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Working Paper

Social communication and discrimination: A video experiment

Jena Economic Research Papers, No. 2010,038

Provided in Cooperation with:

Max Planck Institute of Economics

Suggested Citation: Greiner, Ben; Güth, Werner; Zultan, Ro'i (2010) : Social communication and discrimination: A video experiment, Jena Economic Research Papers, No. 2010,038, Friedrich Schiller University Jena and Max Planck Institute of Economics, Jena

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JENA ECONOMIC RESEARCH PAPERS



2010 – 038

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by

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www.jenecon.de

ISSN 1864-7057

The JENA ECONOMIC RESEARCH PAPERS is a joint publication of the Friedrich Schiller University and the Max Planck Institute of Economics, Jena, Germany. For editorial correspondence please contact markus.pasche@uni-jena.de.

Impressum:

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D-07743 Jena
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SOCIAL COMMUNICATION AND DISCRIMINATION:
A VIDEO EXPERIMENT*

BEN GREINER,[†] WERNER GÜTH,[‡] AND RO'I ZULTAN[§]

22 JUNE 2010

ABSTRACT

We report on an experiment using video technology to manipulate pre-play communication protocols in the lab and to study purely social effects of communication on donations and discrimination between potential receivers. The experimental design eliminates strategic factors by allowing two receivers to unilaterally communicate with an anonymous dictator before the latter decides on her gifts. Through the use of three communication setups (none, audio, and audio-visual) we show and analyze the existence of purely social effects of communication. We find that a silent channel leads to discrimination between potential receivers based on impression formation, but does not affect average levels of donations. When the auditory channel is added, average donations increase. The social processes invoked are heterogeneous and communicator-specific but not unsystematic.

Keywords: bargaining, communication, discrimination,
n-person dictator game, video experiment

JEL Classification: C72, C91, D64

*An earlier version of this paper with preliminary results was distributed under the title “Let the Dummy Talk! Unilateral Communication and Discrimination in Three-Person Dictator Experiments”. We would like to thank seminar participants at Jena, Jerusalem, Erfurt, Berlin, Würzburg, and Mannheim for helpful discussions. Financial support from the Max Planck Society is gratefully acknowledged.

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I INTRODUCTION

Traditionally, most human interactions throughout history were done face-to-face. With the advance of communication technology, however, many interactions have become anonymous and impersonal. Although communication can play a crucial role in strategic interactions, game theoretical models focus on the content of the communication, so that face-to-face communication in itself has no impact on the theoretical results. Moreover, cheap talk messages and costly messages alike should have no effect on rational players when the preferences of the players are strictly opposed and commonly known (c.f. Austen-Smith and Banks 2000, Crawford and Sobel 1982). Nonetheless, it is by now well established that people in fact behave differently when interacting with others following pre-play face-to-face communication (e.g. Dawes 1990, Frohlich and Oppenheimer 1998). Specifically, in simple bargaining games, face-to-face communication has been shown to induce generosity (Roth 1995, Schmidt and Zultan 2005).

Explanations for these effects can be broadly categorized as belonging to two general types. The first type of explanation attributes communication effects to changes in preferences, triggered by acquaintance with attributes of others. Examples are group identity or empathy.¹ Experimental explorations of the identifiable victim effect (Schelling 1968) have established that people give more to identified recipients (Small and Loewenstein 2003) and when more information is provided about the recipient (Bohnet and Frey 1999, Charness and Gneezy 2008). These effects are taken to reflect a genuine change in the social preferences of the "dictator" participants. Face-to-face communication automatically implies identifiability and vividness, and therefore is likely to have an effect on social preferences. However, previous studies have failed to find an effect for mere visual exposure or vividness (Bohnet and Frey 1999, Jenni and Loewenstein 1997).

Another branch of the literature sees pre-play communication effects as caused by strategic aspects: since verbal and non-verbal channels of communication eliminate anonymity, players confront something like a repeated

¹See Dawes (1990) for experiments on group identity and cooperation.

game where their reputation is at risk. Additionally, face-to-face communication serves to support and enhance the strategic aspects of the communication, making promises, threats, or coordination proposals strategically meaningful (Brosig, Ockenfels and Weimann 2003).

Roth (1995) referred to these explanations as the Uncontrolled Social Utility Hypothesis and the Communication Hypothesis. He uses an ultimatum bargaining experiment to compare two conditions of pre-play face-to-face communication: unrestricted, and restricted to non-game topics. Both communication treatments are equally successful in inducing nearly equal splits, thus rejecting the Communication Hypothesis of additional strategic effects.² Conversely, Brosig et al. (2003) observe in 4-person public goods experiments that lifting anonymity (via video screen) does not enhance contributions, and thus cannot find support for pure Social Utility. However, none of the existing studies was aimed at disentangling the different types of processes, making it hard to rule out either one. On the one hand, even under restricted bilateral communication, strategic effects of non-verbal communication and reputation concerns may still be present in ultimatum and public good games. On the other hand, social utility theories require more than pure visual identification to stimulate social processes.

We introduce an experimental design enabling us to rule out strategic effects, and thus to examine purely *social* effects of communication. Specifically we implement a three-person dictator game with unilateral video-based pre-play communication from receivers to dictators.³ In this environment, communicators have no strategic power. Therefore, the design does not allow for (explicit or implicit) strategic information such as threats or promises in the video messages. Furthermore, the addressed powerful player is not susceptible to (out-of-the-lab) reputation effects since the one-sidedness of communication makes her completely anonymous. Thus, all communication effects obtained in this experiment can be construed as purely social effects

²In a similar setup, but using the strategy method, Schmidt and Zultan (2005) show that responders' strategies are actually less cooperative in an unrestricted treatment than in a no-communication treatment.

³We assume that communication effects are not restricted to actual face-to-face encounters, but can also be attained by video-mediated communication (Brosig et al. 2003).

and attributed to changes in the preferences of the dictators due to the communication received from the recipients. Our game is actually very much alike the TV ads published by charities in which potential receivers directly speak into the camera. Thus, our results are also applicable to the question of how to increase generosity using one-sided communication channels.

Additionally, having more than one receiver allows us to study communication-based discrimination in this context. In real life, communication between two persons is seldom totally isolated. People are embedded in social networks, or there are even unrelated bystanders present. Those third parties may be indirectly affected by the communication between the two direct communicators. In particular, we hypothesize that there are not only direct social effects of communication, but also indirect positive or negative external spillover effects. Social communication between two persons might weaken social ties and lower generosity to third parties, e.g. in the sense of crowding out, or it may increase such generosity as a by-product of increased sociability.

We distinguish three communication treatments: a no-communication baseline, a video-only treatment where both receivers are seen, but not heard by the dictator, and an audio-visual treatment where additionally one receiver is heard, but not the other.⁴ To control for social perceptions, we elicit ratings of receivers in the communication treatments utilizing the semantic differential of activity, evaluation, and potential (Osgood, Suci and Tannenbaum 1957).

According to our experimental results donations are higher when the recipient is both seen and heard. Mere visual exposure is not enough to induce dictator generosity.

Social ratings of receivers show high correlations with generosity towards them and discriminate both within and between receiver pairs in both the visual and audio-visual treatment. Dictators' decisions are comparably cor-

⁴These treatments are comparable to the anonymity, one-way identification, and one-way identification with information treatments in the classroom experiment of Bohnet and Frey (1999). Our design adds direct comparisons of the same visual information with and without the auditory channel, and provides additional information on discrimination between receivers and the effects of impression formation across specific communicating receivers.

related with similar ratings obtained from neutral judges, indicating a causal relationship between the impression made by a recipient and what she receives. However, the specific content of the messages does not appear to play a systematic role in the dictators' decisions.

Thus, purely social factors play a role in communication in bargaining, at least when strategic issues are absent. Unilateral communication generates social ties towards communicators, even when the audio channel is omitted. With the audio channel these ties translate to significantly higher donations to (some) receivers, but in a discriminatory way.

The paper proceeds as follows: Section II introduces our experimental design and procedures in detail. Section III presents our results, and Section IV concludes.

II EXPERIMENTAL DESIGN AND PROCEDURES

II.A Experimental design

Studying how different communication possibilities affect sharing and induce discrimination requires at least three parties, one who allocates and two who may be treated differently. Relying on minimal group size, our experimental paradigm is a three-person dictator game. Dictator X can distribute a pie of 17 Euros between herself and two receivers Y and Z (with Y being the "talking" receiver, see below), who have no strategic influence and can merely hope that the dictator will be generous. The possible allocations (x, y, z) with $x, y, z \geq 0$ and $x + y + z = 17$ are additionally restricted by $x \in \{0, 2, 4, 6, 8, 10, 12, 14\}$ and $y, z \in \{1, 2, 3, 4, 5, 6, 7, 8, 9\}$. Of the altogether 40 possible allocations (x, y, z) the dictator must select one, which forces X to prefer one receiver and excludes equal payoffs of all three participants: if dictators favor one receiver over the other, this should show up in our data, while indifference between receivers should average out.

Before the dictator makes her allocation decision the receivers may unilaterally communicate to her by the means of a video message. We distinguish three treatments:

- Treatment N (no communication): the dictator X just chooses an allocation.
- Treatment V (visual communication): before selecting the allocation the dictator sees a video of both receivers without hearing them.
- Treatment AV (audio-visual communication): the dictator sees both receivers and hears one of them, namely Y , before deciding.

We used the same receivers (video messages) in all treatments, allowing for statistically more powerful paired comparisons. Corresponding to the treatment, the audio was turned off for both receivers (receiver Z only) in treatment V (AV).

Discrimination in the no-communication treatment would be completely arbitrary and is therefore unexpected to exceed the minimally enforced amount. When both receivers are seen, but not heard, differential treatment would reveal that dictator participants establish one-sided empathy relations purely based on appearance.⁵ Thus only in the audio-visual treatment could substantive arguments matter.

By keeping dictator anonymity, our experimental scenario excludes strategically important information in the sense of threats or promises in video messages, allowing us to interpret resulting effects as being purely social. However, this does not preclude that communicators strategically try to invoke those social effects by deliberately choosing the specific social content of their message and hint at emotional reactions and dissatisfaction that might affect dictator participants.

⁵We assume that lipreading is not feasible, as receiver pictures on the video screens were rather small, taking only one quarter of the screen size, and both receivers move their mouths simultaneously in treatments V and AV. However, we cannot exclude this possibility completely.

II.B Experimental procedures

All experimental sessions took place in the video laboratory of the Max Planck Institute in Jena, Germany.⁶ We conducted five identical sets of sessions, one in summer 2003 and four in spring 2009. Each of those five sets involved 16 receivers (matched to 8 pairs of two), and three times 8 dictators for the three treatments N, V, and AV. Thus, we had altogether 80 (=5x16) receivers and 120 (=5x3x8) dictators in the experiment. In each set, the 8 dictators in each treatment decided subsequently for all 8 receiver pairs in the set, all in the same (randomized) order. We collected 8 distributional decisions from each dictator representing one statistically independent observation.

For each treatment, each receiver pair was randomly matched with one dictator for payment after the experiment. Dictators were paid according to the allocation choice for their receiver pair. Receivers received the average of their selected allocations in the three treatments.⁷ This design (including the existence of different treatments) was commonly known.

Participants were only female students from universities in Jena and were invited using the online recruitment system ORSEE (Greiner 2004). The invitation procedure was conducted separately for participants in dictator and receiver roles, respectively, to guarantee anonymity outside the controlled experimental conditions.

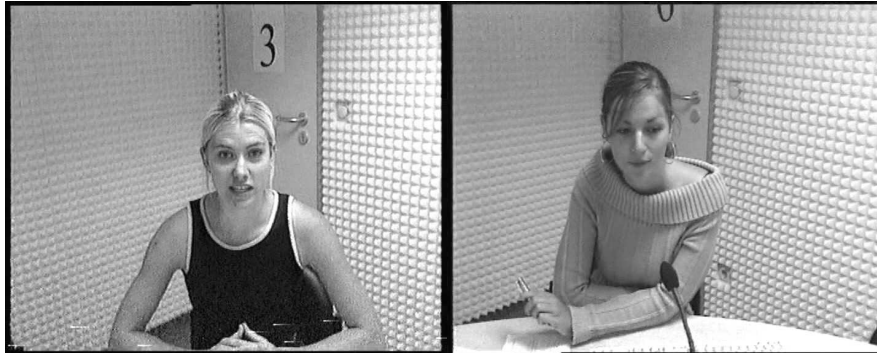
Upon arrival each participant was led to one of eight sound-proof booths, each equipped with computer, computer screen, video camera, video screen and microphone. In each of the five sets of sessions, receivers arrived and played in two cohorts of 8 subjects each. They received the instructions which were (announced to be) the same for all treatments and roles, and were told that they will be either in role *Y* or *Z*. After reading their instructions they had 10 minutes to prepare their talk. The video message was created by

⁶Instructions can be found in Appendix A. Transcripts of video messages can be requested from the corresponding author.

⁷Thus, dictators increased a receivers's outcome by only 1/3 Euro with each Euro given away. This, however, was the case in all three treatments, such that between-treatment comparisons are not affected.

letting participants speak freely into the camera for 2 minutes. We imposed no restriction on what to say. As recording a video message might be an unusual situation for participants, they received the opportunity to record their message twice. Without having seen the messages, receivers chose which of the two messages should be used for the remaining procedure.

FIGURE 1
EXAMPLE OF THE VIDEO SCREEN WITH A RECEIVER PAIR



The experimenters edited the video messages according to the respective treatment. For each set of sessions (each involving 16 receivers and 24 dictators), we formed 8 receiver pairs, which remained unchanged for the rest of the experiment.⁸ The order of videos was determined randomly. To avoid effects of the video position, we altered the position of the talking receiver in each round, such that in half of the receiver pairs the talking receiver's screen was on the left respectively right-hand side.

On the following day,⁹ dictator participants arrived and played in cohorts of 8 subjects. Dictators received the same instructions as the receivers. They were informed about their role and treatment, i.e. whether or not they see

⁸In one set, the matching excluded those subjects from the role of the talking receiver *Y* who stopped talking after a short time into the 2 minutes period. Note, however, that this asymmetry is constant across treatments, and thus does not affect our paired comparisons. The matching procedure in the four other sets was randomized to enable some further within-treatment comparisons.

⁹In all 5 sets of sessions the three dictator sessions for the different treatments were conducted on the same day, with the order of treatments rotated across sets.

the videos and hear one receiver. After the instruction phase, dictators played 8 rounds, one for each receiver pair. In each round, first the prepared video was played, with the communication channeled according to treatment. Next, the dictator chose the allocation (x, y, z) on a computer screen. Finally, dictators rated the receivers they saw. In the control treatment N, the first and third steps were left out, and the dictators were simply asked to wait for two minutes between rounds.

The ratings were elicited using bipolar scales: active - passive and lively - dull, attractive - unattractive and pleasant - unpleasant, strong - weak and influential - not influential, corresponding to the three factors of the semantic differential (Osgood et al. 1957) – activity, evaluation, and potential, respectively.¹⁰ Following the bipolar ratings, the dictators were asked to note for each receiver whether they have seen her before or know her personally. Of the 1280 different dictator-receiver combinations in the two communication treatments, dictators indicated that they personally knew a receiver only 12 times (5 and 7 times in treatments V and AV, respectively) and reported to have previously seen a receiver 38 times (29 and 9 times in treatments V and AV, respectively). Receivers who had been previously seen received significantly more (see the regressions reported in Table 2).¹¹

After playing all eight rounds, payoffs were calculated as described above. Dictator participants were immediately paid in cash and left the laboratory whereas receiver participants were paid later, as dictators had still to decide.¹² The sessions lasted on average about 60 minutes for dictators and 45 minutes for receivers. The average earnings per play were 15.19 Euros for dictators, 7.39 Euros for talking receivers *Y*, and 7.46 Euros for non-talking receivers *Z*. All earnings include a show-up fee of 4 Euros.

¹⁰Appendix B contains the supplementary instructions given to participants for these ratings.

¹¹The lack of a significant effect for receivers who were identified as personal acquaintances may be due to the small number of observations available.

¹²Receiver participants could collect their payments either at the institute's office, the next time they participated in an (other) experiment, or by meeting with the experimenters at specific times and places.

III RESULTS

Due to our experimental design we collected 40 statistically independent observations in each treatment (eight dictators in each of the five sets), each comprising 8 distributional decisions with respect to the eight receiver pairs in the set. We start our analysis with overall effects of communication channels. Next, we review the social ratings provided by dictators and the effect on their decisions. The analysis of those social ratings is complemented by an analysis of comparable ratings elicited from external judges. Finally, we address the elicited fairness notions of the participants.

TABLE 1
AVERAGE RELATIVE SHARES AND VARIANCES OVER ALL RECEIVER GROUPS, AND TESTS ON TREATMENT DIFFERENCES AND DISCRIMINATION

Treatment	x/p	y/p	z/p	$ \frac{y-z}{p} $	$\sigma_x^2 \cdot 10^2$	$\sigma_y^2 \cdot 10^2$	$\sigma_z^2 \cdot 10^2$
<i>Averages</i>							
N	.67	.16		.076	.560		.312
V	.65	.18		.095	.362	.408	.365
AV	.60	.21	.19	.103	.473	.516	.384
<i>Mann-Whitney-U tests</i>							
V vs. N	-	-		.004***	-		.065*
AV vs. V	-	-	-	-	-	-	-
AV vs. N	.076*	.009***	-	.000***	-	.031**	-
<i>Wilcoxon Matched Pairs Signed Ranks tests</i>							
V y vs. z							-
AV y vs. z						.007***	.011**

x/p , y/p , and z/p denote the average relative allocation to players X , Y , and Z , respectively. $|\frac{y-z}{p}|$ denotes the average total difference between y/p and z/p . Tests are two-sided. '-' means non-significant, *, **, *** indicates significance on the 10%, 5%, 1% level, respectively.

III.A Communication channel effects

The statistical analyses reported below are based on dictators as independent units of observation. The results are complemented by regressions

which account for receiver pair effects as well as for the relatedness of single dictator's decisions.

Table 1 lists the average relative shares of the pie for all treatments and roles. Additionally, results of non-parametric tests on overall treatment effects are reported. Self-allocations of dictators decrease, and donations to the (talking) receiver increase correspondingly when adding communication channels starting from the baseline over the visual to the audio-visual treatment. The effects observed are significant only for dictators' average self-allocation and the donations to talking receivers when comparing treatments AV and N, as well as being weakly significant when comparing the talking receivers' allocation in treatments AV and V. The talking receiver also received significantly more when compared to her partner within treatment AV.

The regressions presented in columns 1-4 of Table 2 support these findings. While the dependent in these four models is the individual donation of the dictator to a receiver, the included explanatory variables are:

- *Video*, a dummy variable for whether this treatment included the video, being 1 in treatments V and AV and 0 in treatment N,
- *Audio*, a dummy variable for whether the audio for one of the receivers was transmitted in this treatment, being 1 in treatment AV and 0 in treatments N and V,
- *Talk*, a dummy variable indicating whether this was the talking receiver (1) or not (0),
- *seen rec. bef.* (for 'seen receiver before'), being 1 if the dictator indicated to have seen this receiver before, outside of the laboratory,
- *know rec.* (for 'know receiver'), being 1 if the dictator indicated to know this receiver, and 0 otherwise.

TABLE 2

LINEAR AND LINEAR MIXED EFFECTS REGRESSION OF RECEIVER ALLOCATION ON TREATMENT, ROLE, AND EVALUATION

Model Type	1		2		3		4		5	
	Linear Model		Linear Model		Mixed Effects REML		Mixed Effects REML		Mixed Effects REML	
	Coeff	StdErr	Coeff	StdErr	Value	StdErr	Value	StdErr	Value	StdErr
Intercept	2.797***	(0.068)	3.189***	(0.248)	2.797***	(0.201)	3.182***	(0.341)	3.647***	(0.392)
Video	0.167*	(0.097)	0.153	(0.095)	0.182	(0.283)	0.180	(0.284)		
Video*Audio	0.205*	(0.118)	0.212*	(0.115)	0.196	(0.307)	0.197	(0.292)	0.279	(0.287)
Video*Audio*Talk	0.415***	(0.137)	0.420**	(0.133)	0.411***	(0.091)	0.411***	(0.091)	0.320***	(0.091)
Video*seen rec. bef.	0.966***	(0.286)	1.263***	(0.281)	0.617***	(0.204)	0.653***	(0.207)	0.337*	(0.186)
Video*know rec.	-0.285	(0.573)	-0.606	(0.562)	0.144	(0.396)	0.084	(0.401)	0.283	(0.359)
EP rating									0.496***	(0.055)
A rating									0.123***	(0.041)
EP other rec.									-0.060	(0.055)
A other rec.									-0.079*	(0.041)
Audio*EP									-0.123	(0.091)
Audio*A									-0.057	(0.059)
Audio*EP other									-0.037	(0.088)
Audio*A other									0.012	(0.066)
Audio*Talk*EP									0.313***	(0.094)
Audio*Talk*A									-0.121*	(0.065)
Audio*Talk*EP other									-0.022	(0.093)
Audio*Talk*A other									0.020	(0.065)
Receiver Pair FE		N		Y		N		Y		Y
Dictator RE		N		N		Y		Y		Y
Groups						120		120		80
StdDev Intercept						0.750		0.714		0.870
StdDev treat ID.L						1.333		1.219		1.230
StdDev treat ID.Q						1.264		1.256		
StdDev Residual						1.154		1.155		1.023
N		1920		1920		1920		1920		1280
Adj. R-squared		0.0284		0.0864						
Aikake IC						6394.3		6477.1		4112.207
BIC						6466.6		6765.0		4418.8
restricted logLL						-3184.2		-3186.5		-1996.1

Note: Standard errors are given in parentheses. *, **, *** indicates significance on the 10%, 5%, 1% level, respectively.

These independents are interacted in a way such that they pick up the marginal effect of an additional feature of the interaction (e.g., Video*Audio picks up the effect of adding the audio channel to the video channel, in addition to the identified effect of the video channel alone). Models 1-4 of Table 2 specify different regression models. Models 1 and 2 are ordinary least square regressions, with model 2 including fixed effects for receiver pairs and model 1 not including such controls. Models 3 and 4 are mixed effects restricted maximum likelihood models, including random effects for dictators within treatments. Model 4 (3) does (not) include fixed effects for receiver pairs.

The results from the four estimations support the conclusion that communication systematically affects donations only to a heard receiver, whereas the effects of mere visual exposure and a heard partner are not robust across different model specifications. The models also indicate a robust positive effect on the donation if the dictator has seen the receiver before, outside the experiment.

Result 1 *Full audio-visual one-sided communication limits the self-serving behavior of dictators. No significant effects are evident for mere exposure. The increase in the talking receiver's share is associated with a decrease in the dictator's share while the non-talking receiver's share remains unchanged.*

We consider two levels of discrimination in donations. Dictators might discriminate a) within receiver pairs, and b) between receiver pairs. As discussed above, we observe systematic discrimination favoring the talking receiver in treatment AV. This is also reflected in the basic tendency of dictators to discriminate, measured by the proportion of dictator decisions which (do not) allocate nearly equal shares to both receivers. Similar discrimination is also evident in treatment V with symmetric receivers roles. In 88.1% of all decisions in treatment N dictators chose a pie distribution with minimal payoff difference between the two receivers (i.e. a difference of 1 Euro). This tendency was significantly lower in treatments V (76.3%) and AV (70.6%, Fisher's Exact tests, two-tailed, both $p < .001$). The difference

between treatments V and AV is not significant ($p = .128$). Correspondingly, the absolute differences between allocations to the talking and the non-talking receiver are significantly higher in treatments V and AV than in treatment N (Table 1, 4th data column).

The variance of a dictator's allocations across different receiver pairs serves as a measure of the dictator's discrimination between receiver pairs. The right part of Table 1 reports averages of this measure over the dictators in a treatment. Variances in allocations increase when adding communication channels starting from the baseline over the visual to the audio-visual treatment. The difference between treatments N and V is only weakly significant, and the increase in variance in treatment AV is only significant for the talking receiver. The between-pairs variance is also significantly higher for the talking receiver's share than for the non-talking receiver's share in treatment AV.

Result 2 *Identifiability significantly increases the discrimination between receivers, even in the absence of verbal information. This discrimination is strongly manifested in the within-pairs comparisons, whereas the evidence for discrimination between groups is mostly apparent for the talking receivers.*

III.B Dictators' Evaluations of Receivers

A series of principal-components analyses on the dictators' ratings of receivers yielded an identical factor solution with two factors:¹³ The first factor (Eigenvalues around 3.0) includes the scales corresponding to the 'evaluation' and 'potency' scales. The second factor (Eigenvalues around 1.5) corresponds to 'activity'. All following analyses of the ratings data are based on two variables based on the two factors, labeled Evaluation/Potency (EP) and Activity (A) henceforth. The average ratings of receivers on those factors are presented in Table 3.

The average ratings do not differ significantly between treatments or between roles. Mann-Whitney-U tests for between-treatment comparisons

¹³The solution of the analysis is identical when performed separately for the talking and non-talking receivers.

TABLE 3
AVERAGE RATINGS ON THE EVALUATION AND THE ACTIVITY
FACTOR

Treatment	EP_y	EP_z	A_y	A_z
<i>Averages</i>				
V	.24 (1.12)	.27 (1.08)	.59 (1.62)	.22 (1.82)
AV	.28 (1.08)	.24 (1.03)	.57 (1.48)	.22 (1.74)
<i>Pearson correlations with corresponding allocations</i>				
V	.469***	.392***	.402***	.287***
AV	.397***	.187***	.170***	.150**

EP_y, EP_z (A_y, A_z) denote the average rating on the evaluation/potency (activity) factor for receivers Y and Z , respectively. Standard deviations are given in parentheses. Pearson correlations are two-sided. *, **, *** indicate significance on the 10%, 5%, 1% level, respectively.

and Wilcoxon signed-ranks tests for between-role (within-treatment) comparisons yielded $p > .5$ in all cases.¹⁴

Strong (Pearson) correlations were found, across factors, roles and treatments, between the dictators' perception of the receivers as reflected in the ratings and the share of the pie allocated to them (see the lower part of Table 3). Additionally, the talking receiver's allocation in treatment AV is negatively correlated with the ratings of the non-talking receiver. However, some of the correlations are due to intercorrelations between the different ratings. These intercorrelations were controlled for in the multivariate estimation presented in column 5 of Table 2. Both dimensions of the receiver evaluations by a dictator are positively correlated with dictator generosity. This effect is differential with respect to talking receivers, the effect of whose perceived evaluation/potency is amplified, while perceived activity is, somewhat surprisingly, weaker than for non-talking receivers. The latter result might arise if a talking receiver is perceived as too assertive to her disadvantage, or if role-induced activity is less crucial for donations.

¹⁴When including the sessions in which the talking receivers were chosen for being more talkative the talking receiver is rated as more active than the non-talking receiver in treatment AV.

Result 3 *The share of a receiver is correlated with her social perception by the dictator. Differential preferences between receivers arise even without the audio channel.*

III.C External evaluations of receivers

The correlations between the dictators' allocation decisions and their evaluations of the receivers suggest a causal relationship between the impression a specific receiver made on the dictator and the amount allocated by the dictator to the receiver. However, such a causal relationship can not be deduced with certainty from correlational data, especially since the dictators evaluated the receivers only after the allocation decision had been made. Thus, it is possible that the dictators wanted their evaluations to be consistent with their previous decisions. To control for the causal relationship between allocations and evaluations, two additional sessions were conducted to obtain external ratings. In each session, 8 independent judges, recruited from the same subject pool as the participants in the experiment, viewed the 32 videos of receiver pairs¹⁵ and rated them on the same scales as the dictators.¹⁶ In order to incentivize the judgments, the judges were paid according to their ability to match the (rounded average) internal ratings of the dictators.¹⁷ The first session was conducted to obtain unbiased ratings. In this session the judges rated the silent videos used in treatment V of the experiment, without being informed about any details of the experiment. Since the judges were not aware of the allocation decisions, we can safely assume that their ratings reflect an unbiased impression of the receivers.

In the second session, another set of judges rated the videos used in treatment AV. In addition to rating the impression made by the receivers, the judges were asked to classify the message made by the talking receiver

¹⁵The additional ratings were obtained only for the 32 videos with random matching of receiver pairs.

¹⁶The experimental instructions for the external ratings sessions can be found in Appendix C.

¹⁷Specifically, judges received 0.07 Euros for each rating which was the closest integer to the average rating given by the 8 dictators in this treatment. The judges earned on average 12.99 Euros including a show-up fee of 4 Euros.

with regard to game content on a Guttman-type scale. Inter-judge reliability for this scale was high and significant (interclass correlation coefficient based on a two-way random model and absolute agreement of .709, $p < 0.001$, see McGraw and Wong 1996).

The external ratings obtained for the silent videos ("unbiased external V impressions") validate the internal ratings made by dictators in the experiment.¹⁸ As data row 2 in Table 4 shows, for both factors we find high correlations between the internal and unbiased external ratings of receivers.¹⁹ Furthermore, correlations between allocations and internal ratings are largely weakened, and mostly disappear, when we calculate them as partial correlations, controlling for the unbiased external ratings (see Table 4, lower part). This indicates that at least a part of the internal ratings are based on a general, unbiased, common impression, independent of previously made decisions, and thus supports to a causal interpretation of Result 3, namely that social evaluations drive allocations. However, the remaining partial correlation for the evaluation/potency factor in treatment V suggests that also idiosyncratic social tastes play a role, which, in our experimental design, are indistinguishable from ex-post rationalization of previous allocation decisions.

The ratings obtained from the judges who knew the experimental rules and could hear the receivers ("external AV impressions") provide similar results. No systematic effect was found for the classification of message content provided by external judges. Discussing the game and suggesting specific allocations apparently does not affect the average allocations made to the talking receiver in a consistent way.

¹⁸The correlation analyses in this section rely on the 64 receivers as independent observations. Since these are essentially within-treatment comparisons, we can neglect dependencies due to different dictators in the different treatments.

¹⁹For both factors we cannot detect a level shift between internal and unbiased external ratings. Since our receiver observations are not independent from each other (as they were reviewed in sets of 16, see discussion above), it would not be reasonable to apply statistical tests here. However, taking averages over receivers as observations and applying two-sided Wilcoxon Matched Pairs Signed Ranks tests, one cannot reject the Null hypothesis that the location shift is equal to zero (all test p-values are larger than 0.1).

TABLE 4
 DIRECT AND PARTIAL, EXTERNAL RATING CONTROLLED CORRELATIONS OF
 AVERAGE EVALUATIONS AND AVERAGE ALLOCATIONS TO RECEIVERS

Internal Rating	Treatment V		Treatment AV	
	<i>EP</i>	<i>A</i>	<i>EP</i>	<i>A</i>
<i>Direct correlation with</i>				
allocation	0.718***	0.662***	0.242*	0.306**
(unbiased) ext. V impression	0.821***	0.875***	0.672***	0.820***
external AV impression			0.775***	0.868***
<i>Partial correlation with allocation,</i>				
<i>controlling for</i>				
(unbiased) ext. V impression	0.482***	0.149	0.143	-0.037
external AV impression			0.007	-0.154

Note: *, **, *** indicate significance on the 10%, 5%, 1% level, respectively. Correlations are based on 64 pairs/triples from the sessions with random receiver pairing: in each treatment/condition, allocations as well as internal and external evaluations of the 64 receivers, averaged over the 8 dictators each receiver met/8 external ratings obtained for this receiver.

Result 4 *The measured social impressions of the receivers by the dictators is valid and largely driven by unbiased perceptions. Thus a causal conclusion can be drawn: the better the impression of the communicating receiver, the more she receives from the dictator.*

III.D Perception of fairness

At the end of all dictator sessions, when all decisions were made, we asked participants what they consider would be a 'fair allocation'. Subjects across the three communication conditions treatments agreed on this aspect: overall, 68.3% of all dictators indicated an allocation closest to equal split;²⁰ the two receiver shares differed by more than the minimally allowed 1 Euro in only 2 out of 120 'fair' allocations, both in treatment AV.

The share of dictators choosing a near-equal allocation as fair is 77.5%

²⁰Providing 6 Euro to the dictator and one of the receivers and 5 Euros to the other receiver.

in the baseline treatment, 55.0% in treatment V, and 72.5% in treatment AV. Although the differences are weakly significant ($\chi^2(2)=5.16$, $p = .076$), the average fair allocation for the dictator did not differ significantly across treatments, being 7.20 in the baseline treatment, 7.05 in treatment V, and 6.55 in treatment AV (Kruskal-Wallis test, $p = .455$).

Pooling data over treatments, the amount that a dictator states to be a fair demand for herself is positively correlated with the amount she keeps (on average across receiver pairs Spearman's $\rho=0.173$, $p = .059$). However, this correlation seems to be driven by 4 subjects indicating that in a fair allocation the dictator gets the maximum amount, and acting accordingly in all 8 rounds. For the other 116 subjects the correlation disappears (Spearman's $\rho=.091$, $p = .334$). Also, "greedy" subjects do not have different fairness perceptions in general: of those who keep the maximum amount over all rounds, 66.7% view the near equal allocation as 'fair', similar to the overall proportion.

Result 5 *Communication has no significant effect on the perception of fairness. Differences between dictators in donations to receivers cannot be explained by different notions of fairness.*

IV CONCLUSIONS

We introduced an experimental paradigm which rules out strategic effects of communication but not social ones, and allows for discrimination between receivers. The use of the video technology plays a significant role in testing the research hypotheses, as it provides full control over the social stimuli used in the experiment. The repeated use of the same videos across treatments enables us to manipulate the communication elements, while keeping other characteristics constant. Furthermore, the recordings allow us to obtain external ratings which validate the internal ratings. Unlike previous experimental uses of video technology of recording only, here the technology itself is utilized in the experimental design, and not just as a way to collect data (cf. Bosman, Hennig-Schmidt and van Winden 2006).

We find that unilateral pre-play communication in the three-person dictator game inspires generosity by dictators. The effect is significant for audio-visual communication. This replicates the findings of Brosig et al. (2003) in video experiments and Bohnet and Frey (1999) in classroom games. However, the social effects of audio-visual communication in our (non-strategic) dictator game are rather small when compared to the effects observed in experiments where communicators have strategic power, such as public goods games or ultimatum games.

Including two receivers in the game allowed us to reveal the social effects of (silent) communication on donation and discrimination. Although, consistent with the findings of Bohnet and Frey (1999), mere exposure does not significantly affect average contribution levels in our experiment, we identify an effect on discrimination. Internal and external social ratings provide evidence that impression formation does play a role in the effect of silent communication. The effects of non-anonymity are closely related to effects of identifiability (Charness and Gneezy 2008, Small and Loewenstein 2003). In line with the conclusions obtained by Bohnet and Frey (1999) and Jenni and Loewenstein (1997), our results provide evidence that mere identifiability associated with silent face-to-face communication may not significantly enhance dictator generosity. However, our analysis of multiple recipients and impression formation allows us to reach a stronger conclusion compared to previous studies. We establish that specific impressions by communication do affect dictators' decisions, although they do not affect average contributions. This effect is robust even though that dictators make repeated allocations to multiple recipients, a design which is expected to reduce the effects of vividness and identifiability (Kogut and Ritov 2005).

The differential effect for recipients who are heard can be hypothesized to be related to *guilt aversion* (Battigalli and Dufwenberg 2007, Charness and Dufwenberg 2006). According to models of guilt aversion, dictators are more generous the more they believe the recipient is expecting to receive. Thus, the video messages may serve as a vehicle to influence dictators' beliefs about the recipient's expectation and consequently affect dictators' allocation decisions. Naturally, this effect is restricted to recipients who are heard

by the dictators. However, recent experimental investigations cast doubt on the validity of guilt aversion as a descriptive principle (Ellingsen, Johannesson, Tjøtta and Torsvik 2010, Vanberg 2008). Furthermore, the analysis of both external and internal ratings of the video messages suggest that dictators' generosity is sensitive to general impression formation rather than to the game-related content of the messages.

Given that a dictator cares about the welfare of a recipient who is heard, the relevance of this effect for the dictator's allocation to another recipient is not readily clear. On the one hand, the effect may benefit also the other recipient. On the other hand, the dictator may set a 'fixed total sacrifice' which is then divided between the recipients (Büchner, Coricelli and Greiner 2007, Selten and Ockenfels 1998), implying that an increase in one recipient's share decreases the other's share. The results in the current experiment show that, while the talking receiver benefits from talking, the allocation to the non-talking receiver in treatment AV remains the same as in treatment V. When a dictator chooses to allocate a higher amount to one recipient, she does so at her own expense.

The variance in allocations is related to the social evaluations of the receivers. In the elicited ratings of the receivers, discrimination is already existent in our video-only treatment V. Adding the audio channel in treatment AV strengthens most ratings and increases allocations to receivers, but in a discriminative way both within and between receiver pairs.

Altogether, our experiment provides clear evidence for positive social effects of communication on dictator donations, thereby replicating and clarifying earlier but more ambiguous findings. Identification per se does not lead to higher donations overall, but is the basis for discrimination between beneficiaries. When identification is accompanied by social verbal content, providing personal information about the beneficiaries, average donations increase. The richer the information, the more it affects the decisions, leading to preferential attitudes and actions.

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APPENDIX FOR ONLINE PUBLICATION

APPENDIX A: EXPERIMENT INSTRUCTIONS

(translated from German)

Welcome and many thanks for your participation in this experiment. **Please do not touch any of the equipment before we ask you to do so.** If you have problems with the equipment or other questions, please use the microphone, or ask one of the experimenters. Please read the following instructions carefully. Instructions are identical for every participant. You are able to earn money during the experiment. The amount you earn depends on your own decisions and the decisions of other participants of the experiment.

1. The experiment

The rules of the experiment are very simple. There are three Persons X, Y and Z. There is a certain amount of money to distribute, which size is 17 Euros. In the experiment, Person X decides how she wants to divide the money. When doing so she is restricted to some rules, which are described in section 2. Before Person X decides about the distribution, she watches a video tape, which was recorded before with Persons Y and Z. Details about this are described in section 3. Exactly as Person X proposed, the amount of money will be distributed and paid out according to the rules in section 4. The procedure of this experiment requires, that the participants in the roles of X, Y and Z participate in the experiment at different dates. Specifically, the participants Y and Z are invited first, while the participants in the role of Person X participate in the experiment at a later date.

2. Rules for distribution

Person X is bound to the following rules for the distribution of the amount of money:

- a) The sum of allocations to the three persons must be 17 Euros.
- b) Person Y and Z may only get either 1, 2, 3, 4, 5, 6, 7, 8, or 9 Euros.
- c) Person X may only get either 0, 2, 4, 6, 8, 10, 12, or 14 Euros.

Therefore, there are 40 distribution possibilities. These are listed in a table at the end of these instructions.

3. Video recording

In the experiment, persons in the role of Y and Z will be given the opportunity to one-sidedly communicate to the person in the role of X. They have 10 minutes to prepare for this. After the preparation time, participants in the roles of Y and Z have two minutes to record a video message. During this time Persons Y and Z are allowed to speak freely about everything, including the experiment. Before her decision the videos of Person Y and Z are presented to Person X. There are three possibilities: 1. Person X sees and hears none of the two Persons Y and Z. 2. Person X sees Person Y as well as Person Z, but cannot hear any of the two. 3. Person X sees Person Y as well as Person Z, but can hear either only Person Y or only Person Z.

4. Calculations of payoffs

Every participant in the role of Y makes up a pair with exactly one participant in the role of Z. The recorded video of this pair will be shown to exactly 24 different participants in the role of X. Every Person X sees 8 different pairs. She decides for every pair which she sees about the distribution of the amount of money. After the experiment one of the 8 pairs will be randomly selected for each Person X. Then, Person X gets the amount which she allocated to herself. Person Y and Z get the average of the amounts, which 3 persons in the role X have allocated to them. Due to the experimental procedure, participants in the role of Y and Z cannot be paid out immediately after the

experiment, because their specific payoff can only be calculated after the participants in the role X have participated in the experiment. To handle the payoffs, one experimenter will be at the university at different times in the following week. The specific dates and locations will be sent early enough by e-mail. However, to pick up your payoff in cash you might come directly to the institute on every working day in the same or the following week, from 9am to 4pm. Participants in the role of X are paid out in cash immediately after the experiment.

If you have any questions regarding these instructions, please ask one of the experimenters.

The 40 different distribution possibilities

<i>x</i>	0	0	2	2	2	2	4	4	4	4
<i>y</i>	9	8	9	8	7	6	9	8	7	6
<i>z</i>	8	9	6	7	8	9	4	5	6	7
<i>x</i>	4	4	6	6	6	6	6	6	6	6
<i>y</i>	5	4	9	8	7	6	5	4	3	2
<i>z</i>	8	9	2	3	4	5	6	7	8	9
<i>x</i>	8	8	8	8	8	8	8	8	10	10
<i>y</i>	8	7	6	5	4	3	2	1	6	5
<i>z</i>	1	2	3	4	5	6	7	8	1	2
<i>x</i>	10	10	10	10	12	12	12	12	14	14
<i>y</i>	4	3	2	1	4	3	2	1	2	1
<i>z</i>	3	4	5	6	1	2	3	4	1	2

APPENDIX B: INSTRUCTIONS FOR RATINGS

(translated from German)

In the following, we will ask you for your evaluation of the persons viewed.

Here it is described how to use the scales. In case you are not sure how to fill out the questionnaire have a look at this instruction again.

In case you find a person to rate very similar to an attribute at the end of the scale, then check one of the following boxes

active passive
 active passive

In case you find a person to rate quite similar to an attribute at the end of the scale, then check one of the following boxes

active passive
 active passive

In case you find a person to rate lightly similar to an attribute at the end of the scale (but not really neutral), then check one of the following boxes

active passive
 active passive

Naturally, the horizontal direction of your cross depends on which of the two attributes on the scale describes the person you are rating best. When the person you are rating can be described neutral with regards to the two attributes, that means that both attributes apply to the person alike, you should mark the box in the middle.

active passive

Please mark down whether you knew the person you are rating before. Please mark whether you have just seen the person (e.g. at university) but not known her personally, or whether you know your partner personally.

APPENDIX C: EXTERNAL RATINGS INSTRUCTIONS

(Translated from German. Text in curved brackets appeared only for the ratings of the silent videos. Text in square brackets appeared only for the ratings of the audible videos.)

Welcome and many thanks for your participation in this experiment. **Please do not touch any of the equipment before we ask you to do so.** If you have problems with the equipment or other questions, please use the microphone, or ask one of the experimenters. Please read the following instructions carefully. Instructions are identical for every participant. You are able to earn money during the experiment. Today you will have to watch some videos from participants from another experiment and estimate how other participants evaluated them. You will earn more money if your estimations are more accurate.

During another experiment some participants recorded a short two-minute video. These videos have been edited such that there were two participants in each video - one on the left side and one on the right side. [Only one of the participants could be heard.] The edited video was shown to other participants before they made their decisions. [These participants had to distribute an amount of 18 Euros between themselves and the two participants on the video.] After they saw the video [and made their decision,] they had to evaluate the persons on the video. The evaluating participants did not get any financial reward for their evaluations (but from their other decisions).

The evaluation questionnaire consisted of {6}[7] questions and looked for the participant on the left side as follows:

active	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	passive
weak	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	strong
pleasant	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	unpleasant
dull	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	lively
unattractive	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	attractive
not influential	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	influential

The same questionnaire was used for the participant on the right side, But the last question (about argumentation) was only asked if the participant could be heard. For the ratings the evaluating participant received instructions, which are enclosed here on a separate sheet of paper.

Today you will see 32 of these videos (taken from different experiments). You will fill in identical questionnaires as seen above. You should, however, not submit your own evaluation, but estimate how the evaluating participants from the other experiments evaluated the persons on the videos. These evaluating participants are very similar to you: They are female students in Jena.

Your payoff depends on how accurate your estimations are. You will receive 0.07 Euros for each of the {384}[416] single evaluations (32 videos x 2 participants x 6[or 7] rating scales) if your estimation is equal to the rounded average of all evaluations. If, for example, the average of all "active"/"passive" evaluations for a given participant in one of the videos from the other experiment equals 4.3, you will earn 0.07 Euros if you chose 4 on "active"/"passive" and you will earn nothing if you chose a different value. We will also ask you for every participant if you know her personally. This question will not affect your payoff. Please answer honestly.

At the end of the experiment you will be informed about how many of your estimations were correct, and you will be paid in cash.