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

PRELIMINARY, COMMENTS WELCOME

Lorenz Goette Egon Tripodi

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Abstract

We study the role of social influence in the context of a charitable giving field experiment. Subjects observe another individual's incentives, but not their behavior. We find evidence of conformity. When individuals believe that incentives make other individuals contribute more, they also increase their charitable contributions. Moreover, conformity is stronger when individuals feel socially close to the other individuals. However, when incentives do not lead to higher expected contributions by other agents, individuals reduce their contributions. We examine whether this reduction can be explained by aversion to incentive inequality (Breza et al., 2017), but find little support. We conclude that norm adherence is weakened when incentives do not raise prosocial behavior.

Keywords: Prosocial behavior, social influence, online experiment.

JEL Classification:

*Goette: University of Bonn, Institute for Applied Microeconomics, Adenauerallee 24-42, 53113 Bonn; lorenz.goette@uni-bonn.de. Tripodi: European University Institute, Department of Economics, Via delle Fontanelle 18, 50014 San Domenico di Fiesole; egon.tripodi@eui.eu This project has benefited from several conversations with Michele Belot, Fabian Dvorak, David K. Levine, and Simone Quercia. We also thank numerous participants of the San Diego Spring School for useful feedback. Research funding from the University of Bonn is gratefully acknowledged. Carina Lenze and Luis Wardenbach provided excellent research assistance. All errors remain our own.

1. Introduction

The increasing social connectivity of modern times fosters opportunities for social interactions and comparisons with peers for many day-to-day decisions. A growing literature illustrates how social information propagates social influence both in online and in offline interactions.[1] People have been found to vote because they observe their friends to vote (Bond et al., 2012), and to donate to charity because their university colleagues (Frey and Meier, 2004) or co-workers (Kessler, 2017) were observed to donate. In these studies, observability makes it hard to understand *why* people are affected by others' behavior. An important question pertaining the mechanisms of social influence is whether people would also vote only because their friends may be *expected* to vote, and whether they would give to charity because their coworkers may be *expected* to donate. Although various models can explain that social influence spreads even for unobservable behavior and absent social information, there is no evidence of the empirical relevance of social influence in choice domains where actions are unobservable. Conformity can explain social influence and can be separately identified from learning about attributes and social norms when others' behavior is not observable.

In this project, we study how social influence can affect prosocial behavior when actions are not directly observable. Social influence is broadly defined as the effect of the observed (or expected) behavior of a relevant social reference group on individual behavior. This behavioral phenomenon makes individual behavior strategic complement to the behavior of others. Such complementarities are often ignored in standard models of prosocial behavior.[2]

We analyze the spread of social influence through a conceptual framework and an experimental design that identify a pure notion of *conformity* – the manifestation of an intrinsic desire to mimic the behavior of a relevant social reference – as a particular mechanism of social influence. *Conformists* are thought to adhere to the (expected) actions of a relevant social reference, which they perceive as having normative influence on their own decisions, even when these actions carry no informational content

[1] From the recent empirical literature, we notice that social influence plays an important role across a broad set of domains that include charitable giving (Lacetera et al., 2016; Kessler, 2017) marketing (Aral and Walker, 2011, 2014; Bapna and Umyarov, 2015), political participation (Bond et al., 2012; Cantoni et al., 2017), and well-being (Aral and Nicolaides, 2017).

[2] 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; Warr, 1982; Roberts, 1984) and impure altruism (Andreoni, 1989, 1990).

about the benefits of taking one action over another. Because such a model assumes that people have in their utility function a desire to adhere to social norms of behavior, rational agents will try to mimic others' behavior, even when actions are unobservable. The empirical investigation of conformity poses at least three important methodological challenges. First, preventing that the behavior of another person gives the opportunity to update either the perceived social norm or the perceived valence of certain actions. Second, identifying what is the relevant social reference whose behavior is taken as a norm to which agents try to adhere. Third, introducing exogenous variation in the (expected) behavior of a social reference. We propose an experimental design that overcomes all three such challenges. To clarify our approach, we illustrate theoretically how heterogeneous monetary incentives to act prosocially can be used to experimentally identify the conformity channel of social influence. Our rich experimental design also provides the tools to shed light on some of the key mitigating factors of social influence.

In a large online experiment, 3,467 strangers engage in pair-wise 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 in the pair. In this task, we keep constant the value that one unit of effort generates for a charity. For each of the two players in a pair, we simultaneously vary one of three levels of piece-rate (*none*, *moderate*, and *high*) personal incentives to generate charitable donations. Experimental variation in the incentives of the other player in the pair to generate donations allows to uncover social influence among partners: if Bob cares to adhere to the behavior of Abigail, an increase in the Abigail's incentives should increase not only the Abigails's donations but also Bob's. Endowed with a measure of expectations about partner's donations, we employ a novel strategy for identifying strategic complementarities in behavior by estimating the contemporaneous effect of partner's incentives on both expectations (about partner's donations) and donations of the player whose incentives were unaffected. Before the treatment manipulation, pairs of subject participate in a joint problem solving task, which we adopt to induce social proximity between paired players (Chen and Li, 2009) and increase relevance of the partner as a social reference. After that, we elicit a survey measure of social proximity (Cialdini et al., 1997). We then use this measure to investigate how social proximity determines how social influence differentially propagates among more and less closely connected partners.

As predicted by our conceptual framework of conformist social influence, we show that when changes in partner's incentives shift expectations about partner's donations, agents also donate more themselves. The significant strategic complementarities in donations are driven by subjects who exhibit a sufficiently strong social connection to their partner. Moreover, the stakes of partner's incentives to donate seem very important in producing the strategic complementarities of social influence. We find that increasing partner's incentives from *none* to *moderate* shifts upwards both expectations about partner's donations and one's own donations. However, an increase in partner's incentives from *moderate* to *high* incentives leads to an insignificant increase in expectations about partner's donations and tears down the positive indirect effects that *moderate* incentives to the partner have on one's own donations.

An important challenge in the interpretation of the evidence is that heterogeneous incentives may damage donors' morale and confound conformity with inequity aversion. We illustrate the predictions of a conceptual framework that incorporates morale effects of incentive inequality (Breza et al., 2017). These predictions are consistent with the behavior of subjects that exhibit a weak connection (below median oneness) to their partner, but fail to capture patterns – of (i) increases in donations when higher incentives to the partner increase incentive inequality and (ii) decreases in donations when higher incentives to the partner decrease incentive inequality – observed among subjects with a strong connection (above median oneness) to their partner. These subjects expect virtually identical donation levels from partners with *moderate* and *high* incentives, but compared to baseline they increase donations by 0.213 ($p = 0.001$) standard deviations when partners get *moderate* incentives and increase donations by just 0.043 ($p = 0.479$) standard deviations when partners get *high* incentives. Having ruled out that these patterns can be explained by incentive inequality, we conclude that larger incentives – that do not increase partner's prosocial behavior – weaken norm adherence.

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; Duffy and Kornienko, 2010) and in the field (Frey and Meier, 2004; Shang and Croson, 2009; Chen et al., 2010; Fellner et al., 2013; Cantoni et al., 2017). Perhaps our key contribution to this literature is to show that, even when behavior is not observable, people subject to social influence try to infer how others behave and mimic their behavior.

The strategic complementarities of social influence are often attributed to behavioral mechanisms that include social learning (Vesterlund, 2003), joint consumption (Bruhin et al., 2015), reciprocity (Rabin, 1993), and conformity (Bernheim, 1994). To this literature, concerned with understanding the behavioral mechanisms of social influence, we make an empirical contribution by providing evidence of the empirical relevance of conformity and showing how social proximity and the economic environment shape social influence.

Compared to existing literature (Gneezy and Rustichini, 2000a,b; Fuster and Meier, 2009), our evidence enriches the understanding of how norm adherence is affected by the economic environment, indicating that the effect of incentives on adherence to social norms of behavior needs not be binary. In fact, we find that the *size* of incentives matters.[3] We also add, to an empirical literature documenting 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 et al. (2017)), 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 future interactions (heterogeneous for stronger and weaker ties) are completely absent.

Most closely related to ours is the work of Kessler (2017), who provides field and laboratory evidence that endorsement 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. We see our work as complementary to Kessler (2017) along two important dimensions: first, we develop an experimental design that makes, to the best of our knowledge, the first attempt to separately identify conformity from social learning; second, we shed light on some of the modulating factors of social influence that are relevant for theory and for the design of institutions and future studies.

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

[3]The theory of *ostracism* from Dutta et al. (2018) predicts that sufficiently large incentives are needed for norm adherence to collapse. Although ostracism is very unlikely to play a role in our environment, our evidence is consistent with this theory.

2. The Experimental Setup

2.1. Experimental Design

The experiment is conducted online with people registered as workers on Amazon Mechanical Turk. The experimental design develops over five steps and features a full 3×3 between-subject design plus an additional control treatment. All subjects take part in the experiment in randomly formed pairs. Prior to being provided details about the main experimental task, subjects make contact with the other player in the pair. Pairs are formed after the first (registration) step, and the first three steps are common to all pairs. At the fourth step, each pair is randomly assigned to one of ten treatments. The experiment is concluded with a short survey and review of the payoffs. We present below each of the five steps in detail.

1. Registration. Invited subjects, access our software interface. They read some general instructions in which we only provide summary information about the key steps of the experiment: subjects are told that they will be randomly paired to another player with whom they will jointly play a first task, and that a second task will follow for each subject to play independently. After reading these instructions, each subject chooses a number from 1 to 6, which they are told will matter for part of their variable pay at the end of the experiment. We introduce *tokens* as the experimental currency. This phase is concluded by a short survey to collect name, gender, age, and experience on Amazon Mechanical Turk, which they are told will be shared only with their partner. [4]

2. Joint problem solving task. As subjects get to this step of the experiment, pairs are formed at random and subjects are introduced to their partner: they read stated name, gender, age, country of residence, and experience on Amazon Mechanical Turk of their partner.[5] All our subjects are resident 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. For this task, pairs of players are faced with four famous paintings. Next to each painting are the names of four artists, and pairs of subjects are paid 20 tokens for each correct name on which they manage to

[4] 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 partner as acquaintance/friend. Chat scripts provide no evidence of pre-existing relationships among paired participants.

[5] The order of arrival to this page constitutes our random matching protocol.

agree.[6] Paired players communicate through a private online chat (see interface in Figure B.3) . Differently from Chen and Li (2009), to further encourage contact, we require that *both* players in a pair guess each artist correctly for correct answers to be valid for bonus pay.[7] Payoffs for this task are not revealed until the end of the experiment.

3. Oneness elicitation. Oneness represents our measure of social proximity for this study. This measure was first proposed by Cialdini et al. (1997) and then validated by Gächter et al. (2015) as a reliable measure of the subjective degree of closeness to another person. The oneness score is a simple mean of two underlying scores: (i) the IOS scale and the (ii) WE scale.[8] Each of these scales ranges from 1 to 7 (with 1 indicating low social proximity, and 7 high social proximity). 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 the other player is going to generate. We treat such point prediction as a good proxy of beliefs of other's giving.[9] 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.[10]

Prior to eliciting beliefs and donations, subjects go through a small training to familiarize themselves with the real effort task, and the software randomly assigns pairs of subjects into one of 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 partners, all players in the

[6] Admittedly, we make the task quite hard by listing after each painting artists who are mostly contemporaries and share a relatively similar style.

[7] Subjects cannot skip the task, but they are free to solve the puzzles without making contact with their partner.

[8] See Figure B.4 for an illustration of the elicitation method of these scales.

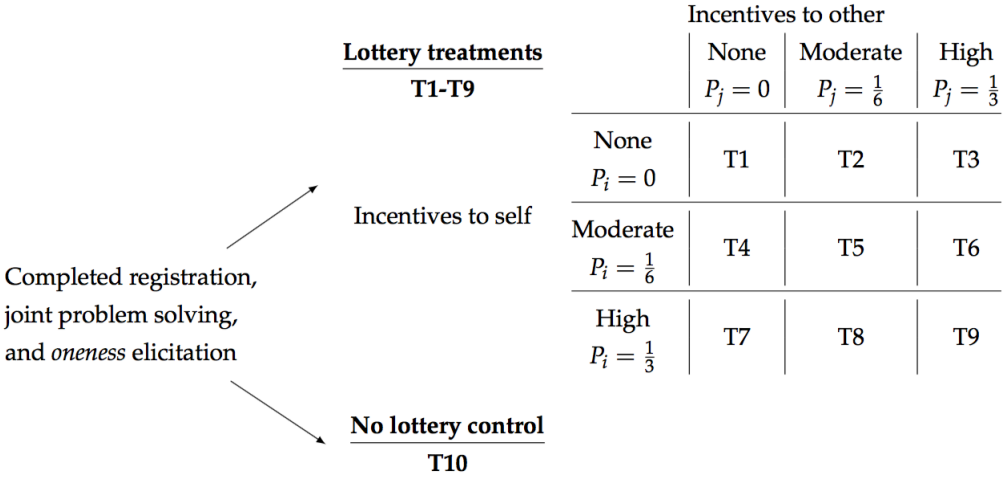
[9] For practical reasons we do not elicit the entire belief distribution, but instead use a measure that most likely captures the perceived mode of giving of the other player. To limit the scope for motivated reasoning, we incentivize correct predictions with a 20 tokens prize.

[10] 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, n.d.).

nine incentivized treatment conditions are provided with ex-ante identical lottery incentives: they earn 50 tokens for each donation generated if the number picked in *stage 1* matches a fair die roll. Across incentivized treatments, we vary, for each player, the *expected* stakes of monetary incentives by means of a simple information device that 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 feedback is provided, depending on the initial number chosen, this may reduce to zero the chances of getting the piece-rate incentive for generating donations (incentives are *none*), or increase chances to 1 in 3 (incentives are *high*). When this feedback 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*). Incentives of the two players in each pair are common knowledge for both players. To make sure that subjects understand both their own and the other player's incentives correctly we directly provide them with the updated probabilities of getting paid for generating 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 partner 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. After carrying out the real effort task that generates the chosen amount to be donated to the charity, subjects answer some unincentivized questions serving as comprehension checks. A review of the individual payoffs concludes the experiment.

Figure 1: Overview of Experimental Design and Treatment Assignment



2.2. Conceptual Framework and Predictions

To organize ideas about our strategy for identifying social influence, consider the following simple model of prosocial behavior. Two agents $a = \{i, j\}$ are prospected with the opportunity to make a donation d_a at a private cost $c(d_a)$. Personal benefit from overall donations $u(\sum_a d_a)$ is increasing and concave in the social value that donations generate.[11] Agents have an intrinsic preference for their own actions d_i to mimic those of their social reference d_j. Such preference is captured by a conformity function $v(.)$ that is convex and monotonically increasing in the gap between d_i and d_j, and satisfies $v(0) = 0$. We write the utility of agent i from contributing d_i as:

$$U(d_i) = u(d_i + d_j) - c(d_i) - \lambda_i \tau_{i,j} v(d_i - d_j) \tag{2.1}$$

Parameter λ_i captures the weight that an individual attributes to mimicking one's own social reference's behavior (Akerlof, 1997; Bernheim, 1994).[12] In a real-world situation, we see this as an individual preference for conforming to the behavior of an esteemed social reference, be it e.g. a parental figure, a role model, a friend. A conformist adheres to a norm of behavior dictated by the action of a relevant social reference.[13] By introducing $\tau_{i,j}$, we allow for the desire to conform to vary in strength depending on the relationship with the social reference: we expect $\tau_{i,j}$ to be affected by the nature of the relationship between i and the social reference j. Social proximity is one measurable element to characterize the nature of social relationships and modulates social influence (Bond et al., 2012).[14] Like in most models of prosocial behavior, donations are strategic substitutes due to the concavity of $u(.)$. Strategic complementarities are introduced by the conformity component of the utility function.[15]

[11] Function $u(.)$ is monotonically increasing and concave; $c(.)$ is monotonically increasing and convex.

[12] Conformity in this literature is distinct from theories of conformity where observing others' behavior is instrumental to uncertainty reduction see e.g. Banerjee (1992) and Bikhchandani et al. (1992).

[13] Sliwka (2007) describes conformists in a related way. In his model some agents are resolute while others are conformists – who think about resolute agents to mimic their expected modes of behavior.

[14] The notion that stronger ties are more relevant for social influence is also common in more sophisticated models of conformism (Patacchini and Zenou, 2012) and status seeking (Immorlica et al., 2017) in social networks.

[15] Super-modularity of the utility function, namely $\frac{\partial U(.)}{\partial d_i \partial d_j} = u''() + \lambda_i \tau_{i,j} v''(.) > 0$, defines strategic complementarity. Given concavity of $u(.)$, a necessary condition for supermodularity is $\lambda_i \tau_{i,j} > 0$.

2.2.1. Uncovering Social Influence Through Incentives

We focus on a simple structure of the utility function in equation (2.1) to straightforwardly illustrate how piece-rate incentives to behave prosocially m can be leveraged to identify the strategic complementarities induced by conformity. We assume the altruistic utility component $u(.)$ to be linear in the social benefit of a donation A, the cost of effort $c(.)$ and the conformity utility $v(.)$ to be quadratic.[16]

$$U(d_i) = (d_i + d_j)A + m_i d_i - c d_i^2 - \lambda_i \tau_{i,j}(d_i - d_j)^2 \tag{2.2}$$

We use symmetry to write the optimal donation level in this simple closed form

$$d_i^* = [A + m_i(c + 2\lambda\tau) + m_j 2\lambda\tau](c + 4\lambda\tau)^{-2}$$

that leads to the following prediction.

Prediction 2.1 (Conformity)**.** *Conformity makes donations strategic complements. Increasing j's incentives to act prosocially has a positive direct effect on j's donations, and a smaller positive indirect effect on i's donations.*

The key implication of this statement is that varying incentives of the social reference to act prosocially can be a sufficient manipulation for detecting social influence in the form of conformity. This theoretical framework offers two approaches to identify conformity through incentives. The first, less data demanding, resorts 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, precisely 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 feature the strategic complementarities of conformity,

[16] Additionally taking into account impurely altruistic preferences for donating to charity (Andreoni, 1989, 1990) shifts the level of donations, and makes i's and j's donations imperfect substitutes, but cannot make donations strategic complements – and therefore is irrelevant to the illustration of social influence. The structure of conformity utility $v(.)$ is typically assumed to be either an absolute value (Bernheim, 1994), or quadratic (Akerlof, 1997). Although typical structural assumption in the conformity literature impose symmetry around the behavior of the social reference, symmetry is not necessary for the strategic complementarities.

changes in j's incentives produce positively correlated effects on these two outcome variables.

Not all agents are expected to conform to their social reference to the same degree. This framework assumes that social proximity, between the agent whose incentives are affected and the social reference, modulates conformity, and that individuals have a potentially heterogeneous intrinsic preference to adhere to social norms of behavior. This framework does not say whether, for an individual, such intrinsic preference should be stable or potentially malleable to incentives. In the data, we expect to observe that a sufficiently weak intrinsic preference to conform (small λ), or sufficiently weak social ties to the social reference (small τ), leave donations insensitive by the incentives of others. Conversely, we also expect a stronger intrinsic preference to conform (large λ) and stronger social ties to the social reference (large τ) to propagate significant conformity. The extent to which one wishes to adhere to the behavior of a social reference λ may be endogenous to incentives. Providing a theory of endogenous norm adherence is beyond the scope of this paper, and we rather study this relationship empirically.

2.2.2. 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. The recent research of Breza et al. (2017) in fact shows that, in a work environment, unjustifiably heterogeneous incentives can introduce a form of inequity aversion (Fehr and Schmidt, 1999) that damages morale to exert effort.

In the setup of a prosocial activity, randomly determined heterogeneities in incentives could be detrimental for one's morale to generate donations for a charity. We explore the predictions of a simple model of prosocial behavior like (2.2), in which the morale utility term from Breza et al. (2017) replaces the conformity term.

$$U(d_i) = (d_i + d_j)A + m_i d_i - c d_i^2 + M(m_i, m_j) d_i \tag{2.3}$$

Morale $M(.)$, 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.

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

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

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

Prediction 2.2 (Incentive Inequality)**.** *If donors' 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 should be monotonically decreasing in the size of the $m_i - m_j$ gap.

Part (ii) of prediction 2.2 indicates that incentive inequality can make i's and j's donations strategic complements. However, notice that incentive inequality may also make donations strategic substitutes: 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 – making donations strategic substitutes when the direct incentive effect on d_j dominates the negative (positive) effect of increased (decreased) inequality on j's morale.

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.[17] This is an online platform where thousands of registered workers are commonly employed in tasks that require

[17] We collect data over more than one session to minimize the risks that server overload could cause the app that we developed using oTree (Chen et al., 2016) to crash.

human intelligence, which is becoming increasingly popular for conducting economic experiments (DellaVigna and Pope, 2016). Some people on this platform queue up well paying tasks that they plan to do later during the day: a 90 seconds time-out in the registration phase of the experiment screens out people that begin the experiment without making progress. 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).[18] In our experiment, subjects that complete the study earn 1.20 USD participation fee plus bonus pay. Tokens constitute the experimental currency at the exchange rate of 1 token=0.005 USD. Completing the experiment took participants 17.06 minutes on average. Including participation fee, on average, subjects earned 2.76 USD, of which 1.13 USD went to the charity of our choice – *Doctors without Borders*. Participation in the experiment is allowed only once, and no retakes are allowed for subjects that accidentally drop out of the study.

3. Experimental Results

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. Pairs of subjects scored on average 40 of the 80 available points in the JPS task: an amount that indicates significant effort considering that random click-through only grants an expected score of 12.8. After this task, we measure an average individual oneness score towards the partner of 2.801 (on a scale between 1 and 7). Across the ten treatment conditions, subjects on average generate 4.558 donations for Doctors withouth Borders, and predict their partner to generate an average of 3.851 donations. Table 1 illustrates detailed summary statistics of the main outcomes of interest across treatments. Table 2 shows balance in pre-treatment measures, and lack of differential attrition across treatments.

[18] 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 US.

Table 1: Donations and Beliefs about Other's Donations Across Treatments
(Means and Standard Errors)

		Beliefs			Donations		
Incentives offered							
No (control)		3.585			3.934		
		(0.205)			(0.222)		
Yes (3x3 treatments)							
		Incentives to other			Incentives to other		
		None	Moderate	High	None	Moderate	High
Incentives to self	None	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)

3.1. Social Proximity

As argued in the conceptual framework, conformity requires some degree of social connection to the social reference.[19] This section discusses the determinants of our measure of social proximity elicited among pairs of strangers after having interacted in the JPS task.

In this task, subjects have to agree on the correct artist to associate to each of four famous paintings. Through a chat box, subjects can make contact to discuss the correct answers and strategies to solve the task. The chat box also introduces each subject to the partner by reporting partner's stated first name, age, gender, level of experience on the Amazon Mechanical Turk platform, and country of 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 social proximity.

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

Table 2: Summary Statistics of Observable Characteristics and Attrition
(Means and Standard Errors in Parentheses)

	Full sample	No lottery	Lottery									p-value
Incentives to self			None			Moderate			High			
Incentives to other			None	Moderate	High	None	Moderate	High	None	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	2.5	2.62	2.582	2.513	2.487	2.529	2.548	2.5	0.882
	(0.021)	(0.068)	(0.065)	(0.065)	(0.066)	(0.066)	(0.069)	(0.064)	(0.068)	(0.065)	(0.065)	
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)	(0.093)	(0.088)	(0.083)	
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.

Table 3: 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 between partners	0.294	0.167
Correlation in oneness scale between partners	0.340	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.

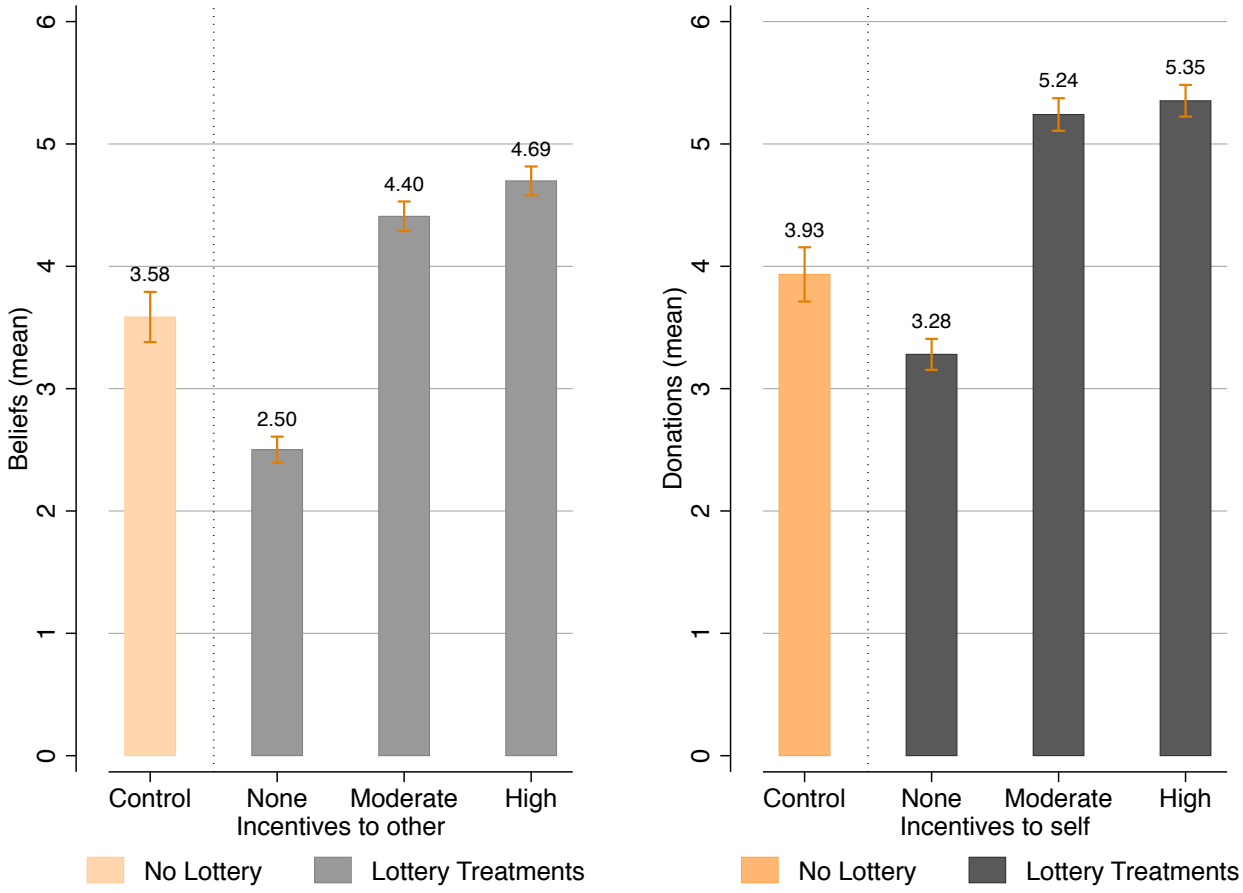
In Table 3, least squares regressions illustrate the correlates of social proximity in this experiment, and highlights the role of both *homophily* (Marmaros and Sacerdote, 2006) and chat box contact (Chen and Li, 2009) in the formation of social proximity. Although age difference with the partner does not seem to be highly predictive of social proximity, the partner being of the same gender and having similar experience on Amazon Mechanical Turk 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.[20] Players that make reciprocal contact with their partner 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)) between the regression residuals of the

[20] 80.4% of subjects used the chatbox to get in touch with the other player, and 64.6% of pairs managed to have a conversation (*contact* = 1). In these conversations, subject share their knowledge about the four paintings, share relevant personal information and considerations (for example, 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 jointly 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.

two players in each pair is an indication of the role played in this experiment by the opportunity to make contact for the development of a social connection.

3.2. Beliefs and Donations

Figure 2: Direct Incentive Effects



Notes: 2914 observations. Each of the gray bars pools the three treatments that maintain constant either partner's incentives (left panel) or player's incentives (right panel). Error bars indicate standard errors of the mean.

Before discussing social influence, at least four points are worth illustrating about the direct (and predicted direct) effects of monetary incentives on donations (Figure 2). First, we notice that beliefs pretty accurately capture the comparative statics of how others react to incentives, but underestimate the generosity of others.[21] Second, we

[21] This evidence is consistent with previous studies finding that average forecasts of subject participants accurately predict experimental results (DellaVigna and Pope, 2016), and that people underestimate others' prosocial attitudes (e.g. Goette et al. (2006)).

find a predictable *disappointment effect*: offering lottery incentives while providing information to the subject of an unlucky lottery draw (incentives are *none*) frustrates donations compared to the payoff equivalent *no lottery* control treatment in which subjects were not told that they might be provided incentives (rank-sum test: $p < 0.001$). Subjects also internalize the *disappointment effect* when they express their belief on the donation behavior of the other player. Third, information that doubles the expected piece rate for generating donations (i.e. from *intermediate* to *high*) has no significant effect on donations (rank-sum test: $p = 0.373$). Fourth, the moderate incentive alone, leads to 1.31 additional donations compared to the *no lottery* treatment. This result is quantitatively relevant because it indicates a crowding-in of donations: providing an incentive to volunteer time and efforts resulted in more resources transferred to charity than were spent to incentivize participants.[22] This result adds to an existing literature that documents the effectiveness of monetary incentives to act prosocially (Landry et al., 2006; Karlan and List, 2007). We find that lottery incentives can *crowd-in* donations. Taken together, these descriptive findings can be summarized in the following result:

Result 1 (Incentive Effects):

- *The availability of incentives to act prosocially increases charitable efforts, but doubling the stakes of incentives does not increase donations significantly.*
- *People that are told they may receive an incentive to donate, but are randomized out of incentives, decrease donations.*
- *The crowd predicts correctly the comparative statics of incentive effects, but systematically underestimates others' prosocial behavior.*

3.3. Conformity as Social Influence

We now move on to illustrate the social influence effects of incentives, through the conformity channel, in two steps. As a first step, we estimate the effect of changes in other's (j's) incentives on one's (i's) donations to determine if these are consistent

[22] Back of the envelope calculations help illustrate that payoffs to the charity were calibrated such that one hour of time volunteered for the charity would generate about 10 USD for the charity and an expected monetary pay to participant of 1.67 USD. In fact, each donation required an average of 86.81 seconds and generated 0.25 USD for the charity for sure and 0.25 USD with probability $\frac{1}{6}$ in the moderate incentive treatments.

with the predictions of the conformity framework. As a second step, to pin down the strategic complementarities of social influence, we estimate the effect of changes in j's incentives on i's beliefs about j's donations to find out if changes in j's incentives move beliefs about j's donations and i's donations in the same direction.

For the first step, we use regression equation (3.1) to estimate the indirect effect of j's incentives on i's donations. This regression model includes treatment indicators for the direct effects of i's incentives on i's donations, indicators for indirect effects of j's incentives on i's donations, makes use of the *no lottery* control treatment to isolate the *disappointment effect* discussed above, and controls for observable characteristics of both players in the pair. According to prediction 2.1, from this regression, we should interpret positive values of β_4 and β_5 – the effects of j's incentives on i's donations – as evidence consistent with the conformity mechanism of social influence.

$$\begin{aligned} Donation_i = \alpha + \beta_1 Lottery_i + \beta_2 Moderate_i + \beta_3 High_i + \\ + \beta_4 Moderate_j + \beta_5 High_j + \boldsymbol{X}_{i,j}\boldsymbol{\gamma} + \varepsilon_i \end{aligned} \tag{3.1}$$

In a second step, we pin down the strategic complementarities of social influence, by additionally estimating the mirror least squares regression (3.2) for individual predictions of the donation of the social reference. If donations are complements, we should observe expected increases in donations of the social reference j in response to j's incentives (δ_1 and δ_2) to move along with increases in i's donations.

$$\begin{aligned} Belief_i = \gamma + \delta_1 Lottery_j + \delta_2 Moderate_j + \delta_3 High_j + \\ + \delta_4 Moderate_i + \delta_5 High_i + \boldsymbol{X}_{i,j}\boldsymbol{\omega} + \epsilon_i \end{aligned} \tag{3.2}$$

The estimation of regression models (3.1) and (3.2) is presented in panels (a) and (b) of Table 4, respectively. Using the control *no lottery* treatment we isolate the *disappointment effect* of being offered a lottery incentive and being randomized out of incentives, which is estimated to decrease donations by 0.831 units (column (2)). Positive expected pay for generating donations increases donations by roughly two units, with no significant difference between the moderate expected pay and the high expected pay. Relevant to social influence, we find that donations increase by 0.356 units ($p = 0.055$)

when the other player receives moderate compared to no incentives, and remain unaltered when the other player receives high expected incentives. This evidence points in the direction of the following result.

Result 2 (Social Influence): *Increasing incentives for the other player to donate from none to moderate increases both expectations about the other's donations and one's donations. Increasing incentives for the other player to donate from none to high, increases expectations about the other's donations, but has instead no effect on one's donations.*

Remember that in a standard model of prosocial behavior, with concave (pure or impure) altruistic utility term $u(.)$, incentives are expected to increase giving. When incentives to donate are heterogeneous, altruistic donors give *less* if others are expected to give *more*.

We instead find, in line with social influence, that the introduction of moderate incentives for the other player to act prosocially lead to both an increase in expectations about donations of the other player and to an increase in one's donations compared to the condition in which the other has no monetary incentive to act prosocially.

Interestingly, we also find no evidence of social influence when the other player faces high incentives to act prosocially. In fact, subjects predict that the other player will respond strongly to high incentives by donating 2.240 ($p < 0.001$) more units than they would without incentives, but such an increase in expected donations from the other player does not seem to have an influence on one's donations. This can indicate either that the strategic substitutability of altruistic giving gets to dominate the strategic complementarity of social influence or that the factor of social influence λ is weakened when the behavior of a social reference is more strongly influenced by monetary self interest. The latter should be the case if the strength of one's desire to conform is a function of the likely intentions that determine the behavior of the social reference. The next two results illustrate the observed modulating forces of social influence and help shed light on this dilemma.

Result 2a (Social Proximity Moderates Social Influence): *Social influence effects are (not) significant among individuals with high (low) social proximity to the social reference.*

Our theoretical framework predicts that the behavior of a closer social reference should have a stronger social influence effect on one's donations. We investigate how

social influence spreads among people with high and low social proximity by partitioning the sample at the median score of oneness and estimating regression equations (3.1) and (3.2) in each of these partitions. To get a sense of what it means, in terms of real social relationships, for a subject to have oneness towards the other below (or above) median, we benchmark oneness measured in our study to Gächter et al. (2015). Average oneness in the bottom half of the oneness distribution of our sample is very close to the lower bound of the spectrum and far below oneness towards an acquaintance in Gächter et al. (2015). Average oneness in the top half of the oneness distribution, instead, is similar to oneness measured towards a non-close friend.[23]

Similar baseline giving for subjects with *high* and *low* social proximity to the other player ($p = 0.369$) indicates that there are no systematic differences in baseline altruism between the two groups. Differences in giving arise for the extent to which subjects are affected by incentives to the other player. Consistent with our prediction, columns (3) and (4) of Table 4 show that social influence is stronger for subjects with higher social proximity to the other player. The behavior of subjects with low social proximity to the other player follows the standard model of prosocial behavior: these subjects monotonically increase donations with monetary incentives, they expect their social reference to do the same, and their giving behavior is not significantly affected by the incentives provided to the partner. If anything, consistent with concave altruistic utility, monetary incentives to the partner monotonically decrease one's own giving: donations decrease by 0.214 units and 0.251 when the partner gets intermediate and high incentives, respectively, but neither of these point estimates are significantly different from zero. High oneness subjects instead behave consistently with the framework of prosocial giving augmented by social influence: when the other player's incentives increase from none to moderate, subjects expect the other player to increase donations by 2.155 units ($p < 0.001$) and they donate 0.837 units ($p < 0.001$) more themselves.

Also among subjects with higher social proximity to the other player, we find the social influence effect to be non-monotonic in partner's incentives. If the behavior of the other player is influenced by high incentives to generate donations, individual giving is not significantly higher than in the case where the partner has no incentives: donations increase only marginally by 0.170 units ($p = 0.472$). For high oneness subjects,

[23] In our sample, average oneness towards the other for subjects with oneness below (above) median is 1.362 (4.032). Gächter et al. (2015) measure average oneness towards an acquaintance, non-close friend, and close relationship to be 2.5, 4.0, and 5.4, respectively.

Table 4: OLS for the Effect of Partner's Incentives on Donations and Beliefs (Coefficient Estimates and Standard Errors in Parentheses)

(a) Outcome: Donations	Full sample		Split by oneness		p-value
			High	Low	
	(1)	(2)	(3)	(4)	(5)
Provided Lottery	-0.712***	-0.831***	-0.665*	-1.066***	0.464
	(0.262)	(0.283)	(0.389)	(0.403)	
Incentives to self *(baseline: None)*					0.052
Moderate	1.964***	1.970***	1.921***	2.037***	
	(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 other *(baseline: None)*					0.016
Moderate		0.356*	0.837***	-0.214	
		(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)	
P-value					
H0: Incentives to self *Moderate = High*	0.649	0.686	0.395	0.087	
H0: Incentives to other *Moderate = High*		0.046	0.007	0.888	
H0: Incentives to other *None = Moderate = High = 0*		0.080	0.003	0.607	

(b) Outcome: Beliefs	Full sample		Split by oneness		p-value
			High	Low	
	(1)	(2)	(3)	(4)	(5)
Provided Lottery	-1.155***	-1.188***	-1.207***	-1.315***	0.822
	(0.237)	(0.256)	(0.358)	(0.348)	
Incentives to other *(baseline: None)*					0.391
Moderate	1.948***	1.962***	2.155***	1.773***	
	(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: None)*					0.435
Moderate		0.336**	0.420*	0.257	
		(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)	
P-value					
H0: Incentives to other *Moderate = High*	0.085	0.095	0.750	0.065	
H0: Incentives to self *Moderate = High*		0.000	0.000	0.109	
H0: Incentives to self *None = 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 the partner each player faces, 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.

we observe that, as long as the partner gets more than no incentives, individual expectations about other's donations are stable around 5.748 (moderate incentives) and 5.820 (high incentives). This is good indication that the estimated 0.667 ($p = 0.007$) difference in donation levels between treatments where the other player get moderate and high incentives cannot be explained by the substitution embedded in altruism, and leads us to conclude in favor of the following result.

Result 2b (Economic Environment and Norm Adherence): *Larger (smaller) incentives to act prosocially faced by the social reference produce weaker (stronger) social influence.*

Subjects with high social proximity to their partners predict their partner to be sensitive to the availability, but not to the stakes, of incentives to generate donations presented in this experiment. At the same time, the stakes of incentives to the other player have different social influence effects on individual donations: subjects increase donations by 0.837 ($p < 0.001$) units when the other player gets moderate incentives and increase donations by 0.170 ($p = 0.472$) units when the other player gets high incentives. Our interpretation of this evidence is that the incentives faced by a social reference importantly affect the extent to which the behavior of a social reference can represent a norm to which one may want to conform. This idea is reminiscent of the influential paper from Gneezy and Rustichini (2000a,b) and the more recent paper from Fuster and Meier (2009). In their experiments, like in ours, incentives weaken adherence to the norms of behavior dictated by the actions of a social reference – let this be a small group or society at large.

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 size of incentives matters. Relatively small incentives to act prosocially can preserve a certain level of norm adherence and produce social influence.[24] When this is the case, our evidence suggests that the power of small incentives could be leveraged to crowd-in donations by naturally inducing *epidemics* of prosocial behavior.

This result is also important for methodological reasons. Increasingly, social scientists are becoming interested in studying the relationship between the beliefs about

[24] Although the absence of social interactions after the donation does not allow us to explain our findings with the same behavioral channel, the theory of Dutta et al. (2018) of ostracism and endogenous social norms demonstrates that sufficiently large incentives are needed for the collapse of norm following.

others' behavior and individual behavior. Such empirical efforts often have to overcome several challenges, which include the notorious reverse causality issue entrenched to the *false consensus* in belief formation.[25] An approach that is growingly being used in the experimental literature, to overcome similar challenges and study the effect of beliefs about others' on individual behavior, is to introduce sources of belief variation that serve as instruments for elicited beliefs (see e.g. Smith (2013); Costa-Gomes et al. (2014)). Result 2b is key for illustrating a violation of the exclusion restriction that would be necessary for using incentives to the social reference as an instrument for beliefs.[26]

3.4. Incentive Inequality and Donor's Morale

The presentation of the results so far has been ignoring the possibility that incentive inequality in itself can affect the morale of an agent to work on a task to generate donations for a charity. Although we think that making very clear to subjects that incentives are allocated randomly and independently from their partner greatly reduces the scope for incentive inequality to be perceived as unfair, we deem appropriate to consider in this section (i) what would be the morale effects of incentives to the other player on on one's donations, (ii) how they confound conformity, and (iii) whether incentive inequality effect alone can explain the observed strategic complementarities in donations.

Section 2.2.2 illustrated the potential morale effects of incentive inequality, as proposed by Breza et al. (2017), highlighting the predicted *main diagonal condition* of this theoretical framework: conditional on one's own incentives, donation levels should be highest when incentives for the other player equal one's own incentives, and monotonically decrease in the gap between one's own and other's incentives.

In this section, we present a test of the joint hypothesis of the *main diagonal condition* to understand whether the morale effects of incentive inequality alone can explain the variation in donation behavior, or the framework of social influence is necessary to reconcile the findings. Similar to Burks et al. (2009), we devise a likelihood ratio test

[25] The concern that beliefs on others behavior more closely reflect the response function of the *observer* rather than the *observed*.

[26] Had we run a partition of our experimental design, with a binary manipulation of incentives, we could be presenting very different (spurious) effects of beliefs on donations, depending on the size of incentives, without being able to test the validity of the exclusion restriction.

of the joint null hypothesis that the *main diagonal condition* is a plausible restriction for globally explaining the experimental data.

The restrictions on average donation levels posed by the *main diagonal condition* are summarized in Table 5. Average donations in the nine incentivized treatments of our experiment can be treated as a nine-dimensional normal distribution with means $\boldsymbol{\mu}_{p_i,p_j}$ (which we treat as unknown) and diagonal covariance matrix $\boldsymbol{\Sigma} = \boldsymbol{\sigma^2}_{p_i,p_j}\mathbb{I}$ (which we treat as known). For the joint test, we use maximum likelihood to determine the vector $\hat{\boldsymbol{\mu}}_{p_i,p_j}$ that best fits the nine dimensional vector of sample means $\overline{\boldsymbol{Donation}}_{p_i,p_j}$ - with and without the inequality constraints imposed by the *main diagonal condition* that we summarize in Table 5. A Likelihood Ratio test from the constrained and unconstrained likelihood functions is used to assess the joint validity of these constraints. The test statistic is $\chi^2_{(d)}$ distributed with degrees of freedom d equal to the number of binding inequality constraints.

Table 5: Inequalities in Average Donations between Incentivized Treatments Predicted by the Main Diagonal Condition

		Incentives to other				
		None		Moderate		High
Incentives to self	None	$\mu_{n,n}$	$>$	$\mu_{n,m}$	$>$	$\mu_{n,h}$
	Moderate	$\mu_{m,n}$	$<$	$\mu_{m,m}$	$>$	$\mu_{m,h}$
	High	$\mu_{h,n}$	$<$	$\mu_{h,m}$	$<$	$\mu_{h,h}$

Table 6 reports the results of Maximum Likelihood estimation and Likelihood Ratio tests. In spite of some local violations of the *main diagonal condition*, which cause some of the inequality constraints to be binding, the test does not reject the constrained model in favor of the unconstrained model. This indicates that donation behavior in the sample could be explained by the social influence effect of incentive inequality on morale.

As previously noted though, response to conformity in our sample is highly heterogeneous among subject who exhibit a close (oneness above median) connection to the social reference and those who do not. We have highlighted that subjects with below median oneness to the social reference display virtually no connection to the other player, and social influence plays seemingly no role at all for these subjects. Testing the *main diagonal condition* separately for low oneness and high oneness subjects helps

shed light on the need for conformity to explain the strategic complementarities in donations exhibited by individuals with high oneness towards their partner.

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

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$			Likelihood Ratio	
		Partner got			Incentives to other				
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to self	None	3.233	3.417	3.190	3.320	3.320	3.190	$\chi^2_{(2)} = 1.877$	0.391
		(0.217)	(0.230)	(0.209)	(0.217)	(0.230)	(0.209)		
	Intermediate	5.042	5.546	5.155	5.042	5.546	5.155		
		(0.233)	(0.235)	(0.224)	(0.233)	(0.235)	(0.224)		
	High	5.299	5.575	5.187	5.299	5.366	5.366		
		(0.233)	(0.229)	(0.212)	(0.233)	(0.229)	(0.212)		

(b) Low oneness		Data			$\hat{\theta}^{ML}_{constrained}$			Likelihood Ratio	
		Incentives to other			Incentives to other				
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to self	None	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)		
	Intermediate	4.778	5.105	4.622	4.778	5.105	4.622		
		(0.337)	(0.339)	(0.332)	(0.337)	(0.339)	(0.332)		
	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			$\hat{\theta}^{ML}_{constrained}$			Likelihood Ratio	
		Incentives to other			Incentives to other				
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to self	None	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)		
	Intermediate	5.263	5.913	5.627	5.263	5.913	5.627		
		(0.322)	(0.323)	(0.298)	(0.322)	(0.323)	(0.298)		
	High	5.103	6.293	5.034	5.103	5.581	5.581		
		(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.

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), instead, 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 partner's incentives

– that *ceteris paribus* increase incentive inequality – increase their own donations. This result is clearly inconsistent with inequity aversion, and is also inconsistent with a concave (altruistic) utility of giving. 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 partners' incentives – that *ceteris paribus* increase incentive inequality – increase their own donations. This result is significant for the decrease in partners' incentives from high to moderate, and may be explained by substitution due to concave (altruistic) utility of giving. However, the fact that expectations about partners' levels of giving are virtually identical between these two groups makes this explanation unlikely.

In the appendix, Table A.7 also illustrates consistent patterns of rejection of the joint test when we impose the *main diagonal condition* on expectations that people form about the behavior of the social reference. This indicates that the social influence effect that people anticipate when forming beliefs about the behavior of a close social reference is unlikely to be mainly driven by inequity effects.

Taken together, the results of the analyses in this sections, indicate the potential for the morale effects of inequity to explain some of the strategic complementarities in donation behavior, but also give a clear indication that the morale effects of inequity alone cannot account for key patterns that are consistent with our conformity framework of social influence.

3.5. Other Mechanisms of Social Influence

Mechanisms such as social learning, social consumption, reciprocity, and conformism have been proposed to explain the large evidence in support of the hypothesis that most individuals are conditional co-operators (Frey and Meier, 2004). Signaling motives to engage in charitable behavior can endogenously determine strategic complementarities to arise. Having provided consistent evidence of the potential for and determinants of strategic complementarities in the decision to behave pro-socially in the previous sections, we conclude with a brief discussion of some of the several mechanisms that, in addition to conformity, are often at the root of strategic complementarities in prosocial behavior. This discussion also helps rack the arguments that curb the potential for other channels of social influence to explain our findings.

Social learning. When people are asymmetrically informed about the state of the world, observing others' behavior can facilitate information aggregation. In any charitable giving context, the social value of a prosocial activity is definitely 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 Doctors without Borders in generating social value may be uncertain and some subjects may know the charity better than others. Our experiment rules out this channel by making others' donations not observables.

Joint consumption. Especially when it comes to volunteer work, this mechanism plays a major role in producing social influence. A multitude of prosocial actions may involve some sort of social gathering. As a result, individuals jointly engage in the prosocial activity enjoying consumption utility that is determined by common experiences and interactions during the activity. The lack of social interactions among participants during the donation makes it easy to rule out this mechanism as potential concurrent for explaining our findings.

Reciprocity. This mechanism of social influence is often appealed in the context of *local* social dilemmas - where agents directly benefit from the prosocial behavior of others. In most cases, charitable giving can be instead regarded as a *global* social dilemma - in the sense that agents benefit from the prosocial behavior of others only to a very marginal extent. We cannot rule out that reciprocity plays some role for highly altruistic types who enjoy private utility from anybody's contributions to the charity, but we deem the relevance of this mechanism as absolutely minor in our charitable giving setup (and in global social dilemmas more generally).

Signaling motives. The unifying theory of Benabou and Tirole (2006) proposes the signaling of altruism and greed as channels that can endogenuously lead to strategic complementarity or substitutability of donations. For the case of a binary donation decision 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. While our context is highly anonymous, and our results are unlikely to be driven by *social* signaling, we recognize that the Benabou and Tirole (2006) model admits a self-image interpretation that abilitates the theory of signaling as potential channel underlying the complementarities in giving that we present. The self-signaling theory, on

the other hand, is silent on whether the behavior of more or less socially close individuals should induce the complementarities of behavior. For this reason, we think that conformity is the most reasonable channel of social influence that operates in our data, but we cannot entirely rule out self-signaling motives as a competing and possibly inter-related explanation.[27]

4. Conclusion

Our evidence speaks more directly, but not exclusively, to the broad set of applications including private contributions to public goods, charitable giving, and volunteer work. We provide evidence that social influence produces strategic complementarities in prosocial behavior that can arise even when others' actions are not observable. Such complementarities in our setup result from the individual desire to conform to the expected behavior of a social reference.

We provide evidence of social influence by not only measuring how donations are affected by changes in the economics environment of others, but also by measuring how agents believe others will react to changes in their economic environment. This approach cleanly identifies strategic complementarities and helps uncover that both social proximity to the social reference and the economic environment are key modulating factors of social influence. In fact, as predicted in our conceptual framework, we find social influence to operate almost exclusively among closer ties. Our data reveals that social influence is weaker when actions of the social reference are driven by stronger incentives that do not affect donations of the social reference. We rule out that the significant strategic complementarities observed for closer ties can be explained by incentive inequality effects and conclude that the economic environment that the social reference faces for acting prosocial determines the strength of norm adherence. Our temptation is to speculate that conformity is shaped by the likely intentions of the social reference: actions that are most likely driven by prosocial motives may be more likely to produce conformity than similar actions that are predominantly driven by self-centered motives.

[27] Jones and Linardi (2014) find that making signaling motives more salient increases conformism - calling this "wallflower" behavior and reinforcing our perception that signaling motives and conformity may have related behavioral roots.

We believe the evidence presented in this paper to be rather important not just for academics, but also for policy design. We show that social influence in prosocial behavior spreads even when others' actions are not observable. We revisit the role of the much feared *small* incentives to act prosocially (Gneezy and Rustichini, 2000b), and find that, through social influence, these can crowd-in prosocial behavior. Our lesson is that incentives can be calibrated to be large enough to shift expectations about the behavior of others, small enough to not spoil the desire to conform to others' prosocial actions, and just enough to produce *epidemics* of prosocial behavior. We do not expect this calibration exercise to be always easy to carry out. Meyer and Tripodi (2017) illustrate what is possibly a successful execution of such calibration exercise: the German blood collection system. An institutional landscape where some of the main blood collection agencies provide small monetary incentives to donate, producing the *largest* per capita supply of blood donations world-wide.

Future research should extend our work in at least two directions. First, relatively little research has been studying the factors that modulate social influence. In line with some of the evidence in Fuster and Meier (2009), Bond et al. (2012) and Kessler (2017), our findings highlight that both the economic environment and social proximity to the social reference modulate the strategic complementarities of social influence, but more research will be needed to establish the robustness of this evidence and better understand the sources of heterogeneous social influence effects. Second, field evidence from less controlled settings will be of paramount importance to determine if, and under what conditions, the strategic complementarities of social influence can dominate the well documented substitutability embedded in the free-riding problem that afflicts the provision of private contributions towards to common pools of resources. We learn from this paper that the intrinsic desire to mimic the behavior of a close social reference can have a significant empirical relevance, but more studies should be conducted to quantify the economic significance of conformity to better appreciate its relevance for the design of markets and institutions.

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A. Additional Tables

Table A.7: Average Beliefs in Lottery Treatments, Maximum Likelihood Estimates (Coefficient Estimates and Standard Errors in Parentheses)

(a) Full sample		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to self			Incentives to self			Likelihood Ratio	
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to other	None	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)		

(b) Low oneness		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to self			Incentives to self			Likelihood Ratio	
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to other	None	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)		

(c) High oneness		Data			$\hat{\theta}^{ML}_{constrained}$				
		Incentives to self			Incentives to self			Likelihood Ratio	
		None	Intermediate	High	None	Intermediate	High	Test statistic	p-value
Incentives to other	None	2.890	3.032	2.859	2.959	2.959	2.859		
		(0.265)	(0.272)	(0.249)	(0.265)	(0.272)	(0.249)		
	Intermediate	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.

B. Additional Figures

Figure B.3: Joint Problem Solving Task Software Interface

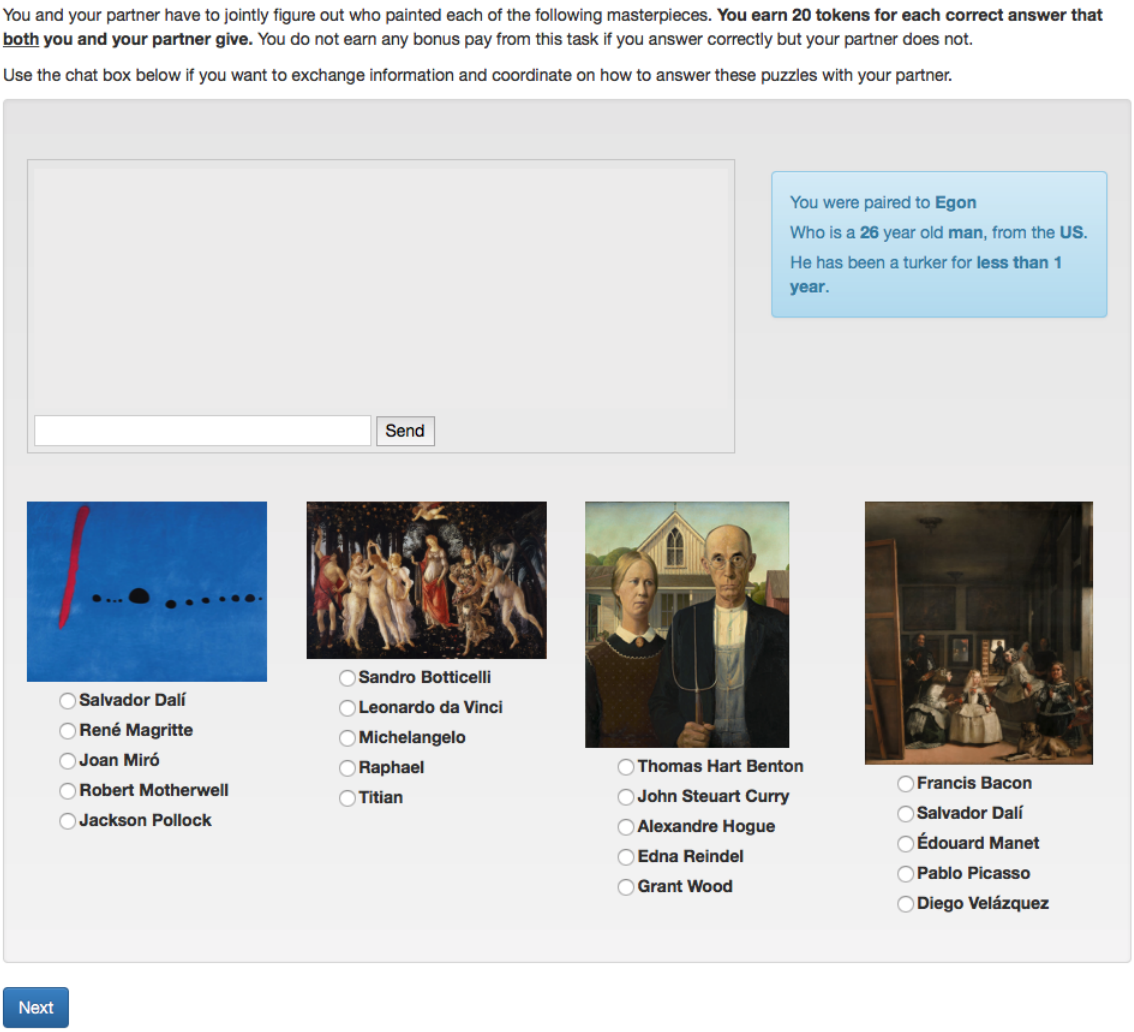

Figure B.4: Elicitation of the IOS (top) and WE (bottom) Scales

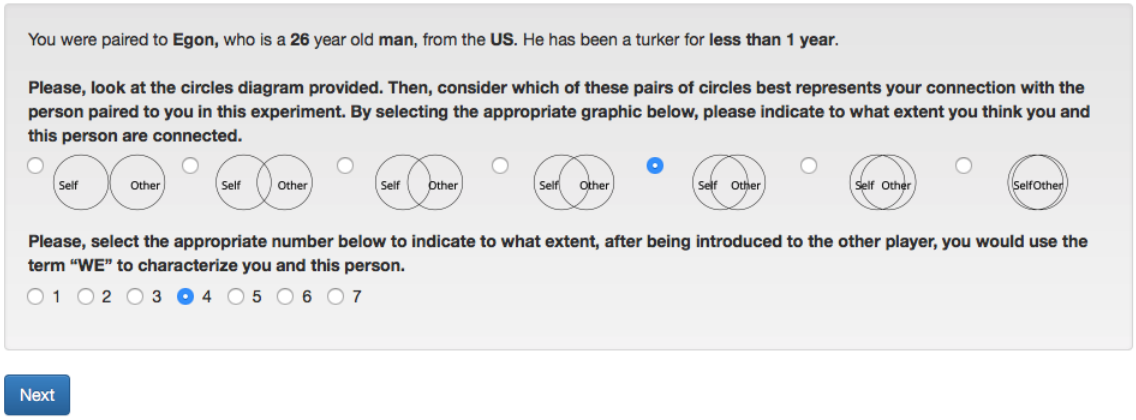

Figure B.5: Elicitation of Beliefs and Donations, and Treatment Assignment