Social Communication and Discrimination: A Video Experiment*

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Abstract

We report on an experiment using video technology to study effects of communication on donations to 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 analyze purely social effects of communication. A silent video 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 by the visual and audio channels are heterogeneous and communicator-specific but not unsystematic.

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

JEL Classification: C72, C91, D64

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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 more 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 the act of face-to-face communication in itself has no impact on the theoretical results. Moreover, cheap talk or costly messages 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 seems 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).[1]

Explanations for these effects can be broadly categorized as belonging to two general types. The first type attributes communication effects to changes in preferences, triggered by learning about attributes of others. Examples are group identity or empathy.[2] The identifiable victim effect (that people give more to identified receivers, see Schelling, 1968; Small and Loewenstein, 2003) gets stronger when more information is provided about the receiver (Bohnet and Frey, 1999; Charness and Gneezy, 2008), suggesting a genuine change in the social preferences of "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 com-

[1] Non-strategic communication may also affect behavior when it is not conducted face-to-face. Electronic chat communication as part of group-identity manipulation was shown to have an effect on strategic decisions by Eckel and Grossman (2005), Chen and Li (2009) and Chakravarty and Fonseca (2010).

[2] See Dawes (1990) for experiments on group identity and cooperation.

munication eliminate anonymity, players are confronted with something like a repeated 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.[3] Conversely, Brosig et al. (2003) observe in 4-person public goods experiments that lifting anonymity (via video screen) does not enhance contributions, and therefore 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.

In this paper we aim to more clearly distinguish how communication influences generosity, by introducing an experimental design ruling out strategic effects in order 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.[4] 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

[3] In 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.

[4] We 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).

one-sidedness of communication makes her completely anonymous. Thus, all communication effects obtained in this experiment can be construed as purely social effects and attributed to changes in the preferences of the dictators due to the communication from the receivers. 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.

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. Having two receivers in our 3-person dictator game allows us to identify effects of communication on dictator discrimination, which are absent in the standard two-players dictator game.

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.[5] To control for social perceptions, we elicit social ratings of receivers in the communication treatments utilizing the semantic differential of activity, evaluation, and potential (Osgood, Suci and Tannenbaum, 1957). We complement this data with similar ratings obtained from external judges, who are either informed or uninformed about the experimental game.

[5]These treatments loosely correspond 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.

Thus, our paper contributes to the literature by 1) testing the robustness of existing results in a new experimental framework utilizing video-technology and controlling for strategic effects of communication, 2) studying communication effects not only on (average) donations but also on discrimination between potential receivers, and 3) analyzing how the formation of social evaluations and impressions based on received communication can explain dictators' donation and discrimination decisions.

According to our experimental results, donations are higher when the receiver is both seen and heard. In line with the existing literature, mere visual exposure is not enough to induce dictator generosity (Bohnet and Frey, 1999). However, while only audio-visual communication is sufficient to increase average donations, discrimination between receivers is already observed with video messages only.

In addition, we find that social ratings of receivers (measured in an activity/potency and an evaluation dimension) are highly correlated with generosity towards them and discriminate both within and between receiver pairs, in both visual and audio-visual treatments. An analysis of the external ratings of the video messages, showing a similar correlation, establishes a causal relationship between the impression made by a receiver and what she receives. However, we find no evidence that specific content of the messages (i.e. whether communicators discuss the game or refer to specific distributions) plays a systematic role in the dictators' decisions.

Thus, our results allow us to establish and characterize previously unstudied effects of communication on dictator game behavior. 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. Dictators' generosity seems to be driven by a general impression formation rather than the game-related content of the messages.

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 will 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.[6] 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.

II.B Experimental procedures

All experimental sessions took place in the video laboratory of the Max Planck Institute in Jena, Germany.[7] 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

[6] We 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.

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

of their selected allocations in the three treatments.[8] 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 participants 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 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.[9]

The experimenters prepared the video messages according to the respective treatment, by matching receiver pairs and muting the sound on one or both of the videos. No other editing was done, so that each resulting video contained the complete two-minutes messages as recorded by the receivers. 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.[10] The order of videos was determined randomly. To avoid effects

[8]Thus, dictators increased a receiver's eventual 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.

[9]Out of 80 receiver participants, 54 chose the second rather than the first recording.

[10]In one set, the matching excluded those participants 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.

FIGURE 1
EXAMPLE OF THE VIDEO SCREEN WITH A RECEIVER PAIR

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,[11] dictator participants arrived and played in cohorts of 8 participants. Dictators received the same instructions as the receivers. They were informed about their role and treatment, i.e. whether or not they see 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 7-point 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

[11] In 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.

potential, respectively.[12] 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).[13]

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.[14] 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.

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

[12]The bipolar scales have two (related) advantages compared to one-sided Likert scales often used in economic experiments. First, they allow the raters to provide negative evaluations. Second, they avoid the positive bias of random noise associated with low evaluations made on a one-sided scale that is truncated at the zero point. German versions of different scales of Osgood et al. (1957) were tested in a pilot study using famous personalities as targets. The six scales used in the experiment proved to be the most reliable in the pilot. Appendix B contains the supplementary instructions given to participants for these ratings.

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

[14]Receiver 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.

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	$\lvert\frac{y-z}{p}\rvert$	$\sigma_x^2 \cdot 10^2$	$\sigma_y^2 \cdot 10^2$	$\sigma_z^2 \cdot 10^2$
Averages							
N	.671	.165		.076	.560	.312	
V	.646	.177		.095	.362	.408	.365
AV	.600	.211	.188	.103	.473	.516	.384
Mann-Whitney-U tests							
V vs. N	-	-		.004***	-	.065*	
AV vs. V	-	-	-	-	-	-	-
AV vs. N	.076*	.009***	-	.000***	-	.031**	-
Wilcoxon Matched Pairs Signed Ranks tests							
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. $\lvert\frac{y-z}{p}\rvert$ denotes the average total difference between y/p and z/p. $\sigma_x^2 \cdot 10^2$, $\sigma_y^2 \cdot 10^2$, and $\sigma_z^2 \cdot 10^2$ denote the average within-dictator variance of donations. 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. Compared to the baseline, donations increase in treatment V by 8% on average, and in treatment AV by 14% and 28%, for the non-talking and talking receiver, respectively. The self-allocations of dictators decrease 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 of the dictator indicated to know this receiver, and 0 otherwise.

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.

TABLE 2

Linear and linear mixed effects regression of receiver allocation on treatment, role, and evaluation

Model	1		2		3		4		5	
Type	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						
Aikaike 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.

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

Our analysis in the previous section revealed that purely visual communication leads to more discrimination both within and across receiver pairs, but has no significant effect on average donations. Only when also adding the audio channel, we observe a significant increase in donations. In order to further explore the mechanism underlying these results, we asked the dictators in our experiment to provide social ratings of the receivers they saw, and complement this data with similar ratings elicited from external raters, who were additionally asked to classify the message content. In this section we review the results from the internal ratings, and find that they are highly correlated with donations towards receivers. In the next section we review the data collected from the external raters, which show similar pattern and let us conclude that donations are driven by social evaluations and not vice versa.

To identify the essential dimensions underlying the six semantic differential rating scales, on which dictators rated receivers, we ran a series of principal components (factor) analyses.[15] These yielded an identical factor solution with two factors: The first factor (Eigenvalue 3.4) includes the scales corresponding to the original 'evaluation' and 'potency' scales, while

[15]Section D of the Online Appendix provides a short discussion and details of the analysis. The solution of the analysis is identical when performed separately for the talking and non-talking receivers.

the second factor (Eigenvalue 1.7) 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. A high score on the EP scale is thus associated with a person perceived as pleasant and strong, whereas the A scale reflects someone who is active and lively. The psychological literature on impression formation suggests that people are likely to form a general impression of a target person and respond to that impression rather than to specific attributes (Asch, 1946; Kelley, 1950; Nisbett and Wilson, 1977). Accordingly, we interpret the two factors as two dimensions of the general impression made by the receiver, and test whether dictator decisions are influenced by this impression.[16] The average ratings of receivers on those factors are presented in Table 3.

TABLE 3
AVERAGE RATINGS ON THE EVALUATION AND THE ACTIVITY FACTOR

Treatment	EP_y		EP_z		A_y		A_z	
Averages								
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)
Pearson correlations with corresponding allocations								
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, measured on a scale from -3 to 3. Standard deviations are given in parentheses. Pearson correlations are two-sided.*,**,*** indicate significance on the 10%, 5%, 1% level, respectively.

The average ratings do not differ significantly between treatments or between roles. Mann-Whitney-U tests for between-treatment comparisons and Wilcoxon signed-ranks tests for between-role (within-treatment) comparisons yield $p > .5$ in all cases.[17]

[16] Evaluation can be taken to reflect a more subjective impression, while activity is driven by receiver's *actions*, which can be assessed in a more objective way (for example, the duration of speech). See Footnotes 17 and 23.

[17] When including the sessions in which the talking receivers were chosen for being more

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). However, some of the correlations might be due to intercorrelations between the different ratings. These intercorrelations are 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 weaker than for non-talking receivers. The latter result might arise if the effect of activity is less crucial for donations if role-induced, or if a talking receiver is perceived as too assertive to her disadvantage.

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 cannot 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 pairs[18] and rated them on the same scales as the

talkative the talking receiver is rated higher on the Activity factor than the non-talking receiver in treatment AV.

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

dictators.[19] In order to incentivize the judgments, the judges were paid according to their ability to match the (rounded average) internal ratings of the dictators.[20]

The first session was conducted to obtain unbiased ratings, by asking judges to rate the silent videos used in treatment V of the experiment. As judges were not informed about any details of the experiment and 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.

The external ratings obtained for the silent videos ("unbiased external V impressions") validate the internal ratings made by dictators in the experiment.[21] As data row 2 in Table 4 shows, for both factors we find high correlations between the internal and unbiased external ratings of receivers.[22] 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 a causal interpretation of Result 3,

[19]The experimental instructions for the external ratings sessions can be found in Appendix C.

[20]Specifically, 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. Incentivizing subjective reports based on correspondence to reports made by others was also used in order to elicit social norms by Krupka and Weber (2009), who used concurrent choices as a criterion for payoff, thus creating a coordination game, in which social norms serve as a focal point. In our case the criterion is external to the rating session, so that responses should not depend on any strategic considerations.

[21]The 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.

[22]For 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

	Treatment V		Treatment AV	
Internal Rating	*EP*	*A*	*EP*	*A*
Direct correlation with				
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***
Partial correlation with allocation, controlling for				
(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.

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.[23] The ratings obtained from the judges who knew the experimental rules and could hear the receivers ("external AV impressions") provide similar results.

Result 4 *The measured social impressions of the receivers by the dictators are valid and largely driven by unbiased perceptions. Thus a causal conclusion can be drawn: the dictator decisions are, to a large extent, driven by the general impression made by the communicating receiver.*

[23]We attribute the lack of significant partial correlation for the A scale to activity being more objectively measurable, see Footnote 16.

In addition to rating the impression made by the receivers, the judges for the treatment AV videos were asked to classify the *content* of the message sent by the talking receiver on a Guttman-type scale. More specifically, the judges were asked to choose the statement that best describes the content of the message out of the following: 'did not talk about the experiment', 'talked about the experiment', 'talked about the rules of the experiment', 'talked about possible distributions of the money', 'talked about which distributions should be chosen', and 'suggested an equal split of the money'. Inter-judge reliability for this scale is 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).

Table 5 shows the distribution of the message content classification by the external judges.[24] Interestingly, only about a third of the communicating receivers made a reference to a possible distribution, or suggested one.[25] We found no systematic effect of the message content on allocations towards the communicator. The mean allocation to a talking receiver was not correlated with the modal ratings of her message's content (r=0.112, $p = 0.540$). Donations to receivers who mentioned possible allocations were not significantly higher than to receivers who did not mention possible allocations at all (21.6% of the pie vs. 20.2%, $p = 0.675$, two-sided Mann-Whitney-U test). Similarly, message content was not correlated with their social ratings provided by the external judges and the dictators (all p-values larger than 0.45). Thus, discussing the game and suggesting specific allocations did not affect the average allocations made to the talking receiver in any consistent way.

[24] Additionally to the distribution of all submitted evaluations we also calculated the median and modal rating per video. If the median would fall between two categories, or if there was a tie between two categories in the mode calculation, then we took the lower rating. This was only the case for 6 median and 2 mode calculations out of the 32 videos, respectively. The mode and median were highly correlated (Spearman's ρ=0.946, $p < 0.001$). The results show that the distribution is robust to the way we calculate it. Below we use the mode as the most natural aggregate over classifications; using the median in the analysis does not yield a different conclusion.

[25] The numbers would be 38% and 34% if we used rounding up rather than rounding down for median and mode classifications, respectively.

TABLE 5
DISTRIBUTION OF MESSAGE CONTENT CLASSIFICATIONS PROVIDED BY EXTERNAL JUDGES

Classification	All	Median per video	Mode per video
Did not talk about the experiment	23%	22%	22%
Talked about the experiment	37%	34%	44%
Talked about the rules of the experiment	7%	13%	3%
Talked about possible distributions of the money	13%	16%	6%
Talked about which distributions should be chosen	11%	13%	13%
Suggested an equal split of the money	10%	3%	13%
Last 3 categories	33%	31%	31%

These results line up well with the finding in the psychology literature that, across various domains, people make (reliable) judgments based on a general impression formed on the basis of minimal information rather than on directly relevant content (Ambady, Bernieri and Richeson, 2000; Ambady and Rosenthal, 1992). Exposure to as little as ten seconds of video recordings typically leads to responses similar to those made on the basis of abundant information and to objective criteria in, for example, clinical judgments (Mintz and Luborsky, 1971; Oltmanns, Friedman, Fiedler and Turkheimer, 2004), assessments of teachers' performance (Ambady and Rosenthal, 1993; Babad, Avni-Babad and Rosenthal, 2004) and perceptions of politicians (Benjamin and Shapiro, 2009). Our result extends these previous findings to economically relevant decisions in a controlled experiment.

III.D Perception of fairness

At the end of the dictator sessions, when all decisions were made, we asked participants what they consider would be a 'fair allocation'. Participants across the three communication conditions treatments agreed on this aspect:

overall, 68.3% of all dictators indicated an allocation closest to equal split;[26] the two receiver shares differed by more than the minimally allowed 1 Euro in only 2 out of 120 'fair' allocations (both cases in treatment AV).

The share of dictators choosing a near-equal allocation as fair is 77.5% in the baseline treatment, 55.0% in treatment V, and 72.5% in treatment AV. Although the differences in shares 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 participants who indicated that in a fair allocation the dictator gets the maximum amount, and acted accordingly in all 8 rounds. If we drop these observations then the correlation disappears (Spearman's rho=.091, $p = .334$). Also, "greedy" participants 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.[27]

Result 5 *Unilateral 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

The contributions of this paper are twofold. First, we replicate existing results on communication effects in an experimental environment using video

[26] Providing 6 Euro to the dictator and one of the receivers and 5 Euros to the other receiver.

[27] Previous studies have found that random allocation to roles creates a self-serving bias in perception of fairness (Babcock and Loewenstein, 1997; Babcock, Loewenstein, Issacharoff and Camerer, 1995; Loewenstein, Issacharoff, Camerer and Babcock, 1993), particularly when different fairness norms are available (Gächter and Riedl, 2005; Konow, 2000).

technology and excluding any strategic effects. Second, our analysis of multiple receivers and impression formation allows us to reach stronger conclusions compared to previous studies.

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 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. Further, 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). Our innovative design and use of video technology thus allow us to explore the effects of communication on social giving and social preferences in more detail, while excluding potential side effects (such as reputation, etc.) present in most experiments on face-to-face communication in the literature.

We find that unilateral pre-play communication in the three-person dictator game inspires generosity by dictators. The effect is significant only for audio-visual communication. Interestingly, 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. Nonetheless, the effects are quite substantial when compared to the (meagre) baseline donation level observed without communication, with almost 30 percent increase in mean donations to a receiver that is heard.

Our findings on mean donations replicate the emerging consensus in the literature that mere visual exposure does not affect social giving. Similar results were obtained by Brosig et al. (2003) in video experiments, Bohnet and Frey (1999) in classroom games, and in the experimental literature on the related issue of identifiability (Charness and Gneezy, 2008; Jenni and Loewenstein, 1997; Small and Loewenstein, 2003). The differential effect for

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

However, by looking at dictator behavior in a game with two receivers, our analysis can go beyond the mean donations to study more complex social effects of (silent) communication. We find that dictator discrimination significantly increases with the addition of the visual channel, thus providing evidence that dictators are responding to the communication, even if they do not generally become more generous towards receivers.

Given that a dictator cares about the welfare of a receiver who is heard, the relevance of this effect for the dictator's allocation to another receiver is not readily clear. On the one hand, the effect may benefit also the other receiver. On the other hand, the dictator may set a 'fixed total sacrifice' which is then divided between the receivers (Büchner, Coricelli and Greiner, 2007; Selten and Ockenfels, 1998), implying that an increase in one receiver's share decreases the other's share. In the current experiment we observe 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 receiver, she does so at her own expense.

We find that social ratings of receivers, elicited from dictators, are highly correlated with donations towards the receivers, suggesting that impression formation plays an important role in the effect communication. This is particularly also true for silent communication. The effect is robust even though dictators make repeated allocations to multiple receivers, a design which is

expected to reduce the effects of vividness and identifiability (Kogut and Ritov, 2005). Thanks to our use of the video technology, we are able to validate this conclusion through external ratings of the video messages. We establish that the perception of the receiver precedes and affects the donations made to her, rather than vice versa, as the converse causal direction can exist only in the internal ratings data. Furthermore, it is indeed a pure social perception that plays the main role in this effect, as it is present even for judges who are not aware of the strategic characteristics of the game. We find no evidence that the communication-specific content, in addition to a general impression formation, has a systematic effect on dictators' decisions.

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 social impression formation and 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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ONLINE APPENDIX for *Social Communication and Discrimination: A Video Experiment*

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

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

Appendix B: Ratings and Rating Instructions

(translated from German)

Elicited ratings

active	o	o	o	o	o	o	o	passive
weak	o	o	o	o	o	o	o	strong
pleasant	o	o	o	o	o	o	o	unpleasant
dull	o	o	o	o	o	o	o	lively
unattractive	o	o	o	o	o	o	o	attractive
not influential	o	o	o	o	o	o	o	influential

Rating instructions

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 X o o o o o o passive
active o o o o o o X 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 o X o o o o o passive
active o o o o o X o 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 o o X o o o o passive
active o o o o X o o 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 o o o X o o o 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 (square) brackets appeared only for the ratings of the silent (audible) videos, respectively.)

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	o	o	o	o	o	o	o	passive
weak	o	o	o	o	o	o	o	strong
pleasant	o	o	o	o	o	o	o	unpleasant
dull	o	o	o	o	o	o	o	lively
unattractive	o	o	o	o	o	o	o	attractive
not influential	o	o	o	o	o	o	o	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.

Appendix D: Principal component analysis of social ratings

Principal component analysis uses the correlations between different variables in order to reduce their number to a small number of meaningful 'dimensions'. The social ratings obtained in the experiment include six different scales, which are theorized to correspond to three separate factors. Accordingly, we use a principal components analysis to validate the factors and test whether they can be further reduced to composite factors.

Principal component analyses based on the correlation matrix were conducted on the six rating scales, averaged for each recipient over the different dictators in the V and AV treatments. To test the robustness of the results,

we conducted separate analyses over all recipients, separately for talking and non-talking recipients, and using different rotation methods (Abdi and Williams, 2010; Joliffe, 2002).

The analysis yields two factors. The first factor, with an Eigenvalue of 3.4, explains 56.1% of the variance, while the second factor, with an Eigenvalue of 1.7, explains 28.0% of the variance. The interpretation of the two factors is facilitated by considering the loadings of the six scales on the two factors in the rotated component matrix (see Table D.1 and Figure D.1). The two scales associated with Evaluation (attractiveness and pleasantness) and the two scales associated with Potency (strength and influence) load high on the first factor. The remaining two scales, associated with Activity (activity and livelihood) load high on the second factor. The Potency scales moderately load on the second factor, but bear more affinity with the Evaluation scales, and were therefore combined with them to generate a single factor. The analysis yields similar results when conducted separately for the talking and non-talking recipients .

References

Abdi, H. and Williams, L.J. (2010), 'Principal component analysis', *Wiley Interdisciplinary Reviews: Computational Statistics* **2**, 433-459.

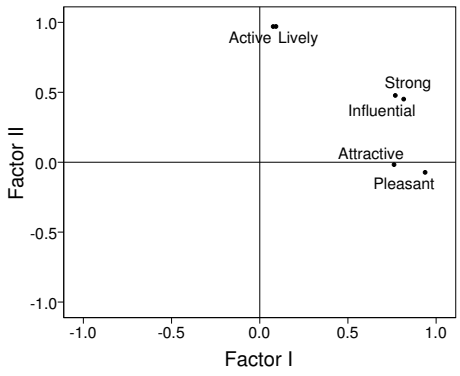
Jolliffe, I.T. (2002), *Principal Component Analysis*, 2nd ed., New York: Springer.

TABLE D.1
COMPONENT MATRICES

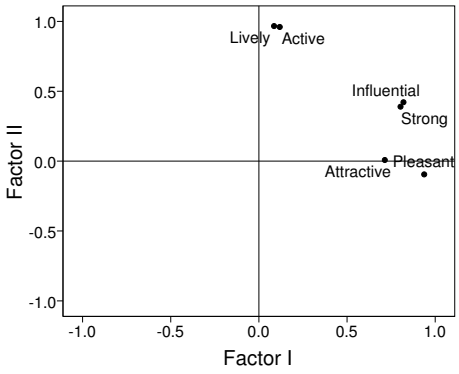
	All recipients					
	Unrotated		Varimax rotation		Quartimax rotation	
	Factor I	Factor II	Factor I	Factor II	Factor I	Factor II
Attractive	.589	-.482	.761	-.017	.759	-.059
Pleasant	.694	-.634	.937	-.072	.932	-.124
Strong	.899	-.097	.768	.477	.793	.434
Influential	.921	-.147	.816	.451	.840	.405
Active	.657	.718	.076	.970	.129	.965
Lively	.670	.709	.092	.971	.145	.964
	Talking recipients					
	Unrotated		Varimax rotation		Quartimax rotation	
	Factor I	Factor II	Factor I	Factor II	Factor I	Factor II
Attractive	.586	-.408	.714	-.008	.714	-.033
Pleasant	.709	-.621	.938	-.095	.931	-.149
Strong	.881	-.149	.803	.390	.825	.342
Influential	.913	-.133	.820	.422	.843	.373
Active	.654	.713	.118	.960	.174	.952
Lively	.631	.737	.086	.967	.142	.960
	Non-talking recipients					
	Unrotated		Varimax rotation		Quartimax rotation	
	Factor I	Factor II	Factor I	Factor II	Factor I	Factor II
Attractive	.657	-.468	.806	.018	.806	-.031
Pleasant	.699	-.621	.932	-.079	.925	-.136
Strong	.917	-.085	.785	.481	.813	.433
Influential	.932	-.158	.842	.431	.866	.379
Active	.650	.737	.079	.979	.139	.973
Lively	.687	.710	.125	.980	.185	.971

FIGURE D.1
FACTORS IN ROTATED SPACE (VARIMAX ROTATION)

(a) All recipients



(b) Talking recipients



(c) Non-talking recipients