Disclosing Conflict of Interest – Does Experience and Reputation Matter?

Christopher Koch [a,*] & Carsten Schmidt [b]

[a] University of Mannheim, Mannheim Business School, Schloss Ostflügel, 68131 Mannheim, Germany
Research Fellow
University of Mannheim, National Research Centre on Decision-Making and Economic Modeling,
Research Fellow
Yale University, Yale Law School, Visiting Researcher (spring & summer 2007)

[b] University of Mannheim, National Research Centre on Decision-Making and Economic Modeling,
L 13, 15, 68131 Mannheim, Germany
Head of the Experimental Laboratory

1st Version: September 2006
This Version: July 2007

Abstract

Disclosure of conflict of interest is currently considered to be an effective tool for reducing the negative effects of impaired auditor independence. However, Cain, Loewenstein, and Moore (2005) suggest that instead, it has perverse effects. Using a controlled laboratory experiment, we confirm the validity of their results and provide evidence for moral licensing as a cause. With auditors' and investors' experience, the results reverse. Furthermore, we identify the disclosure of conflict of interest as a potential impediment to reputation formation.

JEL: C91, D82, D83, G28, M42

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

---

* Corresponding author. Tel.: +49 621 181 2351; fax: +49 621 181 1694. E-mail address: ckoch@bwl.uni-mannheim.de; christopher.koch@yale.edu. The paper is available at: http://ssrn.com/abstract=928927. We gratefully acknowledge the valuable comments received from two anonymous reviewers. The authors are also thankful to Daylian M. Cain, W. Robert Knechel, Brian Mittendorf, Jens Wüstemann, Jannis Bischof, Christine Nolder, and the participants of the Ph.D. research seminar at the Yale School of Management for helpful comments on earlier drafts of this paper. The paper has benefited from presentation at the 29th Annual Congress of the European Accounting Association in Dublin, Ireland, the 2006 Annual Meeting of the American Accounting Association, the 2006 European meeting of the Economics Science Association (ESA) in Nottingham, the 2006 German Experimental Economists Association (GEW) meeting in Magdeburg, and the 2007 Northeast Regional Meeting of the American Accounting Association in Hartford. The financial support of the German Research Foundation (DFG Grant SFB504) is greatly appreciated.

1. Introduction

Disclosure of auditors' fees has been mandatory in the US since February 2001 (SEC, 2000), and will be mandatory in all EU countries from 2008 on (European Union, 2006). Its aim is to ameliorate the potentially negative effects of high nonaudit fees on auditor independence. Disclosure is considered a viable solution by legislators, since it presumably bears little cost and has the potential benefit of making investors better informed in their investment decisions (SEC, 2000; 2003). This rationale is best described by the famous quote from the Supreme Court Justice Louis Brandeis that "sunlight is the best disinfectant" (Brandeis, 1967).

However, disclosing conflict of interest by disclosing auditors' fees may have unintended shortcomings. First, it may send the wrong signals to investors. While investors often believe that high nonaudit fees impair auditor independence, empirical research has not yet provided clear evidence that this is really the case (Francis & Bin, 2006). Such a potentially erroneous belief on the part of investors may also affect market efficiency, because evidence from experimental economics suggest that these beliefs can be very persistent even if feedback indicates otherwise (Davis & Hollie, Forthcoming; Dopuch, King & Schwartz, 2003). Second, and more important for our research question, Cain, Loewenstein and Moore (2005) (hereinafter referred to as CLM) provide experimental evidence that the disclosure of conflict of interest – in our context the disclosure of auditors' fees – has perverse effects on auditors' behaviour.[1] Their findings suggest that it gives auditors moral license to allow their independence to be impaired. Investors are worse off as a result, since they are not able to fully anticipate these perverse effects on auditors' behaviour.

We set up an experiment to test for the potentially perverse effects. We reduce the complexity of CLM's experimental design and construct a more controlled environment. In our setting, investors are paid based on the accuracy of their estimates for the value of assets. The information that they receive in order to conduct this task comes from a better-informed auditor. This shall be compared with a scenario in which auditors provide information to investors by certifying financial statements. Investors' and auditors' incentives are deliberately misaligned, as auditors have incentives to bias the investors upward in their estimates. As an experimental treatment, this conflict of interest is either disclosed to investors or not.

[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 extend the CLM experiment by testing for the influence of experience and reputation, two factors which are often considered to be the main characteristics of the auditing environment (Bonner & Lewis, 1990; Chaney & Philipich, 2002; Wilson Jr. & Grimlund, 1990). We introduce experience by repeating the task over nine periods and providing detailed feedback after each period. We investigate reputation by either letting each auditor interact with the same investor over all periods, or letting each auditor interact with a different investor in each period.

Our experiment has three main contributions. First, even with our more controlled design, we can confirm the finding of CLM that the disclosure of conflict of interest can have perverse effects on auditor independence in a setting of inexperienced subjects. We are able to provide evidence that this behaviour can be traced to moral licensing. Second, the effects of this disclosure on auditors' independence reverse with experience: after having received feedback, auditors send less biased advice to the investor in the disclosure condition than in the no disclosure condition, as predicted by economic theory. We find evidence that this reversal might be explained by a diminished influence of moral licensing and a stronger influence of economic factors. Third, we find that the disclosure impedes the opportunities for auditors to form a reputation for credibility: auditors provide less useful advice and investors provide less accurate estimates.

With our findings, we contribute to experimental economics research on auditor independence (Calegari, Schatzberg & Sevcik, 1998; Dopuch & King, 1991; Dopuch, King & Schwartz, 2001; Schatzberg, 1990; Schatzberg & Sevcik, 1994) and auditor reputation formation (King, 1996; Mayhew, 2001; Mayhew, Schatzberg & Sevcik, 2001). More specifically, we connect to the research of Dopuch, King and Schwartz (2003) and Davis and Hollie (forthcoming), who test for the effect of disclosure of auditors' fees on the efficiency of investor decision making. In our experiment, we test additionally for the effects that disclosure has on auditor independence by using human auditors instead of robot auditors as in the earlier studies. More generally, we contribute to the literature addressing the common phenomenon of conflict of interest in professional advisor-client relationships, e.g. financial analysts (SEC, 2003), physicians (Kassirer, 2001), real estate agents (Levitt & Syverson, 2005), and insurance agents or salesmen (Shapiro, 2003).

This paper proceeds as follows. In the next section, we discuss arguments for mandating disclosure of audit fees and the potential shortcomings of this solution. In section 3, we present economic and psychological theories on the effects of disclosure of conflict of

interest, and derive our hypotheses for the settings of inexperienced and experienced subjects. In section 4, we describe our research design and compare it to the one used by CLM. In section 5, we report the results of our quasi-replication of the CLM experiment, and additionally the effects of experience and reputation in this setting. We conclude the paper by summarizing our results, discussing possible implications for the audit environment, and addressing limitations of the study.

2. Disclosure as one solution to the conflict of interest

2.1. The reasons for mandating the disclosure of auditors' fees and empirical evidence

Auditors' fees might create an economic bond between the auditor and the audited company (DeAngelo, 1981; Watts & Zimmerman, 1981). Legislators in both the US and the EU worry that this is especially true for fees from nonaudit services. The concern is that management might abuse its power to award nonaudit services by rewarding dependent auditors (SEC, 2000). This concern was fuelled by the rising proportion of nonaudit fees to total auditor fees in the 1990s (SEC, 2000).

To tackle this problem, the SEC followed a "two-pronged approach" in 2000 (SEC, 2000). Nonaudit services were limited, and the disclosure of all auditors' fees was mandated. In 2003, a third prong was added by requiring the audit committees' preapproval for nonaudit services. Furthermore, the requirements outlined by the 2000 SEC regulation were tightened. The provisions regarding nonaudit services were further restricted, and the categorization of auditor fees changed. Now, the disclosure must take place separately for audit fees, audit-related fees, tax fees, and all other fees for the two most recent fiscal years (SEC, 2003).

The 2006 EC Directive also requires the disclosure of auditors' fees as stated in Article 49. The 27 member states of the European Union are required to transform EC directives within 18 months. Therefore, the disclosure of auditors' fees shall be mandatory in all states of the European Union by 2008 at the latest.

The aim of such disclosure is to "shed light on the independence of public companies' auditors [to] assist investors in making investment […] decisions" (SEC, 2003). The hope is that "investors will be able to evaluate for themselves whether the proportion of fees for audit and non-audit services causes them to question the auditor's independence" (SEC, 2000).

A recent empirical study by Francis and Ke (2006) suggests that the disclosure of audit fees provides new information to investors, and that investors react to this information. Before the disclosure of auditors' fees, the market reacted in the same way to earnings surprises of

companies that paid high nonaudit fees to auditors as to earnings surprises of companies that did not. Once the disclosure was mandatory, the market did react comparatively less strongly to the earnings surprises of companies that paid high levels of nonaudit fees.

The result suggesting that investors perceive independence to be impaired when the auditor receives high levels of nonaudit fees is supported by an extensive body of literature. In surveys, a significant number of respondents raised this concern (Hartley & Ross, 1972; Lavin, 1977; Pany & Reckers, 1984). Experiments using hypothetical scenarios showed that high levels of nonaudit fees can also affect the behaviour of decisionmakers. Investors and lenders were more sceptical about providing money to companies where the auditor received high nonaudit fees (Dykxhoorn & Sinning, 1982; Lowe & Pany, 1995; Pany & Reckers, 1987; Shockley, 1981). Recent empirical studies add further evidence showing that high nonaudit fees affect market data, e.g. share prices, bond prices, and responsiveness of share prices to earnings (Brandon, Crabtree & Maher, 2004; Higgs & Skantz, 2006; Khurana & Raman, 2006; Krishnan, Sami & Zhang, 2005; Mishra, Raghunandan & Rama, 2005).

2.2. Potential shortcomings of the disclosure of auditors' fees

One potential shortcoming of the disclosure of auditors' fees is that it might send the wrong signals to investors. Investors' belief regarding impaired auditor independence in the case of high nonaudit fees might not correspond with reality, since empirical research has not yet provided clear evidence for this. The findings for 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, are mixed. While Frankel, Johnson and Nelson (2002) find a positive association, Ashbaugh, LaFonde, and Mayhew (2003), Chung and Kallapur (2003), Larcker and Richardson (2004) show that this may hold only for certain companies. This is also supported by other studies that do not find a significant relationship (Antle, Gordon, Narayanamoorthy & Zhou, 2006; Craswell, Stokes & Laughton, 2002; DeFond, Raghunandan & Subramanyam, 2002; Higgs & Skantz, 2006; Huang, Mishra & Raghunandan, 2007; Kinney Jr., Palmrose & Scholz, 2004; Reynolds, Deis & Francis, 2004; Ruddock, Taylor & Taylor, 2006).

Potential misinformation by investors from the disclosure of nonaudit fees might affect market efficiency. Dopuch, King, and Schwartz (2003) show this to be a possibility, since people tend to be belief persistent; they often have difficulties in correcting their beliefs, even after having received feedback indicating that those beliefs are wrong. The results are

supported by an extension of that experiment, conducted by Davis and Hollie (2007) who also find that the disclosure of nonaudit fees has detrimental effects on market efficiency.

When investors perceive auditor independence to be lower in the context of nonaudit services, and then subsequently receive information about the provision of nonaudit services, they will demonstrate less trust in the certified financial statement of the audited company. Therefore, companies will be more reluctant to purchase nonaudit services, and auditors more reluctant to offer them. This outcome is an express intention of the SEC, at least for those nonaudit services that have a detrimental effect on auditor independence (SEC, 2000). However, this might also carry negative effects, insofar as positive spillover effects exist from audit services to nonaudit services and vice versa. That is also one reason why the SEC ended its policy of mandating auditors' fees disclosure in 1982, following its introduction in 1978, due to its concerns about an "unwarranted curtailment of nonaudit services" (SEC, 1982, p. 3810). In the UK, nonaudit fees also decreased after the introduction of disclosure requirements in 1991 (Iyer, Iyer & Mishra, 2003). The current drop in nonaudit service fees today (Khurana & Raman, 2006) might also be attributed to some degree to the requirement of their disclosure. Experimental research suggests that independent audit committees fear approving even those nonaudit services that they consider valuable, due to concerns about a negative effect on investors' perception of independence (Gaynor, McDaniel & Neal, 2006).

3. Theory and hypotheses of the effects of disclosure of conflict of interest

3.1. Economic theory

Crawford and Sobel (1982) describe a theoretical framework for analyzing sender-receiver relationships. A sender (the auditor) has private information and transmits a message to a receiver (the investor) who has the task of estimating the value of a quantity. The incentives of both parties are common knowledge. This model can be used to predict how the behaviour of the sender and receiver will depend on the alignment of their respective incentives. It has also been regularly used in the accounting context, for example, to examine the role of multiple users of the message (Newman & Sansing, 1993), the effects of proprietary costs of private information (Gigler, 1994), and imperfect private information (Fischer & Stocken, 2001).

In the auditing context, the Crawford and Sobel model can be applied to the scenario in which the auditor's incentives are common knowledge, e.g. when the disclosure of auditors' fees is mandatory. The model predicts that when the incentives of the auditor and the investor

are fully aligned, the auditor (referred to as she) reports truthfully, and the investor (referred to as he) fully trusts her advice. When the incentives are misaligned beyond a certain threshold, the auditor's report is completely uninformative and should be ignored by investors. As the level of alignment increases below this threshold, the report should gradually become more useful, and investors in turn should rely more and more on this report.

The role played by non-disclosure of incentives has not yet been analyzed in this framework. One reason might be that such an analysis would presuppose investors' beliefs about auditors' incentives in the absence of relevant information. Recent research in experimental economics suggests that people lacking information about the incentives of the other party trust that party's report by default (Gneezy, 2005). The results of the empirical study of Francis and Ke (2006), described above, is consistent with this finding. Investors trusted the certified financial statement as long as they were not informed that the company had paid high nonaudit fees to auditors. Therefore, it seems reasonable to assume that the investor in the no disclosure condition trusts the auditor, at least a priori. This gives the auditor the opportunity to exploit the trust of the investor and to gain higher payoffs by sending upward-biased advice.

To derive economic predictions about the effects of disclosure of conflict of interest, we compare the predictions derived above for the no disclosure and the disclosure conditions. This involves an assumption that the incentives of the auditor and the investor will be misaligned above the specific threshold described above, given that this is the condition we examine in our experimental design. Investors will be making more upward-biased estimates in the no disclosure condition, as they trust the auditors' report to some extent, while they will ignore auditors' report in the disclosure condition. Auditors will send more upward-biased advice in the no disclosure condition, since they can exploit investors' trust, while giving completely uninformative advice in the disclosure condition, since they anticipate that investors will ignore their advice. These predictions are consistent Brandeis's metaphor of sunlight and disinfectant: the disclosure of conflict of interest is predicted to eliminate the bias in auditors' report, as well as in investors' estimates.

3.2. Psychological theory

CLM argue that disclosure of conflict of interest might have psychological "perverse effects". They provide two main reasons why auditor independence will suffer when auditors' fees are disclosed. First, those auditors feel morally licensed to have their independence

impaired when investors are informed of the auditors' incentives to do so. They will consider it "fair play" to betray the investor when he is expecting such behaviour. Comparably, the cost of lying (Gneezy, 2005) will be removed when auditors consider their impaired independence to no longer constitute lying. Second, auditors will allow more aggressive earnings management in the disclosure condition in order to counteract a potentially higher level of investor scepticism. As CLM put it, they will shout even louder if investors informed of these incentives cover their ears.

CLM also suggest that investors are worse off when a conflict of interest is disclosed. They argue that investors will be unable to ignore the information sent by the auditor, due to the curse of knowledge bias (Camerer, Loewenstein & Weber, 1989; Tversky & Kahneman, 1974). As auditors' advice becomes more upward-biased in the disclosure condition, as described above, the investors' estimate will be biased upward accordingly.

3.3. Effects of experience and disclosure of conflict of interest

On the question of the effects of disclosure of conflict of interest on auditor independence, psychological theory and economic theory provide opposite answers. CLM found that psychological factors could explain the results of their quasi-one-shot experiment. One reason for that might be that many people do not fully consider strategic interactions and are unable to accurately predict the behaviour of the other party (Camerer, Ho & Chong, 2003). Receiving feedback and gaining experience could change subjects' behaviour over time. It might be expected that the predictive power of economic theory increases when subjects receive information about the behaviour of the other party, while the predictive power of the psychological theory decreases when subjects learn the costs associated with non-optimal behaviour. Recent field research in economics also suggests that psychological findings dominate in the short-term, but that economic theory has a higher predictive power for long-term behaviour (Gneezy & List, 2006).

Following this reasoning, we predict that psychological factors will dominate in settings with inexperienced subjects as observed earlier by CLM. Firsts, subjects will be more likely to use heuristics and consider ethical factors in the first period, given that they do not fully anticipate the costs associated with this behaviour (Gneezy 2006). The auditor will be more reluctant to exploit the investor by sending upward biased advice in the no disclosure condition, while feeling morally licensed to do so in the disclosure condition. The investor will be prone to blindly accepting her advice, resulting in a higher upward bias in the

disclosure condition where the auditor feels morally licensed to exploit his trust. Second, limited strategic reasoning will lead the auditor to bias her advice upward in the disclosure condition, not anticipating that the investor should fully ignore that advice according to economic theory. The investor will actually follow the advice of the auditor in this condition, not anticipating that advice should not be useful for him according to economic theory.

We predict that the behaviour of experienced subjects will follow economic theory. In the no disclosure condition, the auditor will begin to give more upward biased advice, having learned that she forgoes compensation by giving accurate advice. In the disclosure condition, the auditor will have learned that her advice is ignored by the investor, and will begin to provide more uninformative and therefore also less upward biased advice. Furthermore, the auditor in the latter condition might realize that the investor who was informed of her payout scheme can fully identify her behaviour as unfair. This can be summed up by the observation that "people might be greedy, but at least wish to appear fair" (Güth, Huck & Ockenfels, 1996). This could constitute an additional reason why the auditor in the disclosure condition would provide less upward biased advice. Meanwhile, with experience, the investor will be less upward-biased with his estimates in the disclosure condition, having learned that it is too costly for him to simply rely on the auditor's advice. Nevertheless, he will remain somewhat upward-biased and will continue trusting the auditor at least to some degree, since the feedback he receives does not fully reveal the auditors' motivation.

H1a: Without experience, auditors are more upward biased in their advice in the disclosure condition than in the no disclosure condition, as predicted by psychological theory.

H1b: With experience, auditors are less upward biased in their advice in the disclosure condition than in the no disclosure condition, as predicted by economic theory.

H2a: Without experience, investors are more upward biased in their estimates in the disclosure condition than in the no disclosure condition, as predicted by psychological theory.

H2b: With experience, investors are less upward biased in their estimates in the disclosure condition than in the no disclosure condition, as predicted by economic theory.

3.4. Effects of reputation and disclosure of conflict of interest

The framework of Crawford and Sobel (1982) has been extended by some economic models to consider repeated interaction. However, these models have concentrated on issues unrelated to our research question, for example, how a sender can convey to the receiver that he is well informed in a setting of aligned incentives (e.g. Morris 2001, Ottivani 2005).

There is an extensive body of general research on reputation in economics. One finding is that the formation of reputation is very context-sensitive (Davis & Holt, 1993, p. 396). As such, experimental economics research in auditing has attempted to identify conditions that foster or impede the formation of reputation for credibility. This research has found that reputation formation occurs more frequently when auditors' misbehaviour imposes higher costs on investors (King, 1996), when accounting standards are less ambiguous (Mayhew et al., 2001), when investors receive timely (Mayhew, 2001) and precise feedback (Schwartz & Young, 2002) about auditors' behaviour.

Disclosure of conflict of interest might be a factor that hinders reputation formation. It might make investors more sceptical, which would make it more difficult and more costly for auditors to build up reputation.

RQ1: Do auditors in the no disclosure condition issue more credible advice than auditors in the disclosure condition?

RQ2: Do investors in the no disclosure condition have more faith in the report of the auditor?

4. Method

4.1. The game

The general structure of the auditor-investor game is as follows. The auditor (she) receives private information about the value of an asset. She gives advice about the value of the asset to the investor (he). He provides an estimate on the value of the asset. [2] In the experiment, misaligned incentives of the two players are induced: the advisor has incentive to overestimate the value of the asset, and the investor has incentive to estimate correctly.

[2] The game incorporates the main features of the following audit scenario. The auditor receives, through her work, rather precise information about the value of an asset. At the end of the audit process, she sends information to the investor indirectly by certifying the financial statement. Apart from the financial statement, the investor has very little information of his own on which he can base his investment decision.

The structure of our experimental implementation of this game is shown in figure 1. It is common knowledge that the value of a hypothetical asset is randomly drawn from values uniformly distributed between 10.01 and 30.00. The auditor receives as private information the interval in which the asset value is located. The interval is restricted to one out of]10,15],]15,20],]20,25], or]25,30]. This procedure of the provision of private information is common knowledge. Both the auditor's advice and the investor's estimate are point values, and can take on any value between 10.01 and 30.00.

The investor is paid based on the accuracy of his estimate, which is common knowledge. He receives the maximum payment of 5 taler if he estimates the value of the asset correctly. If he estimates the value as 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 is intended to mirror an environment in which investors do better the more accurately they estimate the value of assets.

The auditor is paid based on how much the investor overestimates the value of the asset. The auditor receives the difference between the estimate of the investor and the value of the asset in taler. She receives the maximum payment of 5 taler when the investor overestimates the value by 5 units or more, and the minimum payment of 0 taler when the investor underestimates the value by any amount.

--- Insert figure 1 about here ---

4.2. Experimental manipulation

The first experimental manipulation is the disclosure to the investor of the auditor's conflict of interest. In the disclosure condition, the investor is precisely informed of the incentives of the auditor. In the no disclosure condition, the investor receives no information at all about the auditor's incentive scheme. He only knows that the auditor is played by one of the other participants. In both conditions, the auditor knows whether the investor is informed about her incentives or not. The disclosure condition shall represent the situation in which the disclosure of fees is mandated; the no disclosure condition shall represent the situation in which it is not mandated.

The second experimental manipulation is the matching of auditors and investors. In all conditions, one auditor interacts with one investor in each period. In the experience condition,

each auditor interacts with the same investor only once (perfect stranger matching). In the reputation condition, the auditor interacts with the same investor over all nine periods (partner matching). We believe that the interaction with the same partner over all nine periods, coupled with precise feedback after each period, provides the opportunity for auditors to build up reputation. We believe so despite the fact that the subjects have knowledge of the number of periods, since experimental economics has shown that subjects usually do not think more than one or two periods ahead (Camerer, Ho & Chong, 2004). We choose not to use alternative designs, e.g. randomly determining the last period or not informing the participants about the number of periods, as these schemes also have their disadvantages (Boatsman & Grasso, 1992).

--- Insert table 1 about here ---

4.3. Experimental procedure

The experiment was conducted at the experimental lab of [*deleted to ensure a blind review*] using subjects recruited from the mailing list of the experimental lab. The decision forms were computerized using zTree (Fischbacher, 1999).[3] We conducted 8 sessions in a 2 x 2 between-subject design, with two sessions for each of the four treatment conditions. In each session 18 to 20 subjects participated, to whom the role of either auditor or investor was randomly assigned in equal proportion. Altogether, 148 subjects participated and all of them completed the session. We used review questions to make sure subjects understood the game and provided a brief post-experimental questionnaire at the end of the experiment. Altogether, the game was played nine periods in either total stranger or partners matching with detailed feedback after each round. Each session lasted about 40 to 50 minutes, with payoffs for each subject ranging from 2.50 € (3.00 $) to 17.50 € (21.00 $). Average payoffs were 8.30 € (10.00 $) which clearly covered opportunity costs of subjects. To determine payoffs, one of the nine periods was randomly selected in each session. The conversion rate of the paid out period was 1 Taler = 3 Euro.

4.4. Comparison to the design of Cain, Loewenstein, and Moore (2005)

Our experimental design is an abstract adaptation of the one used by CLM (2005). While subjects are labelled “advisors” and “estimators” in CLM (2005), they are labelled “type A”

[3] The program code of the experiment described in this paper is available from the authors upon request.

and “type B” in our experiment to avoid the confounding effects of framing. While estimators in CLM (2005) estimate the value of coins in a jar, investors in our experiment estimate the value of a randomly drawn number.

We extend the design of CLM (2005) by introducing opportunities for gaining experience and forming reputation. We test for the effects of experience by playing 9 periods and providing precise feedback about the value of the asset after each period. Although CLM (2005) also use a multi-period setting consisting of 6 periods, they essentially adopt a one-shot design, where subjects received either no feedback (for the first 3 periods) or virtually useless feedback (for the last 3 periods). This latter feedback was the information about the true value of the jar of coins. It was virtually useless to the investor due to a major flaw in the experiment which related to the way in which auditors received private information. Since auditors were untrained in estimating the value of jars of coins, they tended to underestimate it. They received the first feedback about the true value of the jar of coins after the fourth period and thus had only two periods left for gaining experience. The consequence was that auditors regularly assumed that they would be sending advice above the true value, even though their advice was actually at or below the true value. Therefore, investors receiving feedback about the true value often thought that auditors wanted to provide accurate or downward biased advice, which they did not. In their data evaluation CLM account for this bias by comparing to a baseline treatment of completely aligned incentives. Since we induce auditors’ private information in a computerized way, a bias is ruled out in our experimental design. This is also one reason why we omit a baseline with complete aligned incentives.

We test for the effects of reputation by manipulating the way in which auditors and investors are matched. As described above, we clearly distinguish between a condition in which auditors are never matched with the same investor again, and a condition in which they are matched with the same investor over all periods. In contrast, CLM (2005) implemented only non-perfect stranger matching. CLM vary the feedback they provide to subjects. Out of the six periods played, subjects received no feedback for the first three periods, and limited feedback for the last three periods. Therefore, the setting of CLM (2005) can be broadly described as one in which subjects have not yet received any feedback, and in which subjects do not expect to be rematched with the same partner. As this setting is comparable to the first period of the experience condition in our experiment, we consider that period to be a quasi-replication of the CLM experiment in a more controlled environment.

Furthermore, we try to enhance the power of CLM's original experimental design in two ways. First, at the beginning of the experiment, we administer and check review questions in order to ensure the comprehensibility of the structure of the experimental structure and the incentive schemes known to the respective subject.. Second, we increase variability of payoffs by putting more weight on variable payoffs and less weight on show-up fees. For example, for the period that was selected for payment, investors received in our experiment about $20 instead of the $5 in the CLM experiment for estimating the asset value correctly.

--- Insert table 2 about here ---

In our experimental setting, the incentives of the auditor and the investor are fully misaligned. We choose such a setting for the following reasons. First, we want to examine a setting in which the danger of impaired auditor independence exists. This reflects the concerns of legislators who have decided to mandate the disclosure of auditors' fees. Beyond that, given the controlled experimental design, it seems uninteresting to examine a setting of perfectly aligned incentives; in our opinion, there is no reason to believe that auditors would not report truthfully and why investors would not fully rely on this report in a setting without conflict. Second, we use such an extreme case in order to improve the power of our experiment. However, we believe that our results can be generalized to settings in which incentives are less misaligned. According to economic theory, the results should be the same as long as the degree of misalignment is above a certain threshold (Crawford and Sobel 1982), and they should change only gradually as the incentives become further aligned (Dickhaut, McCabe & Mukherji, 1995).

4.5. Statistical Modelling

Unless otherwise mentioned, we use two-sided parametric models that control for the individual subject and the interval of the asset value in all of our statistical analyses. We control for the individual subject by including it as a random effect in the model. Roughly speaking, this has the effect that for each condition only one mean decision is used for each individual in the statistical tests; more precisely speaking, this has the effect that the variance and the number of degrees of freedoms are adjusted for the possibility that the decisions by the same individual in different periods are not independent of each other. We control for the interval of the asset value by including it as a fixed effect in the model, since the bias

contained in the advice and estimate depend on it by definition. We classify the interval as an ordinal variable.

Our main dependent variables are the advice bias and estimate bias, defined as the difference between the advice or estimate and the expected asset value. The expected asset value is the midpoint of the interval of the private information of the auditor.[4]

5. Results

5.1. Results for the experience setting: auditors

We expect that the "perverse effects" of disclosure will appear in the quasi-replication of CLM (2005), which is the first period of the experience setting (H1a). After subjects have received feedback and had the opportunity to gain experience, we expect that these psychological effects will become weaker, and that disclosure will improve auditor independence as predicted by economic theory (H1b).

We can confirm H1a. In the first period, advice bias is larger in the disclosure condition (5.17) than in the no disclosure condition (2.52). As we consider the first period as a quasi-replication of CLM (2005), a one-sided test can be justified which yields a significant effect when controlling for the interval of the asset value ($p = 0.034$).

H1b can also be supported. After the auditors receive feedback, the effect reverses: from the second period on, auditors give more biased advice in the no disclosure condition than in the disclosure condition. This effect is highly significant for the second period (means = 6.88 vs. 2.44, $p = 0.010$), and marginally significant for the 2nd to 9th period (means = 6.32 vs. 4.37, $p = 0.058$).

--- Insert figure 2 about here ---

In a general linear model including all periods of the experience setting and adding period as a further control variable, a strong reversal of the effect of disclosure from the first to the second period can be observed. The interaction between disclosure and a dummy variable indicating 1st vs. 2nd to 9th period is highly significant ($p = 0.010$). The interaction between 1st

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

vs. 2nd to 9th period becomes even stronger when excluding period as an effect (p = 0.015). The interaction between disclosure and 1st period vs. 2nd is also highly significant (p = 0.003).

Comparing the change in behaviour from the 1st to the 2nd to 9th period separately for both disclosure conditions suggests that the interaction effect is driven mainly by a change in the behaviour of auditors in the no disclosure condition. While the advice bias decreases only insignificantly in the disclosure condition (mean = 5.17 vs. 4.37, p = 0.263), it increases highly significantly in the no disclosure condition (mean =2.52 vs. 6.32, p = 0.000).

--- Insert table 3 about here ---

In follow-up analyses, the notion that the higher advice bias in the first period of the disclosure condition can be attributed to moral licensing, and that the effect of moral licensing diminishes with experience is supported. At the end of the experiment, we asked the subjects whether they would feel guilty in a hypothetical situation in which the auditor sent upward biased advice to the investor in our experimental setting. We classified auditors as feeling guilty when they reported feelings of guilt of 4 and above on a 7-point Likert scale. While 6 out of 18 auditors in the no disclosure setting reported that they would feel guilty when giving upward-biased advice, this feeling of guilt is absent for all subjects in the disclosure setting. The difference in the attitude of feeling guilty is highly significant (p=0.002), and provides support for CLM's claim that the disclosure of conflict reduces feelings of guilt when giving wrong advice.

We further analyze how the feeling of guilt affects the behaviour of auditors in the no disclosure condition over time. While in the first period those 6 auditors who reported feelings of guilt provide less biased advice compared to the 12 auditors who reported no feeling of guilt, this effect disappears from the second period on (see figure 3). The interaction effect between the feeling of guilt and the 1st vs. the 2nd to 9th period is highly significant (p = 0.006)

--- Insert figure 3 about here ---

5.2. Results for the experience setting: investors

Following CLM (2005), we hypothesized that investors will be more biased in the disclosure condition than in the no disclosure condition during the first period due to the curse of knowledge bias (H2a), and that this effect will reverse once investors have received feedback (H2b).

We do not find any evidence for H2a that investors suffer from the curse of knowledge bias in the first period. Although auditors' advice is more upward biased in the disclosure condition, investors are not more misled by this, and do not anchor on this advice. In fact, investors in the disclosure condition indicate scepticism, as their estimates are slightly downward biased (mean = -1.31, SD = 5.97). Meanwhile, investors are slightly upward biased in the no disclosure condition (mean = 1.53, SD = 7.15). The difference becomes significant when controlling additionally for advice ($p = 0.015$). This means that investors in the disclosure condition give lower estimates than investors in the no disclosure condition when both groups receive similar advice.

The tendency observed in the first period for investors in the disclosure condition to be less biased than investors in the no disclosure condition holds over time. The difference between the two disclosure conditions is highly significant for the second to ninth period ($p = 0.000$), thereby supporting H2b. While the estimates of investors in the disclosure condition are mostly unbiased (mean = -0.78, SD = 2.09), the estimates of investors in the no disclosure condition are, on average, upward biased (mean = 2.52, SD = 2.37).

In summary, we find no effect of experience on estimate bias. Investors in the disclosure condition are less biased in all periods; the two interaction effect between the 1st period vs. 2nd to 9th period and disclosure ($p = 0.641$) is statistically insignificant.

5.3. Results for the reputation setting: auditors[5]

In the reputation setting, auditors' average advice bias is not affected by the disclosure of conflict of interest. The advice bias in the disclosure condition (mean = 3.49, SD = 3.77) and in the no disclosure condition (mean = 3.58, SD = 2.07) are not significantly different ($p = 0.706$). Consistent with this, we find that in the reputation setting, the feeling of guilt when giving upward biased advice is about the same for both the disclosure and no disclosure conditions (disclosure: mean = 3.00, SD = 1.89; no disclosure: mean = 3.06, SD = 1.73; p

[5] In our analysis of the reputation setting, we exclude the ninth period in order to avoid potential confounding last period effects. Results do not change qualitatively when including the ninth period.

(two-sided t-test) = 0.926). This suggests that disclosure does not relieve the auditor of moral concerns when she interacts repeatedly with the same investor.

While the average advice bias is the about the same in both conditions, the absolute advice bias is significantly lower ($p = 0.025$) in the no disclosure condition (mean = 4.61, SD = 1.92 vs. mean = 5.87, SD = 2.29). This indicates that auditors deviate less from their private information in the no disclosure condition; they provide more accurate advice. The accuracy of advice can also be illustrated by comparing the degrees to which the auditor incorporates her private information into her advice. In the disclosure condition, she provides about the same advice regardless of the interval of her private information. On average, she raises her advice by only 1.14 units when receiving an interval which is 5 units higher. In contrast, the auditor takes her private information considerably into account in the no disclosure condition, raising her advice by 3.13 for each interval. Therefore, the results suggest that in the no disclosure condition, the auditor attempts to build up reputation for credibility, whereas this does not happen in the disclosure condition.

--- Insert figure 5 about here ---

5.4. Results for the reputation setting: investors

Investor's estimates in the no disclosure condition are more upward biased than in the disclosure condition to a highly significant degree ($p = 0.004$). In the disclosure condition, investors are mostly unbiased in their estimates (mean = -0.60, SD = 2.41). In the no disclosure condition, investors are upward biased (mean = 2.02, SD = 1.61).

However, the absolute estimate bias is lower ($p = 0.014$) in the no disclosure condition (mean = 3.45, SD = 1.45) than in the disclosure condition (mean = 5.64, SD = 1.61). This can be explained as follows. In the disclosure condition, investors estimate the value of the asset at about 20, regardless of its true value. The advice of the auditor is not useful for them. Therefore, their estimates are not biased, but nevertheless inaccurate. In the no disclosure condition, investors are able to adjust their estimates in accordance to the true value of the asset. Even though they are upward biased, their estimates are rather accurate.

--- Insert figure 6 about here ---

5.5. Robustness check: overall regression

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 (see table 4). As these factors by definition depend on the interval of the asset, we include interval as a control variable.

First, we examine the effects on the advice bias. Disclosure has no significant main effect, but it interacts with other factors. Its interaction with 1st period vs. 2nd to 9th period is marginally significant, and its three-way interaction with 1st period vs. 2nd to 9th period and matching is significant. This provides evidence that the behaviour of the auditor changes strongly from the first to second period in the experience setting, but not in the reputation setting. Furthermore, disclosure interacts with interval, which shows that the accuracy of advice is generally lower when conflict of interest is disclosed.

Reputation has no main effect on advice bias, but its interaction effect with interval is highly significant. This indicates that the advice bias is smaller for lower intervals in this treatment. The three-way interaction of reputation, interval and disclosure shows that the higher accuracy can especially be observed in the no disclosure condition of the reputation setting.

We observe a time effect in the data. Advice bias becomes larger over time, especially in the reputation setting. Meanwhile, advice bias is not affected by the behaviour of the investor in the previous period.

Second, we examine the effects on the estimate bias. Disclosure is a significant main effect, suggesting that disclosure of conflict of interest helps investors to provide unbiased estimates. However, the interaction effect with the interval of private information belies the assumption that the accuracy of estimates necessarily improves when conflict of interest is disclosed.

Reputation has no main effect on the estimate bias. However, the interaction effect with interval implies that the estimates' accuracy is higher in the reputation setting, especially for the no disclosure condition as suggested by the three-way interaction of reputation, interval and disclosure.

No general time effect of disclosure on the estimate bias can be observed. However, investors react to a high advice bias from the auditor in the previous period by lowering their estimates in the following period.

--- Insert table 4 about here ---

6. Conclusions

Mandatory disclosure of audit and nonaudit fees has recently been revived in the U.S. (SEC, 2000; 2003) and will soon be prescribed in all countries of the European Union (European Union, 2006). The hope is this disclosure will ameliorate potential problems that arise from impaired auditor independence by enabling investors to make better informed investment decisions. However, the actual effect of the disclosure of auditors' fees on auditor independence in fact has hardly been researched yet. Empirical examination of this research question has been difficult, due to missing data on auditors' fees prior to disclosure, and due to the inherent noisiness of proxies for auditor independence. To our knowledge, the CLM study is the first that experimentally examine the effects of disclosure of conflict of interest on auditors' independence. We advance their research by using a more controlled experimental design, and extend their setting by testing for effects of experience and reputation – two main characteristics of the audit environment.

Our main results are the following. First of all, with inexperienced subjects, we are able to replicate the perverse effects of disclosure of conflict of interest on auditor independence, which were reported by CLM, in a more controlled experiment. We provide evidence that this effect can actually be attributed to moral licensing as hypothesized by CLM: subjects in the no disclosure condition state that they would feel more guilty when providing upward-biased advice than subjects in the disclosure condition, and those subjects that report that they would feel guiltier actually send less biased advice. Second, we find that these results reverse as subjects gain experience. From the second period on, auditors' advice is less biased in the disclosure condition. This is consistent with economic theory. Psychological factors seem to become less important, as the behaviour of subjects who would feel guilty when sending upward biased advice no longer differs in their behaviour from the other subjects. Third, our results suggest that disclosing conflict of interest can hinder the reputation formation of auditors. In the no disclosure condition, auditors try to build up reputation by sending useful advice, while in the disclosure condition, the advice they send is uninformative. This also affects investors, who give less calibrated estimates in the disclosure condition, even though their estimates are also less upward biased. We do not observe any effects associated with moral licensing as we find that both the level of guilty feelings when sending upward biased advice, and the advice bias, remain the same for both disclosure conditions.

With our findings, we are able to provide further evidence that there is the danger of perverse effects on auditor independence when a conflict of interest is disclosed, due to moral licensing. At the same time, however, we also reveal limitations of this moral licensing effect. It does not seem to affect behaviour after subjects have received feedback and gained experience. Furthermore, in settings of repeated interaction with the same partner, the level of moral concern does not decrease when the conflict of interest is disclosed. Therefore, these potentially perverse effects of disclosure reported by CLM seem to be of little relevance for the audit environment in which auditors and investors interact repeatedly over several years.

Furthermore, we show that disclosure of conflict of interest can have an additional side effect. It may impede reputation formation: auditors provide less accurate advice and are less trusted by investors. Thus, the disclosure of the auditors' fee might lower the value of the audit. This may constitute an indirect form of sanctioning of those services that are perceived by the financial market to be detrimental to auditor independence. This effect might be intentional to some degree, also the SEC address this issue in their promulgated rules (SEC, 2000). However, the disclosure might also have the negative effect of making audit committees reluctant to pre-approve even those nonaudit services that they consider valuable (Gaynor et al., 2006). A further unintended effect would be that high audit fees are also sanctioned, as investors perceive high audit fees as a threat to auditor independence (Gul, 1991). This seems especially problematic, since high audit fees are often considered to be a marker for quality in auditing research (Abbott, Parker, Peters & Raghunandan, 2003; Goodwin-Stewart & Kent, 2006; Ho & Mande, 2005).

Besides the limitation of experimental research in general, one specific limitation of our experiment is that we restrict our focus to a scenario in which (a) auditors have incentives to impair their independence and (b) the incentives of advisors and investors are completely misaligned. In this respect we follow CLM, because we believe that this scenario is especially interesting given that it is the one which legislators most fear. We also focus on this scenario in order to be able to derive economic hypotheses and ensure the statistical power of the experiment. As described above, we believe that the tendencies of our results can also be generalized to scenarios in which the incentives of auditors and investors are more aligned. One opportunity for future research would be to explicitly examine such a scenario of partly aligned incentives, e.g. by incorporating litigation risk into the setting, by giving auditors the opportunity to influence the alignment of incentives, or by allowing for competition between the auditors.

References

Abbott, L.J., Parker, S., Peters, G.F., & Raghunandan, K. (2003). The Association between Audit Committee Characteristics and Audit Fees. *Auditing: A Journal of Practice & Theory 22(2)*, 17-32.

Antle, R., Gordon, E., Narayanamoorthy, G., & Zhou, L. (2006). The Joint Determination of Audit Fees, Non-Audit Fees, and Abnormal Accruals. *Review of Quantitative Finance & Accounting 27(3)*, 235-266.

Ashbaugh, H., LaFond, R., & Mayhew, B.W. (2003). Do Nonaudit Services Compromise Auditor Independence? Further Evidence. *Accounting Review 78(3)*, 611-639.

Boatsman, J.R., & Grasso, L.P. (1992). A Perspective on the Use of Laboratory Market Experimentation in Auditing Research. *Accounting Review 67(1)*, 148-156.

Bonner, S.E., & Lewis, B.L. (1990). 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., Crabtree, A.D., & Maher, J.J. (2004). Nonaudit Fees, Auditor Independence, and Bond Ratings. *Auditing: A Journal of Practice & Theory 23(2)*, 91-105.

Cain, D.M., Loewenstein, G., & Moore, D.A. (2005). The Dirt on Coming Clean: Perverse Effects of Disclosing Conflicts of Interest. *Journal of Legal Studies 34(1)*, 1-25.

Calegari, M.J., Schatzberg, J.W., & Sevcik, G.R. (1998). Experimental evidence of differential auditor pricing and reporting strategies. *Accounting Review 73(2)*, 255-275.

Camerer, C., Ho, T., & Chong, K. (2003). Models of Thinking, Learning, and Teaching in Games. *American Economic Review 93(2)*, 192-195.

Camerer, C., Loewenstein, G., & Weber, M. (1989). The Curse of Knowledge in Economic Settings: An Experimental Analysis. *Journal of Political Economy 97(5)*, 1232-1254.

Camerer, C.F., Ho, T.-H., & Chong, J.-K. (2004). A Cognitive Hierarchy Model of Games. *Quarterly Journal of Economics 119(3)*, 861-898.

Chaney, P.K., & Philipich, K.L. (2002). Shredded Reputation: The Cost of Audit Failure. *Journal of Accounting Research 40(4)*, 1221-1245.

Chung, H., & Kallapur, S. (2003). Client Importance, Nonaudit Services, and Abnormal Accruals. *Accounting Review 78(4)*, 931-955.

Craswell, A.T., Stokes, D.J., & Laughton, J. (2002). Auditor Independence and Fee Dependence. *Journal of Accounting & Economics 33(2)*, 253-275.

Crawford, V.P., & Sobel, J. (1982). Strategic Information Transmission. *Econometrica 50(6)*, 1431-1451.

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

Davis, S.M., & Hollie, D.Y. (Forthcoming). The Impact of Nonaudit Service Fee Levels on Investors' Perceptions of Auditor Independence. *Behavioral Research in Accounting*.

DeAngelo, L.E. (1981). Auditor Independence, "Low Balling", and Disclosure Regulation. *Journal of Accounting & Economics 3(2)*, 112-127.

DeFond, M.L., Raghunandan, K., & Subramanyam, K.R. (2002). 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., McCabe, K.A., & Mukherji, A. (1995). An experimental study of strategic information transmission. *Economic Theory 6(3)*, 389.

Dopuch, N., & King, R.R. (1991). The Impact of MAS on Auditors' Independence: An Experimental Markets Study. *Journal of Accounting Research 29(3)*, 60-98.

Dopuch, N., King, R.R., & Schwartz, R. (2001). Independence in Appearance and in Fact: An Experimental Investigation. *Contemporary Accounting Research 20(1)*, 79-114.

Dopuch, N., King, R.R., & Schwartz, R. (2003). An Experimental Investigation of Retention and Rotation Requirements. *Journal of Accounting Research 39(1)*, 93-117.

Dykxhoorn, H.J., & Sinning, K.E. (1982). Perceptions of Auditor Independence: its Perceived Effect on the Loan and Investment Decisions of German Financial Statement Users. *Accounting, Organizations & Society 7(4)*, 337-347.

European Union. (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* 87-106.

Fischbacher, U. (1999). z-Tree - Zurich Toolbox for Readymade Economic Experiments - Experimenter's Manual.

Fischer, P.E., & Stocken, P.C. (2001). Imperfect Information and Credible Communication. *Journal of Accounting Research 39(1)*, 119-134.

Francis, J.R., & Bin, K. (2006). Disclosure of Fees Paid to Auditors and the Market Valuation of Earnings Surprises. *Review of Accounting Studies 11(4)*, 495-523.

Frankel, R.M., Johnson, M.F., & Nelson, K.K. (2002). The Relation between Auditors' Fees for Nonaudit Services and Earnings Management. *Accounting Review 77(4)*, 71-105.

Gaynor, L.M., McDaniel, L.S., & Neal, T.L. (2006). The Effects of Joint Provision and Disclosure of Nonaudit Services on Audit Committee Members' Decisions and Investors' Preferences. *Accounting Review 81(4)*, 873-896.

Gigler, F. (1994). Self-Enforcing Voluntary Disclosures. *Journal of Accounting Research 32(2)*, 224-240.

Gneezy, U. (2005). Deception: The Role of Consequences. *American Economic Review 95(1)*, 384-395.

Gneezy, U.R.I., & List, J.A. (2006). Putting Behavioral Economics to Work: Testing for Gift Exchange in Labor Markets using Field Experiments. *Econometrica 74(5)*, 1365-1384.

Goodwin-Stewart, J., & Kent, P. (2006). Relation Between External Audit Fees, Audit Committee Characteristics and Internal Audit. *Accounting & Finance 46(3)*, 387-404.

Güth, W., Huck, S., & Ockenfels, P. (1996). Two-Level Ultimatum Bargaining with Incomplete Information: An Experimental Study. *The Economic Journal 106(436)*, 593-604.

Gul, F.A. (1991). Size of Audit Fees and Perceptions of Auditors' Ability to Resist Management Pressure in Audit Conflict Situations. *Abacus 27(2)*, 162-172.

Hartley, R.V., & Ross, T.L. (1972). MAS and Audit Independence: An Image Problem. *Journal of Accountancy 134(5)*, 42-51.

Higgs, J.L., & Skantz, T.H. (2006). Audit and Nonaudit Fees and the Market's Reaction to Earnings Announcements. *Auditing: A Journal of Practice & Theory 25(1)*, 1-26.

Ho, Y.L., & Mande, V. (2005). The Relationship of Audit Committee Characteristics with Endogenously Determined Audit and Non-Audit Fees. *Quarterly Journal of Business & Economics 44(3/4)*, 93-112.

Huang, H.-W., Mishra, S., & Raghunandan, K. (2007). Types of Nonaudit Fees and Financial Reporting Quality. *Auditing: A Journal of Practice & Theory 26(1)*, 133-145.

Iyer, G.S., Iyer, V.M., & Mishra, B.K. (2003). The Impact of Non-Audit Service Fee Disclosure Requirements on Audit Fee and Non-Audit Service Fee in the United Kingdom: an Empirical Analysis. *Advances in Accounting 20*, 127-140.

Kassirer, J.P. (2001). Financial Conflict of Interest: An Unresolved Ethical Frontier. *American Journal of Law and Medicine 27(2&3)*, 149-162.

Khurana, I.K., & Raman, K.K. (2006). Do Investors Care about the Auditor's Economic Dependence on the Client? *Contemporary Accounting Research 23(4)*, 977-1016.

King, R.R. (1996). Reputation Formation for Reliable Reporting: An Experimental Investigation. *Accounting Review 71(3)*, 375-396.

Kinney Jr., W.R., Palmrose, Z.-V., & Scholz, S. (2004). Auditor Independence, Non-Audit Services, and Restatements: Was the U.S. Government Right? *Journal of Accounting Research 42(3)*, 561-588.

Krishnan, J., Sami, H., & Zhang, Y. (2005). Does the Provision of Nonaudit Services Affect Investor Perceptions of Auditor Independence? *Auditing: A Journal of Practice & Theory 24(2)*, 111-135.

Larcker, D.F., & Richardson, S.A. (2004). Fees Paid to Audit Firms, Accrual Choices, and Corporate Governance. *Journal of Accounting Research 42(3)*, 625-658.

Lavin, D. (1977). Some Effects of the Perceived Independence of the Auditor. *Accounting, Organizations & Society 2(3)*, 237-244.

Levitt, S.D., & Syverson, C. (2005). Market Distortions when Agents are Better Informed: The Value of Information in Real Estate Transactions. *NBER Working Paper No. 11053*. Available at http://pricetheory.uchicago.edu/levitt/Papers/LevittSyverson2004.pdf

Lowe, D.J., & Pany, K. (1995). CPA Performance of Consulting Engagements with Audit Clients: Effects on Financial Statement Users' Perceptions and Decisions. *Auditing: A Journal of Practice & Theory 14(2)*, 35-53.

Mayhew, B.W. (2001). Auditor Reputation Building. *Journal of Accounting Research 39(3)*, 599-617.

Mayhew, B.W., Schatzberg, J.W., & Sevcik, G.R. (2001). The Effect of Accounting Uncertainty and Auditor Reputation on Auditor Objectivity. *Auditing: A Journal of Practice & Theory 20(2)*, 49-70.

Mishra, S., Raghunandan, K., & Rama, D.V. (2005). Do Investors' Perceptions Vary with Types of Nonaudit Fees? Evidence from Auditor Ratification Voting. *Auditing: A Journal of Practice & Theory 24(2)*, 9-25.

Newman, P., & Sansing, R. (1993). Disclosure Policies with Multiple Users. *Journal of Accounting Research 31(1)*, 92-112.

Pany, K., & Reckers, P.M.J. (1984). Non-Audit Services and Auditor Independence - A Continuing Problem. *Auditing: A Journal of Practice & Theory 3(2)*, 89-97.

Pany, K., & Reckers, P.M.J. (1987). Within- Vs. Between-Subjects Experimental Designs: A Study of Demand Effects. *Auditing: A Journal of Practice & Theory 7(1)*, 39-53.

Reynolds, J.K., Deis, J.D.R., & Francis, J.R. (2004). Professional Service Fees and Auditor Objectivity. *Auditing: A Journal of Practice & Theory 23(1)*, 29-52.

Ruddock, C., Taylor, S.J., & Taylor, S.L. (2006). Nonaudit Services and Earnings Conservatism: Is Auditor Independence Impaired? *Contemporary Accounting Research 23(3)*, 701-746.

Schatzberg, J.W. (1990). A Laboratory Market Investigation of Low Balling in Audit Pricing. *Accounting Review 65(2)*, 337-362.

Schatzberg, J.W., & Sevcik, G.R. (1994). A Multiperiod Model and Experimental Evidence of Independence and "Lowballing". *Contemporary Accounting Research 11(1)*, 137-174.

Schwartz, S.T., & Young, R.A. (2002). A Laboratory Investigation of Verification and Reputation in a Repeated Joint Investment Setting. *Contemporary Accounting Research 19(2)*, 331-342.

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

SEC. (2000). Final Rule: Revision of the Commission's Auditor Independence Requirements, Release No. 33-7919. Available at http://www.sec.gov/rules/final/33-7919.htm

SEC. (2003). Final Rule: Strengthening the Commission's Requirements Regarding Auditor Independence, Release No. 33-8183. Available at http://www.sec.gov/rules/final/33-8183.htm

Shapiro, S.P. (2003). 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. (1981). Perceptions of Auditors' Independence: An Empirical Analysis. *Accounting Review 56(4)*, 785-800.

Tversky, A., & Kahneman, D. (1974). Judgment under Uncertainty: Heuristics and Biases. *Science 185(4157)*, 1124-1131.

Watts, R.L., & Zimmerman, J.L. (1981). The Market for Independence and Independent Auditors.

Wilson Jr., T.E., & Grimlund, R.A. (1990). An Examination of the Importance of an Auditor's Reputation. *Auditing: A Journal of Practice & Theory 9(2)*, 43-59.

Figure 1
Auditor-Investor Game (Disclosure condition)

In the figure, we show the disclosure condition of the auditor-investor game. In the no disclosure condition, the investor is not informed about the payoff function of the auditor

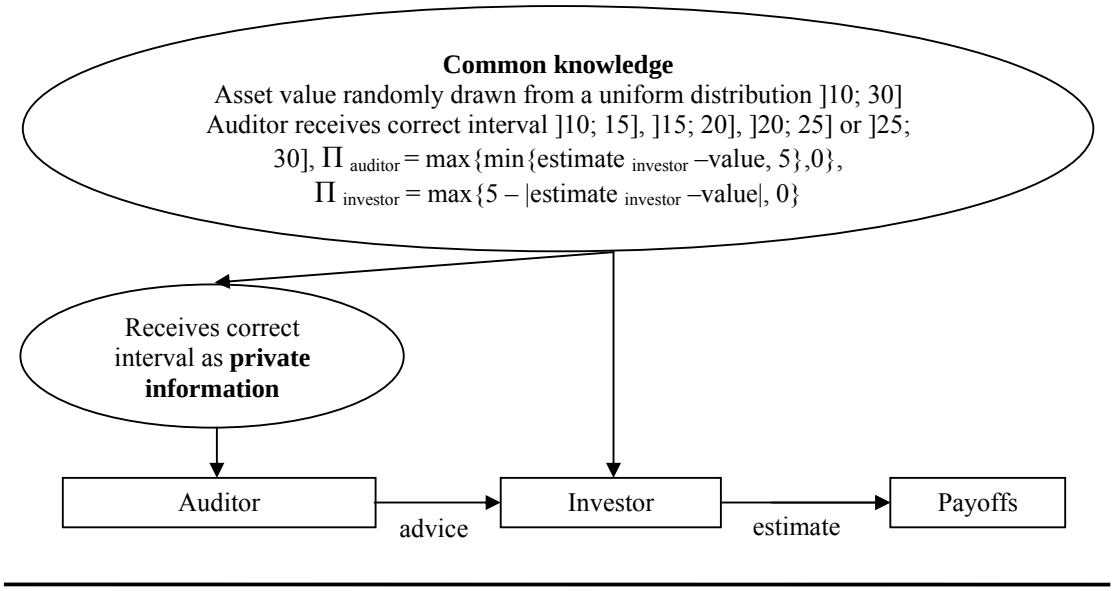


Figure 2
Advice bias by period (experience setting)

In the experience setting, the auditor interacts with the same investor only once.

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

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.

The p-values reported are the p-values for two-sided effect tests of disclosure on advice bias in a regression model in which it is controlled for the interval of the private information and for subject (n=36).

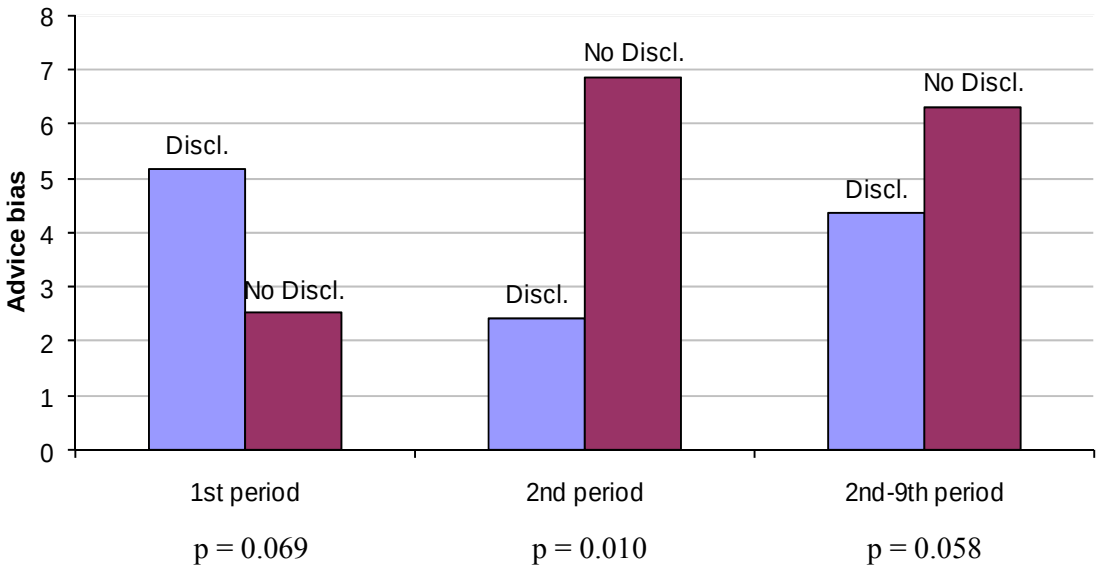


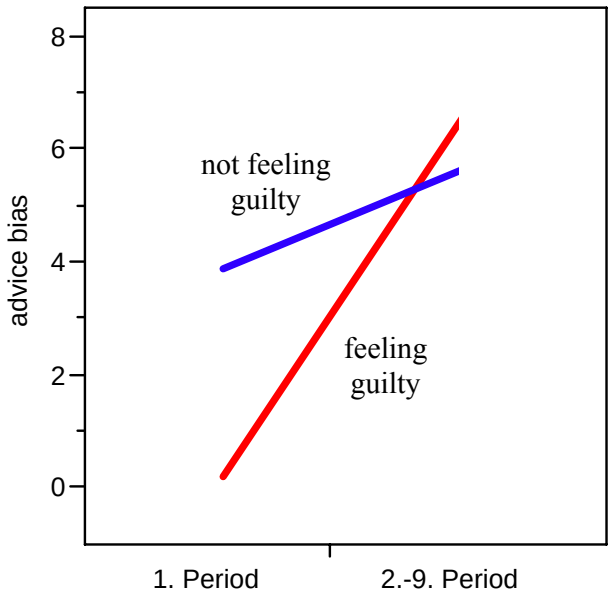
Figure 3
Interaction effect of feeling of guilt and period on advice bias
(no disclosure condition of the experience setting)

The interaction of period and the feeling of guilt of the auditors are plotted. To extract the feeling of guilt, auditors were asked whether they would feel guilty when giving upward biased advice is morally correct on a 7-step Likert scale. Answers ranged from 1 to 5. Answers from 1-3 were classified as "not feeling guilty" (12 subjects), answers from 4-5 were classified as "feeling guilty" (6 subjects).

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

In the model, it is controlled for the interval of the asset (ordinal).

168 observations, 18 subjects as random effect.



P=0.006

Figure 4
Estimate bias by period (Experience setting)

In the experience setting, the auditor interacts with the same investor only once.

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.

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

The p-values reported are the p-values for two-sided effect tests of disclosure on estimate bias in a regression model in which it is controlled for the interval of the private information and for subject (n=36).

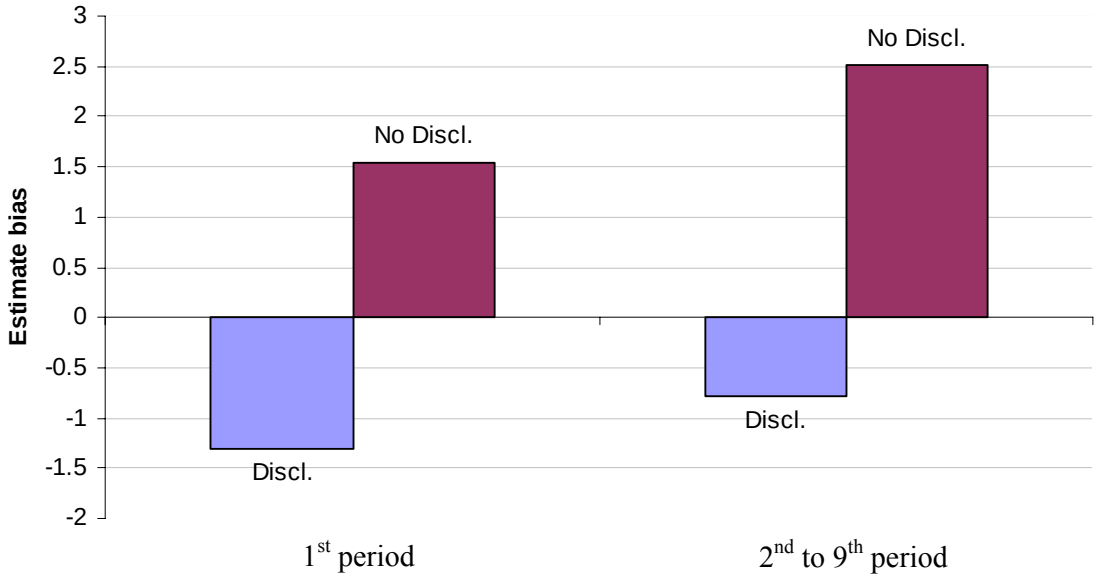
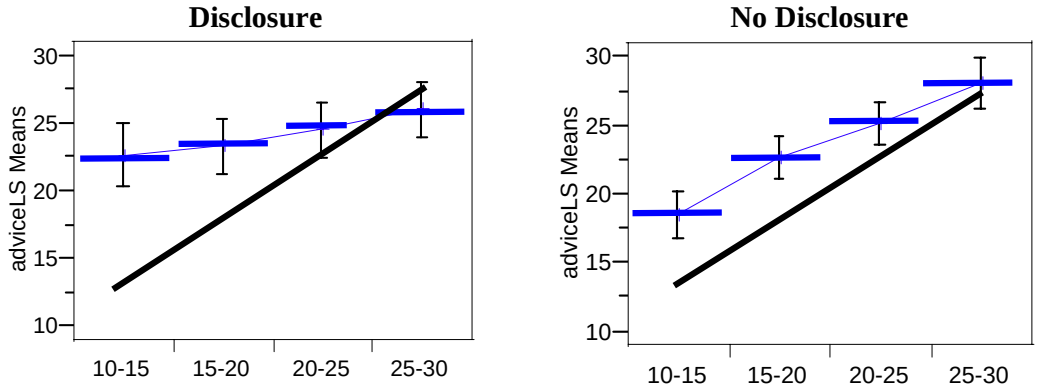


Figure 5
Advice for each interval of the value of the asset (reputation setting)

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



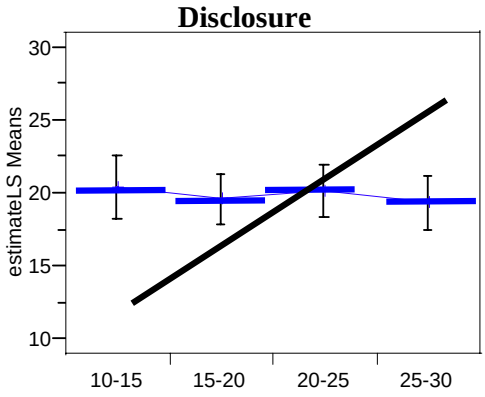
slope = 1.14 (SD = 0.39)

slope = 3.13 (SD = 0.37)

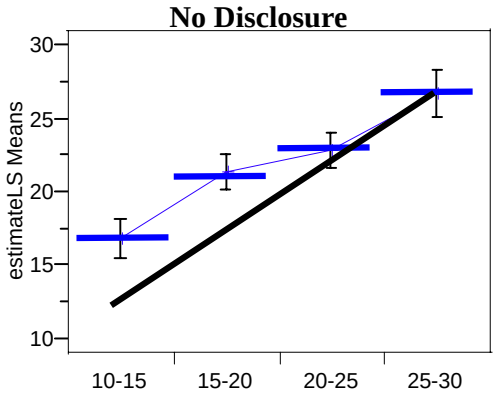
Figure 6
Estimate for each interval of the value of the asset (reputation setting)

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
Experimental treatments

		Disclosure of Conflict of Interest	No disclosure of Conflict of Interest
Experience setting	perfect stranger matching	2 sessions = 36 subjects	2 sessions = 36 subjects
Reputation setting	partner matching	2 sessions = 40 subjects	2 sessions = 36 subjects

*The study included 148 subjects; each subject participated in 9 periods

Table 2
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: 13.00\$ 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	$\Pi_{investor} =$ $\begin{cases} 5.00 & \lvert\text{estimate - value}\rvert \le 0.50 \\ 4.50 & 0.51 < \lvert\text{estimate - value}\rvert \le 1.00 \\ 4.00 & 1.01 < \lvert\text{estimate - value}\rvert \le 1.50 \\ \dots & \dots \\ 0.00 & \lvert\text{estimate - value}\rvert \ge 5.01 \end{cases}$	$\Pi_{investor} =$ $\max\{5 - \lvert\text{estimate}_{investor} - \text{value}\rvert, 0\}$
Review questions	No	Yes
Information provided on conflict of interest	„Auditor is paid on how high you estimate"	Detailed information about incentive scheme and review questions
Auditor's information	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 located:]10,15];]15,20];]20,25];]25,30]
Investor's information	Observing jar from the distance	Information of the interval]10,30] and its distribution, Informed that auditor receives true interval.
Passing on of advice	Shuffling 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 3
Interaction of period and disclosure (experience setting)

Advice bias = advice – expected value of the asset
Disclosure (nominal) = 1 if disclosing conflict of interest, 0 otherwise
2^{nd} to 9^{th} Period (nominal) = 1 for 2^{nd} to 9^{th} period, 0 for 1^{st} period
36 subjects as random effect, controlled for interval (ordinal)

Advice bias as dependent variable	Mean (SE)		Mean (SE)	
Disclosure	0.22 (0.51)		-0.21 (0.79)	*
2^{nd} to 9^{th} Period	0.52 (0.34)			
2^{nd} to 9^{th} Period*Disclosure	-1.11 (0.35)	***		
2^{nd} Period			0.29 (0.45)	
2^{nd} Period*Disclosure			-1.68 (0.46)	***
Adjusted R^2	0.658		0.719	
n	324		72	

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

Table 4
Generalized Least Squares (GLS) random effects regression analysis of the determinants of advice bias and estimate bias

Advice bias = advice – expected value of the asset
Estimate bias = estimate – expected value of the asset
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	SE	Mean	SE
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