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Let me see you!: video experiment on the social dimension of risk preferences

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Let Me See You! A Video Experiment on the Social Dimension of Risk Preferences

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Abstract

Previous studies have shown that decision makers are less other-regarding when their own payoff is risky than when it is sure. Empirical observations also indicate that people care more about identifiable than unidentifiable others. In this paper, we report on an experiment designed to explore whether rendering the other identifiable – via a short speechless video – can affect the relation between other-regarding concerns and attitudes toward social risk. For this sake, we elicit risk attitudes under two treatments differing in whether the actor can see the other or not. We find that seeing the other does not affect behavior significantly: regardless of the treatment, individuals are mainly self-oriented as to social allocation of risk, though they are other-regarding with respect to expected payoff levels.

JEL-classification: C90, D63, D81

Keywords Risk attitudes, other-regarding concerns, identifiability

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1 Introduction

There is ample empirical evidence that people exhibit other-regarding preferences when their action affects both their own and other individuals' payoffs. However, the experimental study of Brennan, González, Güth, & Levati (forthcoming) shows that when one's own and/or another person's payoff is risky, other-regarding individuals' behavior is influenced by the riskiness of own payoff but not by that of the other's payoff. This finding is confirmed by Güth, Levati, & Ploner (2005), who extended Brennan et al.'s experiment so as to include deferred outcomes. According to Güth et al.'s data, even though decision makers are other-regarding when monetary payoffs are certain and immediate, they disregard the other and focus on their own condition when the latter becomes risky and/or delayed.

In both these previous studies of the relation between other-regarding concerns and attitudes toward own and other's risk, the "other" has no specific identity: she is an unknown individual. In this sense, she can be considered a "statistical" victim and, as such, be contrasted to an "identifiable victim" (Schelling, 1968).¹ While statistical victims are merely represented via numerical indexes, thereby lacking any social closeness, identifiable victims may be associated with oneself and, thus, trigger some kind of lifeboat ethics (Hardin, 1974) or what has come to be known as the *identifiable victim effect*.

Here, we apply the research design from Brennan et al. (forthcoming), but render "the other" an identifiable victim by giving her a specific identity. Will Brennan et al.'s results of no relation between other-regarding concerns and risk attitudes endure if the decision maker can see the other? Or will the decision maker weigh the risk borne by another person similarly to the risk borne by herself when the other becomes identifiable?

¹The way in which Brennan et al. (forthcoming) capture other-regarding concerns can be compared to the one by dictator experiments. The "other" is, indeed, a mere recipient whose monetary payoff hinges on the decision maker's actions. Since the other can be harmed by an act of the decision maker, she can be viewed as a "victim". For a similar interpretation of the recipient in dictator games, see Bohnet & Frey (1999).

The relevance of the partner's representation has firstly been acknowledged by Schelling (1968). Casual empirical observations support the idea that people care more about identifiable than statistical victims. For instance, in order to collect funds, charities and other humanitarian organizations often resort to vivid representations of needy people (like, e.g., short videos or pictures) and only seldom rely on relevant statistics alone.

Although casual empiricism seems to support Schelling's intuition that identifiable victims evoke sympathy and a sense of moral responsibility that is lacking in considerations of statistical victims, until recently there was very little positive evidence for such an effect. In the socio-psychological literature, for instance, Jenni & Loewenstein (1997) detected no effect of the "vividness" with which a victim is described on subjects' desire to save that victim. In the economic literature, Bohnet & Frey (1999) found for dictator games that pure one-way identification (only the dictator can visualize the recipient) does not yield significant differences in mean offer rates when compared with an anonymous baseline condition, though two-way identification (the dictator and the recipient look at each other for a couple of seconds) and one-way identification with information (the recipient can report some personal information) elicit a more generous behavior than the baseline. Similarly, Brosig, Ockenfels, & Weimann (2003) showed that mere identification of others via video screen does not enhance contributions in 4-person public goods experiments.

According to Small & Loewenstein (2003), demonstrating the identifiable victim effect proved difficult because identifying a victim generally means providing information about her and, thus, any observed increment in empathy toward identifiable victims may be due to the information provided rather than to identifiability *per se*. Small & Loewenstein (2003) circumvented this problem, and provided the first unconfounded empirical demonstration of the effect, by showing that simply indicating that there is a specific victim, without providing any personal information, increases caring.

In this paper, we report on an experiment designed to explore whether and to what extent a weak form of identifiability (or “determinateness”, as named by Small & Loewenstein, 2003), namely presenting a short speechless video of the other to the decision maker, affects the relation between other-regarding concerns and attitudes toward own and other’s risk. Following Brennan et al. (forthcoming), we ask the decision maker to evaluate four different allocations, each of which assigns a risky or certain payoff to herself and to another participant. As elicitation procedure we use the incentive compatible random price mechanism introduced by Becker, DeGroot, & Marschak (1964). Given that Brennan et al.’s results reveal a significant difference in individual valuations of risky prospects in the willingness-to-accept (but not in the willingness-to-pay) treatment, we employ only the willingness-to-accept mode. Thus, each participant is endowed with a prospect and asked to state the minimum price at which she is willing to sell it. From joint comparison of the valuations we can infer how much the decision maker cares for the risk borne by the other who is potential object of benevolence. We shall call the other player “dummy”, as she has no strategic influence, and can merely hope that the decision maker may be generous.

Our two treatments include a no-communication baseline, and a video treatment where the dummy is seen, but not heard by the decision maker. Video portraying of people with whom one interacts is particularly relevant in the perspective of new developments in telecommunication technologies allowing for remote transmission of multimedia signals. We caution from the start that our study is exploratory, and that given the lack of theoretical and empirical research in this area, any firm conclusions about the impact of communication media are necessarily subject to further studies.

2 The experiment

To investigate the relation between other-regarding concerns and risky outcomes, we relied on the random price mechanism (Becker et al., 1964) and elicited individual valuations of several prospects. Valuations were defined as certainty equivalents in the form of willingness to accept a randomly fixed amount of money to forego a given prospect. Each prospect allocated payoffs both to the decision maker, X , and to another participant, Y . More specifically, each member of the pair received either a sure payoff u or a lottery ticket U assigning \underline{U} or \bar{U} with $1/2$ probability each. The relation between the different payoffs was given by $0 < \underline{U} < u < \bar{U}$ and $EU = (\underline{U} + \bar{U})/2 = u$.

Denoting by P_{ij} the prospect assigning i to X and j to her passive partner Y , where $i, j = u, U$, we allowed for the following 4 prospects:

P_{uu} : both X and Y got u ,

P_{uU} : X got u and Y got U ,

P_{Uu} : X got U and Y got u ,

P_{UU} : both X and Y got U .

The decision maker was asked to submit a minimum selling price for each prospect, $b(P_{ij})$, where $0 < \underline{b} \leq b(P_{ij}) \leq \bar{b}$. Then a random draw from a uniform distribution determined an offer $p \in [\underline{p}, \bar{p}]$ with $0 \leq \underline{p} \leq \underline{U} < \bar{U} \leq \bar{p}$. If $p \geq b(\cdot)$, X sold the prospect and collected the random price p , while Y received nothing. If $p < b(\cdot)$, X kept the prospect, and she as well as Y obtained a realization of the payoffs specified by the prospect. Riskiness of the final payoff for all possible bids was preserved by imposing $\underline{p} < \underline{b} < \bar{b} < \bar{p}$. Thus, notwithstanding $b(P_{ij}) = \underline{b}$ (or $b(P_{ij}) = \bar{b}$) X could never be sure whether she would keep the prospect or not.

A risk-neutral decision maker who cares only for her own payoff should submit $b(P_{ij}) = u = EU$ for each of the 4 prospects. However, if the decision maker cares for her partner and wants to increase the chances of keeping the

prospect, she should report $b(P_{ij}) > u$. Comparing bids across prospects in each treatment allows us to disentangle attitudes toward one's own risk from attitudes toward another person's risk.

To examine whether and to what extent rendering the passive partner Y an identifiable victim affects the benevolence of X , valuations of the four prospects were collected under two treatments. In the *baseline treatment*, X just submitted her bid. This control treatment is a mere replication of Brennan et al.'s experiment (with slightly different instructions). In the *video-treatment*, before deciding about her bids, X saw a short (about 2 minutes) speechless video of her passive partner, being informed that the video portrayed the subject labelled Y in the instructions (see the appendix for an English translation), and that she herself was not recorded during the experiment. The two treatments were administered in a between-subjects design, i.e., different groups of subjects were used for each treatment.

Method

Participants

The computerized experiment took place at the laboratory of the Max Planck Institute in Jena (Germany). The experiment was programmed using the z-Tree software (Fischbacher, forthcoming). To avoid possible gender discrimination, participants were only females. Overall, forty-eight subjects (matched in 24 pairs) participated in two control sessions and fifty acted as decision makers X in four video sessions. To check for possible idiosyncratic effects of a specific face, four subjects were used as dummies for all sessions of the video-treatment, and each of the fifty decision makers saw only one dummy. In particular, thirteen observations were collected for faces 1 and 2, and twelve for faces 3 and 4. All participants in the control treatment as well as X -participants in the video-treatment were students at the University of Jena. They earned, on average,

€11.87 (including a show-up fee of €4).² To avoid confounding effects due to possible prior personal acquaintance, Y -participants in the video-treatment were young researchers working at the Max Planck Institute and not involved in the present research. Their average earnings were about €28 (with no show-up fee).

Procedures

The video of the four dummies was recorded in a separate session. Upon arrival at the video-laboratory, each dummy was led to one of the sound-proof booths, and received the experimental instructions that she had to read carefully, before answering a control questionnaire. Dummies knew that they would have been recorded while performing these tasks. After answering correctly the control questions, they were informed that they would act in the role of Y and, thus, could collect their (potential) experimental earnings after some days. Figure 1 reports snapshots of the videos employed in the experiment. In the figure, the four faces are blurred because of privacy reasons, but in the original videos they were perfectly visible.

Insert Figure 1 about here

To collect a high number of independent observations per treatment, we used the strategy method for decision makers X . This means that, in both treatments, each X -participant had to submit four reservation prices, one for each prospect. In order to avoid portfolio-diversification effects decision makers were paid according to one choice only. The parameter values were equal to those used by Brennan et al. (forthcoming). In particular, the lower and upper bounds, \underline{p} and \bar{p} , of the uniform distribution from which the random offer prices were selected amounted to 4 and 50 ECU, respectively. Decision makers in either treatment could submit any integer value between 8 and 46 ECU. The prospect's parameters were $u = 27$, $\underline{U} = 16$, and $\bar{U} = 38$.

²The experimental money was the ECU (Experimental Currency Unit) with 10 ECU = €2.50.

3 Experimental results

Table 1 reports descriptive statistics on valuations of each prospect in the two treatments. Data referring to the video-treatment are pooled across the four dummies.

Insert Table 1 about here

Average reservation prices in the two treatments are rather similar: in general, they are above the opportunistic, risk-neutral prediction given by $b(P_{ij}) = 27$. Only for the prospect where one's own payoff is risky, P_{Uu} , median bids do not exceed 27 under both the control- and the video-treatment.³ Pair-wise comparisons of reservation prices within treatment confirm that valuations are significantly different only when introducing risk in X 's own payoff (in the control-treatment, $p = 0.041$ for P_{Uu} vs. P_{uu} , and $p = 0.036$ for P_{Uu} vs. P_{uU} ; in the video-treatment the respective p -values are $p = 0.028$ and $p = 0.034$; two-sided Wilcoxon signed-rank tests).

Although median valuations of each prospect are slightly higher under the video-treatment than under the control, reservation prices do not differ significantly across treatments ($p > 0.1$ for all four comparisons; two-sided Wilcoxon rank-sum test). Moreover, reservation prices for the prospects that are risky for the decision maker tend to exhibit larger variability without video than with it.

The above findings indicate that presenting a video of the passive partner (rendering her an identifiable victim) does not alter the relation between other-regarding concerns and risk attitudes significantly. Regardless of whether Y is seen or not, decision makers X show other-regarding concerns when they can rely on a sure reward, but riskiness of own reward induces a decrease in reservation price.

³It is worthwhile noting that, regardless of the treatment, the prospect with risk for both parties (P_{UU}) is evaluated better than the prospect where only X 's own payoff is risky (P_{Uu}). A possible explanation, which deserves further research, is that people prefer sharing the risk to bearing it alone.

Is there any idiosyncratic effect of a face? In other words, do decision makers react to the person they saw? Figure 2 informs on the distribution of choices for each prospect, separately for each dummy in the video-treatment and for the control.

Insert Figure 2 about here

The valuations for the dummy with face 1 display the highest variability. The dummy with face 2, differently from the other three dummies, triggers mean reservation prices that are always below 30. However, Wilcoxon rank-sum tests (two-sided) indicate that the prospects' valuation does not vary with faces systematically.⁴ Overall, the pattern of choices are similar across seen dummies and treatments, with the prospect allocating a risky payoff to X , but not to Y , receiving the lowest valuations.

Table 2 complements our findings of no treatment effects on reservation prices. It reports the results of a generalized linear random-effects model (based on a Poisson distribution) regressing reservation prices on dichotomous variables. *OwnRisk* takes value 1 for the prospects with risky payoff for X (i.e., P_{Uu} or P_{UU}) and 0 otherwise. *OtherRisk* equals 1 for the prospects involving risk for Y (i.e., $P_{u,U}$ or $P_{U,U}$) and 0 otherwise. Finally, *Video* is 1 for the video-treatment and 0 for the control.

Insert Table 2 about here

The regression analysis confirms that the only factor affecting valuations significantly is the presence of risk in the decision maker's payoff. Decision makers dislike prospects that entail risk for themselves, but do not show any significant reaction to the riskiness of the other's payoff. Moreover, in agreement with our previous analysis, seeing the other does not induce significantly different reservation prices. (the coefficient of the dummy *Video* is not significant).

⁴A slightly significant difference is detected for prospect P_{uU} when comparing face 2 and face 4 ($p = 0.040$). For all other comparisons, we cannot reject the hypothesis that valuations of two faces are the same at the 5%-significance level.

The specification of the model in Table 2 does not contain any term controlling for the four faces. Including the different faces as explanatory variables and considering their interaction with the other independent variables do not change qualitatively the results, and the Akaike Information Criterion increases from 2088 to 2101. This corroborates that no idiosyncratic face effect is present in the data.

4 Conclusion

It is widely believed that people are more generous toward an identifiable (determined) third person than toward a statistical (indeterminate) other (Schelling, 1968). Empirical research investigating the effect of identification on benevolent behavior is mixed. On the one hand, the studies by Fetherstonhaugh, Slovic, Johnson, & Friedrich (1997) and Small & Loewenstein (2003) have provided support for the identifiable victim effect. On the other hand, the results by Jenni & Loewenstein (1997), Bohnet & Frey (1999), and Brosig et al. (2003) indicate that vividness is not sufficient to produce that effect, and that results depend on the specific form of identification.

In this paper, we relied on the experimental setting of Brennan et al. (forthcoming) to investigate whether a weak form of identifiability, i.e., showing a short speechless video of the other, affects the relation between other-regarding concerns and attitudes toward own and other's risk.

Our results suggest that simply watching the other does not influence the decision maker's attitudes toward the risk borne by this third person who is potential object of benevolence. Regardless of whether or not decision makers could see the other before making their decisions, they show other-regarding concerns when their own payoff is sure, but disregard the other and focus on their own condition when the latter becomes risky.

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Appendix: Translated instructions

In this appendix we report the instructions for the video-treatment. The instructions for the control were adapted accordingly.

Welcome and thanks for participating in this experiment. You receive €4.00 for having shown up on time. Please read the following instructions carefully. From now on any communication with other participants is forbidden. If you have any questions, please raise your hand. We will answer your questions individually. The unit of experimental money will be the ECU (Experimental Currency Unit), where 1 ECU = €0.25.

In this experiment, participants are randomly divided into pairs. In each two-person group, there will be one person of type X and one person of type Y . You will learn your type at the beginning of the experiment.

Each member of a pair will face 4 different prospects, each of which pays to X and Y some positive amounts of ECU. These payments can be either *certain* or *uncertain*. The certain payment gives 27 ECU for sure. The uncertain payment consists of a *lottery* giving either 16 ECU or 38 ECU, where both amounts are equally likely.

The 4 prospects that X and Y will face are the following:

1. X gets 27 ECU for sure, and Y gets the lottery.
2. X gets the lottery, and Y gets 27 ECU for sure.
3. Both X and Y get 27 ECU for sure.
4. Both X and Y get the lottery.

Please, note that in the experiment the order of the four prospects can be different from that given above.

X -participants' task

X 's task is to report the lowest amount of ECU for which she would be willing to sell each prospect. In other words, X has to state a minimum selling price for each of the four prospects. Each of X 's four choices must be not smaller than 8 ECU and not greater than 46 ECU. Furthermore, it must be an integer number (i.e., 8, 9, 10, ..., 44, 45, 46).

Y -participants' task

Y will not have any choice to make. She will be a "non-active" member of the pair,

whose payoff merely hinges on X 's decisions about the prospect.

The payoffs of both types will depend on the choices made by X , and on two random choices made by the computer. These random choices determine the “relevant” prospect, and an “integer” between 4 and 50.

More specifically, payoffs are determined as follows.

1. Once the X -member of the pair has made her choices, the computer will select one of the four prospects faced by this player as the “relevant prospect”, where all four prospects are equally likely.
2. The computer will randomly choose an “integer” between 4 and 50. You can think of this choice as drawing a ball from a bingo cage containing 47 balls numbered 4, 5, . . . , 50. Any number between 4 and 50 is equally likely.

The final payoff of X and Y is computed by comparing this random integer to the minimum selling price reported by X for the relevant prospect.

- If the random integer is smaller than the minimum selling price reported by X for the relevant prospect, X keeps the relevant prospect and both X and Y obtain the payments specified by it.
- If the random integer is equal to or greater than the minimum selling price reported by X for the relevant prospect, X sells the relevant prospect and earns an amount of ECU equal to the random integer. In this case, Y earns nothing.

EXAMPLE:

Suppose that the prospect paying to X 27 ECU for sure and to Y either 16 or 38 ECU is the relevant prospect, and that X has reported a minimum selling price of 20 ECU for that particular prospect.

- If the computer chooses the integer 18, X keeps the prospect (because $18 < 20$). This implies that X earns 27 ECU, and Y obtains either 16 or 38 ECU, where these two amounts are equally likely.
- If the computer chooses the integer 22, X sells the prospect (because $22 > 20$). This implies that X earns 22 ECU, and Y earns nothing.

Before the experiment starts, you will have to answer some control questions to verify your understanding of the rules of the experiment.

Please remain quiet until the experiment starts and switch off your mobile phone. If

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you have any questions, please raise your hand now.

Transcript of the message that X saw on her screen before starting the experiment

You are participant X .

You will now see a two-minute video-clip of participant Y in your pair. This video-clip was recorded earlier when Y participated in the same experiment as you. Y has not received any payment yet. The amount Y will earn depends on your decision. You will not be filmed during this experiment.

Table 1: Descriptive statistics on reservation prices

| Treatment | N^a | Prospect | Mean | Median | Std. dev. |
|----------------|-------|-----------|-------|--------|-----------|
| <i>Control</i> | 24 | <i>uu</i> | 33.38 | 30.00 | 8.14 |
| | | <i>uU</i> | 32.33 | 30.00 | 7.19 |
| | | <i>Uu</i> | 26.75 | 25.00 | 10.77 |
| | | <i>UU</i> | 28.92 | 28.50 | 10.25 |
| <i>Video</i> | 50 | <i>uu</i> | 32.10 | 32.50 | 8.52 |
| | | <i>uU</i> | 31.32 | 32.00 | 7.80 |
| | | <i>Uu</i> | 28.70 | 27.00 | 8.61 |
| | | <i>UU</i> | 30.32 | 30.00 | 9.01 |

^a N denotes the number of observations.

Table 2: Generalized linear mixed-effects regression on reservation prices

| | Coefficient | Std. Error |
|--|-------------|------------|
| Intercept | 3.449*** | 0.054 |
| <i>OwnRisk</i> | -0.112*** | 0.036 |
| <i>OtherRisk</i> | -0.024 | 0.035 |
| <i>Video</i> | 0.041 | 0.067 |
| <i>OwnRisk</i> × <i>OtherRisk</i> | 0.079 | 0.051 |
| <i>OwnRisk</i> × <i>Video</i> | -0.109 | 0.064 |
| <i>OtherRisk</i> × <i>Video</i> | -0.007 | 0.062 |
| <i>OwnRisk</i> × <i>OtherRisk</i> × <i>Video</i> | 0.030 | 0.090 |

*** = 1% significance level



Face 1



Face 2

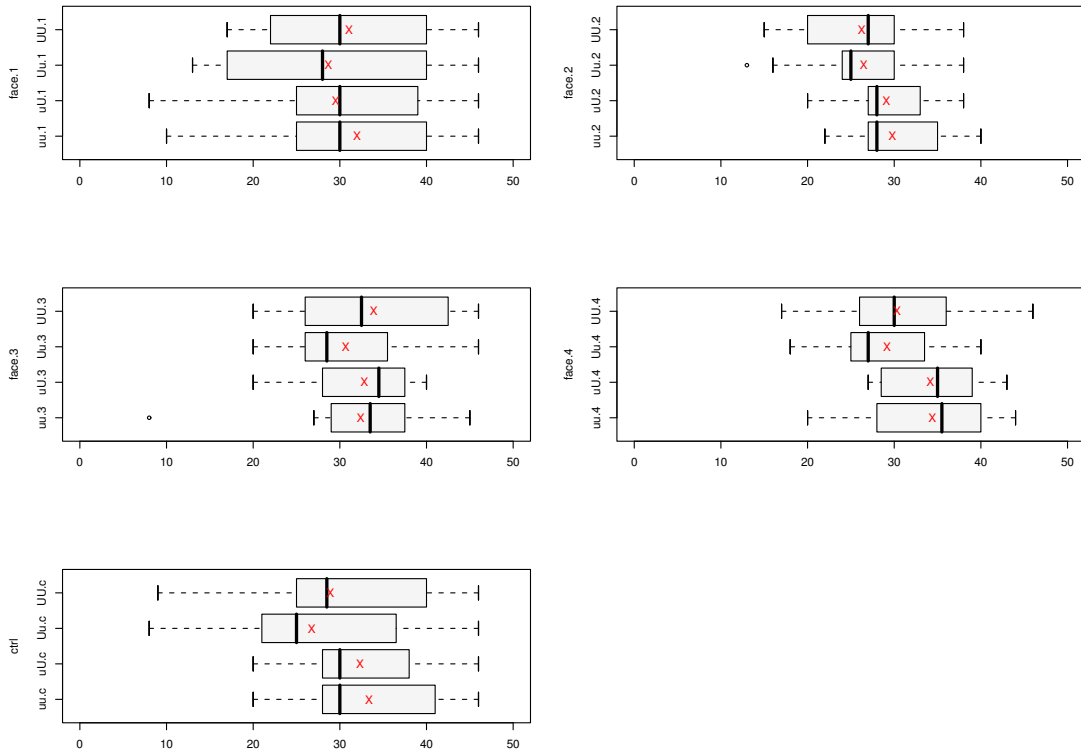


Face 3



Face 4

Figure 1: Snapshots of the four videos employed in the experiment



Note: × denotes the mean of the distribution.

Figure 2: Box plots of the distribution of reservation price in each prospect, separately for the 4 dummies in the video-treatment and for the control