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Understanding Contract Audits: An Experimental Approach

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Abstract

A contract audit is a buyer-initiated audit of prices and other conditions, which aims to decrease the information asymmetry between a buyer and a seller. Contract audits are frequently used in monopolistic or oligopolistic markets as in government procurement. We draw upon three distinct literatures to develop hypotheses (transaction cost theory, the theory of planned behavior and social preference theory) and test these hypotheses using data obtained in a laboratory experiment. Our results show that contract audits are widely used, a phenomenon that cannot be explained by traditional economic reasoning. Their demand is well explained by the theory of planned behavior, however. Our data show that contract audits decrease transaction costs by increasing the probability of successful negotiations. Audits lead to an increased share of the trade surplus for the buyer, but this increased welfare may be offset by the audit costs. They also yield lower transaction prices.

Key words: contract auditing, experimental economics, value of the audit

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1. Introduction

A contract audit is a buyer-initiated audit of prices and other conditions (*e.g.*, terms of delivery, warranty, terms of payment), which aims to decrease the information asymmetry between a buyer and a seller. Though contract audits are widely used, especially in markets where the seller has market power (cf. section 2), the current literature provides little insight into the conditions under which buyers will initiate a voluntary contract audit. Similarly, the effects of contract auditing are little known.[1] We aim to fill these gaps by using a series of laboratory experiments to identify the factors that explain this decision and to investigate how transaction prices are affected by contract audits.

In essence, a buyer contemplating using a contract audit trades off the expected benefits of reduced asymmetry of information with the costs of the audit. It is important to note, however, that the reduced asymmetry does not necessarily lead to reduced prices. This is especially so for cases where the seller is a monopolist and has more bargaining power than the buyer, which is the situation we will focus on. In this case, the asymmetry of information may not matter at all. Standard economic reasoning implies that the monopolist will demand the price that maximizes expected profits and the buyer will accept it if the benefit she gets from the good is higher than this price. In this case, a contract audit will not provide any information that may influence the price.[2] Yet, contract audits are often done in practice[3] and *communis opinio* is that they reduce transaction prices (DCAA 2008). Hence, explanations for why contract audits are done and what their effects are must go beyond this standard economic reasoning. Psychological and sociological factors such as the attitude towards

[1] In fact, we could not find any relevant academic paper using common literature search methods.
[2] If there are at least two sellers, Bertrand-type competition may reduce prices to marginal costs, making information asymmetry irrelevant. This assumes, however, that collusion between companies is impossible. We will avoid this kind of complications by focusing on the monopoly case.
[3] In a survey on contract audit issues in 24 NATO and Partner countries, about half of the countries indicate that contract audits are performed in defense procurement without a legal obligation to do so (NATO AC/327, 2007).

audits, the 'fairness' of a price and the effects of subjective norms appear to play important roles. We will discuss such explanations in the following section.

Currently, little is known about the value of contract audits.[4] One of the reasons for the limited information on the value effects of contract auditing is the near impossibility to use metrics based on field data because this would require an assessment of the enhanced competitiveness of purchase prices as a result of contract auditing. Because it is virtually impossible to isolate the factors that influence procurement prices, a controlled environment is needed. A laboratory experiment allows for such a controlled environment.

This potential for laboratory experimentation in auditing has been recognized before (*e.g.*, DeJong *et al*. 1985; DeJong and Forsythe 1992; Wallin 1992; Maines *et al.* 2006). Closest to our research are Kachelmeier (1991) and Dopuch et al. (1989). Kachelmeier (1991) uses experiments to measure the effects of auditing on managerial remuneration for prudent financial reporting. His findings provide empirical support for the conclusion that multi-period market mechanisms in combination with auditing can mitigate welfare losses due to information asymmetries. However, the exact effects of auditing on managerial remuneration remain equivocal. Dopuch *et al.* (1989) measure the economic efficiency in experimental markets with and without different credibility mechanisms available to sellers and buyers. Their results show that each of these mechanisms individually increases economic efficiencies, reducing moral hazard and adverse selection problems, but maximum efficiency is not achieved. Our paper complements these studies in various ways. First, we investigate a monopolistic market. This mirrors the procurement situation that motivates our research. Second, we aim to use the possibility offered by laboratory control to directly measure the costs and benefits of contract audits in order to better understand their raison-d'être. Finally,

[4] Similarly, the evidence on the value of financial audits is also limited, despite a more extensive body of research in this area as initiated by Chow (1982) and elaborated upon from a demand-for-audit perspective by, e.g., Lin Seow (2001) and Collis et al. (2004).

we will use our experimental data in combination with behavioral theories to develop a better understanding of why individuals initiate contract audits in the first place.

A priori, we distinguish two factors that may affect the choice to conduct a contract audit and the audit's effects. One is related to the social-hierarchical environment that the buyer is in. This factor is an external (political or social) pressure on the individuals that are directly involved in initiating contract audits and negotiating contracts. In a large organization aiming to buy, decision makers at the top of the hierarchy are often not the one's conducting the negotiations with a seller. They may, however, be held responsible by stockholders and the general public. Even without specific knowledge of the negotiations themselves, a contract audit may legitimize the outcome. This may provide a reason, for example, to create a 'social norm' that contract audits should be done.[5] For the negotiator, this kind of pressure creates an additional reason to choose to have an audit done, aside from reasons directly related to the negotiations with the seller.[6]

The second factor that may affect contract audits is whether or not the outcome of the audit is known to the seller. The asymmetry of information without audit is reduced by an audit. But if the audit outcome is to some extent subject to random variation (which is usually the case, because most contract audits provide estimates of relevant factors), then some asymmetry remains. An additional asymmetry occurs if the seller is not informed about the outcome. In this case, the informational symmetry that gave rise to the audit in the first place is reduced by the audit but this is offset by the buyer's informational advantage with respect to the audit outcome. Because asymmetric information is known to affect bargaining outcomes (Samuelson 1984) these two distinct kinds of asymmetry (with or without seller

[5] Of course, an alternative is to make contract audits mandatory, which is often the case (cf. fn. 3). Here, we are interested in voluntary audits, however.

[6] Simple advice (Schotter and Sopher 2003) and observation by third parties (Andreoni and Bernheim 2009) both may have strong effects on economic decisions, even when there is no rational reason for this to occur. When combined, advice and observation may give rise to social norms that strongly affect choices (Schram and Charness 2011).

knowledge of the audit outcome) may be important for the consequences of a contract audit. In particular, we expect buyers to obtain better outcomes (e.g., a higher surplus from trade) when they have the advantage of private knowledge of the audit outcome than when this is shared with the seller.

The results of our experiments confirm the observations from the field that contract audits are used despite the fact that economic theory suggests that they should not be demanded. We show that this demand is stimulated by (political) pressure to have audits conducted, and argue that this pressure creates a social norm that affects the buyer's behavior. Moreover, our results show that the number of successful negotiations (and thereby the aggregate efficiency) increases when audits are used. As noted above, none of these results can be explained with standard economic theory. Also the overall effect of contract audits is that they yield lower transaction prices and higher gross efficiency. Finally, our results show that – contrary to our expectation– buyers benefit from sellers knowing the outcome of the audit; their share of the surplus from trade is highest when they demand an audit, the result of which is known to both the buyer and the seller.

The remainder of this paper is organized as follows. Section 2 sets the stage by describing in more detail what contract audits are, and what types we are interested in. Section 3 presents a discussion of possible theories explaining the use of contract audits and general hypotheses. Our experimental procedures and design are described in section 4 which also specifies the hypotheses for our experimental environment. Section 5 briefly describes the game theoretical predictions for our experimental parameters. Section 6 presents our results and section 7 concludes.

2. Setting the Stage: Contract Audits

Contract audits are usually initiated by organizations that buy through formal written contracts in imperfect markets of goods and services.[7] Investment selection in the markets we are thinking of cannot be made under competitive conditions – *e.g.*, by public tender – because there are only one or a few suppliers.[8] This may lead to non-competitive prices and other unfavorable contract conditions. In practice, the contract price is often determined by the seller's costs and a profit mark-up. However, without audit, the costs are known only to the seller, who may use this informational advantage to increase the mark-up.

Take as an example a nation's Ministry of Defense that is out to buy new fighter jets. It generally does not have many vendors to choose from. This lack of competition raises obvious problems of pricing that government institutions will want to avoid. To deal with this, contract audits can be used. Since World War II, this type of auditing has been increasingly used in defense and space procurement by the U.S. and Western European governments (NATO AC/327, 2007) with the sole aim of decreasing the negative effects of vendors' moral hazard by enhancing the fairness of contract prices.[9] Although the Contract Audit Manual of the US Defense Contract Audit Agency (DCAA 2008) is the main source of guidance for contract auditing, its applicability far extends beyond governments since any market with only one or a few suppliers may use this type of audit.

If a buyer decides to have a contract audit undertaken, she starts by giving the assignment to an auditor. The contract auditor will be given access to confidential seller company data, such as price calculations, breakdowns of costs and underlying data. It is therefore necessary that the seller agrees to have an audit performed. After finishing the audit

[7] Contract audits are typically conducted before negotiations take place. In this sense, the term 'pre-contract audit' better expresses what is meant. We stick to the more commonly used 'contract audit'.
[8] In addition, the number of buyers in these markets is often limited, for example in cases of government procurement.
[9] For example, the DCAA in the U.S. lists amongst the goals of an audit: "the accuracy and reasonableness of contractors' cost representations" and "the appropriateness of contractual provisions" (DCAA 2008).

the auditor reports to the potential buyer on the acceptability of the contract price asked by the seller. If this price is not acceptable to the buyer, the auditor will also propose an alternative (lower) price to the buyer. The buyer uses the auditor's advice in subsequent negotiations with the seller. In practice, a contract audit is a market good that comes in various quality levels at distinct prices. The buyer can buy more experienced auditors at higher prices and contract auditors can decide how much time to spend on the audit, depending on how much they are being paid for the job. More experienced auditors and more time spent on auditing both lead to an expected decrease in the asymmetry in the information between buyer and seller.

Contract audits may be performed before, during or after the writing of a contract, or even at its termination. The most common contract audit is performed before a contract is written to facilitate contract agreement by diminishing information asymmetries. We therefore focus on such pre-contract audits. Further, a contract audit may be compulsory or voluntary. For many government contracts a contract audit is not required by law.[10] Therefore this paper focuses on voluntary audits, where the procurer can choose whether or not to initiate a contract audit. Finally, contract audits are typically observed in markets with a limited number of vendors. We therefore focus on monopolistic markets.

Of course, a buyer may choose to negotiate with the seller without a contract audit. She may ask the seller to supply all information deemed necessary, but is dependent on the seller's good faith to do so honestly and completely. With a high degree of information asymmetry the moral hazard implicit in the relationship potentially leads to overpricing. On the other hand, the buyer is not completely dependent on the seller's whims. For many of the products we are interested in there are usually only a few potential buyers. Negotiations are typically bilateral. If the buyer represents a large nation such as the U.S., this will give her some leverage vis-à-vis the seller. Nevertheless, governments of smaller nations will typically

[10] See fn. 3.

have less bargaining power. In our experiments, negotiations will indeed be bilateral, but −as in most of the cases we are interested in− sellers will, in the end, have more bargaining power than buyers.

Contract audits are not financial audits. Although they ultimately aim to have financial implications by reducing the agreed-upon contract price, they are not audits as defined by leading standard setters (IAASB 2011; AICPA 2011). However, contract audits may be considered assurance services, which by definition are aimed at improving the quality of information for decision making (IAASB 2011). Within the broad category of assurance services, contract audits are attestation services that result in a written report about the reliability of another party's assertion (*i.e.*, a vendor claiming that a contract is fairly specified). An interesting feature of contracts audits is that in theory their value can be calculated in a quite straightforward way by the reduction in the contract price after a contract audit has been done minus the costs of the contract audit. As we will discuss later on there are some practical issues that make calculating the value of the contract audit almost as complex and subjective as that of the financial audit, however.

3. Theory and Hypotheses

As in any type of audit the simple fact that information asymmetry is reduced by a contract audit might explain its existence. If a reduction of this asymmetry, *ceteris paribus*, increases the likelihood that the buyer will engage in a trade, then the audit has some economic value for the seller, for which he may be willing to pay by reducing the price.[11] However, the monetary value of this reduced asymmetry remains unknown. Moreover, an opposite effect may also occur. We can think of ways in which a contract audit improves the credibility of the

[11] We thank the editor for pointing this out to us.

seller, for which the buyer is willing to pay. Both effects can also be observed in so-called credence goods (Wolinsky 1995). However, such goods will not affect behavior in our setting that follows standard economic reasoning. As we will see below, standard theory predicts that sellers' decisions will be determined by their market position, not by the preceding buyer decision about the contract audit. To explain the widespread and persistent use of contract audit and its assumed monetary value we thus need to look beyond standard theory.

To develop a framework for understanding contract audits and their consequences, we address the issue from different theoretical angles. On the one hand, we consider the normative aspect of why contract audits may exist by applying the economic theory of organizations, in particular, agency theory and the theory of transaction costs. These theories look upon organizations from a contractual perspective. Though they do not explain why individuals actually choose to have audits done, these theories provide a rationale for their existence. Our experiments will allow us to test the premises underlying this rationale. From a more positive perspective, the theory of planned behavior and economic theories of other-regarding behavior will be used to explain why contract audits are undertaken and how they may affect the outcome of negotiations. These theories will allow us to derive a set of hypotheses about why individuals choose to have a contract audit done and what transaction prices to expect, which we will be able to test with our experimental data.

Though the relationship between buyer and seller is not a standard principal-agent setup (Pratt and Zeckhauser 1985), the information asymmetry between (potential) contract partners is very similar and yields transaction costs like any principal-agent relationship. We know from the agency literature that formal contracts cannot solve this asymmetry, but principals typically have various instruments at their disposal to limit and monitor the agent's actions. Contract auditing may be justified as a form of monitoring that helps to decrease the asymmetric-information problems associated with incomplete contracts.

Many of the buyers in the situations we are thinking of are government bodies. The government is not a typical buyer, however. In fact, the government itself may be seen as the agent in a different principle-agent setting. A contract audit is then a way to assure the principle (e.g., the general public or electorate) that the government engages in prudent contracting (Jensen and Meckling 1976), since the government would otherwise bear the agency costs of a lack of public trust. This is another perspective, from which the existence of contract audits may be justified.[12] Note, however, that this is not the type of agency costs that we address in this study. Instead, we focus on the transaction costs related to the trade between the buyer and seller in a procurement environment.

Transaction costs theory points to costs needed to complete a transaction that are over and above the direct production costs. Examples of transaction costs related to procurement include the costs of assessing the fairness of offered prices and costs related to negotiations over and closing of contracts. Two important sources of transaction costs are bounded rationality and opportunism. Control structures (*e.g.*, competitive markets) provide institutional frameworks aimed at reducing transaction costs (Williamson, 1979). Possible control structures vary from closing deals on a market and negotiating contracts between autonomous parties to control transactions and closing transactions within an organization whereby parties lose their autonomy (Williamson, 1985).

One can use theory to determine optimal control structures. Contract auditing may be an effective instrument to control the costs of transactions and the closing of contracts, and can be applied when contracts between autonomous parties are negotiated. It may also reduce the costs related to seller opportunism. This would increase the suitability of contract audits as part of a control structure for negotiations on non-competitive markets. Whether or not contract audits do indeed constitute a suitable element of a control structure for this

[12] We are grateful to an anonymous referee for pointing out this perspective.

environment is largely an empirical question. Our experiments provide data allowing us to address this issue. In particular, we can investigate whether contract audits facilitate the success of the negotiation process. We will do so formally by testing the hypothesis that contract audits increase the probability that trades take place:[13]

H_{1A} (transaction cost theory): Contract audits increase the probability of agreement in the negotiations.

Support for H_{1A} would indicate that contract audits do decrease transaction costs (for example, by reducing seller opportunism). It would not necessarily mean that *all* such costs are reduced, however. For example, there may also be costs related to the negotiation process itself. Our second hypothesis related to transaction cost theory is specifically directed at such transaction costs. In particular, we consider the costs related to the duration of negotiations and hypothesize that contract audits reduce these by making it easier to reach an agreement.

H_{1B} (transaction cost theory): Contract audits result in shorter negotiations.

The contractual perspective thus aims at explaining how contract auditing may reduce information asymmetries and transaction costs, serving the goal of prudent contracting (which is central in the DCAA 2008 definition of contract auditing[14]). On the other hand, contract auditing may not be the only instrument available to achieve prudent contracting. The question then arises as to why organizations or individuals within these organizations opt for

[13] All hypotheses derived in this section assume monopolistic conditions. They hold for environments with monopsonistic demand as well as if there are multiple buyers. Formally, they are compared to null hypotheses based on standard economic thinking (e.g., no contract audits will be demanded because they would have no effect on prices).

[14] The contract audit Manual (CAM) of the US Defense contract audit Agency (DCAA) describes the goals and methods of contract auditing as follows (DCAA (2008) CAM Vol.1, Chapter 1, paragraph 1-104.2): *'The purpose of contract auditing is to assist in achieving prudent contracting by providing those responsible for Government procurement with financial information and advice relating to contractual matters and the effectiveness, efficiency, and economy of contractor's operations. Contract audit activities include providing professional advice on accounting and financial matters to assist in the negotiation, award, administration, re-pricing and settlement of contracts. Audit interest encompasses the totality of contractor's operations. Audits are performed to assure the existence of adequate controls, which will prevent or avoid wasteful, careless and inefficient practices by contractors. These audits include the evaluation of a contractor's policies, procedures, controls and actual performance, identifying and evaluating all activities which contribute to, or have an impact on, proposed or incurred costs of Government contracts.'*

contract auditing. We draw upon the theory of planned behavior (TPB) to address this question (Ajzen 1991).

TPB is "one of the most widely known and applied psychological action theories" (Greve 2001:435). It has been used extensively in many disciplines, including health care, psychology, organization studies, and information systems, but only in a relatively small number of accounting studies (e.g., Bobek and Hatfield 2003; Carpenter and Reimers 2005; and Dowling 2009). A point of departure for the theory is that an individual's behavior is a function of her intended behavior. At first sight this may seem tautological, but there are numerous examples where intended and actual behavior are not aligned.[15] Think, for example, of intended gym visits and actual workouts. Assuming the intention-behavior link, TPB seeks psychological explanations for individual actions by postulating three antecedents of the intention or motivation to engage in certain behavior, like initiating a contract audit. Essentially, this makes TPB a theory about intentions, though ultimately behavior is measured as the dependent variable (Greve 2001).

The first antecedent is the individual's attitude, i.e., her evaluation or appraisal of the specific choices. A buyer who believes that the contract will improve if a contract audit is undertaken, clearly has a positive attitude towards the contract audit and hence is more likely to develop an intention to use one, and will therefore more likely engage in one. The second antecedent is a subjective norm that represents external (*e.g.*, social) pressures to engage or not to engage in the behavior in question. If society or superiors put pressure on the buyer to engage in a contract audit, then there is a higher motivation to do so.[16] In many cases, the underlying mechanism will be one where such pressure (which typically combines a statement of what the buyer 'ought to do' with third-party observation of the buyer's choice)

[15] See Greve (2001) for an extensive discussion on the relationship between action (behavior) and intention and the implications of this interaction for TPB.
[16] As discussed above, agency theory provides an explanation for public or political pressure to engage in contract audits.

yields a belief that there is a (social) norm that prescribes using an audit (Schram and Charness 2011). The third antecedent is the perceived behavioral control. This refers to the perceived ability to perform certain behavior, sometimes referred to as 'self-efficacy' (Dowling 2009). A buyer who believes that organizing and executing a contract audit can easily be done at a reasonable price will be more likely to intend to use an audit and therefore more likely to hire a contract auditor.

Obviously, these antecedents are not independent. For example, one's attitude towards certain behavior may shape one's perception of behavioral control. Similarly, interaction effects may exist between subjective norms with both attitude (*e.g.*, societal or political pressure that influences the attitude toward a contract audit) and perceived behavioral control (*e.g.*, societal or political pressure that influences the perception of the feasibility of a contract audit).

TPB sets out to explain behavior that is germane to specific tasks, circumstances and situations. Hence, an application to contract auditing may reveal more accurate predictions of behavior than a theory that only incorporates more generic personality traits such as abilities and attitudes. However, for TPB to provide accurate predictions, −in addition to the aforementioned relationship between behavioral intentions and behavior− three conditions must be met (Ajzen 1991, p.185). First, measures of attitudes, norms and perceived control must be aligned with measures of behavior. Second, ability and motivation must remain stable. Third, perceptions of ability must realistically reflect actual control. Note that laboratory control allows one to create an environment that closely fulfils these three conditions. For example, (1) we can directly measure attitudes, pressure, perceived control and behavior; (2) we can keep the environment constant; and (3) we can create maximum clarity about what an individual can do and what the consequences of distinct choices are. From an empirical perspective, we can add that knowing an individual's attitudes, perceived

control and the pressures she is subjected to, provides an excellent opportunity to manipulate these in a controlled setting, allowing for careful testing of TPB's predictions with respect to the individual choice for a contract audit. For this testing, we formulate the following hypotheses on the *antecedents* of contract auditing:

> H_{2A} *(TPB)*: The more positive the buyer's attitude toward contract auditing is, the more likely she is to initiate a contract audit.
>
> H_{2B} *(TPB)*: As the pressure to initiate a contract audit increases, the buyer will more often initiate a contract audit.
>
> H_{2C} *(TPB)*: If a buyer believes a contract audit is relatively easy to organize, she is more likely to initiate one.

As mentioned above, these hypotheses may seem tautological at first sight. One way to test such alleged tautology is to use Smedslund's (1984) suggestion and ask whether results opposite to the hypothesis could be reason to reject the theory or would necessarily lead to rejection of the methodology or context (see Greve 2001 for a discussion). The latter scenario would point to a tautology. For each of H_{2A}, H_{2B}, and H_{2C}, rejection could lead to refutation of TPB, however. For example, data rejecting H_{2A} (implying that a positive attitude towards contract audits does not lead to a higher probability of using one) would mean that TPB is rejected, perhaps in favor of an alternative theory that allows for a discrepancy between intentions and behavior (as in the gym-workout example mentioned above). Similarly, the second antecedent would be rejected if cost-benefit analyses dominate contract audit choice. In that case, contrary to TPB's prediction, there would be no relationship between pressure and intentions (and therefore between pressure and behavior). A similar argument can be made for H_{2C}.

Though the TPB explains why people choose the instrument, it is not a theory that addresses the benefits of contract auditing. As stressed above, in monopolies sellers can

dictate prices, as a result of which there is no direct reason –auditing does not affect market power– to change transaction prices based on the results of an audit. Even if the demand side is also monopsonistic, there is no reason to expect an audit to have an effect. This only holds if negotiators are purely self-interested, however. Results from a plethora of recent economic experiments question this assumption. Observed deviations from self-interest have laid the foundation for models of social preferences. Different types of models have been formulated describing distinct deviations from self-interest. These can be categorized as: (i) models of inequity aversion, which assume that people attribute negative utility to earnings-differences across individuals (*e.g.*, Fehr and Schmidt 1999, Bolton and Ockenfels 2000); (ii) models of preferences for efficiency or maximum total surplus (*e.g.*, Charness and Rabin 2002); (iii) reciprocity models where individuals want to reward (punish) (un)kind behavior of others (*e.g.*, Dufwenberg and Kirchsteiger 2004). Social preferences in general and inequity aversion in particular can be important elements in explaining the effect of contract audits on transaction prices. Contract auditing helps to reduce information asymmetry but it may also change the perception of fairness based on the auditor's report of the acceptability of the contract price to the potential buyer. This can have its effect on transaction prices. In particular, social preference theory suggests that vendors may lower their ask price or buyers may increase their bid prices if a contract audit indicates that the originally suggested contract price is somehow unfair.[17] If sellers have preferences for efficiency, they may also lower their ask price if they think this increases the probability of an agreement.[18] Given the assumed overpricing in case of a monopoly without contract audit, the most likely scenario is that the price will decrease as a consequence of fairness considerations.[19] This yields:

[17] Sellers may also decrease ask prices if they believe that the buyer is more likely to reject an offer after an audit has given evidence that it is somehow unfair.
[18] Note that if no one trades at a loss, the only source of inefficiency is a missed trade in a situation with a positive trade surplus (i.e., a buyer's value that is higher than the seller's cost).
[19] Note that a similar prediction follows from transaction cost theory, which predicts that contract audits will reduce seller opportunism.

H_3 (social preference theory): Contract audits result in lower prices compared to prices based on negotiations with no contract audit.

The extent to which prices are reduced is likely to depend on whether or not the outcome of the audit is known to the seller. As discussed in the introduction, buyers may exploit their advantageous situation if only they know the outcome, which would yield lower prices. This predicts that seller information will increase prices. On the other hand, whether or not the outcome of the audit is known to the seller may also affect her assessment of what the buyer considers to be a 'fair' outcome. It may therefore affect the offer the seller makes. In particular, a seller's bid may depend on her assessment of the buyer's inequity aversion. If so, the extent to which she is willing to reduce the price may be larger if she knows that the buyer has observed a low cost estimate.[20] This reasoning predicts that seller information will reduce prices. Hence, there are two opposing forces concerning the effect of seller information on prices. Because the second effect (a lower bid by the seller) is an indirect effect (i.e., based on her beliefs about the buyer), we hypothesize that the first effect (reduced advantageous position for the buyer) is stronger, which gives:

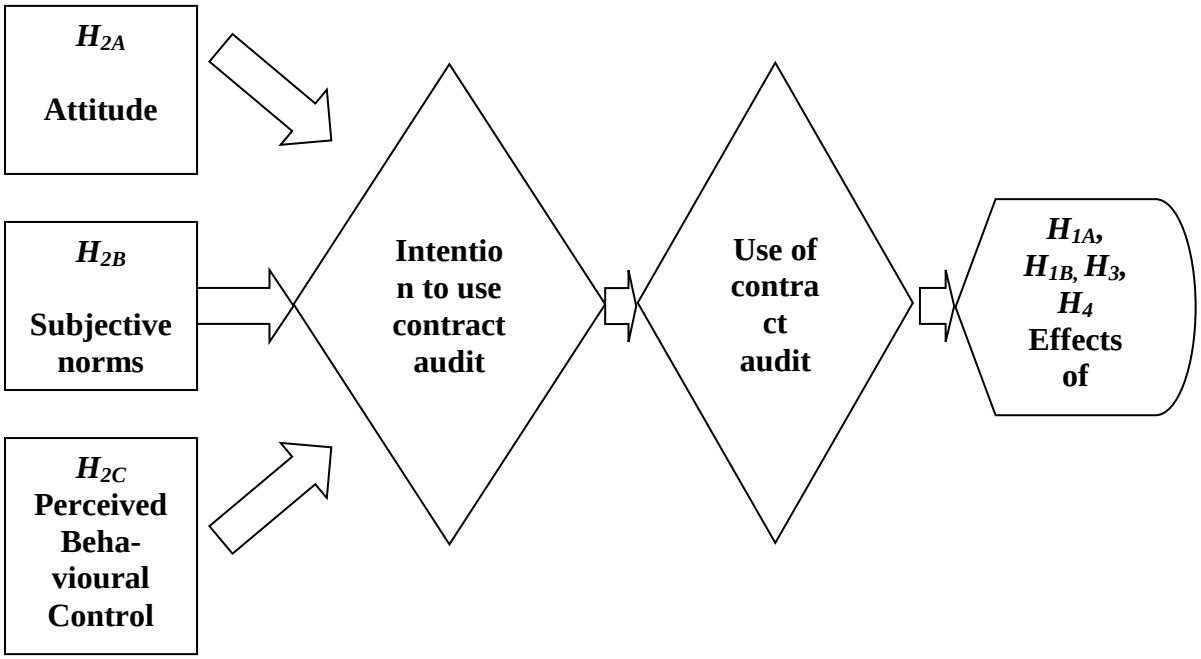
H_4 (asymmetric information): Prices are higher when sellers know the outcome of a contract audit than when they do not.

All in all, transactions costs theory explains how contract auditing can reduce transaction costs and TPB suggests that organizations may engage in contract auditing even in monopolistic markets. Together, TPB, transactions costs theory, and social preference theory imply that in monopolies where individuals have initial positive attitudes toward contract auditing and subjective norms are imposed to engage in it, audits may be undertaken and lead to lower contract prices as a result of social preferences. In turn, this may reinforce the

[20] The reverse is less likely to occur. Because we assume that sellers tend to ask high prices, inequity will generally be disadvantageous for the buyer. An (observed) high cost estimate will reduce this inequity but we predict that this will not be enough to give rise to an increased bid.

favorable attitude of buyers toward contract auditing. As a summary, the interrelation between our hypotheses is represented in figure 1.

Figure 1: Hypotheses



4. Experimental Procedures and Design

Ten sessions were run in November 2007 at the laboratory of the Center for Research in Experimental Economics and Political Decision-making (CREED) at the Faculty of Economics and Business of the University of Amsterdam.[21] The 228 participants were students from the Bachelors and Masters programs at the University of Amsterdam. Each session had 24 participants except the first, which only had 12. Sessions lasted on average about 90 minutes. Participants were paid a show-up fee of € 7 upon arrival and their earnings from the negotiations at the end of the session. Earnings in the experiment are in experimental 'francs', which are exchanged for euros at a rate of €1=10 francs at the end of the experiment. On average, subjects earned €18.80, including the show-up fee.

[21] Appendix A provides a translation of the computerized instructions.

Subjects negotiate in 10 independent periods. Negotiations in the laboratory are bilateral; in every period subjects play a sequential two-person bargaining game with incomplete information, where a buyer and seller negotiate to determine the price of a hypothetical good. Participants stay in the same role (either buyer or seller) throughout the experiment. To avoid reputation formation and other repeated game effects, subjects are randomly re-matched with new partners at the start of every period. Matches were made within matching groups of six. Hence, a session with 24 participants yields four matching groups. Subjects in distinct matching groups cannot be paired.

If a trade is agreed upon, the seller produces the good at cost c. c is randomly drawn from the set {1, 2,..., 74, 75} (with equal probability for each element) at the beginning of every period and made known only to the seller. The good has value v to the buyer, which is private information. v is randomly drawn (with equal probability) from the set {25, 26,..., 99, 100} at the beginning of every period and made known only to the buyer. All random draws are independent across periods and subjects. If in a period the players agree to trade at price p, earnings are $p–c$ for the seller and $v–p$ for the buyer. Accumulated earnings across 10 periods are exchanged for euro's and paid to subjects at the end of the experiment. If a trade is agreed upon, trade surplus is $v–c$. Note that this can vary between –50 and +99. The a priori probability that a buyer and seller can engage in a mutually beneficial trade (*i.e.*, the probability that v>c) is approximately 78%.

Negotiations are structured as follows.[22] Each period in the experiment can contain up to three rounds of negotiations. In the first round the seller makes an initial proposal a_1 (under the restriction that $a_1 \geq c$); the buyer reacts by accepting a_1 (in which case $p = a_1$) or by

[22] This following design choice is motivated by concerns of external validity. For example, procurement typically starts with the buyer allocating a budget for a good or service (represented by v). The seller is then invited to make an offer. The buyer may accept it or propose an alternative, lower price. Dependent upon various factors, including technical complexity and risks in developing and producing the requested good or service, one or more iterations are possible. In each iteration, the buyer and seller can accept the price offered by the other party or propose a new price.

submitting a bid $b_1 < a_1$ (under the restriction that $b_1 \leq v$). In the second round the seller can accept b_1 ($p = b_1$) or submit a proposal $a_2 > b_1$ (with $a_2 \geq c$). The buyer reacts by accepting a_2 ($p = a_2$) or by submitting $b_2 < a_2$ (with $b_2 \leq v$). In the third and final round the seller can accept b_2 ($p = b_2$) or submit $a_3 > b_2$ (with $a_3 \geq c$). The buyer then either accepts ($p = a_3$) or rejects a_3. This process may or may not result in a completed transaction within three rounds. Note that we attribute relatively strong negotiating power to the seller by giving her the opportunity to make a final take-it-or-leave-it offer. This, once again, reflects the situation in the procurement example motivating this research and more generally holds in situations with a monopolistic seller. Most buyers are, in the end, dependent on the firm's final offer.

As a benchmark, we ran 2 sessions (6 matching groups) without contract audits. In all other treatments, the buyer can initiate a contract audit – at a fixed fee of 5 francs – in order to acquire information about c.[23] As in real world markets, the option to choose an audit is only available at the start of negotiations, *i.e.*, in the first round, before the seller makes her first offer a_1. Having an audit conducted changes the buyer's earnings to $v - p - 5$, if an agreement is reached. If there is no sale, the buyer still has to pay for the audit, so her earnings are negative for that round (–5). The audit provides the buyer with an estimate, c_0, of the seller's true costs, c. This estimate is a noisy signal with expected value equal to c. More specifically, we first determine $c^*_0 = c + \varepsilon$, where ε is randomly drawn from the set {–15, –14,…, 14, 15}, with equal probability for each element in this set. Then: $c_0 = c^*_0$, if $1 \leq c^*_0 \leq 75$; $c_0 = 1$, if $c^*_0 < 1$ and $c_0 = 75$, if $c^*_0 > 75$.

For two of our hypotheses (specifically, H_{2B} and H_4), we introduce treatment variables that vary across subjects. The first intends to vary the pressure put on buyers to have an audit conducted (as to operationalize the second antecedent of TPB). In government institutions or

[23] We considered making the audit costs dependent on the buyer value, v. To aid us in this decision, we collected data from the contract audit department of the Dutch Ministry of Defense about hours spent on specific audits and the corresponding procurement budgets. We found no evidence of such a relation and therefore resort to the more simple case of fixed costs.

businesses, this pressure typically comes from higher levels of government or management, stressing the advantages of an audit. More generally, such pressure may come from principles in a principle-agent setting. Of course, there is no government or management in the laboratory. In many ways, however, participants may see the experimenter as an authority. In some sessions, we therefore stressed that the experimenter advised the use of contract audits. More specifically, before the start of round 5, the following text appeared on the buyers' screens:[24]

> "As organizers of this experiment, we are in favor of you using an audit. In our experience, this is beneficial for price formation. We therefore advise that you use an audit in every period."

Additionally, before the start of round 8, the following text was sent to buyers:

> "We once again point out that we, as organizers of this experiment, are in favor of you having an audit conducted. In our experience, this is beneficial for price formation. We therefore advise that you use an audit in every period."

The treatments with messages will be used to test the effect of pressure as formalized in H_{2B}. Note that the statements we provide to participants are intended to create something that may be interpreted as an 'experimenter demand effect'. Whereas such effects should usually be avoided because they cause unintended and uncontrolled influences on subject choices, they are intended and controlled in our design. They serve to mirror parallel influences (pressure) that occur outside of the laboratory. Note that in the experiment, the advice given runs counter to the direct economic interests of the participants, since (as explained above) in theory there is no economic gain to be obtained by using an audit.

Our second treatment variable deals with the information sellers have with respect to the contract audit and serves to create cross-treatment differences in information asymmetry

[24] Obviously, subjects were not informed that there would be messages. Therefore, rounds 1-4 are identical in treatments with and without messages.

that may affect negotiators' fairness perceptions. Sellers always know whether or not an audit is conducted. Whether or not the outcome of the audit is known to the seller before negotiations is varied across sessions. This will allow us to test H_4.

We ran a full 2x2 design of these two treatments, with 2 sessions (8 matching groups) per treatment cell. Table 1 summarizes these treatments with contract audits and introduces the labels we will use to describe each treatment combination.

Table 1: Treatment Overview

	No Pressure	**Pressure**
Seller not Informed	*noPressure/noInfo*	*Pressure/noInfo*
Seller Informed	*noPressure/Info*	*Pressure/Info*

To test hypotheses H_{2A} and H_{2C}, we need information about the buyers' attitudes towards contract audits as well as their perceived behavioral control. We collect this information in a post-experimental questionnaire.[25] In particular, we used the following two items. Each of which required a response on a 7-point Likert-scale varying from "completely disagree" to "completely agree".[26]

Item 1:"By using audits, considerable amounts can be saved on prices paid for goods."

This item measures the subject's *attitude* towards contract auditing. It is an indication of the subject's belief that contract auditing may contribute to the realization of the goal to

[25] There is, of course, when applying a questionnaire after the actual decision, a danger that cognitive dissonance will bias responses towards choices made. Subjects may attempt to justify the use of an audit by responding that money may be saved and that an audit is a good tool. On the other hand, applying the questionnaire first would potentially steer subjects in certain directions just by the nature of the questions. We consider the potential cognitive dissonance the lesser of the two evils but recognize that we, in the end, can only measure a correlation between decisions and responses, not a causal relation.
[26] The complete questionnaire used is available from the authors upon request.

economize on contract prices. The higher the score on this item, the more positive the subject's attitude towards contract auditing.

Item 2: "In this experiment, the buyer has a good tool available, which can be used to get information about the seller's costs and a judgment as to how reasonable the seller's ask price is."

This item measures the subject's *perceived behavioral control*, which is an indication of how realistic the subject finds it that he actually can have a contract audit done. The higher the score on this item, the more the subject believes he can have a contract audit done.

Finally, we will test hypotheses H_{1A}, H_{1B}, (the chance of agreements and the duration of negotiations) and H_3 (prices with and without contract audits) with data on agreements, negotiations, prices and audit costs as collected in our laboratory sessions.

To summarize, our experimental design allows us to test H_{1A}, H_{1B} and H_3 using observed trades with and without contract audit; H_{2A} and H_{2C} by using responses to the post-experimental questionnaire; and H_{2B} and H_4 by comparing across treatments (pressure and seller information, respectively). More formally, denote by *ca* a dummy variable equal to 1(0) if buyer-seller negotiations took place after (without) a contract audit. *a* and *o* give, respectively, the average numbers of agreements and offers needed to obtain agreement. H_{1A} and H_{1B} can then be tested using our experimental data by:

H_{01}: $a|_{ca=0} = a|_{ca=1}$ versus H_{1A}: $a|_{ca=0} < a|_{ca=1}$;

H_{01}: $o|_{ca=0} = o|_{ca=1}$ versus H_{1B}: $o|_{ca=0} > o|_{ca=1}$.

For testing H_{2A} using questionaire responses, let $\alpha_i \in \{1,\ldots,7\}$ denote subject *i*'s agreement with item 1 and for H_{2C} define $\gamma_i \in \{1,\ldots,7\}$as *i*'s agreement with item 2. CA_i gives the fraction of periods in which buyer *i* chose to have a contract audit and $CA(t)$ denotes the

average fraction of contract audits in treatment *t*. Then, H_{2A}, H_{2B}, and H_{2C} can be formally tested as follows with our laboratory data:

H_{02}: $CA_i = CA_j$, $\forall i,j$, versus H_{2A}: $CA_i > CA_j$, if $\gamma_i > \gamma_j$;

H_{02}: $CA(t_1) = CA(t_2)$, $\forall t_1, t_2$, versus H_{2B}: $CA(Pressure/\Delta) > CA(noPressure/\Delta)$;

H_{02}: $CA_i = CA_j$, $\forall i,j$, versus H_{2C}: $CA_i > CA_j$, if $\gamma_i > \gamma_j$.

Finally, let $p(t)$ denote the average agreed upon price in treatment *t*. H_3 and H_4 are tested by:

H_{03}: $p(\Delta)|_{ca=0} = p(\Delta)|_{ca=1}$ versus H_3: $p(\Delta)|_{ca=0} > p(\Delta)|_{ca=1}$;

H_{04}: $p(Info)|_{ca=1} = p(NoInfo)|_{ca=1}$ versus H_4: $p(Info)|_{ca=1} > p(NoInfo)|_{ca=1}$.

5. Benchmark Predictions

For the parameters in our experiment we can easily characterize the Nash equilibrium in the negotiations, if we assume risk neutrality and payoff-maximizing behavior. Using backward induction, the monopolist will determine the price that maximizes expected payoff in round 3 of the negotiations and the buyer will accept any offer that gives her positive earnings. For the final round of negotiations, we therefore have the following equilibrium strategies:[27]

$$\textit{Seller}: \quad \textit{choose } a_3^* \equiv \arg\max_a \Pr(a < v) * (a - c) + \Pr(a \geq v) * 0$$

$$\textit{Buyer}: \quad \begin{cases} \textit{accept } a_3^* \textit{ if } v > a_3^* \\ \textit{reject } a_3^* \textit{ if } v \leq a_3^* \end{cases}. \qquad (1)$$

Knowing equilibrium behavior in the final round, all subgame perfect Nash equilibria consist of seller asking at least a_3^* in rounds 1 and 2 and the buyer applying the same acceptance rule in all three rounds, while offering any $b_i \leq a_3^*$ if she rejects in rounds *i*=1,2. Finally, there are equilibria where the buyer rejects in round 1 or 2 and offers $b_i = a_3^*$, which is subsequently accepted by the seller. Though this yields a large set of subgame perfect equilibria, the

[27] We assume here that the buyer will reject if indifferent (i.e., the offer is equal to her value), but qualitatively nothing changes if we assume she would accept in this case.

outcome is always the same: there is a trade at price $p = a_3^*$, if $v > a_3^*$, and no trade otherwise. Finally, some straightforward calculations yield $a_3^* = 50 + \frac{c}{2}$.[28] Because prices in the experiment were integer numbers, the equilibrium price is the integer closest to a_3^*. Note that this implies that $p > c$ for any transaction, because $c \leq 75$. This equilibrium holds for all of our treatments, irrespective of the possibility of an audit. In fact, in this equilibrium, buyers will never initiate a costly audit because it will not affect the price. Note that in this equilibrium, hypotheses 1, 2, 3 and 4 are redundant.

Note that the equilibrium probability that a trade will take place is decreasing in *c*. For $c = 1$, sellers will ask 51 and this will be accepted in 49/76 (64%) of the cases. For $c = 75$, the equilibrium is for sellers to ask 88, which will be accepted only 12/76 (16%) of the time. On average, agreements will be reached in 41% of the negotiations. Hence, the Nash equilibrium is inefficient (recall that in 78% of the cases a mutually profitable trade is possible). Moreover, conditional on an agreement, the expected distribution of surplus always favors the seller.[29] Hence, the equilibrium may change if preferences are non-selfish, as assumed in social preference theory (e.g., Fehr and Schmidt 1999; Bolton and Ockenfels 2000). In particular, a seller may be willing to reduce the price to share surplus more equally with the buyer.[30] In fact, maintaining the assumption of risk neutrality, fairness considerations will always lead to lower prices than in the subgame perfect Nash equilibrium based on self-interest. Moreover, if fairness considerations are stronger when the trading partner is aware of the distribution (*i.e.*, this means that the buyer has information about the seller's costs), than a contract audit may lead to a reduction of the price. This supports the reasoning underlying

[28] Given the distribution of *v*, $\Pr(a<v)=(100-a)/76$. The first derivative of the seller's earnings to *a* is then $(-2a+100+c)/76$.
[29] For the lowest costs, $c = 1$, the seller will earn 50 and the average buyer will have $v = 75.5$ and earn 24.5. At the other extreme ($c = 75$), the seller earns 13, the average $v = 94$, giving expected buyer earnings of 6.
[30] Of course, a price reduction by one unit will decrease the seller's earnings by one unit but increase the buyer's expected earnings by less than one, because the buyer's expected value decreases as the set of values larger than the ask price increases by one.

hypothesis 3. If, this effect is weakened when the seller knows what information the buyer has received, this provides support for H_4.

6. Results

In this section, we first analyze the choice to have contract audits undertaken and test the hypotheses we derived from TPB (H_{2A}, H_{2B}, H_{2C}). Then, we study the extent to which agreements are reached and the consequences this has for market efficiency. This includes our tests of H_{1A} and H_{1B}. This is followed by an analysis of the consequences of audits, i.e., the prices and observed distribution of surplus between buyers and sellers. This includes our tests of H_3 and H_4.

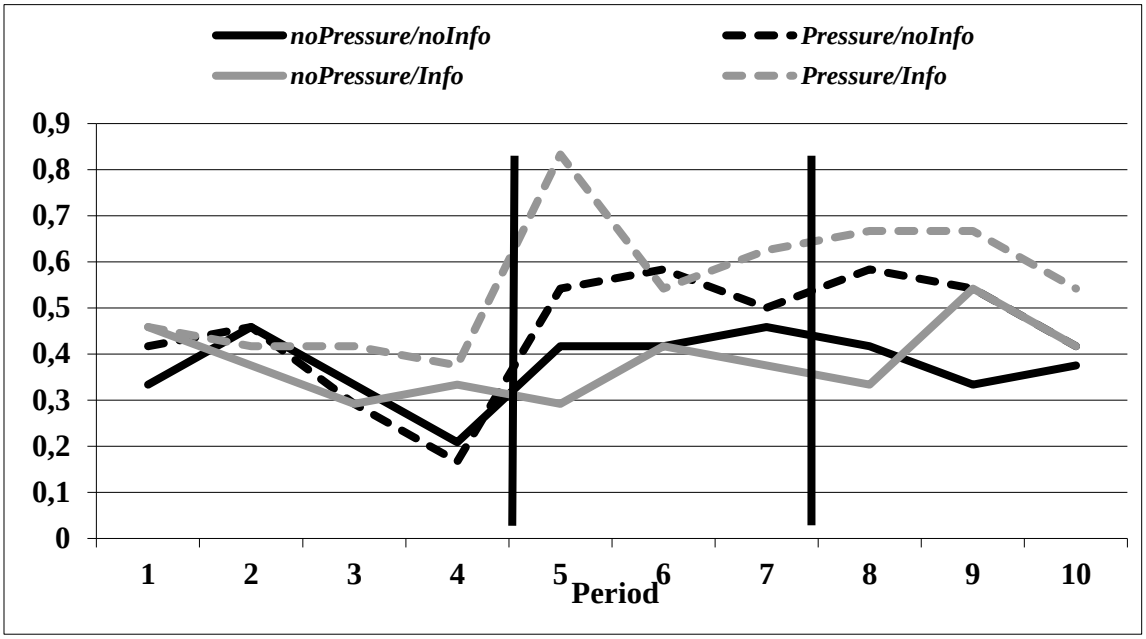
In aggregate, we observed 1140 negotiations (38 matching groups, 10 rounds, 3 pairs per round), but the interaction between subjects causes statistical dependencies within these observations. Given that we do not know the structure of the correlations across observations, unless indicated otherwise, we treat the matching group as the independent unit of observation. Hence, each session provides us with four independent observations for our statistical tests.[31]

6.1. Contract Audits

On average, audits were initiated in 44% of the cases where they were possible. This varied across treatments, however. Figure 2 shows the development of the fraction of audits for the various treatments.

[31] An exception is the first session which only had 12 participants (in two matching groups).

Figure 2: Contract Audits across Periods



Notes. Lines show across rounds the fraction of negotiations in which contract audits were initiated. Vertical lines indicate the moment in which messages were sent to buyers in the *Pressure/noInfo* and *Pressure/Info* sessions. Treatment acronyms are defined in table 1.

From the figure, it would appear that seller information about the outcome of the audit does not matter much (the differences between *noPressure/noInfo* and *noPressure/Info* and between *Pressure/noInfo* and *Pressure/Info* are small). In fact, differences across treatments appear to be small in the first four periods. On the other hand, the message sent to buyers at the start of periods 5 and 8 does seem to boost the number of audits done. A direct statistical test pertaining to this effect will be presented when discussing the results with respect to our hypotheses, below. Before doing so, we present a multivariate probit analysis aimed at explaining the buyer's decision to ask for a contract audit. Table 2 presents the results. The table's note specifies the regression equation.

Table 2: The Demand for Contract Audits

variable	coefficient	z-value
constant	–1.343	6.83**
1 After Pressure	0.813	4.63**
2 After Pressure	0.577	3.22**
3 After Pressure	0.433	2.32*
Info to Seller	0.140	0.81
Period	0.002	0.10
Buyer Value	0.015	7.30**

Notes. Columns 2 and 3 show the estimated coefficient vector (β) and its *z*-value, respectively, of the random effects probit estimation of $Pr_{it} = \Phi(\sum_i X'_{it}\beta + \mu_j)$ where Pr_{it} gives the probability that buyer *i* in matching group *j* will demand an audit in period *t*. Φ denotes the cumulative normal distribution and *X* is the vector of independent variables described in the first column. μ_j is a (white noise) matching-group specific error that corrects for the independence of observations caused by the interaction between individuals within a matching group. The independent variables are defined as follows. *1 After Pressure* = 1 in the period directly following the message projected on the buyer's screen (i.e., periods 5 and 8) and 0, otherwise. *2 After Pressure* = 1, in the 2nd period after the message (periods 6 and 9) and 0, otherwise. *3 After Pressure* = 1, in the 3rd period after the message (periods 7 and 10) and 0, otherwise. *Info to Seller* = 1, in sessions where sellers know the outcome of the audit and 0, otherwise. *Period* = number of the period ($\in\{1,\ldots, 10\}$). *Buyer Value* = realized *v*. *=statistically significant at the 5%-level; **= statistically significant at the 1%-level.

The results confirm that the fact that sellers receive information about the outcome of the audit does not affect the buyer's decision to have one done. Moreover, after correcting for other variables, there is no trend across periods. The message broadcast on the buyer's screen with the experimenter's endorsement of audits does have a strong and significant effect, however. This effect gradually wears down, though, as the coefficient for the period directly following the endorsement is 0.813 whilst it is down to 0.433 two periods later. Finally, a buyer is more likely to ask for an audit, the higher her value (v) for the good is. This may be caused by a combination of two effects. First, the lower v is, the lower the expected surplus will be, and hence the lower the room to negotiate. Buyers with a low value will tend to accept any prices lower than v. Moreover, the expected profit is lower for low v, so the costs of an audit will weigh more heavily on the buyer. In short, fewer contract audits are purchased for lower buyer valuations, because a buyer with low value does not believe that the audit will help her earn more money. In contrast, a higher value increases the potential surplus a buyer

is trying to capture and an audit that helps capture this will therefore be viewed as more valuable.

To test H_{2A} and H_{2C} (derived from TPB) we use subjects' responses to the post experimental questionnaire, as described in section 4. In the four treatments with audits all 96 buyers filled out the questionnaire. Table 3 gives an overview of their responses to the questions concerned and the fractions of subjects who initiated a contract audit. Focusing first on the rows denoted by "Response" we see that for both questions responses are distributed across the full spectrum of the extent of agreement.

Table 3: Questionnaire Responses

Response category:		**1**	**2**	**3**	**4**	**5**	**6**	**7**
Attitude	Response	0.10	0.11	0.13	0.19	0.20	0.18	0.09
	Audit	0.22	0.27	0.34	0.37	0.60	0.49	0.72
Perceived Behavioral Control	Response	0.01	0.08	0.10	0.11	0.31	0.23	0.15
	Audit	0.20	0.23	0.22	0.36	0.42	0.54	0.68

Notes. *Attitude* is measured by the extent of agreement to the statement: "By using audits, considerable amounts can be saved on prices paid for goods." *Perceived Behavioral Control* is measured by the extent of agreement to the statement: "In this experiment, the buyer has a good tool available, which can be used to get information about the seller's costs and a judgment as to how reasonable the seller's ask price is." In both cases, responses vary from 1: "completely disagree" to 7: "completely agree". The rows "Response" show the distributions across the seven levels of agreement. The rows "Audit" show the fraction of subjects at each level of agreement that chose to have an audit done.

First consider H_{2A}, which predicts that buyers with a more positive attitude towards contract audits are more likely to initiate them (formally: $CA_i > CA_j$, if $\alpha_i > \alpha_j$). In table 3, the attitude towards audits increases as we move from left to right. We indeed observe that the fraction of audits increases almost monotonically. This increase is statistically significant (Kruskal-Wallis, $\chi 2 = 26.29$ ($p<0.01$). Hence, the null hypothesis of no effect ($CA_i = CA_j$, $\forall i,j$) is rejected in favor of H_{2A}.

H_{2B}, that government pressure enhances the use of audits (H_{2B}: $CA(Pressure/\delta) > CA(noPressure/\delta)$ is tested by our treatment where messages are sent to the buyers at the start of rounds 5 and 8. The regressions reported in table 2 provide statistical support for this hypothesis: more audits are requested in the periods following these endorsements. We can

also test this hypothesis directly within the treatment with endorsements. Comparing the use of audits in periods 1-4 (i.e., before any message was sent) to that in periods 5-10, we see the fraction of audits increasing from 0.38 to 0.59. This difference is statistically significant (Wilcoxon signed rank test, Z = -2.692, $p<0.01$).[32] Once again, the null hypothesis of no effect ($CA(t_1) = CA(t_2)$, $\forall t_1, t_2$) is rejected in favor of H_{2B}.

Finally, H_{1C} predicts that buyers with a higher perception of control are more likely to request a contract audit (H_{2C}: $CA_i > CA_j$, if $\gamma_i > \gamma_j$). We test this using the data reported in the last two rows of table 3. Again, moving from left to right in the row marked "Response" indicates more agreement, *i.e.*, higher perceived control. We observe that the fraction of audits increases (almost) monotonically from left to right. This effect is statistically significant (Kruskal-Wallis, $\chi 2 = 22.64$, $p<0.01$) and the null ($CA_i = CA_j$, $\forall i,j$) is again rejected in favor of the alternative H_{2C}.

This support for the three hypotheses H_{2A}, H_{2B} and H_{2C} demonstrates that the antecedents (attitude, subjective norms and perceived behavioral control) affect the decision to have a contract audit done in the way predicted by the TPB. This supports application of this theory to individuals' decisions to initiate contract audits.

6.2. Agreements and Efficiency

The overall percentage of agreements reached was 59.8 %. Table 4 gives the fraction of cases in which price agreement was reached per treatment. The table shows that this percentage is slightly higher in the sessions where we endorsed audits in rounds 5 and 8 (*Pressure/noInfo* and *Pressure/Info*). However, differences between treatments are not significant. The table also shows that agreements are much more frequent after a contract audit has been executed

[32] This effect is not due to experience. For the sessions without endorsements the fraction of audits increases only slightly, from 0.35 to 0.40 (Wilcoxon signed rank test, Z = -1.062, not significant).

Table 4: Agreements across Treatments

Treatment	agreements
Benchmark	0.57
noPressure/noInfo	0.58
noPressure/Info	0.59
Pressure/noInfo	0.61
Pressure/Info	0.63
Overall	**0.60**
Contract Audit	0.71
No Contract Audit	0.53^{+}

Notes. Numbers present the fraction of cases in which an agreement was reached. Cases above the overall fraction refer to treatments (cf. section 3): Benchmark: session without audits. Below the overall fraction we show results for trades with and without audit. $^{+}$0.52 if sessions where contract audits are impossible are excluded.

(71% *vs*. 52%). This difference is statistically significant (Wilcoxon signed rank test, Z = – 3.796, $p<0.01$). The higher fraction of agreements after an audit is direct support in favor of hypothesis H_{1A} ($a|_{ca=0} < a|_{ca=1}$), rejecting the null of no differences (H_{01}: $a|_{ca=0} = a|_{ca=1}$). This reduction in transaction costs (more successful trades) may be due to the fact that potential trade surplus is on average higher for trades with an audit: the average difference between value and costs ($v–c$) is 30.2 in negotiations with audit and 21.1 when no audit is initiated.[33]

A second way in which audits may affect transaction costs is through the efficiency of the negotiation process itself. We measure this by the average number of offers made before an agreement is reached. This is shown in table 5.

Table 5: Average Number of Offers made before Agreement is reached

	Benchmark	***noPressure/noInfo***	***noPressure/Info***	***Pressure/noInfo***	***Pressure/Info***	**Overall**
No Audit	5.83	5.84	5.85	5.86	5.85	5.85
Audit	--	5.76	5.88	5.89	5.80	5.83

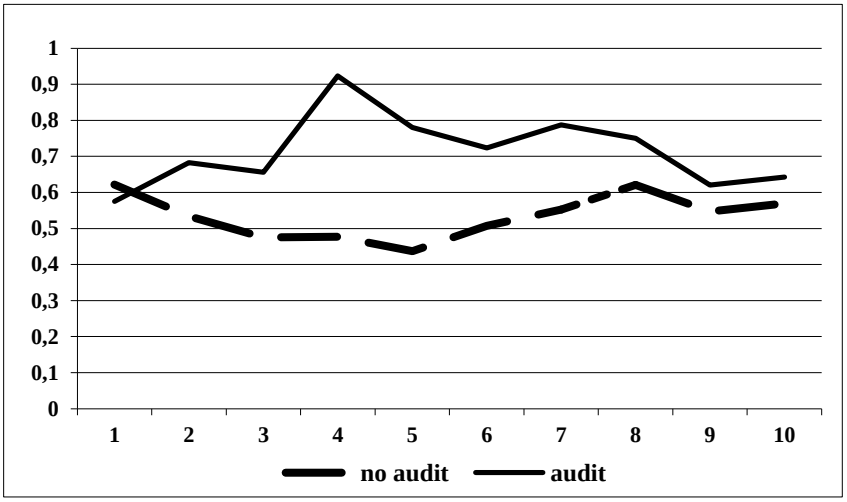
Notes. Cells represent average number of offers made before agreement is reached for the treatment denoted by the column, distinguishing between trades with and without audit in the rows. Note that two offers (bid and counterbid) equal one round of negotiation and the maximum number of possible offers is six.

[33] As we observed in table 2, buyers with high values are more likely to request audits. On average, higher values lead to higher surplus. This may explain the higher surplus when there are audits. We will see below, however, that audits increase the probability of agreement even after correcting for buyers' values and sellers' costs.

The table shows that differences across treatments and between agreements with and without audit are very small. None of these differences are statistically significant. Hence, we find no support for H_{1B} that contract audits result in less effort to negotiate an agreement (H_{1B}: $o|_{ca=0} > o|_{ca=1}$). [34]

Figure 3 shows the development of agreements across periods. It exhibits no clear trend, indicating no development as subjects gain experience. Because the task at hand is not a difficult one *per se*, we conclude that from the beginning, subjects understand what they are asked to do. Moreover, note that the difference between the fractions of negotiations that lead to agreement with and without audit exists in all periods except the first.

Figure 3: Agreements across Periods



Notes: Lines show fraction of agreements across periods, aggregated across all treatments.

To further study the determinants of reaching an agreement, we ran a random effects probit model explaining their occurrence. Table 6 presents the results, which clearly show that the treatment variables do not affect the probability of an agreement. Both the seller's costs and the buyer's value do, however. As expected, agreement is less likely for higher costs or lower values. Furthermore, even when correcting for costs and values, we observe that agreements

[34] As pointed out by an anonymous referee, the 'costs' of an additional round of negotiations are small in our experiment. There is no explicit cost to further negotiation, just time, which is barely an issue because participants have to wait for the next round, anyway. Introducing explicit costs of negotiation rounds is certainly an interesting idea for future experiments.

are more likely after a contract audit has been done. This may be an indirect consequence of sellers adjusting their ask prices after audits. This random effects probit model again provides strong support for H_{1A}: contract audits result in a higher frequency of price agreements.

Table 6: Agreements

variable	coefficient	z-value
constant	–1.929	8.92**
Benchmark	0.063	0.37
After Pressure	0.050	0.34
Info to Seller	–0.137	1.15
Period	0.017	0.81
Seller Costs	–0.053	16.0**
Buyer Value	0.069	18.3**
Audit Done	0.331	2.62**

Notes. Columns 2 and 3 show the estimated coefficient vector (β) and its *z*-value, respectively, of the random effects probit estimation of $Pr_{lt} = \Phi(\sum_1 X'_{lt}\beta + \mu_j)$ where Pr_{lt} gives the probability that pair l in matching group j reaches an agreement in period t. Φ denotes the cumulative normal distribution and X is the vector of independent variables described in the first column. μ_j is a (white noise) matching-group specific error that corrects for the independence of observations caused by the interaction between individuals within a matching group. The independent variables are defined as follows. *Benchmark* = 1 when no audits are possible, and 0, otherwise. *After Pressure* = 1, in periods after the message has been projected on the buyer's screen (*cf.* section 3), and 0, otherwise. *Info to Seller* = 1, in sessions where sellers know the outcome of the audit and 0, otherwise. *Period* = number of the period ($\in\{1,\ldots, 10\}$). *Seller Costs* = realized *c*. *Buyer Value* = realized *v*. *Audit Done* = 1 if an audit had been conducted, and 0, otherwise. *=statistically significant at 5%-level; **= statistically significant at 1%-level.

Aside from the effect of a contract audit *per se* on the frequency of agreements, the *outcome* of the audit may have an effect. This especially applies when both parties (buyer and seller) know the audit result before negotiations start, as in the *noPressure/Info* and *Pressure/Info* treatments. We calculated the fraction of agreements for distinct differences between the audit result and actual costs. A lower difference might make negotiations easier by better matching both sides' expectations. Table 7 shows the results.[35]

[35] We thank an anonymous referee for suggesting this analysis.

Table 7: Agreements based on Audit

Δ between audit result and *c*	*noPressure/noInfo & Pressure/noInfo*		*noPressure/Info & Pressure/Info*		**Overall**	
$-15 \geq \Delta < -10$	0.58	43	0.77	35	0.67	78
$-10 \geq \Delta < -5$	0.29	17	0.78	32	0.61	49
$-5 \geq \Delta < 0$	0.69	48	0.69	36	0.69	84
0	1.00	2	1.00	1	1.00	3
$0 > \Delta \leq 5$	0.83	41	0.84	44	0.84	85
$5 > \Delta \leq 10$	0.76	21	0.62	26	0.68	47
$10 > \Delta \leq 15$	0.65	26	0.73	51	0.70	77
Overall	0.67	198	0.75	225	0.71	423

Notes. The first column presents a classification of the size of the difference between the audit result and *c*. Recall $c^*_0 = c + \varepsilon$, where ε is randomly drawn from the set {−15, −14,…, 14, 15}. Hence, $\Delta = c^*_0 - c = \varepsilon$. Numbers in the columns 2, 4 and 6 present the fraction of cases in which an agreement was reached, only for those cases in which an audit was requested. Numbers in the columns 3, 5 and 7 present the corresponding number of cases (= number of audits) used for calculating the fractions.

Only a few systematic patterns can be distinguished in these data. Most noticeable is perhaps the case where the difference is 0 and agreements are always reached. This result is based on a very small number of cases (3 in total) however. A second observation is that small positive differences ($0 > \Delta \leq 5$) yield more agreements than larger (or negative) differences. In these cases the buyer receives a slight overestimation of true costs. Irrespective of whether the seller knows this estimate, many agreements are reached. In general, however, the size of the difference between estimated and actual costs does not affect agreements.

The extent to which agreements are reached is the most important determinant of efficiency in this environment. Efficiency basically requires that agreement is reached if and only if $v > c$.[36] Realized surplus without (with) audit is $v - c(-5)$ if a trade is realized and 0 (–5), otherwise. Table 8 presents realized surplus as a fraction of potential surplus across treatments

Table 8: Efficiency

		Benchmark	*noPressure/noInfo*	*noPressure/Info*	*Pressure/noInfo*	*Pressure/Info*
No Audit		0.91	0.88	0.82	0.89	0.89
Audit	Gross Efficiency	--	0.93	0.96	0.95	0.95
	Net Efficiency	--	0.83	0.85	0.84	0.85

Notes. Numbers show fraction of potential trade surplus realized. Treatment acronyms are defined in table 1. *Gross (Net) Efficiency* measures realized surplus before (after) deducting audit costs.

[36] Recall that the Nash equilibrium outcome is inefficient.

distinguishing between trades with and without audit. For the cases with audit, the table distinguishes between gross and net efficiency, where the latter deducts audit costs from the realized trade surplus. The table shows that all experimental markets (treatments) are highly efficient. In all cases more than 80% of possible surplus is realized, in spite of the incomplete information about the other negotiator's costs or value. When an audit is used to alleviate this incompleteness, realized gross efficiency is always higher, which is a direct consequence of the positive effect that audits have on agreements (together with the fact that subjects almost never trade at a loss). Across all treatments (excluding the benchmark) the difference in gross efficiency with and without audit is statistically significant (Wilcoxon signed rank test, Z = -2.749, $p<0.01$).

Taking account of audit costs, net efficiency is only improved by audits in the *noPressure/Info* treatment (note that net efficiency and gross efficiency are equal in the no-audit control treatment).[37] Finally, across all treatments, the difference between net efficiency without and with audit is statistically insignificant. Considering the difference per treatment, it is only significant for the *Pressure/noInfo* case (Wilcoxon signed rank test, Z = -1.820, $p<0.05$). Of course, the outcomes with respect to net efficiency are closely related to the choice of parameters, including audit costs equal to 5 francs and an accuracy of the estimate in a 30 franc band. For example, setting the audit costs at any other amount x directly changes realized surplus ($v–c–x$) and net efficiency.[38] The chosen costs are relatively high. Given the distributions of values and costs, the expected surplus from trade ($v–c$) is 62.5 – 37.5 = 25, so

[37] The result for *NoPressure/Info* seems mainly due to the relatively low efficiency of 0.82 without audits. We see no obvious reason for this low efficiency. Neither the number of trades nor the number of audits is exceptional in this treatment. A closer look at the data shows that this low efficiency is not due to trades with negative surplus (*i.e.*, $c > v$), either. What remains is the simple observation that traders for some reason were less able to reach agreement when it was beneficial to both to do so.

[38] In a second series of treatments we experimented with other (lower) levels of audit costs. In these treatments audits improve net efficiency. We will report on these new experiments in future work.

the costs constitute 20% of the expected surplus.[39] For these reasons, we prefer to analyze the effects on gross efficiency, keeping in mind that efficiency-enhancing contract audits do come at a cost. These costs, however, are included in the gross efficiency measure; they constitute a transfer from the buyer to the auditor, and an argument can be made that they should be included in welfare considerations.

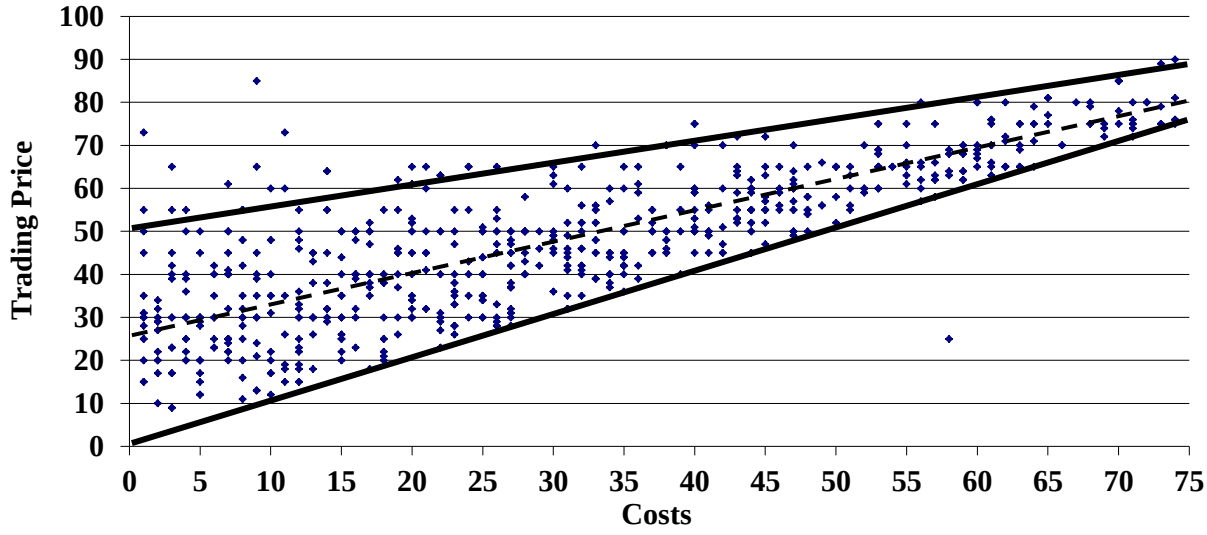
6.3 Price Formation and Surplus Division

As derived in section 5, traditional economic reasoning assuming self-interested preferences yields a subgame perfect Nash equilibrium where sellers ask a price that increases linearly in their costs: $a_3^* = 50 + \frac{c}{2}$. Some of these ask prices will be accepted, others will not, depending on the buyer's value, *v*. Within the set of accepted asks, the equilibrium price is therefore predicted to have the same relationship to costs: $p = 50 + \frac{c}{2}$. As argued above, prices below this line may indicate social preferences. To start, figure 4 therefore shows a scatterplot showing observed cost–price combinations and their relationship to this line.

It is clear from this figure that almost all transactions are at prices between costs and the Nash prediction. There is only one trade where $c > p$.[40] The estimated linear relationship shows that on average sellers are willing to seriously reduce prices compared to the equilibrium based on self-interest. To further investigate the extent to which they are willing

[39] We thank an anonymous referee for pointing this out.
[40] This occurred in the first period of the first session, when a seller accepted a bid below her or his costs.

Figure 4: Prices vs. Costs



Notes. Dots indicate for all trades the combination of seller's costs and agreed upon price. The lower line indicates the points for which $p = c$, i.e., seller earnings are zero. The upper line shows the relationship p=50+c/2 that is predicted by the subgame perfect Nash equilibrium for the negotiations. The dashed line represents the relationship p = 26.81 + 0.686*c, which is the random effects OLS relationship estimated from all trades.

to reduce prices, we estimate an index of deviation from the equilibrium, denoted by φ. Using the cost level as a lower benchmark for this deviation, we define:

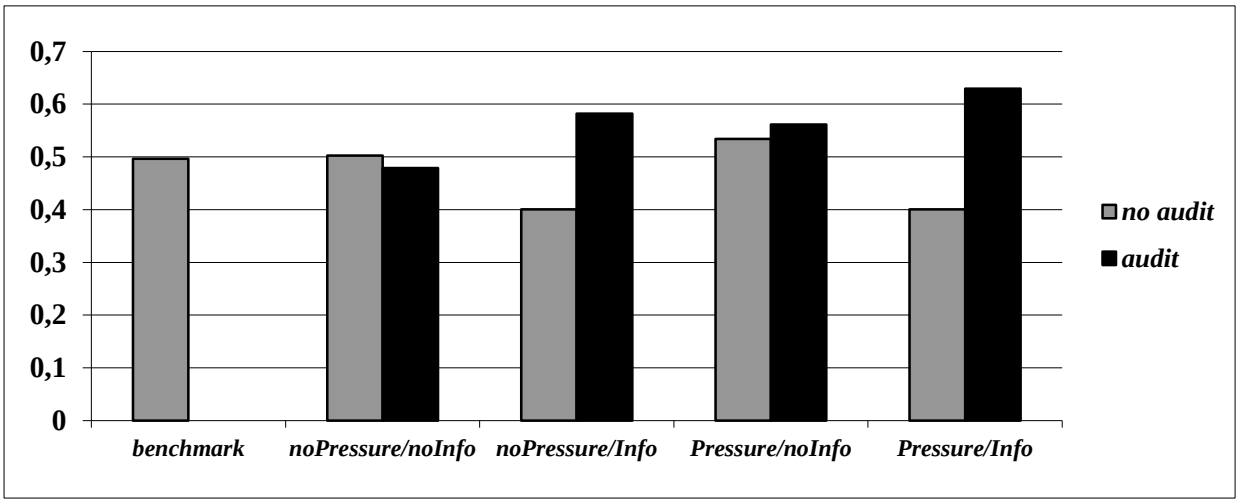
$$\varphi \equiv \frac{a_3^* - p}{a_3^* - c} = \frac{50 + c/2 - p}{50 - c/2}. \qquad (2)$$

Note that $\varphi > 0$ (< 0) for prices below (above) the subgame perfect Nash equilibrium for self-interested sellers (a_3^*). Moreover, $\varphi > 1$ (<1), for prices below (above) seller's costs. Hence a value $0 < \varphi < 1$ indicates a price between costs and the equilibrium. The closer φ is to zero, the closer the price is to the equilibrium. Figure 5 shows the average value of φ for our treatments.

It appears that in most cases, prices end up about halfway between costs and the equilibrium price. This also holds for the treatments where sellers do not know the outcome of the audit ('*noInfo*'), irrespective of whether or not an audit was done. Things change when sellers do know the outcome of an audit. In both *noPressure/Info* and *Pressure/Info*, sellers

are willing to move approximately 60% of the way towards their own cost level if the buyer had an audit conducted. Interestingly, they seem to compensate this in negotiations without

Figure 5: Average Deviation from Equilibrium



Notes. Bars show the average value of φ, as defined in eq. (2). Treatment acronyms are defined in table 1.

audit. In contrast to all other cases, they only deviate from the equilibrium by 40% when the buyer does not ask for an audit in a situation where the seller would know its outcome. Differences in the mean deviation from equilibrium between the audit and no audit choices (Figure 5) are significant for the *noPressure/Info* treatment (Wilcoxon signed rank test, Z = -2.366, p<0.01) and the *Pressure/Info* treatment (Wilcoxon signed rank test, Z = -2.380, p<0.01).

The prices at which agreements are reached directly determine the division of surplus between buyer and seller. Obviously, the higher the price is, the lower the buyer's part of trade surplus will be. Denote the buyers share in the surplus of a realized trade at price p by $\theta_b \equiv \frac{v-p}{v-c}$, with $0 \leq \theta_b \leq 1$. In section 5 we showed that in the equilibrium for self-interested sellers the buyers get less than half of the surplus, *i.e.*, $\theta_b < 0.5$. Downward deviations from equilibrium will increase the buyer's share, however. Indeed, across all trades, buyers in our experiment manage to appropriate over 50% of the surplus, the average value of θ_b is 0.58. Moreover, the results in figure 5 lead us to expect similar levels of θ_b in the benchmark and in

the *noPressure/noInfo* and *Pressure/noInfo* treatments. This is indeed what we observe, with the average value varying between 0.50 in the *noPressure/noInfo* cases where no audit is done and 0.62 when there is an audit in *Pressure/noInfo*. Buyers' success is (as expected from figure 5) more sensitive to audits when the seller knows the outcome. In *noPressure/Info*, the buyer share is only 0.45 without audit, but is up to 0.64 with audit. When there is also pressure (*Pressure/Info*), θ_b is on average 0.49 without audit. In the majority of these cases an audit is conducted, however, and the buyers manage to obtain more than 2/3 of the surplus: θ_b=0.69. All in all, the most effective way of making sellers reduce their prices and thereby increasing the buyers' share in trade surplus is by having contract audits conducted and making the result known to the sellers.

To obtain more insight in variables determining the buyer's share in trade surplus, we ran a multivariate regression with the θ_b as a dependent variable. To account for censoring at the left by *0* (buyers will generally not trade at a loss) and at the right by *1* (sellers will not trade at a loss) we use a random effects tobit regression, as specified in the note of table 9.

Table 9: Buyer Share

variable	coefficient	z-value
constant	0.425	13.57**
Benchmark	0.026	0.69
After Pressure	0.016	0.62
Info to Seller	–0.057	1.80
Period	–0.005	1.47
Surplus	0.003	6.89**
Audit Done	0.088	3.21**
Interaction	0.107	2.82**

Notes. Columns 2 and 3 show the estimated coefficient vector (β) and its *z*-value, respectively, of the random effects tobit estimation of $\theta_{mt} = \sum_m X'_{mt}\beta + \varepsilon_{mt}+\mu_j$ where θ_{mt} gives the buyer share reached by pair *m* in matching group *j*, conditional on an agreement being reached, left-censored at buyer share *0* and right-censored at buyer share *1*. *X* is the vector of independent variables described in the first column. ε_{mt} is white noise error and μ_j is a (white noise) matching-group specific error that corrects for the independence of observations caused by the interaction between individuals within a matching group. The independent variables are defined as follows. *Benchmark* = 1 when no audits are possible, and 0, otherwise. *After Pressure* = 1, in periods after the message has been projected on the buyer's screen (*cf.* section 3), and 0, otherwise. *Info to Seller* = 1, in sessions where sellers know the outcome of the audit and 0, otherwise. *Period* = number of the period ($\in\{1,\ldots, 10\}$). *Surplus* = (realized *v* - realized *c*). *Audit Done* = 1 if an audit had been conducted, and 0, otherwise. *Interaction* = 1 if *Info to Seller* = 1 and *Audit Done* = 1, and 0, otherwise. *=statistically significant at 5%-level; **= statistically significant at 1%-level.

Table 9 shows that the treatment variables themselves have no direct effect on buyer share. Furthermore we see that buyer shares are higher as surplus increases. It seems that when surplus is larger buyers also are able to negotiate relatively lower prices and thereby claim a larger part of the surplus, although this effect is small (0.3%-points). Important is what we learn from the effect of audits in table 9. The regression results show that after correcting for the other variables, an audit is predicted to increase the buyer share by almost 8.8%-points. This effect is statistically highly significant. The interaction between having contract audits conducted and making the result known to the sellers accounts for the biggest effect, however. On average buyer share rises by 10.7%-points when a contract audit is conducted and the result is made known to the seller.

We now turn to the hypotheses we formulated on the effects of contract auditing on prices (H_3: $p(\pi_B)|_{ca=0} > p(\pi_B)|_{ca=1}$) and the effects of seller information (H_4: $p(\pi_B|Info)|_{ca=1} > p(\pi_B|NoInfo)|_{ca=1}$). Table 10 gives the average trading price per treatment. Note the difference with the numbers underlying figure 5. In the figure, deviations from equilibrium are depicted, which corrects for differences in cost realizations across treatment. For testing the hypothesis, the prices themselves are more relevant.

Table 10: Average Prices

	Benchmark	***noPressure/noInfo***	***noPressure/Info***	***Pressure/noInfo***	***Pressure/Info***
No Audit	50.44	47.38	47.89	45.37	51.96
Audit	--	48.09	47.41	46.77	42.98

Notes. Cells represent average transaction prices for the treatment denoted by the column, distinguishing between trades with and without audit in the rows.

First note that average price differences are small, and across treatments no differences are statistically significant at conventional levels. Within treatments, the difference between trades with and without an audit is only significant for *Pressure/Info* (Wilcoxon signed rank test, $Z = -2.380$, $p<0.01$). Hence, with these tests we can only reject the null of no differences

in prices ($p(\cdot)|_{ca=0} = p(\cdot)|_{ca=1}$) in favor of H_3 for the treatment where buyers are pressured to use audits and sellers are told the outcome of the audit before the negotiations start. Moreover, for both the *noPressure* and *Pressure* treatments, trades after an audit are at a lower price when the seller knows the outcome than when she does not. These differences are not significant for the treatments without pressure (Wilcoxon signed rank test, Z = –0.471) nor when we create a pressure to demand an audit (Wilcoxon signed rank test, Z = –0.157), however. Hence, the null hypothesis of no effect H_{04} ($p(\cdot/Info)|_{ca=1} = p(\cdot/NoInfo)|_{ca=1}$) cannot be rejected in favor of H_4 ($p(\cdot/Info)|_{ca=1} > p(\cdot/NoInfo)|_{ca=1}$.

The prices in table 10 are confounded with differences in realized costs and values, however. To obtain a better understanding of the prices agreed upon, we ran a multivariate regression with the price as a dependent variable. To account for censoring at the left by *c* (sellers will generally not trade at a loss) and at the right by *v* (buyers will not trade a loss) we use a random effects tobit regression, as specified in the note of table 11.

Table 11: Price Agreements

variable	coefficient	z-value
constant	15.73	8.78**
Benchmark	–0.256	0.14
After Pressure	0.815	0.55
Info to Seller	1.703	1.24
Interaction	–4.146	2.15*
Period	0.229	1.64
Seller Costs	0.613	30.9**
Buyer Value	0.190	9.11**
Audit Done	–4.893	5.89**

Notes. Columns 2 and 3 show the estimated coefficient vector (β) and its *z*-value, respectively, of the random effects tobit estimation of $p_{kt} = \sum_k X'_{kt}\beta + \varepsilon_{kt}+\mu_j$ where p_{kt} gives the price agreed upon by pair *k* in matching group *j*, conditional on an agreement being reached, left-censored at costs *c* and right-censored at value, *v*. *X* is the vector of independent variables described in the first column. ε_{kt} is white noise error and μ_j is a (white noise) matching-group specific error that corrects for the independence of observations caused by the interaction between individuals within a matching group. The independent variables are defined as follows. *Benchmark* = 1 when no audits are possible, and 0, otherwise. *After Pressure* = 1, in periods after the message has been projected on the buyer's screen (*cf.* section 3), and 0, otherwise. *Info to Seller* = 1, in sessions where sellers know the outcome of the audit and 0, otherwise. *Interaction* = 1 if *Info to Seller* = 1 and *After Pressure* = 1, and 0, otherwise. *Period* = number of the period ($\in\{1,\ldots, 10\}$). *Seller Costs* = realized *c*. *Buyer Value* = realized *v*. *Audit Done* = 1 if an audit had been conducted, and 0, otherwise. *=statistically significant at 5%-level; **= statistically significant at 1%-level.

A first thing to note from this table is that the treatment variables themselves have no effect on the price. Information to the seller about the audit only affects prices if the buyer has been pressured to conduct audits, and *vice versa*.[41] There is also no trend across periods. Costs and values matter, however. Prices are higher as costs increase and also as values are higher. The latter does not necessarily mean that buyers influence the price in the negotiations, however. Because higher price offers by the sellers are more likely to be accepted by high-value buyers, a positive correlation between prices and values would be expected even if buyers had no chance to negotiate and could only accept or reject the seller's ask price.

Our main concern in table 11 is the effect of audits. The regression results show that after correcting for the other variables, an audit is predicted to decrease the price by 4.9 francs. This effect is statistically significant, providing strong support for H_3.

7. Discussion

In this paper we have investigated the reasons why buyers in a situation that mirrors procurement in monopolistic markets choose to initiate a contract audit and have carefully analyzed the effects of the audits. Both the antecedents and consequences of contract audits have thus far been neglected in the literature. With respect to the antecedents, our results confirm three hypotheses that follow directly from the theory of planned behavior. A positive attitude towards audits, perceived behavioral control over audits and (political) pressure to have audits conducted all positively contribute to the probability that an audit will be undertaken. As for the consequences of audits, our results confirm that audits reduce transaction costs by yielding more successful negotiations. They also yield lower transaction prices and higher gross efficiency. Finally, we observed that prices tend to be lower than those predicted by the

[41] We conclude this from the high (negative) coefficient for the interaction effect.

subgame perfect Nash equilibrium of our setup. In fact, without audit, the division of the surplus in trades is close to an equal split.

The fact that contract audits are used at all is remarkable in the light of standard economic theory, which predicts that they will not affect negotiations and will therefore not be used. In contrast, we find that contract audits are used for about half of the trades and increase the number of successful negotiations by almost 37%.[42] This is what causes the increased gross efficiency. The extent to which buyers benefit from the audits depends partly on whether or not the seller knows the outcome. In the markets we study, buyers tend to obtain approximately 50% of the gains from trade, even without audits. With audits, this increases to over 2/3 of the gains. Remarkably, the highest buyer gains from trade are observed when the audit results are known to the sellers. This statistically strong effect is difficult to rationalize with traditional theories. In particular, it is somewhat of an anomaly that buyers benefit from taking away their informational advantage. Finding explanations for this anomaly is an interesting avenue for future research.

The government's concern in this type of markets may not always be (gross) efficiency. For example, if the government is the buyer, it may be more interested in buyer surplus. Our findings indicate that it is not obvious that the increased buyer surplus will be large enough to cover the audit costs. At least for the parameters used in our experiments, the price reduction was insufficient to cover the costs of the audit. Further policy recommendations that follow from our findings depend on the desirability of contract audits, however. If the government has reasons to favor application of audits in procurement markets, it can stimulate their use by providing information to the government institutions concerned that creates positive attitudes and the perception that audits work. Combined with direct political pressure to apply audits, this will increase their use.

[42] If we just consider treatments that allow audits (see text under Table 4) then the exact calculation of this number is: 70.92% - 51.96 = 18.96%. 18.96%/51.96% = 36.49%.

Limitations of this study are germane to any experimental economics approach where markets are simulated in a computer laboratory and where often students are used as decision makers. These problems with external validity need to be weighed against the strong internal validity of laboratory experiments (*e.g.*, the control over costs and values and the possibility to vary treatments variables one at a time). For a discussion of the trade-off between internal and external validity, see Schram (2005). Here, it suffices to say that our results can serve as a starting point for further experimentation –both in the laboratory and in the field– that will help us to further understand the reason why buyers have contract audits done as well as contract audits' consequences when they are indeed used.[43] One element that can be considered is a test of the robustness of our results to variations in the cost/benefit ratio of audits (i.e., ratio of audit costs to precision of the estimate).

[43] There are some obvious candidates for further research in the laboratory. These include testing with various costs and choices between various kinds of audit.

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Appendix A: Experimental Instructions

This appendix gives the English translation of the original Dutch instructions for the sessions with audit where the seller received information about the audit outcome. *[Italics in square brackets indicate places where alternative texts were used for the treatment without audit and the treatment where only buyers received the audit information]*. The instructions were programmed as html pages. Horizontal lines indicate page separations.

Welcome

(page 1 of 8)

You are about to participate in an experiment. The instructions are simple. If you follow them carefully, you may earn a lot of money. Your earnings will be paid to you in euros at the end of the experiment. This will be done confidentially, one participant at a time. In addition, you may keep the 7 euro you received when entering.

In the experiment all calculations are in **´experimental francs´**. At the end of the experiment francs will be converted to euros. The exchange rate used is **1 euro for 10 francs**.

These instructions cover 8 *[no audit treatment: 6]* pages like this one. When reading these instructions you may move forward and backward by clicking the mouse on the 'next page' or 'previous page' buttons at the bottom of each page. Sometimes a page will be larger than your screen. In that case you can use the scroll bar and your mouse to read through the entire page.

next page

Rounds and Groups

(page 2 of 8)

The experiment consists of **10 rounds**.

In each round you will participate in negotiations. These negotiations take place between **one buyer and one seller**. The seller offers one unit of an imaginary good for sale. A transaction will take place if the buyer and seller agree on the price. How this adds to your earnings will be explained below.

Only 2 people participate in the negotiations: the buyer and the seller. You will have the **same role** in all rounds: either buyer, or seller. This will be determined before the start of the first round. The other buyer or seller with whom you will negotiate is always a randomly chosen other participant with that role. **This other participant changes every round**. However, you don’t know who s/he is.

The composition of the negotiating-pairs is anonymous. You don’t know who you are dealing with. Others don’t know if you are dealing with them.

previous page next page

Buying and selling the good

(page 3 of 8)

If the buyer buys the good from the seller, she pays the **price they agreed upon**. How this price is established will be explained later.

To produce the good, the seller has to endure **costs**. These costs will be determined at the start of each round **for each seller separately** by letting the computer randomly select an integer number **between 1 and 75**. Each number has the same chance of being selected.

The good is valuable to the buyer because we give it a **value**. If the buyer buys the good we pay this value. This value will be determined by the computer at the start of each round **for each buyer separately**. It is a randomly chosen integer number **between 25 and 100**. Each number has the same chance of being selected.

The costs for the seller and the value to the buyer will be selected independently and will not be disclosed to the other party.

If the buyer and the seller reach an agreement a total surplus can be realised −depending on the costs and value− that can reach a maximum of 99. The agreed upon price determines the division of this surplus between the buyer and seller. Notice that if the costs are higher than the value, no price exists for which both the buyer and the seller can make a profit. In that case at least one party prefers not to reach an agreement.

If an agreement is reached, the earnings for the buyer are equal to the difference between the value and the agreed upon price. The earnings for the seller are equal to the difference between the agreed upon price and the costs.

Therefore:

Earnings of the seller = agreed upon price − costs.

Earnings of the buyer = value – agreed upon price.

previous page next page

Conducting an audit

[this page is not included in the treatment without audit]
(page 4 of 8)

Because the buyer has no information about the seller's costs, it is not possible to judge the fairness of the price asked by the seller.

Before negotiations start the buyer is offered the possibility to have an **audit** conducted to determine the fairness of the price asked. This audit is performed by the computer. Based on the true costs for the seller in the round concerned, the computer gives an estimate of these costs to the buyer. This estimate will also be disclosed to the seller before negotiations start. *[in the treatment with only buyer information about the audit this sentence is replaced by: This estimate will not be disclosed to the seller]* How this estimate is determined will now be explained.

The estimate of the audit **may deviate from the true costs**. The estimate can be too high or too low.

To be more precise, the buyer in this experiment can choose between two possibilities at the start of the negotiations;

(1) have no audit conducted. The buyer will get no information about the seller's costs;

(2) have an audit conducted. The buyer receives an estimate of the seller's costs. This estimate is an integer number randomly chosen in the range between the true costs minus 15 francs and the true costs plus 15 francs. Each number in this range has an equal chance of being chosen.

After the negotiations have started no more audits can be conducted.

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Costs of an audit

[this page is not included in the treatment without audit]
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There are costs related to having an audit conducted. More precisely:

If the choice is **to have no audit conducted no costs** will be charged.

If the choice is **to have an audit conducted 5 francs** will be charged on the buyer.

Because the costs of the audit will be subtracted from the earnings of the buyer, it holds for the buyer that:

Earnings of the buyer = value – agreed upon price − costs of the audit.

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Negotiations
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Negotiations take place by alternating bids. The seller has at most 3 chances to ask a price. The buyer has at most 2 possibilities to make a bid.

More precisely, the negotiations proceed as follows:

After the disclosure of the costs to the seller and the value to the buyer and after a possible audit has been performed, **the seller opens negotiations** by announcing an ask price to the buyer. This ask price **has to be larger than or equal to the costs** of the seller.

The buyer then has the opportunity to either accept this ask price or make a counter-offer. A counter-offer (offered price) has **to be lower than or equal to the value** of the buyer and the seller's ask price.

As long as the other's price has not been accepted, the buyer and the seller can make counter-offers. On all occasions the seller's ask price must be higher than or equal to the costs and higher than or

equal to the previous price offered by the buyer.
Every price offered by the buyer must be lower than or equal to the value and lower than or equal to the previous price asked by the seller.

If both parties have **twice** made an unaccepted bid, the seller can make a **final bid**. **The buyer can only accept or reject** this final ask price. In case the buyer rejects it there are no earnings for either of them. The buyer does, however, have to pay the costs of the audit if there was one. *[this last sentence is dropped in the treatment without audit]*

If the buyer or seller accepts the counter-party's price during the negotiations, this is the **agreed upon price**.

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Information

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During the negotiations both buyer and seller can see all information about these negotiations on screen.

In this way, the **seller sees her or his costs** on screen from the start of each round and the **buyer sees her or his value** on screen from the start of each round. Both the seller and the buyer see the **result of a chosen audit**. *[this last sentence is dropped in the treatment without audit; in the treatment with only buyer information about the audit this sentence is replaced by: The buyer also sees the result of a chosen audit]* In addition, information about the stage in which the **negotiations** are and all **prices offered and asked** will be shown on screen.

The buyer and seller fill out the **input** of a new bid in a designated place, after which the **'confirm'** button has to be clicked.

After the buyer and seller have finished the negotiations they have to **wait** until all participants are ready, after which the next round will start.

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Finally

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This brings you to the end of these instructions. When everybody has finished, we will start the first round of the experiment.

When the first round starts, you can read at the top left part of your screen whether you are a buyer or seller in today's experiment.

If you have finished reading these instructions, you can indicate this by clicking the 'ready' button (at the bottom of this screen). Afterwards please wait patiently until everyone has finished. This may take a little while, so we kindly ask for your patience.