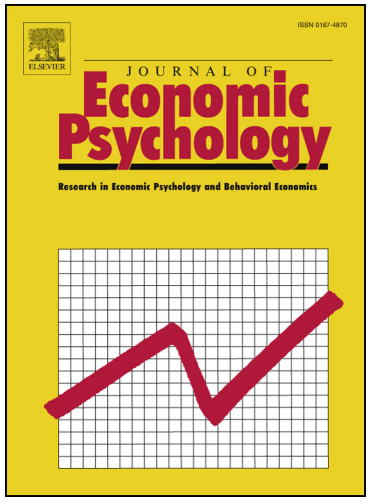


Bargaining under Surveillance: Evidence from a Three-Person Ultimatum Game

Lauri Sä äksvuori, Abhijit Ramalingam

PII: S0167-4870(15)00105-1
DOI: http://dx.doi.org/10.1016/j.joep.2015.08.007
Reference: JOEP 1851

To appear in: *Journal of Economic Psychology*

Received Date: 26 August 2013
Revised Date: 14 August 2015
Accepted Date: 20 August 2015

Please cite this article as: Sä äksvuori, L., Ramalingam, A., Bargaining under Surveillance: Evidence from a Three-Person Ultimatum Game, *Journal of Economic Psychology* (2015), doi: http://dx.doi.org/10.1016/j.joep.2015.08.007

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

Lauri Sääksvuori[1, 2] and Abhijit Ramalingam[3]

[1] University of Hamburg, School of Business, Economics and Social Sciences, Department of Economics, Von-Melle-Park 5, 20146 Hamburg, Germany

[2] National Institute for Health and Welfare, Centre for Health and Social Economics, Finland

[3] School of Economics and Centre for Behavioral and Experimental Social Science, University of East Anglia, Norwich NR4 7TJ, United Kingdom

Correspondence to: lauri.saaksvuori@thl.fi

Abstract:

This paper investigates how the transparency of decision-making affects preferences over distributional outcomes. We also examine what motivates individuals to voluntarily invest economic resources to monitor decision-making processes. We find that third-party monitoring does not affect distributional outcomes in a three-person ultimatum game. Our results show that a large majority of individuals is willing to pay for a right to monitor decision-making processes despite pecuniary incentives to the contrary. We observe that third-parties are over-confident in believing that an opportunity to scrutinize decision-making processes changes distributional outcomes for their own benefit. Our results suggest that people may over-estimate the effect of transparent decision-making on economic outcomes.

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

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

I. Introduction

Many economic and political decisions are made either in transparent or secretive circumstances. We regularly observe political, legal and private disputes over citizens' rights to monitor decision-making processes. Historically, influential political movements have been launched aiming to increase the transparency of political and economic decision-making by granting citizens access to government records and decision-making venues to monitor political decision-making processes.[1] Currently, the emergence of modern information technologies, particularly the Internet and social media, is changing the traditional landscape of governance, management and negotiations by fuelling even greater public demand for more transparent decision-making.[2]

This paper experimentally investigates how the knowledge of being under surveillance during a negotiation process influences preferences over distributional outcomes. We also examine what motivates individuals to voluntarily 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 investigate how the knowledge of being under surveillance influences resource allocation through bargaining and whether people have an intrinsic motivation to observe bargaining processes that determine distributional outcomes.

Scientific interest in the influence of surveillance on human behavior 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

[1] There is a long history of demanding unimpeded public access to government documents. Sweden was probably the first country to adopt laws granting all citizens unrestricted access to government documents which were not deemed particularly confidential. These laws were adopted in Sweden in 1776.

[2] There have traditionally been differences in access to government documents. For instance, while the Federal Reserve Open Market Committee and the US Supreme Court have for long published their minutes and released the votes of individual committee members, the views of the individual members of the Governing Council of the European Central Bank and the EU Court of Justice have typically not been publicly accessible. The European Central Bank published the minutes of a governing council meeting for the first time on February 19, 2015.

given indirect attention by measuring their performance.[3] The experiments at the Hawthorne Works not only laid a foundation for studies that focus on the 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 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 connects to a voluminous literature studying the importance of social structure on economic behavior. Social identity and social distance between 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 other person's name (Charness and Gneezy, 2008) and seeing the other person's picture (Andreoni and Petrie, 2004; Eckel and Petrie, 2011) increase the salience of other-regarding concerns in economic decision-making. Likewise, people are known to behave more pro-socially when their decisions are observable to their teammates (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).

[3] 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.

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.[4] 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. 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 diffusion of information between individuals and the contents of pre-play communication. Even though there is no prior evidence directly showing that third-party surveillance alters the contents of communication, it can be hypothesized that third-party surveillance diminishes the effectiveness of communication to facilitate economic coordination and cooperation by inhibiting individuals' willingness to reveal their preferences and intentions.[5]

[4] 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. (2013) 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.

[5] Brosig et al. (2003) observe that the communication medium influences both the level and the stability of cooperation in a public goods game, even though the contents of communication are not influenced by the communication medium. Oprea et al. (2014) show that the effectiveness of communication as a coordination device depends on the richness of the message space. While these studies do not directly investigate the influence of third-party surveillance on the effectiveness of communication, they suggest that the richness of communication may impact the effectiveness of interpersonal communication to facilitate economic coordination and cooperation.

We use a three-person ultimatum game as a workhorse to investigate the effects of transparency on bargaining behavior and distributional preferences. The game provides an ideal environment to study the effects of third-party monitoring as it consists of a very simple bargaining scenario with an inactive third-party whose payoff is affected by the bargaining outcome. The innovation of this paper is to endow the third-party with a right to observe other participants' pre-play communication. This allows us to examine whether the knowledge of being under third-party surveillance during a pre-play communication stage increases proposed allocations to third-parties. In addition, we implement three treatments 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, which vary the information given to the negotiators about the presence of third-parties, enable us to distinguish strategically motivated purchasing decisions from an intrinsic motivation to acquire a right to observe the negotiations.

Our results show that the monitoring of decision-making processes by third-parties does not influence 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 despite pecuniary incentives to the contrary. We observe that individuals in our experiment are over-confident in believing that an opportunity to monitor decision-making processes changes 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. Overall, our results suggest that people may over-estimate the effect of transparent decision-making on economic outcomes. 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.

II. Experimental Design and Behavioral Predictions

2.1 Experimental Design

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

to be divided among the proposer, a responder and an inactive third-party.[6] The proposer makes an offer of a split of the 29 ECUs among the three parties. Thereafter, the responder decides whether to accept the offer as proposed. If the proposer's offer is accepted by the responder, the offer is implemented and players' earnings are according to the proposal. If the 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, open-ended communication also enables players to develop negotiation 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 seem a limitation of the study, it is worth mentioning that several potential applications of this study comprise of situations where the negotiation process is documented in a written form. Some illustrative examples of communication between participants are shown in the Electronic Supplementary Material (ESM). 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

[6]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.

bargaining game and the communication stage prior to the game. In treatment *Baseline*, inactive third-parties do not observe the course of negotiations between the proposer and responder. In treatment *Observation*, inactive third-parties are 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*, inactive third-parties are given an option to purchase a right to observe the course of negotiations. In treatment *Purchase-other*, inactive third-parties are 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 their payoff. In both *Purchase* and *Purchase-other*, the proposer and responder are aware that a third-party can potentially observe their discussion but are not informed about actual third-party presence during the negotiations. In treatment *Signal*, inactive third-parties are given an option to purchase a right to observe the course of negotiations. In this treatment, the proposer and responder are informed whether a third-party is monitoring the course of negotiations before beginning the communication phase. Table 1 summarizes the five treatments.

Table 1. Experimental Treatments.

Treatment	Third-party observes negotiation	Third-party may buy a right to monitor the negotiation	Negotiators know whether third-party is observing	Location	N. of groups
Baseline	No	No	Yes	G / UK	16 / 15
Observation	Yes	No	Yes	G	25
Purchase	Endogenous	Yes	No	G / UK	17 /14
Purchase-other	Endogenous	Yes	No	UK	17
Signal	Endogenous	Yes	Yes	G	24

Note: G = Germany, UK = United Kingdom

In treatments *Purchase*, *Purchase-other* and *Signal*, where third-parties have an option to purchase a right to monitor the negotiations, we elicit individuals' 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 the 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 a sum of money equal to the random draw and does not observe the negotiation process. The prices are 0 ECU, 0.20 ECUs, 0.50 ECUs, 1 ECU, 2 ECUs, 3 ECUs, 4 ECUs and 5 ECUs.[7]

In all treatments, individuals acting as third-parties are requested to report their expectations about the bargaining outcome. This elicitation of individuals' expectations renders it possible to assess whether greater transparency through a right to monitor the negotiation process affects individuals' 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.[8] 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

[7] 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 price lists and many practical issues related to explaining the logic of the mechanism to experimental subjects. Notably, a specific price list may create an anchor which influences the stated willingness to pay. Here we are not interested to estimate subjects' valuations for a right to observe as such, but draw relative 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.

[8] 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.

ECUs. For experimental simplicity, the reward decreases in a linear fashion as the deviation increases.[9]

2.2 Behavioral Predictions

Assuming selfish preferences, the predictions of standard game theory for our experiment are straightforward.[10] However, years of experimental research supported by behavioral theories have shown that people may have manifold motivations and exhibit a variety of behavioral biases. Our approach to generate behavioral predictions 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 a positive self-image and social reputation.

There is voluminous theoretical and empirical literature documenting the existence of in-group favoritism (for instance, 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 processes 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 monitoring 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; Battigalli 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

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

[10] 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.

proposer and responder, we would expect that they allocate more resources to a third-party in a treatment where third-parties have an exogenously granted right to monitor the negotiation process than in a treatment where there is no opportunity to monitor the negotiation process. Thus, we summarize the expected impact of an exogenously granted right to monitor the negotiations between the proposer and responder on resource allocations as follows:

Hypothesis 1: *An exogenously granted right to observe the negotiations increases proposed allocations to third-parties relative to the Baseline.*

While Hypothesis 1 focuses on the aggregate effect of exogenous third-party monitoring on proposed allocations, treatments *Purchase* and *Purchase-other* enable to investigate the effect of endogenous monitoring opportunities on proposed allocations. In particular, if proposers react to third-party monitoring by increasing their proposed allocations and believe that a sufficiently large number of third-parties is willing to acquire a right to monitor the negotiations, it would be expected that the mere possibility of being observed by a third-party influences the allocation of resources. However, the aggregate effect of endogenous third-party monitoring on proposed allocations depends on proposers' beliefs about the probability that they are monitored. Thus, it can be hypothesized that the effect of endogenous monitoring opportunities on proposed allocations is smaller than the effect of an exogenously granted right to monitor the negotiations.

It can be hypothesized that proposers' and responders' beliefs about third-parties' willingness to pay for a right to monitor the negotiations depend on whether a third-party may observe the negotiation process between the proposer and responder who decide about the third-party's outcome. In particular, proposers and responders may expect that third-parties who may buy a right to observe negotiations between a proposer and responder other than the proposer and responder who decide about their pay-off may have a lower willingness to pay for a right to observe the negotiations. In this case, it would also be less likely that a third-party is observing the negotiations in treatment *Purchase-other* than in treatment *Purchase.* Assuming that the proposed aggregate allocations are sensitive to the expected presence of third-parties, it would be expected that an opportunity to buy a right to monitor the negotiations increases proposed allocations to third-parties less in treatment *Purchase-other* than in treatment *Purchase*. We summarize these considerations as follows:

Hypothesis 2A: *An opportunity to buy a right to monitor the negotiations increases proposed allocations to third-parties relative to the Baseline.*

Hypothesis 2B: *An opportunity to buy a right to monitor the negotiations increases proposed monetary allocations to third-parties less than an exogenously granted right to monitor the negotiations.*

Hypothesis 2C: *The effect of endogenous monitoring opportunities on proposed monetary allocations is smaller in treatment Purchase-other than in treatment Purchase.*

Hypotheses 1 and 2 focus on the aggregate effect of third-party monitoring on proposed allocations, but do not pay direct attention to the behavioral mechanisms that may explain greater allocations to third-parties in treatments with monitoring opportunities. However, treatments *Purchase* and *Purchase-other* enable us to more carefully investigate the behavioral factors that may guide the allocation decisions towards third-parties. 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 enables us to investigate the relationship between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations. In particular, if participants' social image concerns are negligible, we would expect proposals that are independent of proposers' and responders' beliefs about third-party presence. However, if a wish to be perceived as a pro-social person in the eyes of other agents is an important 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.

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:

Hypothesis 3A: *There is a positive association between the beliefs of the proposer and responder about third-party presence and proposed monetary allocations.*

Hypothesis 3B: *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 to explore whether individuals are willing to invest economic resources to monitor negotiations where two other agents bargain over outcomes which affect the payoffs of all parties.

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 (Loewenstein, 1994), a desire to reduce perceived social distance between the agents (Sherif 1958, Leeson 2008) and uncertainty avoidance. 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.

Moreover, treatments *Purchase and Purchase-other* enable us to explore different motivations that may guide participants' decisions 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 the negotiation process between a proposer and responder who do *not* determine their payoffs. 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 own monetary outcome. 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*. We summarize these considerations as follows:

Hypothesis 4A: *There is a positive willingness to pay for a right to observe the negotiations.*

Hypothesis 4B: *There is greater willingness to pay for a right to observe negotiations when third-parties observe the negotiation process between the proposer and responder who bargain over third-parties' own outcome than when third-parties observe 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 changes distributional outcomes for their own benefit. There are multiple behavioral mechanisms that may lead third-parties to believe that their presence changes distributional outcomes for their benefit. Particularly noteworthy mechanisms 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 changes distributional outcomes for their benefit. By contrast, we simply hypothesize that a chance of signaling presence during negotiations at large increases the willingness to pay for a right to monitor negotiations over distributional outcomes.

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

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 experimental sessions were conducted at the laboratories of the Max Planck Institute of Economics in Germany and of the University of East Anglia in the UK. The experiment was 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 information 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 significant differences between the two locations enables us to compare the treatment *Purchase-other* with other treatments regardless of where the treatments were conducted.

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

We first investigate the impact of third-party surveillance on proposed resource allocations. 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

[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.

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.

		Allocation to Responder (ECUs)			Allocation to third-party (ECUs)		
Treatment	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.

Table 2 presents average allocations by proposers to 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. 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 the treatments ($p = 0.5367$).[13]

[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.

[13] There are also no significant differences in allocations to the proposer ($p = 0.2530$) and to the responder ($p = 0.9117$).

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.

Table 3 shows the p-values for two-sided Mann-Whitney tests that compare allocations to the third-parties across all pairs of treatments. Overall, Table 3 shows that there are no significant differences in offers made to the third-parties between any two treatments.[14] In particular, we find that an exogenously imposed third-party surveillance in treatment *Observation* does not affect proposed resource allocation compared to the treatment where there is no possibility to observe the negotiations (*Baseline*) (Mann-Whitney test: $p = 0.635$). This gives our first result.

Result 1: *Compared to the Baseline, an exogenously granted right to observe the negotiations does not affect proposed allocations to third-parties.*

Likewise, an opportunity to buy the right to monitor negotiations in treatments *Purchase, Purchase-other* and *Signal* does not increase proposed allocations relative to those in the *Baseline* ($p > 0.10$ in all cases). This is also the case when the proposer and responder are aware of third-party presence (*Signal – Obs.*). Furthermore, allocations to the third-parties in treatment *Observation* and in other treatments with endogenous observation are not significantly different from each other. Finally, allocations to third-parties in treatment *Purchase-other* are not

[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. We note that the statistical inference here may suffer from multiple comparisons problem. If we apply the Holm-Bonferroni correction to account for multiple comparisons, none of the comparisons is statistically significant at the 10 % level.

significantly different from those in treatment *Purchase* (p = 0.869). Thus, we do not find any evidence in support of Hypothesis 2.

Result 2A: *Compared to the Baseline, an opportunity to buy a right to observe negotiations does not affect allocations to the third-party.*

Result 2B: *Allocations to the third-party are unaffected by whether a right to observe negotiations is exogenously granted or endogenously determined by the third-party.*

Result 2C: *Allocations to the third-party are unaffected by which group the third-party can observe.*

Results 1 and 2 suggest 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 an indicator of the perceived irrelevance of 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 for a right to observe the communication in treatments *Purchase* and *Purchase-other*.[15] Table 4 presents proposers' and responders' average estimates about third-parties' willingness to pay (WTP) for a right to observe the communication in these two treatments. For reference, we also 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, two-sided) comparing the estimates of the proposer and responder with the third-parties' actual WTP by treatment.

[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. 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 are in parentheses. Figures in brackets are p-values.

Table 4 shows that the proposer and responder over-estimate third-parties' willingness to pay in both treatments. Mann-Whitney test statistics show that the responders significantly over-estimate third-parties' willingness to pay in treatment *Purchase*, while both the proposer and the responder significantly over-estimate 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 Pearson product-moment correlation coefficient between the proposers' estimates of the third-parties' maximum willingness to pay and the proposed allocations to third-parties is -0.1220 ($p = 0.6777$, two-sided) in treatment *Purchase* and -0.354 ($p = 0.163$, two-sided) 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 ($p = 0.845$, two-sided) in treatment *Purchase* and -0.209 ($p = 0.423$, two-sided) 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 3: *The proposer and responder over-estimate 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.* [16]

[16] It is 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. An analysis of the average length of negotiations in

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 is 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*, 0.87 ECUs in treatment *Purchase* and 0.78 ECUs in treatment *Purchase-other*. Wilcoxon Signed-rank tests confirm that the average willingness to pay is significantly greater than zero ($p < 0.001$, two-sided) in all three treatments.

Result 4: *On average, third-parties have a positive willingness to pay for a right to observe the negotiations between the proposer and responder.*

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.

[Figure 1 about here.]

Figure 1. Third-parties' willingness to pay to observe the communication.

each treatment is reported in the Electronic Supplementary Material. We find that third-party surveillance does not affect the length of the negotiation period between the proposer and responder.

[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.

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 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 determining the probability of purchasing a right to observe – Random Effects Logit.

Probability of Purchasing a Right to Observe – Random Effects Logit			
Variables	(1)	(2)	(3)
Price	-2.208***	-1.889***	-1.863***
	(.374)	(.126)	(.442)
Belief		0.126	0.135
		(.085)	(.091)
Price x 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 > χ^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 5: *The fraction of third-parties who are willing to observe the communication is decreasing in price. However, 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.*

Finally, 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, two-sided).

Result 6. *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 7. *Third-parties' willingness to pay does not depend on the possibility to signal their presence during the negotiations.*

3.3 Beliefs and earnings of third-parties

We have observed that a large fraction of individuals is willing to pay for a right to observe the negotiation process despite the fact that surveillance does not affect resource allocation. Yet, an equally policy-relevant question is whether people believe that their presence during the negotiations affects distributional outcomes. Even though we do not have formal hypotheses concerning people's beliefs about the impact of their presence, we conduct an ex-post analysis of the relationship between third-parties' beliefs and their 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 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 is 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 third-parties are most over-optimistic about their own outcome when they have a chance to signal their presence to the negotiators.

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

	Expected Allocation to responder (ECUs)			Expected Allocation to third-party (ECUs)		
Treatment	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 difference between the reported beliefs about proposed allocations and the empirically observed actual average allocations. Number of observations in *Baseline* = 31, in *Observation* = 25, in *Purchase* = 31, in *Purchase-other* = 17 and in *Signal* = 24. Asterisks indicate the significance of Mann-Whitney test statistics based on a comparison between the reported beliefs and empirically observed actual distributions. ***Significant at 1%, **Significant at 5%, *Significant at 10%.

[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.

Result 9: *Third-parties are over-optimistic about their own outcome. Third-parties are most over-optimistic about their own outcome when they have a chance to signal their presence to the negotiators.*

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 large public interest to facilitate the openness of decision-making on various scales, there have been no consistent attempts to study the impact of transparency 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 which offer opportunities to purchase surveillance rights to examine what motivates individuals to invest economic resources to monitor decision-making processes.

There are three key findings. First, we find no evidence that the knowledge of being under surveillance influences distributional outcomes. Second, a large majority of individuals is willing to pay for a right to monitor decision-making processes over distributional outcomes despite pecuniary incentives to the contrary. Third, we observe that third-parties are over-confident in believing that an opportunity to scrutinize decision-making processes changes distributional outcomes for their own benefit.

The procedures used in this experiment to model economic decision-making and third-party monitoring are stylized versions of naturally-occurring decision-making processes. 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. Likewise, we 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 behavior in our experiment (see, for instance, Zizzo, 2010). However, we find that the vast majority of participants report a monotonically decreasing willingness to observe as the price increases. This observation suggests 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 behavior in our experiment.

In our experiment, all participants are anonymous to each other and cannot be personally identified. Hence, our experiment implements a remarkably weak form of third-party monitoring. Third-parties rarely act in isolation in naturally-occurring situations and are as powerless as the third-parties in our experiment. The ubiquity of word-of-mouth networks and opportunities to make private knowledge available to large audiences through media may heighten the value of information gained through third-party monitoring and affect the consequences of third-party surveillance. Likewise, third-parties' rank in a social hierarchy and centrality in a social network may influence the effect of third-party surveillance in naturally-occurring situations. For instance, Breza et al. (2015) find in their lab-in-the-field experiment that surveillance by high-centrality third-parties has substantially larger impact on trust game outcomes than surveillance by third-parties with low ranks in the centrality distribution. These observations overall indicate that the highly stylized and anonymous decision environment applied in this study may attenuate the impact of third-party monitoring on distributional outcomes.

Despite the limitations of our study, we observe certain characteristic patterns of economic decisions-making that may provide insight into the effects of third-party monitoring on the allocation of economic resources. One possible interpretation of our results is that the electronic monitoring of decision-making processes in an anonymous setting does not reduce the social distance between the agents to an extent that is necessary to influence resource allocations. Another possible interpretation is that the decision to monitor the decision-making process may be seen by the negotiators as an attempt to control their actions. Consequently, it is possible that proposers and responders perceive surveillance as a signal of distrust, leading to resentment and less pro-social behavior (Falk and Kosfeld, 2006). Likewise, the fact that third-parties are over-confident in believing that an opportunity to monitor decision-making processes changes distributional outcomes for their own benefit may be relevant to understand the rationale behind the persistent demands for more transparent decision-making. However, it is difficult to judge whether the tendency to over-estimate the effect of transparent decision-making on economic outcomes reflects a stable cognitive bias that is replicable in other environments or is just an experimental artifact inherent to the design of our study. The robustness of our results and their

external relevance has to be further studied before we can begin to make forceful arguments about the generalizability of our results.

Acknowledgements: Financial support was provided by the Max Planck Society, Yrjö Jahnsson Foundation and the School of Economics at the University of East Anglia. The authors would like to thank the editor and two anonymous reviewers for their helpful comments. The authors declare that they have no conflicts of interest.

REFERENCES:

Akerlof, G. A., & Kranton, R. E. (2000). Economics and identity. *Quarterly Journal of Economics, 115(3)*, 715-753.

Alesina, A.F., & Giuliano, P. (2009). Preferences for redistribution. *NBER, Working paper 14825, National Bureau of Economic Research, Inc.*

Andreoni, J., & Petrie, R. (2004). Public goods experiments without confidentiality: a glimpse into fund-raising. *Journal of Public Economics*, 88(7), 1605-1623.

Andreoni, J., & Bernheim, B. D. (2009). Social image and the 50–50 norm: A theoretical and experimental analysis of audience effects. *Econometrica*, 77(5), 1607-1636.

Ariely, D., Bracha, A., & Meier, S. (2009). Doing good or doing well? Image motivation and monetary incentives in behaving prosocially. *The American Economic Review*, 99(1), 544-555.

Bateson, M., Nettle, D. & Roberts, G. (2006). Cues of being watched enhance cooperation in a real-world setting. *Biology Letters*, 2(3), 412-414.

Battigalli, P., & Dufwenberg, M. (2009). Dynamic psychological games. *Journal of Economic Theory*, *144*(1), 1-35.

Becker, G., Degroot M. & Marschak, J. (1964). Measuring utility by a single-response sequential method. *Behavioral Science*, 9(3), 226-232.

Bernheim, B. D. (1994). A theory of conformity. *Journal of Political Economy*, 102(5), 841-877.

Bénabou, R. & Tirole, J. (2006). Incentives and Prosocial Behavior. *American Economic Review*, 96(5), 1652-1678.

Breza, E., Chandrasekhar, A.G., & Larreguy, H. (2015). Network Centrality and Institutional Design: Evidence from a lab Experiment in the Field. *NBER Working Paper, 20309.*

Brosig, J., Weimann, J., & Ockenfels, A. (2003). The Effect of Communication Media on Cooperation. *German Economic Review,* 4(2), 217-241.

Burnham, T., & Hare, B. (2007). Engineering cooperation: Does involuntary neural activation increase public goods contributions? *Human Nature*, 18(2), 88-108.

Cason, T., & Mui, V-L. (1997). A laboratory study of group polarization in the dictator game. *Economic Journal*, 107(444), 1465-1483.

Charness, G., & Gneezy, U. (2008). What's in a name? Anonymity and social distance in dictator and ultimatum games. *Journal of Economic Behavior & Organization*, 68(1), 29-35.

Crawford, V. (1998). A survey of experiments on cooperation via cheap talk. *Journal of Economic Theory*, 78(2), 286-298.

Dana, J., Weber, R. A., & Kuang, J. X. (2007). Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness. *Economic Theory*, 33(1), 67-80.

Eckel, C., & Petrie, R. (2011). Face value. *American Economic Review,* 101(4), 1497-1513.

Falk, A., & Kosfeld, M. (2006). The Hidden Costs of Control. *American Economic Review*, 96(5), 1611-1630.

Farrell, J. (1995). Talk is cheap. *American Economic Review, Papers and Proceedings*, 85(2), 186-190.

Fehr, E., & Fischbacher, U. (2004). Third-party punishment and social norms. *Evolution and Human Behavior*, *25*(2), 63-87.

Fehr, E., & Schneider, F. (2010). Eyes are on us, but nobody cares: are eye cues relevant for strong reciprocity? *Proceedings of the Royal Society B: Biological Sciences*, 277(1686), 1315–1323.

Fischbacher, U. (2007). z-Tree: Zurich Toolbox for Ready-made Economic experiments. *Experimental Economics, 10(2),* 171-178.

Glazer, A., & Konrad, K. A. (1996). A signaling explanation for charity. *American Economic Review, 86(4),* 1019-1028.

Güth, W., & van Damme, E. (1998). Information, strategic behavior, and fairness in ultimatum bargaining: An experimental study. *Journal of Mathematical Psychology, 42(2-3),* 227-247.

Haley, K.J. & Fessler, D. (2005). Nobody's watching? Subtle cues affect generosity in an anonymous dictator game. *Evolution and Human Behavior, 26(3),* 245-256.

Henrich, J., Boyd, R., Bowles, S., Camerer, C., Fehr, E., Gintis, H., & McElreath, R. (2001). In search of homo economicus: behavioral experiments in 15 small-scale societies. *American Economic Review, 91(2),* 73-78.

Herrmann, B., Thöni, C., & Gächter, S. (2008). Antisocial punishment across societies. *Science, 319*(5868), 1362-1367.

Hoffman, E, McCabe, K. & Smith, V.L. (1996). Social distance and other-regarding behavior in dictator games. *American Economic Review, 86(3),* 653-660.

Ireland, N. J. (1994). On limiting the market for status signals. *Journal of Public Economics, 53(1),* 91-110.

Isaac, R.M., & Walker, J.M. (1988). Communication and free-riding behavior: The voluntary contribution mechanism. *Economic Inquiry, 26(4),* 585-608.

Kahneman, D., & Knetsch, J. L. (1992). Valuing public goods: the purchase of moral satisfaction. *Journal of Environmental Economics and Management, 22(1),* 57-70.

Konow, J. (2000). Fair shares: Accountability and cognitive dissonance in allocation decisions. *American Economic Review, 90(4),* 1072-1091.

Klor, E., & Shayo, M. (2010). Social identity and preferences over redistribution. *Journal of Public Economics, 94(3-4),* 269-278.

Krupka, E., & Weber, R. A. (2009). The focusing and informational effects of norms on pro-social behavior. *Journal of Economic Psychology*, *30*(3), 307-320.

Landsberger, H. (1958). *Hawthorne Revisited. Management and the worker: Its critics, and developments in human relations in industry*. Ithaca, N.Y: Cornell University Press.

Ledyard, J. (1995). *Public goods: A survey of experimental research*, in: The Handbook of Experimental Economics; Kagel, John H. and Roth, Alvin E. (eds.), Princeton University Press, Princeton.

Leeson, P.T. (2008). Social Distance and Self-Enforcing Exchange. *The Journal of Legal Studies,* 37(1), 161-188.

Leibbrandt, A., & López-Pérez, R. (2012). An exploration of third and second party punishment in ten simple games. *Journal of Economic Behavior & Organization*, *84*(3), 753-766.

Levitt, S.D., & List, J.A. (2007). What do laboratory experiments measuring social preferences reveal about the real world? *Journal of Economic Perspectives, 21(2),* 153–1747.

Levitt, S.D., & List, J.A. (2011). Was There Really a Hawthorne Effect at the Hawthorne Plant? An Analysis of the Original Illumination Experiments. *American Economic Journal: Applied Economics, 3(1),* 224–238.

Loewenstein, G. (1994). The psychology of curiosity: A review and reinterpretation. *Psychological Bulletin*, 116(1), 75-98.

Mayo, Elton, (1933). *The Human Problems of an Industrial Civilization.* New York: Macmillan Company.

Nettle, D., Z. Harper, A. Kidson, R. Stone, I.S. Penton-Voak, & M. Bateson (2013). The watching eyes effect in the Dictator Game: It's not how much you give, it's being seen to give something. *Evolution and Human Behavior, 34(1),* 35-40.

Oprea, R., Charness, G. & Friedman, D. (2014). Continuous time and communication in a public-goods experiment. *Journal of Economic Behavior & Organization, 108, 212-223.*

Ostrom, E., Gardner, R. & Walker, J. (1994). *Rules, Games and, Common-Pool Resources*. University of Michigan Press, Ann Arbor.

Rabin, M. (1994). A model of pre-game communication. *Journal of Economic Theory, 63(2)*, 370-391.

Raihani, N. J., & Bshary, R. (2012). A positive effect of flowers rather than eye images in a large-scale, cross-cultural dictator game. *Proceedings of the Royal Society B: Biological Sciences, 279(1742)*, 3556 – 3564.

Rigdon, M., Ishii, K., Watabe, M., & Kitayama, S. (2009). Minimal social cues in the dictator game. *Journal of Economic Psychology*, *30(3)*, 358-367.

Read, D. (2005). Monetary incentives, what are they good for? *Journal of Economic Methodology*,12(2), 265-276.

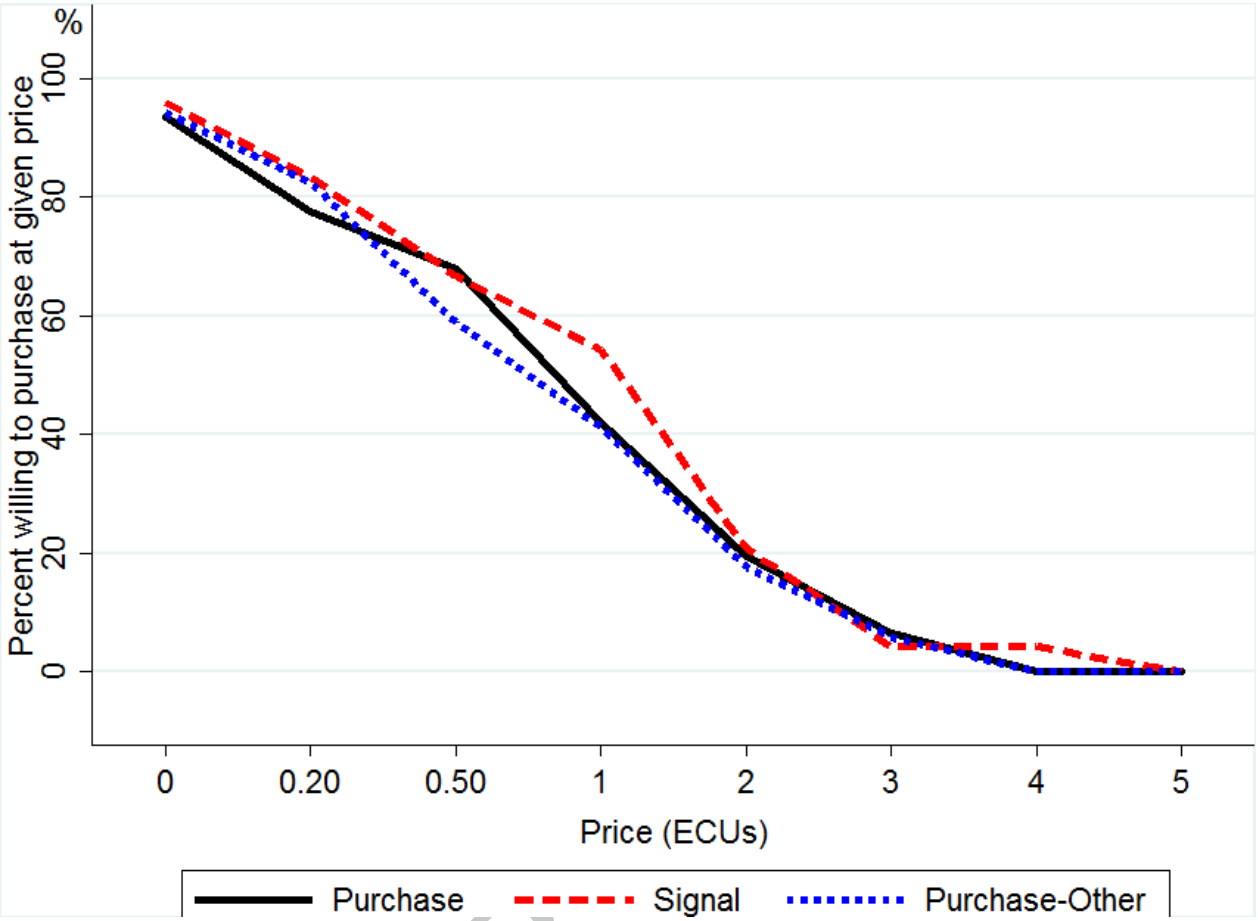
Roth, A. E., Prasnikar, V., Okuno-Fujiwara, M., & Zamir, S. (1991). Bargaining and market behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An experimental study. *American Economic Review, 81(5)*, 1068-1095.

Sherif, M. (1958). Superordinate Goals in the Reduction of Intergroup Conflict. *The American Journal of Sociology*, 63(4), 349-356.

Tajfel, H., & Turner, J. C. (1979). An integrative theory of intergroup conflict. *The Social Psychology of Intergroup Relations, 33(47)*, 74.

Turner, J. C., Brown, R. J., & Tajfel, H. (1979). Social comparison and group interest in ingroup favouritism. *European Journal of Social Psychology*, *9*(2), 187-204.

Zizzo, D.J. (2010). Experimenter demand effects in economic experiments. *Experimental Economics*, 13(1), 75-98.



Highlights:

- We investigate how transparency influences preferences over distributional outcomes.
- We enable third-parties to observe negotiations between two other individuals.
- Third-party surveillance does not affect outcomes in a three-person ultimatum game.
- Individuals may over-estimate the effect of transparent decision-making on economic outcomes.