

# Are Victims Truly Worse Off in the Presence of Bystanders? Revisiting the Bystander Effect

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*Previous studies have shown that individuals are less likely to help a person in need when there are “bystanders” present who can also offer help. We designed an experiment to re-examine this “bystander effect” using modified dictator games. We find lower giving rates in the presence of bystanders, confirming the existence of a bystander effect. However, we also show that the recipient’s welfare is greater when bystanders are present, challenging the usual interpretation that the bystander effect is due to an erosion of prosocial values and social norms.*

## LA SITUATION DES VICTIMES EMPIRE-T-ELLE VRAIMENT EN PRÉSENCE DE TÉMOINS ? UN RÉEXAMEN DE L’« EFFET DU TÉMOIN »

*Des études antérieures ont montré que la volonté d’aider une personne dans le besoin est plus faible en présence de « témoins » qui peuvent également offrir leur aide. Nous proposons un réexamen expérimental de cet « effet du témoin » à l’aide de jeux du dictateur modifiés. Les taux de transfert observés sont inférieurs en présence de témoins, ce qui confirme l’existence d’un effet du témoin. Cependant, les résultats montrent également que le bien-être du receveur est supérieur lorsque des témoins sont présents, ce qui remet en cause l’interprétation habituelle selon laquelle l’effet du témoin est dû à une érosion des valeurs prosociales et des normes sociales.*

*Keywords: bystander effect, bystander dilemma, diffusion of responsibility, dictator game, social norms*

*Mots clés: effet du témoin, dilemme du témoin, diffusion de la responsabilité, jeu de dictateur, normes sociales*

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## INTRODUCTION

In many situations, the creation of beneficial outcomes for others and/or society depends on the actions of a single individual. Examples include cleaning a communal area in shared residences, volunteering for a social activity or event, and many types of situations in which one person is in need of help from another individual. There is a large experimental literature supporting the idea that, in such situations, the presence of multiple individuals who can potentially generate the beneficial outcome, is detrimental. This phenomenon, which has been labelled the “bystander effect,” was first documented in the seminal paper by Darley and Latané [1968]. Participants in their experiment witnessed a confederate staging an epilepsy seizure. Darley and Latané [1968] found that subjects who believed that other participants were also witnessing the seizure were less likely to react to the incident and call for help compared to those who believed they were alone. A large literature in economics and psychology has developed since this study confirming that the presence of multiple potential “helpers” lowers the rate at which each single individual is willing to help (e.g., Darley and Latané [1968]; Latané and Darley [1970]; Cryder and Loewenstein [2012]; see Fischer et al. [2011] for a review).<sup>1</sup>

At the heart of this phenomenon lies the existence of strategic interdependence between the actions of the multiple individuals who can potentially generate the beneficial outcome: only one person is needed to produce this outcome, and yet everyone can volunteer to do so. This interdependence between bystanders’ actions introduces different possible motivations that may reduce each individual’s willingness to benefit others when this is costly for the individual. For instance, if one bystander believes that someone else may actually help, she may be tempted to free-ride. If she instead believes that no one else will help, then the blame for not helping can be diffused among all bystanders, which may lower the psychological cost of guilt (Latané and Nida [1981]; Dana, Weber and Kuang [2007]). Both mechanisms may affect the social norm to help those in need, as suggested by Darley and Latané [1968]. Thus, the conventional wisdom is that the presence of bystanders “erodes” prosocial values and social norms.

In this paper we report an experiment that challenges this relatively pessimistic interpretation of the bystander effect. Much of the focus of this literature has been on the individual probability to offer help (the so-called “helping rate”). However, we conjecture that the mere fall in the helping rate is not necessarily a good measure to gauge whether the presence of bystanders results in an erosion of prosocial values or indeed harms the victim. We identify two reasons why this may be the case. First, when several bystanders can help, the helping rate would have to fall considerably for the victim to be less likely to receive help by *at least* one of them. Consider the case where the helping rate is 50% when there is only one bystander, implying that the recipient receives help with 50% probability. If the number of bystanders triples, the recipient would

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1. The bystander effect has been studied in various settings in the economics literature, including dictator games (e.g., Dana, Weber and Kuang [2007]; Panchanathan, Frankenhuys and Silk [2013]), sender-receiver games (e.g., Behnk, Hao and Reuben [2017]), lying games (e.g., Weisel and Shalvi [2015]; Barr and Michailidou [2017]), and market games (e.g., Falk and Szech [2013]; Bartling, Weber and Yao [2015]).

be less likely to receive help by at least one of them only if the helping rate falls below 21%. Yet, a review by Latané and Nida [1981] shows that the reduction in helping rates in the presence of bystanders observed in experimental studies is typically small. Thus, it is unclear whether the victim is actually harmed by the presence of bystanders.<sup>2</sup>

Second, we notice that, in practice, individuals differ in their ability to help others, and so whether or not a drop in the helping rate is detrimental to the victim depends on *whose* help (the most or least able individuals) is crowded out in the presence of bystanders.<sup>3</sup> If the individuals who abstain from helping are those who are least able to help, then the average quality of help (conditional on help being offered) may actually increase despite a fall in the helping rate, and the victim may potentially be better off. In this sense, the very reason why a low-ability individual might abstain from providing help in the presence of bystanders could be genuine care for the person in need, rather than an erosion of prosocial values and social norms. However, if the high-ability individuals are those who largely abstain from helping, then the fall in the helping rate may lead to a fall in the quality of help provided, making the victim worse off. To the best of our knowledge, none of the previous studies on the bystander effect allow for an analysis of the extent or quality of help that is received by the victim.

Taken together, these two arguments challenge the conventional interpretation of the bystander effect: the mere fact that the helping rate falls in the presence of bystanders may not necessarily imply a reduction in the welfare of the victim, or be driven by mechanisms that imply an erosion of prosocial values and social norms. Despite a drop in the helping rate, the victim may not be less likely to receive help from at least one bystander, and the quality of the help received may actually be higher; either of these effects may suffice to increase the victim's expected welfare. This also opens the possibility that the observed reduction in the helping rate may not necessarily be driven by a weakening of prosocial values and social norms.

In the second section, we describe an experiment designed to probe this conjecture. Our experimental paradigm is based on a dictator game modified to capture the essence of the bystander dilemma. In our "Bystander" treatment, a group of seven dictators is matched with one recipient. Dictators simultaneously choose whether or not to give money to the recipient and, if they choose to give, how much (an amount between £1 and £10). The rule for determining the earnings of the recipient is as follows: if at least one dictator gives, the recipient receives positive earnings with certainty; among the dictators who choose to

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2. See Campos-Mercade [2017] on this point. Moreover, note that this is also the case in the volunteer's dilemma (VOD). In the VOD, a group of individuals can volunteer to produce a public good. A dilemma arises since volunteering is in each individual's interest if no one else volunteers, but volunteering when at least one other individual does so only results in a deduction of money with no further increase of the public good. These studies typically show that, although the individual propensity to volunteer decreases with group size, the probability that at least one group member volunteers (and so the public good is provided) does not (Franzen [1995]; Weesie and Franzen [1998]; Goeree and Holt [2005]; Goeree, Holt and Smith [2017]).

3. In many real life situations where an individual needs help (e.g., medical emergencies or situations involving crime), the outcome for the victim crucially depends not only on whether someone is willing to help, but also on the extent to which the person intervening has the necessary skills and resources to help (e.g., they may or may not be a medical doctor or an off-duty policeman).

give, one is selected at random and their choice is implemented. If all dictators choose not to give, then the recipient receives zero earnings.

The Bystander treatment captures the essence of the bystander dilemma by introducing a strategic interdependence among the seven dictators. The earnings of the recipient depend on the simultaneous decisions of all dictators, and how the choice of one dictator affects the recipient's earnings depends on the choices of the other six dictators. For instance, if a dictator chooses not to give, this may either result in zero earnings for the recipient (if all other six dictators also do not give) or not (if at least one other dictator gives). In this setting, the bystander effect can thus arise through two mechanisms that have been frequently mentioned in the literature as an explanation of the effect. First, dictators may expect others to give and therefore be tempted to free-ride on the benevolence of other dictators. Second, a "diffusion of responsibility" could occur whereby dictators abstain from giving because they believe that all other dictators are also abstaining, which makes everyone equally responsible for the bad outcome for the recipient.

We compare the Bystander treatment with a "Control" treatment in which there is no strategic interdependence between the dictators. As in the Bystander treatment, also in Control we match seven dictators with one recipient. However, differently from Bystander, the earnings of the recipient are computed using the choice of one, randomly chosen, dictator (regardless of whether they decided to give or not). Thus, although the number of dictators per group is the same in the Control and Bystander treatments (so as to control for possible effects related to the mere presence of other individuals), free-riding and diffusion of responsibility, which are at the heart of the bystander effect, cannot play a role in Control. This is because in Control the impact of each dictator's decision on the recipient's earnings is independent from the decisions of the other dictators in the group.<sup>4</sup>

Importantly, in both treatments, dictators choose not only whether to give, but also how much. This allows to verify whether the presence of bystanders has an effect on the "quality" of giving. As explained above, it is not clear whether the presence of bystanders affects positively or negatively the quality of help provided and the recipient's expected welfare. In our experiment, recipients will be worse off in expectations in the Bystander treatment only if the average amount given, conditional on giving, falls below the average amount given in the Control treatment. Otherwise, the recipient will not be worse off, and may in fact be better off in Bystander than in Control, even if the proportion of dictators who give falls.

In the third section, we report our data analysis, which focuses on three aspects of our treatment comparison.<sup>5</sup> First, we focus on the proportion of dictators

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4. Since our design holds constant the number of dictators in the decision situation across the Control and Bystander treatments, we cannot capture the differential effect of "being alone" in the decision situation as opposed to "being in the presence of others." To the extent that the "bystander effect" is due—at least in part—to this mechanism, our design is not well suited to study it. We further discuss this limitation of our design in the second section below.

5. Both treatments were run across two different conditions, in which subjects were either anonymous to one another, or identified by their name initials. We found no differences in any of the experimental outcomes across the two anonymity conditions and so we pool data across these conditions. We discuss the lack of effect of our anonymity manipulation in the third section and present a disaggregated analysis of data in the Online Appendix I, DOI : 10.3917/reco.706.0927.

who choose to give a positive amount to the recipient (what we call the “giving rate”). We find a marginally significant (at the 10% level) reduction in the giving rate in the Bystander treatment compared to Control (33% compared to 44%). This reproduces the commonly observed bystander effect whereby, in the presence of other individuals who can affect the welfare of a recipient, the willingness of each single individual to give is reduced.

Second, we compare the likelihood that the recipient will receive positive earnings across the two treatments. As explained above, this can be lower in Bystander than in Control, but only if the giving rate falls by a large amount so as to contrast the positive effect of having more individuals who are in principle “able” to give. We find that recipients are in fact significantly more likely to receive positive earnings in Bystander than in Control. Thus, if one focuses on this specific aspect of the recipient’s welfare, the recipient is actually better off in the presence of bystanders.

Third, we compare the recipient’s expected earnings across the two treatments. We find that these are significantly higher in the Bystander treatment compared to Control. The reason why this is the case is that, despite the fall in the giving rate, the average amount given, conditional on giving, is not significantly different between the two treatments. Thus, the “quality” of giving is not crowded out by the presence of bystanders. This, combined with the fact that the giving rate only falls by little in Bystander, implies that the recipient is better off in expectations in the presence of bystanders.

In addition to the above treatment comparisons, we also report an exploratory analysis of the motivational aspects behind dictators’ behaviour by using an incentivized measure of their beliefs about the behaviour of other dictators as well as results from a second experiment in which we measure the effect of the Bystander treatment on the social norm of giving using the norm elicitation task developed by Krupka and Weber [2013]. We find little evidence that free-riding or diffusion-of-responsibility considerations that weaken social norms are at play in our experiment.

In the fourth section, we discuss our results and conclusions. Although in our experiment we do observe a reduction in the individual willingness to give to others in the presence of bystanders, we find that overall this does not translate in lower expected earnings for the recipient. Indeed, recipients in our experiment are on average better off in the Bystander treatment than in Control. Our results call into question the conventional and relatively pessimistic interpretation of the bystander effect proposed in the previous literature.

## EXPERIMENTAL DESIGN

### Experimental Game and Treatments

Our experiment is based on a modified version of the dictator game. Subjects were randomly divided into groups of eight and then randomly assigned a role: one subject was assigned the role of recipient (called “Participant A” in the experiment) and seven subjects the role of dictator (“Participant B”). Dictators received an endowment of £10, while the recipient’s endowment was

£0. Recipients had no decision to make in the experiment. Each of the seven dictators in a group had to simultaneously choose whether or not to transfer a positive amount to the recipient. If they decided to transfer a positive amount, they had to specify the exact amount by choosing an integer between £1 and £10. Although all seven dictators in each group were asked to make this choice, at most one dictator in each group would have their choice implemented. Which choice was implemented was randomly determined after all dictators had made their decision so that all choices were elicited in an incentive-compatible way. Dictators who were not selected kept their initial endowment.

The Control and Bystander treatments differ in the rule used to determine which of the seven dictators' choices was selected to be implemented. In our Control treatment, each dictator had the same probability (equal to  $1/7$ ) of being selected and having their choice implemented, regardless of their own choice or the choices of the other dictators in their group. In the Bystander treatment, the probability that a dictator was selected depended both on their own choice and on the choices of other dictators in their group. If a dictator decided not to give, the probability of being selected was either zero (if at least one other dictator gave) or  $1/7$  (if none of the other dictators gave). If a dictator decided instead to give, the probability of being selected was equal to  $1/n^+$ , where  $n^+$  is the number of dictators choosing to give.

Our Bystander treatment introduces a strategic interdependence between dictators' actions that allows for the presence of two key phenomena typically present in the bystander dilemma (Dana, Loewenstein and Weber [2012]). First, it introduces *free-rider incentives* for dictators who are willing to give money to the recipient, but still prefer that someone else bears the cost of giving. For instance, if at least one other dictator gives, these dictators can withdraw their giving without causing the recipient to earn nothing. Second, the Bystander treatment allows for a *diffusion of responsibility* among dictators since the blame of a bad outcome for the recipient can be shared among all the dictators in the group. For example, if the recipient ends up earning nothing, the blame falls equally on all dictators in the group: it would have been enough that any of them had chosen to give for the recipient to receive positive earnings.<sup>6</sup>

This strategic interdependence between dictators' actions is not present in our Control treatment. Free-riding on other dictators is not possible since each dictator's choice has a  $1/7$  probability of being implemented, regardless of what they or others do. For the same reason, there cannot be any diffusion of responsibility since the earnings of the recipient depend entirely on the dictator whose choice is selected to be implemented.

Note that our design keeps constant across treatments the number of dictators who are matched with the recipient. This is because we wanted to be able to isolate the "bystander effect" that is due to the strategic interdependence between dictators' actions rather than to the mere *presence* of bystanders. However, a potential drawback of this design choice is that, if the "bystander effect" is in part due to the behavioural difference between the psychology of "being alone" and the psychology of "being in the presence of others," then our experiment

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6. Note that because choices are made simultaneously we can exclude social comparison effects such as those proposed by Latané and Nida [1981], Latané and Dabbs Jr. [1975], and Bicchieri [2006], whereby individuals may imitate each other's inaction.

cannot capture this. Therefore, the results of our treatment comparisons should be interpreted holding this caveat in mind.

Our treatments also keep constant the procedures to elicit dictators' choices. In particular, in both treatments dictators were asked to make a choice before knowing whether their choice would be implemented. In the Bystander treatment, this reflects the underlying structure of the simultaneous game. In the Control treatment dictators' choices are independent from one another so our procedure is essentially akin to using the strategy method (Selten [1967]).<sup>7</sup>

In our treatment comparisons, we are mainly interested in studying the relation between dictators' giving and recipient's earnings across the two treatments. As discussed above, a reduction in the giving rate (i.e., the proportion of dictators who choose to give, used as an estimate of an individual's propensity to give) in the Bystander treatment may not necessarily imply that the recipient is worse off than in Control. To see this note that in the Bystander treatment the recipient's earnings are either equal to zero (if all dictators in a group do not give) or (if at least one dictator chooses to give) to the average amount given by dictators who make a positive transfer, denoted  $\bar{t}_B^+$ . The probability that at least one dictator gives is equal to  $1 - (1 - p_B)^n$  where  $p_B$  is the giving rate in Bystander and  $n$  is the number of dictators in the group.<sup>8</sup> Thus, the recipient's expected earnings in Bystander are  $[1 - (1 - p_B)^n] \bar{t}_B^+$ . In the Control treatment, the recipient's expected earnings simply equal the average amount given by all dictators, denoted  $\bar{t}_C$ . Note that this can be rewritten as  $p_C \bar{t}_C^+$ , i.e., the product between the giving rate and the average amount given by dictators who make a positive transfer in Control. The recipient's expected earnings are thus lower in Bystander if:

$$[1 - (1 - p_B)^n] \bar{t}_B^+ < p_C \bar{t}_C^+$$

This expression implies that a fall in the giving rate in Bystander might not necessarily translate into a reduction in the recipient's expected earnings. First, consider the case where the average amount given (conditional on giving) is the same across the two treatments,  $\bar{t}_B^+ = \bar{t}_C^+$ . Then the recipient will be worse off in expectations in the Bystander treatment only if the giving rate in Bystander falls below  $1 - (1 - p_C)^{1/n}$ . In our treatment, where  $n = 7$ , this implies a substantial drop in the giving rate. For example, if the giving rate in Control is 50%, the giving rate in Bystander would have to fall below 9.4% for the recipient to be worse off in expectations. Note also that the same condition holds for the probability that the recipient receives positive earnings. Thus, according

7. Note that this implies that, in both treatments, dictators' actions are implemented probabilistically. This may have an impact *per se* on the propensity to give since dictators' attitudes towards risk may play a role on their giving decisions. Indeed, Cettolin, Riedl et Tran [2017] show that risk preferences matter in allocation games where the final amount transferred by an allocator to a recipient is determined probabilistically.

8. Here we are assuming that each dictator has a propensity to give a positive amount to the recipient equal to  $p_B$ , and thus the probability that in a group of  $n$  dictators, all of them give nothing is equal to  $(1 - p_B)^n$ .

to this measure of recipient's welfare (whether or not the recipient ends up with zero earnings), a substantial drop in the propensity to give is needed to make the recipient worse off.<sup>9</sup>

Moreover, whether or not the recipient will be worse off also depends on how the average amount given (conditional on giving) compares between the two treatments. This is a feature that previous studies of the bystander effect could not analyse because of their focus on binary decision environments where subjects could only either help/give or not. In contrast, we are interested in studying how the presence of bystanders affects the "quality" of help provided and not just *whether* help is provided. Thus, in our design dictators can transfer any integer amount between £1 and £10 to the recipient, if they decide to give.

The average amount given (conditional on giving) may fall in Bystander relative to Control if there is an erosion of prosocial values and social norms in the presence of bystanders. For example, dictators may reduce the amount given in order to free-ride on other dictators. Similarly, dictators may give less due to a diffusion of responsibility, if they believe that others also give little and feel that giving small amounts is more acceptable if everyone else does so.

On the other hand, there is a possibility that the presence of bystanders may increase the average amount given (conditional on giving). This is because in the Bystander treatment dictators may prefer giving nothing instead of a small amount in order to increase the expected earnings of the recipient. To see this, note that in the Control treatment the recipient's expected earnings increase monotonically in the amount transferred by a dictator. In contrast, a crucial feature of our Bystander treatment is that a dictator  $i$  affects the recipient's expected earnings positively only if they make a transfer that is greater than the average positive transfer of the other dictators in the group. Otherwise, the recipient would be better off if the dictator had abstained from giving. For example, a dictator would increase the recipient earnings by choosing not to give compared to giving £1 if the average positive transfer of all other dictators is greater than £1. This mechanism would essentially "crowd-out" the giving of those who are prepared to give little, letting instead "large donors" volunteer to help the recipient. Thus, the overall impact on the recipient's welfare may be positive.

## Beliefs and Norms Elicitation

The previous discussion suggests that a reduction in the giving rate in the Bystander treatment may not necessarily be driven by an erosion of prosocial values and social norms due to free-riding incentives or diffusion of responsibility. In order to gain a better understanding of subjects' motivations in the experiment, we elicited dictators' beliefs about other dictators' giving and the recipient's earnings in their group.

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9. While in our experiment we keep the group size constant, it may be interesting to study how variations in group size induce differences in giving rates and in the recipient's welfare. In particular, there may be non-monotonicities in the effect of  $n$  on giving, which are not captured by the simple calculation shown above. See Campos-Mercade [2017] for a recent experiment on the relation between group size and bystander effect.

The belief elicitation task was identical across treatments and was implemented after subjects had made their choices, but before they learned the outcomes of the dictator game. Dictators answered nine questions in total. In seven of these questions, each dictator was asked to guess how many of the other six dictators had chosen not to transfer, transfer £1, £2, £3, £4, £5, and £6 or more to the recipient.<sup>10</sup> In the other two questions, dictators' were asked about their beliefs about the recipient's final earnings as well as their second-order beliefs about the recipient's beliefs of his/her own earnings. All questions were incentivised by paying dictators £1 for giving the correct answer to one randomly selected question.<sup>11</sup>

To further probe the motivations behind dictator's giving in the two treatments, we also conducted a norm elicitation experiment to study how the presence of bystanders affected the social norms of giving in our setting. This experiment was conducted with a new group of subjects who had not taken part in the main experiment and used the norm elicitation method introduced by Krupka and Weber [2013]. Subjects read a vignette that described the dictator game, including a description of each of the possible actions dictators could take. Approximately half of the subjects read a description of the game played in the Bystander treatment, while others read a description of the Control treatment. After reading the vignette, subjects were asked to rate the social appropriateness of the actions available to the dictator, using a 6-point scale ranging from "very socially inappropriate" to "very socially appropriate." Subjects rated the actions independently but were given monetary incentives to coordinate their ratings with those of other subjects: at the end of the experiment, subjects were randomly matched into pairs and one of the rated actions was selected at random. If the subjects in the pair had rated the selected action in the same way, they both earned £8; otherwise they both earned £0.<sup>12</sup>

Note that our elicitation of norms involves "third-parties," i.e., subjects who did not themselves play the dictator game in either the Bystander or Control treatment. This is standard procedure in experiments using the Krupka-Weber norm-elicitation method. The use of such "between-subject" design is justified on the grounds that the norms that the procedure aims to elicit are "social" in the sense of being shared understandings of what is appropriate or inappropriate among a reference group of individuals (in our case, the reference group may be defined as the group of participants in experiments at our institution). Since both the behavioural and norm-elicitation experiments sample from the same population, it is reasonable to assume that the two samples hold the same social norms regarding the target behaviour elicited by the procedure.<sup>13</sup>

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10. Since transfers above the equal split (i.e., in excess of £5) are extremely rare in dictator games, we grouped the options of transferring £6, £7, £8, £9, and £10 into one question. This reduced the number of questions that subjects had to answer and made the task more manageable. Only 5 of the 252 dictators thought that another dictator had chosen to transfer between £6 and £10.

11. We also measured recipients' beliefs about how much the dictators in their group had chosen to transfer to them, their own earnings from the game, and how much the dictators in their group expected the recipient to earn on average. The recipient's questions were not incentivised so that dictators knew that recipients would earn £0 from the experiment if they did not receive anything in the dictator game.

12. These procedures follow Krupka, Leider and Jiang [2016]. The six possible ratings were: "very socially inappropriate," "socially inappropriate," "somewhat socially inappropriate," "somewhat socially appropriate," "socially appropriate," and "very socially appropriate."

13. It is of course also possible to elicit norms using a "within-subject" design, where norms are elicited from the same subjects whose behaviour is observed in the experiment (e.g., Barr, Lane and

## Procedures

The experiments were programmed in z-Tree (Fischbacher [2007]) and were run at the computerized laboratory of the University of Nottingham with students recruited through the online recruitment system ORSEE (Greiner [2015]). The main experiment was run with 288 subjects (252 dictators), evenly divided between Bystander and Control treatments.<sup>14</sup> We recruited another 122 subjects for the norm elicitation experiment (62 in Bystander and 60 in Control). Average earnings were £9.3 in the main experiment (ranging from £0 to £11) and £7.4 in the norm elicitation experiment (ranging from £4 to £12).<sup>15</sup> All sessions finished within one hour. Instructions for the experiments are reproduced in the Online Appendix II.

## RESULTS

The Bystander and Control treatments were run under two conditions. In one condition subjects remained fully anonymous to one another throughout the experiment, whereas in the other condition they were partially identified by the first letters of their forename and surname. This manipulation aimed to explore whether a reduction in anonymity moderates the bystander effect. We found no effect of a reduction in anonymity on giving or norms of giving and so in the remainder of the paper we pool the data from the two conditions.<sup>16</sup> We further discuss this manipulation in the Online Appendix I, where we also present a replication of the analysis reported in the paper disaggregated by the two conditions.

### Giving Behaviour and Recipient Earnings

Figure 1 shows the distribution of amounts transferred by the dictators in the two treatments and Table 1 summarizes the main outcomes of the experiment.

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Nosenzo [2018] for a study using norms elicited in a within-subject designs). However, a potential caveat to keep in mind is that, as discussed by D'Adda, Drouvelis and Nosenzo [2016], there may be spillovers between the norm-elicitation task and the behavioural task in within-subject designs.

14. Thus, our treatments are administered using a between-subject design. An interesting avenue for further research could be to design a within-subject experiment to study the individual-level determinants of the bystander effect.

15. There was no show-up fee for subjects in the main experiment because we wanted recipients to receive positive earnings only if dictators gave to them. A £4 show-up fee was paid in the norm elicitation experiment.

16. Our manipulation to reduce anonymity is fairly subtle and prevents full identification since subjects only learn each other's name initials. Thus, the null result may be interpreted as a failure of the manipulation to reduce anonymity across treatments. On the other hand, previous experiments that have used stronger manipulations to reduce anonymity (e.g., revealing family names; or having decisions announced in public) have produced mixed results (see, e.g., Dufwenberg and Muren [2006]; Charness and Gneezy [2008]), which may also explain why we do not find an effect in our experiment.

Figure 1. Distribution of transfers, by treatment

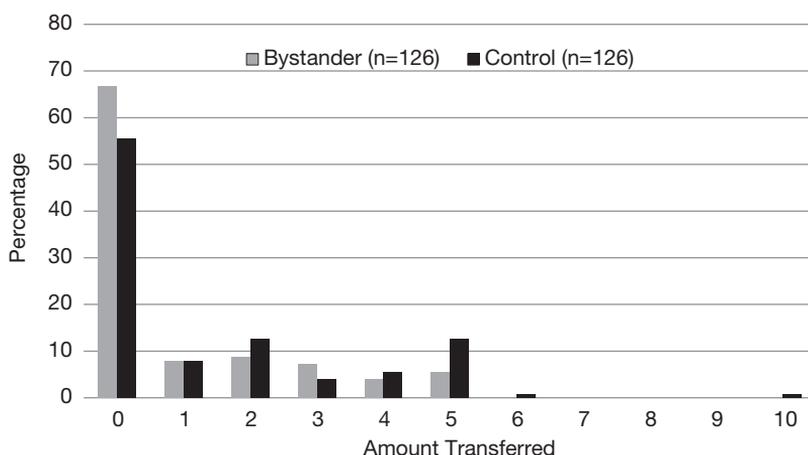


Table 1. Main outcomes of the experiment, by treatment

	Bystander	Control
Giving rate (% of dictators who chose to give)	33%	44%
Average amount given	0.90 (1.51)	1.44 (2.01)
Average amount given, conditional on giving	2.71 (1.40)	3.23 (1.82)
Probability that recipient receives positive earnings	94%	44%
Recipient's expected earnings	2.55	1.44

Note : Standard deviations in parentheses. In Bystander the probability that the recipient receives positive earnings is computed as  $1 - (1 - p_B)^n$ , where  $p_B$  is the giving rate and  $n = 7$ . In Control it is equal to the giving rate. In Bystander the recipient's expected earnings are computed as  $[1 - (1 - p_B)^n] \bar{T}_B^+$  where  $\bar{T}_B^+$  is the average amount given, conditional on giving. In Control the recipient's expected earnings are equal to the average amount given.

The proportion of dictators who gave a positive amount to the recipient, i.e., the giving rate, is lower in Bystander (33%) than in Control (44%). This difference is marginally significant at the 10% level ( $\chi^2(1) = 3.27$ ,  $p = 0.07$ ).<sup>17</sup> This result is in line with the findings from the previous literature on the bystander effect and confirms that the presence of bystanders reduces each individual's willingness to give. The average amount given is also lower in Bystander (0.90) than in Control (1.44), and the difference is significant at the 5% level (Fisher's randomization test,  $p = 0.02$ ).<sup>18</sup>

To investigate the effect of the presence of bystanders on the recipient's welfare, we start by comparing the probability that the recipient receives positive earnings in the two treatments. This equals the probability that a randomly

17. The tests reported in the paper are two-sided and treat data from each subject as an independent observation, unless indicated otherwise. Thus, in the main experiment the tests are based on 126 observations per treatment.

18. See Moir [1998] for a discussion of the randomization test.

selected dictator chooses to give in the Control treatment, and the probability that at least one out of seven dictators give in the Bystander treatment. Assuming that the giving rate is an estimate of an individual's propensity to give, these two probabilities can be derived from the giving rates observed in the experiment, as discussed in the previous section. The probability of receiving positive earnings is much higher in the Bystander treatment (94%) compared to the Control treatment (44%). We can also consider the realised randomization of dictators into different groups and compute the probability of positive recipient earnings in each group, given the choices of the seven dictators in that group. We confirm a similar difference across treatments: the probability for the recipient to receive positive earnings is significantly higher in Bystander compared to Control (89% vs. 44%, Fisher's randomization test,  $p < 0.01$ ).<sup>19</sup> Thus, the positive effect of the increase in the number of individuals who can give outweighs the negative effect of the drop in the giving rate, leaving the recipient better off in terms of the probability of receiving positive earnings.

We next analyse the effect of treatment on the recipient's expected earnings. These depend on both the probability of receiving positive earnings and the average amount given, conditional on giving. We find that there is no significant difference in the average amount given conditional on giving between the Bystander treatment and Control treatment (2.71 vs 3.23, Fisher's randomization test,  $p = 0.14$ ).<sup>20</sup> This means that we find no significant evidence of a crowding out of the "quality" of giving in the presence of bystanders. The fact that the average amount given conditional on giving is not different across treatments, combined with the fact that the recipient is more likely to receive positive earnings in Bystander than in Control, implies that the recipient's expected earnings are in fact higher in the Bystander treatment relative to Control (2.55 vs. 1.44). To test the significance of this difference, we consider the realized randomization of dictators into different groups and compute the recipient's expected earnings in each group, given the choices of the seven dictators in that group. We find that the recipient's expected earnings are significantly higher in Bystander than in Control (2.42 vs. 1.44, Fisher's randomization test,  $p = 0.01$ ).<sup>21</sup>

## Beliefs and Norms across Treatments

In this section we present an exploratory analysis of the motivations underlying the behaviour observed in the main experiment. We are particularly interested in understanding whether the fall in giving rate and average amount given observed in the Bystander treatment are to be interpreted as due to an erosion of prosocial values and social norms. The fact that, as we have seen in the previous section, the recipient's welfare is actually higher in Bystander than Control suggests that this may not be the case. Our analysis of dictators' beliefs and norms reinforces this notion.

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19. This test treats each group as an independent observation and is thus based on 18 observations per treatment.

20. This test focuses only on dictators who gave a non-zero amount and is thus based on 42 observations in Bystander and 56 observations in Control.

21. This test treats each group as an independent observation and is thus based on 18 observations per treatment.

As discussed above, there are two mechanisms that could lead to a fall in the giving rate in the Bystander treatment. On the one hand, in the presence of bystanders dictators may be tempted to free-ride on others and thus reduce their own giving. This mechanism implies that dictators who do not give should hold high beliefs about others' giving, hence suggesting a negative correlation between dictators' beliefs and whether they choose to give or not in the Bystander treatment. However, the beliefs data show that this is not the case. Compared to dictators who give a positive amount, dictators who do not give believe that significantly fewer others in their group give and that the average amount given is significantly smaller (Fisher's randomization tests, both  $p < 0.01$ ).<sup>22</sup> More generally, a Tobit regression of own giving on a dictator's belief of the average amount given by others in their group shows a strong positive correlation between the two measures (coefficient 2.19,  $t = 5.04$ ,  $p < 0.01$ ).

The second mechanism proposes that the presence of bystanders triggers a diffusion of responsibility whereby giving little is more acceptable when one believes others in the group are also giving little, since the responsibility for the low earnings of the recipient can be shared with others. The positive correlation between dictator's own giving and beliefs of others' giving is consistent with this mechanism.<sup>23</sup> However, if reduced giving indeed stems from a perception that it is more socially acceptable to avoid giving in the presence of other, then one would also expect to observe a shift in the social appropriateness of giving low amounts in the Bystander treatment compared to Control. The data collected in the norms elicitation experiment do not support this.

Figure 2 shows, for each treatment, the average social appropriateness ratings elicited in the norm elicitation experiment for each of the actions available to the dictator.<sup>24</sup> In both treatments, transferring nothing to the recipient was viewed as the least socially appropriate action. The level of social appropriateness gradually increases with the amount given by the dictator, up to the action of transferring £5 (which represents an equal split between the dictator and the recipient) and decreases slightly thereafter. This general pattern of appropriateness ratings closely resembles that typically obtained in standard two-player dictator games (e.g., Krupka and Weber [2013]; Erkut, Nosenzo and Sefton [2015]).

Importantly, we find only small differences in the way the actions are rated across the two treatments. In fact, a series of Fisher's randomization tests performed for each of the 11 possible dictator's actions reveals no significant differences between appropriateness ratings in the Bystander and Control treatments

22. We can reconstruct a belief of the amount given by others for almost all dictators in the sample. Five dictators believed that one other dictator gave £6 or more to the recipient. For these dictators, we do not know the exact amount that they believed others transferred to the recipient. For simplicity, we arbitrarily set this value equal to £6. The results are unchanged if we instead drop these five dictators from the analysis.

23. This positive correlation is also consistent with a "false consensus effect" whereby individuals report beliefs that others act in similar ways as themselves (Ross, Greene and House [1977]; Engelmann and Strobel [2000], [2012]).

24. Following Krupka and Weber [2013], we assigned evenly-spaced values of  $-1$  for the rating "very socially inappropriate,"  $-0.6$  for the rating "socially inappropriate,"  $-0.2$  for the rating "somewhat socially inappropriate,"  $0.2$  for the rating "somewhat socially appropriate,"  $0.6$  for the rating "socially appropriate," and  $1$  for the rating "very socially appropriate." We report the full distributions of appropriateness ratings in the Online Appendix III.

(in all cases the  $p$ -values exceed the 10% threshold).<sup>25</sup> Thus, giving little is not viewed as more appropriate in the presence of bystanders, as a diffusion of responsibility mechanism would suggest. More generally, we do not find evidence in favour of the conjecture proposed by Darley and Latané [1968] that social norms are more accepting towards the action of not giving when individuals are in the presence of bystanders.

Figure 2. Social appropriateness ratings, by treatment



## CONCLUSIONS

We designed a dictator game experiment to re-examine the interpretation of the “bystander effect” made in the previous literature. The bystander effect occurs when an individual witnessing a person in need of help is less likely to offer help when other individuals, who can also offer help, are present. At the heart of the bystander effects lies a strategic interdependence between the actions of the individuals who can potentially offer help, whereby it is sufficient that one person volunteers to help in order for the victim to receive help. This introduces two possible mechanisms that may lead to reduced willingness to help. On the one hand, individuals may be tempted to free-ride on others’ help and so abstain from helping when there are others who can help instead. On the other hand, the responsibility to help may be “diffused” between several bystanders so that everyone feels less responsible if the recipient receives no help. Both mechanisms point towards an erosion of prosocial values and norms as the cause for the bystander effect observed in previous studies.

In line with the previous literature, in our modified dictator game we observe a drop in the individual willingness to give to a recipient when there is strategic

25. These tests are based on 62 observations in Bystander and 60 observations in Control. We use a Bonferroni correction to account for the fact that we are performing multiple tests of the same hypothesis. In fact, we cannot reject the null hypothesis of no differences in the ratings even without the Bonferroni correction (for all tests, the uncorrected  $p > 0.39$ ).

interdependence among dictators' actions relative to a control treatment where such interdependence is absent. However, we also show that this drop in the willingness to give does not lead to a reduction of the recipient's welfare. First, the probability that at least one dictator gives to the recipient is higher in the presence of bystanders than in the control treatment. Second, because the average amount given conditional on giving is not different between treatments, the recipient's expected earnings are actually higher when bystanders are present.

These findings call into question the relatively pessimistic interpretation of the bystander effect as being driven by an erosion of prosocial values and social norms: based on these measures of the recipient's welfare, subjects in the bystander treatment do not seem to behave any less prosocially than those in the control treatment. Our analysis of dictators' beliefs and of norms of giving supports this as we do not find evidence to corroborate either free-riding on others' provision of help or a diffusion of responsibility explanation for the reduction in giving observed in the experiment.<sup>26</sup>

Overall, our experiment shows that the presence of bystanders may, in some situations, benefit the person in need. This may be of particular relevance in situations where the required help could be provided by one person only and where individuals may differ in the quality of help that they are willing or able to provide. Examples of such situations include hospital appeals for organ donations and blood transfusions as well as requests made within organizations for one member to perform a certain task outside normal duties. In our study, we hold constant the number of individuals present and let dictators make their choices simultaneously to control for other potential effects from the presence of bystanders that may occur from the mere presence of a higher number of individuals. A potential area for future research could be to investigate how prosocial behaviour and the expected outcome for the recipient are sensitive to variations in the sheer number of bystanders.<sup>27</sup>

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26. An important qualification of our conclusions, however, is that our experiment is designed to isolate the bystander effect stemming from the existence of a strategic interdependence between the bystanders' actions. Our experiment is not well-suited to study any effect that may arise due to the mere presence of other decision-makers.

27. For a first step in this direction, see Campos-Mercade [2017].

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