

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

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Abstract Previous studies have shown that individuals 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. We report on an experiment designed to explore whether rendering the other identifiable—via a speechless video—can affect the relation between other-regarding concerns and preferences over 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 self-oriented as to allocation of risk, though they are other-regarding with respect to expected payoffs.

Keywords Risk attitudes, other-regarding concerns, identifiability

JEL classification C90, D63, D81

1. Introduction

There exists a substantial body of evidence showing that people exhibit other-regarding preferences when their own payoff is certain. Yet, in real life, how one's own choices affect oneself and others is hardly ever deterministic. Think, for instance, of classic public good problems like waste recycling and climate change or of situations where people share a common fate (like in joint ventures) but there exists the possibility to “ostracize” someone. The question of whether—and to what extent—people care for others in situations with risk has been addressed by Brennan et al. (2008), who find that 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 et al. (2008), who extended Brennan et al.'s experiment so as to include deferred outcomes. According to Güth et al.'s data study, 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. This result is attributed to cognitive crowding-out in the sense that “own risky and delayed decision

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problems tend to crowd out concerns toward others' problems possibly due to some cognitive and emotional overload" (Güth et al. 2008, p. 270).

In both these previous studies, the "other" has no specific identity: she is an unknown individual. Here, we apply Brennan et al.'s (2008) research design, but render "the other" identifiable by presenting a short speechless video of her to the decision maker. Seeing the other may induce the decision maker to adopt the other's perspective. This is likely to evoke more empathy,¹ and to increase the willingness to make sacrifices and to provide help. Moreover, identification of the other may help reduce *social distance* and encourage some level of social responsibility (see, e.g., Andreoni and Petrie, 2004, and references therein).² The greater empathic response to an identifiable third person and/or the lower social distance may, thus, help overturn Brennan et al.'s findings of no relation between other-regarding concerns and attitudes toward social risk.

Casual empirical observations support the idea that people care more about identifiable than unidentifiable others. Although cautious estimates suggest that a speed limit of 130 km/h on German highways would save at least 1000 lives per year (Gohlisch and Malow 1999), Germany has not yet introduced speed limits, providing evidence for how reckless individuals are when victims are anonymous. On the other hand, the amount of money and effort Americans spent for saving the 18-month-old Jessica McClure ("Baby Jessica"), who fell down a narrow well in Texas in 1987 and whose face was constantly shown on every news channel, shows how people can become very emotional when the victim is identifiable. Therefore, when collecting funds, charities very often resort to vivid images of needy individuals rather than providing just statistics, although the latter would describe necessities more adequately.

Numerous experiments in the behavioral decision making literature have tested Schelling's (1968) intuition that identified targets evoke a stronger emotional and moral reaction than unidentified targets, thereby boosting benevolent behavior. For instance, Kogut and Ritov (2005) find that a single identified individual (identified by a name and a picture) triggers more donations than a group of eight identified individuals, and more than both a single and a group of unidentified individuals. In a field study, Small et al. (2007) gave people the opportunity to contribute up to \$5 of their earnings to the charity "Save the Children". Donations could go either to a specific 7-year old girl from Mali named Rokia or to unspecified needy individuals in Southern Africa and Ethiopia. Contributions to Rokia were far greater than contributions to unidentified individuals, even though contributions to Rokia decreased significantly when her story was presented together with statistical information.

The impact of non-anonymity on benevolent behavior has also been addressed by experimental economists. Bohnet and Frey (1999a, 1999b) used prisoner's dilemma and dictator games to study whether "silent identification suffices to induce a larger degree of solidarity than anonymous conditions" (Bohnet and Frey 1999b, p. 46). They

¹ Davis (1994) and Batson et al. (1995), among others, view the adoption of the other's perspective as a necessary condition for empathy to emerge.

² By "social distance" we mean the emotional proximity which ties oneself to the other. The concept of social distance has a long history in the social science literature (Bogardus 1928) where it is presumed to have many different aspects. Economists, however, have mainly focused on one particular feature: anonymity.

find that two-way identification (the group members stand up and look at each other in silence) increases dictator giving and increases cooperation in a four-person prisoner's dilemma. Moreover, in the dictator game, one-way identification (only the dictators see the recipient) induces more other-regarding behavior than anonymity. Similarly, Burnham (2003) reports that dictators seeing a picture of their recipients more likely divide the money equally than dictators who do not know their counterpart. Charness and Gneezy (2008) find that dictators (but not proposers in ultimatum games) are more generous when they know the family name of their counterpart.

The main goal of the present study is to explore whether and how reducing anonymity influences other-regarding behavior not only when one's own and others' payoffs are sure (which is common) but also when they are risky.³ To shed light on this issue, we run two variants of Brennan et al.'s (2008) experiment. In Brennan et al., the decision maker is asked to evaluate four different allocations, each of which assigns a risky or certain payoff to herself and to another participant. Our no-communication control treatment replicates Brennan et al.'s study. Our experimental treatment, labeled "video treatment", has each decision maker seeing a two-minute silent video of the other before deciding. As compared to a still picture, a speechless video renders identification more profound and reliable. Under both treatments, the elicitation procedure is the incentive compatible random price mechanism introduced by Becker et al. (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, in both treatments, each participant is endowed with a prospect and asked to state the minimum price at which she is willing to sell it.⁴ By comparing valuations within each treatment we can infer how much the decision maker cares for the risk borne by the other. By the same token, comparison of valuations across treatments should reveal whether relaxing anonymity yields other-regarding individuals to weigh the risk borne by the other similarly to the risk borne by themselves.

Before discussing the experiment and the results in more detail, let us comment on the issue of possible deception in our design. The problem is that we do not inform the subjects that we use the same identifiable passive other for several decision makers. By this means, we can control for idiosyncratic effects of a specific face without altering the decision makers' intrinsic preferences to the risk borne by the other. According to an anonymous referee, this procedure may imply that we lied to the decision makers insofar as they assumed that all subjects were recruited in the same way. Specifically, the

³ A study verging on ours is Hsee and Weber (1997), who confronted subjects with a series of risky choices in order to explore whether people can accurately predict others' risk preferences both when the others are anonymous students and when the other is vivid and concrete (the person sitting nearby). They find a self-other discrepancy only if the target of prediction is abstract, but not if it is vivid. Note, however, that Hsee and Weber are not interested in how risk preferences are related to other-regarding concerns or in how attitudes toward own and others' risk interact, which are crucial aspects of our design.

⁴ Although one may argue that a comprehensive study should have elicited the willingness to pay as well, we are interested more in the differences among individual valuations of the several prospects than in absolute valuations of each prospect. Thus, we do not check whether our findings are robust with respect to the method of eliciting certainty equivalents. Samuelson and Zeckhauser (1988) and Tietz (1992) provide experimental evidence on the endowment effect.

referee questions the following passage of the instructions “*participants are randomly divided into pairs. In each pair, there will be one person of type X and one person of type Y.*” Note that this sentence does not state that appointment to the roles is random. It only specifies that the division into pairs is random. As seats were assigned in a random way, the sentence is definitely true. Furthermore, the sentence contains no indication of the procedure used to recruit participants. In our view, experimentalists cannot avoid possible misinterpretations by participants. Thus, the norm of experimenter’s honesty can only mean that whatever is said in the instructions corresponds to the truth. As emphasized by Hertwig and Ortmann (2008, p. 62), “a consensus has emerged across disciplinary borders that intentional provision of misinformation is deception and that withholding information about research hypotheses, the range of experimental manipulations, or the like ought not to count as deception.”

The rest of the paper is organized as follows. In Section 2, the different prospects, the various treatments, and the experimental procedures are described in detail. The experimental results are reported in Section 3. Section 4 discusses the results and concludes.

2. The experiment

2.1 Decision task and treatments

To investigate the relation between other-regarding concerns and risky outcomes, we rely on the random price mechanism (Becker et al. 1964) and elicit individual valuations of several prospects. Valuations are defined as certainty equivalents in the form of willingness to accept a randomly fixed amount of money to forgo a given prospect. Each prospect allocates payoffs both to the decision maker, X , and to another participant, Y . More specifically, each member of the pair receives 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 is given by $0 < \underline{U} < u < \bar{U}$ and $E[U] = (\underline{U} + \bar{U})/2 = u$.

Let P_{ij} denote the prospect assigning reward i to X and reward j to her passive partner Y . We allow for the following four prospects:

- P_{uu} : both X and Y get u , i.e., the sure payoff;
- P_{uU} : X gets u and Y gets U , i.e., the decision maker gets the sure payoff and her partner faces the lottery;
- P_{Uu} : X gets U and Y gets u , i.e., the decision maker faces the lottery and her partner the sure payoff;
- P_{UU} : both X and Y get U , i.e., the lottery.

The decision maker is 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 determines 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 sells the prospect and collects the random price p , while Y receives nothing. If $p < b(\cdot)$, X keeps the prospect and she, as well as Y , obtains a realization of the payoffs specified by the prospect. Riskiness of the final payoff for all possible bids is 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 can

never be sure whether she would keep the prospect or not. In what follows, we shall call the passive player “dummy”, as she has no strategic influence, and can merely hope for the decision maker’s generosity.

A risk-neutral decision maker who cares only for her own payoff should submit $b(P_{ij}) = u = E[U]$ for each prospect. However, if the decision maker cares for the dummy 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. In particular, we can assess preferences over individual risk by comparing bids for the prospects where own payoff is risky to bids for the prospects where it is not (i.e., evaluating how $b(P_{uu})$ and $b(P_{uU})$ are in relation to $b(P_{Uu})$ and $b(P_{UU})$, respectively). Similarly, we can assess preferences over the other’s risk by comparing $b(P_{uu})$ to $b(P_{uU})$ and $b(P_{Uu})$ to $b(P_{UU})$.

To examine whether and to what extent rendering the dummy Y identifiable affects the benevolence of decision maker X , valuations of the four prospects are collected under two treatments. In the *control treatment*, X is informed that she is matched with a participant of type Y , whose identity she will never know. This control treatment is a replication of Brennan et al.’s (2008) experiment with slightly different instructions (see the appendix for an English translation). In the *video treatment*, X watches a short (about 2 minutes) speechless video of her passive partner prior to her decisions. X is informed that the video portrays the subject labeled Y in the instructions, and that she herself is not recorded during the experiment. The two treatments are administered in a between-subjects design, i.e., different groups of subjects are used for each treatment.

2.2 Procedures

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 2007). To avoid possible gender discrimination, participants were (aware to be) only females.⁵ After being randomly seated at a computer terminal, participants received written instructions. Understanding of the rules was checked by a control questionnaire that subjects had to answer before the experiment started. The instructions were the same for all participants (regardless of their role) and for both treatments. X -participants in the video treatment learnt that they would have been presented with a video of her counterpart only after having answered the control questions.

Overall, forty-eight subjects participated in two control sessions (24 were assigned to the role of X and 24 to the role of Y), 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. 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. We did not reveal to the decision makers in the video treatment that the dummy she was confronted with could collect money from several X -participants. We do not think that this feature of our

⁵ Some previous experiments, such as Eckel and Wilson (2003) and Meier (2007), have shown that gender can change the nature of other-regarding behavior.

design violates the norm of experimenter's honesty for two reasons. First, we did not lie to the participants. Second, the honesty norm does not entail saying everything to the participants. On the contrary, if an aspect of the design is deemed as important (here, controlling for the idiosyncratic effects of a specific face) but its disclosure to the participants might affect their behavior,⁶ then it is tolerable and even recommended not to inform participants of this aspect (see, e.g., Ellingsen et al. 2010).

All participants in the control treatment as well as X -participants in the video treatment were female students at the University of Jena. They earned, on average, €11.87 (including a show-up fee of €4).⁷ To avoid confounding effects due to possible prior personal acquaintance, the four dummies in the video treatment were female young researchers (all below 28 years old) working at the Max Planck Institute, rather than self-recruited participants. Their average earnings were about €28 (with no show-up fee).

The video of the four dummies was recorded in a separate session.⁸ Upon arrival at the video laboratory, each dummy was led to one of the sound-proof booths and received the experimental instructions. Dummies knew that they would have been recorded while performing their task. After answering correctly the control questions, they were informed that they would act in the role of Y and 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.

To collect a large 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. Like in Brennan et al. (2008), choices were elicited in a random order to exclude ordering effects. 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. (2008). 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 $\bar{U} = 38$.

3. Experimental results

Table 1 reports descriptive statistics on valuations of each prospect in the two treatments.⁹ For the reader's convenience, we have included in the bottom panel of Table 1

⁶ Telling X -participants in the video treatment that Y could receive money from more decision makers would confound the effect of seeing Y with a possibly lower willingness to keep the prospect when each Y can collect payments from many others.

⁷ The experimental money was the ECU (Experimental Currency Unit) with 10 ECU = €2.50.

⁸ The decision makers were informed that the videos of their passive partner had been recorded earlier and that Y had not yet received any payment. It is rather unlikely that the decision makers knew the young researchers in the role of Y because the Max Planck Institute is not part of the University of Jena.

⁹ Here, we pool the data from the video treatment across the four dummies in order to study the pure effects of identification. Later on, we will show that pooling is justified.

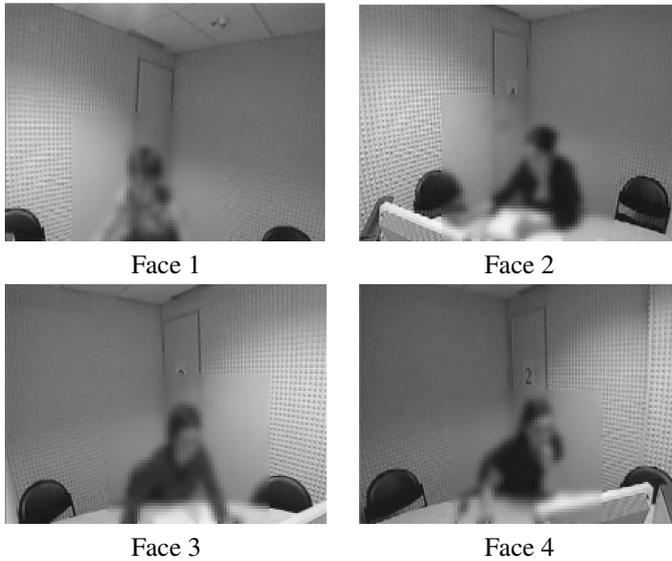


Figure 1. Snapshots of the four videos employed in the experiment

the results of Brennan et al. (2008). Wilcoxon rank-sum tests fail to reject the null hypothesis of equality between our control treatment and Brennan et al.'s WTA-treatment ($p > 0.113$ for all four comparisons).

Table 1. Descriptive statistics on reservation prices

Treatment	N	Prospect	Mean	Median	S.D.
Control	24	P_{uu}	33.38	30.00	8.14
		P_{uU}	32.33	30.00	7.19
		P_{Uu}	26.75	25.00	10.77
		P_{UU}	28.92	28.50	10.25
Video	50	P_{uu}	32.10	32.50	8.52
		P_{uU}	31.32	32.00	7.80
		P_{Uu}	28.70	27.00	8.61
		P_{UU}	30.32	30.00	9.01
Brennan et al. (2008)	30	P_{uu}	30.80	30.00	7.71
		P_{uU}	29.13	27.50	8.95
		P_{Uu}	27.10	25.00	10.74
		P_{UU}	27.63	25.50	11.16

Notes: N denotes the number of observations.

Contrary to our conjecture that a vivid other, by stimulating an emphatic response, would raise the willingness to keep the prospect, average reservation prices in the two

treatments are rather similar. In general, average reservation prices are above the opportunistic, risk-neutral prediction given by $b(P_{ij}) = 27$, indicating that decision makers, in principle, care for the other. 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. Own risk aversion, as revealed by $P_{Uu} < P_{uu}$ or $P_{UU} < P_{uU}$, seems to dominate the concern for the other.

A further insight into the impact of own and other's risk on prospects' valuations comes from the comparisons shown in Table 2. The net effect of risk on reservation prices is generally negative for both the control and the video treatment. The only exception is represented by the prospect P_{UU} , which is evaluated more than P_{Uu} on average. Moreover, Table 2 makes evident that the average distances are smaller in the video treatment than in the control. Yet, a series of Wilcoxon rank-sum tests does not identify any significant difference between the two treatments ($p \geq 0.441$ always).

Table 2. Average effects of own risk and other's risk on reservation prices

Treatment	Impact of own risk		Impact of other's risk	
	$P_{uu} - P_{Uu}$	$P_{uU} - P_{UU}$	$P_{uu} - P_{uU}$	$P_{Uu} - P_{UU}$
Control	6.625	3.417	1.042	-2.167
Video	3.400	1.000	0.788	-1.620

Pairwise 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, $p = 0.038$ for P_{Uu} vs. P_{uu} , $p = 0.037$ for P_{Uu} vs. P_{uU} , and $p = 0.035$ for P_{UU} vs. P_{uu} ; in the video treatment the respective p -values are $p = 0.028$, $p = 0.034$, and $p = 0.063$; two-sided Wilcoxon signed-rank tests).¹⁰ Using a Kruskal-Wallis rank sum test to compare all four prospects simultaneously, we can reject the null hypothesis of no difference in prospects' valuations for the control treatment ($p = 0.037$), but not for the video treatment ($p = 0.101$). Thus, reservation prices for the four prospects are more similar when the dummy is no longer faceless. 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 ($p > 0.10$ 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 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.

¹⁰ It 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}), even though the difference in valuations is not statistically significant ($p > 0.05$ in both cases; Wilcoxon signed-rank test).

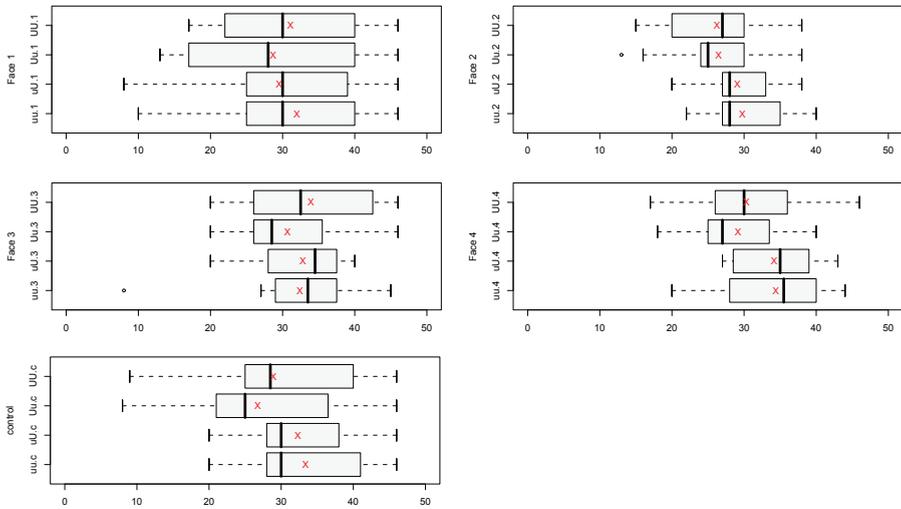


Figure 2. Box plots of the distribution of reservation price in each prospect, separately for the 4 dummies in the video treatment and for the control

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.

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.¹¹ 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 3 complements our findings. It reports the results of a Poisson generalized linear mixed model with individual reservation prices as dependent variable and dichotomous variables as independent variables. *OwnRisk* takes value 1 for the prospects with risky payoff for X (i.e., P_{Uu} or P_{UU}) and 0 otherwise, thus capturing the effect of risk in one's own payoff on valuation of the prospects. *OtherRisk* equals 1 for the prospects involving risk for Y (i.e., P_{uU} or P_{UU}) and 0 otherwise; therefore it measures the impact that risk in other's payoff has on valuations. Finally, *Video* is 1 for the video treatment and 0 for the control. The explanatory variable of major interest to our research is the interaction of *Video* with *OtherRisk* because it allows assessing whether rendering the other visible encourages prosocial attitudes or not.

¹¹ A 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. Kruskal-Wallis rank sum tests (one for each prospect) confirm that valuations of the same prospect do not differ across different faces ($p > 0.10$ for all four comparisons).

Table 3. Generalized linear mixed-effects regression on reservation prices

	Coefficient	S.E.
Intercept	3.482***	0.054
<i>OwnRisk</i>	-0.211***	0.043
<i>OtherRisk</i>	-0.022	0.042
<i>Video</i>	-0.032	0.065
<i>OwnRisk</i> × <i>Video</i>	0.094**	0.045
<i>OtherRisk</i> × <i>Video</i>	-0.007	0.045
<i>OwnRisk</i> × <i>OtherRisk</i>	0.089**	0.042

Notes: *** = 1% significance level; ** = 5% significance level.

The regression analysis confirms that the presence of risk in the decision maker's payoff significantly affects prospects' valuations. 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). A positive and significant effect is observed for the interaction terms *OwnRisk* × *Video* and *OwnRisk* × *OtherRisk*, meaning that the decision makers' dislike of their own risk is less pronounced when they can see a video of the other and when the other is facing risk. This corroborates the pattern emerging from the comparisons in Table 2.

The specification of the model in Table 3 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 slightly worsen the goodness of fit of the model (the Akaike Information Criterion increases from 543.9 to 552.3). This corroborates that no idiosyncratic face effect is present in the data.

4. Discussion

Casual observation and systematic research indicate that people are more benevolent toward an identifiable third person than toward an unidentifiable other. In particular, previous experimental studies have shown that relaxing anonymity while still forbidding dialogue influences people's behavior (see, e.g., Bohnet and Frey 1999a, 1999b; Burnham 2003). Using the experimental setting of Brennan et al. (2008), we have investigated whether being exposed to a silent video of the other affects the interrelation of other-regarding concerns and attitudes toward own and other's risk. As seeing the other renders her 'vivid' and as 'vividness' should evoke empathic responses, we expected decision makers in our video treatment to have attitudes to risk faced by the other similar to those they exhibit to risk they themselves face.

Our results, however, reveal that watching the other does not influence the decision maker's attitudes toward the risk borne by this third person. Regardless of whether or not decision makers can see the other prior to their decisions, they are self-centered as

to the allocation of risk and other-regarding only when their own payoff is sure. Thus, we confirm that while social preferences are behaviorally relevant in a world with commonly known material payoffs, risky payoffs to oneself tend to mitigate other-regarding concerns.¹²

A simple justification for the more selfish behavior exhibited in case of risky prospects is aversion to own riskiness. A further plausible explanation draws on G uth et al.'s (2008) conjecture and suggests that dealing with probabilities requires more cognitive thoughts which may lessen empathy even toward identifiable targets. Dual-process models in social cognition suggest that people use two modes of thinking and deciding: one deliberative and calculative and the other affective (e.g., Epstein 1994; Kahneman and Frederick 2002). Although the affective mode may dominate when the target of thought is specific and vivid (Sherman et al. 1999), previous research reveals that affective responses can be weakened by inducing people to think deliberatively (Wilson et al. 2000; Small et al. 2007). Handling stochastic prospects may lead to a more extensive use of the deliberative system. In this sense, our probabilistic decision task might have reduced decision makers' inclination to kindness.

Our data reveal that the prospect allocating a risky payoff to the decision maker receives higher valuations when also the payoff to the other is risky than when it is not. This result may be caused either by the decision maker's aversion to getting a worse deal than the other (as suggested by one of our anonymous referees) or by a reduction in the distaste for risk when risk is borne also by the other in the reference group. Our design does not allow us to reach definite conclusions on this issue, but the experimental evidence garnered here represents a stimulus for further targeted research.

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¹² Levati et al. (2009) reached a similar conclusion regarding the interaction of risk and other-regarding concerns, but in a very different context. In their paper, introducing risky payoffs and imperfect information in linear public goods games decreases significantly initial contributions.

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A. Appendix: Translated instructions

A.1 General instructions (common to all treatments)

Welcome and thanks for participating in this experiment. You will 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 pair, 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, ..., 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.

Calculation of payoffs

The payoffs of both types will depend on the choices made by X , and on two random choices made by the computer. More specifically, once the X -member of the pair has made her choices, the computer will select

- (i) one of the four prospects as the "relevant prospect", where all four prospects are equally likely;

- (ii) a “random integer” between 4 and 50, where all numbers from 4 to 50 are equally likely. You can think of this choice as drawing a ball from a bingo cage containing 47 balls numbered 4, 5, . . . , 50.

The final payoffs of X and Y are computed by comparing this “random integer” to the minimum selling price reported by X for the “relevant prospect”.

- (i) 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.
- (ii) 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.

- (i) 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.
- (ii) 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 you have any questions, please raise your hand now.

A.2 Transcript of the message that X saw on her screen before starting the experiment in the video treatment

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.