Social Influence in Prosocial Behavior: Evidence from a Large-Scale Experiment*

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Abstract

We propose a novel experiment that prevents social learning, thus allowing us to disentangle the underlying mechanisms of social influence. Subjects observe their peer's incentives, but not their behavior. We find evidence of conformity: when individuals believe that incentives make others contribute more, they also increase their contributions. Conformity is driven by individuals who feel socially close to their peer. However, when incentives are not expected to raise their peer's contributions, participants reduce their own contributions. Our data is consistent with an erosion of norm-adherence when prosocial behavior of the social reference is driven by extrinsic motives, and cannot be explained by incentive inequality or altruistic crowding out. These findings show scope for social influence in settings with limited observability and offer insights into the mediators of conformity.

Keywords: Prosocial behavior, social influence, online experiment.

JEL Classification: C90, D83, D91

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1. Introduction

The increasing social connectivity of modern times fosters opportunities for social interactions and comparisons with others. A growing literature illustrates how information and cues about the behavior of others can induce social influence: the effect of others' actions on individual behavior. Social influence plays an important role across a broad range of domains that includes charitable giving (Frey and Meier, 2004), financial decision making (Bursztyn et al., 2014), marketing (Bapna and Umyarov, 2015), political participation (Cantoni et al., 2017), tax evasion (Drago et al., 2020), and well-being (Aral and Nicolaides, 2017). While in most social influence studies individuals *observe* others' behavior, various models (e.g. Bernheim, 1994; Akerlof, 1997) explain the spread of social influence even for unobservable behavior via *conformity*. Isolating this behavioral mechanism requires ruling out the learning opportunities derived from observing the actions of others.

In this paper, we study social influence in prosocial behavior through conformity, i.e. when actions are *not* directly observable. Social influence makes actions of connected agents interdependent, but such interdependencies are often ignored in standard models of prosocial behavior.[1] We examine how information about others' environments generates social influence. We intentionally shut out observability. This allows us to disentangle the mechanisms of social influence and to assess the scope of social influence in applications where information about the behavior of others is harder to access compared to information about the constraints, incentives or institutions that others face.

We analyze social influence through a conceptual framework and an experimental design focused on a notion of conformity that is due to identification with a peer and her motives. When individuals encounter someone doing good out of intrinsic motives, they are inspired (or compelled) to conform to this behavior, and deviating creates a psychological cost. Given this preference, agents use the economic environment to infer intentions of the social reference and attempt to conform to their behavior even

[1]Much of the theoretical literature models prosocial behavior and public good contributions as games of strategic substitutes. The most prominent examples of such theories are represented by models of pure altruism (Becker, 1974) and impure altruism (Andreoni, 1989, 1990).

when this is not observable.

In a large-scale online experiment, 2,914 individuals engage in pairwise interactions before they independently take part in a real effort donation task. The two main outcomes of interest are (i) the amount of charitable donations individually generated through the donation task and (ii) expectations of the amount generated by the other player within the pair. In our experiment, individuals can generate donations to a charity through a tedious physical task. We experimentally manipulate private incentives for making donation: for each player in a pair, we cross-randomize one of three levels of piece-rate (*zero*, *moderate*, and *high*) private incentives to generate donations for *Médecins Sans Frontières*. Variation in the incentives of the other player in the pair allows us to uncover social influence among peers: if an agent cares to conform, an increase in her peer's incentives will have both a direct effect on her peer's donations and an indirect effect on the agent's donations as she tries to minimize distance with the actions of her peer. We can then identify the social influence effects of peer incentives and evaluate different behavioral motives by estimating the contemporaneous effect of peer incentives on both expectations—about donations of the peer—and donations of the agent whose incentives are held constant. Before the treatment manipulation, pairs of subjects participate in a joint problem solving task, which we adopt to induce social proximity between paired players (Chen and Li, 2009; Chen and Chen, 2011) and increase relevance of the peer as a social reference. After that, we elicit a survey measure of social proximity (Cialdini et al., 1997), which we use to investigate how social proximity determines propagation of social influence.

We find evidence of social influence in donations: when the peer's incentives increase from *zero* to *moderate*, subjects expect their peer to increase donations and in turn, they donate more themselves. These effects are entirely driven by subjects who exhibit a close social connection to their peer, for whom the effect of increasing the peer's incentives from *zero* to *moderate* on donations is as large as half the effect of increasing *their own* private incentives from *zero* to *moderate*. However, when the peer's incentives further increase from *moderate* to *high*, we find a different result: individuals correctly expect their peer's donations to not be affected by higher incentives, and they themselves donate less when their peer has *high* incentives. Thus, individual

donations respond non-monotonically to peer's incentives. These effects are, again, entirely driven by the subsample of individuals who feel socially close to their peer. For individuals who do not feel close to their peer, we cannot reject the null of no social influence.

We propose a mechanism related to Fuster and Meier (2009), and argue that the strength of the desire to conform depends on whether the peer engages in the behavior for non-selfish reasons. Higher incentives for the peer can thus have an ambiguous effect on behavior. If incentives are "too generous", the peer's behavior may no longer be viewed as non-selfish, and the desire to conform weakens. Thus, individuals may well reduce effort in response to higher incentives for their peer. We formalize this intuition in a model and show that non-monotonicities as observed in our experiment can be generated in a simple version of the model.

One might suspect that higher peer incentives reduce an individual's contributions due to substitution effects from models of impure altruism. However, for such substitution to occur, it needs to be the case where the individual expects a higher contribution from her peer. In our setting, this is clearly not the case.

Differences in incentives between individuals may also give rise to incentive-inequality effects described in Breza et al. (2017). Incentive inequality in that sense predicts that, conditional on own incentives, donations decrease with the difference in incentives to the peer. Thus, they predict a monotonicity with regard to that gap. However, our non-monotonicities arise for all levels of own incentives. In most of these cases, incentive inequality would predict the opposite pattern. We develop a formal test and can reject that incentive inequality explains the pattern we find.

Our work broadly contributes to the large literature in economics and psychology that has studied empirically whether social information can produce social influence on prosocial behavior, both in the lab (Cason and Mui, 1998; Bohnet and Zeckhauser, 2004; Eckel and Wilson, 2007; Krupka and Weber, 2009; Servátka, 2009; Duffy and Kornienko, 2010; Bigenho and Martinez, 2019) and in the field (Frey and Meier, 2004; Shang and Croson, 2009; Chen et al., 2010; Fellner et al., 2013; Cantoni et al., 2017; Bruhin et al., 2020). Our main contribution to this literature is to show that observing the behavior of others is not necessary for people to be subject to social influence. In

fact, they will attempt to infer how others behave and conform to that behavior.

We also contribute to a growing literature that tries to disentangle the mechanisms of social influence. While we are not the first that try to separately identify social learning from conformity (Bursztyn et al., 2014; Lahno and Serra-Garcia, 2015; Gilchrist and Sands, 2016), our experiment is, to the best of our knowledge, the first with a focus on conformity in an environment that entirely removes any opportunity for social learning. Moreover, compared to these papers, we are the first to study conformity in the prosocial domain: Bursztyn et al. (2014) investigate social learning and the shared experience of holding an asset as distinct mechanisms of peer effects in financial decisions; Lahno and Serra-Garcia (2015) isolate conformity in lottery choice through a decision environment stripped down of complexity to minimize the scope for social learning; Gilchrist and Sands (2016) use weather instruments to estimate the effect of cumulative movie viewership on the probability of going to watch a movie and run various robustness checks to rule out social learning about quality of the movie.

Our findings have implications for a large literature on social influence and incentives for charitable giving and volunteering (e.g. Eckel and Grossman, 2003; Landry et al., 2006; Huck et al., 2015; Meer, 2017; Perez-Truglia and Cruces, 2017), furthering our understanding of the forces that modulate the channels of social influence. It enriches the literature on the damaging role of incentives on norm-adherence (Gneezy and Rustichini, 2000a,b; Fuster and Meier, 2009), by demonstrating a more nuanced role of incentives. Furthermore, we add, to an empirical literature on the role of social proximity in social influence mediated by social information (see e.g. Topa, 2001; Leider et al., 2009; Bond et al., 2012; Dimant, 2018), evidence that social proximity also modulates social influence in the absence of social information. This evidence is important because it shows that social proximity matters even when benefits of (and opportunities to punish in) future interactions are absent.

Most closely related to ours is the work of Kessler (2017), who provides field and laboratory evidence that public endorsement of peers to a charitable cause can produce large complementarities in giving even when the actual amount of money donated is not observable. He proposes social learning and conformity as primary behavioral channels to explain such findings. Our work complements this paper in two important

ways: First, Kessler (2017) shows that endorsements affect beliefs about the quality of a charity and others' donations. Our experiment is designed to hold constant beliefs about the quality of the charity to make a first attempt at separately identifying conformity from social learning in the prosocial domain. Second, we use a novel approach to identify social influence based on private incentives to donate in newly-formed social bonds. This allows us to learn new lessons about the interaction between prosocial motivations, social proximity and conformity.

The remainder of this paper is organised as follows. section 2 presents the experimental design and predictions. section 3 illustrates the results and discusses mechanisms of social influence. section 4 concludes.

2. The Experimental Setup

2.1. Experimental Design

We conduct an online experiment with registered workers from Amazon Mechanical Turk. The study develops over five stages, featuring a full 3×3 between-subject design plus an additional control treatment. All subjects in the experiment are randomly grouped into pairs. Prior to learning about the main experimental task, subjects make contact with the other player in the pair. Pairs are formed after Registration, and the first three stages are common to all pairs. In the fourth stage, each pair is randomly assigned to one of ten treatments. The experiment concludes with a short survey and review of the payoffs. We present each stage in detail below.[2]

1. Registration. Invited subjects accept the general conditions for participating in the experiment before accessing the software interface. The study begins with general instructions that outline the key stages of the experiment: subjects are informed that they will be randomly paired with another player with whom they will jointly complete the first task, followed by the second task to be completed independently. After reading the initial set of instructions, each subject chooses a number from 1 to 6, which they are told will matter for the variable component of their pay at the end of the experiment. We introduce *tokens* as the experimental currency. This stage is concluded by a short

[2]Full experimental instructions can be found in the supplemental material, Appendix C.

survey to collect demographic information (i.e. name, gender, age, and experience on Amazon Mechanical Turk), which subjects are told will only be shared with their peer.[3]

2. Joint problem solving task. As subjects progress to this stage of the experiment, pairs are formed at random and subjects are introduced to their peer: they are presented with the demographic information of their peer (i.e. stated name, gender, age, country of residence, and experience on Amazon Mechanical Turk) on their computer screen. [4] All our subjects are residents in the United States.

Similar to Chen and Li (2009), we use a joint problem solving task to favor the formation of a social connection between paired players. In this task, pairs of players see the same four famous paintings. For each painting, subjects are incentivized to identify – in coordination with their peer – the corresponding artist from a list of five: each subject in the pair earns 20 tokens each time *both* players give the correct artist for the same painting.[5] Paired players can solve the task through a private online chat (see interface in Figure B.3). We differ from Chen and Li (2009) by making rewards dependent on both own and peer's answers to increase incentives for establishing social contact. Payoffs are revealed at the end of the experiment.

3. Oneness elicitation. We measure social proximity with the *oneness* scale. There are two main reasons why this is a natural choice for the study: The *oneness* scale has been found to explain social proximity for dyadic relationships relatively well in comparison to more involved questionnaire-based scales from social psychology (Gächter et al., 2015), and it is fast and simple to administer (see Figure B.4). The oneness scale was first proposed by Cialdini et al. (1997) as a simple mean of two underlying scores: (i) the Inclusion of Other in the Self (IOS) scale and the (ii) WE scale. The IOS scale (Aron et al., 1992) is an easy-to-administer pictorial measure of social proximity between the research subject and a related person, constructed by simply asking subjects to indicate which of seven diagrams, composed of two increasingly overlapping circles, best represents their connection to the related person of interest. Cialdini et al.

[3]We cannot verify that this information is truthfully provided. We ask people to provide a name to facilitate interactions, but we did not expect players to recognize the peer as acquaintance/friend. Chat scripts provide no evidence of pre-existing relationships among paired participants.
[4]The order of arrival to this page constitutes our random matching protocol.
[5]We make the task hard by listing possible artists from relatively similar epoch and style.

(1997) later proposed to integrate the IOS scale with the WE scale, which asks subjects to express the extent to which they would refer to themselves and another person of interest as *we*, to capture complementary aspects of group membership embedded in social relationships. Both scales are elicited without incentives.

4. Donation task. For this task, subjects have to decide how many donations to generate for charity and make a point prediction about the number of donations their peer will generate. We treat such point prediction as proxy of beliefs of peer's giving.[6] To limit the scope of anchoring effects, we elicit expectations and desired number of donations simultaneously. After recording the two variables, subjects carry out the real effort task that generates these donations. Each donation requires entering 100 sequences of keystroke combinations "w"-"e" on a computer keyboard.[7]

Prior to eliciting beliefs and donations, subjects go through a small training exercise to familiarize themselves with the real effort task, and this allows us to screen out subjects who are unable to solve the task. Thereafter, the software randomly assigns pairs of subjects to one of the ten different treatments.

Our experimental treatment manipulations simultaneously vary incentives to behave prosocially for both subjects in a pair. To make it very clear that variation in monetary incentives is random and independent between peers, all players in the nine incentivized treatment conditions are provided with ex-ante identical lottery incentives. This is also important for ensuring that different incentives could not be viewed as a signal for the importance of the task (Ellingsen and Johannesson, 2008). Subjects earn 50 tokens for each donation generated if the number picked in *stage 1* matches the roll of a fair die. Across incentivized treatments, we vary the *expected* stakes of monetary incentives for each player by means of a simple information device. The device randomly determines whether to disclose if the matching die has a face number between the largest three or the smallest three figures of a die. When this signal is provided, depending on the initial number chosen, this either reduces to

[6]For practical reasons we do not elicit the entire belief distribution, but instead use a measure that most likely captures the perceived mode of giving of the peer. To limit the scope for motivated reasoning, we incentivize correct predictions with a 20 tokens prize.

[7]We choose a sterile task to limit the scope for confounding factors. A similar task has been used in other experiments studying incentives for charitable giving (Ariely et al., 2009; Meyer and Tripodi, 2017), and effort provision (DellaVigna and Pope, 2016, 2017).

zero the chances of getting the piece-rate incentive to generate donations (incentives are *zero*), or it increase chances to 1 in 3 (incentives are *high*). When this signal is not provided, the probability of getting the piece-rate incentive for generating donations is not updated and remains 1 in 6 (incentives are *moderate*).[8] To make incentives common knowledge within each pair, we reveal to subjects their peer's signal and initial chosen number. We also make sure that subjects understand both their own and peer's incentives by (i) framing as "lucky" ("unlucky") the die roll when incentives are *high* (*zero*) and (ii) directly providing them with the updated probabilities of receiving the piece-rate to generate donations (see Figure B.5 for an example). This information revelation scheme produces variation in the magnitude of expected incentives for acting prosocially, for both player i and peer j of each pair, in a full 3×3 between-subject design. We enrich this design with a control *no lottery* condition. Figure 1 schematizes the experimental design.

5. Exit. In the final stage, subjects answer some unincentivized questions to check comprehension. The summary of individual payoffs concludes the experiment.

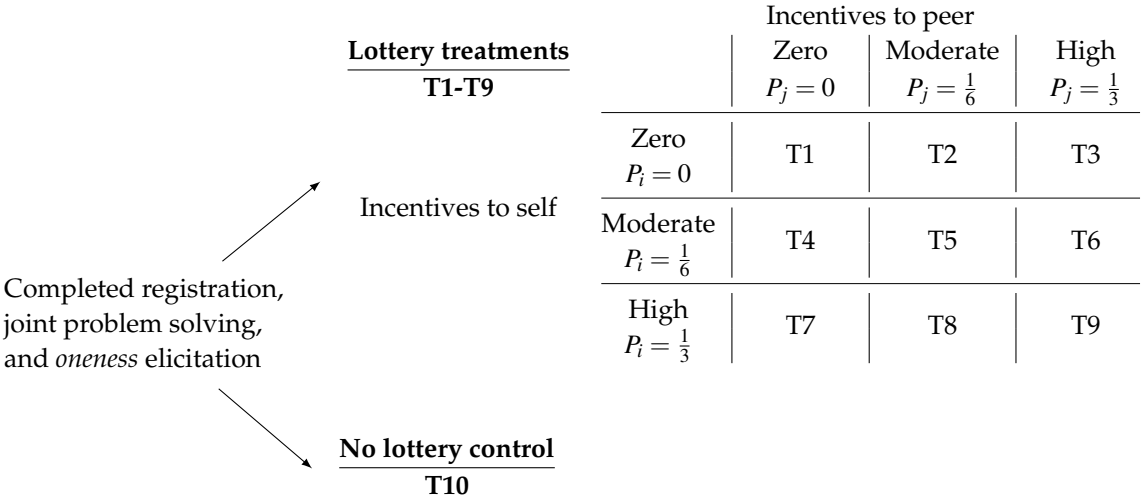


Figure 1: Overview of Experimental Design and Treatment Assignment

[8]We prefer this probabilistic approach over randomizing a deterministic piece rate to reduce disappointment in pairs where one subject receives no incentive and her peer receives high incentives. The main disadvantage is that it potentially introduces subjective evaluations of probabilities (see e.g. page 637 of DellaVigna, 2018, for a discussion of the mixed evidence on probability weighting in real effort experiments). However, this approach has the great advantage that, by reducing disappointment, it helps avoid differential attrition across treatments.

2.2. Conceptual Framework and Predictions

To formalize our strategy for identifying social influence, consider the following simple model of prosocial behavior. Two agents $a = \{i, j\}$ are presented with the opportunity to choose a donation effort d_a. There are four components to their utility: donations create at a monotonically increasing and convex private cost $c(d_a)$. Personal benefit from donations is a heterogeneous altruism component v_a per unit of d, distributed according to c.d.f $F(v_a)$ in the population, and a monetary benefit m. Agents have a preference (or feel pressured) to conform to their peer. We follow Sliwka (2007) in assuming that people conform to the *natural* behavior of their peer d_j^n, which is j's behavior absent pressures to conform.[9] That preference is captured by a loss function $\kappa_{i,j}(\cdot)$ that is convex, monotonically increasing in the absolute distance between d_i and the expected d_j^n (because there is heterogeneity in v_j). [10] We write the utility of agent i from contributing d_i as:

$$U(d_i|m_i, m_j) = (v_i + m_i)d_i - c(d_i) - \kappa_{i,j}(|d_i - E(d_j^n|\mathscr{A}_j(m_j)|) \tag{2.1}$$

where $E(d_j^n|\mathscr{A}_j(m_j)) = E_{v_j}\big(\underset{d_j}{\text{argmax}}\ (v_j + m_j)d_j - c(d_j)|v_j \in \mathscr{A}_j(m_j)\big)$. We use this model to understand contributions to large charities, for which changes of a few dollars in aggregate donations are the proverbial drop in the ocean. Hence we consider a model in which the marginal altruistic utility from donating to the charity is constant, but the model can certainly be extended to allow for decreasing marginal returns.[11]

The key feature of our model is the function $\kappa_{i,j}(\cdot)$. It combines the standard forces of conformism (Akerlof, 1997; Bernheim, 1994) with the innovation that the strength of conformity depends on how normatively "attractive" the role played by the peer is (Kelman, 1961). A role is normatively attractive if an agent desires to identify with it. We model this by assuming that an individual's cost from deviating depends on

[9] This formulation shuts out second-order strategic effects. It considerably simplifies the analysis, as it turns the solution into a maximization problem.

[10] We also normalize $\kappa_{i,j}(0) = 0$.

[11] In the appendix, we consider a model of impure altruism with diminishing marginal utility. We show that it predicts that an agent's donation are globally declining in her peer's incentives.

whether her peer engaged in the behavior for non-selfish reasons. We specify this as

$$\kappa_{i,j}(|d_i - E(d_j^n|\mathscr{A}_j(m_j)|) = -\frac{\lambda_{i,j}}{2} Pr(\mathscr{A}_j(m_j))(d_i - E(d_j^n|\mathscr{A}_j(m_j)))^2$$

with $\mathscr{A}_j(m_j) = \{v_j \in V : c(d_j^n)/d_j^n > m_j\}$. Thus, the set $\mathscr{A}_j(m_j)$ represents all agents for whom choosing d_a given the monetary incentive m_j does not cover their cost of effort. The more non-selfish types there are, the stronger the conformism the individual feels towards that behavior. The parameter $\lambda_{i,j} \geq 0$ measures the importance of conformity costs relative to the marginal utility of money, and may vary between individuals depending on how socially close they feel to each other (Bond et al., 2012; Gioia, 2017).

For the case of quadratic costs $c(d)$, it is easy to show that $\Pr(\mathscr{A}_j) = 1 - F(m_j)$, i.e. the fraction of non-selfish types is the density to the right of m_j in the distribution of altruism parameters $F(v_a)$.[12] In this case, the objective function simplifies to

$$U(d_i|m_i, m_j) = (v_i + m_i)d_i - \frac{cd_i^2}{2} - \frac{\lambda_{i,j}}{2}(1 - F(m_j))(d_i - E(d_j^n|m_j))^2 \tag{2.2}$$

This yields the first-order condition that implicitly defines the optimal d_i

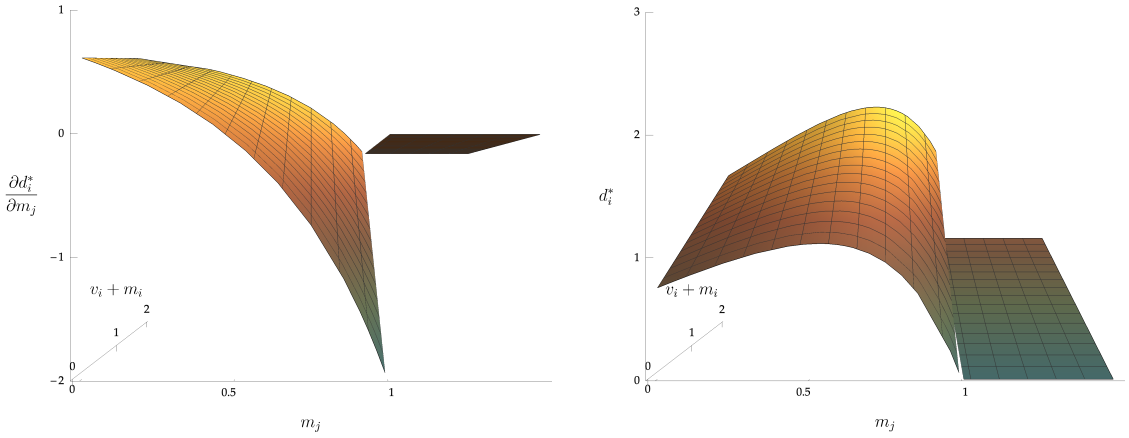
$$d_i = \frac{v_i + m_i + \lambda_{i,j}(1 - F(m_j))(d_i - E(d_j^n|m_j))}{c} \tag{2.3}$$

The equation illustrates how changes in m_j act through two channels on the individual's optimal behavior. The "traditional" conformism effect (Akerlof, 1997; Bernheim, 1994) acts through $E(d_j^n|m_j)$: higher incentives to j increase the normal effort d_j^n and thus act to increase d_j in equation (2.3). The second channel acts through composition effects: higher m_j reduces the fraction of individuals $1 - F(m_j)$ who engage in the behavior for non-selfish reasons in equilibrium. Thus, while the traditional conformism channel is unambiguously positive, the second channel acts against this and can overturn the sign of the overall effect.

In Figure 2, we illustrate the predictions of this model when $v \sim \mathscr{U}[0,1]$ at varying levels of the private benefits to contribute $v_i + m_i$. The left panel shows how j's incentives m_j have positive effects on i's donations when incentives are low; these effects are

[12] Because quadratic costs imply $d_j^n = \frac{v_j + m_j}{c}$, it follows that $\Pr(\mathscr{A}_j) := \Pr(c(d_j^n)/d_j^n > m_j) = \Pr\left(\frac{v_j + m_j}{2} > m_j\right)$.

decreasing in j's incentives and tend to become negative when m_j becomes large relative to $v_i + m_i$. When incentives are sufficiently large that the set of agents who engage in the prosocial activity out of altruism is empty, agents feel no need to conform and changes in m_j have no effect on d_i. These patterns translate into the non-monotonic relationship between m_j and d_i that is illustrated in the right panel of the figure.



Note: The left panel graphs the marginal effects of increasing the peer's incentives (m_j) on the agent's donations (d_i) at different levels of the agent's private benefit to donate ($v_i + m_i$). The right panel graphs the agent's donations (d_i) as a function of her own private benefits to donate ($v_i + m_i$) and the peer's incentives (m_j).

Figure 2: Own donations as a function of own and peer's monetary incentives

In Appendix A.1, we study the model in a more general setting and show that for a general distribution of types $F(v)$ and a large set of cost functions, with constant elasticity of effort $k \leq 1$, there exists a threshold for $\tilde{m}_j$ above which i becomes unresponsive to changes in the incentives of her peer.

This theoretical framework offers two approaches to identify conformity through incentives. The first, less data demanding, hinges on estimating the indirect effect of changes in j's incentives to donate on i's donation behavior: conformity predicts that an increase in j's incentives should increase i's donations. The second, identifies the strategic complementarities of conformity by considering the effect of changes in j's incentives on both i's expectations about j's donations and i's donations: if donations are affected by conformity, changes in j's incentives shift both i's beliefs about j's donations and i's donations in the same direction.

The framework also provides an explanation for why not all actions of a social reference may lead to conformity in the same way. Much like in theories of prosocial

behavior with incentives, e.g. for social signaling (Benabou and Tirole, 2006) and peer punishment (Dutta et al., 2018), the extent to which agents wish to adhere to the behavior of a social reference can be endogenous to incentives. Our experimental design allows us to separate conformity from these alternative explanations, to be discussed in section 3.4.

2.3. Procedures

To uncover the role and determinants of the conformity channel of social influence, we conduct six sessions of the experiment in 2017, between July 30 and August 4, recruiting 3,467 subjects on Amazon Mechanical Turk.[13] This is an online platform that is becoming increasingly popular for conducting economic experiments (DellaVigna and Pope, 2016) where thousands of registered workers are commonly employed in tasks that require human intelligence. Compared to lab subjects, workers on this platform are more heterogeneous in terms of socio-economic characteristics and have been found to exert more attention to experimental instructions (Hauser and Schwarz, 2016).[14] In our experiment, subjects that complete the study earn 1.20 USD participation fee plus bonus pay depending on their behavior during the experiment. Tokens constitute the experimental currency at the exchange rate of 1 token=0.005 USD. Completing the experiment took participants 17 minutes and 4 seconds on average. Including participation fee, on average, subjects earned 1.63 USD for themselves, and generated 1.13 USD donations for the charity of our choice – *Médecins Sans Frontières*. For subjects that do not spend time on the donation task, the experiment only took 10 minutes and 33 seconds; these subjects earned 1.34 USD, including participation fee, on average. Such average earnings are comparable to the 7.25 USD hourly earnings accumulated by the most productive 4% of workers on this platform and are significantly higher than the median hourly earnings of 2 USD (Hara et al., 2018). Participation in the experiment is allowed only once, and no retakes are granted to subjects that

[13]The experimental software is programmed in oTree (Chen et al., 2016). We collect data over multiple sessions to minimize risks of overloading our server.

[14]Like other studies conducted on this platform, we restrict participation in our experiment to workers with an approval rate above 90%. We also restrict participation to workers residing in the U.S.

accidentally drop out of the study.[15]

2.4. Randomization Checks

From the total of 3,467 subjects that began the experiment, we work with a sample of 2,914 subjects who completed both the joint problem solving (JPS) task and the donation task. In the JPS task, subjects score an average of 40 out of the 80 available points. After the task, subjects report a 2.8 oneness towards their peer on average (on a scale between 1 and 7). Across the ten treatment conditions, subjects generate 4.6 donations, on average, for *Médecins Sans Frontières*, and predict their peer to generate an average of 3.9 donations. Table 1 shows balance in pre-treatment measures and attrition. The lack of differential attrition across treatments attenuates concerns of disappointment effects from our treatment manipulations.

2.5. Social Proximity

As argued in the conceptual framework, conformity requires some degree of social connection to the social reference.[16] This section discusses interpretation and determinants of our measure of social proximity, which we elicit among pairs of strangers after they interact in the JPS task.

Recall that in this task, pairs of subjects are presented with four paintings and they need to agree on the correct artist to associate from a list of five artists for each painting. Social contact within each pair occurs in the chat box that allows for instrumental coordination on answers and strategies to solve the task.[17] An average score of 40 out of 80 available points indicates significant coordinated effort to solve the common puzzles; random click-through from both subjects would predict an expected score of

[15] A 40-minute timer is implemented to encourage subjects to complete the experiment timely and without distractions. Furthermore, to discourage speeding behavior and the use of bots, we implement a practice of the real effort task before treatment assignment.

[16] Studying behavioral mechanisms that operate via social interactions is methodologically complex. Some papers leverage existing social relationships and identities, while others induce the formation of social relationships and identities within the experiment (Goette et al. (2012) and Chen et al. (2014) for reviews of this literature). For our investigation, to avoid contaminating the conformity with other forms of social influence deriving from the prospects of future interactions, we choose the approach of building social relationships among randomly and anonymously matched strangers.

[17] To solve puzzles, many of the subjects realize that they can use Google image search, and they tend to split up paintings to search with their peer.

Table 1: Summary Statistics of Observable Characteristics and Attrition
(Means and Standard Errors in Parentheses)

	Full sample	No lottery	Lottery									p-value
Incentives to self			Zero			Moderate			High			
Incentives to peer			Zero	Moderate	High	Zero	Moderate	High	Zero	Moderate	High	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
					a) Measured before treatment							
Male	0.452	0.449	0.437	0.441	0.465	0.408	0.473	0.451	0.453	0.458	0.484	0.861
	(0.009)	(0.029)	(0.029)	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.030)	(0.029)	(0.028)	
Age group	2.524	2.491	2.473	2.500	2.620	2.582	2.513	2.487	2.529	2.548	2.500	0.882
	(0.021)	(0.068)	(0.065)	(0.065)	(0.066)	(0.066)	(0.069)	(0.064)	(0.065)	(0.065)	(0.068)	
Experience	2.605	2.774	2.567	2.666	2.662	2.624	2.564	2.632	2.604	2.568	2.403	0.280
	(0.028)	(0.092)	(0.089)	(0.091)	(0.089)	(0.089)	(0.093)	(0.086)	(0.088)	(0.083)	(0.093)	
Points JPS task	40.199	37.979	40.333	40.966	42.324	39.443	41.392	39.934	41.079	39.535	39.226	0.924
	(0.619)	(1.957)	(1.906)	(1.985)	(1.982)	(2.004)	(2.034)	(1.965)	(1.955)	(1.795)	(2.029)	
Oneness	2.801	2.704	2.847	2.784	2.894	2.793	2.885	2.773	2.831	2.691	2.819	0.829
	(0.030)	(0.093)	(0.096)	(0.097)	(0.097)	(0.098)	(0.103)	(0.094)	(0.095)	(0.096)	(0.089)	
					b) Measured after treatment							
Dropout	0.159	0.138	0.167	0.167	0.147	0.171	0.152	0.163	0.165	0.169	0.151	0.974
	(0.006)	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.020)	(0.019)	(0.020)	(0.019)	(0.020)	
Observations	2914 [3467]	287 [333]	300 [363]	290 [348]	284 [333]	287 [346]	273 [322]	304 [363]	278 [333]	301 [362]	310 [365]	

Notes: p-value in column (12) is for a one-way ANOVA on ranks (Kruskal-Wallis) test comparing the ten treatment groups in columns (2) to (11). Except for dropout rates ("Dropout"), all statistics refer to the final sample of subjects who completed the experiment. Dropout rates of subjects after treatment assignment computed on the samples reported in square brackets in the "Observations" row.

3.2. The chat box also introduces each subject to the peer by reporting peer's stated first name, age, gender, level of experience on the Amazon Mechanical Turk platform, and common US residence. The *oneness* measure of social proximity is meant to capture the extent to which basic demographic information and contact with the other player in the JPS task facilitate the formation of perceived social proximity.[18]

To put into perspective the kind of social proximity captured by the oneness scale, it is worth comparing the levels we measure to existing estimates. In other studies, on the same scale from 1 to 7, oneness towards an acquaintance, non-close friend, and close relationship is measured to be on average 2.5, 4.0, and 5.4, respectively (Gächter et al., 2015). In our sample, we measure greatly different levels of oneness, with an inter-quartile range capturing half of the entire range of possible realizations: the first quartile of the distribution is 1, the median is 2.5, the third quartile is 4. Expectedly, many subjects exhibit no social proximity to their peer in the experiment. But it is interesting to notice that at least half of the sample exhibits social proximity towards their peer – a stranger with whom they have recently made contact to solve puzzles – similar to social proximity that other studies observe towards acquaintances. This is not a *causal* effect of JPS interactions on social proximity, but gives an indication that the JPS does harness social proximity. More direct causal evidence can be found in Gioia (2017).

In Table B.7, least squares regressions illustrate the correlates of social proximity, and highlights the role of both *homophily* (Marmaros and Sacerdote, 2006) and chat box contact (Chen and Li, 2009) in the formation of social proximity. Although age difference between the paired players does not seem to be highly predictive of social proximity, the peer being of the same gender and having similar experience on the platform predict significantly higher oneness. The fit of this simple linear regression model improves remarkably when we include a binary indicator – *contact* – for whether players made reciprocal contact through the chat box provided.[19] Players that make reciprocal

[18] Figure B.6 provides the distribution of the two psychological scales underlying oneness. These two scales are strongly correlated ($\rho = 0.731$), with the WE scale exhibiting a relatively multi-peaked distribution compared to the clear single peak of the IOS scale (at the lowest level of social proximity). All analyses presented in the results section are robust to replacing either of these two scales as measures of social proximity.

[19] 80.4% of subjects used the chat box to make contact with the peer, and 64.6% of pairs managed to have a conversation (*contact* = 1). In these conversations, subject share their knowledge of the paintings, share

contact with their peer report 67.5% higher social proximity, and although the decision to engage in chat interactions is endogenous, the relatively strong correlation of 0.294 (column (1)) is indicative of the role of social contact for the development of social connection.

3. Experimental Results

3.1. Descriptive Evidence

In Table 2, we summarize average beliefs and donations across treatments, drawing the patterns of interest that will be explained in the next section.

Own incentives. Donations are weakly monotonic in personal incentives. They strongly increase when incentives go from *zero* to *moderate* and appear to flatten out when incentives are *high*. *Moderate* incentives also increase donations compared to a control treatment in which subjects are not incentivized and incentives are never mentioned. Beliefs indicate that individuals anticipate these patterns of direct incentive effects correctly, although they systematically underestimate the levels of others' generosity.[20]

Peer incentives. Donations systematically respond non-monotonically to peer incentives: for any level of personal incentives, donations increase when peer incentives go from *zero* to *moderate*, and decrease when incentives go from *moderate* to *high*. Subjects anticipate such comparative statics remarkably well. They anticipate that an increase in their own incentives from *zero* to *moderate* is going to increase donations of their peers and a further increase from *moderate* to *high* decreases their donations.

3.2. Evidence of Social Influence

In this subsection we test the statistical significance of these patterns and interpret the evidence through the lens of our social influence framework. We also examine

relevant personal information and considerations (e.g., one says "If my husband was here he would know, he is an art teacher lol", some other says that "Modern art sucks".), and agree upon strategies to solve the task (e.g. "You betcha. I'm googling the heck out of it right now. I've got Miro for the first one, Botticelli for the second, Grant Wood for the 3rd, working on the 4th."). Scripts of these conversations can be made available upon request.

[20] Consistent with studies finding that research subject accurately predict experimental results (DellaVigna and Pope, 2016), but people underestimate others' prosocial attitudes (e.g. Goette et al., 2006).

Table 2: Beliefs and Donations Across Treatments (Means and Standard Errors)

		Beliefs about peer's donations			Own donations		
Incentives offered							
No (control)		3.585			3.934		
		(0.205)			(0.222)		
Yes (3x3 treatments)							
		Incentives to peer			Incentives to peer		
		Zero	Moderate	High	Zero	Moderate	High
Incentives to self	Zero	2.540	4.331	4.637	3.233	3.417	3.190
		(0.182)	(0.215)	(0.208)	(0.217)	(0.230)	(0.210)
	Moderate	2.585	4.832	5.086	5.042	5.546	5.155
		(0.193)	(0.213)	(0.207)	(0.233)	(0.235)	(0.224)
	High	2.374	4.100	4.374	5.299	5.575	5.187
		(0.174)	(0.201)	(0.195)	(0.233)	(0.229)	(0.212)

whether the evidence can be explained by other theories of prosocial behavior.

$$Donation_i = \alpha + \beta_1 Lottery_i + \beta_2 Moderate_i + \beta_3 High_i + \beta_4 Moderate_j + \beta_5 High_j + X_{i,j}\gamma + \varepsilon_i \tag{3.1}$$

We use a linear regression model (3.1) to estimate how donations are affected by the economic environment. Denoting an agent by i and her peer by j, this model estimates both the effect of i's incentives on i's donations as well as the effect of j's incentives on i's donations. We allow for the effects of incentives to be non-monotonic by coding incentive as categorical variables. The regression model also includes an indicator for the *no lottery* control treatment that isolate disappointment effects of not receiving the incentives, as well as controls for observable characteristics of both players in a pair.

$$Belief_i = \phi + \delta_1 Lottery_j + \delta_2 Moderate_j + \delta_3 High_j + \delta_4 Moderate_i + \delta_5 High_i + X_{i,j}\omega + \varepsilon_i \tag{3.2}$$

We also estimate the mirror regression model (3.2) for individual beliefs on the donations of her peer. This allows us to test the unique prediction of the social influence framework that changes in peer incentives can cause a change in beliefs about how peer j behaves and a change in the behavior of agent i in the same direction. The estimates of regression models (3.1) and (3.2) are presented in panels (a) and (b) of Table 3,

respectively.

Consistent with the descriptive evidence, column (1) of panel (a) shows that increasing an agent's incentives from *zero* to *moderate* increases donations by 1.964 units ($p < 0.001$), while increasing incentives from *moderate* to *high* does not lead to a further increase in donations ($p = 0.649$). Agents also respond to changes in peer incentives, but do so non-monotonically (column (2)). Increasing peer incentives from *zero* to *moderate* increases donations by 0.356 units ($p = 0.055$), but donations drop when the peer incentives further increase from *moderate* to *high* ($p = 0.046$).

Panel (b) shows that agents anticipate these incentive effects. They anticipate that increasing someone's private incentives from *zero* to *moderate* will have a strong positive effect, and the effect of increasing incentives further will be subtle. They also predict that their peers will react to peer incentives non-monotonically. In fact, they believe that an increase in their own incentives from *zero* to *moderate* causes their peer to donate 0.336 extra units ($p = 0.044$), but a further increase in their own incentives from *moderate* to *high* would cause peer donations to drop ($p < 0.001$).

The non-monotonicity of donations in peer incentives is driven by subjects with a strong connection to their peer. When we estimate (3.1) and (3.2) separately for subjects above and below the median level of social proximity we find that socially distant subjects monotonically increase donations with monetary incentives, and they expect their peer to do the same. Yet, their giving behavior is not significantly affected by the incentives provided to their peer.[21] If at all, monetary incentives to the peer monotonically decrease a subject's own giving: donations decrease by 0.214 units and 0.251 units when the peer gets moderate and high incentives, respectively. Notwithstanding, these point estimates are not significantly different from zero. Socially close subjects respond differently to changes in the incentives of their peer ($p = 0.016$ for joint F-test for equality of effects between high and low oneness subjects).[22] When peer incentives increase from zero to moderate, subjects expect their peer to increase donations by 2.155 ($p < 0.001$) units and they donate 0.837 ($p < 0.001$) units more them-

[21] We partition the sample at the median score of oneness. For robustness, we try sample splits at the median score of the JPS task and at the median score of just one of the two psychological scales that are used to construct oneness; the results are qualitatively the same.

[22] In Table B.8 we illustrate the robustness of this result in a pooled specification that interacts the treatment with an indicator for high social proximity.

Table 3: Incentive Effects on Donations and Beliefs

		Full sample		Split by oneness		H_0 p-value:
				High	Low	*High = Low*
Outcome:		(1)	(2)	(3)	(4)	(5)
(a) Donations						
	Provided Lottery	-0.712***	-0.831***	-0.665*	-1.066***	0.464
		(0.262)	(0.283)	(0.389)	(0.403)	
	Incentives to self *(baseline: Zero)*					
	Moderate	1.964***	1.970***	1.921***	2.037***	0.052
		(0.183)	(0.182)	(0.254)	(0.260)	
	High	2.047***	2.044***	1.712***	2.502***	
		(0.179)	(0.179)	(0.242)	(0.259)	
	Incentives to peer *(baseline: Zero)*					
	Moderate		0.356*	0.837***	-0.214	0.016
			(0.186)	(0.259)	(0.268)	
	High		-0.001	0.170	-0.251	
			(0.180)	(0.236)	(0.269)	
	Constant	4.663***	4.650***	4.896***	4.248***	0.369
		(0.368)	(0.368)	(0.500)	(0.539)	
	Incentives to self, *High - Moderate*	0.083	0.073	-0.209	0.465*	
		(0.182)	(0.182)	(0.246)	(0.272)	
	Incentives to peer, *High - Moderate*		-0.356**	-0.667***	-0.037	
			(0.178)	(0.245)	(0.262)	
	H_0 p-value: Incentives to peer					
	Zero = Moderate = High = 0		0.080	0.003	0.607	
(b) Beliefs						
	Provided Lottery	-1.155***	-1.188***	-1.207***	-1.315***	0.822
		(0.237)	(0.256)	(0.358)	(0.348)	
	Incentives to peer *(baseline: Zero)*					
	Moderate	1.948***	1.962***	2.155***	1.773***	0.391
		(0.161)	(0.160)	(0.222)	(0.221)	
	High	2.237***	2.240***	2.227***	2.218***	
		(0.158)	(0.158)	(0.211)	(0.229)	
	Incentives to self *(baseline: Zero)*					
	Moderate		0.336**	0.420*	0.257	0.435
			(0.167)	(0.222)	(0.240)	
	High		-0.253	-0.337	-0.105	
			(0.160)	(0.221)	(0.227)	
	Constant	4.273***	4.274***	4.800***	3.625***	0.075
		(0.341)	(0.341)	(0.458)	(0.495)	
	Incentives to peer, *High - Moderate*	0.288*	0.278*	0.072	0.445*	
		(0.168)	(0.167)	(0.226)	(0.242)	
	Incentives to self, *High - Moderate*		-0.589***	-0.757***	-0.362	
			(0.158)	(0.219)	(0.225)	
	H_0 p-value: Incentives to self					
	Zero = Moderate = High = 0		0.001	0.003	0.267	
	Observations	2914	2914	1571	1343	

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$

Notes: All specifications include gender, age group, and experience, of both the player and her peer, as well as session dummies. Column (5) presents joint F-tests for the null hypotheses that point estimates – for each group of variables – are equal in the high and low oneness subsamples. Standard errors are clustered at the pair level. Results are qualitatively very similar in a seemingly unrelated regression framework that allows for correlation in the error term of individual beliefs and donations.

selves. However, when peer incentives increase from *moderate* to *high*, subjects again believe that the incentive increase does not affect ($p = 0.750$) peer donations (correctly so given that such increase in incentives for high oneness subjects does not increase donations significantly ($p = 0.395$)), and donations *decrease* by 0.667 ($p = 0.007$) units and individuals.[23]

The evidence is clear that the economic environment of the peer shapes willingness to behave prosocially. This is evidence of conformity, with *zero*-to-*moderate* changes in incentives causing individual donations and beliefs about the donations of others to move in the same direction. At the same time, the evidence indicates that the desire to conform diminishes when peer incentives are "too generous". Fewer donations are made when peer incentives are *high* in spite of the fact that expected donations from the peer do not decrease.

This evidence is explained by a model of social influence in which conformity is driven by identification with altruistic intentions. As we show in Section 2.2, such model captures that changes in peer incentives not only affect donations of the peer, but also the intensity of their altruistic intentions. The effects of peer incentives on donations are non-monotonic because any loss in psychological utility due to norm deviation is dampened when the norm is determined by weak altruistic intentions.

Wald estimates for the effect of beliefs on donations implied by our reduced form regressions results help appreciate diminishing conformity more directly. From column (2), we obtain that a one unit change in beliefs from increasing the peer's incentives from *zero* to *moderate* increases donations by 0.182 units ($p = 0.035$), while a one unit change in beliefs from increasing the peer's incentives from *zero* to *high* has no effect on donations ($b_{Wald} = -0.0003$, $p = 0.997$). This pattern of diminishing influence of beliefs about others on individual prosocial behavior is even more pronounced for high oneness subjects (column (3)).[24]

Alternatively, the non-monotonic effects of peer incentives on donations may be

[23] Importantly, differences in behavior across socially close and socially distant individuals does not appear to be driven by differences in pro social orientation. In fact, we can use the control treatment to show that in the absence of incentives subjects with high social proximity to their peer do not systematically donate differently from subjects with low social proximity to their peer ($p = 0.973$, see Figure B.7).

[24] The belief change due to increasing the peer's incentives from *zero* to *moderate* increases one's donations by 0.388 units ($p < 0.001$), while the belief change due to increasing the peer's incentives from *zero* to *high* has a precise null effect on donations of 0.076 ($p = 0.448$).

driven by a substitution effect due to altruistic crowding-out. Altruistic agents may decrease their donations when they expect that incentives cause the peer to increase donations. However, in such a model, an agent's donations decrease globally with her peer's incentives: the reason is that increased donations by the peer always lower the marginal utility of one's own donation. Thus, impure altruism alone cannot explain our findings, as we find non-monotonic effect of the peer's incentives on the agent's behavior.

Another hypothesis is that substitution effects co-exist with conformity and explain the diminishing conformity when peer incentives are *high*. This hypothesis is also at odds with the evidence. Low oneness subjects believe that their peer respond to incentives strongly and monotonically, but their donations do not respond to the incentives of their peer ($p = 0.607$). All the non-monotonic response to peer incentives is driven by high oneness subjects. However, the pattern in this group is also inconsistent with the substitution hypothesis. They believe that changing incentives for their peer from *moderate* to *high* has no significant effects on the peer's donations ($p = 0.750$) and yet they react by reducing their own donations by 0.667 units ($p = 0.007$).

The diminishing conformity interpretation is reminiscent of influential papers by Gneezy and Rustichini (2000a,b) and the more recent study of Fuster and Meier (2009). From their experiments, these authors conclude that incentives weaken adherence to the norms of behavior dictated by the actions of a social reference – let this be a small group or society. An important novel element of distinction of our findings is that incentives do not seem to simply shut down adherence to social norms: in fact, the magnitude of incentives matters. Relatively small incentives to act prosocially can preserve a certain level of norm adherence and produce social influence.[25] When this is the case, our evidence suggests that larger incentives are more likely to backfire on the positive spillovers of social influence, and perhaps the power of small (but not large) incentives could be leveraged to crowd in donations.

[25]Ostracism as in Dutta et al. (2018) allows us to endogenize social norms to demonstrate that it is not the mere incidence of payments that damages norm following, but sufficiently large incentives are instead needed. Albeit aligned with our evidence, for the absence of social interactions *after* the donation, we cannot meaningfully use this theory to explain our findings.

3.3. Incentive Inequality and Donor's Morale

In the interpretation of our results, we have so far ignored the possibility that incentive inequality in itself may affect an agent's morale to work on a task to generate donations for a charity. To assess this potential mechanism, we consider a model that incorporates such effects from incentive inequality (Breza et al., 2017). Such a model predicts that conditional on one's own incentives, donation levels should be highest when incentives for both players in a pair are equal, and monotonically decrease with the gap between one's own and peer's incentives. In our setting, this implies a set of inequality relationships in average level of giving between treatments, that we derive in Appendix A.3 and summarize in Table 4. We refer to these inequality relationships as the *main diagonal condition*.

Table 4: Inequalities in Average Donations between Incentivized Treatments Predicted by the Main Diagonal Condition

		Incentives to peer				
		Zero		Moderate		High
Incentives to self	Zero	$\mu_{n,n}$	$>$	$\mu_{n,m}$	$>$	$\mu_{n,h}$
	Moderate	$\mu_{m,n}$	$<$	$\mu_{m,m}$	$>$	$\mu_{m,h}$
	High	$\mu_{h,n}$	$<$	$\mu_{h,m}$	$<$	$\mu_{h,h}$

These predictions immediately appear in contrast with raw averages of donations presented in Table 2, where we observe that conditional on the agent's private incentives, increasing inequality often leads to more donations. Instead of focusing on local violations, we devise a likelihood ratio test of the joint null hypothesis that the *main diagonal condition* explains the first moments of donations and beliefs in our data (Burks et al., 2009). These tests, which are discussed in detail in Appendix B.1, largely reject the joint hypothesis for both donations and beliefs. Rejections are especially strong when we focus on the behavior and beliefs of high oneness subjects. Taken together these findings rule out incentive inequality as an explanation for our social influence effects.

3.4. Other Mechanisms of Social Influence

Mechanisms such as social learning, social consumption, reciprocity, and conformism have been proposed to explain the large body of evidence in support of the hypothesis that most individuals are conditional co-operators (Frey and Meier, 2004). In this section we discuss other mechanisms that can generate spillovers of giving in applications similar to the one we consider. Further, we explain how the experimental design allows us to rule out these explanations.

Social learning. When people are asymmetrically informed about relevant parameters, observing others' behavior can facilitate information aggregation. In any charitable giving context, the social value of a prosocial activity is uncertain, and the attitudes of others towards the charitable activity may indeed be informative about the quality of the charity or the social norm of giving to the specific cause. Our experiment excludes any scope for social learning. We make clear to subjects that the value generated from a donation is 0.25 USD and that this is common knowledge. Yet, the effectiveness of *Médecins Sans Frontières* in generating social value may be uncertain and some subjects may know the charity better than others. Our experiment rules out this channel by keeping donations private.

Joint consumption. Especially for volunteer work, this mechanism can play an important role in producing social influence. The prosocial action may involve social activities that confer consumption utility from forming relationships, sharing common experiences, and other pleasant interactions during the activity. The lack of social interactions among participants during the donation makes it easy to rule out this mechanism.

Reciprocity. This mechanism is often used to explain behavior in *local* social dilemmas - where agents directly benefit from the prosocial behavior of others. In most cases, charitable giving can instead be regarded as a *global* social dilemma, in the sense that agents only benefit marginally from the prosocial behavior of others. In such settings, we cannot expect that reciprocity could generate first order effects.

Signaling motives. The theory of Benabou and Tirole (2006) proposes the signaling of altruism and greed as channels that endogenuously lead to strategic complementarity or substitutability of donations. They show that complementarities arise when,

as more people decide to donate, the image of the pool of donors deteriorates faster than the image of non-donors. Our context is highly anonymous and our results are unlikely to be driven by *social* signaling. At the same time, we recognize that the Benabou and Tirole (2006) model admits a self-image interpretation.[26] However, for self-signaling to explain variation in donations, the treatment should affect the inference individuals can make about their own type, which cannot be in our setting where peer incentives are random.[27]

Social influence in work effort. One possibility is that the social influence observed in this study may have to do with conformity in work effort rather than in prosocial behavior. While we do not have a parallel experiment to rule out this channel, prominent existing studies on social influence in the workplace (e.g. Mas and Moretti, 2009; Bandiera et al., 2010) show that some degree of socialization or observability of co-workers' effort during the activity is necessary for this channel to matter empirically. Because subjects in our experiment work on the real effort task for the charity in isolation from their peer, we believe that this channel plays a trivial role, if any.

4. Conclusion

This study proposes a novel experiment to study social influence independently of social learning. In our experiment, pairs of players collaborate on a task that provides the opportunity to develop social proximity with their peer. Each individual then independently generates donations to a charity through a tedious task, knowing both her incentives and the incentives of her peer.

We provide evidence that information about the economic environment faced by a social reference is sufficient to spread social influence. Agents respond to increases in their peer's incentives by expecting that their peer will donate more and in turn, they donate more themselves. Our result are in line with a model of social influence in which conformity is driven by identification with an attractive role (Kelman, 1961). We find that conformity in donations is stronger when the agent feels socially close to

[26] Especially, we do not dispute that signaling motives and conformity may have related behavioral roots. Jones and Linardi (2014) find that making signaling motives more salient increases conformism.

[27] Random assignment of peer incentive m_j implies that for inference about own altruism type v_i, without observing peer donation d_j, $E_i(v_i|d_i, m_i, m_j) = E(v_i|d_i, m_i)$.

her peer, and her response to the incentives of the peer are non-monotonic.[28]

Our results also have methodological implications. Increasingly, social scientists are becoming interested in studying the relationship between beliefs about others' behavior and individual behavior. Such empirical efforts often have to overcome several challenges, including the notorious reverse causality issue of *false consensus*.[29] An approach that is increasingly used in experiments to overcome similar challenges is to introduce sources of belief variation that serve as instruments for beliefs (see e.g. Smith, 2013; Costa-Gomes et al., 2014). The non-monotonicity of donations in peer's incentives is a warning that different incentives can generate IV estimates that are potentially contradictory if we do not account for the model through which beliefs cause behavior.

This evidence is informative of the mechanisms underlying conformity. As noted by Dutta et al. (2018), whether conformity is a preference or a social norm is difficult to say in most empirical settings. An individual may prefer to *internalize* social norms instead of doing the introspection needed to determine her favorite strategies. While we do not make this distinction, we think that our design makes it hard for individuals to internalize social norms for these not being readily available. In fact, because others' behavior is not observable, in order to enjoy any of the benefits of internalization, subjects would first have to accurately assess what is the social norm in a relatively unfamiliar environment.

An implication of our results is that small incentives can be more effective at crowding in prosociality, and non-pecuniary interventions may be better suited to leverage social influence. Market designers should be cautious with changing incentives for activities that are partly regulated by a social contract because larger incentives are more likely to backfire on social influence. Consistent with this interpretation is the surprising evidence that *better* paid police officers in West Africa become *more* corrupt (Foltz and Opoku-Agyemang, 2015).

More broadly, by distinguishing conformity from social learning, our results illustrate the potential of social influence even in settings where social information is un-

[28] Our setup does not distinguish between probabilistic and deterministic incentives. While it is possible that this probabilistic framing reinforces the effects, it is difficult to see how the framing alone (without the higher expected payment) would generate the non-monotonicity we observe.

[29] The concern that beliefs reflect more the response function of the *observer* than the one of the *observed*.

likely to be informative of the quality of an activity. This improves our understanding of the propagation of social influence in other applications, like exercising (Aral and Nicolaides, 2017) and political mobilization (Bond et al., 2012), water and energy conservation (Ferraro and Price, 2013; Allcott and Rogers, 2014) where personal tastes are likely unaffected by social information. A concern in this literature is that social-norm information can be a double-edged sword, for it may lead to bunching of outcomes around the norm. However, many of these recent field studies find that social-norm information also leads to adjustment in the socially desirable direction for individuals that are already doing better than what is dictated by the social norm. Our results suggest that previous findings can be explained by conformity to expectation of how others will react to social information. Assessing the portability of our results is an interesting avenue for future research.

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A. Theoretical Appendix

A.1. More general framework

Consider the following more general formulation of the model presented in section 2.2 for any distribution of types $F(v)$ and cost function $c(\cdot)$ such that $c'(\cdot) > 0$, $c''(\cdot) > 0$ and $c(0) = 0$.

$$U(d_i) = (v_i + m_i)d_i - c(d_i) - \frac{\lambda_{i,j}}{2} Pr(\mathscr{A}_j(m_j))(d_i - E(d_j^n|\mathscr{A}_j(m_j)))^2$$

where $\mathscr{A}_j(m_j) = \{v_j \in V : c(d_j^n)/d_j^n > m_j\}$. Assuming that $Pr(\mathscr{A}_j(m_j))$ and $E(d_j^n|\mathscr{A}_j(m_j))$ are continuous in m_j, the model gives the following comparative statics for the effect of peer's incentives on individual donations:

$$\frac{\partial d_i^*}{\partial m_j} = \Lambda \left[\frac{\partial Pr(\mathscr{A}_j(m_j))}{\partial m_j} \left(E(d_j^n|\mathscr{A}_j(m_j)) - d_i^* \right) + Pr(\mathscr{A}_j(m_j)) \frac{\partial E(d_j^n|\mathscr{A}_j(m_j))}{\partial m_j} \right]$$

where $\Lambda = \frac{\lambda}{c''(d_i^*) + \lambda Pr(\mathscr{A}_j(m_j))}$ and $\mathscr{A}_j(m_j) = \{v_j \in V : c(d_j^n)/d_j^n > m_j\}$. This result leads to the following Lemma:

Lemma 1. *For cost functions with constant elasticity of donation effort $k \leq 1$, (i) peer's incentives tend to increase (decrease) individual donations for the more (less) altruistic and those with higher (lower) private incentives m_i to donate. However, (ii) there exists a $\tilde{m}_j$ threshold above which $\frac{\partial d_i^*}{\partial m_j}$ is 0 for any i.*

Part (i) of the lemma follows from the observation that a necessary condition for $\frac{\partial d_i^*}{\partial m_j}$ to be negative when $\frac{\partial Pr(\mathscr{A}_j(m_j))}{\partial m_j}$ is negative is that $E(d_j^n|\mathscr{A}_j(m_j)) \geq d_i^*$. This prediction is specific to this model and runs counter to models of (impure) altruism where donations are more likely to be strategic substitutes for more altruistic donors. To see why this happens, notice that conformity compels less altruistic individuals to donate more than they would like, and increasing incentives for the social reference has two effects on the utility loss from not conforming $Pr(\mathscr{A}_j(m_j))(d_i - E(d_j^n|\mathscr{A}_j(m_j)))^2$: First, an increase due to the increase in d_j^n; second, a decrease due to the decrease in $Pr(\mathscr{A}_j(m_j))$.

The latter can be seen as alleviating the pressure to conform and helps individuals adjust their donations towards their natural type.

Part (ii) of the Lemma requires proof, which we provide below.

Proof. The main step of this proof is to show that when the elasticity of donation effort with respect to the private value of effort $v_j + m_j$ is less or equal to 1, $Pr(\mathscr{A}_j(m_j))$ is decreasing and $E(d_j^n|\mathscr{A}_j(m_j))$ is increasing in m_j.

Because $\frac{c(d_j^n)}{d_j^n}$ is monotonically increasing in m_j and v_j, all we need for $\frac{\partial Pr(\mathscr{A}_j(m_j))}{\partial m_j} \leq 0$ is to show that the marginal altruistic types are excluded from $\mathscr{A}_j(m_j)$ when m_j increases.

That is, take $v_j^{'}$ and $m_j^{'}$ s.t.

$$\frac{c(d_j^n(v_j^{'}, m_j^{'}))}{d_j^n(v_j^{'}, m_j^{'})} = m_j^{'},$$

we want to show that a positive h implies

$$\frac{c(d_j^n(v_j^{'}, m_j^{'} + h))}{d_j^n(v_j^{'}, m_j^{'} + h)} \leq m_j^{'} + h.$$

Notice that this is always the case if $\frac{\partial}{\partial m_j}\left[\frac{c(d_j^n)}{d_j^n}\right] \leq 1$. With that in mind, consider

$$\frac{\partial}{\partial m_j}\left[\frac{c(d_j^n)}{d_j^n}\right] = \frac{(c'(d_j^n)d_j^n - c(d_j^n))\frac{\partial d_j^n}{\partial m_j}}{(d_j^n)^2} = \frac{(v_j + m_j)}{d_j^n}\frac{\partial d_j^n}{\partial m_j} - \frac{c(d_j^n)}{d_j^n}\frac{\frac{\partial d_j^n}{\partial m_j}}{d_j^n}$$

which for cost functions characterized by constant elasticity k of donations with respect to the the value of donations $(v_j + m_j)$ can be rewritten as[30]

[30] Using

$$k = \frac{v_j + m_j}{d_j^n}\frac{\partial d_j^n}{\partial (v_j + m_j)} = \frac{v_j + m_j}{d_j^n}\frac{\partial d_j^n}{\partial m_j}$$

$$\frac{\partial}{\partial m_j}\left[\frac{c(d_j^n)}{d_j^n}\right] = k - \frac{c(d_j^n)}{d_j^n}\frac{k}{(v_j+m_j)}$$

using $c''(d) > 0$, $c'(d) > 0$, and $c(0) = 0$, we know that $0 \leq \frac{c(d_j^n)}{d_j^n} \leq (v_j + m_j)$. In turn, $k \leq 1$ is sufficient condition for $\frac{\partial}{\partial m_j}\left[\frac{c(d_j^n)}{d_j^n}\right] \leq 1$.

Under the same conditions, $E(d_j^n|\mathscr{A}_j)$ is increasing in m_j. This is because, as we have shown, larger incentives drive less altruistic types out of $\mathscr{A}_j(m_j)$ and because $\frac{\partial d_j^n}{\partial m_j} > 0$ for any v_j. □

A.2. Impure Altruism

In this section, we consider the properties of a model in which individuals have decreasing marginal utility from aggregate donations to the charity. We model this as

$$u_i = (v_i + m_i)d_i + g(d_i + d_j) - \frac{c}{2}d_i^2 \tag{A.1}$$

where we can now distinguish between warm-glow and pure altruism in an impure altruism function that is linear in warm glow $v_i d_i$ and has pure altruism $g(D)$ with $g'(D) > 0$, but $g''(D) < 0$. In addition, we require that $|\frac{cg''(D)}{c-g''(D)}| < 1$ in order to guarantee interior solutions. The Nash equilibrium in this game is characterized by an analogue to equation 2.3:

$$d_i = \frac{m_i + v_i + g'(d_i + d_j)}{c} \tag{A.2}$$

and the corresponding condition for j. Performing comparative statics on A.2 show that an agent's donations are increasing in her own incentives, holding those of her peer constant:

$$\frac{\partial d_j}{\partial m_j} = c\frac{c - g''}{(c-g'')^2 - (cg'')^2} > 0 \tag{A.3}$$

Notice that the denominator in A.3 is positive because of the existence condition for interior solutions. For the impact of the peer's incentive on an agent, we find that

$$\frac{\partial d_i}{\partial m_j} = \frac{cg''}{c - g''} \frac{\partial d_j}{\partial m_j} < 0 \tag{A.4}$$

Thus, with (global) diminishing marginal utility from donations to the charity, an agent's donations are strictly decreasing in her peer's incentives, as claimed in the text. □

A.3. Incentive Inequality

One possible objection to leveraging heterogeneous monetary incentives to act prosocially for investigating the conformity channel of social influence is that incentive inequality in itself could be a source of strategic complementarities in donations. Recent research from Breza et al. (2017) shows that unjustifiably heterogeneous incentives in work environment can introduce a form of inequity aversion (Fehr and Schmidt, 1999) that damages morale to exert effort. The morale effect of incentive inequality is a salient form of inequity aversion even when opportunities for comparing realized payoffs are limited, and remains meaningful when payoff disparities depend on effort (rather than allocation decisions). In this section, we illustrate when this form of inequity aversion can induce strategic complementarities in donations.

Consider a simple model of prosocial behavior similar to the one presented in appendix A.1, and replace the conformity term with the morale utility term from Breza et al. (2017).

$$U(d_i) = (v_i + m_i)d_i - \frac{c}{2}d_i^2 + M(m_i, m_j)d_i \tag{A.5}$$

Morale $M(\cdot)$, as illustrated below, is a function of the gap in incentives between i and j, and allows for additional direct psychological incentive effects. Parameters α and β capture the extent to which people differentially dislike disadvantageous and advantageous inequality, respectively. The function $g(m_i)$ captures any sort of direct psychological effects of incentives, and $f(\cdot)$ is monotonically increasing in the gap between incentives and satisfies $f(0) = 0$.

$$M(m_i, m_j) = g(m_i) - \alpha f(m_i - m_j | m_i < m_j) - \beta f(m_j - m_i | m_i > m_j)$$

From this simple model we can derive the closed form of the optimal donation level, which is interpreted in the prediction that follows.

$$d_i^* = c^{-1}\left[v_i + m_i - \alpha f(m_i - m_j | m_i < m_j) - \beta f(m_j - m_i | m_i > m_j) + g(m_i)\right]$$

Prediction (Incentive Inequality)**.** *If donor's morale is damaged by incentive inequality, (i) at any m_i, i's donations are monotonically decreasing in the size of incentive inequality, and (ii) an increase (decrease) in either i's or j's incentives that reduces (increases) incentive inequality increases (decreases) donations of both i and j.*

The obvious implication of (i) is what we label a *main diagonal condition*: holding i's incentives constant, i's donations should be highest when incentives are homogeneous, and monotonically decreasing in the size of the $m_i - m_j$ gap.

Part (ii) further illustrates when incentive inequality introduces strategic complementarities in donations. However, notice how an increase (decrease) in m_j that accentuates (reduces) the gap between m_i and m_j decreases (increases) d_i and has a mixed effect on d_j – strengthening the strategic substitution of donations when the direct incentive effect on d_j dominates the negative (positive) effect of increased (decreased) inequality on j's morale.

B. Empirical Appendix

B.1. Morale Effects of Incentive Inequality

In this section, we test for the morale effects of incentive inequality using a joint test of the *main diagonal condition* that the model in Appendix A.3 implies.

The test of this joint hypothesis builds on Burks et al. (2009). We treat average donations in the nine incentivized treatments of our experiment as a nine-dimensional normal distribution with means μ_{p_i,p_j} (which we treat as unknown) and diagonal covariance matrix $\Sigma = \sigma^2{}_{p_i,p_j}\mathbb{I}$ (which we treat as known). For the joint test, we use maximum likelihood to determine the vector $\hat{\mu}_{p_i,p_j}$ that best fits the nine dimensional vector of sample means $\overline{Donation}_{p_i,p_j}$ - with and without the inequality constraints imposed by the *main diagonal condition*. A Likelihood Ratio test from the constrained and unconstrained likelihood functions is used to jointly assess these constraints. The test statistic is $\chi^2_{(d)}$ distributed with degrees of freedom d equal to the number of binding inequality constraints.

Table B.5 reports the raw first moments of the nine-dimensional distribution, the moments estimated with constrained Maximum Likelihood (constrained by the main diagonal condition), and the corresponding Likelihood Ratio tests. Both for the whole sample, and splitting the sample by oneness. Looking at the whole sample, one can notice qualitative violations of the main diagonal condition that cause the constrained estimates of the first moments to differ from the raw means. However, such violations are not sufficiently strong to reject the joint hypothesis ($p = 0.391$).

Next, we test the restriction on the samples split by oneness. In panel (b), we confirm that the restrictions imposed by inequity aversion cannot be rejected among low oneness subjects ($p = 0.737$). In panel (c), we strongly reject the *main diagonal condition* among high oneness subjects ($p = 0.002$). To understand how inequity aversion is rejected for more socially close subjects, it is worth interpreting the two main local violations that determine the results of the joint test. The first local violation is due to the change in average donations between groups of players who get randomized out of incentives: increases in their peer's incentives – that *ceteris paribus* increase incentive inequality – increase their own donations. This result is clearly inconsistent with the

morale effects of incentive inequality, and is also inconsistent with a concave altruistic utility of giving.[31] The second local violation is due to the change in average donations between groups of players who get randomized into relatively high incentives (*good news*): decreases in their peers' incentives – that *ceteris paribus* increase incentive inequality – increase their own donations. This result is significant for the decrease in peers' incentives from high to moderate, and may be explained by substitution due to concave (altruistic) utility of giving. However, evidence that expectations about peers' levels of giving are virtually identical between these two groups makes this explanation unlikely.

The *main diagonal condition* has a mirror set of conditions on beliefs across treatments. Table B.6 also shows rejection of the conditions imposed by the morale effects on incentive in equality on beliefs.

Taken together, the results of the analyses highlight that the complementarities observed in the data are at variance with the predictions of inequity aversion. This contrast is particularly stark among subjects with closer connection to their peer, which leaves our conformity framework as the more plausible explanation for our findings.

B.2. Additional Tables

[31] The standard framework of inequity aversion (Fehr and Schmidt, 1999), is less tractable in our setting because realized payoff inequality depends both on incentives provided and effort choices. Such a framework does however make the clear prediction that peer's incentives should not affect individual donations when an agent gets no incentives, and the t-test for one of the two local violations ($\hat{\mu}_{n,n} = \hat{\mu}_{n,m}$) reported in Table B.5 panel (c) provides evidence against this prediction.

Table B.5: Average Donations in Lottery Treatments, Maximum Likelihood Estimates (Coefficient Estimates and Standard Errors in Parentheses)

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to peer			Incentives to peer			Main Diagonal	
		Zero (1)	Moderate (2)	High (3)	Zero (4)	Moderate (5)	High (6)		p-value (7)
Incentives to self	Zero	3.233	3.417	3.190	3.320	3.320	3.190	LR: $\chi^2_{(2)} = 1.877$	0.391
		(0.217)	(0.230)	(0.209)	(0.217)	(0.230)	(0.209)		
	Moderate	5.042	5.546	5.155	5.042	5.546	5.155	Local Violations: t-tests	
		(0.233)	(0.235)	(0.224)	(0.233)	(0.235)	(0.224)	H0: $\hat{\mu}_{n,n} = \hat{\mu}_{n,m}$	0.551
	High	5.299	5.575	5.187	5.299	5.366	5.366	H0: $\hat{\mu}_{h,m} = \hat{\mu}_{h,h}$	0.215
		(0.233)	(0.229)	(0.212)	(0.233)	(0.229)	(0.212)		

(b) Low oneness		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to peer			Incentives to peer			Main Diagonal	
		Zero (1)	Moderate (2)	High (3)	Zero (4)	Moderate (5)	High (6)		p-value (7)
Incentives to self	Zero	3.190	2.667	2.593	3.190	2.667	2.593	$\chi^2_{(1)} = 0.113$	0.737
		(0.320)	(0.304)	(0.299)	(0.320)	(0.304)	(0.299)		
	Moderate	4.778	5.105	4.622	4.778	5.105	4.622	Local Violations: t-tests	
		(0.337)	(0.339)	(0.332)	(0.337)	(0.339)	(0.332)	H0: $\hat{\mu}_{h,n} = \hat{\mu}_{h,h}$	0.727
	High	5.549	4.889	5.382	5.456	4.889	5.456		
		(0.370)	(0.323)	(0.331)	(0.370)	(0.323)	(0.331)		

(c) High oneness		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to peer			Incentives to peer			Main Diagonal	
		Zero (1)	Moderate (2)	High (3)	Zero (4)	Moderate (5)	High (6)		p-value (7)
Incentives to self	Zero	3.270	4.099	3.614	3.635	3.635	3.614	$\chi^2_{(2)} = 12.443$	0.002
		(0.295)	(0.333)	(0.285)	(0.295)	(0.333)	(0.285)		
	Moderate	5.263	5.913	5.627	5.263	5.913	5.627	Local Violations: t-tests	
		(0.322)	(0.323)	(0.298)	(0.322)	(0.323)	(0.298)	H0: $\hat{\mu}_{n,n} = \hat{\mu}_{n,m}$	0.057
	High	5.103	6.293	5.034	5.103	5.581	5.581	H0: $\hat{\mu}_{h,m} = \hat{\mu}_{h,h}$	0.003
		(0.297)	(0.316)	(0.277)	(0.297)	(0.316)	(0.277)		

Notes: Degrees of freedom of the Likelihood Ratio test statistic equal the number of binding inequality constraints imposed by the composite null hypothesis. Empirical standard errors of the means are directly fed into the maximum likelihood routine.

Table B.6: Average Beliefs in Lottery Treatments, Maximum Likelihood Estimates (Coefficient Estimates and Standard Errors in Parentheses)

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$			Main Diagonal	
		Incentives to self			Incentives to self				
		Zero	Moderate	High	Zero	Moderate	High		p-value
		(1)	(2)	(3)	(4)	(5)	(6)		(7)
Incentives to peer	Zero	2.540	2.585	2.374	2.561	2.561	2.374		
		(0.182)	(0.193)	(0.174)	(0.182)	(0.193)	(0.174)		
	Moderate	4.331	4.832	4.100	4.331	4.832	4.100	$\chi^2_{(2)} = 6.277$	0.043
		(0.215)	(0.214)	(0.201)	(0.215)	(0.214)	(0.201)		
	High	4.637	5.086	4.374	4.637	4.708	4.708		
		(0.208)	(0.207)	(0.195)	(0.208)	(0.207)	(0.195)		

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$			Main Diagonal	
		Incentives to self			Incentives to self				
		Zero	Moderate	High	Zero	Moderate	High		p-value
		(1)	(2)	(3)	(4)	(5)	(6)		(7)
Incentives to peer	Zero	2.124	2.053	1.754	2.124	2.053	1.754		
		(0.241)	(0.264)	(0.225)	(0.241)	(0.264)	(0.225)		
	Moderate	3.703	3.992	3.357	3.703	3.992	3.357	$\chi^2_{(1)} = 0.041$	0.840
		(0.303)	(0.296)	(0.262)	(0.303)	(0.296)	(0.262)		
	High	3.907	4.301	4.213	3.907	4.256	4.256		
		(0.316)	(0.310)	(0.303)	(0.316)	(0.310)	(0.303)		

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$			Main Diagonal	
		Incentives to self			Incentives to self				
		Zero	Moderate	High	Zero	Moderate	High		p-value
		(1)	(2)	(3)	(4)	(5)	(6)		(7)
Incentives to peer	Zero	2.890	3.032	2.859	2.959	2.959	2.859		
		(0.265)	(0.272)	(0.249)	(0.265)	(0.272)	(0.249)		
	Moderate	4.901	5.530	4.878	4.901	5.530	4.878	$\chi^2_{(3)} = 12.248$	0.007
		(0.297)	(0.292)	(0.293)	(0.297)	(0.292)	(0.293)		
	High	5.157	5.783	4.500	5.124	5.124	5.124		
		(0.270)	(0.267)	(0.254)	(0.270)	(0.267)	(0.254)		

Notes: Degrees of freedom of the Likelihood Ratio test statistic equal the number of binding inequality constraints imposed by the composite null hypothesis. Empirical standard errors of the means are directly fed into the maximum likelihood routine.

Table B.7: OLS for Determinants of Social Proximity (Coefficient Estimates and Standard Errors in Parentheses)

Outcome: Oneness scale	(1)	(2)
Contact		1.434***
		(0.057)
Male	0.120*	0.139**
	(0.062)	(0.056)
Same gender	0.236***	0.180***
	(0.061)	(0.056)
Age, absolute difference	-0.003	-0.001
	(0.003)	(0.003)
Experience, absolute difference	-0.080***	-0.072***
	(0.024)	(0.022)
Constant	3.051***	2.118***
	(0.122)	(0.116)
Observations	2914	2914
R^2	0.014	0.189
Correlation in regression residuals (oneness scale) between peers	0.294 (0.340)	0.167 (0.340)

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$
Notes: All specifications include age group, experience, and session dummies. Standard errors are clustered at the pair level.

Table B.8: Incentive Effects on Donations (Coefficient Estimates and Standard Errors in Parentheses)

Outcome: Donations	(1)	(2)
Lottery	-0.763***	-0.835***
	(0.288)	(0.282)
Incentives to self (baseline: *Zero*)		
Moderate	1.968***	1.968***
	(0.184)	(0.181)
High	2.090***	2.058***
	(0.180)	(0.177)
Incentives to peer (baseline: *Zero*)		
Moderate	-0.286	-0.195
	(0.258)	(0.255)
High	-0.287	-0.263
	(0.261)	(0.258)
Moderate × High oneness	1.170***	1.049***
	(0.349)	(0.343)
High × High oneness	0.486	0.445
	(0.332)	(0.328)
Constant	3.906***	4.680***
	(0.262)	(0.345)
Controls	No	Yes
Observations	2914	2914

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$
Notes: Specification with controls includes age group, experience, and session dummies. Standard errors are clustered at the pair level.

B.3. Additional Figures

Figure B.3: Joint Problem Solving Task Software Interface

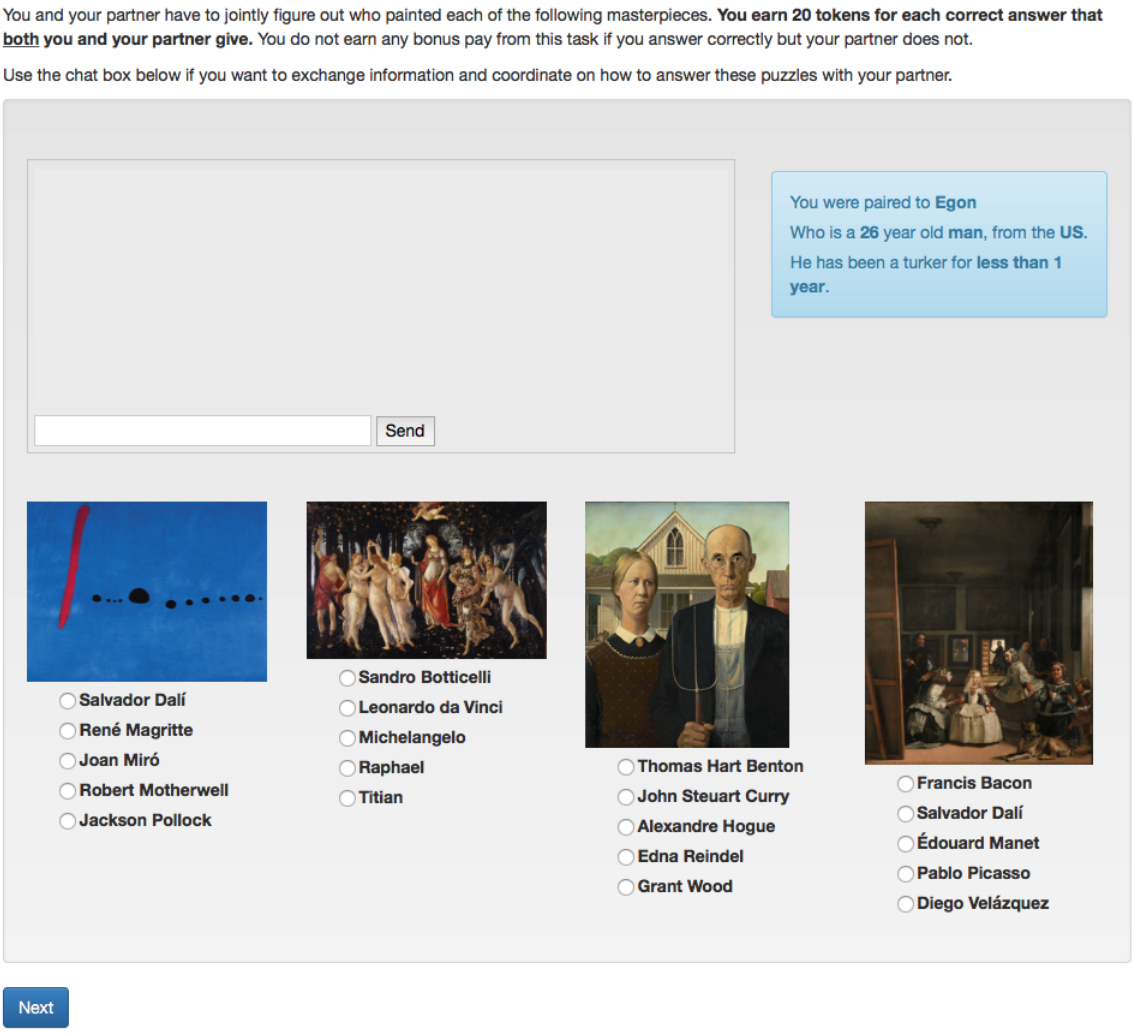


Figure B.4: Elicitation of the IOS (top) and WE (bottom) Scales

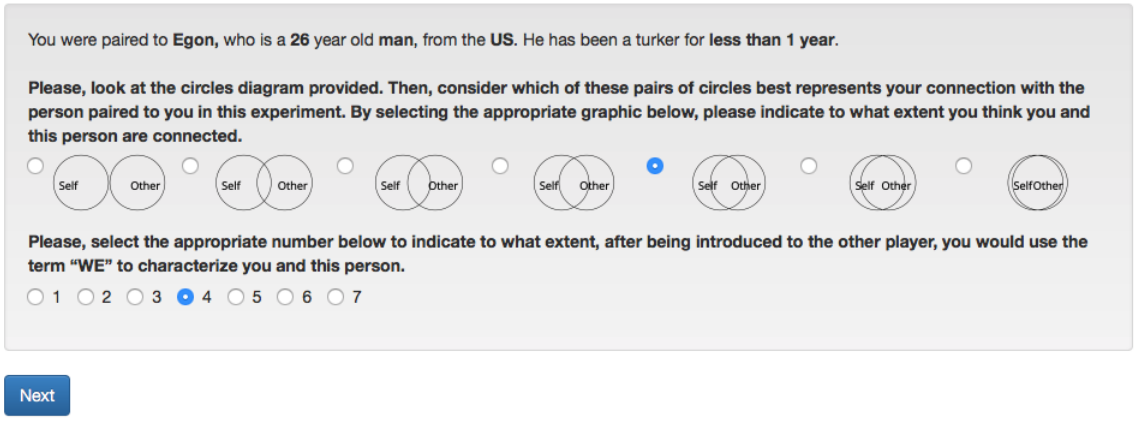


Figure B.5: Elicitation of Beliefs and Donations, and Treatment Assignment

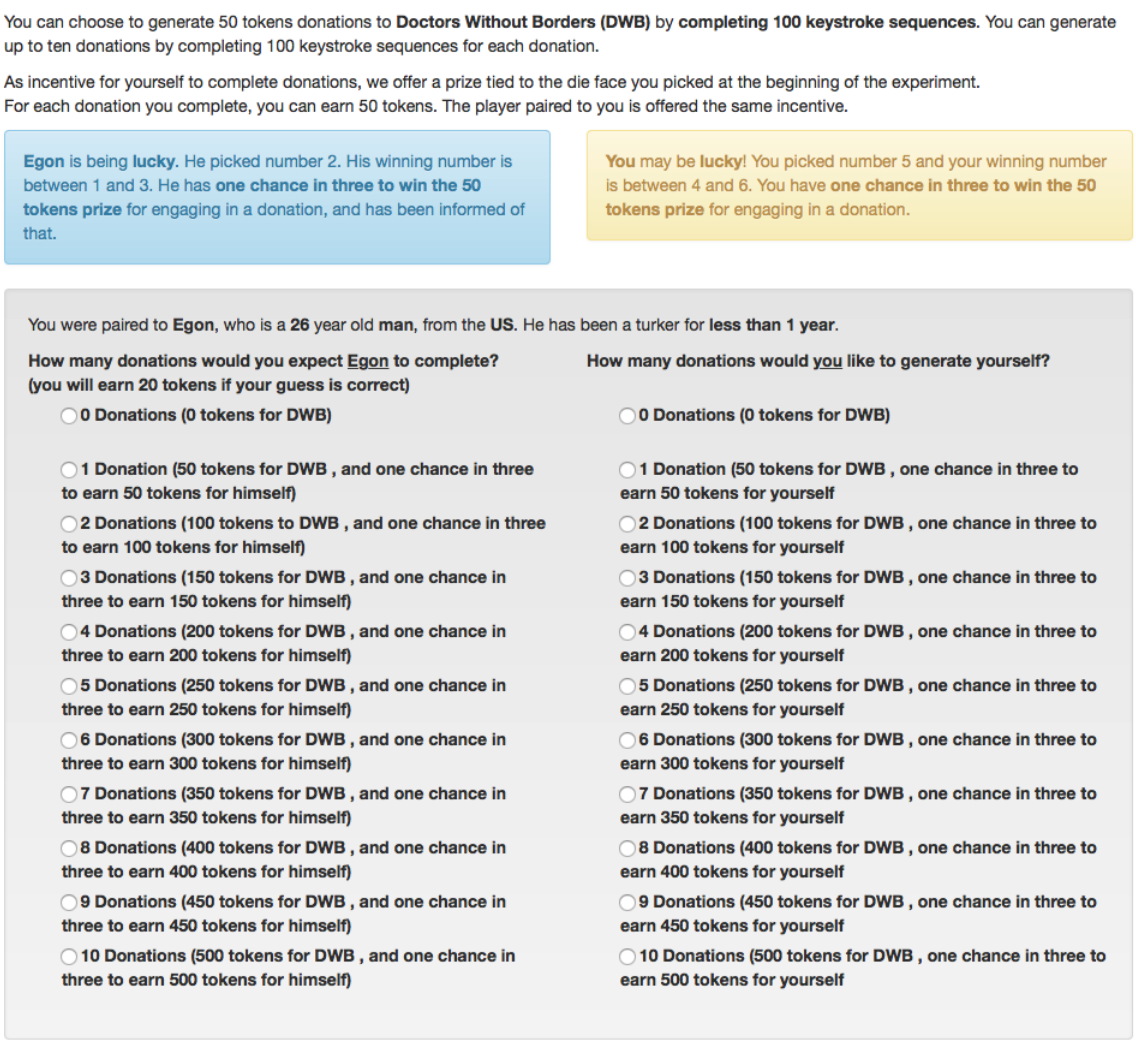


Figure B.6: Distribution of Social Proximity Scales

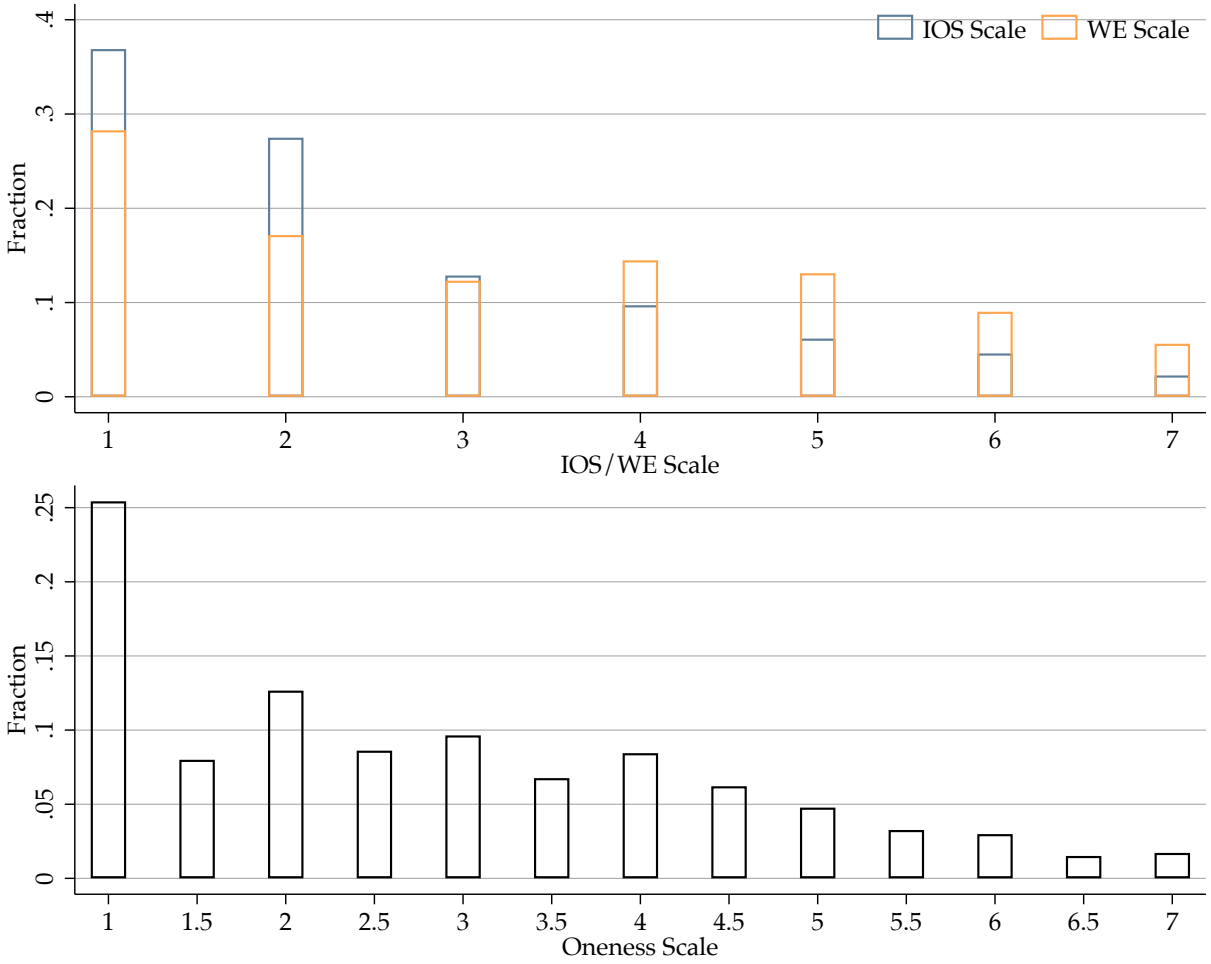
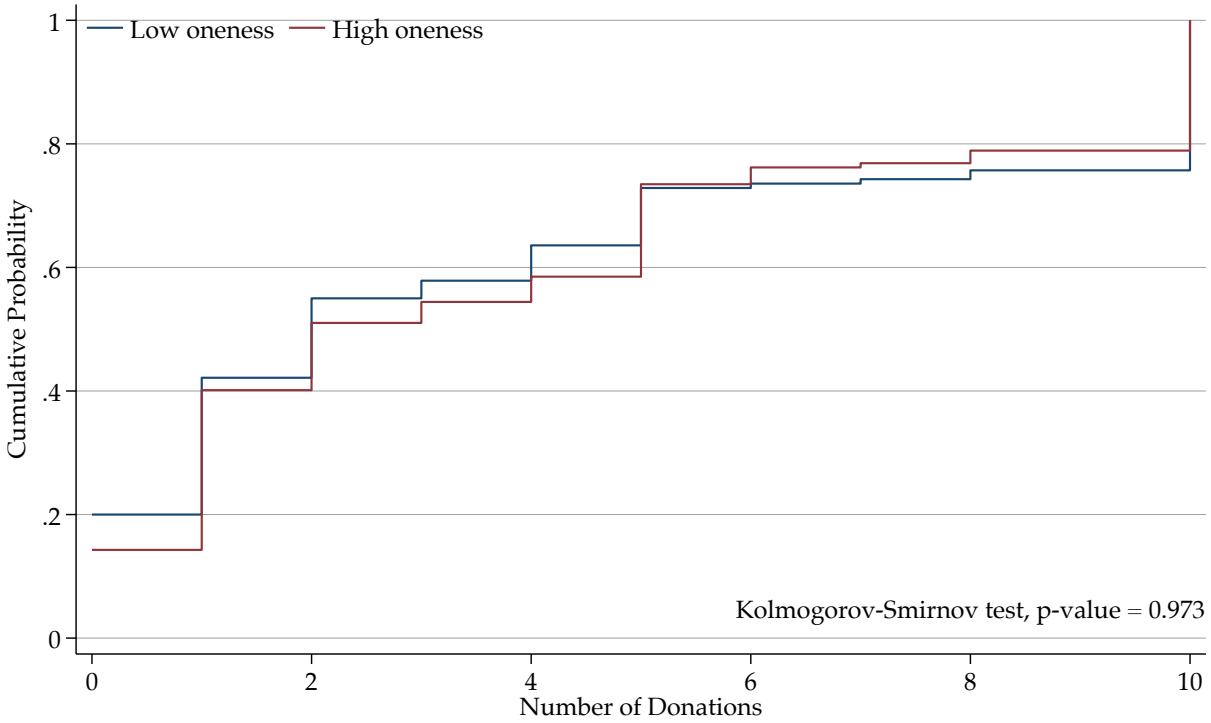


Figure B.7: Cumulative Density Function of Donations in Control Treatment, by Oneness Above/Below Median



C. Complete Instructions

C.1. Page 0: Consent

Please read this before clicking "Accept"

This HIT is an academic experiment on economic decision making. Based on how you play the experiment, we will donate money to a charitable organization.

By participating in this experiment, you are participating in a study performed by researchers at the University of Bonn. All data collected in this study are for research purposes only.

The experiment requires you to press keys on your keyboard. You thus need full dexterity in at least one hand. The experimental software complies with modern web standards, but may require a physical keyboard to detect your keystrokes. For part of the experiment you will be interacting with another player. To ensure that interactions occur in a timely manner we give each participant 5 minutes maximum to complete each of the following two pages. For the rest of the experiment a session timeout applies. Your session expires 40 minutes after you accept this HIT. If you do not want to complete the HIT within 40 minutes, we advise to return the HIT. We will not be able to approve work for timed out HITs.

Compensation: After completing this HIT, you will receive your reward plus a bonus payment that is based on how you play the experiment.

Legal information: Your participation is voluntary. You may stop participating at any time by closing the browser window or the program to withdraw from the study. Any reports and presentations about the findings from this study will not include any information that could identify you. We may share the data we collect in this study with other researchers doing future studies; if we share your data, we will not include any information that could identify you. By accepting this HIT, you indicate that you are older than 18 years and agree to participate in this experiment.

C.2. Page 1: Introduction

In this study each participant will be given the opportunity to engage independently in a real effort game. By participating you create value for a charity.

Part of your variable bonus may be uncertain. For this we will ask you to pick a number between 1 and 6, which the experimental software will match to a digital roll of die.

What face of a die would you pick? [drop-down list]

For this experiment you will be paired to another player that is currently participating in the same experiment. Given that part of the experiment will involve common problem solving, we would like pairs of players to get to know each other. For this, on the next page we are collecting some basic socio-demographic information, which will be shared with the paired participant. The socio-demographic information collected is minimal and does not make you personally identifiable.

Throughout the experiment, you will engage in tasks that will determine your variable bonus. Completing tasks you accumulate tokens. Tokens are converted to USD at the end of the HIT. One token is worth 0.005 USD.

This experiment is a research effort to understand economic behavior. In what follows there will be no deception: we will do nothing different from what is explained to you. For any question do not esitate to contact us.

C.3. Page 2: Survery on Demographic Information

We would like paired players to know a bit about each other. For this, we are collecting some basic socio-demographic information, which will be shared with the other participant.

What is your first name? [text field]

What is your age? [drop-down list]

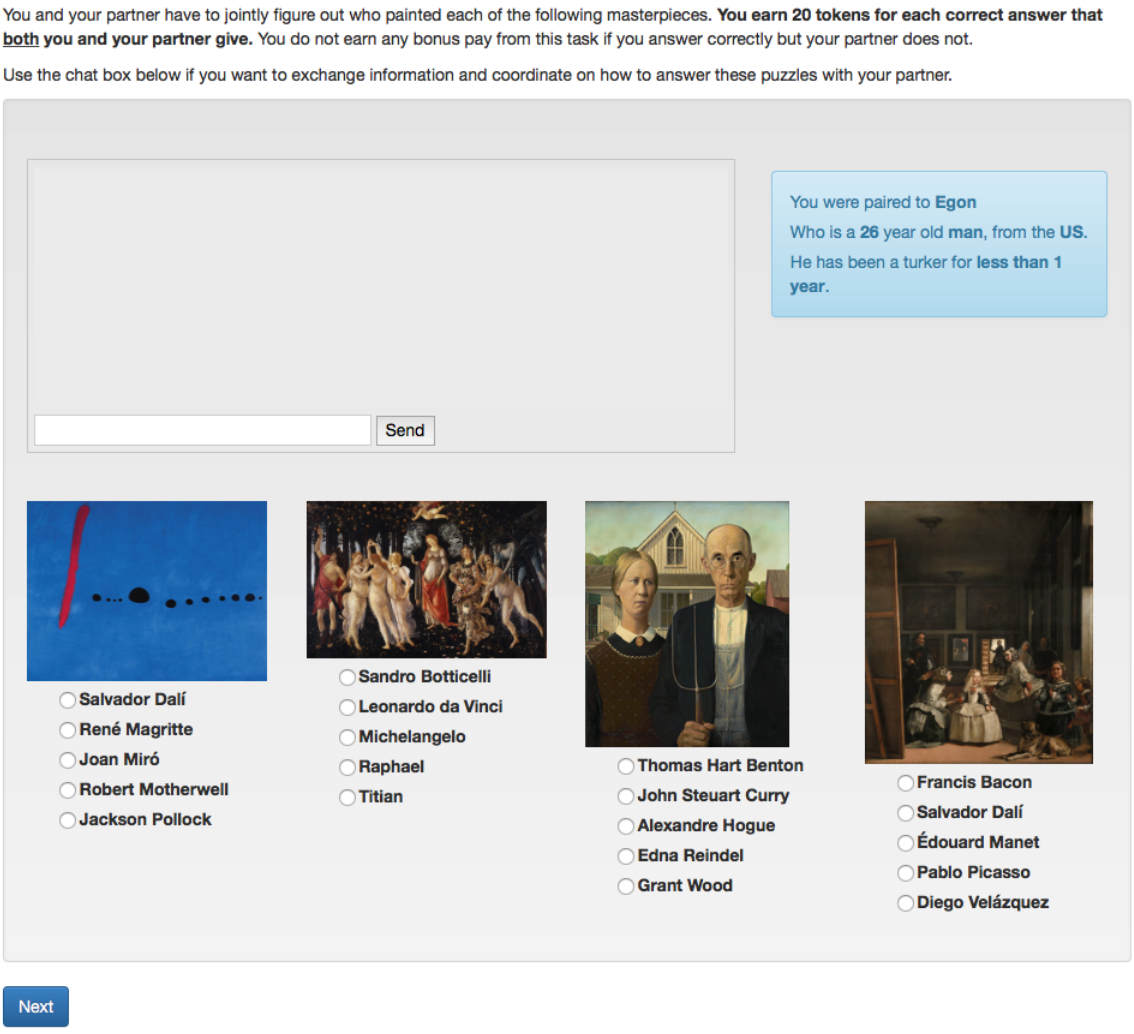
What is your gender? [drop-down list]

For how long have you been a turker? [drop-down list]

C.4. Page 3: Wait Page

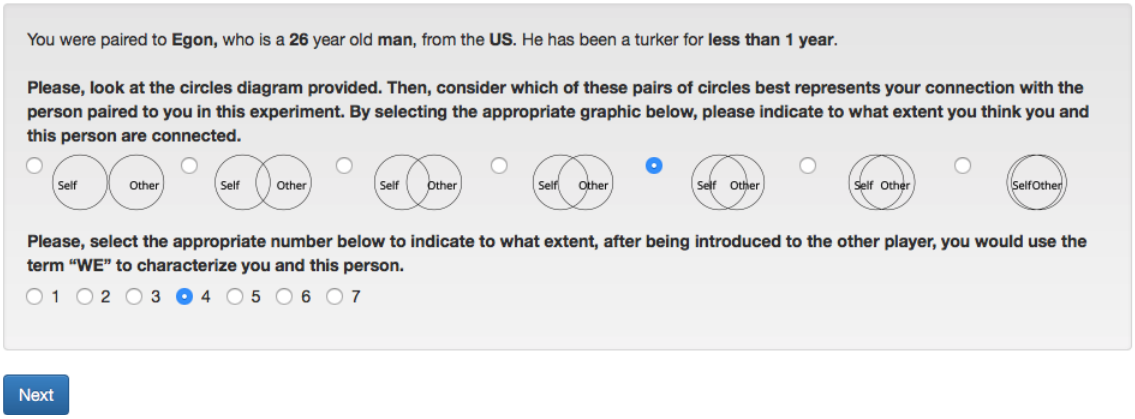
Please wait. Pairs are being formed.[32]

C.5. Page 4: Joint Problem Solving Task



[32] At this point of the experiment, each subject gets paired, randomly and anonymously, to another study participant.

C.6. Page 5: Oneness Elicitation



C.7. Page 6: Instructions for Donations

You will be able to engage in charitable giving by working on a simple assignment. Please carefully read the instructions below. Shortly, you will have the chance to familiarize yourself with this assignment in a training session. This will not affect your donation or payoffs. After the training, we will explain the payoffs for this task.

The assignment involves consecutively pressing the keys w e on your keyboard. You need to press the keys in this order. The keys are highlighted on the keyboard below. The software will display the number of successfully completed sequences.

You generate a donation to Doctors without Borders by completing a given number of sequences. A bar will indicate your progress towards this number.

In this example, you are asked to complete 100 keystroke sequences to generate a donation. Remember that this is just an example so that you can familiarize yourself with this assignment.

Please complete the training by pressing **w e** on your keyboard.

C.8. Page 7: Elicitation of Beliefs and Donations, and Treatment Assignment

You can choose to generate 50 tokens donations to Doctors Without Borders (DWB) by completing 100 keystroke sequences for each donation.

As incentive for yourself to complete donations, we offer a prize tied to the die face you picked at the beginning of the experiment. For each donation you complete, you can earn 50 tokens. The player paired to you is offered the same incentive.[33]

[Name_other_player] is being [lucky/unlucky]. [He/She] picked number [n]. [His/Her] winning number is between [1 and 3/4 and 6]. [He/She] has [no chance/one chance in three] to win the 50 tokens prize for engaging in a donation, and has been informed of that.[34]

[Name_other_player] picked number [n]. [He/She] has one chance in six to win the 50 tokens prize for engaging in a donation, and is aware of that.[35]

You may be [lucky/unlucky]. You picked number [m] and your winning number is between [1 and 3/4 and 6]. You have [no chance/one chance in three] to win the 50 tokens prize for engaging in a donation.[36]

You picked number [m]. You have one chance in six to win the 50 tokens prize for engaging in a donation.[37]

You were paired to [Name_other_player], who is a [age_other_player] year old[man/woman] from the US. [He/She] has been a turker for [less than 1 year/1 year/2 years/more than 2 years].

[33] Text displayed only if incentives were available.
[34] Text displayed only if other player's incentives were either *Zero* or *High*.
[35] Text displayed only if other player's incentives were *Moderate*.
[36] Text displayed only if personal incentives were either *Zero* or *High*.
[37] Text displayed only if personal incentives were *Moderate*.

How many donations would you expect [Name_other_player] to complete? (you will earn 20 tokens if your guess is correct)

○ 0 Donations (0 tokens for DWB)

○ 1 Donation (50 tokens for DWB, and one chance in [six/three] to earn 50 tokens for [him/her]self)

○ 2 Donations (100 tokens for DWB, and one chance in [six/three] to earn 100 tokens for [him/her]self)

○ 3 Donations (150 tokens for DWB, and one chance in [six/three] to earn 150 tokens for [him/her]self)

○ 4 Donations (200 tokens for DWB, and one chance in [six/three] to earn 200 tokens for [him/her]self)

○ 5 Donations (250 tokens for DWB, and one chance in [six/three] to earn 250 tokens for [him/her]self)

○ 6 Donations (300 tokens for DWB, and one chance in [six/three] to earn 300 tokens for [him/her]self)

○ 7 Donations (350 tokens for DWB, and one chance in [six/three] to earn 350 tokens for [him/her]self)

○ 8 Donations (400 tokens for DWB, and one chance in [six/three] to earn 400 tokens for [him/her]self)

○ 9 Donations (450 tokens for DWB, and one chance in [six/three] to earn 450 tokens for [him/her]self)

○ 10 Donations (500 tokens for DWB, and one chance in [six/three] to earn 500 tokens for [him/her]self)

How many donations would you like to generate yourself?

○ 0 Donations (0 tokens for DWB)

○ 1 Donation (50 tokens for DWB, and one chance in [six/three] to earn 50 tokens for yourself)

○ 2 Donations (100 tokens for DWB, and one chance in [six/three] to earn 100 tokens for yourself)

○ 3 Donations (150 tokens for DWB, and one chance in [six/three] to earn 150 tokens for yourself)

○ 4 Donations (200 tokens for DWB, and one chance in [six/three] to earn 200 tokens for yourself)

○ 5 Donations (250 tokens for DWB, and one chance in [six/three] to earn 250 tokens for yourself)

○ 6 Donations (300 tokens for DWB, and one chance in [six/three] to earn 300 tokens for yourself)

○ 7 Donations (350 tokens for DWB, and one chance in [six/three] to earn 350 tokens for yourself)

○ 8 Donations (400 tokens for DWB, and one chance in [six/three] to earn 400 tokens for yourself)

○ 9 Donations (450 tokens for DWB, and one chance in [six/three] to earn 450 tokens for yourself)

○ 10 Donations (500 tokens for DWB, and one chance in [six/three] to earn 500 tokens for yourself[a])

[a] Text displayed only if private incentives are available with positive ex-interim probability.

C.9. Page 8: Donation Task

You have chosen to make [D] donations. For this you will have to complete [D×100] keystroke sequences to generate these donations

Please complete the donation to Doctors without Borders by pressing w e on your keyboard.

C.10. Page 9: Short Questionnaire

Thank you for completing the donation task. Please fill out the short questionnaire below and then go to the next page to review payoffs and complete the HIT.

You and your partner could earn 20 tokens for guessing correctly how many donations the other did. Aside from the guessing question, was it clear to you that the number of donations that YOU made was not directly affecting the payoff of the other player? [Yes/No]

You and your partner could earn 20 tokens for guessing correctly how many donations the other did. Aside from the guessing question, was it clear to you that the number of donations that the OTHER made was not directly affecting your payoff? [Yes/No]

Did you realize that the amount donated to charity was increasing in the donations that both you and the other player made? [Yes/No]

In choosing how many donations to make, were you influenced by the thought of the number of donations the other person was going to make? [Yes/No]

Expecting that the other person could make more donations, makes you want to donate [More/Less/Indifferent]

In other contexts, when you are about to make a charitable donation, do you ever consider whether and how much other people are contributing to the same cause? [Always/Very often/Sometimes/Rarely/Never]

In other contexts, when you are about to make a charitable donation, expecting that other people could make more donations, makes you want to donate [More/-Less/Indifferent]

Please recall the screen where you chose how many donations to make. What

were the chances YOU had to win the lottery for participating in the donation task? [No chances/One chance in six/One chance in three/Cannot recall]

Please recall the screen where you chose how many donations to make. What were the chances the OTHER player had to win the lottery for participating in the donation task? [No chances/One chance in six/One chance in three/Cannot recall][38]

[38] Questions displayed only if incentives were available.