (2005)	**This study**
Task	Estimating value of a jar of coins	Estimating value of an asset
Value of Asset	Six jars with values from \$10 to \$30	Random value from a uniform distribution from 10.01 to 30.00
Incentives	Base Payment 7.50 \$ Variable Payment 0–5.50\$ Min: 7.50\$ Max: 13.50\$ Random period is paid out	Base Payment 2.50 € Variable Payment 0–15 € Min. 2.50€ Max. 17.50€ Random period is paid out
Auditors' payoff function	$\Pi_{auditor} = \begin{cases} 0 & \text{estimate - value} < 0.5 \\ 1.00 & 0.5 \le \text{estimate - value} \le 1 \\ 1.90 & 1.01 \le \text{estimate - value} \le 1.50 \\ \dots & \dots \\ 5.50 & \text{estimate - value} \ge 5.01 \end{cases}$	$\Pi_{auditor} = \max\{\min\{\text{estimate}_{investor} - \text{value}, 5\}, 0\}$
Investors' payoff function	$\Pi_{investor} = \begin{cases} 5.00 & \lvert\text{estimate - value}\rvert \le 0.50 \\ 4.50 & 0.51 < \lvert\text{estimate - value}\rvert \le 1.00 \\ 4.00 & 1.01 < \lvert\text{estimate - value}\rvert \le 1.50 \\ \dots & \dots \\ 0.00 & \lvert\text{estimate - value}\rvert \ge 5.01 \end{cases}$	$\Pi_{investor} = \max\{5 - \lvert\text{estimate}_{investor} - \text{value}\rvert, 0\}$
Control question	No	Yes
Disclosing conflict of interest	„Auditor is paid on how high you estimate“	Detailed information about incentive scheme and control questions
Information of the auditor	Observing physical jar of coins closely (true values: \$10.01, \$12.50, \$15.58, \$19.83, \$24.00, \$27.06)	Interval in which the value of the asset is uniformly distributed:]10,15];]15,20];]20,25];]25,30]
Information of the investor	Observing jar from the distance	Information of the total interval of]10,30] and its distribution
Passing on of advice	Shuffeling of advice forms; 6 periods with 3-5 auditors	Matching by computer: I. 9 periods total stranger matching II. 9 periods partner matching
Feedback	No feedback in periods 1-3. Limited feedback in periods 4-6.	Feedback about the value of the asset and the payout payoff after each period.

Table 2
Experimental treatments

Experimental treatments[*]	Disclosing conflict of interest to investor	
	Disclosure	*No disclosure*
Replication of CLM (1st period of the *perfect stranger matching*)	2 sessions = 18 cohorts = 36 subjects	2 sessions = 18 cohorts = 36 subjects
Experience setting (2nd to 8th period of the *perfect stranger matching*)		
Reputation setting (2nd to 8th period of the *partner matching*)	2 sessions = 20 cohorts = 40 subjects	2 sessions = 18 cohorts = 36 subjects

[*]The study included 148 subjects; each subject participated in 9 rounds

Table 3
Descriptive and test statistics for advice and estimates for all settings

Advice = auditors' advice
Advice bias = advice – expected value of the asset
Exp. estimate = estimate of the investor as expected by the auditor
Estimate = investors' estimate
Estimate bias = estimate – expected value of the asset
Estimate error = | estimate – expected value of the asset|
Expected value of the asset = mean value of the interval of the asset
n=20 for the disclosure/reputation setting, for all other settings n=18

			Disclosure Mean (SD)		No disclosure Mean (SD)	
Replication: 1st period perfect stranger matching	Auditor	Advice	26.28	(4.37)	23.35	(6.11)
		Advice bias	5.17	(5.19)	2.52	(7.07)
		Exp. estimate	22.61	(3.93)	22.72	(4.68)
	Investor	Estimate	19.81	(4.21)	22.36	(4.85)
		Estimate bias	-1.31	(5.97)	1.53	(7.15)
		Estimate error	4.53	(3.97)	5.31	(4.87)
Experience: 2nd-8th period perfect stranger matching+	Auditor	Advice	24.72	(2.97)	26.44	(2.22)
		Advice bias	4.40	(4.12)	6.28	(3.06)
		Exp. Estimate	21.83	(2.84)	23.37	(2.22)
	Investor	Estimate	19.67	(2.55)	22.94	(2.37)
		Estimate bias	-0.65	(2.21)	2.78	(2.37)
		Estimate error	5.88	(1.08)	6.04	(1.13)
Reputation: 2nd-8th period partner matching+	Auditor	Advice	24.24	(3.38)	23.39	(2.99)
		Advice bias	3.71	(3.76)	3.87	(1.95)
		Exp. Estimate	20.89	(1.91)	21.88	(2.33)
	Investor	Estimate	19.87	(2.31)	21.68	(2.66)
		Estimate bias	-0.67	(2.63)	2.16	(1.57)
		Estimate error	5.89	(1.82)	3.47	(1.36)

+Qualitative results hold when using 1st-9the or 2nd-9th period data

Table 4
Average payoffs by disclosure for the experience and reputation setting

Payoffs are the average hypothetical payoffs for the 2nd to 8th period. They are hypothetical payoffs, because not the average payoff was paid out but only one period was randomly chosen as the payoff period at the end of the session.

Coordination is measured by dividing the total of auditors' and investors' average payoff by the total maximum payoff of five taler.

n=20 for the disclosure/reputation setting, for all other settings n=18

		Disclosure payoffs mean (SD)	No disclosure payoffs mean (SD)	*p (two-sided t-test)*
Experience:	Auditor	1.66 (0.62)	2.65 (0.81)	0.001**
2nd-8th period perfect	Investor	1.23 (0.54)	1.20 (0.53)	0.858
stranger matching+	Coordination	57.6%	77.0%	0.004**
Reputation:	Auditor	1.60 (0.69)	2.18 (0.72)	0.017**
2nd-8th period partner	Investor	1.48 (0.63)	2.10 (0.73)	0.009**
matching+	Coordination	61.8%	85.6%	0.001**
Experience vs.	Auditor	0.837	0.061*	
reputation	Investor	0.273	0.001**	
p (two-sided t-test)	Coordination	0.537	0.019**	

*(**) significant at the 10% (5%)-level.

+Qualitative results hold when using 1st-9th or 2nd-9th period data (see Appendix A)

Table 5
Generalized Least Squares (GLS) random effects regression analysis of the determinants of advice bias and estimate bias

Disclosure (nominal) = 1 if disclosing conflict of interest, 0 otherwise
Reputation (nominal) = 1 if partners matching, 0 if strangers matching
1st Period (nominal) = 1 if period equals one, 0 otherwise
Period (continuous) = experimental period 1 to 9
Interval (continuous) = 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]
Estimate $Bias_{-1}$ (continuous) = Estimate bias of the investor with whom the auditor interacted in the previous period, empty in 1st period
Advice $Bias_{-1}$ (continuous) = Advice bias of the investor with whom the auditor interacted in the previous period, empty in 1st period
n=666 observations, 74 subjects as random effect

Coefficients	Dependent variable			
	Advice bias		Estimate bias	
	Mean	SD	Mean	SD
Intercept	11.28	(0.60)***	11.95	(0.58)***
Effects				
Disclosure	0.17	(0.40)	-1.19	(0.36)***
Reputation	-0.64	(0.40)	-0.08	(0.36)
1st Period	-0.07	(0.30)	-0.63	(0.32)*
Period	0.33	(0.07)***	-0.04	(0.08)
Interval	-3.32	(0.15)***	-4.17	(0.16)***
Disclosure*1st Period	0.53	(0.30)*	0.26	(0.32)
Disclosure*Period	-0.03	(0.07)	-0.00	(0.08)
Disclosure*Interval	-0.62	(0.15)***	-0.66	(0.16)***
Disclosure*Reputation	0.08	(0.37)	0.28	(0.33)
Reputation*1st Period	0.17	(0.30)	0.29	(0.32)
Reputation*Period	0.21	(0.07)***	0.09	(0.08)
Reputation*Interval	0.52	(0.15)***	0.63	(0.16)***
Reputation*Disclosure*1st Period	-0.55	(0.25)**	0.06	(0.26)
Reputation*Disclosure*Interval	-0.34	(0.15)**	-0.56	(0.16)***
Estimate $Bias_{-1}$	0.02	(0.03)		
Advice $Bias_{-1}$			-0.20	(0.03)***
Adjusted R^2	0.566		0.603	

*(**)[***] significant at the 10% (5%) [1%]-level

Appendix A

Translation of the 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 neighbor. 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.

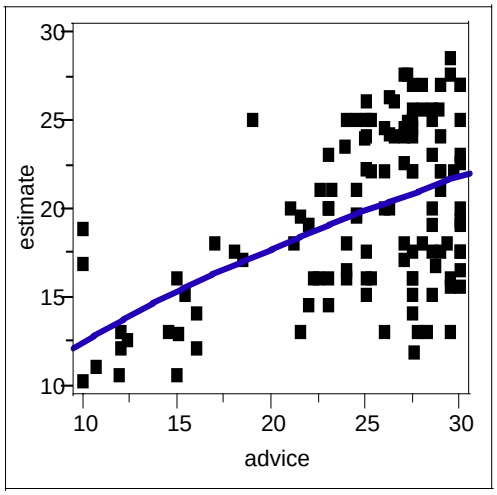
What is A's and B's payoff?

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

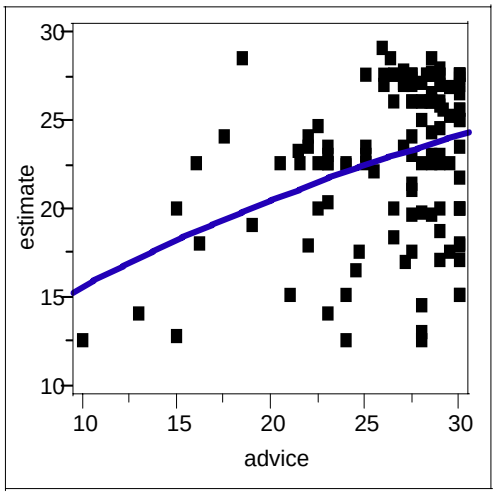
Appendix B: Not for publication

Figure 8
Correlation of auditors' advice and investors' estimates, square root function fitted

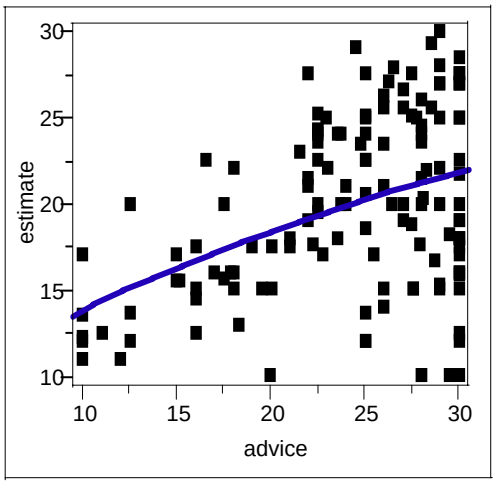
Disclosure (Experience condition)



No Disclosure (Experience condition)



Disclosure (Reputation condition)



No Disclosure (Reputation condition)

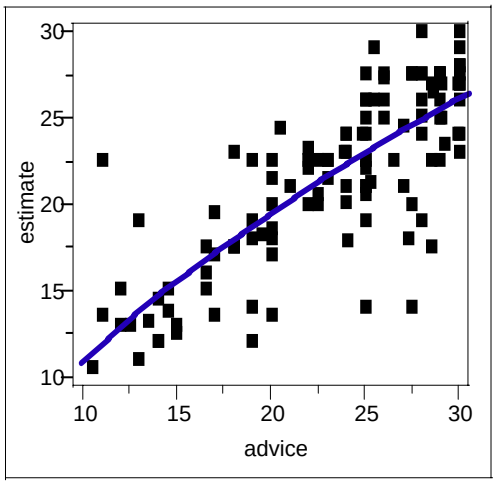


Table 3a
Descriptive and test statistics for advice and estimates, 1st-9th period

			Disclosure mean (SD)		No disclosure mean (SD)	
Experience: 1^{st}-9^{th} period perfect stranger matching	Auditor (n=18)	Advice	25.01	(2.78)	26.21	(2.23)
		Advice bias	4.46	(3.60)	5.90	(2.71)
		Exp. estimate	21.96	(2.56)	23.28	(2.10)
	Investor (n=18)	Estimate	19.72	(2.50)	22.72	(2.13)
		Estimate bias	-0.84	(2.05)	2.41	(2.21)
		Estimate error	5.64	(1.04)	5.99	(1.17)
Reputation: 1^{st}-9^{th} period partner matching	Auditor (n=18-20)	Advice	24.21	(3.56)	23.58	(2.79)
		Advice bias	3.60	(3.76)	3.89	(2.12)
		Exp. estimate	20.89	(1.91)	21.88	(2.33)
	Investor (n=18-20)	Estimate	19.76	(2.20)	21.72	(2.44)
		Estimate bias	-0.84	(2.29)	2.03	(1.70)
		Estimate error	5.86	(1.46)	3.76	(1.40)

Table 3b
Descriptive and test statistics for advice and estimates, 2nd-9th period

			Disclosure mean (st. d.)		No disclosure mean (st. d.)	
Experience: 2^{nd}-9^{th} period perfect stranger matching	Auditor (n=18)	Advice	24.86	(2.93)	26.57	(2.25)
		Advice bias	4.37	(3.96)	6.32	(2.84)
		Exp. estimate	21.88	(2.82)	23.35	(2.30)
	Investor (n=18)	Estimate	19.70	(2.42)	22.77	(2.29)
		Estimate bias	-0.78	(2.09)	2.52	(2.37)
		Estimate error	5.78	(1.32)	6.08	(1.26)
Reputation: 2^{nd}-9^{th} period partner matching	Auditor (n=18-20)	Advice	24.46	(3.44)	23.80	(2.84)
		Advice bias	3.80	(3.92)	4.18	(2.03)
		Exp. estimate	20.92	(2.13)	21.85	(2.43)
	Investor (n=18-20)	Estimate	19.72	(2.19)	21.77	(2.56)
		Estimate bias	-0.94	(2.41)	2.15	(1.66)
		Estimate error	6.11	(1.55)	3.82	(1.32)

Nr.	Author	Title
06-10	Christopher Koch Carsten Schmidt	Disclosing Conflict of Interest - Does Experience and Reputation Matter?
06-09	Clemens Kroneberg Volker Stocké Meir Yaish	Norms or Rationality? The Rescue of Jews, Electoral Participation, and Educational Decisions
06-08	Guido Cozzi Paolo Giordani Luca Zamparelli	An Uncertainty-Based Explanation of Symmetric Growth in Schumpeterian Growth Models
06-07	Volker Stocké	Explaining Secondary Effects of Familiesí Social Class Position. An Empirical Test of the Breen-Goldthorpe Model of Educational Attainment
06-06	Volker Stocké Tobias Stark	Trust in Surveys and the Respondentsë Susceptibility to Item Nonresponse
06-05	Clemens Kroneberg	The Definition of the Situation and Variable Rationality: The Model of Frame Selection as a General Theory of Action
06-04	Rainer Greifeneder Cornelia Betsch	Maximieren und Bedauern: Skalen zur Erfassung dipositionaler Unterschiede im Entscheidungsverhalten
06-03	Volker Stocké Christian Hunkler	Measures of Desirability Beliefs and their Validity as Indicators for Socially Desirable Responding
06-02	Anders Anderson	Is Online Trading Gambling with Peanuts?
06-01	Volker Stocké Tobias Stark	Political Involvement and Memory Failure as Interdependent Determinants of Vote Overreporting
05-43	Volker Stocké Tobias Stark	Stichprobenverzerrung durch Item-Nonresponse in der international vergleichenden Politikwissenschaft
05-42	Volker Stocké	Response Privacy and Elapsed Time Since

Nr.	Author	Title
05-41	Josef Hofbauer Jörg Oechssler Frank Riedel	Brown-von Neumann-Nash Dynamics:
05-40	Markus Glaser Thomas Langer Jens Reynders Martin Weber	Framing Effects in Stock Market Forecasts: The Difference Between Asking for Prices and Asking for Returns
05-39	Tri Vi Dang	Alternating Offer Bargaining with Endogenous Information: Timing and Surplus Division
05-38	Tri Vi Dang	On Bargaining with Endogenous Information
05-37	Patric Andersson	Overconfident but yet well-calibrated and underconfident: A research note on judgmental miscalibration and flawed self-assessment*
05-36	Peter Dürsch Albert Kolb Jörg Oechssler Burkhard Schipper	Rage Against the Machines:
05-35	Siegfried K. Berninghaus Hans Haller Alexander Outkin	Neural Networks and Contagion
05-34	Jacques Durieu Hans Haller Philippe Solal	Interaction on Hypergraphs
05-33	Markus Glaser Martin Weber	Which Past Returns Affect Trading Volume?
05-32	Zacharias Sautner Martin Weber	Corporate Governance and the Design of Stock Option Programs
05-31	Zacharias Sautner Martin Weber	Subjective Stock Option Values and Exercise Decisions: Determinants and Consistency
05-30	Patric Andersson Richard Tour	How to Sample Behavior and Emotions of Traders: