A cooperative culture may affect trustworthiness assessments derived from the human face

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Abstract

Trust is crucial for successful social interactions and personal well-being. Among the many mechanisms potentially favoring the evolution of trust, spillovers of social institutions on subsequent behavior have received little attention within evolutionary psychology. Although a plethora of research has investigated pro-social spillovers in economic experiments, research has thus far largely overlooked the impact of spillovers on person perception. Additionally, given that most studies on such spillovers use one-off measurement of the subsequent behavior, the stability of pro-social spillovers over time has not been investigated so far. In a three-stage laboratory experiment ($n = 208$), we provide evidence that pro-social spillovers occur even when perceptual cues provide an additional source of information to base decisions on. In Stage 1, participants played a series of trust games against unknown, videotaped targets to assess their baseline trust behavior against strangers, taking into account their trust perception. Subsequently, we randomly exposed them to a repeated prisoner's dilemma in which cooperation is either favored ("C-culture") or discouraged ("D-culture"). In the final stage – another series of 30 trust decisions against different targets – participants from the "C-culture" initially respond with higher levels of trust. This spillover effect, however, is short-lived and behavior converges back to the pre-intervention levels. Thus, our findings confirm pro-social spillovers even when participants behavior may also be influenced by perceptual cues but raise the question about their temporal stability. We complemented the experiment with an independent survey ($n = 132$) on the trustworthiness of the targets. Results show that these perception affects quickly gain predictive quality, suggesting the intervention effect only temporarily crowds out the trustworthiness perception as a key driver of trust behavior.

Keywords: behavioral spillovers; face perception; repeated prisoner's dilemma; trust; trust game; trustworthiness

1. Introduction

Trust is regarded as essential for a well-functioning society (Dunning et al., 2019) and for individual well-being (Weiss et al., 2020), but trusting exposes people to the threat of being exploited by “free-riding” individuals. As the exploitation of trust is often conceptualized as the “rational decision” in behavioral models grounded in rational choice and as such (seemingly) favored by natural selection (Rotics & Clutton-Brock, 2021), the evolution of trust and cooperation in humans and other species is a core research interest across many scientific disciplines, particularly evolutionary psychology (Fetchenhauer et al., 2010; Pradel et al., 2009).

Like any other social exchange, trust begins with the perception of the counterpart (Moskowitz & Gill, 2013), and in order to cooperate successfully, individuals have to assess others’ trustworthiness. Recent research emphasized the importance of perceiving others’ trustworthiness when it comes to people’s actual, real-world trust decisions (Weiss et al., 2020). Although trust also depends on stable inter-individual differences in the dispositional propensity to trust, situational variation accounts for the largest portion of the variance in trust decisions in everyday interactions. Thus, real-life trust decisions seem substantially influenced by the perception of the other person’s trustworthiness. And in fact, being perceived as trustworthy considerably affects important life outcomes, among them voting success in democratic societies (Franklin & Zebrowitz, 2016; Little et al., 2007), sentencing decisions in criminal courts (Wilson & Rule, 2016), or personal income (Fruhen et al., 2015). But what determines the perception of trustworthiness?

Research has thus far mostly investigated variables related to the interaction partner and how these affect the perception of her trustworthiness. As the face is the main source of visual data for the emotions of people (Todorov & Oosterhof, 2011), research has suggested that people infer traits, including trustworthiness, largely from facial features (see Walker & Vetter, 2009, or Todorov, 2012, for an overview). These inferences from facial features generally happen unconsciously (Stewart et al., 2012) and effortlessly (Bonnefon et al., 2013), typically evidenced by the speed with which such judgments are reached (Olivola & Todorov, 2010). That said, traits related to survival in an evolutionary

sense are judged the fastest (Bar et al., 2006). As trustworthiness is regarded as such a core trait, it does not surprise that it is also assessed in less than a second (De Neys et al., 2017; Klapper et al., 2016), with no improvement when taking more time (Todorov et al., 2009). Which features underlie these judgments has been the topic of much research (e.g., Dotsch & Todorov, 2012, Ewing et al., 2015, Todorov et al., 2015, and Yang et al., 2011). Factors associated with higher perceived trustworthiness include being female (Dong et al., 2018; Wincenciak et al., 2013), lower facial width-to-height ratio (i.e., as higher width is linked to testosterone-based male aggression, see Stirrat & Perrett, 2010), higher face-typicality (Sofer et al., 2015), genuine smiles (Centorrino et al., 2015), or momentary positive mood (i.e., happiness, see Sutherland et al., 2017).

Notwithstanding the importance of these findings, research has thus far largely overlooked the effects of situational cues stemming from the social environment, in particular how cooperative versus non-cooperative experiences shape judgments of human trustworthiness. In other words: Are trustworthiness ratings affected by incidental (i.e., cultural or situational) exposure to either a high or low trust environment? The present research aims to answer this question through a three-stage controlled behavioral experiment in which we are able to infer changes in trust perception following random assignment to either a cooperative or non-cooperative environment that leaves the “objective” beliefs regarding interaction partners unaffected.

On a purely behavioral level, previous research points to strong effects of exposure to cooperative situations, largely stemming from experimental economic research on “spillover effects” of cooperation-enhancing institutions. Humans are a species frequently “institutionalizing” trust and cooperation by means of social rules and practices that facilitate cooperative solutions. Cooperation-enhancing institutions include the opportunity of punishing free-riders (Fehr & Gächter, 2002), ostracism (Feinberg et al., 2014), repeated interaction with the same individuals to exploit reputational information (Trivers, 1971), or other forms of local governance approaches (Ostrom, 1990). Importantly, institutional solutions overcoming the “tragedy of the commons” (Hardin, 1968) typically have a twofold effect, combining the (direct) effect of people cooperating to avoid being subjected to

punishment or ostracism and the (indirect) effect of changing people's beliefs about others' cooperation (Shinada & Yamagishi, 2007). Intriguingly, cooperation-enhancing institutions attempting to secure the provision of public "goods" or to inhibit the creation of public "bads" are so effective that they even quickly emerge endogenously in highly stylized cooperation games (Gürerk et al., 2006). Once such institutions are in place, individuals governed by these institutions share trust, expected trustworthiness, and actual trustworthiness (Fehr, 2009).

That said, institutions are also "weak" as they are – by design – limited in scope (Engl et al., 2017). Even very strong institutions cannot secure cooperative behavior fully as much of people's behavior remains outside of the reach of the institutions. Researchers have therefore developed an interest in how institutions indirectly affect behavior and have pointed to considerable "spillover effects" of institutions to outside situations.

Broadly speaking, a spillover effect occurs when an institution affects behavior outside of it in the "spirit" of that institution (Galizzi & Whitmarsh, 2019). In a laboratory context, this may encompass a cooperative decision in a prisoners' dilemma immediately after having played a public good game with strong cooperation-inducing institutions such as punishment. Through a spillover effect, decision makers may continue to behave pro-socially even after the pro-social institution has been revoked (e.g., Engl et al., 2017, and Peysakhovich & Rand, 2016). Economic games, however, largely occur under conditions of anonymity and in front of a computer where perceiving the interaction partner is made (purposefully and by design) impossible. As a result, research has not yet investigated how an incidental exposure to a "culture of cooperation" affects subsequent trust perception and decision-making based thereon. The present research aims to contribute to filling this research gap.

2. The current research

Aiming to answer the question about how incidental exposure to cooperative or non-cooperative experiences shapes expected trustworthiness, the present research relies on a tightly controlled laboratory experiment. Thereby, we observe trust decisions made based on facial features without

compromising anonymity. In particular, in a three-stage experiment, we first measure a baseline of people's trust through incentivized bets against the (actual) trustworthiness of a series of video-taped target persons (Stage 1). Then, we selectively expose our experimental participants to either a cooperative or deceptive experience by manipulating the incentives to cooperate in stochastically repeated prisoners' dilemmas (Stage 2). Finally, we observe their perceptual/behavioral reaction in decisions against another set of target persons (Stage 3). Based on the literature pointing to considerable spillover effects in social choice (see Table S1 in the Supplementary Material for an overview of the most relevant work on pro-social spillovers), we expect a reaction of trust decisions depending on the experimental condition. In addition, our repeated measures design allows us to observe the durability of these effects.

3. Methods

3.1. Open science statement and research ethics

All materials, data, and code to replicate the statistical analyses are available on the Open Science Framework ([original link redacted for peer review, anonymized link for review: https://osf.io/n5zu8/?view_only=9c61f56e05bb4dc78804694d5e92a023]). We confirm that we report all experimental conditions. The main hypothesis, the sampling decisions, and the procedure to identify a behavioral spillover effect have been pre-registered. In addition, all potential data exclusions follow the pre-registered protocol. The study received approval from the Ethics Committee of the [redacted for peer review] (approval number: 112019). The study followed the Declaration of Helsinki and all participants gave written informed consent. Our study did not involve deception. Participants have received a lump sum payment for participation plus the behavior-dependent payoff resulting from three stages of the experiment. Average payment for study participation was 28.97 [local currency, redacted for peer review, equivalent to 31.47 USD]. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

3.2. Stimulus material (“Trust targets”)

The experiment involved trust decisions in two stages (Stage 1 and Stage 3, see below). Trust decisions were made against video-taped individuals, which we refer to as “targets”. Each target was videotaped prior to our experiment. Each video showed a person from a previous independent experiment (similar to Fetchenhauer et al., 2010, Fetchenhauer & Dunning, 2010, and Gollwitzer et al., 2012) introducing him- or herself to the camera. The videos shown and their order was chosen randomly from a sample of 168 videos available to the researchers. Videos were identical for all participants. In Stage 1 as well as Stage 3, our experimental participants watched 30 muted¹ videos, each 15 seconds long, displayed on a large screen in the front of the laboratory. Targets’ trustworthiness had been elicited from an actual trust game using the strategy method in an independent lab prior to the experiment. Targets initially made the decisions to return any integer amount between zero and double the points received for each possible amount received from a potential trustor (integer between one and four points). In the context of our experiment, the targets’ trustworthiness was thus predetermined, but unknown to the participants.²

As an additional measure of trustworthiness and friendliness of each target, we had an independent sample of volunteers evaluate the trustworthiness and friendliness of the 60 targets ($n = 132$, 51% female, $M_{age}=22$, $SD_{age}= 2.30$). These volunteers were recruited in a large lecture on business administration and viewed the same set of silent videos as the experimental participants. For each target, respondents were asked to rate the trustworthiness and the friendliness of the targets on 5-point Likert scales (see Supplementary Material for the full survey). Survey respondents did not receive behavior-dependent compensation. Based on these ratings, all targets received an average score regarding their trustworthiness and friendliness.

Targets in Stage 1 (pre-intervention) and Stage 3 (post-intervention) did not systematically differ. Specifically, there were no statistically significant or otherwise meaningful differences in terms of gender distribution (57% vs. 50% female targets; $\chi^2(1, n = 60) = 0.07, p = 0.80$), average trustworthiness ($M_{first-stage} = 3.26$ and $M_{third-stage} = 3.21$; Wilcoxon rank-sum test: $W = 483.5, p = 0.63$), or average

friendliness ratings ($M_{\text{first-stage}} = 3.52$ and $M_{\text{third-stage}} = 3.35$; Wilcoxon rank-sum test: $W = 513.5$, $p = 0.35$).

Furthermore, the distributions of independent trustworthiness and friendliness scores in Stages 1 (pre-intervention) and 3 (post intervention) are close to identical (see Figure 1).

Figure 1

Density of targets' trustworthiness and friendliness scores

3.3. Participants and procedure

The experiment was conducted at the [redacted for peer review] lab of the [redacted for peer review] between the 8th and the 10th of October 2019. It was implemented with z-Tree (Fischbacher, 2007). We recruited 208 participants (aged 18-44, 113 female) from the participant pool of the [redacted for peer review] lab who each participated in one of 13 sessions. For all three experimental stages, instructions were provided to the participants on-screen (see Supplementary Material for the full experimental instructions). Participants only saw the instructions for the current stage (they were informed in the beginning that the experiment consisted of three stages). In the first and second stage, control questions were asked to ensure participants understood the instructions. In case a participant did not answer a question correctly, he or she received another set of verbal instructions.

Stage 1 (Baseline trust assessment). In the first stage, we elicited participants' baseline trust behavior. Participants made decisions in response to viewing 30 trust targets. Each decision involved an incentivized bet against the trustworthiness of the target mimicking the payoff of a trust game (see Jaeger et al., 2020, for the identical method). An incentivized bet has the advantage that behavior measures trustworthiness with lower risk of socially desirable answering patterns and circumvents the risks compared to an actual trust game that people make trust decisions based on norms. Prior research in social psychology points to the fact that people send money in a trust game as a means of expressing a social norm even when they believe the interaction partner will *not* return any money (Dunning et al., 2014). Each participant received an endowment of 100 ECU (= 0.25 [local currency, redacted for peer review, equivalent to 0.27 USD]) per decision and had the option to "invest" 0, 25, 50, 75 or 100 ECU. Participants only received feedback about their total earnings at the end of the stage. Thus, they made 30 decisions without the ability to learn or update their strategies based on feedback.

Stage 2 (Experimental manipulation). The second stage involved the experimental manipulation. Participants were randomly assigned to an experimental game favoring the emergence of cooperation or favoring deception. The treatment manipulation was adopted from Peysakhovich and Rand (2016) and exactly half of the participants in each session were enrolled into either experimental condition through a pre-defined mechanism assigning computer seats into a condition. Participants randomly drew a number assigning them to a computer seat when entering the laboratory. Thus, our method ensured randomization into experimental conditions. Randomization checks confirm that there were no substantial differences in age and gender distribution between experimental conditions, nor did earnings in Stage 1 differ significantly (see Table S2 in the Supplementary Material).

After enrollment into the experimental conditions, participants played stochastically repeated prisoner's dilemma (PD) games in randomly matched pairs with members from the same experimental condition. The matching procedure was random imperfect stranger matching.³ In each single PD, the paired participants simultaneously choose an action, "cooperate" or "defect". In order not to prime

participants, the actions were labeled “A” and “B” rather than “cooperate” or “defect”. The interaction with the same partner continued with probability δ . With probability $1 - \delta$, the interaction ended, and participants were randomly matched to a new partner. The experimental manipulation was implemented through variation of the payoff parameters and the probability to continue another round with the same individual (like in Peysakhovich & Rand, 2016). First, the “gain from defection” was varied to be larger in the D-culture (50% of payoff if both cooperate (= 40 ECU), versus 25% (= 20 ECU) in the C-culture, see Table 1). Second, the continuation probability was higher in the C-culture ($\delta = 7/8$, versus $\delta = 1/8$ in the D-culture).

Table 1

Payoffs in prisoner's dilemma, by experimental condition

C-culture	C	D	D-culture	C	D
C	80, 80	0, 100	C	80, 80	0, 120
D	100, 0	20, 20	D	120, 0	20, 20

To control for random variation in game lengths between sessions we generated one set of game length for each of the treatments using a geometric distribution with parameter $p = 1 - \delta$ (following the procedure of Dreber et al., 2008, Fudenberg et al., 2012, Peysakhovich & Rand, 2016, and Rand et al., 2015). Participants played 40 rounds in total, split into 8 supergames in the C-culture and 34 supergames in the D-culture (see Table S3 in the Supplementary Material for the specific game lengths used).

To account for expected earning differences (i.e., cooperative behavior yields higher payoffs), participants received differing initial endowments (550 ECU in the C-culture and 1600 ECU in the D-culture). Hence, participants ended this stage with a similar expected income. In doing so, we ensured differences we observed later are not due to lower income by the D-culture participants.⁴

Stage 3 (Post-intervention trust). Stage 3 was identical to Stage 1, but with a different set of 30 trust targets. As in Stage 1, participants made a series of 30 incentivized bets with respect to the trustworthiness of a novel set of target individuals.

4. Results

4.1. Manipulation check

The manipulation proved effective. We observe consistently higher levels of cooperation in the C-culture compared to the D-culture when looking at the share of cooperative decisions in each round of Stage 2 (see Figure 2).

Figure 2

Share of participants cooperating in every round of the repeated prisoner's dilemma in the intervention stage, with locally estimated (LOESS) 95% confidence intervals

To show the efficacy of the experimental manipulation (i.e., higher cooperation rates in the C-culture), we estimated logit regressions of the decision to cooperate on the experimental conditions (dummy-coded: 1 = C-culture), both uniquely involving the experimental condition (Table 2, Model 1) and including the decision number and their interaction (Table 2, Model 2). The decision number

serves as a proxy for the duration of experiencing the C-culture or D-culture. As additional robustness checks, we control for participants' age and gender as well as comprehension of the study instructions. The efficacy of the manipulation remains significant after including these controls (see Table S4 in the Supplementary Material).

Table 2

Regressions of decision to cooperate dummy on C-culture dummy (Model 1) and interacting with decision number (Model 2)

Variable	Model 1			Model 2		
	OR	SE	p	OR	SE	p
Intercept	0.44	0.09	<0.001	0.60	0.11	<0.001
C-Culture	3.33	0.94	<0.001	3.01	0.87	<0.001
Decision number				0.99	0.00	<0.001
C-Culture X Decision number				1.01	0.01	0.537
Observations		8320			8320	
Individuals		208			208	
R ² Tjur		0.084			0.088	

Note: Table displays odds ratios (OR) from logit regressions with decision to cooperate dummy (defection as base) as the dependent variable. Standard errors (SE) are clustered at unique treatment group level, i.e. every treatment group in every session. Bold p-values indicate significance at the 0.05-level.

4.2. Main results

4.2.1. No behavioral differences in pre-intervention behavior

We first confirm that a-priori behavior (Stage 1) is identical between the two experimental conditions. As participants have not experienced any intervention at this stage, any differences we observe between participants are due to their intrinsic characteristics. As the allocation to treatment groups was randomized, we should not observe any systematic differences in behavior between treatment groups before Stage 2. The regressions of baseline contribution on the experimental manipulation, its interaction with the decision round, as well as under demographic and

comprehension controls do not suggest significant differences prior to the intervention (see Table S5 in the Supplementary Material).

4.2.2. Post-intervention effects

To identify the existence and stability of an effect of the experimental manipulation on Stage 3 trustworthiness assessments, we analyze whether the intervention changed participants' decisions following the exposure to the C- or D-culture and how stable this behavior change is. Figure 3 displays the main results and shows an initial spike in the behavioral difference between the C-culture and D-culture individuals, immediately following Stage 2. However, this initial behavioral change quickly returns to the pre-intervention baseline. This result shows that perceptual spillover effects are caused by the differential exposure to a cooperative versus non-cooperative environment, but also re-converge back to the level observed in the pre-intervention stage.

Figure 3

Difference between average trust game contributions in C-culture and D-culture in Stage 1 and 3, with locally estimated (LOESS) 95% confidence intervals

Regression results confirm this finding. The models show that the C-culture dummy has no significant effect by itself, but a significant interaction effect between C-culture dummy and decision round corroborates the finding displayed in Figure 3. Table 3 displays the regression results. Additional models presented in the Supplementary Material show the robustness of these results to including various additional control variables or changing the model specifications (see Tables S6 – S8 in the Supplementary Material for details).

Table 3

Linear regressions of post-intervention TG contribution on C-culture dummy and average baseline contribution (Model 1), and interacting with decision number (Model 2)

Variable	Model 1			Model 2		
	B	SE	p	B	SE	p
Intercept	0.20	0.11	0.066	-0.34	0.14	0.015
C-Culture	0.04	0.07	0.596	0.48	0.18	0.007
Baseline contribution	0.91	0.04	<0.001	0.91	0.04	<0.001
Decision number				0.01	0.00	<0.001
C-Culture X Decision number				-0.01	0.00	0.012
Observations	6240			6240		
Individuals	208			208		
R ² / R ² adjusted	0.336 / 0.336			0.339 / 0.339		

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Standard errors (SE) are clustered at participant level. Bold p-values indicate significance at the 0.05-level.

Finally, to show how the experimental manipulation temporarily (partially) “crowds out” decision-making based on perception, we test how predictive the independent trustworthiness score is of actual, incentivized decision-making (using the trustworthiness score as a proxy for the trustworthiness perception of the targets). And in fact, trustworthiness scores become increasingly predictive of behavior over time. Whereas the effect is rather small immediately following the experimental manipulation, the effect becomes stronger in later rounds (see Figure 4 for a depiction,

and Table S9 in the Supplementary Material for the associated regression models). That said, even the small slope of trustworthiness perception in the early rounds of the post-intervention stage is still as high as the corresponding high slope of the C-culture dummy (0.21 vs. 0.19, see Figure 4).

Figure 4

Johnson-Neyman plots of the change in effect of the C-culture dummy (top panel) and the trustworthiness score (bottom panel) over the post-intervention rounds

In conclusion, when looking at the effect of the intervention on trust decisions, we observe that there is a positive and significant effect of the C-culture treatment on trust behavior immediately following the intervention. Yet, this effect quickly converges back to the pre-intervention level, suggesting that the durability of the cultural exposition is rather low. Trustworthiness perception, on the other hand, quickly increases in predictive power after the intervention.

5. Discussion

We present evidence from a laboratory experiment that institutions favoring cooperation cause spillovers to subsequent trust behavior even when strong perceptual cues are present. Exposure to a cooperative institution increases trust in a series of decisions against video-taped individuals

immediately after the intervention. In fact, our evidence even suggests that the imprinting of the institution partially replaces trustworthiness perception as the primary source of information when making trust decisions immediately after the intervention. As our second main finding, however, we observe the effect does not persist, and the trust-enhancing impact quickly erodes, bringing behavior back towards the pre-intervention baseline. At the same time, the relevance of trustworthiness perception increases over the course of the post-intervention rounds.

Our paper contributes to at least two streams of the literature on pro-social behavioral spillovers: First, psychological research into person perception has accumulated considerable evidence about the importance of trustee perception that research into behavioral spillovers has thus far largely overlooked by focusing on measuring the spillover effects in anonymized interactions. We inform the discussion on the relevance and effectiveness of institutional spillovers by showing that spillovers also exist when perceptual cues are available.

Second, although research has repeatedly documented the existence of spillovers, they have rarely been investigated in terms of their longevity. Multi-disciplinary research on behavioral spillovers was mainly focused on identifying institutions that provide spillovers and environments where these spillovers take effect. Much like the quick erosion of the effects of increased reciprocity following gift-exchange in more naturalistic settings (e.g., Gneezy & List, 2006), the importance of spillovers may sometimes be exaggerated by research designs focusing on the immediate aftermath of the intervention. Our study adds to this body of knowledge by specifically shedding light on the temporal stability of these effects.

Additionally, we contribute to the literature on trustworthiness perception. Previous studies have already shown that trustworthiness perception is by no means immune to rather basic manipulation (e.g., Engell et al., 2010). Our results show that the manipulation of institutional norms – that are itself unrelated to the perception – even for only a few minutes during a laboratory experiment already changes the participants' otherwise perception-based trust behavior.

One important question we cannot fully answer with our research design, though, is whether the change in behavior stemming from the institutional spillover is caused by a *change* in trustworthiness perception of the targets or rather a partial *replacement* of the perception as a major source of information when taking the decision to trust. As we specifically chose not to measure participants' trustworthiness perception because we focused on the trust game contribution as a behavioral measure, we have no way of assessing the (potential) change in perception specifically. While the survey-based trustworthiness score is a valid proxy for the trustworthiness perception of the targets absent any intervention (given the sample sizes in experiment and survey and the similarity of participant pools we have no reason to doubt the resemblance in the aggregate perception of the targets' trustworthiness in both samples), we confine ourselves to observing that the perception temporarily has a lower influence on the trust behavior. That said, Figure 4 also cautions us not to overestimate the effect of the intervention as the magnitude of the slope of the C-culture dummy never exceeds that of the slope of the trustworthiness score. Additionally, both variables' impact is dwarfed by the strong predictive power (in both magnitude and significance) of the participants' individual propensity to trust (as measured by the average baseline contribution). Furthermore, it should be noted that our findings with respect to the intervention effect wearing off are somewhat difficult to translate to practical applications (experimental rounds as units of measure for time passed do not have an obvious real-world equivalent). While we can relate it to the duration of the intervention (based on Figure 3 and Figure 4, we can estimate that after 40 rounds of intervention, the effect starts declining after five post-intervention rounds), we can certainly not turn this into a meaningful recommendation such as "after a 40-day-intervention, you should expect people to behave differently for half a week".

Instead of allowing obvious practical inferences, however, our results set the basis for various avenues of further research on the robustness and longevity of pro-social institutional spillovers. One promising area is the identification and evaluation of drivers and inhibitors of spillover persistence. Specifying and comparing the duration of different pro-social spillovers would also be important to

investigate in the future. Furthermore, we strongly encourage further research into the specific processes that lead to institutional spillovers to take effect on trust behavior. Only when these additional pieces are added to the understanding of the efficacy of institutions with respect to spillovers can institutions be designed to not only generate desirable spillovers but also make them last.

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Footnotes

¹ Vogt et al. (2013) have validated this “thin slice” (see Ambady & Rosenthal, 1992) approach and investigated the differences between muted videos and videos with the sound switched on. They did not find any difference, hence we decided to work with muted videos only.

² This also implies that the behavior of our participants had no implications on the payoffs of the trustees as they were part of a different and already finished experimental procedure.

³ Although this approach implied the possibility of rematches with the same partner, we chose it for two reasons. First, Peysakhovich and Rand (2016) show that around 20 rounds of this type of intervention are needed to establish a clear pattern of cooperativeness or non-cooperativeness. Given that we could have only 8 participants per treatment in every session, perfect stranger matching was thus not possible. Second, as participants did not learn anything about their partner (the partners were only identified as the “other person”) there was no possible way any kind of reputation building could happen. After being informed that a new matching had taken place both participants in any given interaction would expect a new partner and behave accordingly even if the new partner was in fact the same as in the previous interaction (or any past interaction).

⁴ Participants in the C-culture had indeed substantially higher payoffs from the repeated prisoner’s dilemma games (mean payoff in C-culture: 2227.89 ECU; mean payoff in D-culture: 1690.39 ECU), but this difference was attenuated by the initial endowments (mean total payoff in C-culture: 2777.89 ECU; mean total payoff in D-culture: 3290.39 ECU).

Supplementary Information for

A cooperative culture may affect trustworthiness assessments derived from the human face

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Supplementary Material 1: Overview of literature on spillover effects

Table S1

Overview of literature on pro-sociality spillovers

Study	Experimental treatment	Effect	Spillover measurement			
			Method	Type	One-shot	Baseline
Harbring (2010)	Tournament vs. team incentives	Competition reduces trust	TG	Seq.	Yes ^a	Yes
Reeson and Tisdell (2010)	High vs. moderate vs. no competition	Competition reduces cooperation	PGG	Seq.	Yes ^b	Yes
Chen (2011)	Competitive payment vs. piece-rate	Competition reduces likelihood of donation	Charitable donation	Seq.	Yes	No
Cason et al. (2012)	Simultaneous vs. sequential	Median game first increases coordination	CG	Seq. & sim.	No ^{b,c}	No
Al-Ubaydli et al. (2013)	Market priming vs. no priming	Market priming increases trust	TG	Seq.	Yes	No
Cason and Gangadharan (2013)	PGG vs. market vs. combined	Competition reduces cooperation	PGG	Sim.	No ^{b,c}	No
Kidd et al. (2013)	Varying rank earnings	Winning increases giving (independent of earnings)	Charitable donation	Seq.	Yes	No
Savikhin and Sheremeta (2013)	Lottery vs. PGG vs. simultaneous	Simultaneous play reduces lottery overbidding	PGG	Sim.	No ^c	No
Ter Meer (2014)	Piece rate vs. tournament vs. team incentives	Competition increases deception	Sender-receiver game	Seq.	Yes	No
Cassar et al. (2014)	Partial vs. impartial institutions	Formal institutions increase trust	TG	Seq.	Yes	Yes
Buser and Dreber (2016)	Winner-takes-all vs. lottery payment vs. piece rate	Winner-takes-all & lottery reduce cooperation	PGG	Seq.	Yes	No
Peysakhovich and Rand (2016)	Cooperative vs. defective culture	Defective culture reduces pro-sociality	PGG, TG, DG, UG	Seq.	Yes	No
D'Adda et al. (2017)	Baseline vs. nudge vs. push	Push measures increase DG pro-sociality	DG or PD	Seq.	Yes	No
Engl et al. (2017)	None vs. exogenous vs. endogenous institution	Institution increases cooperation	PGG	Seq. & sim.	No ^{b,c}	No
Grosch et al. (2017)	Competitive vs. threshold vs. random payments	Payment spread in competition reduces pro-sociality	PGG & SVO	Seq.	Yes	Yes
Stagnaro et al. (2017)	Levels of central punishment in PGG	Centralized punishment increases cooperation	DG & TPPG	Seq.	Yes	No
Arechar et al. (2018)	Order of cooperative and defective culture (RPD)	No effect w.r.t. DG behavior	DG	Seq.	Yes	Yes
Drouvelis and Marx (2018)	Piece-rate vs. revenue sharing	Group compensation increase giving	Charitable donation	Seq.	Yes	No
Bruin et al. (2019)	Single vs. different partner in initial and subsequent game	Experienced cooperation increases own cooperation	PGG	Seq.	Yes ^b	Yes
Ockenfels and Schier (2020)	Order of game types and of belief elicitation and behavior	Misaligned incentives reduce cooperation behavior and beliefs	TG & CG	Seq.	Yes	No
Von Bieberstein et al. (2020)	Gain vs. loss contracts	No spillovers observed	TG	Seq.	Yes	No

Abbreviations: CG: coordination game; DG: dictator game; PD: prisoner's dilemma; PGG: public goods game; SVO: social value orientation game; TG: trust game; TPPG: third party punishment game; UG: ultimatum game; seq.: sequential; sim.: simultaneous.

^aSeveral rounds are played in the final phase, but results are averaged.

^bWhile the interaction lasts more than one period it is one supergame and can thus be interpreted as one-shot.

^cSimultaneous spillovers are not measured one-shot, but the spillover-inducing institution is reinforced continuously.

Supplementary Material 2: Randomization check of participants**Table S2***Randomization check of participants (C-culture vs. D-culture treatment)*

Variable	C-culture	D-culture	Test statistic	<i>p</i>
Gender (%)	55	54	$\chi^2(1, n = 208) = 0$	1
Age (avg.)	23	23	W = 5,908	0.247
Stage 1 earnings (avg.)	3582	3650	W = 5,439.5	0.943

Note: Table displays gender percentages, average ages and Stage 1 earnings in both treatment groups as well as results from statistical tests of differences in gender (χ^2 -test of proportions), age and Stage 1 earnings distribution (both Wilcoxon rank-sum test).

Supplementary Material 3: Game lengths in intervention stage**Table S3***Intervention Stage supergame lengths*

Supergame number	C-culture	D-culture
1	10	2
2	8	1
3	3	1
4	1	1
5	6	1
6	1	1
7	9	1
8	2	1
9		1
10		1
11		1
12		1
13		1
14		1
15		1
16		1
17		1
18		1
19		1
20		1
21		1
22		2
23		2
24		1
25		1
26		3
27		1
28		1
29		1
30		1
31		1
32		2
33		1
34		1

Supplementary Material 4: Robustness checks of manipulation check**Table S4**

Regressions of decision to cooperate dummy on C-culture dummy including participant demographic and controls

<i>Variable</i>	Model 3			Model 4			Model 5		
	<i>OR</i>	<i>SE</i>	<i>p</i>	<i>OR</i>	<i>SE</i>	<i>P</i>	<i>OR</i>	<i>SE</i>	<i>p</i>
Intercept	0.69	0.41	0.533	0.62	0.13	0.019	0.80	0.45	0.700
C-Culture	3.11	0.86	<0.001	2.92	0.84	<0.001	3.00	0.85	<0.001
Decision Number	0.98	0.00	<0.001	0.98	0.00	<0.001	0.98	0.00	<0.001
C-Culture X Decision Number	1.01	0.01	0.440	1.01	0.01	0.437	1.01	0.01	0.442
Demographic controls	Yes			No			Yes		
Comprehension controls	No			Yes			Yes		
Observations	8320			8320			8320		
Individuals	208			208			208		
R ² Tjur	0.114			0.096			0.122		

Note: Table displays odds ratios (OR) from logit regressions with decision to cooperate dummy (defection as base) as the dependent variable. Demographic controls are age and gender (1 = female), comprehension controls are dummy variables indicating whether a participant gave a wrong initial answer to the comprehension questions related to Stage 2. Standard errors (SE) are clustered at unique treatment group level, i.e. every treatment group in every session. Bold p-values indicate significance at the 0.05-level.

Supplementary Material 5: Regression confirming no pre-intervention differences**Table S5**

Linear Regressions of pre-intervention TG contribution on C-culture treatment without controls (Pre-Model 1), interacting with decision number (Pre-Model 2) and including controls for participant demographics and comprehension (Pre-Model 3)

Variable	Pre-Model 1			Pre-Model 2			Pre-Model 3		
	B	SE	p	B	SE	p	B	SE	p
Intercept	2.10	0.09	<0.001	2.12	0.09	<0.001	1.81	0.47	<0.001
C-Culture	-0.11	0.12	0.338	-0.13	0.12	0.290	-0.13	0.12	0.293
Decision number				-0.00	0.00	0.596	-0.00	0.00	0.596
C-Culture X Decision number				0.00	0.00	0.778	0.00	0.00	0.778
Demographic controls		No			No			Yes	
Comprehension controls		No			No			Yes	
Observations		6240			6240			6240	
Individuals		208			208			208	
R ² / R ² adjusted		0.002 / 0.002			0.002 / 0.002			0.023 / 0.022	

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Standard errors (SE) are clustered at participant level. Demographic controls are age and gender (1 = female), comprehension controls are dummy variables indicating whether a participant gave a wrong initial answer to the Stage 1 comprehension question. Bold p-values indicate significance at the 0.05-level.

Supplementary Material 6: Robustness checks of main results**Table S6**

Linear regressions of post-intervention TG contribution on C-culture dummy and average baseline contribution, interacting with decision number, including controls

Variable	Model 3			Model 4			Model 5		
	B	SE	p	B	SE	p	B	SE	p
Intercept	-1.70	0.77	0.027	-1.74	0.19	<0.001	-3.10	0.78	<0.001
C-Culture	0.53	0.18	0.004	0.48	0.18	0.007	0.53	0.18	0.004
Decision number	0.01	0.00	<0.001	0.002	0.00	0.456	0.002	0.00	0.456
Baseline contribution	0.71	0.16	<0.001	0.91	0.04	<0.001	0.71	0.16	<0.001
C-Culture X Decision number	-0.01	0.00	0.012	-0.01	0.00	0.012	-0.01	0.00	0.012
Demographic controls	Yes			No			Yes		
Comprehension controls	Yes			No			Yes		
Earning controls	Yes			No			Yes		
Target controls	No			Yes			Yes		
Observations	6240			6240			6240		
Individuals	208			208			208		
R ² / R ² adjusted	0.340 / 0.340			0.388 / 0.388			0.392 / 0.391		

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Standard errors (SE) are clustered at participant level. Demographic controls are age and gender (1 = female), comprehension controls are dummy variables indicating whether a participant gave a wrong initial answer to the comprehension questions, earning controls are the total earnings from Stages 1 and 2 and target controls are the target's gender (1 = female), independently rated trustworthiness and friendliness. Bold p-values indicate significance at the 0.05-level.

Table S7

Mixed-effects linear regressions of post-intervention TG contribution on C-culture dummy and average baseline contribution (RE Model 1), and interacting with decision number (RE Model 2), with participant random effects

<i>Variable</i>	RE Model 1			RE Model 2		
	<i>B</i>	<i>SE</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>p</i>
Intercept	0.20	0.10	0.060	-0.34	0.14	0.014
C-Culture	0.04	0.07	0.590	0.48	0.15	0.006
Baseline contribution	0.91	0.04	<0.001	0.91	0.04	<0.001
Decision number				0.01	0.00	<0.001
C-Culture X Decision number				-0.01	0.00	0.011
Random Effects						
σ^2		0.96			0.95	
τ_{00}		0.22 Participant			0.22 Participant	
ICC		0.18			0.18	
N		208 Participant			208 Participant	
Observations		6240			6240	
Marginal R^2 / Conditional R^2		0.335 / 0.457			0.338 / 0.461	

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Bold p-values indicate significance at the 0.05-level.

Table S8

Mixed-effects linear regressions of post-intervention TG contribution on C-culture dummy and average baseline contribution, interacting with decision number, with participant random effects, including controls

Variable	RE Model 3			RE Model 4			RE Model 5		
	B	SE	p	B	SE	p	B	SE	p
Intercept	-1.70	0.72	0.018	-1.74	0.19	<0.001	-3.10	0.73	<0.001
C-Culture	0.53	0.18	0.003	0.48	0.18	0.006	0.53	0.18	0.003
Decision number	0.01	0.00	<0.001	0.00	0.00	0.451	0.00	0.00	0.451
Baseline contribution	0.71	0.15	<0.001	0.91	0.04	<0.001	0.71	0.15	<0.001
C-Culture X Decision number	-0.01	0.00	0.011	-0.01	0.00	0.011	-0.01	0.00	0.011
Random Effects									
σ^2	0.95			0.86			0.86		
τ_{00}	0.22 Subject_Unique_ID			0.22 Subject_Unique_ID			0.22 Subject_Unique_ID		
ICC	0.19			0.20			0.20		
N	208 Subject_Unique_ID			208 Subject_Unique_ID			208 Subject_Unique_ID		
Demographic controls	Yes			No			Yes		
Comprehension controls	Yes			No			Yes		
Earning controls	Yes			No			Yes		
Target controls	No			Yes			Yes		
Observations	6240			6240			6240		
Marginal R ² / Conditional R ²	0.341 / 0.463			0.387 / 0.511			0.390 / 0.514		

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Standard errors (SE) are clustered at participant level. Demographic controls are age and gender (1 = female), comprehension controls are dummy variables indicating whether a participant gave a wrong initial answer to the comprehension questions, earning controls are the total earnings from Stages 1 and 2 and target controls are the target's gender (1 = female), independently rated trustworthiness and friendliness. Bold p-values indicate significance at the 0.05-level.

Table S6 shows that the results are robust to including controls for participant demographics (age and gender), comprehension (dummy for wrongly answering each control question initially) and previous earnings in Stages 1 and 2 (model 3) and target characteristics (gender, independent trustworthiness and friendliness ratings, model 4) as well as all controls (model 5). Of these variables, only the coefficients of the target controls reach significance in line with related results, i.e. female targets receive a higher contribution (corresponding to Buchan et al., 2008, who found the same effect for trust games without facial stimuli, and Dong et al., 2018, who observed that facial trustworthiness perception was higher for female targets) as do targets with a higher rating in trustworthiness and friendliness (corresponding to Rezlescu et al., 2012, and Todorov et al., 2015). The marginal effects of all three variables are substantially smaller than the treatment effect (coefficients: target gender = 0.19, trustworthiness = 0.39, friendliness = 0.16). All results are also robust to including participant random effects (see Table S7 and Table S8).

Supplementary Material 7: Regression confirming treatment replaces perception**Table S9**

Linear regressions of post-intervention TG contribution on baseline contribution, target characteristics and trustworthiness, interacting with decision number without (Model 1) and with (Model 2) participant controls

<i>Variable</i>	Model 1			Model 2		
	<i>B</i>	<i>SE</i>	<i>p</i>	<i>B</i>	<i>SE</i>	<i>p</i>
Intercept	-0.36	0.44	0.420	-1.55	0.86	0.070
Baseline contribution	0.91	0.04	<0.001	0.71	0.16	<0.001
Target gender 1 = female	0.19	0.03	<0.001	0.19	0.03	<0.001
Friendliness score	0.14	0.05	0.005	0.14	0.05	0.005
Trustworthiness score	0.04	0.15	0.784	0.04	0.15	0.784
Decision number	-0.03	0.01	0.002	-0.03	0.01	0.002
Trustworthiness X Decision number	0.01	0.00	0.006	0.01	0.00	0.006
Participant controls	No			Yes		
Observations	6240			6240		
Individuals	208			208		
R ² / R ² adjusted	0.388 / 0.387			0.391 / 0.390		

Note: Table displays unstandardized beta coefficients (B) from linear regressions with TG contribution as the dependent variable. Standard errors (SE) are clustered at participant level. Participant controls are demographic controls (age and gender, 1 = female), comprehension controls (dummy variables indicating whether a participant gave a wrong initial answer to the comprehension questions) and earning controls (total earnings from Stages 1 and 2). Bold p-values indicate significance at the 0.05-level.

Supplementary Material 8: Experimental instructions as displayed to participants

Note: Experimental instructions were presented to the participants in German and formatted differently to optimize screen utilization in the lab. Original German instructions are available upon request.

Screen 1 – Introduction

Welcome!

Thank you very much for participating in this experiment.

Please read the instructions by clicking on the „Next“ button.

As soon as you have read the instructions, you can start the experiment.

Screen 2 – General Instructions

Please read the following instructions carefully. The experiment is paid, that means you will earn money during the experiment. How much you earn depends on your decisions. During the experiment amounts of money will be shown in **Experimental Currency Units**. At the end of the experiments ECU will be converted to [local currency] (conversion rate: **400 ECU = 1 [local currency, redacted for peer review, equivalent to 1.09 USD]**) and paid out to you. In addition to the money you earn with your decisions you will receive a fixed amount of **4 [local currency, redacted for peer review, equivalent to 4.36 USD]**. This amount will be included in your final payment.

Your decisions will be completely anonymous, and the dataset will not allow any inference on your person. We are interested in your decisions, **there are no right or wrong answers!**

Please do not talk to any other person anymore from now on and raise your hand if you have a question. One of the experimenters will come to you. Please switch your mobile phone off or in airplane mode. For the duration of the experiment, you will need your full attention.

The experiment consists of three parts. Your decisions in one part do not influence subsequent parts.

Screen 3 – Stage 1 instructions

Instructions part 1

In the first part, you will observe another person and take a decision based on that. In the interaction a person A receives a budget. Person A decided, which share of this budget (0, 25%, 50%, 75% or 100%) to send to person B. The amount is doubled and sent to person B. Person B decides at the same time, which share of the received amount she would like to send back. Person B takes this decision for every possible amount she can receive.

You will take the role of person A and receive a budget of **100 ECU**. You will watch a short video sequence of another person who takes role of person B and already decided, how much she wanted to send back. Then you decide, whether you would like to send 0, 25, 50, 75 or 100 ECU to person B. This amount is doubled. You receive the amount that person B previously decided to send back.

You have 25 seconds for the decision. If you have not decided after 25 seconds, you will send 0 ECU to the other person.

Income

At the end of every interaction you receive the following income:

100 ECU – (amount you sent to the other person) + (amount the other person sent back)

Example

You send 75 ECU.

The other person receives 150 ECU.

The person in the video had previously decided to send back 100 ECU in case she received 150 ECU.

Hence, your pay-out is: $100 \text{ ECU} - 75 \text{ ECU} + 100 \text{ ECU} = 125 \text{ ECU}$

Screen 4 – Transition to comprehension questions

We will now ask a comprehension question. After that, the experiment will begin.

Screen 5 – Stage 1 comprehension question**Comprehension question**

You send 50 ECU of your budget (100 ECU), the other person sends back 75 ECU. How much will you receive in total?

- ☐ 75 ECU
- ☐ 100 ECU
- ☐ 125 ECU
- ☐ 150 ECU

Screen 5b – Wrong answer (only shown if answer was incorrect)

Unfortunately, your answer was incorrect. Please raise your hand and wait until one of the experimenters comes to you.

Screen 6 – Confirmation

In case of any uncertainties or questions you have now once more the opportunity to call one of the experimenters. To do so, raise your hand and stay seated until the experimenter comes to your place.

I have understood the rules and I am ready for the experiment!

Screen 7 – During video

Please make yourself ready for the decision and watch the video.

Screen 8 – Stage 1 decision**Your decision**

Please decide now how much money you want to send.

(Attention: if you have not decided after the time is up, you will send 0 ECU to the other person)

- ☐ 0 ECU
- ☐ 25 ECU
- ☐ 50 ECU
- ☐ 75 ECU
- ☐ 100 ECU

Screen 9 – Ending of Stage 1

This part of the experiment is no finished. You have earned **[total Stage 1 income]** ECU in total.

Screen 10 – Stage 2 Instructions 1

The second part is about completing several so-called interactions with one other person. The other person is another participant of this experiment. In the beginning of this part, you receive [endowment (treatment-specific)] ECU.

In the beginning of every interaction, one other person will be randomly assigned to you. After that, you will interact with this person for a random number of rounds.

In every round you will choose one of two options: A and B. At the end of the round you and the other person will receive a pay-out in ECU. This pay-out depends on your own decision as well as the decision of the other person.

You have 30 seconds to choose.

Here you can see how your income and that of the other person is determined:

Your income			Income of the other person		
	Other person chooses A	Other person chooses B		Other person chooses A	Other person chooses B
You choose option A	80 ECU	0 ECU	You choose option A	80 ECU	100/120 ECU
You choose option B	100/120 ECU	20 ECU	You choose option B	0 ECU	20 ECU

Example

If you choose A and the other person chooses A as well, you both receive **80 ECU** each.

If you choose A and the other person chooses B, you receive **nothing** and the other person receives **100/120 ECU**.

If you choose B and the other person chooses A, you receive **100/120 ECU** and the other person receives **nothing**.

If you choose B and the other person chooses B as well, you both receive **20 ECU** each.

Rounds per interaction

After every round, the probability of another round is $7/8$ / $1/8$. With a probability of $1/8$ / $7/8$ the interaction will end. This probability does not change and is independent of how many rounds have been played already.

When the interaction ends you will be randomly matched with another person in the room. Your decisions influence neither the number of interactions you will have nor the number of rounds in every interaction.

You income in every round is shown on the screen at the end of the round and is added to you total stock of ECU. Your total stock of ECU is paid out to you at the end of the experiment based on a conversion rate of **400 ECU = 1 [local currency, redacted for peer review, equivalent to 1.09 USD]**.

Note: The payoff for the combination BA and the continuation probability differ depending on the treatment. Participants were only shown the number for their respective treatment group.

Screen 11 – Transition to comprehension questions

We will now ask two comprehension questions. After that, the experiment will begin.

Screen 12 – Stage 2 comprehension question 1

Comprehension question 1

You choose option A, the other person chooses option B. How many ECU will you receive?

- ☐ 0 ECU
- ☐ 20 ECU
- ☐ 80 ECU
- ☐ 100 ECU

Screen 12b – Wrong answer (only shown if answer was incorrect)

Unfortunately, your answer was incorrect. Please raise your hand and wait until one of the experimenters comes to you.

Screen 13 – Stage 2 comprehension question 2

Comprehension question 2

What is the probability that the current interaction will go on?

- ☐ 1/8
- ☐ 4/8
- ☐ 7/8

Screen 13b – Wrong answer (only shown if answer was incorrect)

Unfortunately, your answer was incorrect. Please raise your hand and wait until one of the experimenters comes to you.

Screen 14 – Confirmation

In case of any uncertainties or questions you have now once more the opportunity to call one of the experimenters. To do so, raise your hand and stay seated until the experimenter comes to your place.

I have understood the rules and I am ready for the experiment!

Screen 15 – New interaction (first round)

You will now be randomly assigned to another person for the first interaction

Screen 16 – Stage 2 decision

Your profit		Options of the other person:	
		A	B
Your options:	A	80	0
	B	100/120	20

Your decision?

- ☐ Option A
- ☐ Option B

Note: The payoff for the combination BA and the continuation probability differ depending on the treatment. Participants were only shown the number for their respective treatment group.

Screen 17 – Results

Your Decision: **[Decision taken]**
 Decision of the other person: **[decision taken by interaction partner]**
 Your income this round: **[income resulting from combination of decisions]**

Screen 18 – interaction continues

This interaction continues.
 You have earned **[total interaction income]** ECU so far.

Screen 19 – interaction ends

This interaction has ended.
 You have earned **[total interaction income]** ECU in total.
 You will be assigned to **another** person for the next interaction.

Session 20 – Ending of Stage 2

This part of the experiment is now finished. You have earned **[total Stage 2 income] ECU** in total.

Screen 21 – Stage 3 instructions**Instructions part 3**

In the third part, you will again observe another person and take a decision based on that. In the interaction a person A receives a budget. Person A decided, which share of this budget (0, 25%, 50%, 75% or 100%) to send to person B. The amount is doubled and sent to person B. Person B decides at the same time, which share of the received amount she would like to send back. Person B takes this decision for every possible amount she can receive.

You will take the role of person A and receive a budget of **100 ECU**. You will watch a short video sequence of another person who takes role of person B and already decided, how much she wanted to send back. Then you decide, whether you would like to send 0, 25, 50, 75 or 100 ECU to person B. This amount is doubled. You receive the amount that person B previously decided to send back.

You have 25 seconds for the decision. If you have not decided after 25 seconds, you will send 0 ECU to the other person.

Income

At the end of every interaction you receive the following income:

$100 \text{ ECU} - (\text{amount you sent to the other person}) + (\text{amount the other person sent back})$

Screen 22 – Confirmation

In case of any uncertainties or questions you have now once more the opportunity to call one of the experimenters. To do so, raise your hand and stay seated until the experimenter comes to your place.

I have understood the rules and I am ready for the experiment!

Screen 23 – During video

Please make yourself ready for the decision and watch the video.

Screen 24 – Stage 3 decision**Your decision**

Please decide now how much money you want to send.

(Attention: if you have not decided after the time is up, you will send 0 ECU to the other person)

- ☐ 0 ECU
- ☐ 25 ECU
- ☐ 50 ECU
- ☐ 75 ECU
- ☐ 100 ECU

Screen 25 – Ending of Stage 3

This part of the experiment is now finished. You have earned [total Stage 1 income] ECU in total.

Screen 26 – Introduction to final questions

Please answer a few short questions in the end.

Of course, we will keep your answers confidential and they do not allow any inference on your person.

Screen 27 – Gender

Your gender?

- ☐ Female
- ☐ Male

Screen 28 – Age

How old are you? ____

Screen 29 – Closing

You have completed the experiment. You have earned [total income] ECU in total.

Thank you very much for participating!

Please wait until your workstation number is called by the experimenter.

Supplementary Material 9: Survey instructions

Note: survey instructions were presented to the participants in German. Original German instructions are available upon request.

Welcome to the study!

General information:

In this study, we would like to gather information about the evaluation of the trustworthiness and the friendliness of other fellow human beings.

Participation in the study voluntary. You can end the survey at any time. The institute [redacted for peer review] is interested in utilizing your data for scientific purposes. Your data will of course be handled anonymously and confidential. The summarized results will be presented in workshops and conferences or scientific journals. Please indicate if you consent with participating under theses conditions:

- ☐ Yes
- ☐ No

Should you have any further questions about the study, feel free to contact [redacted for peer review]
Thank you very much for participating!

Instructions:

In den following 15 minutes we ask you to assess the trustworthiness and the friendliness of different people based on 60 short videos. Every video takes about 15 seconds after which the next video with the next person will start. Please evaluate the people you see quickly and intuitively. There are no right or wrong answers.

Please assess person X. This person is...

	Fully disagree	Rather disagree	Neither agree nor disagree	Rather agree	Fully agree
Trustworthy					
Friendly					

Note: the question and format were the same for all targets.

Demographic information:

Gender:

- ☐ male
- ☐ female
- ☐ other

Age:

Major:

Minor:

You have reached the end of the study. Thank you very much for participating!

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