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by Lauri Sääksvuori and Abhijit Ramalingam

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Bargaining under Surveillance:
Evidence from a Three-Person Ultimatum Game

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Keywords: Bargaining, Communication, Distributional preferences, Experiment, Negotiations, Surveillance

JEL-Code: C72, C92, D01, D03, D83

I. Introduction

We regularly observe political, legal and private disputes over individual rights to monitor decision-making processes. Leakages from closed-door negotiations are known to be capable of changing the course of political and economic trajectories. The emergence of modern information technologies, particularly the Internet and social media, is changing the traditional landscape of governance, management and negotiations by fuelling public demand for more transparent decision-making. Lately, entire political movements have been launched aiming to increase the transparency of political and economic decision-making by means of increasing electronic governance and real-time broadcasts that document political decision-making processes on various scales.[1]

This paper examines how the knowledge of being under surveillance affects preferences over distributional outcomes. In addition, we examine what motivates people to invest economic resources to monitor decision-making processes. Laboratory experiments provide a controlled environment where these questions can be studied. In particular, our experiment renders it possible to examine whether people have an intrinsic motivation to observe decision-making processes that determine distributional outcomes.

Scientific interest in the influence of surveillance on human behaviour is not new. The well-known illumination experiments with a modest aim to measure the impact of factory lighting on worker productivity at the Hawthorne Works between 1924 and 1932 turned out to be a landmark event in the development of social sciences (Mayo, 1933). The later re-analysis of these experiments let Henry A. Landsberger (1958) to coin the term "Hawthorne Effect" to describe a phenomenon that occurs when individuals alter their behavior due to the feeling that they are under surveillance or are given indirect attention by measuring their performance.[2] The experiments at the Hawthorne Works not only laid a foundation for studies that focus on the

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[1] There is a long history of demanding unimpeded public access to government documents. Probably the first laws granting all citizens unrestricted access to government documents that were not deemed particularly confidential were adopted in Sweden in 1766. A well-known recent example of a political movement that mentions greater transparency of political and economic decision-making processes by electronic means as its primary political goal is the Pirate Parties International.

[2] There is persistent interest in the original Hawthorne data leading to several attempts to re-analyse and reinterpret the original data. Levitt and List (2011) suggest in a recent contribution that the original interpretation of the data is likely to be fictional due to deficiencies in the experimental design.

effects of scrutiny on human behavior but also provide important guidance on how to develop new experimental practices to avoid the fate of the original Hawthorne Experiments.

An important contribution of the original Hawthorne experiments and the follow-up studies has been the insight that mere scrutiny may alter subjects' behavior in behavioral experiments. This knowledge has sensitized researchers to develop research methodologies that enable them to control for confounds due to experimental demand effects. A related issue often raised in the literature pertains to the external validity of laboratory experiments conducted under the surveillance of the experimenter. Hence, Levitt and List (2007) mention the scrutiny associated with participation in economic experiments as one of the greatest threats to the external validity of economic experiments. This paper contributes to this discussion by showing that the surveillance of *decision-making processes* during a laboratory experiment is unlikely to influence distributional outcomes.

This paper connects to a voluminous literature studying the importance of social influence on economic behavior. Social identity and social distance between the agents are known to influence preferences for redistribution both in experimental investigations (Hoffman et al., 1996; Klor and Shayo, 2010) and naturally-occurring situations (Alesina and Giuliano, 2009). In particular, experimental evidence indicates that reduced social distance by different means such as knowing the person's name (Charness and Gneezy, 2008) and seeing the person's picture (Andreoni and Petrie, 2004; Eckel and Petrie, 2011) increase the salience of other-regarding concerns in economic decision-making. Likewise, humans are known to behave more pro-socially when they think that another person is monitoring their donations in the dictator game (Cason and Mui, 1997). In addition to the literature documenting the effects of social distance on economic behavior between two directly involved parties, there is substantial evidence showing that third-parties whose economic outcome is not directly affected by a norm violation are willing to alter distributional outcomes at their own cost (Fehr and Fischbacher, 2004). Furthermore, it appears that the strength of third-party punishment is proportional to the size of the pay-off inequality between the individuals and can largely be explained by other-regarding motivations (Leibbrandt and Lopez-Perez, 2012).

Recent research has established that the relationship between distributional outcomes and social distance is sensitive to low-level cues. Pro-social behavior towards anonymous recipients and contributions to public goods are known to increase under watching eyes (Bateson et al., 2006). In the same vein, Haley and Fessler (2005), Burnham and Hare (2007) and Rigdon et al. (2009) report experimental results indicating that the presence of subtle eye-like stimuli guides choice behavior towards more generous allocations.[3] Drawing on the extensive evidence suggesting the importance of social distance on human behavior, our study examines the role of third-party monitoring as a means to reduce social distance between the decision-makers and an inactive third-party.

Communication between agents is known to play a pivotal role in economic bargaining and strategic interaction. Thus, one of the most conspicuous results in the experimental literature is that pre-play communication between the actors enhances cooperativeness and equalizes distributional outcomes (Isaac and Walker, 1988; Ostrom et al., 1994; Ledyard, 1995). This has in turn led to important refinements of economic theory showing that the dissemination of information about players' preferences and intentions increases the likelihood of establishing stable agreements among independent decision-makers (Rabin, 1994; Farrell, 1995; Crawford, 1998). However, very little is known about the influence of third-party surveillance on the information content of pre-play communication. It can be hypothesized that third-party surveillance changes individuals' willingness to reveal their preferences or intentions, diminishing the effectiveness of communication to facilitate economic coordination and cooperation.

We use a three-person ultimatum game as a workhorse to examine the effect of third-party surveillance on bargaining behavior and distributional preferences. The game provides an ideal environment to study surveillance as it consists of a very simple bargaining scenario with an inactive third-party. The innovation of this paper is to endow the third-party with a right to observe other participants' pre-play communication. In addition, we implement three treatments

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[3] The study by Haley and Fessler (2005) has led to several follow-up studies examining the robustness of the watching eyes effect. For example, Raihani and Bshary (2012) report no watching eyes effect in the Dictator Game. Fehr and Schneider (2010) report no effect in the Trust Game. Nettle et al. (2012) conduct a meta-analysis of the studies of the watching eyes effect in Dictator Game experiments published to date and conclude that the effect is robust. However, they suggest that instead of making people directionally more generous, watching eyes primarily reduce variation in social behavior.

which offer opportunities to purchase surveillance rights. Our design renders it possible to estimate the demand for a right to observe decision-making processes in the absence of participation rights in the actual decision-making process. Furthermore, treatments where information about surveillance by third-parties' is either conveyed to the negotiating parties or not enable us to distinguish strategically motivated purchasing decisions from an intrinsic motivation to acquire a right to observe the negotiations.

The three-person ultimatum game is often cited as an example of the striking power of other-regarding preference models to organize empirically observed data. Our results point to the limitations of other-regarding preference models by showing that electronic monitoring of decision-making processes by third-parties does not affect distributional outcomes in a three-person ultimatum game. At the same time, we find that a large majority of individuals is willing to pay for a right to monitor decision-making processes over distribution outcomes despite pecuniary incentives to the contrary. We observe that people are notably overconfident in believing that an opportunity to scrutinize decision-making processes will shift distributional outcomes for their own benefit. As a result, offering third-parties an opportunity to buy a right to monitor decision-making processes in a three-player ultimatum game increases disadvantageous pay-off inequality between the third-parties and other individuals. Economic theory may need to account for these behavioral characteristics to explain the rationale behind the persistent demands for more transparent economic and political decision-making.

The remainder of the paper is organized as follows. Section II describes the experimental design and our behavioral propositions. Section III presents the results. Section IV contains concluding remarks.

II. Experimental Design and Behavioral Propositions

2.1 Experimental Design

In this study, individuals play a one-shot three-player ultimatum bargaining game (Güth and van Damme, 1998). The proposer is endowed with 29 Experimental Currency Units (ECUs) which

need to be divided between the proposer, responder and inactive third-party.[4] If the proposer's offer is accepted by the responder, the proposed offer is implemented and players' earnings are according to the proposal. If the proposer's offer is rejected by the responder, all players have zero earnings. The rules of the game specify that the proposals are to be made in integers. Assignment to different roles is random and performed at the beginning of the experiment.

Before the proposal is made, the responder and proposer are given an opportunity to communicate with each other. The communication takes place by exchanging written messages in a computerized chat for a maximum time of 180 seconds. This open-ended communication between the proposer and responder allows negotiating a mutually agreeable outcome that may diminish the likelihood of rejected proposals. However, at the same time, open-ended communication enables players to develop negotiating strategies and pose threats aiming to maximize individual earnings. The use of a controlled laboratory experiment renders it possible to preserve full anonymity among the participants and isolate the effect of mere textual exchange of messages from visual or verbal cues such as vocal intonation, facial expressions and body language. We do not limit the content of the communication between the participants by any means except for restrictions against revealing one's true identity, usage of insulting language and threats or offers of side-payments that take place outside of the experimental laboratory. Even though concentration on written communication may be seen as a limitation of the current study, it is worth mentioning that several important applications areas of this study comprise of situations where the bargaining process is documented in written form. Some illustrative examples of communication between participants are shown in the Electronic Supplementary Material. These examples also show the type of information that third-parties can observe if they have a right to monitor the discussion.

This study includes data from experimental sessions conducted in Germany (Max Planck Institute of Economics, Jena) and the United Kingdom (University of East Anglia, Norwich). There are five different treatments in this study. Common to every treatment is the three-player ultimatum bargaining game and the communication stage prior to the game. In treatment *Baseline*, the inactive third-party does not observe the course of negotiations between the proposer and

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[4]The endowment of 29 ECUs removes the option of equal two-way split between the proposer and responder and the option of equal three-way split between all players. We chose this endowment size to avoid the implementation of regularly observed equal splits to achieve greater variance in behavioral outcomes and the contents of the communication.

responder. In treatment *Observation*, the inactive third-party is able to observe the communication between the proposer and responder. In *Observation*, the proposer and responder know that the bargaining takes place under third-party surveillance. In treatment *Purchase*, the inactive third-party is given an option to purchase a right to observe the course of negotiations. The proposer and responder are not informed about the actual third-party presence during the negotiations. In treatment *Purchase-other*, the inactive third-party is given an option to purchase a right to observe the course of negotiations between some other proposer and responder than the proposer and responder who determine his/her payoff. Again, the proposer and responder are not informed about the third-party presence during the negotiations. In treatment *Signal*, the inactive third-party is given an option to purchase a right to observe the course of negotiations. In this treatment, the proposer and responder are informed before beginning the communication phase whether the third-party is monitoring the course of negotiations. Table 1 shows the five treatments.

Table 1. Experimental Treatments.

Treatment	Third-party observes negotiation	Third-party may buy a right to monitor the negotiation	Location	Number of groups
Baseline	No	No	G, UK	16, 15
Observation	Yes	No	G	25
Purchase	Endogenous	Yes	G, UK	17, 14
Purchase-other	Endogenous	Yes	UK	17
Signal	Endogenous	Yes	G	24

Note: G = Germany, UK = United Kingdom

In treatments *Purchase*, *Purchase-other* and *Signal*, where the third-party has an option to purchase a right to monitor the negotiations, we elicit subjects' willingness to pay for a right to monitor by applying a variant of the random price mechanism (Becker et al., 1964). Third-parties must decide for an array of prices if they will take the offered money and forego a right to observe the negotiations, or forego the money and gain a right to observe. After the third-parties have made their decision for each price, the computer draws a random price. If this random price is smaller than or equal to individual's stated willingness to pay, the individual foregoes the money and gains a right to observe. If the random price is larger than the stated willingness to

pay, the individual receives money according to the random draw. The prices are 0 ECUs, 0.20 ECUs, 0.50 ECUs, 1 ECU, 2 ECUs, 3 ECUs, 4 ECUs and 5 ECUs.[5]

In addition to the option to purchase a right to monitor the course of negotiations, individuals acting as third-parties are requested to report their expectations about the bargaining outcome in all treatments. This elicitation of individuals' expectations renders it possible to assess whether greater transparency through a right to monitor the negotiation process affects people's expectations about distributional outcomes. In treatments conducted in the UK (treatments *Purchase* and *Purchase-other)*, we also elicit the beliefs of the proposer and responder about the third-parties' willingness to pay for a right to monitor the negotiations.[6] This practice renders it possible to examine whether individuals' expectations about the presence of a third-party are associated with distributional outcomes. Moreover, eliciting individuals' expectations about a third-party presence in treatments *Purchase* and *Purchase-other* enables us to study whether individuals expect that third-parties' willingness to pay for a right to monitor the negotiations differs between these treatments.

We reward accurate expectations by paying a bonus based on deviations from the observed distributional outcome and expressed maximum willingness to pay. The maximum reward in the case an individual manages to forecast the exact distributional outcome is 5 ECUs. The maximum reward in the case an individual manages to forecast the exact maximum willingness to pay is 2 ECUs. For experimental simplicity, the reward decreases in a linear fashion as the deviation increases.[7]

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[5] Prior research has shown the susceptibility of the random price mechanism to several potential problems that may bias the estimation of revealed valuations. These problems include at least the construction of the price list that may create an anchor for the stated willingness to pay and many practical issues related to explaining the logic of the mechanism to experimental subjects. Here we are not interested to estimate subjects' valuations for the right to observe as such, but draw qualitative results about subjects' purchasing behavior in different treatments. We use the same price list in treatments conducted in Germany and in the UK. All prices are in Experimental Currency Units (ECUs). ECUs are converted to euro and pounds at an exchange rate of 1:1.

[6] The fact that we elicit the beliefs of the proposer and responder about the third-parties' willingness to pay for a right to monitor the negotiations in treatment *Purchase* in experimental sessions conducted in the UK, but not in experimental sessions conducted in Germany does not allow direct comparisons of the behavior of individuals in the UK and Germany in treatment *Purchase*. However, the fact that we do not observe any differences between the individuals in the UK and in Germany in any treatment substantiates our interpretation that there are no cultural differences between our British and German subjects in this experiment.

[7] The family of quadratic scoring rule offers incentive compatible alternatives for the linear rule. However, the practical implementation of quadratic scoring rules in complex decision environments is a serious challenge (see e.g. Read, 2005).

2.2 Behavioural Propositions

Assuming selfish preferences, the predictions of standard game theory for our experiment are straightforward.[8] However, years of experimental research supported by behavioral theories have shown that people have different motivations and exhibit a variety of behavioral biases. Our approach to generate behavioral propositions assumes that having a right to observe the negotiation process over distributional outcomes may affect the social distance between agents and influence preferences for redistribution. Moreover, we assume that people are susceptible to image concerns and may desire to maintain positive self-image and social reputation.

There is a voluminous theoretical and empirical literature documenting the existence of in-group favoritism (Tajfel and Turner, 1979). This in-group favoritism is often expressed in the allocation of resources (Turner et al., 1979). Assuming that a right to monitor the decision-making process influences participants' perceived membership in a group and strengthens the bond linking the third-party to the proposer and responder, we would expect that third-party presence during the negotiations increases allocations to the third-party.

Theories suggesting that people may desire to maintain a comfortable self-image and positive social reputation offer a complementary alternative to predict the impact of third-party presence in our experiment. If people attach non-negligible value to their self-image (Kahneman and Knetsch, 1992; Akerlof and Kranton, 2000; Konow, 2000; Bénabou and Tirole, 2006; Dana et al., 2007; Krupka and Weber, 2009) or act based on a wish to be perceived as pro-social persons (Ireland 1994; Bernheim 1994; Glazer and Konrad, 1996; Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009; Ariely et al. 2009; Batigalli and Dufwenberg, 2009), we would expect that these concerns may influence participants' behavior in our experiment. In particular, if the knowledge of being monitored by a third-party affects the image concerns of the proposer and responder, we would expect that they allocate more resources to a third-party in treatments where a third-party may monitor the negotiation process. Thus, we summarize the expected impact of a right to monitor the negotiations between the proposer and responder on resource allocations as follows:

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[8] We acknowledge that game theory itself makes no assumptions about the nature of individuals' preferences. Assuming different preferences may lead to different equilibria and predict differences in behavior between our experimental treatments.

Proposition 1: *A right to observe the negotiations increases proposed allocations to a third-party.*

While Proposition 1 focuses on the aggregate effect of third-party presence on proposed allocations, it does not pay direct attention to the behavioral mechanisms that may explain greater allocations to third-parties. However, treatments *Purchase* and *Purchase-other* enable us to more carefully investigate the behavioral factors that may guide the allocation decision towards a third-party. In treatment *Purchase (UK)*, we elicit the beliefs of the proposer and responder about third-parties' willingness to pay for a right to monitor the negotiations. This allows us to investigate the relationship between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations.

If the maintenance of a comfortable self-image is an important concern and determinant of pro-social behavior in our experiment, we would expect that the beliefs about the presence of a third-party are not associated with the proposed allocations. However, if a wish to be perceived as a pro-social person in the eyes of others is an important concern and a determinant of pro-social behavior in our experiment, we would expect that beliefs over the presence of a third-party are associated with the proposed allocations. In this case, the mere threat of being observed by a third-party could potentially influence the allocation of resources.

At the same time, the desire to be perceived as a pro-social person in the eyes of others may not be independent of the audience. Thus, by means of treatment *Purchase-other*, we may test whether the potential association between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations is conditional on the identity of the third-party. In particular, if the wish to be perceived as a pro-social person in the eyes of those people whose pay-off depends on the outcome of the bargaining process is stronger than the wish to be perceived as a pro-social person among a more general audience, we would expect that the association between the beliefs and proposed resource allocation is stronger in treatment *Purchase* than in treatment *Purchase-other*. We summarize these considerations as follows:

Proposition 2A: *There is a positive association between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations.*

Proposition 2B: *The association between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations is stronger in treatment Purchase than in treatment Purchase-other.*

As noted earlier, people often have various behavioral motivations beyond their economic self-interest. The experiment conducted in this study enables us to explore whether individuals are willing to invest economic resources to monitor negotiations where two other agents bargain over distributional outcomes. While there are multiple potential explanations for a willingness to invest economic resources to monitor negotiations over distributional outcomes, we are first interested to explore whether individuals have a willingness to pay for a right to monitor negotiation processes at large.[9]

Proposition 3: *There is a positive willingness to pay for a right to observe the negotiations.*

While Proposition 3 does not differentiate between alternative explanations for a positive willingness to invest economic resources to monitor negotiations over distributional outcomes, treatments *Purchase-other* and *Signal* enable us to explore different motivations that may guide participants' decision to purchase a right to monitor the negotiations. In particular, we have designed the treatment *Purchase* to investigate whether participants attach particular value for a right to observe negotiations which determine their own economic outcome. At the same time, the treatment *Purchase-other* enables us to examine whether our participants are willing to pay for a right to observe any negotiation process between the proposer and responder. By comparing participants' willingness to pay in treatments *Purchase* and *Purchase-other*, we examine whether participants are willing to pay for a chance to actively reduce the uncertainty concerning their

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[9] There are numerous potential explanations why a person could be willing to pay for a right to monitor negotiations over distributional outcomes. Potential explanations include, for example, a preference for curiosity, uncertainty avoidance and a desire to reduce perceived social distance between the agents. An alternative explanation for participants' willingness to pay for a right to observe is to understand the monitoring of instant messaging as a consumption good that enables individuals to derive direct satisfaction from observing the negotiation process. As such, we also cannot completely exclude the possibility that participants are willing to pay for a right to monitor the negotiations due to cues about what constitutes appropriate behaviour in our experiment (experimenter demand effect). However, we find that the vast majority of participants report a monotonically decreasing willingness to observe as the price increases. This observation leads us to suggest that participants at least carefully consider their decisions when indicating their highest willingness to pay and that their decisions are influenced also by factors other than cues about what constitutes appropriate behaviour in our experiment.

own monetary outcome.[10] If participants attach particular value to a chance to reduce uncertainty concerning their own monetary outcome, we would expect that there is greater willingness to pay for a right to observe negotiations in treatment *Purchase* than in treatment *Purchase-other*.

Proposition 4: *There is greater willingness to pay for a right to observe negotiations when the third-party observes the negotiation process between the proposer and responder who decide about the third-party's outcome than when the third-party observes the negotiation process between any other proposer and responder.*

In treatments *Purchase* and *Purchase-other*, the proposer and responder are aware that a third-party may monitor their negotiations, but they cannot verify the potential presence of a third-party. However, in treatment *Signal* the proposer and responder are informed whether a third-party is monitoring their negotiations. Consequently, third-parties may deliberately decide to signal their presence during the negotiations. In this case, a greater willingness to pay for a right to observe the negotiations can be justified by pecuniary incentives if third-parties believe that their presence shifts distributional outcomes for their benefit. There are multiple behavioral mechanisms that may lead third-parties to believe that their presence shifts distributional outcomes for their benefit. Particularly noteworthy ones may include the reduction of social distance and increasing the need to be perceived as pro-social persons among the proposers and responders. However, our experiment is not designed to investigate the potential factors that may lead third-parties to believe that their presence shifts distributional outcomes for their benefit. By contrast, we investigate whether a chance of signalling presence during negotiations increases the willingness to pay for a right to monitor negotiations over distributional outcomes.

Proposition 5: *There is greater willingness to pay for a right to observe negotiations when the third-parties can signal their presence during the negotiations.*

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[10] The theory of uncertainty avoidance (Hofstede, 1980) posits that people actively try to reduce uncertainty by controlling their future environment. However, the theory of uncertainty avoidance mainly focuses on cross-cultural comparisons on societies' tolerance for uncertainty and ambiguity. The theory does not make sharp predictions whether people actively try to reduce uncertainty by buying a right to observe decision-making processes which determine their own economic outcome.

2.3 Experimental Procedure

A total of 384 subjects (219 women, 165 men) participated in 19 different sessions (10 in Germany and 9 in the UK) in the experiment. The number of participants per session varied from 12 to 27 participants. The experiment was conducted at the laboratories of the Max Planck Institute of Economics in Germany and of the University of East Anglia in the UK, programmed and run using z-Tree (Fischbacher, 2007). Participants were mainly undergraduate students (Mean age: 22.5 years, Min: 18, Max: 55, Standard deviation: 3.9) from a wide range of academic disciplines. Upon arriving at the laboratory, participants were randomly assigned to their cubicles preventing communication and visual interaction. They were given detailed instructions and a number of quiz questions on paper. After the subjects had familiarized themselves with the rules of the game by reading the instructions, the experimenter ensured that the rules of the game were common knowledge by reading aloud a detailed summary of the instructions. Subjects then took a post instruction quiz and were not allowed to continue until all answers were correct. Participants were paid after the experiment privately in cash according to their earnings from the experiment. On average, the experiment lasted for 30 minutes. Earnings in ECUs were converted to euro or pounds at an exchange rate of 1:1. Average earnings in Germany were 12.83€ including a show-up fee of 2.50€. Average earnings in the UK were £12.17 including a show-up fee of £2.

III. Results

The main scientific objective of this paper is to identify characteristic patterns of economic decision-making under third-party surveillance. As noted in Section 2, this study includes data from experiments conducted in Germany and in the United Kingdom. Existing empirical evidence suggests that there may be systematic differences in the behavior of subjects across different societies (Roth et al., 1991; Henrich et al., 2001; Herrmann et. al, 2008). Hence, we first test for differences in behavior between the two geographic locations. We do not find significant differences between the locations.[11] As a result, we combine the data collected in these two locations for treatments *Baseline* and *Purchase*. Furthermore, the fact that we do not find

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[11] The only significant difference we find between the two locations is in the optimism of third-parties. Third-parties in the UK expect to receive significantly higher earnings than third-parties in Germany. A detailed analysis of the location effects is made available in the Electronic Supplementary Material.

significant differences between the two locations enables us to compare the treatment *Purchase-other* with other treatments regardless of where the treatments where conducted.

3.1 The impact of third-party surveillance on proposed resource allocation

We first investigate the impact of third-party surveillance on proposed resource allocations. Table 2 presents average allocations proposed by the proposers to the responders and third-parties in each treatment.[12] Furthermore, in treatment *Signal*, we distinguish between the situations where a third-party has successfully acquired a right to observe the negotiations and where a third-party has missed an option to purchase a right to observe the negotiations. Notably, the acceptance rate of proposed allocations was 100 percent in all treatments conducted in Germany. In the UK, one responder did not accept the proposal in treatment *Baseline*. Likewise, one responder did not accept the proposal in treatment *Purchase* in the UK. These observations suggest that the proposer and the responder regularly come to an agreement during the negotiation process and implement an allocation that is almost always accepted by the responder.

Table 2. Proposed average and median allocations to the receiver and third-party by treatment.

Treatment		Allocation to Responder (ECUs)			Allocation to third-party (ECUs)		
	N	Mean	Median	Std.	Mean	Median	Std.
Baseline	31	11.65	12.00	1.70	4.94	5.00	3.52
Observation	25	11.36	11.00	1.68	4.48	5.00	2.86
Purchase	31	11.45	12.00	2.14	4.77	5.00	3.28
Purchase-other	17	11.35	11.00	1.46	5.06	5.00	3.09
Signal – Obs.	8	11.75	12.00	1.49	4.50	5.00	2.26
Signal – No obs.	16	11.75	11.50	1.88	3.19	3.00	2.66

Note: Each participant acting as a proposer generates one observation. N = Number of observations. Std. = Standard deviation. Obs = Third-party observes the course of negotiations, No Obs. = Third-party does not observe the course of negotiations.

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[12] Table 2 presents average allocations proposed by the proposer taking into account the effect of rejected proposals. Results presented in Tables 2 and 3 are robust to the exclusion of rejected proposals.

We find that third-party surveillance does not affect proposed resource allocations. A Kruskal-Wallis test shows that, overall, there are no significant differences in the proposed allocations to third-parties across treatments (p = 0.5367).[13] Table 3 shows the p-values for Mann-Whitney tests that compare allocations to the third-parties across all pairs of treatments. In particular, when comparing proposed resource allocations between the treatments where there is no possibility to purchase a right to observe the negotiations (treatments *Baseline* and *Observation*), we find that exogenously imposed third-party surveillance does not affect proposed resource allocation (Mann-Whitney test: p = 0.635, two-sided). Likewise, there is no significant difference in proposed resource allocations between situations where the third-parties have voluntarily foregone an opportunity to monitor the negotiations (*Signal – No observation*) and where there is no option to observe (*Baseline*) (Mann-Whitney test: p = 0.102, two-sided). Overall, Table 3 shows that there are no significant differences in offers made to the third-parties between any two treatments.[14]

Table 3. Allocations to the third-parties: Pairwise comparisons of treatments, p-values of Mann-Whitney tests

	Observation	*Purchase*	*Signal-Obs*	*Signal – No Obs*	*Purchase-other*
Baseline	0.635	0.779	0.633	0.102	0.991
Observation	-	0.783	1.000	0.153	0.579
Purchase	-	-	0.696	0.117	0.869
Signal - Obs	-	-	-	0.204	0.679
Signal-No Obs	-	-	-	-	0.064*

Note: Obs = Third-party observes the course of negotiations, No Obs. = Third-party does not observe the course of negotiations. * - sig. at 10%.

Result 1: *Allocations to third-parties are not affected by third-party surveillance*.

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[13] There are also significant differences in allocations to the proposer (p = 0.2530) and to the responder (p = 0.9117).
[14] The only significant difference (at the 10% level) is between *Purchase-other* and *Signal – No Obs*. In this case, a third-party receives a lower amount when the proposer and responder know that a third-party is not observing in contrast to a situation where a third-party can at best observe the discussion in some other group. If we combine the observations *Signal – Obs*. and *Signal – No Obs*. we get 24 observations for the treatment *Signal*. In this case, none of the pairwise differences is statistically significant.

Result 1 suggests that third-party surveillance does not affect proposed resource allocations. The fact that the proposed allocations to the third-parties are not greater in treatments *Observation* and *Signal* than in treatment *Baseline* is a strong signal of the perceived irrelevance of a third-party presence in the decision-making process of the proposer and responder. However, in treatments *Purchase* and *Purchase-other*, the proposer and responder do not know whether a third-party is present. In the face of this uncertainty, it is expected that the proposer and responder form beliefs about third-party presence. Despite the fact that there are no differences in proposed allocations between the treatments, it is possible that offers to third-parties are sensitive to such beliefs about the presence of a third-party.

To explore this possibility, we elicited the beliefs of the proposer and responder about third-parties' maximum willingness to pay to for a right to observe the communication in treatments *Purchase* and *Purchase-other*.[15] Table 4 presents the average estimates of the proposer and responder over the third-parties' willingness to pay (WTP) for a right to observe the communication in these two treatments. For reference, we present the third-parties' actual average willingness to pay in these treatments. Additionally, Table 4 also presents Mann-Whitney tests (z-stats and p-values) comparing the estimates of the proposer and responder with the third-parties' actual WTP by treatment.

Table 4. Mean estimates of third-parties maximum willingness to pay (ECUs).

Treatment	Third-party WTP	Estimate by		M-W tests	
		Proposer	Responder	Proposer	Responder
Purchase	0.87	1.46	1.68	1.560	2.053**
	(0.80)	(1.12)	(0.77)	[0.1187]	[0.0400]
Purchase-other	0.78	1.49	1.57	2.688***	2.459**
	(0.85)	(1.08)	(0.95)	[0.0072]	[0.0139]

Note: Number of observations in treatment *Purchase* = 14 and in treatment *Purchase-other* = 17. Standard deviations in parenthesis. Figures in brackets are p-values. *** - sig. at 1%, ** - sig. at 5%.

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[15] As described in Section 2, we elicit the beliefs of the proposer and responder about third-parties' maximum willingness to pay in treatment *Purchase* only in experimental sessions conducted in the United Kingdom.

Table 4 shows that the proposer and responder overestimate third-parties' willingness to pay in both treatments. Mann-Whitney test statistics show that the responders significantly overestimate third-parties' willingness to pay in treatment *Purchase*, while both the proposer and the responder significantly overestimate third-parties' willingness to pay in treatment *Purchase-other*. However, we find that the beliefs of the proposer and responder about the third-party presence are not associated with the proposed allocations. The correlation between the proposers' estimates of the third-parties' maximum willingness to pay and the proposed allocations to third-parties is -0.1220 (p = 0.6777) in treatment *Purchase* and -0.354 (p = 0.163) in treatment *Purchase-other*. The correlation between the responders' estimates of the third-parties' maximum willingness to pay and the proposed allocation to the third-parties is 0.057 (p = 0.845) in treatment *Purchase* and -0.209 (p = 0.423) in treatment *Purchase-other*. We take these observations as evidence that the beliefs of the proposer and responder about the third-party presence are not associated with the proposed resource allocations.

Result 2. *The proposer and responder overestimate third-parties' willingness to pay for a right to observe the negotiations. However, the beliefs of the proposer and responder about third-party presence are not associated with the proposed resource allocations.*

It is theoretically conceivable that the impact of third-party surveillance is not limited to proposed resource allocations, but affects the very process of negotiating an agreeable outcome. Consequently, in addition to pure monetary outcomes, we measure the time the proposer and responder spend on discussing their strategies during the negotiations stage. In our experiment, the proposer and responder were instructed to proceed to a subsequent decision stage immediately after they have finished the negotiations, rendering it possible to measure the effective length of every negotiations process. By comparing the effective negotiation time between treatments, we do not find evidence that third-party surveillance affects the effective length of bargaining between the proposer and responder.[16] The average length of bargaining time in treatment Baseline is 128.16 seconds (Std. = 61.13) and in treatment Observation 123.72

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[16] The fact that the proposer and responder were requested to proceed to a subsequent decision stage right after the negotiations does obviously not oblige participants to immediately proceed to the next stage after ending the active bargaining phase. Thus, the voluntary decision to proceed before the maximum negotiation length of 180 seconds creates an imperfect measure for the length of effective negotiation time. To circumvent the problem that bargainers merely wait until the maximum length has been reached, we measure the negotiation time until the first person quits the negotiation. The average length of the negotiations is reported by treatment in the Electronic Supplementary Material.

seconds (Std. = 57.77). The average length of bargaining time by each treatment is presented in the Electronic Supplementary Material.

Result 3. *Third-party surveillance does not affect the length of the negotiation period between the proposer and responder.*

3.2 Demand for a right to observe the negotiation process

In the following, we investigate the demand for a right to observe the negotiation process. Figure 1 shows the percentage of individuals in treatments *Purchase*, *Purchase-other* and *Signal* who are willing to purchase a right to observe the course of negotiations between the proposer and responder at each price.[17] We find that a substantial fraction of third-parties are willing to pay for a right to observe the communication at a positive price. For example, when the right to observe can be acquired at the price of 0.50 ECUs, 68 percent of the individuals in treatment *Purchase* are willing to purchase an access to the negotiation records. Overall, the average willingness to pay is 0.93 ECUs in treatment *Signal*, 0.83 ECUs in treatment *Purchase* and 0.78 ECUs in treatment *Purchase-other*. Wilcoxon sign Signed-rank tests confirm that the average willingness to pay is significantly greater than zero ($p < 0.001$) in all these treatments. At the same time, Figure 1 shows that the demand for a right to observe is downward-sloping. The percentage of individuals willing to pay for the information decreases as the price increases. Figure 1 documents that the demand becomes weak when the price is 2 ECUs or above. Finally, in all three treatments, there is not a single individual who is willing to pay the maximum price of 5 ECUs for a right to observe the negotiations.

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[17] We presented to the participants a price list as described in Section 2.1 and asked them to indicate if they would be willing to observe the negotiations at *each* price. We find that there were 20 participants (28 percent) who submitted 'incomplete' price lists. While these participants indicated the highest price they would be willing to pay, they did not indicate that they would be willing to pay any or some of the lower prices. Here, we adopt a convention that the highest indicated price is an individual's maximum willingness to pay and include these individuals in our analysis. We also analyze the willingness to pay after excluding individuals with incomplete price lists. These results are reported in the Electronic Supplementary Material. Excluding individuals with incomplete price lists does not change our findings.
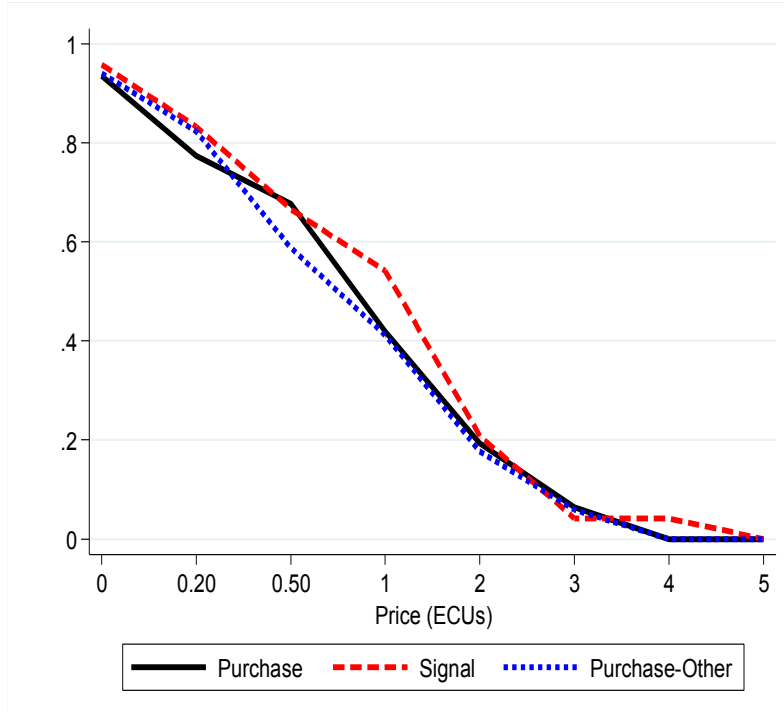
Figure 1. Third-parties' willingness to pay to observe the communication

In Table 5, we report random-effects logistic regression coefficients for a probability to purchase a right to observe. As expected, these models show that the probability to purchase is negatively related to price. At the same time, the reported models enable us to investigate the impact of participants' beliefs about their own outcome on their willingness to pay for a right to observe. In Figure 1, we observe that a certain fraction of individuals is willing to forego a right to observe the negotiations even at a zero price. In addition, about 20 percent of the individuals are willing to pay no more than 0.20 ECUs for a right to observe the negotiations. Are these individuals who decide to forego a chance to monitor the negotiations at low prices systematically different from the individuals who are willing to pay a higher price for a right to observe? One potential explanation is that individuals who expect to receive only a small fraction of the available resources are more willing to remain ignorant about the course of negotiations to avoid observing how the responder and receiver ignore fairness considerations towards these individuals. The regression coefficients in Table 5, however, do not lend support to this conjecture. We find that expectations about the negotiators' benevolence towards the third-party do not impact third-parties' willingness to purchase information about the course of negotiations in any of the three

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treatments. Moreover, we do not find any impact of demographic factors on third-parties'
willingness to pay. All reported results are robust to the inclusion of location and session specific
fixed effects.

Table 5. Factors affecting the probability of purchasing a right to observe –
Random Effects Logit

Probability of Purchasing a Right to Observe – Random Effect Logit			
Variables	(1)	(2)	(3)
Price	-2.208***	-1.889***	-1.863***
	(.374)	(.126)	(.442)
Belief		0.126	0.135
		(.085)	(.091)
Price*Belief		-0.043	-0.047
		(.060)	(.054)
Constant	1.860**	1.125	1.869
	(.874)	(.976)	(1.860)
Demographic Controls	No	No	Yes
Session fixed effects	Yes	Yes	Yes
Observations	576	576	576
Number of subjects	72	72	72
Log likelihood	-193.85	-192.87	-192.50
$Prob > \chi^2$	< 0.000	< 0.000	< 0.000

Note: Random-effects logit coefficients. Numbers in parentheses indicate robust standard
errors clustered at an individual level. All models include session fixed effects. Model 3
includes demographic control variables. Demographic variables include Age, Gender and
Language (Native German or English speaker / Other native language). ***Significant at
1%; **Significant at 5%;

Result 4a. *Third-parties have a positive willingness to pay for a right to observe the negotiations*
between the proposer and responder. The fraction of third-parties who are willing to observe the
communication is decreasing in price.

Result 4b. *Third-parties' willingness to pay for a right to observe the negotiations between the*
proposer and responder is not associated with their beliefs about their own outcome.

Figure 1 suggests that there are only modest differences between treatments in third-parties'
willingness to pay for a right to observe the negotiations. Pairwise non-parametric tests confirm

this impression. Mann-Whitney tests show that third-parties' willingness to pay does not significantly differ across treatments ($p > 0.50$ for all pairwise comparisons).

Result 5. *Third-parties willingness to pay does not differ between the situations where they can observe the communication in their own group and where they can observe the communication in some other group.*

Result 6. *Third-parties willingness to pay does not depend on the possibility to signal their presence during the negotiations.*

3.3 Beliefs and earnings of the third-parties

We have observed that a large fraction of individuals is willing to pay for a right to observe the negotiation process despite that fact surveillance does not affect the resource allocation. Yet, an equally policy-relevant question is whether people believe that their presence during the negotiations affects distributional outcomes. Table 6 reports summary statistics about third-parties' beliefs by treatment and non-parametric test statistics based on a comparison between the reported beliefs and empirically observed actual distributions reported in Table 2. First, when comparing third-parties' beliefs with the empirically observed distributional outcomes, we find that the third-parties are systematically over-optimistic about their own outcome.[18] Second, we observe that third-parties are the most over-optimistic about their own outcome in treatment *Signal* where they have a chance to signal their presence to the negotiators. This qualitative observation in confirmed by testing the differences in third-parties' expectations about their outcome and empirically observed actual outcomes across treatments. We find that the third-parties are more over-optimistic about their own outcome in treatment *Signal* than in treatments *Baseline* (Mann-Whitney test: $p < 0.01$, two-sided), *Observation* (Mann-Whitney test: $p < 0.01$, two-sided), *Purchase* (Mann-Whitney test: $p < 0.01$, two-sided) and *Purchase-othe*r (Mann-Whitney test: $p = 0.114$, two-sided). We take this as evidence that the third-parties are the most over-optimistic about their own outcome when they have a chance to signal their presence to the negotiators.

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[18] It is noteworthy that in treatments *Baseline* and *Observation*, where there is no option to acquire a right to observe the negotiations, participants acting as third-parties are only requested to report their expectations about the proposers' behavior. In other words, reporting the expectations about the distributional outcomes is the only active decision made by the third-parties during the experiment. Consequently, it is not possible that the reported beliefs are biased due to hedging strategies.

Table 6. Expected average allocations to the responder and third-party by treatment.

Treatment	Expected Allocation to responder (ECUs)			Expected Allocation to third-party (ECUs)		
	Mean	Std.	Difference	Mean	Std.	Difference
Baseline	10.94	2.02	-0.71	6.68	3.44	1.74*
Observation	10.72	2.48	-0.64*	6.16	3.34	1.68*
Purchase	11.16	2.05	-0.29	6.19	3.97	1.42
Purchase-other	10.82	1.74	-0.52	7.59	3.16	2.53**
Signal	10.63	1.91	-1.12*	7.21	3.43	3.58***

Note: Variable Difference is defined as a differences between the reported beliefs about proposed allocations and the empirically observed actual average allocations reported. Number of observations in Baseline = 31, in Observation = 25, in Purchase = 31, in Purchase-other = 17 and in Signal = 24. Asterisks indicate the significnace of non-parametric tests statistics based on a comparison between the reported beliefs and empirically observed actual distributions. *** - sig. at 1%, ** - sig. at 5%, * - sig. at 10%.

Result 7. *Third-parties are over-optimistic about their own outcome. Third-parties are the most over-optimistic about their own outcome when they have a chance to signal their presence to the negotiators.*

Our results clearly show that the vast majority of individuals are willing to pay for a right to monitor decision processes over distributional outcomes. At the same time, we find that third-party surveillance does not affect distributional outcomes in our experiment. Third-parties also evidently receive the smallest relative share of the total endowment. Thus, by paying for a right to observe the negotiations the vast majority of individuals participating in our experiment are effectively paying to increase disadvantageous pay-off inequality between themselves and individuals acting as proposers and responders. We conclude that the willingness to pay for a right to scrutinize the negotiations process cannot be solely explained by narrowly self-interested strategic reasoning or motivation to reduce inequality between individuals acting in different roles. This impression is further strengthened by the fact that in our experiment individuals are willing to pay equally high amounts for a right to observe the negotiation process between some other proposer and responder than the proposer and responder who determine their pay-off. We

interpret these findings as evidence that individuals may derive intrinsic value from being able to monitor decision-making processes.

IV. Concluding remarks

People repeatedly demand greater transparency of decision-making from their governments and other bodies of decision-making. Information about the decision-making processes is considered to be essential to hold governments to account, to maintain integrity in the public sector and to guarantee a level playing field for enterprises. Despite a large public interest to facilitate the openness of decision-making on various scales, there have been no consistent attempts to study the impact of third-party surveillance on people's distributional preferences over outcomes. This paper aims to begin to fill this gap by reporting an experiment that adds an inactive third-party to an economic bargaining game and endows this person with a right to observe other individuals' pre-play communication. Moreover, we implement various treatments that offer opportunities to purchase surveillance rights to examine what motivates people to invest economic resources to monitor decision-making processes in the absence of participation rights.

There are three key findings. First, a large majority of individuals is willing to pay for a right to monitor decision processes over distributional outcomes despite pecuniary incentives to the contrary. This suggests that people may derive intrinsic value from being able to observe decision-making process over economic outcomes. However, it needs to be acknowledged that a decision to buy a right to monitor the decision-making process in our experiment has only limited absolute monetary consequences to third-parties which may limit the inferences that can be drawn from our study. Second, and arguably more importantly, we show that the knowledge of being under electronic surveillance does not influence distributional outcomes. Third, third-parties are the most over-optimistic about their own outcome when they have a chance to signal their presence to the negotiators.

The procedures used in this experiment to model economic decision-making and surveillance rights are necessarily stylized versions of naturally-occurring decision-making processes, but may be directly analogous to bargaining processes in electronic marketplaces and negotiations over the Internet and phone. Our results suggest, for example, that adding third-party recipients to e-mail negotiations is unlikely to alter the optimal persuasion tactics or change the balance of power between the negotiators. At the same time, our results suggest that creating a sense of

presence through observing other people over the Internet does not reduce the social distance between the actors enough to affect peoples' preferences over distributional outcomes.

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Electronic Supplementary Material for

Bargaining under Surveillance:

Evidence from a Three-Person Ultimatum Game

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I. Testing for Locations Effects

As mentioned in our article, we conducted two treatments, *Baseline* and *Purchase*, in two different locations - the Max Planck Institute (MPI) of Economics in Germany and the University of East Anglia (UEA) in the UK. Existing empirical evidence suggests that there may be systematic differences in the behavior of subjects across different societies (Roth et al., 1991; Henrich et al., 2001; Herrmann et. al, 2008). To test whether such differences can be found in our data set, we compare the behavior of subjects between these two locations in each of the two treatments.

Table S1 presents summary statistics about the proposed allocations in both treatments in the two locations. Table S1 also shows the p-values of (two-sided) Mann-Whitney tests for differences in allocations between locations for each role. These tests show that there are no significant differences between locations for any of the three roles in proposed allocations and in earnings. All proposed offers were accepted by the responders at the MPI. One offer was rejected in each of the two treatments at the UEA. The near complete acceptance rate, combined with very similar proposals in the two locations, implies that the mean individual earnings are very similar between locations for all three roles ($p > 0.20$ in all cases).

Table S1. Mean proposed allocations by the proposer (A) to the responder (B) and the third-party C in ECUs

		Baseline				*Purchase*		
	n	**A**	**B**	**C**	**n**	**A**	**B**	**C**
MPI	16	12.50	11.75	4.75	17	13.12	11.53	4.35
		(2.34)	(1.39)	(3.42)		(3.10)	(2.15)	(2.67)
UEA	15	12.33	11.53	5.13	14	12.36	11.36	5.29
		(2.06)	(2.03)	(3.74)		(3.03)	(2.21)	(3.95)
p-value		0.9839	0.8715	0.7174		0.2250	0.4799	0.3405

Note: Figures in parentheses are standard deviations

We now turn to the differences in the actions of the third-parties. Table S2 presents third-parties' estimates about the proposed allocations to the responders and third-parties in the two locations. Table S2 also presents the p-values of (two-sided) Mann-Whitney tests for differences in estimated allocations between locations for each role. Table S2 shows that there are no significant differences between locations in third-parties' estimates about proposed allocations to the responders. However, Table S2 shows that the third-parties are

systematically more optimistic about their own outcomes at the UEA than at the MPI. The same effect is observed independent of the treatment. In both treatments, individuals at the UEA expect to receive about 2 ECUs more than their counterparts at the MPI.

Table S2. Third-parties mean estimates of proposed allocations (in ECUs)

		Baseline			*Purchase*	
	n	**B**	**C**	**n**	**B**	**C**
MPI	16	11.25	5.63	17	10.88	5.06
		(1.73)	(3.48)		(1.80)	(3.65)
UEA	15	10.60	7.80	14	11.50	7.57
		(2.29)	(3.12)		(2.35)	(4.03)
p-value		0.2848	0.0535		0.4892	0.0537

Note: Figures in parentheses are standard deviations

Finally, we examine third-parties willingness to pay to monitor the communication between the proposer and responder. Figure S1 presents the fraction of third-parties who are willing to monitor the communication at each price in treatment *Purchase* at the MPI and at the UEA. Figure S1 suggests that there are no systematic differences between the two locations. The mean willingness to pay is 0.78 ECUs at the MPI and 0.87 ECUs at the UEA. A Mann-Whitney test shows that the difference is not significant ($z = 0.569$; $p = 0.44$, two-sided).
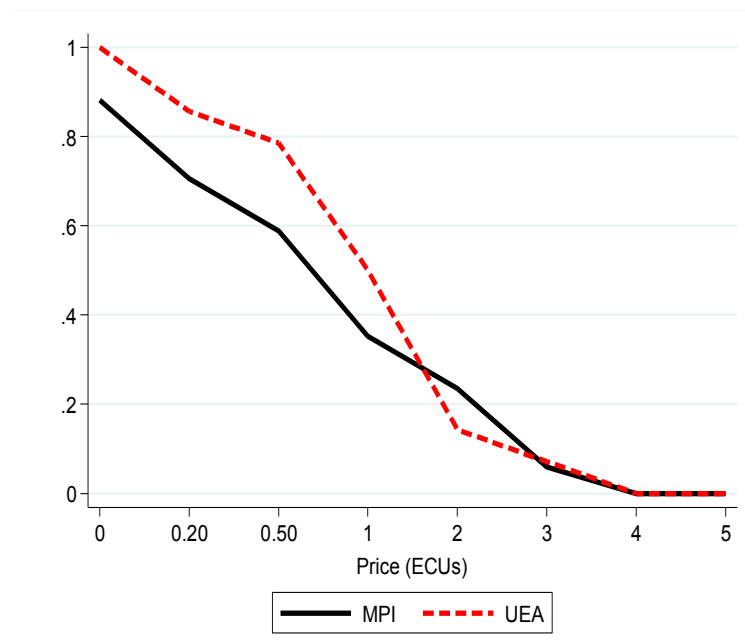
Figure S1. Willingness to pay to monitor communication between the proposer and responder in treatment *Purchase*

Overall, the analysis in this section shows that there are no significant differences, except for the third-parties expectations about their own outcome, in behavior of individuals between the two locations. In particular, there are no significant differences in the behavior of proposers and responder between these two locations.

II. Supplementary Analyses

II.1 Bargaining Time and Content

We report in our research article that third-party surveillance does not affect the length of the negotiation period between the proposer and responder. To support this conclusion, Table S3 presents summary statistics about the time spent on bargaining (in seconds) in each of the treatments. We measure the time spent in the communication phase by timing the length of the communication phase until the first person quits the communication by leaving the chat and proceeding to the next decision stage. Thus, we measure the time effectively spent on communication by both persons in each pair.

Table S3. Time spent in communication phase (seconds)

Treatment	Observations	Mean	SD
Baseline	31	128.16	61.13
Observe	25	123.72	57.77
Purchase	31	119.19	54.30
Signal-Observe	8	137.63	55.01
Signal-Not observe	16	137.82	48.34
Purchase-other	17	132.47	57.54
Total	129	127.57	55.75

Table S3 suggests that there is very little variation in the time spent on communication across treatments. Mann-Whitney tests show that there are no significant differences in any pairwise comparison between the treatments ($p > 0.10$ in all cases). We conclude that there is no evidence to suggest that third-party surveillance would affect the effective length of bargaining between the proposer and responder.

We present below some typical extracts from the conversations between the proposers and responders in the UK. Here we focus on extracts from the UK to avoid any misapprehensions due to translation. However, a cursory overview of the Germany communication protocols shows that the contents of the communication are essentially the same across locations. It

appears that proposers and responders agree in all treatments that there are few reasons to be particularly benevolent towards a third-party. This conclusion is reached by most communicating pairs even if they begin by proposing a roughly equal split among all three persons.

Baseline (3 groups):

R: Hi
P: A: 10, B:10, C:9
P: ?
R: Since Person C has no say over the proposed split and I do I would suggest increasing our sharw
R: shaare*
P: alright
P: whats your suggestion?
R: 12, 12, 5
P: good
R: agreed?
P: yes
R: excellent
P: shall we continue?
*** End of conversation ***

P: A: 13, B: 13, C: 3 ?
R: Like it
P: Sorted then
R: Well that was easy
P: haha
*** End of conversation ***

P: I was thinking 10 for mr and u and 9 for person c as they can win more
R: that's a good idea
R: absolutely agree
P: shall we go ahead with it by pressing continue then?
R: yes
*** End of conversation ***

Purchase (3 groups)

R: helloe what is your plan
R: :)
P: Hi - we have two options; we can either split it between ourselves (14.50 each) OR split it between three of us (9.50 each) - what do you think?
R: i will prefer split betweeen us
P: just you and me?
R: yes
P: okay great - well in that case I'll go for £14.50 to you and to me and then nothing for player C, are you sure that is okay with you?

5

R: yes

P: oh wait - it's 29 ECUs so it'll have to be round numbers? so that would be 15 and then 14?

R: ok i will have 15 then

P: okay then.

P: thanks :) very productive

*** End of conversation***

P: hi..... i am thinking of dividing....10 to myself and u and 9 to person c?

R: yes, i will accept that offer

P: ????

P: oki...

*** End of conversation***

P: I will spilt half between you and me (person A and person B) and nothing for person C

R: yes do it

P: ok

P: make sure you dont decline

P: otherwise we will both get nothing

R: in promise i wont

P: good good

R: need money man lol

P: good good

P: we wont find out who is person C anyway

P: and neither will he / she find out who we are

P: if they dont allow 14.5 for each of us

P: i will give you 15

P: and myself 14

P: deal deal

P: ?

P: but how many rounds do we have?

*** End of conversation***

Purchase-other (3 groups)

R: what do you propse?

R: ???

R: almost a minute gone

P: Hi, Im going to divide the highest amount for me, and the lowest amount for person C

R: thats not acceptable with me

R: i wont accept and we wont get anything

R: how about split 14 for you 15 for me

R: 0 for person C

P: In more detail, i divide 10 for u and 5 for C because C cannot see this conversation

R: i want more than 10

R: offer me 15 and i will accept

R: person C does not need anything

P: 12 for u and 3 for C

R: person C does not need anything

R: it is between me and you

R: 14 for you

R: 15 for me
R: and i will accept on the next screen
*** End of conversation***

P: what are you thinking?
R: 11pounds for each of us and the rest for c?
P: is there anything to stop us going for £12 or £13 pounds each...?
P: Agree we should be equal though
R: then maybe 12 for us?
R: 13 pounds seems like too little for c
P: Sounds good
R: cool! 12 pounds then
P: so you would be happy with £12 each yeah?
R: yeahh
*** End of conversation***

R: Hi
P: Hi!
R: What do u think
P: well, we could split it to 14 for each of us, leaving just 1 for person C, if that isn't too mean!
R: haha it is indeed abit too mean
R: but
R: come to think of it
R: max reward he/she can get is 5
R: its not too bad
R: right?
P: exactly yeah, plus 2 for showing up. Still not a bad result...
R: okay well the perk of being A and B
R: we can get mean
R; so yea
P: im sure the karma will come around again in another experiment
P: cool 14 / 14 / 1 decided?
R: okay
R: deal
P: sweet
*** End of conversation***

II.2 Willingness to Pay after Eliminating Inconsistencies

We find that there are some third-parties who are inconsistent in their reporting of maximum willingness to pay to observe the negotiation between the proposer and the responder. In particular, we find that there are several instances where a third-party simply indicates the highest price he/she is willing to pay but does not indicate that he/she is willing to pay some or all of the lower prices. There were 6 subjects with inconsistent price lists (3 in each

location) in treatment *Purchase*, 9 subjects in treatment *Purchase-other* and 5 subjects in treatment *Signal*. Including these potential misconceptions and related mistakes in our data may result in incorrect interpretations. To test the robustness of our results, we estimate third-parties' willingness to pay in all treatments after dropping those individuals whose reported willingness to pay is inconsistent in the sense mentioned above.
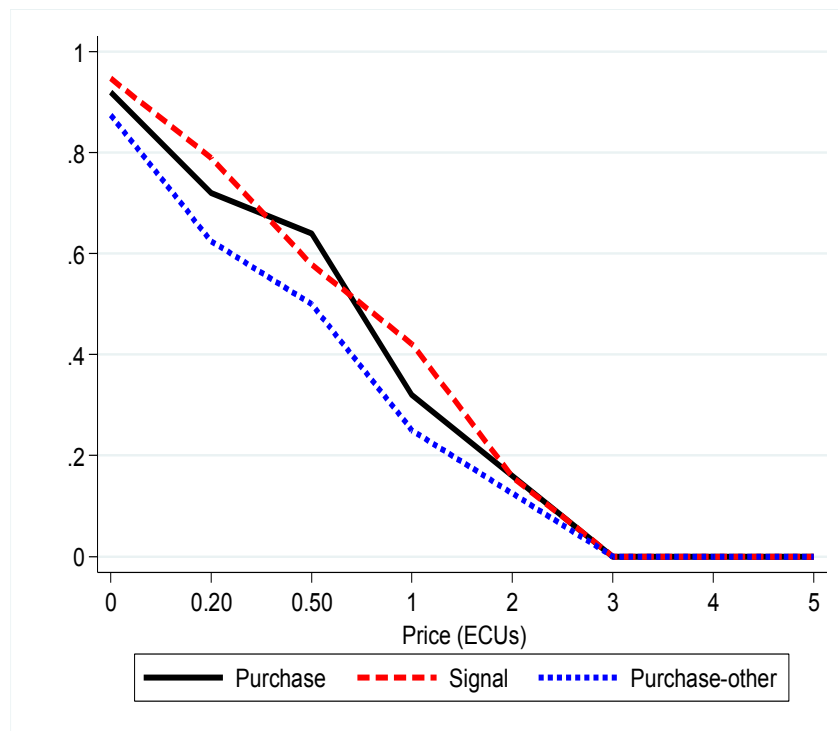

Figure S2. Third-parties' willingness to pay for a right to monitor the communication, excluding the individuals who report inconsistent price lists

When we exclude the individuals who report inconsistent price lists from our data, the average price a third-party is willing to pay is 0.66 ECUs in treatment *Purchase*, 0.70 ECUs in treatment *Signal* and 0.53 ECUs in treatment *Purchase-other*. Consistent with the results reported in our article, we find that the willingness to pay is the highest in treatment *Signal* and the lowest in treatment *Purchase-other*. Also, as reported in our article, none of these differences is significant ($p > 0.40$ for all pairwise treatment comparisons). Finally, we compare the mean willingness to pay in this subset of third-parties with the mean willingness to pay of all third-parties as reported in Section 3.2 of our article. We do not find significant differences in any of the treatments ($p > 0.10$ in all cases).
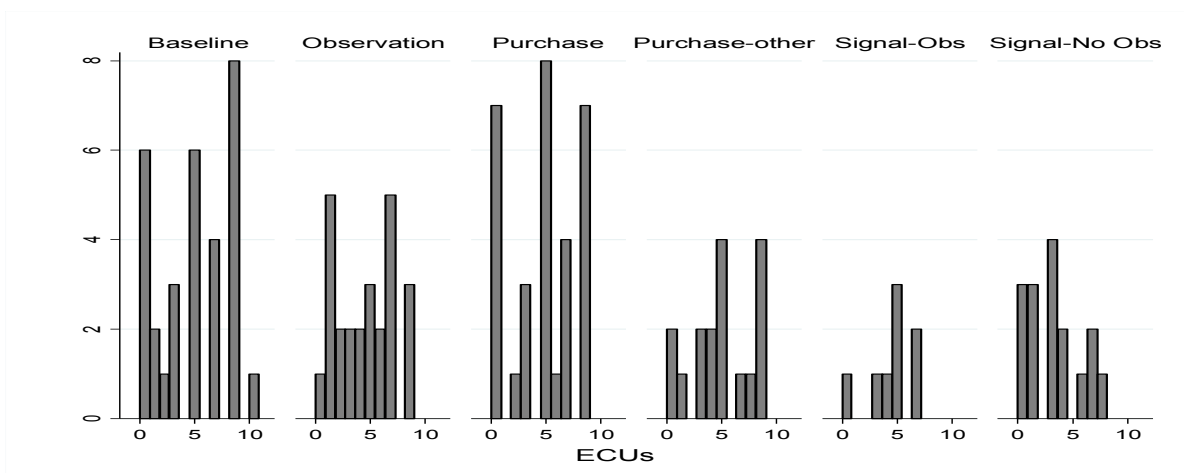
II.3 Additional Figures

(a) To the proposer
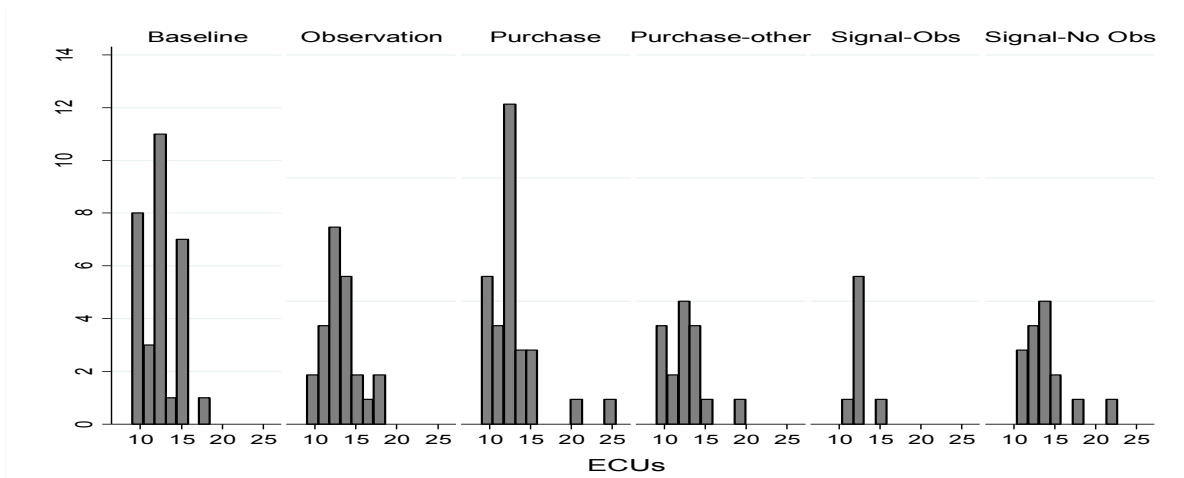
(b) To the responder
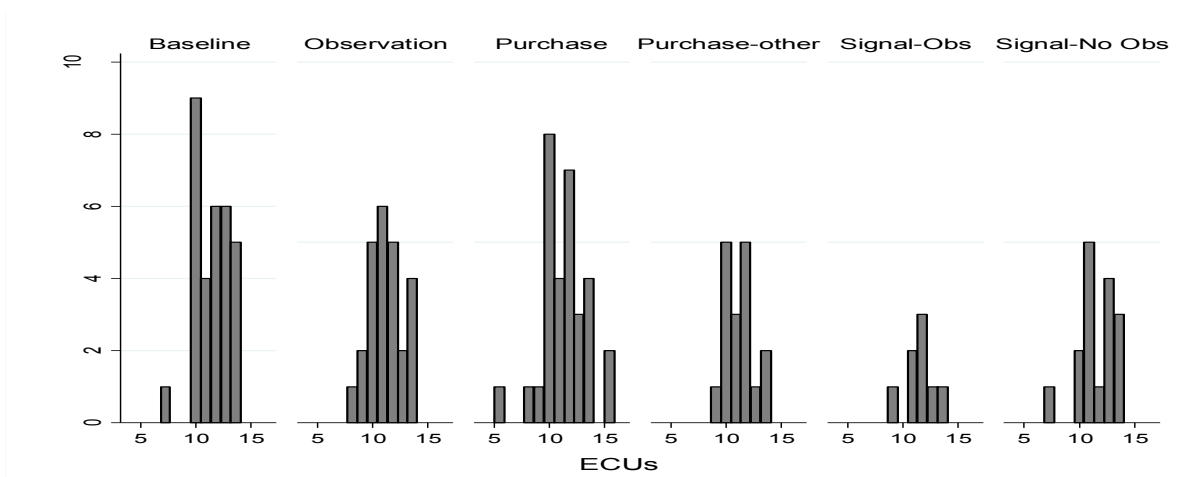
(c) To the third-party

Figure S3. The distribution of proposed offers by treatment.

III. Experimental Instructions

Here we present the experimental instructions used in treatment *Purchase-other* conducted in at the University of East Anglia, Norwich. These are based on an English translation of the German instructions used in Germany. We present the instructions used in treatment *Purchase-other* as they are the most comprehensive instructions.

Instructions

Thank you for your interest in participating in our experiment. For your participation you will receive £2. In addition, you can earn a considerable amount of money depending on your own decisions and the decisions of other participants. Your earnings will be privately paid to you in cash at the end of the experiment.

These instructions are solely your private information. **During the experiment you are not allowed to communicate with anybody.** Please switch off your mobile phone. Any violation of these rules will lead to exclusion from the experiment and all payments. If you have any questions at any time during the course of this experiment, please raise your hand and a member of the experimenter team will privately assist you.

This experiment is structured so that the other participants will not be able to trace your decisions or earnings to you personally. You will record your decisions privately at your computer terminal. During the experiment all decisions and transfers are made in Experimental Currency Units (ECUs). At the end of the experiment, your total earnings will be calculated in ECUs and converted to Pounds at the following rate:

1 ECU = 1 Pound

Basic Structure

There are three different types of persons in this experiment (Persons A, B and C). Your type will be randomly selected at the beginning of the experiment. Thereafter, you will be matched with two other types of persons. Person A has to decide how to divide 29 ECUs among the three persons. This decision will be communicated to Person B and Person B

decides if he/she accepts the allocation as proposed. If Person B agrees, all three persons will be paid according to the proposal. If Person B declines the proposed allocation, each person will get nothing.

Communication

Prior to the allocation decision, Persons A and B will have a chance to exchange written communication. The communication takes place in a chat window and lasts for a maximum time of 180 seconds. A clock will show you how much time you have left for communication. If Persons A and B need less than 180 seconds to communicate with each other, they can advance to the decision stage by pressing the 'Continue' button on their computer screens. Please note that both participants have to confirm the decision to proceed to the decision stage.

During the communication period, Persons A and B may discuss anything they like, including the best approach to the experiment, what they plan to do, or what they expect from the other persons. However, there are two important restrictions on the types of messages that can be sent. **(I.)** You may not send a message that aims to identify you or other persons. Thus, you may not use your real name, nicknames, or self-descriptions of any kind ("It is John Smith here", "Hi, I am Sandy, a French major", "I am the guy in a red shirt next to the window", or even, "As a woman [young person, international student, etc.], I think…"). **(II.)** No threats or promises pertaining to the events that occur after the experiment has been finished are allowed. No abusive language is allowed.

The team organizing this experiment will screen your messages. If your message is found to violate these rules, you may be excluded from the experiment and all payments.

Prior to the communication period, Person C decides if he/she wants to <u>observe</u> the communication between Persons A and B through a chat window. However, Person C **cannot** observe the communication between Persons A and B in his/her own group. Person C can observe the communication between Person A and Person B in **some other group in the lab**. Person C may <u>not</u> take part in the discussion.

Persons A and B will <u>not</u> be informed whether Person C (from another group) observes the discussion before the communication begins.

The decision process for Person C

Person C may either receive an additional amount of money or he/she can observe the communication (in another group). A decision to observe the communication essentially means that Person C pays a price to observe the discussion. There are eight possible amounts Person C may receive if he/she decides <u>not</u> to observe the communication. These amounts are 0, 0.20, 0.50, 1, 2, 3, 4 and 5 ECUs. For each of these amounts, Person C must decide if he/she will forgo the amount and instead gain the right to observe the communication between Persons A and B (in another group). Alternatively, Person C can choose to take the amount and forgo the right to observe the communication.

The computer then randomly picks one of these amounts. The realization of each amount is equally likely. If the amount chosen by the computer is smaller than or equal to the highest amount for which Person C is willing to observe the communication, then Person C will observe the communication and not receive the amount. If the chosen number is higher, then Person C will receive the amount and will not be able to observe the communication between Persons A and B.

For example, suppose that Person C is willing to pay any amount up to 3 ECUs to observe the discussion (in another group). This means that there is a 75% (=6/8) probability that Person C will observe the discussion and does not receive 0, 0.20, 0.50, 1, 2 or 3 ECUs. By comparison, there is a 25% probability that Person C will not be able to observe the discussion, but receives either 4 or 5 ECUs.

[Please note that the numbers used in the example have been chosen arbitrarily. They give no information about how you and other persons are expected to decide during the experiment.]

Bonus stage (for Person C)

In addition to the described decision situation, we ask Person C to estimate the **proposed allocation by Person A in <u>your</u> group.** This estimation takes place <u>before</u> the communication and decision stages. Person C is requested to estimate (in integers) how many

ECUs Person A in your group allocates to Person B and to Person C in your group. Person C will be rewarded for the accuracy of his/her estimate (see example).

(I.) If the estimate exactly corresponds with the actual allocation, Person C will receive a reward of 5 ECUs **(II.)** If the estimate deviates by one ECU from the actual allocation, Person C will receive a reward of 4 ECUs. **(III.)** If the estimate deviates by two ECUs from the actual allocation, Person C will receive a reward of 3 ECUs. **(IV.)** If the estimate deviates by three ECUs from the actual allocation, Person C will receive a reward of 2 ECUs. **(V.)** If the estimate deviates by four ECUs from the actual allocation, Person C will receive a reward of 1 ECU. **(VI.)** If the estimate deviates by five or more ECUs from the actual allocation, Person C will not receive a reward.

How is the deviation calculated?

We will calculate the absolute deviation between your estimate and the actual proposal separately for allocations to Person B and Person C. The sum of these deviations will give the total deviation that is used to determine the payoff.

For example, you estimate that Person A in your group will propose 10 ECUs to Person B and 19 ECUs to Person C. Suppose Person A proposes 8 ECUs to Person B and 21 ECUs to Person C. The total deviation is calculated as $(10 - 8) + (21 - 19) = 4$. Person C will be rewarded with 1 ECU.

[Please note that the example has been chosen arbitrarily. It will not give you any indication of how Person A will decide during the experiment]

Bonus stage (for Persons A and B)

In addition to the described decision situation, we ask Persons A and B to estimate the **highest amount for which Person C** in **the other group** is willing to observe the communication between the two of you. This estimation takes place <u>before</u> the communication and decision stages. You will see the same eight amounts that Person C can choose from - 0, 0.20, 0.50, 1, 2, 3, 4 and 5 ECUs. Choose **ONE** of these amounts – the one that you think is the maximum that Person C (in the other group) will be willing to pay. Persons A and B will be rewarded for the accuracy of their estimates (see example).

(I.) If the amount you have chosen exactly corresponds with the actual highest amount chosen by Person C (in the other group), you will be rewarded with 2 ECUs. **(II.)** If the

amount you have chosen is one amount higher or one amount lower than the actual highest amount chosen by Person C, you will be rewarded with 1 ECU. **(III.)** If the amount you have chosen is more than one amount away (higher or lower), you will not receive a reward.

For example, you estimate that Person C (in the other group) will be willing to pay a maximum of 3 ECUs to observe the communication between Persons A and B in your group. Suppose Person C (in the other group) has actually chosen 2 ECUs as the highest amount that he/she is willing to pay. Since the amount you have chosen is one amount higher than that chosen by Person C, you will receive a reward of 1 ECU.

[Please note that the example has been chosen arbitrarily. It will not give you any indication of how Person C will decide during the experiment]

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