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The bystander effect in an *N*-person dictator game

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ABSTRACT

Dozens of studies show that bystanders are less likely to help victims as bystander number increases. However, these studies model one particular situation, in which victims need only one helper. Using a multi-player dictator game, we study a different but common situation, in which a recipient's welfare increases with the amount of help, and donors can share the burden of helping. We find that dictators transfer less when there are more dictators, and recipients earn less when there are multiple dictators. This effect persisted despite mechanisms eliminating uncertainty about other dictators' behavior (a strategy method and communication). In a typical public goods game, people seem to transform the situation into an assurance game, willing to contribute if certain others will too. Despite similarities, people do not treat a recipient's welfare like a public good. Instead, people seem to transform the situation into a prisoner's dilemma, refusing to help whatever others do.

Introduction

The bystander effect

In the wake of Catherine Genovese's tragic murder in New York in 1964, the *bystander effect* became a textbook example of how group settings alter individual behavior (Darley & Latané, 1968; Latané & Darley, 1968). Initial reports claimed as many as 38 people witnessed the attack and did nothing—not even call the police. Reanalyzing the case, Manning, Levine, and Collins (2007) questioned some of the reported details, including the actual number of bystanders and whether any of them called the police. Whatever the details of that case, dozens of subsequent experiments have found that a bystander is less likely to help a victim when others are present (reviewed in Fischer et al., 2011; Latané & Nida, 1981). And as a consequence, victims may be worse off when more bystanders are present.

In one of the first experimental demonstrations of the bystander effect, participants thought they were engaged in a conversation over an intercom with one, three, or five other students about problems associated with college life (Darley & Latané, 1968). The other participants were, in fact, confederates. During the conversation, one of the confederates went into a seizure. Participants took longer and were less likely to respond in the three- and five-person conditions than in the one-person condition. In another study, participants witnessed smoke entering the room through a wall vent (Latané & Darley, 1968). Participants were alone in the room, with two other naïve participants, or with two confederates who were instructed to do nothing. Lone participants reported the fire more often than participants in either of the group conditions. Subsequent studies of the bystander effect have been extended to nonemergency situations. For example, people in groups are less likely to report a malfunctioning camera being used in a scientific study (Misavage & Richardson, 1974), help pick up dropped pencils or coins (Latané & Dabbs, 1975), answer the door (Levy et al., 1972), and stop to help a stranded motorist (Hurley & Allen, 1974).

Although the bystander effect seems to be a robust and general phenomenon, the strength of the effect depends on the details of the experimental setting. The bystander effect is reduced when the victim is clearly in need of help and when other bystanders advocate helping or label the situation an emergency (Latané & Nida, 1981). Similarly, the presence of a perpetrator in critical situations also reduces the magnitude of the bystander effect (Fischer et al., 2011). Fischer et al. suggest that dangerous situations and emergencies are recognized faster and more clearly than nonemergencies, and bystanders are more likely to intervene whether or not other bystanders are present, thereby lessening the bystander effect. This leads to the somewhat counterintuitive finding that the bystander effect is stronger in nonemergency settings than in emergency settings (Fischer et al., 2011). The bystander effect is also stronger when bystander intervention involves financial or opportunity costs than when bystanders incur physical costs (Fischer et al., 2011).

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A different helping situation

Bystander intervention studies teach us about helping behavior in groups. But how general are the lessons? To our knowledge, all but one have been exemplars of just one kind of social dilemma, one in which a victim needs the help of only one bystander; additional interventions do not further help the victim. Many helping situations have a different payoff structure, one in which a recipient's welfare increases with the amount of help received. Wiesenthal, Austrom, and Silverman's (1983) study is the exception to this rule. They asked patrons in a pub to donate money to victims of an earthquake in Guatemala. They found that the average contribution decreased as the number of patrons increased. Here, we build on this finding with a series of studies conducted in a controlled laboratory setting. Unlike traditional bystander studies, help can be partitioned, each donor can offer a specific amount, and the sum of individual helping decisions determines the recipient's welfare.

The N-person dictator game

We designed a variant of the *dictator game* (Forsythe, Horowitz, Savin, & Sefton, 1994) to model situations in which recipients are better off with more help and there are multiple potential donors. In the standard dictator game, the experimenter endows one participant, the *dictator*, with a sum of money and endows the other participant, the *recipient*, with nothing. The dictator decides how much to keep and how much to transfer to the recipient. In anonymous, one-shot games with undergraduate participants, dictators transfer 20–30% of the endowment (Camerer, 2003; Engel, 2011). Transfers are often bimodally distributed: some dictators transfer close to half of the endowment, others nothing (Fehr & Schmidt, 1999).

We modified the dictator game to allow one, two, or three dictators to make allocations to a single recipient (for other studies with multiple dictators, see Cason & Mui, 1997; Dana, Weber, & Kuang, 2007; Dufwenberg & Muren, 2006; Luhan, Kocher, & Sutter, 2009; Ottone, 2008; we review these studies and compare them to ours in the 'General Discussion'). Each dictator decides how much of her endowment to transfer to the recipient, and the recipient goes home with the sum of the transfers. We endow dictators with $24 in the one dictator condition, $18 in the two dictators condition, and $16 in the three dictators condition. With these endowments, the average welfare in a group is $12, regardless of the group size. This average is the sum of dictators' endowments divided by group size (i.e., the number of dictators plus the recipient). We define an 'equal share' as the amount of money each dictator must transfer so that all group members go home with $12, corresponding to $12 with one dictator, $6 with two, and $4 with three.

We evaluate the effect of group size on helping in two ways. First, we compare the average dictator transfer as a fraction of the equal share across conditions. If there is a bystander effect, this fraction will decrease as group size increases. Second, we compare the average recipient payoff across conditions. In a typical bystander study, recipients could, in principle, be better off in groups, even with a bystander effect.[1] In the *N*-person dictator game, recipients necessarily earn less money if dictators transfer a smaller fraction of the equal share in groups.[2]

We measure help with the amount of money dictators transfer to recipients. We realize that the decision to transfer money in a laboratory study may be psychologically distinct from deciding whether to intervene in real-world emergencies. We offer several justifications for our experimental approach. First, as we noted above, the bystander effect is not limited to emergency situations (Hurley & Allen, 1974; Latané & Dabbs, 1975; Levy et al., 1972; Misavage & Richardson, 1974). In fact, the effect seems to be stronger in nonemergency situations (Fischer et al., 2011). Second, the amount of money at stake does not seem to affect behavior in relatively simple and easy-to-understand economic games, like the dictator game (Camerer, 2003; Carpenter, Verhoogen, & Burks, 2005; Engel, 2011; Forsythe et al., 1994; List & Cherry, 2008). Third, economic games have been a tool of choice in the study of social dilemmas (e.g., Dawes, McTavish, & Shaklee, 1977; Kerr, Garst, Lweandowski, & Harris, 1997; Orbell, van de Kragt, & Dawes, 1988; Sally, 1995). Moreover, our method offers advantages over traditional helping studies. Using money as a proxy for help allows us to precisely quantify effects and test psychological and behavioral models. And, there is no need to deceive participants, a merit for some (e.g., Hertwig & Ortmann, 2008), but not for others (e.g., Cook & Yamagishi, 2008).

Theoretical foundations

Models of rational choice theory can account for the bystander effect (Harrington, 2001; for related models, see Diekmann, 1985; Krueger & Massey, 2009). In the typical experiment, each bystander has the option to help or to not help, and the victim needs the help of only one bystander. The payoff structure is a volunteer's dilemma (Diekmann, 1985) if we assume that bystanders (1) prefer that victims are helped, and would help if they believe no one else will, but (2) bystanders prefer that others provide the help, and will not help if they believe that someone else will. If we assume all bystanders share these preferences, and the interaction is one-shot without the possibility to communicate, the game-theoretic equilibrium predicts that a particular bystander's probability of helping decreases, and that a victim is less likely to receive help, as the number of bystanders increases (Harrington, 2001). This 'rational' explanation of the bystander effect does not, of course, justify it.

But we are interested in a different situation, one in which the decision lies on a continuum between helping a lot and not helping at all, and the welfare of a recipient increases with the amount of help received. A lone dictator (or bystander) weighs the utility she gains from keeping money for herself and the utility she gains from transferring money to the recipient. When there are multiple dictators (or bystanders), the situation is more complicated. Now a dictator must weigh the utility of money for herself, the utility she gains from transferring money to the recipient, and strategic considerations about the behavior of other dictators. Although it is easy to assign monetary consequences to different choice outcomes in the *N*-person dictator game, it is not obvious how people will behave.

To predict behavior, we need to know how people psychologically transform these different outcomes into subjective utilities. Drawing from interdependence theory, what we need is the mapping from the *given decision matrix* (e.g., monetary payoffs) to the *effective decision matrix* (i.e., subjective utilities; Kelley & Thibaut, 1978; see also McClintock & Liebrand, 1988; Van Lange & Joireman, 2008). We consider three psychological transformations of the given decision matrix: into a volunteer's dilemma, into an assurance game, or into a prisoner's dilemma (for overviews of social

[1] To see this, suppose that lone bystanders help with 80% probability, whereas, in groups of two, each bystander helps with only 70% probability. With two bystanders, a victim receives help with 91% probability.

[2] To see this, suppose dictators transfer 50% of the equal share whether alone or paired with another dictator. In both cases, recipients would earn $6. However, if lone dictators transfer 50% of the equal share, and dictators in groups of two transfer only 25%, recipients would earn $6 when matched with one dictator and $3 when matched with two.

dilemmas, see Dawes, 1980; Kollock, 1998; Liebrand, 1983; Messick & Brewer, 1983).[3]

A participant transforms the *N*-person dictator game into a volunteer's dilemma if the utility he derives from the recipient earning more declines with how much the recipient already has. A dictator will transfer more when he believes others will transfer less, and less when he believes others will transfer more, resulting in a negative correlation between behavior and belief. This transformation is consistent with the social motive of altruism, in which utility derives directly from the recipient's welfare (Becker, 1974; MacCrimmon & Messick, 1976).

Alternatively, a participant may transform the situation into an assurance game. This might be because the *N*-person dictator game shares similarities with the public goods game. Contributing to a public good and helping someone in need are both materially costly and generate benefits for all group members (assuming others value the public good and the recipient's welfare). Multiple studies suggest that people transform public goods into games of assurance, and are willing to contribute if they are certain others will contribute too (Croson, Fatas, & Neugebauer, 2005; Fischbacher, Gächter, & Fehr, 2001; Frey & Meier, 2004; Kurzban & Houser, 2005; Shang & Croson, 2009). The assurance game transformation applies if a dictator derives utility from being part of a group that helps a recipient, but derives disutility when he is the only one helping. If so, a dictator will match the transfers of others, resulting in a positive correlation between behavior and belief. This transformation is consistent with the social motive of reciprocity (Charness & Rabin, 2002; Falk & Fischbacher, 2006; Rabin, 1993; Sugden, 1984).

Finally, a participant transforms the situation into a prisoner's dilemma if his disutility of transferring money always trumps the utility derived from the recipient earning a bit more. This predicts that a dictator will not transfer money to the recipient, regardless of what he believes others will transfer. This transformation is consistent with self-interest.

Outline of the paper

In Study 1, we vary the number of dictators simultaneously transferring to a single recipient. In Study 2, we elicit fully specified transfer strategies: dictators make a binding transfer decision for every possible transfer amount the other dictator could have made. This method eliminates any uncertainty a dictator may have about how much her fellow dictator's transfer. And, in Study 3, we allow dictators to communicate using text messages to arrive at mutually agreed upon and binding transfers. In the 'General Discussion', we summarize the results, discuss them in light of theory, and outline follow-up studies.

Study 1: the *N*-person dictator game

The goal is to determine whether the bystander effect extends to helping situations in which the recipient's welfare increases with the amount of help received. We conducted a modified dictator game, varying the number of dictators simultaneously allocating money to a single recipient. A bystander effect implies that the average dictator transfer, as a fraction of the equal share, declines with the number of dictators.

Methods

Participants

One hundred and ninety-eight undergraduates participated in Study 1 (M_{age} = 20.4 years, *SD* = 3.1, range: 17–43; 56% females). We paid participants a $5 show-up payment and an average of $12 for the dictator game.

Design

We varied the number of dictators simultaneously transferring to a single recipient in a between-participants design. All participants in a session were assigned to the same condition. In the one dictator condition (1D), we grouped participants into pairs composed of one dictator endowed with $24, and one recipient endowed with nothing. In the two dictators condition (2D), we grouped participants into triads composed of two dictators each endowed with $18, and one recipient endowed with nothing. In the three dictators condition (3D), we grouped participants into quartets composed of three dictators each endowed with $16, and a recipient endowed with nothing. In the 2D and 3D, dictators made simultaneous transfers, ignorant of each other's choices. Furthermore, a dictator never learned how much the other dictator(s) transferred. In the 2D and 3D, after making decisions, dictators guessed how much money other dictators transferred.

Procedure

The study was conducted in the California Social Science Experimental Laboratory (http://www.cassel.ucla.edu/) over the course of 13 sessions, ranging in size from 8 to 20 participants. We collected data from 22 groups of participants for each condition. Participants were seated at computer terminals and separated by partitions. We informed participants that their decisions would remain anonymous[4]: neither experimenters nor other participants could associate decisions with specific individuals. There was no deception.

After instruction,[5] we randomly assigned participants to a group and to a role (dictator or recipient). During the experiments, we used the label 'allocator' instead of 'dictator'. While dictators made their transfer decisions, recipients predicted how much money they would receive. To incentivize these predictions, we paid recipients a $3 bonus if correct. Dictators were informed that recipients could earn this bonus. After the game, participants indicated their age and sex, and answered questions about the study.

Beta-binomial distribution

Because dictator transfer distributions were U- and J-shaped, ANOVA is not appropriate to test the effect of condition on transfers. Instead, we fit beta-binomial distributions to the data.[6] The beta-binomial distribution, a family of discrete probability distributions defined on a fixed interval, results from compounding a binomial and beta distribution. Like the normal distribution, the beta-binomial distribution is shaped by two parameters, denoted by α and β. Unlike the normal distribution, the beta-binomial can assume many shapes, such as a uniform distribution (when $\alpha = \beta = 1$), a U-shaped distribution (when $\alpha < 1$ and $\beta < 1$), a normal distribution

[3] We use Liebrand's (1983) definition of a social dilemma, characterized by the following two properties: (1) individuals have a strategy which yields them the best payoff in at least one configuration of strategy choices and that negatively impacts the payoffs of others, and (2) everyone choosing this particular strategy results in a deficient outcome. This definition includes the volunteer's dilemma (Diekmann, 1985; also known as the game of chicken, the hawk-dove game in Maynard Smith, 1982, and the snowdrift game in Sugden, 1986), and the assurance game (Sen, 1967; also known as the trust game in Liebrand, 1983, and the stag hunt game in Skyrms, 2004).

[4] The goal of ensuring anonymity was to minimize reputational concerns, which seem to increase potential helpers' public self-awareness and affect helping behavior in bystander interventions (Van Bommel, Van Prooijen, Elffers, & Van Lange, in press).

[5] The oral instructions for all studies can be found in the Supplementary material.

[6] To do this, we used Benjamin Bolker's 'bbmle' package within the R platform (2008, R Development Core Team, The R Foundation for Statistical Computing, Vienna, Austria).

(when $\alpha > 1$ and $\beta > 1$), and a J-shaped distribution that is strictly decreasing (when $\alpha < 1$ and $\beta > 1$) or increasing (when $\alpha > 1$ and $\beta < 1$). These two parameters, α and β, can be thought of as weights given to either end of the distribution.[7]

We use a parameterization of the beta-binomial in which one parameter (p) measures the mean and the other (Θ) measures the over-dispersion. These parameters are computed as follows: $p = \alpha/(\alpha + \beta)$ and $\Theta = \alpha + \beta$. Θ is referred to as the over-dispersion in reference to the binomial distribution. As Θ becomes larger, the beta-binomial takes on a binomial distribution shape. As Θ becomes smaller, the dispersion increases, and the distribution assumes a U-shape.

Because the beta-binomial is defined on a fixed interval, we had to choose lower and upper bounds to model dictator transfers. Transferring zero is the natural lower bound. We set the upper bound to the equal share for each condition ($12 in the 1D, $6 in the 2D, and $4 in the 3D). More-than-equal share transfers were reset to the appropriate equal share. Only three out of 132 dictator transfers (2%) were above the threshold. The results presented below do not change when these transfers are excluded.

Model selection

We used a maximum likelihood model selection approach (Burnham & Anderson, 2002, 2004; Burnham, Anderson, & Huyvaert, 2011) to compare hypotheses about dictator transfers and to estimate model parameters (i.e., the mean and the over-dispersion).[8] We used the Akaike information criterion corrected for small sample size (AIC_c) to compare the models. This approach credits models for how well they fit the data and penalizes them for the number of parameters they use to obtain that fit, taking into account the number of observations. The best approximating model is the one with the smallest AIC_c score. We also calculated the Akaike weight for each model, ranging from zero to one, which represents the probability that a given model is the best approximating model in the set. These weights can be interpreted as degrees of evidentiary support for each model.

To test for the bystander effect, we evaluate hypotheses about differences in the mean dictator transfer across conditions. Because these hypotheses concern average transfers, we fit one over-dispersion parameter across all conditions (i.e., $\Theta_{1D} = \Theta_{2D} = \Theta_{3D}$). We consider the following four hypotheses:

1. Dictator number has no effect on the mean transfer (p: 1D = 2D = 3D). This can be considered the 'null' hypothesis.
2. The mean transfer is different when there is one dictator than when there are multiple dictators (p: 1D $\neq$ 2D = 3D).
3. The mean transfer is different when there are one or two dictators than when there are three (p: 1D = 2D $\neq$ 3D).
4. The mean transfer is different for each dictator number (p: 1D $\neq$ 2D $\neq$ 3D).

Bootstrap simulation

We used a bootstrap simulation to compute confidence intervals for recipient payoffs across conditions (Efron & Tibshirani, 1993). For the 1D, we sampled 22 dictator transfers with replacement, computed the sample mean, and repeated the procedure 10,000 times. For the 2D and 3D, we followed a similar procedure, sampling 44 and 66 dictator transfers with replacement, adding up two and three of these transfers to create 22 recipient payoffs, computing the mean, and repeating the procedure 10,000 times. We recorded the mean and the 'bootstrap percentile confidence interval' (i.e., the 2.5th and 97.5th percentiles of these bootstrapped distributions; Carpenter & Bithell, 2000).

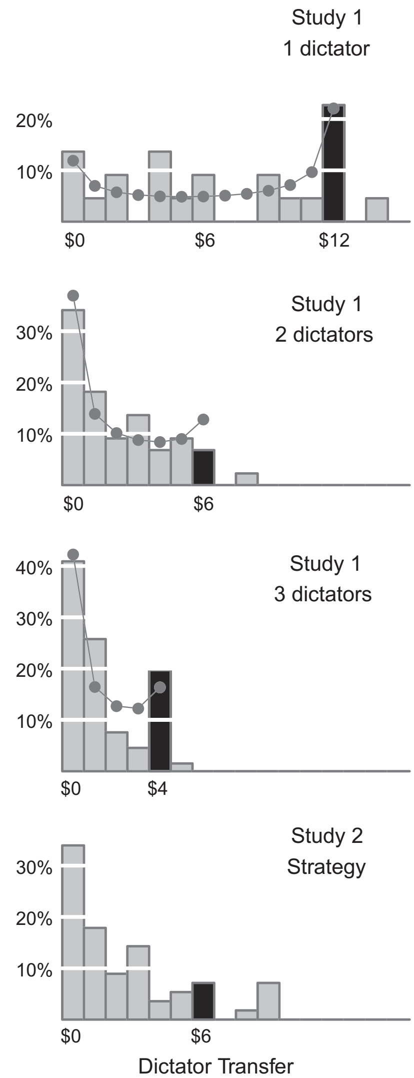

Fig. 1. Dictator transfers in Studies 1 and 2. The dark bars represent the equal shares, the amount of money each dictator needs to transfer so that all group members go home with $12. The dotted lines in the top three panels represent fitted values from the best model in Table 2.

Results

Dictator transfers

The average transfer was $6.68 ($SD$ = $4.73, Coefficient of Variation[9] (CV) = 71%) in the 1D,[10] and declined to $2.09 ($SD$ = $2.19, CV = 105%) in the 2D and $1.41 ($SD$ = $1.60, CV = 113%) in the 3D (Fig. 1).

In the model selection, hypotheses 2 and 4, which separate the mean transfer in the 1D from the 2D and 3D, collectively garner 86% of the weight, providing strong support for the hypothesis that the mean transfer in the 1D is different from the mean transfers in

[7] Bolker (2008) describes the beta-binomial distribution and uses it to model ecological data. Smithson and Verkuilen (2006; see also, Cribari-Neto & Zeileis, 2010) describe how to use the beta distribution for analyzing psychological data.

[8] We used Benjamin Bolker's 'bbmle' package within the R platform.

[9] The coefficient of variation (CV) is computed by dividing the standard deviation by the mean, resulting in a dimensionless number useful for comparing variation across groups when the means are different.

[10] In a standard dictator game, using a similar procedure in the same laboratory, Haley and Fessler (2005) found an average transfer similar to the 1D in this study, corroborating our results. We mention this because the 1D will serve as a comparison benchmark for Studies 2 and 3.

Table 1
Models explaining dictator transfers in Study 1.

Model[a]	Mean[b]	Parameters[c]	AIC_c[d]	Weight[e] (%)
2	1D ≠ 2D = 3D	3	462.5	64
4	1D ≠ 2D ≠ 3D	4	464.6	22
1	1D = 2D = 3D	2	466.4	9
3	1D = 2D ≠ 3D	3	467.6	5

[a] Model numbers match the numbers from the methods section of Study 1.
[b] Models are defined in terms of how means were fit to the different conditions.
[c] Number of model parameters.
[d] Akaike information criterions corrected for small sample size, AIC_c, associated with each model.
[e] AIC_c weights associated with each model.

the multiple dictator conditions (Table 1). Model 2, which combines 2D and 3D, is three times as likely as Model 4, which separates them (i.e., 64% vs. 22%).

A strong bystander effect emerges in Model 2, which separates the mean of the 1D from the 2D and 3D (Table 2). In the 1D, the estimated average transfer is 57% of the $12 equal share ($6.84). In the 2D and 3D, the estimated averages are 36% of the equal shares ($2.16 and $1.44).

Dictator beliefs about other dictators' transfers

Transfer amounts positively correlated with beliefs about how much other dictators would transfer (2D: $r = 0.62$, 95% CI [0.40, 0.78]; 3D: $r = 0.74$, 95% CI [0.61, 0.84]). The most common set of responses was to transfer nothing and predict that the other dictator(s) would transfer nothing too.

Recipient payoffs

Excluding the bonus for correctly guessing the transfer amount, recipients earned about $2.50 less in 2D and 3D than in the 1D (Fig. 2). The average recipient payoff was $6.68 (*SD* = $4.73, *CV* = 71%) in the 1D, $4.18 (*SD* = $2.53, *CV* = 61%) in the 2D, and $4.23 (*SD* = $2.88, *CV* = 68%) in the 3D.

The bootstrapped mean payoff for recipients in the 1D is outside of the confidence intervals for recipient payoffs in both the 2D and 3D; and the bootstrapped mean payoffs for recipients in both the 2D and 3D are outside of the confidence interval for recipient payoffs in the 1D (Table 3).

Recipient predictions

Recipients did not anticipate a bystander effect, believing they would receive about the same amount no matter how many dictators transferred to them (Fig. 2). The average prediction was $4.86 (*SD* = $4.71, *CV* = 97%) in the 1D, $4.04 (*SD* = $5.37, *CV* = 133%) in the 2D, and $4.54 (*SD* = $4.21, *CV* = 93%) in the 3D. Across conditions, most recipients predicted they would receive nothing. Only four out of 66 correctly predicted how much they would receive.

Table 2
Parameter estimates for dictator transfers in Study 1.

Model[a]	Mean (95% CI)[b]			Over-dispersion (95% CI)[c]
	1D	2D	3D	1D, 2D, 3D
2	.57 (.42,.71)	.36 (.20,.53)	.36 (.20,.53)	.98 (.69,1.40)
4	.57 (.42,.71)	.35 (.18,.54)	.36 (.19,.54)	.98 (.69,1.40)
1	.40 (.33,.46)	.40 (.33,.46)	.40 (.33,.46)	.92 (.65,1.31)
3	.43 (.34,.52)	.43 (.34,.52)	.36 (.24,.49)	.93 (.66,1.32)

[a] Model numbers correspond to Table 1.
[b] Estimated means and 95% confidence intervals for the different conditions for each model. Means are presented as fractions of the equal shares, which are $12, $6, and $4 for the three conditions.
[c] Estimated over-dispersions and 95% confidence intervals for each model.

Discussion

We modeled a helping situation in which a recipient's welfare increases with the amount of help received using an *N*-person dictator game, varying the number of dictators transferring money to a single recipient. We found strong evidence for a bystander effect: lone dictators transferred 57% of the equal share; dictators in groups transferred only 36%. As a result, recipients earned 50% more in the 1D than in the 2D and 3D.

Our results match the findings of Wiesenthal et al. (1983) who solicited contributions to help earthquake victims from pub patrons in groups of varying sizes: there was a decrease in the average contribution when comparing one donor to two, but not between two and three. These findings contrast with the pattern in typical bystander studies in which recipients (or victims) need the help of only one bystander. In a meta-analysis of bystander studies, Fischer et al. (2011) found no significant bystander effect when comparing situations with one bystander to situations with two bystanders. The bystander effect only emerged when comparing one bystander with more than two bystanders. However, as we suggested in the introduction, the situation modeled by the *N*-person dictator game is not the same as traditional bystander studies. In the Supplementary material, we extend Fehr and Schmidt's (1999) inequality aversion model to the *N*-person dictator game and show that the bystander effect is expected to become weaker with increasing bystander number.

To infer how participants transformed the situation, we asked dictators to predict other dictators' transfers. Transfers positively correlated with beliefs, consistent with an assurance game transformation. In an assurance game, people prefer that everyone cooperate (the utility-dominant equilibrium). But with simultaneous decisions, the risk that others might not cooperate can lead players to defect (the risk-dominant equilibrium). Notably, the most common set of responses was to transfer nothing and believe the other dictator(s) would transfer nothing too. However, a prisoner's dilemma transformation is also consistent with this response.

There are two problems with inferring the transformation process from the design of Study 1. First, dictators transferred before predicting others' transfers, and so it is possible that these beliefs rationalized, rather than motivated, behaviors. Second, the transformation from a given matrix to an effective one is a psychological process within an individual. In this study, however, we measured only one behavior and one belief for each participant, and so we should be cautious drawing inferences about intra-individual processes (Molenaar & Campbell, 2009). To address these limitations we conducted additional studies.

Study 2: the *N*-person strategy method dictator game

In Study 1, participants seemed to transform the *N*-person dictator game into either an assurance game, willing to transfer the equal share in group settings only if they believed others would do the same, or a prisoner's dilemma, transferring nothing regardless of the other dictators' transfers. In an assurance game, cooperation requires mutual trust. Thus, uncertainty about other dictators' transfers might have contributed to the decline in transfers in the multiple dictator conditions.

We use a strategy method in Study 2 to eliminate uncertainty. We allow participants to specify a transfer amount for each transfer amount the other dictator can choose. This means that dictators face no risk of cooperating when others defect. If people transform

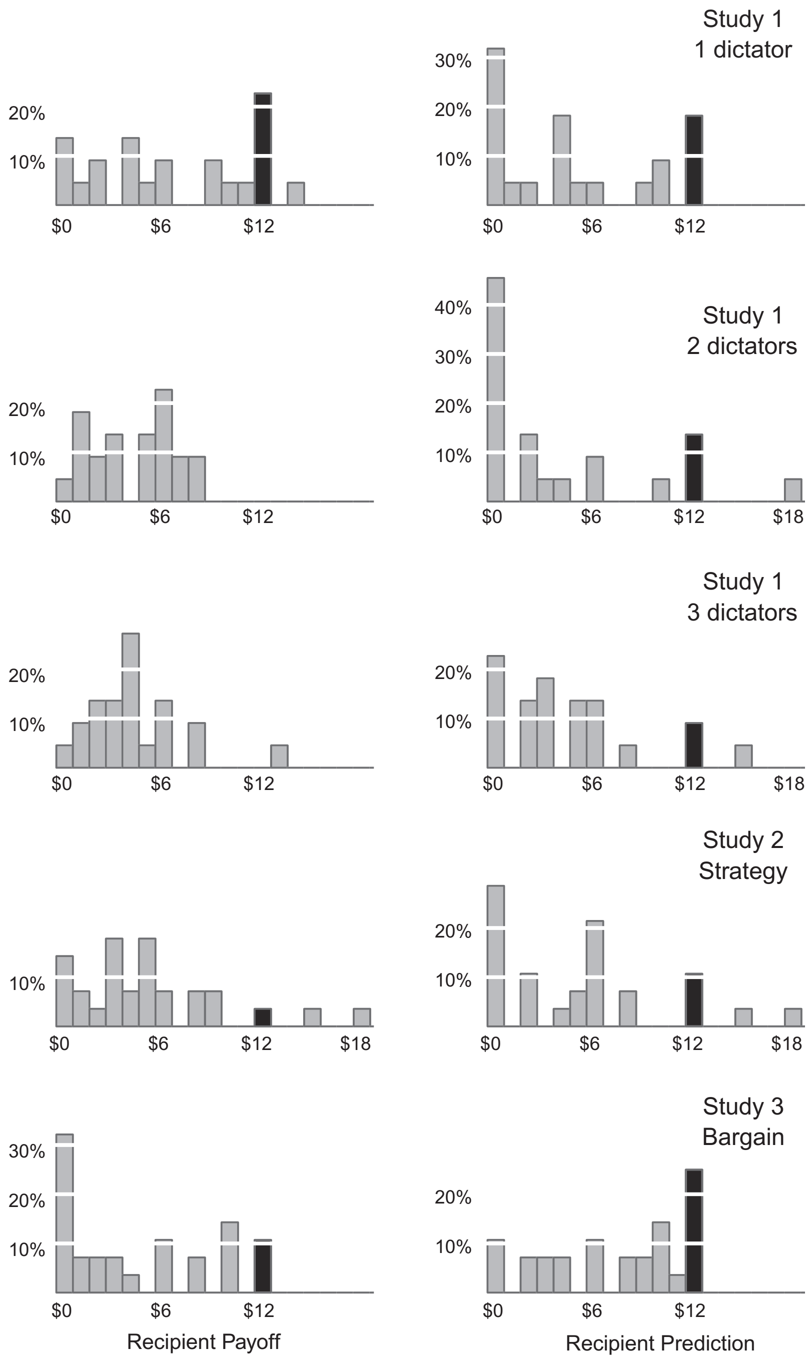

Fig. 2. Recipient payoffs and predictions for Studies 1–3. The dark bars correspond to an even split. One recipient predicted $36 in the strategy method of Study 2. This prediction is not depicted.

Table 3
Recipient payoffs computed by bootstrap simulation.

Condition	Study	Sample size	Simulated mean payoff ($)	95% CI ($)	Coefficient of variation (%)
1 Dictator	1	22	6.68	4.73,8.64	71
2 Dictators	1	22	4.18	2.91,5.50	73
3 Dictators	1	22	4.22	3.14,5.36	65
Strategy	2	28	4.81	3.39,6.39	85
Bargaining	3	28	4.52	2.89,6.18	100

Note. The methods for computing these values are presented in the methods sections.

the situation into an assurance game, dictators should match their responses to the transfers of their partners. If, instead, they transform the situation into a volunteer's dilemma, dictators will transfer more when their partners transfer less, and less when their partners transfer more. And if they transform it into a prisoner's dilemma, dictators will transfer nothing, regardless of how much their partners transfer.

Methods

Participants

Eighty-four undergraduates participated in Study 2 (M_{age} = 20.1 years, *SD* = 1.8, range: 18–25; 54% females). We paid participants a $5 show-up payment and an average of $12 for the dictator game.

Design

We grouped participants into triads composed of two dictators and one recipient. We endowed dictators with $18 and recipients with nothing. To implement the strategy method, we randomly assigned dictators to one of two roles. *Proposers* chose how much of their endowment to transfer to the recipient. *Responders* chose a transfer amount for each possible proposer transfer amount. So, a responder chose how much to transfer if the proposer transferred $0, $1, ..., $18. We paid recipients the proposal amount and the corresponding response amount.

In the strategy method, responders made contingent choices for all possible proposals, thereby eliciting fully specified strategies. Dictators made choices both as proposers and as responders. Within each group, one dictator first indicated a proposal and then a set of responses; the other dictator first indicated responses and then a proposal. Afterward, we randomly selected one dictator in the group to be the proposer and the other to be the responder. We informed participants of this during the instruction phase. The *dual-role* method allowed us to gather more data per participant and to correlate proposals with response strategies. Reviewing dozens of studies, Brandts and Charness (2011) found that the strategy and dual-role methods usually result in the same pattern of responses as the one-decision, single-role method.

Procedure

The study was run using the same general procedures as detailed in Study 1. We conducted three sessions, two with 27 people and one with 30. We collected data from 28 triads.

Bootstrap simulation

To estimate the mean and confidence interval for recipient payoff, we used a bootstrap simulation similar to the one outlined in Study 1. We sampled 28 dictator proposals with replacement from the observed set of 56 proposals. For each of these 28 proposals, we sampled a corresponding response, randomly chosen from one of the 56 observed response schedules. We summed the sampled proposals and responses to generate a set of recipient payoffs, computed the mean, and then repeated the procedure 10,000 times. We recorded the 2.5th and 97.5th percentiles of the bootstrapped distribution.

Results

Dictator proposals

The average proposal was $2.41 ($SD$ = $2.75, CV = 114%), 40% of the equal share (Fig. 1). Dictators proposing first transferred an average of $2.03 ($SD$ = $2.97, CV = 146%). Dictators proposing after responding transferred an average of $2.79 ($SD$ = $2.50, CV = 90%).

Dictator responses

The average response across all participants and across the range of actual proposals ($0–$9) was $2.33 ($SD$ = $2.98, CV = 128%). There was little contingency in responses at the population level. For a $0 proposal, the average response was $2.48 ($SD$ = $3.46, CV = 140%); for a $6 proposal, the average response was $2.43 ($SD$ = $2.97, CV = 122%); and for a proposal of $9, the average response was $1.98 ($SD$ = $2.60, CV = 131%).

Although there was little contingency at the population level, there was considerable variation among individuals (Fig. 3). Most participants indicated a more or less linear response strategy, so we fit a line to each set of responses using least-squares regression and characterize each responder with two numbers: an intercept (i.e., how much a participant transferred if the other dictator transferred nothing) and a slope (i.e., how a participant's responses varied with the other dictator's transfers). We used these estimates to categorize participants into four response strategies. We categorized a participant as *self-regarding* if the absolute value of the estimated slope was less than 10% and the estimated intercept was less than $1; as a *fixed responder* if the absolute value of the estimated slope was less than 10% and the estimated intercept was greater than or equal to $1; as a *compensator* if the estimated slope was less than or equal to –10%; and as a *matcher* if the estimated slope was greater than or equal to 10%. This categorization scheme resulted in 21 self-regarding types (38%), 8 fixed responders (14%), 20 compensators (36%), and 7 matchers (13%).

Correspondence between dictator proposals and responses

Each participant made proposals and responded to possible proposals from the other dictator. There was a strong positive correlation between the amount participants proposed and the estimated intercept of their response strategies, which indicates how much they would transfer if the other dictator transferred nothing (r = 0.83, 95% CI [0.72, 0.90]). This means that participants proposing small amounts also responded with small amounts when their partners proposed nothing, and participants proposing large amounts also responded with large amounts when their partners proposed nothing. There was a weak negative correlation between proposal amount and estimated slope of the response strategy, which indicates how participants responded to others' transfers (r = –0.34, 95% CI [–0.55, –0.08]). Participants proposing small amounts tended to be matchers, whereas participants proposing large amounts tended to be compensators.

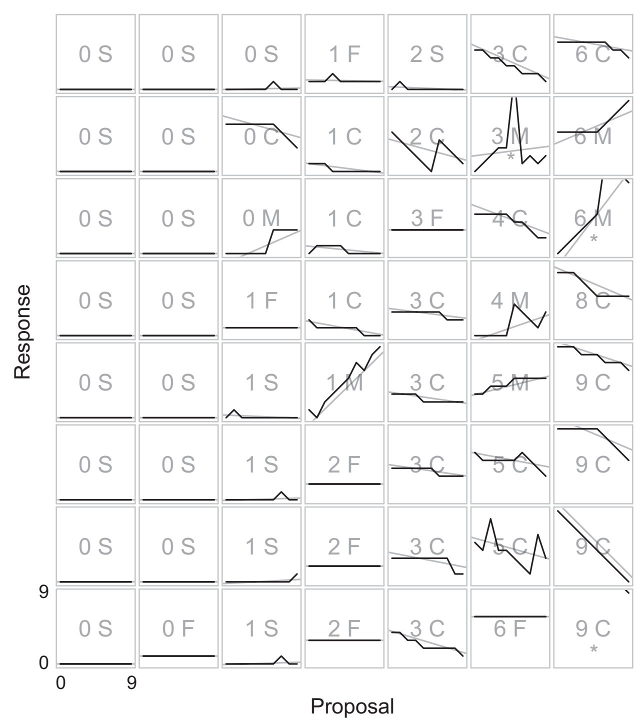

Fig. 3. Dictator response strategies in Study 2. Each panel shows the response strategy for one participant. The number in the middle of each panel represents the participant's proposal. The letter in the middle of each panel represents the responder type ('S' for 'self-regarding', 'F' for 'fixed responder', 'C' for 'compensator, and 'M' for 'matcher'; classification described in the text). The black line depicts the participant's responses. The other dictator's possible proposals are plotted on the horizontal axis, and the response amount for each proposal is plotted on the vertical axis. The gray lines depict ordinary least-squares regression lines. We marked three panels with asterisks. These participants responded with more than $9 for some proposals. The panels are sorted top-to-bottom, left-to-right first by proposal amount, then by mean response amount, and finally by standard deviation in response amount.

Recipient payoffs

Excluding the bonus for correctly guessing the transfer amount, recipients earned an average of $5.11 (*SD* = $4.41, *CV* = 86%). The bootstrapped mean recipient payoff is $4.81 (Table 3).[11]

Recipient predictions

Recipients predicted they would earn an average of $6.32 (*SD* = $7.65, *CV* = 121%; Fig. 2). Five of the 28 recipients guessed correctly, earning the bonus.

Discussion

The bystander effect observed in Study 1 might have been due to uncertainty about what others would do. If dictators transform the situation into an assurance game and fear that others will transfer nothing, they too will transfer nothing. This is consistent with the pattern of transfers and beliefs in Study 1. To evaluate this possibility, we used a strategy method so that responders faced no uncertainty.

Half of the dictators were non-contingent responders; and of these, two thirds transferred nothing to the recipient. Transferring nothing is consistent with a prisoner's dilemma transformation. Half of the dictators were weakly contingent responders; of these, two thirds were compensators (consistent with a volunteer's dilemma transformation) and one third were matchers (consistent with an assurance game transformation). These results suggest that the bystander effect documented in Study 1 was not due to uncertainty about other dictators' transfers. Instead, a substantial fraction of dictators seems to feel less concern for the recipient's welfare when there were other potential helpers, suggesting a prisoner's dilemma transformation.

It is possible that the strategy method did not fully achieve the intended purpose. In particular, the method's 'cold' nature may not have sufficiently activated social motives (e.g., specifying a full set of contingent responses may not be equivalent to real-world social action; Casari & Cason, 2009). Moreover, the strategy method eliminates uncertainty only for responders; proposing dictators do not know their partners' response strategies. To address this, we conducted a third study in which dictators can communicate with each other and arrive at mutually agreed-upon transfers.

Study 3: the *N*-person dictator game with communication

In this study, we allowed dictators to communicate with one another using text messages, and transfers were finalized only when both dictators were mutually satisfied with the transfer set. This requirement prevented a dictator from pledging one amount and then transferring a different amount. If uncertainty and mistrust cause the bystander effect in the *N*-person dictator game and the strategy method dampens social motives, the bargaining method should ameliorate the effect, increasing transfers.

Methods

Participants

Eighty-four undergraduates participated in Study 3 (M_{age} = 19.4 years, *SD* = 1.8, range: 18–28; 52% females). We paid participants a $5 show-up payment and an average of $12 for the dictator game.

Design

We grouped participants into triads composed of two dictators and one recipient. We endowed dictators with $18 and recipients with nothing. The two dictators could send each other text messages. Recipients were not privy to these conversations. During the chat, dictators made simultaneous transfer proposals. Each dictator then chose whether to accept or reject the set of transfers. If both accepted, the bargaining ended; if either rejected, the pair entered another round of bargaining.

After the game, we measured participants' *social value orientations* (MacCrimmon & Messick, 1976; McClintock, 1972; Messick & McClintock, 1968; Van Lange, 1999) using the instrument presented in Van Lange, Otten, Bruin, & Joireman, 1997.[12] We categorized participants based on their choices on a set of hypothetical resource divisions. For each decision, participants chose between an option resulting in an equal distribution ('prosocial'), an option garnering the highest payoff for the chooser ('individualistic'), and an option maximizing the difference between the chooser's payoff and the other person's payoff ('competitive'). Following Van Lange et al. (1997), we categorized participants as 'prosocial', 'individualist', or 'competitive' if six or more of their choices agreed. Otherwise, participants were unassigned. Following Van Dijk, De Cremer, and Handgraaf (2004), we then categorized individualists and competitors as 'proself' as both value their own outcome more than the outcomes of others.

Procedure

The study was run using the same general procedures as detailed in Study 1. We conducted three sessions, two with 27 people and one with 30. We collected data from 28 triads.

Bootstrap simulation

We used a bootstrap simulation to estimate the mean and confidence interval for recipient payoffs. Because dictators bargained to arrive at their transfer amounts, individual transfers are not independent. So we sampled 28 recipient payoffs from the observed payoffs with replacement, computed a mean, and repeated the procedure 10,000 times. We recorded the 2.5th and 97.5th percentiles of the bootstrapped distribution.

Results

Dictator proposals

In this study, the paired set of transfers resulting from bargaining is the unit of analysis. Dividing the sum of each pair by two, the average per-dictator transfer was $2.25 (*SD* = $2.25, *CV* = 100%).

Dictators coordinated their transfers, with 21 of 28 pairs bargaining to matched transfers (Fig. 4). Nine pairs transferred nothing to the recipient, and six transferred {$5,$5} or {$6,$6}. In the groups settling on {$5,$5}, several dictators described this as the best arrangement because if the recipient predicted that she would receive $10, all three group members would go home with $13.[13]

The social value orientations of dyads explained much of the variation in transfers. The average per-dictator transfer was $4.00 in the 10 pairs with two prosocial individuals (*SD* = $2.05, *CV* = 51%), $1.17 in the 12 pairs with one prosocial and one proself individual (*SD* = $1.30, *CV* = 112%), and $1.50 in the 6 pairs with two proself individuals (*SD* = $2.51, *CV* = 167%).

Recipient payoffs

Excluding the bonus for correctly guessing the transfer amount, recipients earned an average of $4.50 (*SD* = $4.49, *CV* = 100%). The

[11] If we exclude one outlier who responded in such a way as to guarantee the recipient $18 (i.e., prepared to transfer $18 if the proposer transferred $0, $17 if the proposer transferred $1, etc.), the average recipient payoff drops to $4.47.

[12] We only learned of this instrument after completing Study 2. In order to keep Study 3 as similar as possible to Studies 1 and 2, we administered the social value orientation instrument after the game.

[13] Four of 28 recipients predicted $10.

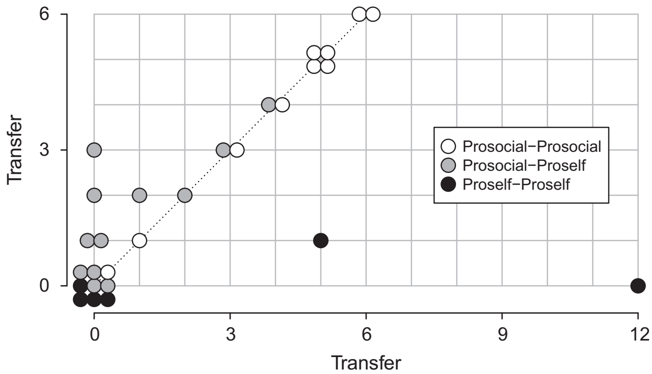

Fig. 4. Paired dictator transfers in Study 3. The transfer of one dictator is shown on one axis, and the transfer of the other dictator on the other axis. The dotted line represents symmetric transfers. The fill of the circles corresponds to different pairings based on categorizing participants according to social value orientation. In the five gray circles above the diagonal, representing prosocial–proself dyads with asymmetric transfers, the prosocial participant transferred more than the proself participant. In the proself–proself dyad off to the right, the dictator who transferred $12 was categorized as competitive.

bootstrapped mean recipient payoff is $4.51 (Table 3). The fate of any particular recipient was highly variable (*CV* = 97%).

Recipient predictions

Recipients predicted they would earn an average of $7.32 (*SD* = $4.23, *CV* = 58%; Fig. 2). Only two of the 28 recipients earned the bonus.

Bargaining

Twenty-one dictator pairs made only one proposal and accepted. The other seven groups settled within four rounds of proposals. In 22 groups, dictators used the messaging option to discuss transfers. In three of these groups, dictators rationalized small transfers, settling on {$1,$1}, {$2,$2}, and {$1,$0}. In three other groups, despite expressions of guilt about not transferring enough, dictators settled on {$0,$0}, {$1,$0}, and {$0,$0}. In six groups, dictators either sent no messages or sent a few messages unrelated to the transfers.

Discussion

This study was designed to eliminate uncertainty about what other dictators would do and to preserve social richness by allowing dictators to discuss their transfers. This difference was enough to change recipients' expectations about what dictators would do: recipients in this study predicted they would earn more than recipients did in Studies 1 and 2. However, communication among dictators did not actually help recipients; the bystander effect persisted. Recipient payoffs were far below the 1D amount in Study 1 and the same as the averages in the other multiple dictator conditions. Communication did have a strong coordinating effect on dictator behavior. The majority of dictator pairs transferred either nothing or equal shares. The social value orientations of dictators explained much of the variation in transfers. Recipients earned an average of $8 when they were the beneficiaries of two prosocials. When one or both dictators were proselfs, recipients earned less than $3.

It is not clear why communication had so little effect on dictator behavior. In standard bystander experiments, communication sometimes has the perverse effect of inhibiting help (Latané & Nida, 1981). On the other hand, communication fosters cooperation in public goods games (reviewed by Balliet, 2010; Sally, 1995). If the *N*-person dictator game is more like a public goods dilemma than a typical bystander intervention, why did communication not increase cooperation?

Messick and Brewer (1983; see also Dawes, van de Kragt, & Orbell, 1990; Dawes et al., 1977) suggest four reasons why communication might foster cooperation in social dilemmas: (1) group members gather information about others' choices; (2) group members make promises; (3) group members persuade others to 'do the right thing'; and (4) communication creates and/or reinforces group identity. In our study, texting allowed dictators to gather information about each other's choices and commitments were enforced because we required dictators to make mutually agreed upon transfers. These factors ought to increase levels of cooperation. At the same time, however, recipients were excluded from the discussion, and so dictators may have felt more comfortable making low transfers. Furthermore, the exclusion of recipients may have reinforced a sense of group identity among dictators, resulting in lower transfers (for a related result in a public goods setting, see Orbell et al., 1988).

General discussion

Bystander intervention studies have modeled helping situations in which a victim needs help from just one bystander (reviewed in Latané & Nida, 1981). There are, however, situations in which a victim's welfare increases with the amount of help received, and donors can share the cost of helping. We used an *N*-person dictator game to model these situations, varying the number of dictators transferring money to a single recipient. In Study 1, we found a strong bystander effect: lone dictators transferred much more than dictators in groups. In Study 2, we used a strategy method to eliminate uncertainty about other dictators' transfers, but the bystander effect persisted. In Study 3, dictators communicated and agreed on how much to transfer. The bystander effect persisted.

We interpret our results using interdependence theory (Kelley & Thibaut, 1978; see also McClintock & Liebrand, 1988; Van Lange & Joireman, 2008). This framework assumes people make decisions based on the effective matrix (i.e., the subjective utilities associated with different outcomes), not necessarily on the given decision matrix (e.g., the monetary payoffs associated with these outcomes). Dictators may transform the *N*-person dictator game into a volunteer's dilemma, helping only if they believe others will not; into an assurance game, helping so long as others help too; or into a prisoner's dilemma, refusing to help when there are others present.

The typical bystander intervention study seems well modeled as a volunteer's dilemma (Diekmann, 1985): people want to see victims helped, but prefer that someone else bears the cost. With these preferences, rational choice theory predicts a bystander effect (Diekmann, 1985; Harrington, 2001; Krueger & Massey, 2009). When alone, a bystander will help if the utility derived from helping outweighs the cost of helping. But with two bystanders, there are three possible outcomes: both bystanders help, one bystander helps, neither bystander helps. The bystander effect may be an efficient solution to this social dilemma: bystanders ensure the welfare of the victim without wasting effort. Psychologically, this outcome may result from a diffusion of responsibility: bystanders shift the responsibility of help onto others (Latané & Nida, 1981).

If most people transform the *N*-person dictator game into a volunteer's dilemma, a dictator should transfer more when she believes others will transfer less, and less when she believes others will transfer more. In contrast, we found a positive correlation between transfers and beliefs in Study 1, suggesting an assurance game transformation. This might make sense given the similarity between the public goods game and the *N*-person dictator game.

Contributing and helping are both materially costly and generate benefits for all group members (assuming others value the public good and the recipient's welfare). However, the modal response in the 2D and 3D was to transfer nothing and believe that others transferred nothing too. This is consistent with both an assurance game and a prisoner's dilemma. We cannot adjudicate between these possibilities. Behavior preceded belief elicitation, and so beliefs may have rationalized, rather than motivated, behaviors. And, we should be cautious drawing inferences about intra-individual transformations from inter-individual data (Molenaar & Campbell, 2009). Studies 2 and 3 addressed these limitations.

In Study 2, we used a strategy method to elicit dictators' response schedules. We categorized dictators into four types: self-regarding, compensators, matchers, and fixed responders. Roughly 40% were self-regarding, consistent with a prisoner's dilemma transformation. Compensators, consistent with a volunteer's dilemma, made up a similar fraction. And, about 13% were matchers, consistent with an assurance game. However, to be categorized as a compensator, a participant had only to decrease her transfer by 10 cents for every dollar increment from the other dictator. Had the criterion been 20 cents, we would have lost half the compensators. Had it been 50 cents, we would have lost 18 of the original 20. In public goods games, one in two participants are contingent cooperators (Croson et al., 2005; Fischbacher et al., 2001; Frey & Meier, 2004; Kurzban & Hauser, 2005; Shang & Croson, 2009). In our study, only one in eight participants were contingently cooperative (i.e., 'matchers'). Study 2 suggests that the prisoner's dilemma is the most common transformation.

In Study 3, dictators coordinated transfers through communication. Transforming the situation into either an assurance game or volunteer's dilemma would lead to equal distributions. With an assurance game, a dictator prefers to help so long as the other dictator helps too. Communication coupled with mutual commitments provides such dictators with a failsafe way of cooperating. With a volunteer's dilemma, each dictator prefers the other to help. But both cannot achieve this outcome. Since each wants to see the recipient helped, they will settle on each helping the same amount. Roughly one in five dictator pairs settled on a more-or-less equal distribution, while two in five settled on keeping all or nearly all of their money.

Across our studies, most participants seemed to transform the *N*-person dictator game into a prisoner's dilemma. In groups with multiple dictators, no matter how much others transferred, most dictators transferred little or nothing despite opportunities to coordinate. In contrast, the majority of lone dictators transferred positive amounts, with an equal share being the most common response. As a result, recipients fared much worse when matched with multiple dictators.

Inequality aversion and the bystander effect

Our results suggest that when there are multiple dictators, most people transform the situation into a prisoner's dilemma and transfer little. However, this does not explain why people behave so differently in the one dictator and multiple dictator conditions. An aversion to inequality may help to explain the results that we obtained in these experiments, and provide a more general explanation for the bystander effect in the *N*-person dictator game.

Over the last few decades, behavioral economists have departed from utility models grounded in narrow self-interest and incorporated social preferences. These models build on the observation that people value the welfare of others as well as their own welfare. The goal is to undergird economics with more realistic models of social psychology and thereby build better economic theory (for a criticism of this approach, see Berg & Gigerenzer, 2010).

Fehr and Schmidt's (1999) model of inequality aversion, one of the more influential social preference models, may shed light on our results. In this model, individuals compare their own payoffs with the payoffs of others, and are inequality averse if they are willing to give up some material payoff to produce more equal outcomes (for similar models, see Bolton & Ockenfels, 2000; Charness & Rabin, 2002; Loewenstein, Thompson, & Bazerman, 1989). Inequality aversion is modeled with two parameters, one measuring the disutility of having more than others and the other measuring the disutility of having less. For our purposes, these can be thought of as guilt and envy.

In the Supplementary material, we show that Fehr and Schmidt's (1999) model predicts a bystander effect in the *N*-person dictator game. Here, we provide an intuition as to why. Consider a sequential *N*-person dictator game in which each dictator commits in turn to a transfer amount. Suppose the last dictator in the sequence is certain that all the other dictators have pledged to transfer the equal share. What will she do? Because the other dictators transferred some of their money to the recipient, they have less than the focal dictator, and so the focal dictator feels only guilt, no envy. She has more money than the recipient and the other dictators. According to Fehr and Schmidt's model, the focal dictator will transfer the equal share only if her guilt parameter is sufficiently strong. Specifically, the parameter must be larger than $(d+1)/(d+2)$, where d refers to the number of other dictators in the group. This means that the amount of guilt the focal dictator must experience in order to motivate her to transfer the equal share increases with group size. A lone dictator transfers if her guilt parameter is larger than 1/2. If there is one other dictator, the guilt parameter must be larger than 2/3. As the number of other dictators becomes large, the requisite magnitude approaches one.

To understand why inequality aversion results in a bystander effect, consider two specific cases: one dictator matched with one recipient, and three dictators matched with one recipient. With one dictator, the initial payoff difference between dictator and recipient is large: the dictator has her whole endowment, while the recipient has nothing. With three dictators, if one dictator knows that the other two pledged to transfer the equal share (1/4 of the endowment), the payoff difference between the focal dictator and the others is not so great: The focal dictator has all of her endowment, while the other two dictators have 3/4 of their endowments and the recipient has 1/2 of the endowment. The smaller the payoff difference, the less guilt experienced. With inequality aversion, a little guilt goes a long way to motivating help when groups are small. In larger groups, guilt must be much stronger. And if there are individual differences in guilt, a smaller fraction of donors will help a recipient as the number of donors increases, even if they are certain that the other donors have pledged their support, resulting in the bystander effect. If dictators believe that others will not transfer the equal share, the bystander effect becomes more pronounced.

Similar studies

In this section, we discuss studies that share some similarity with our design. This will help to motivate our discussion of follow up studies.

Many-to-many helping

Cason and Mui (1997) studied a 'team dictator game'. Dictators made two allocation decisions. In the individual condition, dictators divided a \$5 sum with a recipient. In the team condition, two dictators divided a \$10 sum with two recipients, with dictators evenly splitting what they kept and recipients evenly splitting what they got. When dictators first made individual choices and then made team choices, the team offers shifted in the other-regarding direction, especially for teams that made more self-regarding individual transfers. Though the sample size was small and the effect modest, Cason and Mui's result is opposite to our results. This may have been due to a different design. In Cason and Mui's study, dictators were forced to evenly split what they kept. In our studies, each dictator had control over his or her endowment. And, in Cason and Mui's team condition, dictators discussed their choice face-to-face, away from everyone else. In our communication condition, discussion was through texting.

In Luhan et al. (2009), subjects transferred as individual dictators, then in groups of three, and then again as individuals. Across the three rounds, the average transfers were €0.94, €0.54, and €0.66 out of €5. In a control condition with dictators making three consecutive individual transfers, the averages were €1.27, €1.17, and €1.25. This pattern is consistent with our studies. Like us, Luhan et al. used texting rather than face-to-face chatting. Like Cason and Mui (1997), Luhan et al. had all team members go home with the same amount of money. Analyzing the chats, the authors speculated that the "mere presence of a selfish team member or the expression of selfish ideas" (p. 35) might have accounted for the lower transfers in teams.

Many-to-one helping

In Dufwenberg and Muren (2006), three dictators decided how to allocate 1000 Swedish kronor (about $110 at the time) among themselves and a recipient. They made these decisions in a face-to-face setting. And, dictators had to evenly split whatever they kept for themselves. The mean recipient payoff was 218 kroner (87% of an even split). The majority of teams settled on an equal split.

Dana et al. (2007) ran a multiple dictators game. In their study, two dictators decided the fate of a recipient. Each dictator chose between a self-interested option in which both dictators earned $6 and the recipient earned $1, and a fair option in which all three players earned $5. The self-interested option would only result if both dictators chose it. If either chose the fair outcome, then all three players went home with $5. This design more closely matches the typical bystander intervention study than it does our studies in that the recipient needs only one dictator to help. Thirteen of 20 (65%) dictators chose the self-interested option. In a control condition, in which one dictator chose between a self-interested option and a fair one, five of 19 (26%) chose the self-interested option.

Ottone (2008) conducted a modified third-party punishment game (Fehr & Fischbacher, 2004). A dictator divided a sum of money with a recipient. A third-party observer could spend his own money to penalize the dictator and/or transfer money to the recipient. Dictators knew this before they made their decisions. Ottone used a strategy method, asking observers what they would do for each possible dictator transfer. Observers punished low transfers, as in other third-party punishment studies (Bernhard, Fischbacher, & Fehr, 2006; Fehr & Fischbacher, 2004; Henrich et al., 2006; Marlowe et al., 2008). Observers' transfers to recipients negatively correlated with dictator transfers. When a dictator transferred nothing, the average transfer from observer to recipient was about €2 out of €10. This transfer tapered off to €1 when a dictator transferred €5 out of €10. If we think about these observers like responders in our strategy method, Ottone's study didn't elicit the same diffusion of responsibility. Ottone's observers seemed to perceive the situation as a volunteer's dilemma and transferred 20% of their endowment to the recipient when the dictator transferred nothing. One difference may have been the roles subjects assumed for themselves. In our studies, no dictator was specifically tasked to look out for the recipient's welfare. In Ottone's study, third-party observers may have felt more responsible for recipients and so transferred a lot of their money to recipients and expended a lot to punish selfish dictators. Conditioned on the dictator giving nothing, observers spent an average of €4 out of €10, €2 to punish the dictator and €2 to reward the recipient.

Follow-up studies

We conclude with suggestions for additional manipulations that might alleviate the bystander effect and help illuminate the psychology underlying the effect in this context.

Communication

In a meta-analysis of social dilemma experiments, Balliet (2010) found that communication fostered cooperation, and face-to-face talking was more effective than written messages. Direct forms of communication as opposed to text messages between dictators may alleviate the bystander effect.

Allowing recipients to participate in discussion may also alleviate the bystander effect. Recipients earn more money in dictator games when they can request specific amounts (Andreoni & Rao, 2011; Rankin, 2006). Requests may have a similar effect in an *N*-person dictator game.

Allowing recipients to communicate with dictators (or read dictators' messages) may also enhance group identity (Dawes et al., 1990), elicit empathy and sympathy (Batson & Ahmad, 2001; Batson & Moran, 1999; Small, 2010), and foster cooperation. If nothing else, dictators may not be comfortable discussing and rationalizing low transfers knowing recipients are observing.

Sanctions

Punishment sustains cooperation in public goods games (Fehr & Gächter, 2002; Yamagishi, 1986). In the third party punishment game, third parties punish dictators for transferring small amounts to recipients (Bernhard et al., 2006; Fehr & Fischbacher, 2004; Henrich et al., 2006, 2010a; Marlowe et al., 2008). It is possible that sanctions would alleviate the bystander effect in the *N*-person dictator game.

Culture

One should be cautious about drawing broad conclusions about the human condition from an experiment conducted on a pool of western undergraduates (Henrich, Heine, & Norenzayan, 2010b). We know that social preferences vary across cultures. Surveying 15 different cultural groups, Henrich et al. (2010a) found considerable variation in dictator transfers. People from cultures more integrated into the market economy transferred more in the dictator game, suggesting that the development of markets coincides with the development of fairness norms. There may also be cross-cultural variation in how people transform situations like the *N*-person dictator game. In many small-scale societies, communities provide social insurance (e.g., Gurven, Allen-Arave, Hill, & Hurtado, 2000): people invest in others, develop reputations for generosity, and build up social capital which can be drawn from in times of need. In groups relying on social insurance, there may be strong norms for helping in many-to-one situations like the *N*-person dictator game. In such groups, people may be less prone to the bystander effect, especially when donors can coordinate their helping decisions and there are sanctioning institutions. The individualism-collectivism dimension may be one place to look for such differences (e.g., Markus & Kitayama, 1991; Triandis, 1989).

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Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.obhdp.2012.06.008.

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