Games and Economic Behavior 132 (2022) 59–72

Contents lists available at ScienceDirect

Games and Economic Behavior

www.elsevier.com/locate/geb

Social proximity and the erosion of norm compliance ☆

Cristina Bicchieri [a], Eugen Dimant [a,d,*], Simon Gächter [b,d,e,*], Daniele Nosenzo [c]

[a] University of Pennsylvania, Center for Social Norms and Behavioral Dynamics, United States of America
[b] University of Nottingham, United Kingdom of Great Britain and Northern Ireland
[c] Aarhus University, Denmark
[d] CESifo, Munich, Germany
[e] IZA, Bonn, Germany

ARTICLE INFO

Article history:
Received 11 April 2021
Available online xxxx

Dataset link: https://osf.io/e5xpm/

JEL classification:
C92
D64
D9

Keywords:
Norm compliance
Social norms
Social proximity

ABSTRACT

We study how compliance with norms of pro-social behavior is influenced by peers' compliance in a dynamic and non-strategic experimental setting. We show that social proximity among peers is a crucial determinant of the effect. Without social proximity, norm compliance erodes swiftly because participants only conform to observed norm violations while ignoring norm compliance. With social proximity, participants conform to both types of observed behaviors, thus halting the erosion of compliance. Our findings stress the importance of the broader social context for norm compliance and show that, even in the absence of social sanctions, norm compliance can be sustained in repeated interactions, provided there is group identification, as is the case in many natural and online environments.

1. Introduction

Decades of experimental research have challenged the notion that individuals are exclusively motivated by material self-interest, showing instead that in many situations they are prepared to sacrifice their own material well-being to increase that of others. One important reason why individuals behave pro-socially is that, in most societies, there are *social norms* that constrain our self-interest and promote other-regarding behavior.[1]

☆ We would like to thank Johannes Abeler, Damon Centola, Gary Charness, Ernst Fehr, Stefano Fiorin, Bernd Irlenbusch, Erin Krupka, George Loewenstein, Michel Maréchal, Nikos Nikiforakis, Dirk Sliwka, Bertil Tungodden, Elke Weber, and two anonymous referees for helpful comments and suggestions. We also appreciate discussions with attendees at the 2018 Cultural Evolution Society Conference, 2019 CESifo Area Conference on Behavioral Economics, 2019 Science of Philanthropy Initiative (SPI) Conference at the University of Chicago, the 3rd Cultural Transmission and Social Norms Workshop at MIT, the 2021 ASSA Annual Meeting, as well as seminar participants at the Behavioral Insights Team UK, the Choice Lab at Bergen, London School of Economics, NYU Abu Dhabi, Stockholm School of Economics, Wharton Business School, and the Universities of Amsterdam, Cologne, Lyon, Oxford, Princeton, and Yale. This work was supported by the European Research Council [grant number ERC-AdG 295707 COOPERATION], the Economic and Social Research Council [grant number ES/K002201/1], and the German Research Foundation (DFG) under Germany's Excellence Strategy — EXC 2126/1–390838866.

* Corresponding authors.
E-mail addresses: cb36@sas.upenn.edu (C. Bicchieri), edimant@sas.upenn.edu (E. Dimant), Simon.Gaechter@nottingham.ac.uk (S. Gächter), daniele.nosenzo@econ.au.dk (D. Nosenzo).

[1] See, e.g., Fisman and Miguel (2007); Fortin et al. (2007); Andreoni and Bernheim (2009); Krupka and Weber (2013); Reuben and Riedl (2013); Rustichini and Villeval (2014); Schram and Charness (2015); Gangadharan et al. (2016); Kimbrough and Vostroknutov (2016); Krupka and Croson (2016); Rosaz et

https://doi.org/10.1016/j.geb.2021.11.012

However, recent experimental evidence suggests that pro-social behavior, and the norms that support it, are fragile. In settings where individuals can observe the behavior of their peers, seeing examples of norm violations has large negative effects on the individual's willingness to comply with the norm (Keizer et al., 2008; Gino et al., 2009a). In contrast, seeing examples of norm compliance has weaker positive effects on compliance, particularly when norm-following runs counter to material self-interest (Thöni and Gächter, 2015; Charness et al., 2019). This asymmetric response to information about others' behavior implies that norm compliance and pro-social behavior tend to wane in settings where individuals can observe each other's behavior (Dimant, 2019; Gächter et al., 2021).[2]

These results paint a bleak picture for societal outcomes that depend on norms of pro-sociality. The picture, however, is incomplete. Most of the evidence on the asymmetric effects of bad and good examples comes from settings where individuals interact anonymously and in groups of strangers, without receiving any signal of the *social (dis)similarity* of the individual to the peers one interacts with. In natural environments, however, people can typically assess the extent to which they share common traits, characteristics, and identities with those they observe (subsequently referred to as "social proximity"). In this paper, we explore whether signals of social proximity can counteract the disproportionate influence of bad examples on norm-following and thus reduce the erosion of compliance.

One reason why social proximity may play a role in moderating the influence of bad examples is that the recognition of similarities between self and others may bring forth a process of group identification, whereby the individual feels that she and her peers belong to the same social group and attaches emotional significance to group membership (Tajfel, 1982). When an individual identifies with the group, she will seek to undertake behaviors that are in line with the expectations of what constitutes "acceptable group behavior" and avoid actions that signal disloyalty or disagreement with the group (Hogg, 1992; Akerlof and Kranton, 2000; Chen and Li, 2009; Tsutsui and Zizzo, 2014; Charness and Holder, 2019). To the extent that the perception of what constitutes acceptable behavior in a given situation is informed by how other group members behave in that situation, this process of group identification will promote *behavioral conformity* within the group.

This implies that in situations that involve compliance with prosocial norms, individuals who identify with the group may pay attention to *both* examples of compliance and deviance. Even when following these examples is materially costly, individuals fear any deviation from the group standard will show a lack of commitment and exclusion from the group. Without group identification instead, examples of deviance may dominate examples of compliance, as not following pro-social norms is typically materially advantageous for the individual (Charness et al., 2019). Thus, through group identification, social proximity may offset the tendency for bad examples to outweigh the influence of good ones.

To test this conjecture under controlled conditions, we designed a laboratory experiment using a Take-or-Give (ToG) donation game (which is inspired by List, 2007, Bardsley, 2008 and Dimant, 2019). Decision-makers can either give money to a charity, take money from it, or retain the initial equal allocation of monetary endowments between oneself and charity. This game allows us to study, within a unified, non-strategic setting, both pro-social behavior (giving to the charity) and anti-social behavior (taking money from the charity). Moreover, using a norm-elicitation experiment, we can show that our experimental participants recognize a social norm in the ToG game that prohibits taking money from charity. Thus, the ToG game represents a perfect paradigm to investigate the fragility of norm-sustained pro-social behavior.

We ran a repeated version of the ToG game where subjects make independent donation decisions across 20 periods. Between treatments, we systematically varied whether subjects can or cannot observe whether other decision-makers have given or taken money from a charity in previous periods. In the baseline condition, "NoObservation", players received no feedback about others' ToG decisions in any of the 20 periods of the game. Thus, in this treatment, it is likely that subjects' decisions are purely based on the initial social norm that prevails in the ToG game.

In our first treatment, "Observation", we informed subjects after each period about the ToG decisions of other subjects in the same session. By observing these decisions, subjects can compare whether others comply with the ToG norm and the extent of any violations. This may lead subjects to revise their initial donation behavior. Based on the evidence discussed earlier, we expect these comparisons to be disproportionately affected by examples of norm violations (which in the ToG game are materially advantageous since taking from the charity increases a subject's payoff), so that the initial level of pro-social behavior in the ToG game may inexorably decline across the 20 periods of the game.

In our final and most novel treatment, "ObservationSP", we combined visibility of peers' decisions with social proximity. In this treatment, subjects can not only repeatedly observe the ToG decisions of their peers, but also learn the extent to which they are similar to these peers in an irrelevant (to the context) and minimal characteristic (knowledge of a sport team victory). As discussed above, sharing a common characteristic, even if it is minimal, may trigger a process of group identification so that subjects may conform to peers' behavior even when doing so is materially costly. Thus, we conjecture that the process of erosion of norm compliance that we expect to observe in the Observation treatment may be substantially reduced in ObservationSP.[3]

al. (2016); Gächter et al. (2017); Kocher et al. (2017); Krupka et al. (2017); Barr et al. (2018); Kimbrough and Vostroknutov (2018); Bicchieri and Dimant (2019); Chang et al. (2019); d'Adda et al. (2020); Bicchieri et al. (2020); Bursztyn et al. (2020); Bašić and Verrina (2020); Dimant (2021).

[2] This asymmetry between the effects of norm-compliant and norm-deviant behavior is reminiscent of the asymmetry between positive and negative reciprocity that has been frequently observed in the literature (for seminal work documenting this see, e.g., Charness and Rabin, 2002; Offerman, 2002).

[3] We extend Dimant (2019) who studies a *one-shot* static version of the ToG game in an *individual decision context*. In our paper we instead study a *multi-period* version of the game that evaluates *individual decisions in a group context*. The crucial advantage of our approach is that we can observe how norm compliance adapts dynamically to information about peer behavior. This allows us to study the factors that may lead to the erosion of norm compliance

Our results confirm our conjectures. We find that in the NoObservation treatment, donation rates to the charity are stable over time. By contrast, donations in Observation decline by about 20% compared to NoObservation. In line with previous findings in the literature, this decline is due to the asymmetric impact of observing compliance with and violation of the ToG norm. Individuals strongly reduce the amount they donate when they observe that others take money from the charity. However, they do not increase the donated amount when they observe that others give to charity. Most importantly, knowledge of social proximity strongly moderates ToG donation behavior. In ObservationSP, the average donation is not significantly different from our NoObservation baseline. The reason is that, in the presence of a signal of social proximity, individuals respond to *both* observed norm compliance and deviance, thus stabilizing donations roughly at their initial level.

These results indicate that observation of peer behavior, and the social proximity to those peers, have both a strong influence on norm compliance. This highlights the importance of the broader social context in driving norm compliance. Further, this shows that studying norm compliance (and pro-social behavior, more generally) in socially sterile settings overestimates the erosion of compliance. It suggests that punishment of norm violations, frequently seen as crucial to sustain norms (e.g., Coleman, 1994; Sutter et al., 2010; Fehr and Schurtenberger, 2018; Dimant and Gesche, 2020; Buckenmaier et al., 2021), is not always needed: even minimal social proximity can prevent the erosion of norm compliance.

2. Experiments

We present data from a total of $N = 1,699$ participants across four distinct experiments (no subject participated in more than one experiment):

1. *Behavioral experiment* utilizing the ToG donation game: $n = 842$ collected in the laboratory from students at the University of Pennsylvania.
2. *Norm elicitation experiments* (following the procedures by Bicchieri and Chavez, 2010 and Krupka and Weber, 2013) to establish which norm exists in the ToG game: $n = 219$ collected from students recruited at the University of Pennsylvania.[4]
3. *Two additional norm elicitation experiments* establishing the robustness of the norm identified in Experiment 2: $n = 464$ collected from a sample of the general population recruited online.
4. *Normative expectations change experiment* to better understand the mechanisms of changes in norm compliance (following the procedure by Bicchieri and Chavez, 2010): $n = 174$ collected in the laboratory from students at the University of Pennsylvania.

For ease of exposition, we only discuss the results of Experiment 1 in detail in the main text. In addition, only key insights from Experiments 2, 3, and 4 are alluded to in the main text, whereas the details of those experiments are provided in the Online Appendix.

2.1. Behavioral experiment: design and data collection

In the ToG game, each subject chooses a charity to be paired with (either Doctors Without Borders, World Wildlife Fund, or UNICEF) and then makes an allocation decision towards this charity. At the start of the game, the subject and the charity are both endowed with 100 ECUs (with 10 ECUs = $ 1). The subject can leave these endowments unchanged ("abstain", in what follows), "give" between 1 and 100 ECUs from his/her endowment to the charity, or "take" between 1 and 100 ECUs from the charity's endowment. Thus, our game is a variant of the dictator game with take options (e.g., List, 2007; Bardsley, 2008; Korenok et al., 2014), but where the recipient is a charity (as in Eckel and Grossman, 1996; Exley, 2015; Grossman and Eckel, 2015; Dimant, 2019; Bolton et al., 2021).

We recruited $N = 842$ participants (University of Pennsylvania students) to make choices in the ToG game. Participants were on average 22 years old and 70 percent were female. The experiment was conducted in zTree (Fischbacher, 2007) and ran in 52 sessions (with between 12 and 18 subjects per session) in the Behavioral Ethics Lab at the University of Pennsylvania across three between-subject treatments, described in more detail below.[5] In each treatment, every participant went sequentially through the same four parts of the experiment (plus a payment phase) that are discussed in detail below. Participants were aware that the experiment consisted of multiple parts, but the details of each new part were only revealed upon completion of the previous parts. All decisions in the experiment were collected anonymously (see Online Appendix for instructions).

in settings where people can interact repeatedly and observe each other's behavior, as they do in most natural and online environments (Buckenmaier and Dimant, 2021).

[4] The elicitation of norms and behavior is performed between subjects (see d'Adda et al., 2016, for a discussion of pros and cons of between- vs within-subject elicitation of norms and behavior). We performed balancing tests between subjects who took part in the norm and behavioral experiment and found only small differences (see Table A.1 in the Online Appendix; subjects in the norm elicitation experiment are on average 1 year older, 8% less likely to be male and their attitude towards charitable giving is one-quarter of a standard deviation higher than subjects in the behavioral experiment).

[5] Table A.1 in the Online Appendix reports balancing tests showing successful treatment randomization.

Part I – Social Proximity Question

At the beginning of the experiment, all participants were asked one incentivized knowledge question (the year in which Philadelphia's baseball team won its last World Series). Participants were paid $1 at the end of the experiment if they provided the correct answer ("2008") and $0 otherwise. Overall, about 27% of our participants knew the correct answer (of those 40% were male and 60% were female). As detailed below, in one of our treatments (ObservationSP) the answers to the knowledge question were used to trigger group identification in Part III of the experiment, by forming groups and informing participants whether other group members had provided a correct or incorrect answer. In the other treatments, participants were also asked the knowledge question, but were not informed about other group members' answers until the end of the experiment.

Part II - Individual Decision (one period)

Next, all participants made one ToG decision. Participants first selected their preferred charity and then chose how much to allocate to it. It was public knowledge that each individual's decision did not affect other participants' payoffs or the payoff of a charity besides their own. This design choice removes any payoff interdependency between participants.

Part III - Individual Decisions (nineteen periods)

Following the individual decision in Part II, participants were assigned to a group of either two ($N = 354$) or four ($N = 488$) participants. The group allocation was determined at random and remained constant throughout Part III. We varied group size because existing evidence suggests that group size affects the pressure to conform with group norms (Bond, 2005). Each participant continued to make the same type of individual decision as in Part II towards his or her charity for another 19 periods. In each period, both the subject and the charity were endowed with 100 ECUs each to avoid any path-dependent carryover effects from previous periods. Part III varies across three treatments, as follows.

In our baseline condition (NoObservation), participants were placed in groups, but no behavior was observable by or revealed to any other participant. We study the natural evolution of ToG decisions across the 20 periods of Part II and III of the experiment.

Our first treatment (Observation) is identical to baseline except that, in each period of Part III, participants received information about the ToG decisions of the other group members in the previous periods of the experiment, including in Part II of the experiment. This feedback information was presented in the form of a history table where subjects could track the ToG decisions of each group member over the whole duration of the experiment. The comparison between Observation and NoObservation measures the effect that repeated exposure to information about others' behavior has on ToG behavior.

Our second treatment (ObservationSP) is identical to Observation, except that in addition to observing group members' ToG decisions, participants also received information about their answer to the knowledge question of Part I. This information was displayed right below the history table in the form of an "answered the question correctly/incorrectly" indicator for each group member. We use this information to manipulate subjects' *social proximity* with other group members.[6] Our manipulation may have triggered social proximity in different ways. The most natural interpretation of the manipulation is that, by revealing whether others are knowledgeable about the achievements of Philadelphia's baseball team, it signals whether or not others are supporters of the team and thus promote group identification along the sport fandom dimension. However, social proximity may have also been triggered by other forms of group identification, including the simple fact of sharing the same answer with others, or gender stereotypes (the belief that only men answered the question correctly and hence group identification along the gender dimension).[7] Regardless of the form of group identification promoted by the manipulation, we use the ObservationSP treatment to assess how the ToG donation decisions are affected by the social proximity of other group members.[8]

[6] Since our subjects are all University of Pennsylvania students, it is possible that a degree of social proximity is present in our experiment, regardless of treatment. Our manipulation aims at increasing the social proximity between subgroups of our subjects beyond this baseline level.

[7] This gender stereotype is actually misguided, as we saw earlier: if anything, female subjects are slightly more likely to answer correctly than male subjects. We decided to use a knowledge question about sports, rather than the minimal-group paradigm (Tajfel and Turner, 1979) or manipulations based on partial decrease of anonymity (e.g., Charness and Gneezy, 2008), to increase the strength of the manipulation. We provide suggestive evidence that this was indeed the case by running an additional experiment (n=300 Amazon Mechanical Turk participants) where we used the (unincentivized) 'Inclusion of the Other in the Self (IOS)' scale (Aron et al., 1992; Gächter et al., 2015). After expressing their preference for sports or paintings, participants were asked to imagine being matched with someone who had the same sports/paintings preference as them and rate their closeness on the IOS scale from 1-7. Result: the sports identity achieved a significantly higher group identification than the minimal group choice ($p<0.01$, see Fig. A.1 in the Online Appendix for details).

[8] Strictly speaking, we would need an additional control treatment to attribute the effect of ObservationSP exclusively to social proximity, namely a treatment where we include in the history table a row with the"answered the question correctly/incorrectly" indicator, but where the values of the indicator are actually determined at random. This would allow to rule out that the effect of ObservationSP is merely a reflection of being shown the additional information.

Part IV - Post-Experimental Questionnaire

We elicited general participant information, such as age and gender, alongside individual behavioral attributes, such as risk attitudes and appreciation for the work of the chosen charitable organization, all of which we control for in our regression analyses.

Payment

All participants were paid a flat show-up fee of $10 in addition to their earnings from their choices. At the end of the experiment, we randomly selected one subject in each session and paid him/her and his/her chosen charity according to the choice the subject had made in one randomly-chosen period. All other subjects in the session received $ 10 (their show-up fee) irrespective of their choices. Participants were informed at the beginning of the experiment of this ''pay one" payoff structure, which we chose to further eliminate any type of interdependencies between subjects while retaining incentive compatibility (for discussions, see, e.g., Charness et al., 2016; Azrieli et al., 2018). The experiment lasted about 45 minutes, yielding an average hourly pay of $18.

2.2. Behavioral experiment: hypotheses

We conjectured that behavior in the ToG game is likely to respond to a *social norm of charitable giving* that prescribes that donating to charity (or at least leaving initial endowments untouched) is socially appropriate, while taking from charity is inappropriate. To empirically confirm that behavior in the ToG game is norm-driven, we conducted a series of norm-elicitation experiments (Experiments 2 and 3, see Online Appendix) measuring which actions are viewed as appropriate in the ToG game. We find that participants recognize that taking from charity is the least appropriate behavior in the ToG game, while giving is most appropriate. Abstaining from changing initial endowments is also appropriate (see Figs. A.13; A.14; A.15; A.16; A.17 in the Online Appendix). These results hold in all variants of our norm-elicitation experiments (with university students as well as members of the general population recruited online; using the Krupka and Weber, 2013 procedure as well as using the procedure proposed by Bicchieri and Chavez, 2010). Moreover, among university students, there is the additional expectation that the norm will be followed in the behavioral experiment. The results of these norm-elicitation experiments clearly indicate that a norm of not taking from charity exists among university students.

The main aim of our paper is to study how compliance with this ToG norm varies across Part III of the behavioral experiment in our two treatments with observation of peers' behavior. Observing others' behavior can affect norm compliance through two mechanisms (Bicchieri, 2006, 2016). First, since an individual's willingness to comply with norms partly depends on whether others also comply, observation of behavior exhibited by similar peers directly affects the extent to which an individual follows the norm. Second, observing others' behavior may also affect an individual's perception of the norm. That is, although our norm-elicitation experiments reveal that a "naive" subject, who has no experience with the ToG, believes that taking is inappropriate, repeated observations of taking by others may induce the subject to revise his or her normative expectations downwards (i.e., consider taking less inappropriate). Thus, according to this second mechanism, examples of deviance can erode the norm itself, which in turn triggers a behavioral adjustment. In our Experiment 4 reported in Online Appendix 4.1 we explore whether this second, indirect mechanism is also at play in our setting. We find evidence in support of it (see Fig. A.18). Our key questions are:

1. Does information about others' behavior have a symmetric influence on behavioral compliance? That is, do examples of compliance by others affect an individual's compliance to the same extent as examples of deviance?
2. Is this process influenced by social proximity?

Regarding our first question, based on the literature on peer effects in pro-social behavior (e.g, Gino et al., 2009a; Thöni and Gächter, 2015; Charness et al., 2019), we expect an asymmetric effect of peer information on norm compliance when individuals interact anonymously and without signals of social proximity. Thus, our first hypothesis is:

H_1: *In Observation, individuals react more strongly to examples of norm violation (taking) than examples of norm compliance (giving/abstaining). Over time, this leads to a decline of compliance with the ToG norm compared to NoObservation.*

Regarding our second question, we conjecture that this asymmetric effect of peer information is due to a self-serving interpretation of information: individuals may ignore evidence of norm compliance because conforming with it is materially costly. However, we expect social proximity to reduce this self-serving use of information. When individuals identify with the group, they aim to conform to what other group members do, since non-conformity may be negatively judged by the group (Hogg, 1992). Thus, social proximity may promote norm compliance by inducing a *heightened sensitivity to behavioral conformity* with other group members. That is, in proximate groups, individuals will strive to pay attention and conform to *both* examples of violations and examples of compliance. These two effects will on average cancel out, stabilizing norm compliance to its initial levels, especially in the most homogeneously proximate groups, where all members give the same answer to the social proximity question. Our second hypothesis is:

Table 1
ToG decisions in Part II of the experiment. Standard deviations in parentheses.

	Average amount given/taken	Percentage of initial takers	Percentage of initial equal split (abstainers)	Percentage of initial givers
NoObservation	-18.9 (57.1)	45%	36%	19%
Observation	-25.2 (55.9)	44%	40%	16%
ObservationSP	-21.8 (51.1)	42%	41%	19%

H_2: *Social proximity reduces the asymmetric effect of peer information: Subjects in ObservationSP will respond to examples of both norm compliance and violation, thus reducing the erosion of norm compliance. This effect will be driven by subjects in ObservationSP that belong to groups that are most socially proximate.*

3. Results

3.1. Behavior in Part II - initial compliance with the ToG norm

The first period of the ToG game (Part II of the experiment) is the same in all treatments and subjects receive no information about the behavior of others or their social proximity. In the Observation and ObservationSP treatments, participants did not know that their decisions in Part II would be displayed to other group members in Part III, ruling out strategic motives to distort their decision. Thus, we can use these initial donation decisions to establish the extent to which the ToG norm is followed when subjects have to rely on their homegrown expectations about the appropriateness of giving/taking.

Table 1 displays the average ToG donation in Part II across the three treatments. Due to lack of group size differences in our main analysis (Table 2), we pooled the data across group sizes. The table also displays the percentage of participants in each treatment who gave, took, or abstained from changing initial endowments.

In all treatments, most participants either gave to charity or abstained from changing initial endowments. Between 42% and 45% of participants instead violated the ToG norm and took from charity. On average, participants took between 18.9 and 25.2 ECUs from the charity. As expected per proper treatment randomization, we find that behavior in Part II does not differ across treatments (Kruskal-Wallis test, df = 2, $p = 0.703$).

These initial results show that, although our norm-elicitation experiments clearly indicate that a strong norm against taking exists in the ToG game, more than two-fifths of participants in the behavioral experiment actually violated the norm. The remaining three-fifths of subjects engaged instead in behavior that is considered socially appropriate. The question we will examine next is how these initial levels of compliance are affected by observation of peer behavior.[9]

3.2. Behavior in Part III - the dynamics of norm compliance

The left panel of Fig. 1 shows the evolution of average donations over the 19 periods of Part III across our treatments. The right panel shows ToG donations averaged across all 19 periods. In both cases, the figures show average donations normalized relative to behavior in Part II (which is displayed as period 0 in Fig. 1, left panel). A positive value indicates that, relative to Part II, the account balance of the charity has increased, and a negative value that it has decreased.

In NoObservation, donations in Part III are similar to Part II (there is only a small average difference of -4.9 ECUs). Using two-sided bootstrapped paired t-tests with group averages as observations, we cannot reject the null hypothesis of no differences in donations between Part II and III ($p = 0.308$). In the absence of new information about peers' behavior, compliance with the ToG norm does not change.

In contrast, when participants received information about peers' behavior, the average amount donated to the charity dropped relative to Part II. Using two-sided bootstrapped paired t-tests with group averages as observations, we detect significant differences between donation behavior in Parts II and III for both Observation ($p < 0.001$) and ObservationSP ($p = 0.008$). However, the decline in compliance appears to be much stronger in Observation, where participants took on average an additional 21.9 ECUs, compared to ObservationSP, where the balance of the charity was reduced only by an additional 7.5 ECUs. A Kruskal-Wallis test confirms that there are significant differences between the three treatments in Part III of the experiment (df = 2, $p < 0.001$).[10]

[9] A possible reason why we observe a large fraction of takers is that sophisticated subjects may have strategically selected the charity they liked *the least* from the list of possible charities, in order to experience lower disutility from subsequent norm violations. An interesting avenue for further research would be to study the extent of this strategic charity selection and its effect on norm compliance.

[10] Results are robust when we focus on groups where the fraction of takers is initially a minority (Fig. A.2 in the Online Appendix), when we focus only on the first period of Part III, or when we take as dependent variable the share of individuals who take instead of the level of giving (see Figs. A.3 and A.4).

Table 2
Random-effects GLS regressions. The dependent variable is donation of subject i in period t of Part III of the experiment. Controls are: gender, age, whether or not the proximity knowledge question has been answered correctly, a measure of self-control taken from Tangney et al. (2004), a variable measuring the (self-reported) appreciation for the work of the chosen charity, a measure for the ability to anticipate future consequences of current behavior taken from Strathman et al. (1994), and a self-reported measure of risk preferences taken from Dohmen et al. (2011). We always use the same set of controls in all of our regressions. Because our analysis does not focus on the role of these controls and to increase readability, the estimates of the controls are not reported in Table 2, but are available upon request. Robust standard errors reported in parentheses (standard errors clustered at the group level). Stars indicate significant differences at the conventional levels of $^{*}p<0.1$, $^{**}p<0.05$, and $^{***}p<0.01$.

DV: Amount Change in Charity Box	(1)	(2)
Treatment (Base level: NoObservation)		
Observation	−18.02***	−4.26
	(3.51)	(3.95)
ObservationSP	−3.67	0.60
	(3.09)	(3.15)
Groupsize = 4	−2.81	−1.73
	(2.64)	(2.36)
Initial Individual Donation (Part II)	0.67***	0.67***
	(0.02)	(0.02)
Period	−0.35***	−0.25***
	(0.08)	(0.07)
Neg. Avg Don. t-1		0.05
		(0.03)
Neg. Avg Don. t-1* Observation		−0.29***
		(0.05)
Neg. Avg Don. t-1* ObservationSP		−0.17***
		(0.05)
Pos. Avg Don. t-1		−0.01
		(0.04)
Pos. Avg Don. t-1* Observation		−0.02
		(0.10)
Pos. Avg Don. t-1* ObservationSP		0.21***
		(0.07)
Zero Avg Don. t-1		−1.18
		(1.45)
Zero Avg Don. t-1* Observation		0.69
		(3.20)
Zero Avg Don. t-1* ObservationSP		2.60
		(2.18)
Constant	−36.95***	−39.85***
	(7.20)	(7.22)
Controls	Yes	Yes
N.	15998	15998
N. Clusters	299	299
R^2	0.47	0.49

We perform bilateral treatment comparisons using multivariate regression analyses that include controls for observable differences across treatment groups. Table 2, column 1, reports the results of random-effects GLS regressions where the dependent variable is the amount given/taken to/from the charity by subject i in period t of Part III. The independent variables are treatment dummies (Observation and ObservationSP, using NoObservation as base category), a dummy for group size (using group size 2 as base category), a variable measuring the amount contributed by the participant in Part II (to control for a participant's underlying predisposition to give or take from the charity), a period variable, and various other controls collected in the post-experimental questionnaire.

We find statistically significant differences between Observation and both NoObservation ($p < 0.001$) and ObservationSP (test of equality of coefficients, $p < 0.001$). However, we do not observe any significant difference between NoObservation and ObservationSP ($p = 0.848$). We also do not detect any difference in behavior across participants who were in groups of 2 or 4 participants. We do not observe any general effect of group size on taking behavior; a separate regression in which

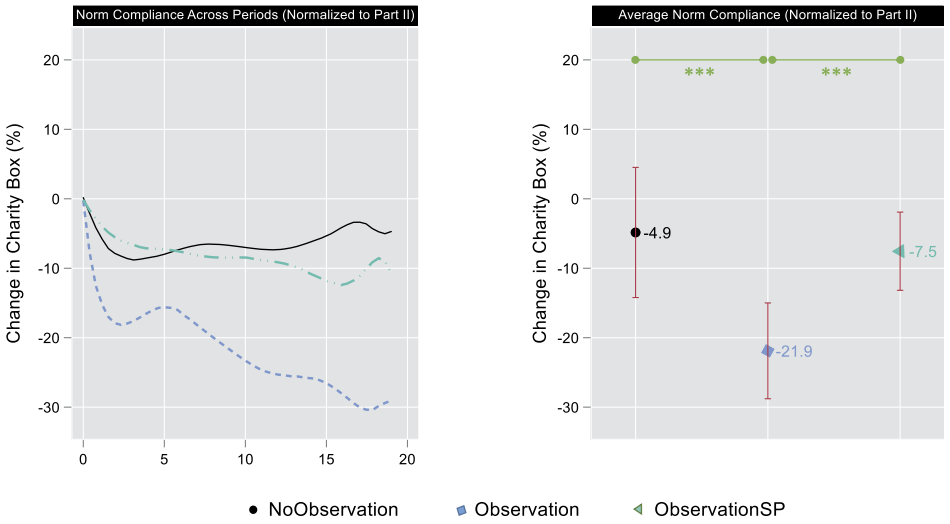

Fig. 1. Left panel: Subject's behavior across treatments and periods in Part III, normalized to the average behavior in Part II (period 0 in the graph). For purposes of readability, lines represent kernel-weighted local polynomial smoothing of degree 5 (for an unsmoothed version of this illustration see Fig. A.9 in the Online Appendix). Right panel: Average subject's donations across treatments, averaged over the 19 periods of Part III. Whiskers indicate 95% confidence intervals. Stars indicate significant differences using random-effects GLS regressions at the conventional levels of $^{*}p<0.1$, $^{**}p<0.05$, and $^{***}p<0.01$.

we interact group size with treatments does not yield any significant differences either. Among the other controls, we find that males, individuals who attach more importance to charitable organizations (self-reported), and participants exhibiting greater ability to anticipate the future consequences of their actions (based on a questionnaire by Strathman et al., 1994) give more. None of the other controls (including answers to the social proximity question) are statistically significant.

Thus, in line with H_1, our results show that when participants only receive feedback about the behavior of other group members, norm violations spread. In contrast, when participants receive both feedback about the behavior and social proximity of other group members, individuals are substantially more likely to comply, which supports H_2.

Why does norm compliance not significantly decline when information about social proximity is available? At the heart of H_1 and H_2 is the idea that, in the absence of signals of social proximity, participants mainly respond to examples of norm violations. Instead, when group members receive signals of others' social proximity, participants also respond to examples of norm compliance, leading to a zero net effect.

To test this conjecture, in column 2 of Table 2 we augment the random-effects GLS regression of column 1 with variables capturing the different types of information that participants were exposed to in Part III, as well as interactions between these variables and the treatment dummies. For the purpose of our analysis, we follow Fischbacher and Gächter (2010) and introduce variables lagged by one period that capture the effect of information diffusion within groups.[11] We distinguish between three types of information. First, we use the variable "*Neg. Avg Don. t-1*" to capture the effect of receiving feedback that other group members on average took money from the charity in the previous period (i.e., participants observed, on average, an example of norm violation). Thus, this variable is equal to the absolute value of the (lagged) average amount transferred to the charity by other group members if this amount is strictly negative, and it is equal to 0 otherwise. Second, we use the variable "*Pos. Avg Don. t-1*" to capture the effect of receiving feedback that the other group members on average gave money to the charity in the previous period. This variable is equal to the (lagged) average amount transferred to the charity by other group members if this amount is strictly positive, and 0 otherwise. Finally, to capture the effect of observing an average zero transfer to the charity in the previous period, we use the dummy variable "*Zero Avg Don. t-1*", which takes value 1 if the amount transferred was zero, and 0 otherwise. We interact each of these variables with our treatment dummies.

The interactions between the lagged donation variables and the Observation dummy capture the differential impact of information about peer behavior in Observation relative to NoObservation.[12] The estimates reveal that observing norm-compliant behavior (i.e., receiving feedback that on average other group members gave to charity or left the charity's endowment untouched) does not statistically significantly affect a participant's decision to give or take money to/from

[11] These results are robust to controlling for higher lags of t_{-2} and t_{-3}. Moreover, these additional regressions (available upon request) show that subjects predominantly respond to the most recent information available: the coefficients of the t_{-2} and t_{-3} lags are almost all insignificant.

[12] Given our interactions, the main coefficients of the lagged donation variables (*Neg. Avg Don. t-1*, *Pos. Avg Don. t-1*, and *Zero Avg Don. t-1*) measure the effects of the average amount transferred by other group members in NoObservation. This is a placebo test for the effects of peer behavior information, as participants in the NoObservation condition did not actually receive feedback regarding amounts transferred by other group members during the experiment. As expected, the estimates show no significant effect of this information on donations in NoObservation.

Table 3
Average donations across the 19 periods of Part III, normalized relative to Part II, in the Observation SP treatment. The statistic is disaggregated based on the degree of within-group social proximity, measured as the share of group members (50%, 75% or 100%) who answered the knowledge question in the same way (correctly or incorrectly). Note that the number of groups with 75% of members who gave the same answer is lower by construction since this only includes groups of size 4 (where the ratio of incorrect/correct can also be 3/1 or 1/3, respectively).

% of Group Members Who Answered the General Knowledge Question in the Same Way (Correctly or Incorrectly)	Number of Groups	Number of Subjects (% Males)	Normalized % Change in Charity Box (mean & SD)
50%	51	130 (35%)	-12.51 (24.65)
75%	19	76 (29%)	-4.87 (21.96)
100%	78	192 (27%)	-5.71 (25.37)

the charity.[13] In contrast, observing norm-violating behavior (taking) has a strong and negative effect on donations. Our estimate suggests that receiving feedback about other group members taking 1 ECU from the charity reduces the amount that is donated on average by -0.29 + 0.05 = -0.24 ECUs (see Table 2, column 2), which is significant at the 1% level.[14] An F-test shows that the effects of examples of violation are significantly larger than the effects of examples of compliance ($p < 0.001$). The interactions between the lagged donation variables and the ObservationSP dummy capture the impact of feedback about others' behavior when participants could also observe a signal of social proximity to the other group members. As in Observation, feedback that on average other group members abstained from changing the charity's endowment does not affect a participant's donation.[15] Also as in Observation, observing norm violations has a negative impact on donations, with an estimated reduction of donations of -0.17 + 0.05 = -0.12 ECUs for each ECU that others took from charity. This effect is significant at the 1% level.[16] However, in contrast to Observation, participants in ObservationSP also responded to examples of giving. The feedback that other group members give on average 1 ECU to charity increases the amount donated by 0.21 - 0.01 = 0.20 ECUs (significant at the 1% level).[17] An F-test does not reject the null that the effects of giving and taking are of similar magnitude ($p = 0.649$).[18]

This analysis reveals that the fundamental difference between the treatments with observation lies in the fact that, without information about social proximity, participants respond significantly only to examples of norm violation. However, information about social proximity induces responses to observations of norm compliance, as well as norm violation. The effects of observing others taking and giving to charity are roughly similar in magnitude. As a consequence, the net effect on donations is small.

A final conjecture we made in H_2 is that the moderating effects of social proximity may be stronger among groups in which all members are similar in the relevant social dimension. We start examining this conjecture by dividing the groups of ObservationSP depending on the percentage of group members who have answered in the same way the knowledge question of Part I. We take this as a measure of social proximity at the group level. The most proximate groups are those where all members have answered the questions correctly, or where they all answered it incorrectly. The least proximate groups are instead those where half of the members answered the questions correctly and half answered it incorrectly. Table 3 shows the average donation in Part III of the experiment disaggregated by level of proximity within a group.

In 53% of groups, all group members gave the same answer to the knowledge question. In 13% of groups, three-quarter of group members gave the same answer, and in 34% of groups only half of the members shared the same answer. The share of male participants is roughly the same across these three types of groups. The last column of the table shows that, relative to Part II, the average amount given to charity in Part III falls by the largest amount among the least proximate groups (-12.51 ECUs). Donations instead fall by approximately -5 ECUs in groups where a majority of subjects (either 75% or 100%) gave the same answer to the knowledge question of Part I. A regression of amount donated on the degree of within-group social proximity (50%, 75% or 100%) reveals a positive and statistically significant relation between social proximity and giving ($p = 0.038$, see Table A.2 for the full regression results).

We further examine the conjecture that social proximity moderates the effect of peer behavior information in the regressions of Table 4. The regressions report the effects of observing peers' behavior (disaggregated in the same way as in Table 2) on donations in ObservationSP. In column 1, we only use observations from groups where all group members gave the same answer (100% correct or incorrect). In column 2, we instead use observations from groups where some group members had answered the question correctly, whereas others had answered it incorrectly.

[13] We cannot reject that *Pos. Avg Don. t-1 + Pos. Avg Don. t-1 ∗ Observation = 0* ($p = 0.870$) or that *Zero Avg Don. t-1 + Zero Avg Don. t-1 ∗ Observation = 0* ($p = 0.810$).

[14] We reject: *Neg. Avg Don. t-1 + Neg. Avg Don. t-1 ∗ Observation = 0* ($p < 0.001$).

[15] We cannot reject that *Zero Avg Don. t-1 + Zero Avg Don. t-1 * ObservationSP = 0* ($p = 0.225$).

[16] We reject: *Neg. Avg Don. t-1 + Neg. Avg Don. t-1 * ObservationSP = 0* ($p < 0.001$).

[17] We reject: *Pos. Avg Don. t-1 + Pos. Avg Don. t-1 * ObservationSP = 0* ($p = 0.005$).

[18] Further analysis also shows that the magnitude of the interactions between *Neg. Avg Don. t-1* and the treatment dummies is significantly different between Observation and ObservationSP (-0.29 vs. -0.17, $p=0.043$). Similarly, the interaction between *Pos. Avg Don. t-1* and the treatment dummies is also significantly different between treatments (-0.02 vs. 0.21, $p<0.01$).

Table 4
Random-effects GLS regressions. Dependent variable is donation of subject *i* in period *t* of Part III of the experiment of ObservationSP since proximity was observable only in this treatment. Robust standard errors reported in parentheses (standard errors clustered at the group level). Column 1 uses observations from groups where all group members answered the knowledge question of Part I in the same way (either correctly or incorrectly). Column 2 uses observations from groups where group members gave different answers to the knowledge question. Results are not affected if we classify the groups where 75% of members gave the same answer as high proximity, instead of low proximity (see also Table A.3). Controls are the same as in Table 2; see note to Table 2. Stars indicate significant differences at the conventional levels of $^{*}p<0.1$, $^{**}p<0.05$, and $^{***}p<0.01$.

DV: Amount Change in Charity Box (only in ObservationSP condition)	(1) Proximate Groups	(2) Non-Proximate Groups
Negative Avg. Donation t-1	−0.16***	−0.09***
	(0.06)	(0.03)
Positive Avg. Donation t-1	0.21***	0.16
	(0.07)	(0.11)
Zero Avg. Donation t-1	−2.24	3.83
	(2.35)	(2.44)
Groupsize = 4	−4.52**	−1.19
	(2.05)	(2.67)
Initial Individual Donation (Part II)	0.66***	0.65***
	(0.05)	(0.06)
Period	−0.16	−0.39***
	(0.14)	(0.14)
Constant	−15.43	−58.46***
	(15.98)	(16.94)
Controls	Yes	Yes
N.	3648	3914
N. Clusters	78	70
R^2	0.55	0.37

The regression results confirm that, among participants of groups with high social proximity (column 1), donations are affected by examples of both giving and taking. In contrast, in groups with low proximity (column 2), participants responded to examples of taking, but not to examples of giving. Moreover, although not significantly different, the point estimates indicate the magnitude of the effects of giving and taking are larger among high versus low proximity groups (0.16 vs. 0.09 for taking; 0.21 vs. 0.16 for giving).[19]

3.3. Heterogeneity in the erosion of norm compliance

An additional question that we address in this subsection is whether initial donation dispositions mediate responses to observation of others' behavior. From a policy perspective, this analysis is interesting because it not only helps to understand who is more susceptible to the effects of observation, but to what extent their behavior changes and contributes to the erosion of norm compliance.

Based on the donation behavior in Part II, we divided participants into three types: givers, takers, and abstainers (see Table 1). To examine how each type is differently affected by peer information, we consider their behavior across two distinct phases of the experiment: Part II, where there was no information about others' behavior, and Part III, where, in the treatments with observability, they continuously received information about other group members' ToG decisions. Fig. 2 plots, separately for each type and treatment, the distribution of donations made in these two phases of the experiment, depicted in blue and grey, respectively (see also Fig. A.8). The vertical lines in each panel indicate average donation behavior and the colors correspond to the respective phase.

We have a number of interesting results. In NoObservation (leftmost column of Fig. 2), most individuals only make small changes to their behavior between Part II and III, which is expected given that there is no observability in this treatment. Givers' donation decrease by 13.1 ECUs ($p = 0.013$). Abstainers, by definition, give 0 to the charity in Part II. In Part III, their

[19] One could further distinguish between groups where all members gave a correct answer and groups where all members gave an incorrect answer. However, we have only a very small number of groups where all members gave a correct answer to the knowledge question (7 groups) and therefore we do not have sufficient statistical power to draw reliable conclusions. Nevertheless, in the Online Appendix (Table A.3, Fig. A.6) we report an analysis analogous to that in Table 4, but where we further split the proximate groups between those where all subjects answered correctly (7 groups) and those where all answered incorrectly (71 groups). In both cases, the coefficients of the lagged variables show that peer compliance has a positive effect on own compliance, while peer violations have a negative effect. The coefficients are larger in groups where all subjects answered correctly. We refrain from commenting on the statistical significance of the regression with 7 groups. The effects in the regression with 71 groups are statistically significant, showing – perhaps surprisingly – that sharing an incorrect answer also triggers a social proximity effect.

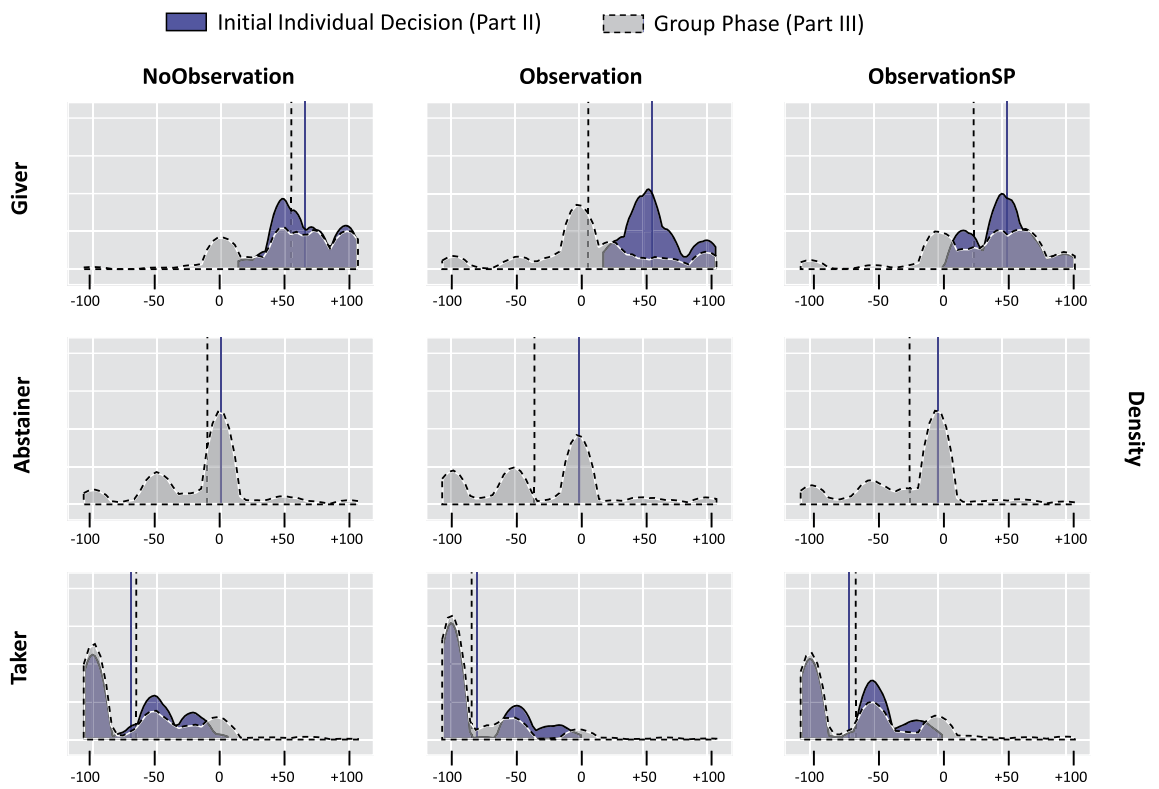

Fig. 2. Distribution of donations (density) disaggregated by group of subject (takers, abstainers, givers, as defined by Part II initial behavior) and treatment. Colors correspond to the respective phases. Blue: donations in Part II. Grey: average donations in Part III. Vertical lines represent averages in those phases. (For interpretation of the colors in the figure(s), the reader is referred to the web version of this article.)

average donation drops by -13.0 ECUs, which is statistically significant ($p = 0.005$). Finally, takers' behavior slightly increases by 4.4 ECUs, which is not statistically significant ($p = 0.140$). The change in behavior of both givers and abstainers is larger than that of takers (both $p < 0.005$); we find no difference between givers and abstainers ($p = 0.994$).[20]

In Observation (Fig. 2, middle column), donations drop substantially. Again, we find the largest changes in behavior for givers and abstainers, while takers only minimally adjust behavior. Takers' donations decrease by -3.8 ECUs ($p = 0.220$). Givers reduce their donations by -51.2 ECUs and abstainers by -30.8 ECUs. Both effects are highly significant ($p < 0.001$) and different from the behavior change of takers (both $p < 0.001$). In addition, the change in behavior of givers is significantly larger than that of abstainers ($p = 0.011$).

We find similar effects in ObservationSP (Fig. 2, rightmost column), albeit smaller in magnitude. Givers' donations drop by -21.7 ECUs ($p < 0.001$). Abstainers' donations drop by -20.1 ECUs ($p < 0.001$). Takers' donations increase by 6.9 ECUs ($p = 0.009$). The change in donations of givers and abstainers is larger than that of takers ($p < 0.001$). We find no difference between givers and abstainers ($p = 0.764$).

The main conclusion from this analysis is that initial givers display the largest changes in behavior between Part II and III of the experiment. They are followed by initial abstainers and initial takers. The reason for this is intuitive: based on Fig. 1, in Part III givers experienced the largest discrepancy between their own behavior and the behavior of their group members. In contrast, takers observed the smallest difference. Therefore, givers had to make the largest (downwards) revisions to their expectations about others' norm compliance after receiving information about how other group members *actually* behaved. Since compliance with norms partly depends on whether others also comply (Bicchieri, 2006), givers' behavior is strongly influenced by what they observe.

[20] To assess the statistical significance of the changes in behavior between Part II and III, we use OLS regressions, conducted separately for each treatment. The dependent variable measures, for each subject, the difference between the donation made in Part II and the average donation made in Part III (thus, the subject is the unit of observation in this analysis). We regress this on the takers/givers/abstainers dummies, and test whether, for each category, the estimated coefficient is significantly different from zero. The standard errors are clustered at the group level. We report two-sided p-values adjusted for multiple hypothesis testing (Benjamini and Hochberg, 1995).

4. Discussion and conclusion

Social norms are a fundamental component of social and economic life. Therefore, it is important to study conditions under which norm compliance occurs. In this paper, we focused on how observing others' behavior influences individual norm compliance. To investigate this, we designed a non-strategic Take-or-Give (ToG) donation game where people could give to charity, take from it, or abstain from changing the initial allocation between the self and the charity. Using a series of norm-elicitation experiments, we established that most people think taking from the charity is socially inappropriate, whereas abstaining or giving to the charity is appropriate. We then examined the effect of letting individuals observe each other's behavior in a repeated version of the ToG game. Our behavioral results reveal a notable asymmetry in the effect of observing peer behavior: observing other anonymous individuals violating the norm (taking from charity) increased the likelihood that the observers transgress as well. Observing that others donate to charity, however, did not increase donations to the charity. In sum, observing socially inappropriate behavior by anonymous people eroded norm compliance in a way that was not compensated by observing socially appropriate behavior. Our additional experiments show that this partly occurs because observing inappropriate behavior erodes the social norm of giving.

While this asymmetry in reactions paints a bleak picture for norm compliance when other anonymous people can be observed, in most real-world interactions individuals can observe their social proximity to the people they interact with. Assessing similarities with others may bring forth a mechanism of group identification that may promote *symmetrical* behavioral conformity within the group. The reason for this phenomenon is that the individual may feel that deviations from group behavior, whether positive or negative, signal a lack of commitment to the group. Individuals fear that this may trigger disapproval by other group members. Thus, they will be more vigilant — and responsive — to both examples of socially inappropriate and socially appropriate behavior. To study this conjecture, in our behavioral experiment we designed a treatment where participants could observe not only other subjects' behavior, but also a minimal cue about social proximity with these subjects (fandom of a sports team). Social proximity significantly reduced the asymmetry in reactions to observing socially inappropriate and socially appropriate behavior. Under social proximity, people not only paid attention to socially inappropriate behavior, but also to socially appropriate behavior, thus halting the erosion of norm compliance.

Our results about the importance of social proximity for norm compliance are in line with a large literature that shows significant differences in behavior towards socially proximate as opposed to socially distant others ("in-groups" vs. "out-groups"). For instance, individuals are more altruistic, trusting, and trustworthy towards in-groups rather than out-groups (McEvily et al., 2006; Balliet et al., 2014; Dimant, 2021), more likely to comply with requests by an in-group rather than an out-group (Burger et al., 2004), and more likely to take an in-group's advice into account (for a review and meta-analysis of this literature, see Gino et al., 2009b; Lane, 2016).

Our main contribution is showing that social proximity can stabilize norm compliance by reducing the asymmetry in reactions to observing norm violations and norm compliance. Punishment, often seen as an important stabilizer of social norms, may not be necessary when social proximity induces emulation of norm compliance. This highlights the importance of integrating the broader social context in the study of norm compliance. Most existing experimental research studies norms in abstract, anonymous, and context-neutral decision settings. While using contextually neutral decision environments is one of the hallmarks of experimental control, we show that this comes at the cost of missing important insights about the drivers of norm compliance. Our results show that one would draw substantially different conclusions about the effects of observation on norm compliance depending on whether interactions occur among strangers or socially proximate individuals.

Our results have also key implications for the design of behavioral change interventions (for a discussion, see Bicchieri and Dimant, 2019). We show that providing information about the behavior of anonymous strangers — an intervention that is commonly used by designers of behavioral change — is likely to backfire when the information reveals that non-compliance is widespread. This is consistent with previous studies finding limited effectiveness for this type of interventions (e.g., Cialdini et al., 2006; Dimant et al., 2020; Bicchieri et al., 2021; Dimant et al., 2021; Gelfand et al., 2021; Ren et al., 2021). Existing research has discussed mechanisms that could counterbalance this negative effect of peer information, e.g., by providing individuals with a reminder of the normative value of compliance (Schultz et al., 2007; Jachimowicz et al., 2018). Our findings suggest an alternative solution: providing individuals targeted information about the behavior of *similar* others may be sufficient to halt the erosion of norm compliance.

Data Availability

Data and scripts can be found following this link.

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.geb.2021.11.012.

References

Akerlof, G.A., Kranton, R.E., 2000. Economics and identity. Q. J. Econ. 115 (3), 715–753.

Andreoni, J., Bernheim, B.D., 2009. Social image and the 50–50 norm: a theoretical and experimental analysis of audience effects. Econometrica 77 (5), 1607–1636.
Aron, A., Aron, E.N., Smollan, D., 1992. Inclusion of other in the self scale and the structure of interpersonal closeness. J. Pers. Soc. Psychol. 63 (4), 596.
Azrieli, Y., Chambers, C.P., Healy, P.J., 2018. Incentives in experiments: a theoretical analysis. J. Polit. Econ. 126 (4), 1472–1503.
Balliet, D., Wu, J., De Dreu, C.K., 2014. Ingroup favoritism in cooperation: a meta-analysis. Psychol. Bull. 140 (6), 1556.
Bardsley, N., 2008. Dictator game giving: altruism or artefact? Exp. Econ. 11 (2), 122–133.
Barr, A., Lane, T., Nosenzo, D., 2018. On the social inappropriateness of discrimination. J. Public Econ. 164, 153–164.
Bašić, Z., Verrina, E., 2020. Personal norms—and not only social norms—shape economic behavior. MPI Collective Goods Discussion Paper, (2020/25).
Benjamini, Y., Hochberg, Y., 1995. Controlling the false discovery rate: a practical and powerful approach to multiple testing. J. R. Stat. Soc., Ser. B, Methodol., 289–300.
Bicchieri, C., 2006. The Grammar of Society: The Nature and Dynamics of Social Norms. Cambridge University Press.
Bicchieri, C., 2016. Norms in the Wild: How to Diagnose, Measure, and Change Social Norms. Oxford University Press.
Bicchieri, C., Chavez, A., 2010. Behaving as expected: public information and fairness norms. J. Behav. Decis. Mak. 23 (2), 161–178.
Bicchieri, C., Dimant, E., 2019. Nudging with care: the risks and benefits of social information. Public Choice.
Bicchieri, C., Dimant, E., Sonderegger, S., 2020. It's not a lie if you believe the norm does not apply: conditional norm-following with strategic beliefs. Working Paper Available at SSRN, https://doi.org/10.2139/ssrn.3326146.
Bicchieri, C., Dimant, E., Xiao, E., 2021. Deviant or wrong? The effects of norm information on the efficacy of punishment. J. Econ. Behav. Organ.
Bolton, G., Dimant, E., Schmidt, U., 2021. Observability and social image: on the robustness and fragility of reciprocity. J. Econ. Behav. Organ. 191, 946–964.
Bond, R., 2005. Group size and conformity. Group Process. Intergroup Relat. 8 (4), 331–354.
Buckenmaier, J., Dimant, E., 2021. The experience is (not) everything: sequential outcomes and social decision-making. Econ. Lett., 109916.
Buckenmaier, J., Dimant, E., Posten, A.-C., Schmidt, U., 2021. Efficient institutions and effective deterrence: on timing and uncertainty of formal sanctions. J. Risk Uncertain., 1–25.
Burger, J.M., Messian, N., Patel, S., del Prado, A., Anderson, C., 2004. What a coincidence! The effects of incidental similarity on compliance. Pers. Soc. Psychol. Bull. 30 (1), 35–43.
Bursztyn, L., Egorov, G., Fiorin, S., 2020. From extreme to mainstream: the erosion of social norms. Am. Econ. Rev.
Chang, D., Chen, R., Krupka, E., 2019. Rhetoric matters: a social norms explanation for the anomaly of framing. Games Econ. Behav. 116, 158–178.
Charness, G., Gneezy, U., 2008. What's in a name? Anonymity and social distance in dictator and ultimatum games. J. Econ. Behav. Organ. 68 (1), 29–35.
Charness, G., Gneezy, U., Halladay, B., 2016. Experimental methods: pay one or pay all. J. Econ. Behav. Organ. 131, 141–150.
Charness, G., Holder, P., 2019. Charity in the laboratory: matching, competition, and group identity. Manag. Sci. 65 (3), 1398–1407.
Charness, G., Naef, M., Sontuoso, A., 2019. Opportunistic conformism. J. Econ. Theory 180, 100–134.
Charness, G., Rabin, M., 2002. Understanding social preferences with simple tests. Q. J. Econ. 117 (3), 817–869.
Chen, Y., Li, S.X., 2009. Group identity and social preferences. Am. Econ. Rev. 99 (1), 431–457.
Cialdini, R.B., Demaine, L.J., Sagarin, B.J., Barrett, D.W., Rhoads, K., Winter, P.L., 2006. Managing social norms for persuasive impact. Soc. Influence 1 (1), 3–15.
Coleman, J.S., 1994. Foundations of Social Theory. Harvard University Press.
d'Adda, G., Drouvelis, M., Nosenzo, D., 2016. Norm elicitation in within-subject designs: testing for order effects. J. Behav. Exper. Econ. 62, 1–7.
d'Adda, G., Dufwenberg, M., Passarelli, F., Tabellini, G., 2020. Social norms with private values: theory and experiments. Games Econ. Behav. 124, 288–304.
Dimant, E., 2019. Contagion of pro- and anti-social behavior among peers and the role of social proximity. J. Econ. Psychol. 73, 66–88.
Dimant, E., 2021. Hate trumps love: the impact of political polarization on social preferences. Working Paper Available at SSRN, https://doi.org/10.2139/ssrn.3680871.
Dimant, E., Gerben, A.v.K., Shalvi, S., 2020. Requiem for a nudge: framing effects in nudging honest. J. Econ. Behav. Organ. 172, 247–266.
Dimant, E., Gesche, T., 2020. Nudging enforcers: how norm perceptions and motives for lying shape sanctions. Working Paper Available at SSRN, https://doi.org/10.2139/ssrn.3664995.
Dimant, E., Pieper, D., Clemente, E.G., Dreber, A., Gelfand, M.J., 2021. Politicizing mask-wearing: predicting the success of behavioral interventions among republicans and democrats. Working Paper Available at SSRN, https://doi.org/10.2139/ssrn.3915256.
Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., Wagner, G.G., 2011. Individual risk attitudes: measurement, determinants, and behavioral consequences. J. Eur. Econ. Assoc. 9 (3), 522–550.
Eckel, C.C., Grossman, P.J., 1996. Altruism in anonymous dictator games. Games Econ. Behav. 16 (2), 181–191.
Exley, C.L., 2015. Excusing selfishness in charitable giving: the role of risk. Rev. Econ. Stud. 83 (2), 587–628.
Fehr, E., Schurtenberger, I., 2018. Normative foundations of human cooperation. Nat. Hum. Behav. 2 (7), 458.
Fischbacher, U., 2007. z-tree: Zurich toolbox for ready-made economic experiments. Exp. Econ. 10 (2), 171–178.
Fischbacher, U., Gächter, S., 2010. Social preferences, beliefs, and the dynamics of free riding in public goods experiments. Am. Econ. Rev. 100 (1), 541–556.
Fisman, R., Miguel, E., 2007. Corruption, norms, and legal enforcement: evidence from diplomatic parking tickets. J. Polit. Econ. 115 (6), 1020–1048.
Fortin, B., Lacroix, G., Villeval, M.-C., 2007. Tax evasion and social interactions. J. Public Econ. 91 (11–12), 2089–2112.
Gächter, S., Gerhards, L., Nosenzo, D., 2017. The importance of peers for compliance with norms of fair sharing. Eur. Econ. Rev. 97, 72–86.
Gächter, S., Molleman, L., Nosenzo, D., 2021. When and why people follow rules. Unpublished Manuscript.
Gächter, S., Starmer, C., Tufano, F., 2015. Measuring the closeness of relationships: a comprehensive evaluation of the 'inclusion of the other in the self' scale. PLoS ONE 10 (6), e0129478.
Gangadharan, L., Jain, T., Maitra, P., Vecci, J., 2016. Social identity and governance: the behavioral response to female leaders. Eur. Econ. Rev. 90, 302–325.
Gelfand, M., Li, R., Stamkou, E., Pieper, D., Denison, E., Fernandez, J., Choi, V.K., Chatman, J., Jackson, J.C., Dimant, E., 2021. Persuading Republicans and Democrats to Comply with Mask Wearing: An Intervention Tournament.
Gino, F., Ayal, S., Ariely, D., 2009a. Contagion and differentiation in unethical behavior: the effect of one bad apple on the barrel. Psychol. Sci. 20 (3), 393–398.
Gino, F., Shang, J., Croson, R., 2009b. The impact of information from similar or different advisors on judgment. Organ. Behav. Hum. Decis. Process. 108 (2), 287–302.
Grossman, P.J., Eckel, C.C., 2015. Giving versus taking for a cause. Econ. Lett. 132, 28–30.
Hogg, M.A., 1992. The Social Psychology of Group Cohesiveness: From Attraction to Social Identity. Harvester Wheatsheaf.
Jachimowicz, J.M., Hauser, O.P., O'Brien, J.D., Sherman, E., Galinsky, A.D., 2018. The critical role of second-order normative beliefs in predicting energy conservation. Nat. Hum. Behav. 2 (10), 757.
Keizer, K., Lindenberg, S., Steg, L., 2008. The spreading of disorder. Science 322 (5908), 1681–1685.
Kimbrough, E.O., Vostroknutov, A., 2016. Norms make preferences social. J. Eur. Econ. Assoc. 14 (3), 608–638.
Kimbrough, E.O., Vostroknutov, A., 2018. A portable method of eliciting respect for social norms. Econ. Lett. 168, 147–150.
Kocher, M.G., Schudy, S., Spantig, L., 2017. I lie? We lie! Why? Experimental evidence on a dishonesty shift in groups. Manag. Sci. 64 (9), 3995–4008.
Korenok, O., Millner, E.L., Razzolini, L., 2014. Taking, giving, and impure altruism in dictator games. Exp. Econ. 17 (3), 488–500.
Krupka, E.L., Croson, R.T., 2016. The differential impact of social norms cues on charitable contributions. J. Econ. Behav. Organ. 128, 149–158.

Krupka, E.L., Leider, S., Jiang, M., 2017. A meeting of the minds: informal agreements and social norms. Manag. Sci. 63 (6), 1708–1729.
Krupka, E.L., Weber, R.A., 2013. Identifying social norms using coordination games: why does dictator game sharing vary? J. Eur. Econ. Assoc. 11 (3), 495–524.
Lane, T., 2016. Discrimination in the laboratory: a meta-analysis of economics experiments. Eur. Econ. Rev. 90, 375–402.
List, J.A., 2007. On the interpretation of giving in dictator games. J. Polit. Econ. 115 (3), 482–493.
McEvily, B., Weber, R.A., Bicchieri, C., Ho, V.T., 2006. Can groups be trusted? An experimental study of trust in collective entities. Handbook Trust Res., 52–67.
Offerman, T., 2002. Hurting hurts more than helping helps. Eur. Econ. Rev. 46 (8), 1423–1437.
Ren, Z.B., Dimant, E., Schweitzer, M.E., 2021. Social motives for sharing conspiracy theories. Working Paper Available at SSRN, https://doi.org/10.2139/ssrn.3919364.
Reuben, E., Riedl, A., 2013. Enforcement of contribution norms in public good games with heterogeneous populations. Games Econ. Behav. 77 (1), 122–137.
Rosaz, J., Slonim, R., Villeval, M.C., 2016. Quitting and peer effects at work. Labour Econ. 39, 55–67.
Rustichini, A., Villeval, M.C., 2014. Moral hypocrisy, power and social preferences. J. Econ. Behav. Organ. 107, 10–24.
Schram, A., Charness, G., 2015. Inducing social norms in laboratory allocation choices. Manag. Sci. 61 (7), 1531–1546.
Schultz, P.W., Nolan, J.M., Cialdini, R.B., Goldstein, N.J., Griskevicius, V., 2007. The constructive, destructive, and reconstructive power of social norms. Psychol. Sci. 18 (5), 429–434.
Strathman, A., Gleicher, F., Boninger, D.S., Edwards, C.S., 1994. The consideration of future consequences: weighing immediate and distant outcomes of behavior. J. Pers. Soc. Psychol. 66 (4), 742.
Sutter, M., Haigner, S., Kocher, M.G., 2010. Choosing the carrot or the stick? Endogenous institutional choice in social dilemma situations. Rev. Econ. Stud. 77 (4), 1540–1566.
Tajfel, H., 1982. Social psychology of intergroup relations. Annu. Rev. Psychol. 33 (1), 1–39.
Tajfel, H., Turner, J.C., 1979. An integrative theory of intergroup conflict. Soc. Psychol. Intergroup Relat. 33 (47), 56–65.
Tangney, J.P., Baumeister, R.F., Boone, A.L., 2004. High self-control predicts good adjustment, less pathology, better grades, and interpersonal success. J. Pers. 72 (2), 271–324.
Thöni, C., Gächter, S., 2015. Peer effects and social preferences in voluntary cooperation: a theoretical and experimental analysis. J. Econ. Psychol. 48, 72–88.
Tsutsui, K., Zizzo, D.J., 2014. Group status, minorities and trust. Exp. Econ. 17 (2), 215–244.