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Do people care about social context? Framing effects in dictator games

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Abstract Many previous experiments document that behavior in multi-person settings responds to the name of the game and the labeling of strategies. With a few exceptions, these studies cannot tell whether frames affect preferences or beliefs. In three large experiments, we investigate whether social framing effects are also present in Dictator games. Since only one of the subjects makes a decision, the frame can affect behavior merely through preferences. In all the experiments, we find that behavior is insensitive to social framing. We discuss how to reconcile the absence of social framing effects in Dictator games with the presence of social framing effects in Ultimatum games.

Keywords Framing · Dictator game · Social preferences

JEL Classification D03

1 Introduction

The power of economic theory derives partly from its parsimonious description of human motivation. Relying on the assumption that people's preferences are stable, an approach most stoutly defended by Becker and Stigler (1977), economists have built a simple, coherent, and general model of human behavior.

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But critics argue that the simplicity and coherence has come at a high price: Preference stability is just too unrealistic an assumption. Social framing effects are considered to furnish a particularly striking example of unstable preferences. A large number of experiments document that behavior in elementary social settings, such as the Prisoners' dilemma, depends on details such as the name that the experimenters have given to the game, or the labeling of the strategies. For example, subjects cooperate more when the situation is called the Community Game than when it is called the Wall Street Game (Liberman et al. 2004; Kay and Ross 2003).[1]

If preferences are so delicately sensitive to context, the external validity of preference data in general, and experimental data in particular, is gravely in doubt (see, e.g., Levitt and List 2007, and references therein). In view of social framing effects, it has been argued that conventional decision theory must be complemented by radically different approaches, involving concepts such as "role theory" (Montgomery 1998), "team reasoning" (Bacharach 1993, 1999; Sugden 1993), and "the logic of appropriateness" (March 1994; Weber et al. 2004). That is, if preferences are to be viewed as stable at all, they must be much richer than Becker and Stigler envisaged.

However, as noted by Fehr and Schmidt (2006) and Rabin (1998) among others, the presence of social framing effects does not prove that preferences are sensitive to context. The experimental evidence is also consistent with the alternative explanation that social context serves as a coordination device—entering subjects' beliefs rather than their preferences.[2] Indeed, after finding that social framing effects disappear when the Prisoners' dilemma is played sequentially, Ellingsen et al. (2012) suggest that social framing effects in the Prisoners' dilemma work primarily through beliefs.[3]

In this paper, we investigate the preference channel more directly, by varying the social frame in a Dictator game. The Dictator game shares the crucial feature of social dilemmas, namely a tension between the material interest of self and the material interest of others. Yet, as the recipient has no move, we know that any effect of the label on dictator behavior must come through the dictator's preferences. While the analogy to the Prisoners' dilemma would become even closer if we would multiply any transfers from the dictator by a constant greater than 1, we have chosen to retain the plain version of the Dictator game for comparability with the majority of previous Dictator game studies.

[1] See also Andreoni (1995), Brewer and Kramer (1986), Cookson (2000), Ellingsen et al. (2012), McDaniel and Sistrunk (1991), McCusker and Carnevale (1995), Pillutla and Chen (1999), Sell and Son (1997), Sonnemans et al. (1998), van Dijk and Wilke (2000), and Zhong et al. (2007). There are also some studies that fail to find the expected social framing effects, notably Brandts and Schwieren (2009), Cubitt et al. (2011), Dufwenberg et al. (2011), and Rege and Telle (2004). However, in Rege and Telle (2004) the effect size is large, so the lack of statistical significance might be due to a small sample, and Dufwenberg et al. (2011) have inadvertently, but instructively, used ambiguous social frames. Most of the above works consider relatively "light" social framing, i.e., manipulations of labels only. The earliest studies, by Deutsch (1958, 1960), apply "heavy" social framing, with more pronounced demands on behavior.

[2] If people care only about the own material payoff, beliefs should play no role in a Prisoners' dilemma. However, various common forms of altruism and reciprocity suffice to map a material Prisoners' dilemma into a game with multiple equilibria; see Ellingsen et al. (2012) for formalities.

[3] Relatedly, Cubitt et al. (2011) study the impact of framing on punishment and emotions in a one-shot social dilemma with and without punishment. They find no evidence that the frame affects punishment behavior or emotions, leading them to conclude that social preferences are relatively stable. However, the framing effect on initial contributions is also insignificant in their study.

To the best of our knowledge, there is no previously published study of social framing effects in Dictator games.[4] But several researchers have thought about the idea, and the conventional wisdom is that behavior will respond significantly.[5] For example, discussing the pros and cons of "neutrally" framed experiments, Loewenstein (1999, p. F31) writes:

> [i]t would be easy, through a suitable manipulation of context, to design a dictator study in which people would give none of their money to a stranger, or one in which most people would give all of their money away.

Likewise, Fehr and Schmidt (2006, p. 659) argue:

> While the experimental results on ultimatum games are fairly robust, the dictator game seems to be a rather fragile situation in which minor factors can have large effects.

At the outset, based on previous experimental and theoretical work reviewed in the next Section, these were our priors too.

We conduct three Dictator game studies, whose design will be motivated below. In the first study, we let dictators either move money from their "own" envelope (give) or move money from the "other's" envelope (take). The dictator is completely anonymous to the recipient as well as to the experimenter, but in one pair of treatments the recipient learns about the game whereas in the other treatment the recipient does not learn at all where the money, if any, comes from. We hypothesized that the dictator would be relatively averse to taking, as this could be seen as violating the other's entitlement, and that the recipient would thus get more money in the take treatment, especially when recipients learn about what has happened. Although there are tendencies in the hypothesized direction, effect sizes are economically tiny and statistically insignificant.

The second study is designed with two objectives in mind. First, we seek to remove confounds that may cloud inference in the first study. For example, the label

[4] In the concluding section, we discuss the closely related unpublished study by Suvoy (2003), which was recently brought to our attention by Sujoy Chakravarty. There we also discuss the findings of Leliveld et al. (2008) concerning framing effects in Ultimatum games. For completeness, let us here also mention one additional related study. Keysar et al. (2008) ostensibly let pairs of subjects play a sequence of two Dictator games, with some pairs acting under a "give" frame and other pairs under a "take" frame. Leaving aside the problem that an even allocation of the money can be attained after any decision by the first mover, the experiment must be interpreted with caution for several reasons. First, only one participant is being paid. Thus, at most one of the subjects in a pair will be playing with real money. When the first mover chooses an even split rather than to keep all the money, this could thus be seen as an implicit suggestion of mutual insurance. Due to risk aversion, both players getting 50 is preferable to both players having an even chance of 100 and 0. Second, the experiment involves severe deception: In fact, there is no first mover. All subjects are second movers, being led to believe that the first mover chose an even split. Apparently many of the participants anticipated being deceived, as data for ten of the fifty participants in the relevant experiment were excluded because these participants didn't believe that there was a second mover.

[5] Besides the evidence that social frames matter in multi-person games, there is evidence that other sorts of frames can matter in single-person decision problems. For example, in a seminal study, Tversky and Kahneman (1981) showed that people's ranking of lotteries depend on whether outcomes are framed as gains or losses. For a survey of individual choice effects of wording, see Levin et al. (1998). For a suggestion how to take account of framing effects in decision theory, see Salant and Rubinstein (2008).

"taking game" may to some subjects have justified violating the other's entitlement. Therefore, in the second study we only manipulate the name of the game and the strategy labels, not the entitlement. In the lightest pair of frames, one game is called the Giving Game and the other is called the Keeping Game, but actions are given a neutral label, "transfer." In the heavier pair of frames, the corresponding actions are called "give" and "keep" respectively. If subjects see labels as normative, then they ought to transfer more money to the opponent in the "give" frame.

Second, we desire a large enough sample size to ensure that any economically significant difference is also statistically significant. To collect such a large sample, we use the online labor market *Amazon Mechanical Turk*, henceforth MTurk, to conduct the experiment electronically. (For a description and discussion of MTurk, we refer to Horton et al. 2011.) However, even with about 200 subjects per treatment, social framing effects are so small as to remain statistically insignificant. To our surprise, an anonymous referee complains that the number of subjects is too large ("doesn't make sense"). We disagree. Unless the objective is to contain costs or to maximize the chance of a false positive result, there is no downside to increasing the number of observations.

As MTurk is a new technology for performing economic experiments, one might wonder whether the lack of statistically significant effects in Study 2 is the result of some limitation of the MTurk platform. To demonstrate that this is not the case, and that MTurk experiments are able to detect framing effects where they do exist, we perform a replication experiment, described in Appendix. Consistent with previous results in the physical lab (Liberman et al. 2004; Kay and Ross 2003), we find that in a one-shot Prisoners' dilemma on MTurk, calling the game the 'Community Game' results in significantly more cooperation than when the game is called the 'Profit Game.'

Finally, at the request of the Editor, we conducted a third Dictator game study in which we adapt the design of Study 1 for MTurk. Again, despite a large sample, we fail to detect any significant framing effects.

Overall, our conclusion is that preferences in Dictator games, at least in our subject pools, are largely immune to mere labeling of games and strategies. In the final section we discuss how to interpret the evidence on framing effects in other settings in light of our finding.

2 Theoretical framework and dictator game evidence

Before discussing social framing effects, it is useful to consider which model best explains the wealth of existing Dictator game data.[6] We shall not here attempt a complete survey of the various approaches to modeling social preferences; see Camerer (2003), Fehr and Schmidt (2006), and Sobel (2005) for surveys, and Andreoni and

[6] At the time of writing, the literature on Dictator games comprises more than 120 published articles; see Engel (2011) for a recent survey.

Miller (2002) for an empirical study that links up with conventional choice theory.[7] But it is noteworthy that several recent proposals revolve around the hypothesis that societies have norms governing the sharing of joint windfalls, and that people trade-off their material benefit against compliance with the social norm; see in particular Andreoni and Bernheim (2009), Konow (2010), and Krupka and Weber (2010).[8] To the extent that norm compliance is observable, it is now also quite clear that people factor their social esteem or image into the decision. That is, norm compliance tends to be greater when behavior is observed and understood by others—both participants in the social situation and spectators.

As Andreoni and Bernheim (2009) show, the following utility function captures many of the observed Dictator game regularities:

$$U_i = u(1-s, m) + \eta_i v(s - 1/2), \tag{1}$$

where s is the share of the windfall that is given to the recipient, $\frac{1}{2}$ is the social norm (the ideal gift) concerning how to split a windfall gain, η_i is a parameter that indexes how strongly a dictator of type i cares about complying with the norm, and m is the dictator's social image—which is given by (the dictators belief about) the recipient's belief about η, conditionally on the dictator's choice of s. The sub-utility functions u and v are assumed to be concave in $(1-s)$ and s respectively, but while u is everywhere increasing in both arguments, v attains a maximum at $s = 1/2$. Heterogeneity in behavior is here explained by the assumption that people differ in their norm-compliance parameter η.

Due to the concern for social image, m, Andreoni and Bernheim's utility function implies that the Dictator game is essentially a signaling game, and in order to make precise predictions about behavior one must significantly refine the set of Bayesian Nash equilibria. Under appropriate refinements, such as D1, the actual distribution of η in the population is relatively unimportant for the qualitative results. Instead, the key is that a larger η only induces larger gifts s up to the point that $s = 1/2$. Thus, even if the social image concern is strong, people stop giving at that point. In this way, the model explains the large frequency of equal splits without assuming that the utility function has a kink at this point. The model also explains the very low frequency of gifts just below $1/2$ and just above 0.

Since people are concerned about their social esteem, the model can furthermore explain why the degree of dictator anonymity matters (Hoffman et al. 1994). Likewise, the model explains why people are willing to pay for quietly exiting the Dictator game rather than going through with their initial allocation, as documented by Dana et al. (2006); see also Broberg et al. (2007) and Lazear et al. (2012). As Andreoni and Bernheim (2009) show, people are also more prone to give small amounts when

[7] Recall that Becker and Stigler (1977), while defending preference stability, never insisted that people ought to be seen as selfish materialists. To the contrary, throughout his career, Gary Becker has been a leading advocate of social preferences. His models are populated with agents having tastes for (out-group) discrimination as well as (in-group) altruism. See, e.g., Becker (1974).

[8] It is quite clear that people are more prone to share windfalls than earned money (e.g., Cherry et al. 2002), and it is likely that this is due to social norms that distinguish the two situations (Cappelen et al. 2007).

the recipient is informed that small gift could have come about by chance—due to circumstances beyond their control; see also Tadelis (2008).[9]

However, the literature is less clear when it comes to the importance of recipient awareness versus experimenter observation and social distance. In their Study 2, Dana et al. (2006) find that average transfers are 33 percent smaller under recipient unawareness, but the number of observations is too small for this difference to be statistically significant. When Koch and Norman (2008) manipulate recipient awareness of the game, with experimenter blindness and large social distance, gifts are only about ten percent smaller when the recipient is unaware of the game being played; again this effect size is not statistically significant.

In addition to the evidence concerning recipient awareness and exit behavior, which Andreoni and Bernheim themselves explicitly address, we would like to add that their model is also capable of explaining the findings by Bardsley (2008) and List (2007), who both show that the introduction of additional "taking" options for the dictator reduces giving. Even if the ideal behavior $s = 1/2$ is the same, the greediest option now involves taking, which means that a smaller gift suffices to separate oneself from the most selfish types. As far as we know, this point has not been made before.[10]

We do not want to claim that the model embodied in (1) can capture all the available Dictator game evidence. First, it seems likely that social distance between the dictator and the recipient also matters significantly. Bohnet and Frey (1999) find that dictators give more when they can see the recipient; similarly Charness and Gneezy (2008) show that gifts go up when the dictator learns the recipient's surname. Reductions in social distance may also explain why pre-play and anticipated post-play messages by the recipient serve to increase giving (Andreoni and Rao 2011; Ellingsen and Johannesson 2008; Houser and Xiao 2009; Mohlin and Johannesson 2008). Similarly, dictators who are exposed to other dictators' decisions or to recipient expectations tend to give more on average, even for a given level of anonymity (Bicchieri and Xiao 2009; Ellingsen et al. 2010; Krupka and Weber 2009).[11] While some of these findings could be due to actual transmission of information about norms and about other people's feelings, a direct effect of social distance probably better fits the sum of the evidence. Nonetheless, it seems to us that "social distance" is a concept that is both more specific and tractable than "social frame," and thus represents less of a challenge for decision theory. We therefore leave it aside here.

[9] Kritikos and Bolle (2005) introduce asymmetric information about the dictator's endowment and show that some dictators with a large endowment choose to give half of the smaller endowment. Related effects were previously documented in the Ultimatum game literature, where experiments vary responders' knowledge of proposers' endowment; see, e.g., Mitzkewitz and Nagel (1993). However, here it is difficult to disentangle effects stemming from social esteem concerns from effects stemming from strategic responder behavior.

[10] Another possibility is that the ideal point changes for some subjects, who think that the norm prescribes an equal split of whatever surplus they have the power to distribute.

[11] There are also several studies suggesting that subtle primes, such as pictures of eyes or a few dots arranged like a face on the computer screen, affect behavior in Dictator games (Haley and Fessler 2005; Rigdon et al. 2009). One interpretation is that these cues subconsciously enhance concerns for social esteem or decrease the felt social distance.

That said, at an abstract level it is easy enough to introduce social framing effects in the above model. Simply multiply the second term by a parameter ϕ^F, denoting the congruence between the frame F and the social norm. Then, the utility function becomes

$$U_i = u(1 - s, m) + \phi^F \eta_i v(s - 1/2). \tag{2}$$

For example, if the equal division norm is expected to be more salient under a Giving frame than under a Keeping frame, the distribution of gifts should move upwards in the former case. Thus, while it seems hard to develop a general model that includes social framing effects (because it requires a metric that relates frames to each other), it is straightforward to measure the magnitude of such an effect in an experiment where the frames, as well as other features, are tightly controlled. Indeed, as the effect is expected to be monotonic across dictator types in our experiment, we may merely compare the mean gifts under the two frames.

One generalization of the model would be to allow multiple social norms. In this case, the frame could indicate which of the norms are more applicable. In fact, even (2) could be interpreted in this way, with selfishness (or competitiveness) being the second norm. A fully satisfactory generalization of the model along these lines would require an explicit treatment of people's uncertainty with respect to the applicability of various norms, and a specification of how frames allow people to update their view about norm applicability. Note that this generalized version of the model lets frames affect beliefs rather than preferences.

It remains to motivate our particular choice of labels. In our first study, we chose to utilize labels that are associated with property rights for three reasons. First, notions of entitlement have similar meanings in many cultures. In fact, the propensity to defend own possessions and to show respect for others' possessions appears to be a basic human trait, present already in childhood and shared with many non-human animals. According to Stake (2004) a sense of property right is instinctive and has deep evolutionary roots; see also Gintis (2007) for a discussion of the relevant literature. A "take" frame that accentuates the opponent's entitlement should therefore leave the opponent with more money than a "give" frame that accentuates the dictator's entitlement. Second, earlier experiments have indicated that entitlements are important for Dictator game sharing. For example, Hoffman et al. (1994) show that dictators are less generous when they have "earned" the right to the dictator position. Third, the chosen labels have natural meanings in the Dictator game; the transfer choice can be interpreted as either "giving" or "taking" depending on who holds the endowment. Other labels that have been used in the social framing literature, such as "the Wall Street game" or "the Community game" would not have the same natural connotation in the Dictator game setting.

Regardless of the reasons, our initial choice of labels was fortuitous, because it overlaps with the labels used by Leliveld et al. (2008). In their study, which our literature search had originally missed, they demonstrate that the entitlement manipulations "giving," "taking," and "splitting" affect behavior in Ultimatum games. We discuss their findings in the final section.

3 The first study

We recruited 400 subjects from public spaces around Harvard University in June 2008. Research assistants handed fliers to passers-by outside of Harvard University's student union (the "Science Center") instructing interested persons to come to a particular room at one of a set list of times.[12] Upon arriving, subjects were randomly directed to one of two rooms, one of which contained dictators only and the other recipients only. Only one treatment was run in both rooms at a given time.

The subjects were largely Harvard summer students, but also included community members and tourists. The average age was 22.0 years (min 15 years, max 71 years), 47.5 % of the subjects were female, and 29 % of the subjects were either economics or psychology majors (and thus may have had prior exposure to the dictator game).

In our 2X2 design, the game is (i) framed either in terms of giving or taking, and (ii) recipients are either uninformed or informed about the game. In each of the four treatments, dictators make a choice about how to divide $10 between themselves and another anonymous subject (the recipient).

We frame the game in the following way. Every dictator receives two numbered envelopes and a set of written instructions. One of the envelopes is marked "You" and the other is marked "Other person." In the giving game, the "You" envelope contains 10 $1 bills and the other envelope is empty, whereas in the taking game the "You" envelope is empty while the "Other person" envelope contains 10 $1 bills. In the instructions, subjects are informed that they will play either the "Giving game" or the "Taking game," and that they will decide how many bills to "give to" or "take from" the recipient. In both the giving game and the taking game, dictators go behind a screen one at a time, and move as much money from one envelope to the other as they wish. Once a dictator reaches a decision, he/she puts the "Other person" envelope in a box marked "Mail," and keeps the "You" envelope. Dictators then fill out a questionnaire on demographics, put it in a box marked "Questionnaire" and leave. Thus the framing is done via both the name of the game and the labeling of the allocation action.

In order to detect any role of esteem concerns, we let recipients be either uninformed or informed about the existence of a game. When uninformed, they do not know they are recipients in a Dictator game. When informed, they are given the same instructions as the dictators in their treatment. By having all recipients, uninformed and informed, fill out a questionnaire on an unrelated topic, uninformed recipients believe they are receiving money for completion of this questionnaire (note that we do not deceive the subjects, since all recipients fill out this questionnaire and we do not explicitly tell the recipients that they receive money for completion of the questionnaire). Dictators know whether their recipient is uninformed or informed, but are unaware of the existence of any of the treatments other than the one they are participating in.

[12] The flier contained the following text: "Participate in a behavioral study in the Science Center, Room 232: 11 am–4 pm. Participation takes about 10 min. You earn $5–15. If you are interested in participating, drop by at 11 am, 12 pm, 12:30 pm, 1 pm 1:30 pm, 2 pm, 2:30 pm, 3 pm, or 4 pm."

When all dictators in a session had made their decisions, the experimenters took the box marked mail to another room, and recorded the content of each envelope without removing any bills from the envelope. The experimenters then took the envelopes to the room with the recipients, and distributed the money to the recipients.

3.1 Results

The average transfer is very similar across the four treatments (Fig. 1). To test for an effect of how the game is framed (giving versus taking), for an effect of recipient information about the payoff structure (uninformed versus informed), and for an interaction between the two, we use ordinary least squares (OLS) regression with robust standard errors,[13] taking percentage transferred as the dependent variable (0 to 100), and including controls for gender and age (Table 1).[14] For completeness, we also report ANOVA results in a footnote following the discussion of the OLS estimates.

We find no significant effect of a Game-name dummy (3.178, $p = 0.371$) or a Recipient-information dummy (coeff $= 4.143$, $p = 0.254$). We also find no significant interaction between the two dummies (coeff $= 3.427$, $p = 0.640$). Thus we find no significant effect of either how the game is framed or whether the recipient is informed of the payoff structure.[15]

The irrelevance of recipient knowledge of the situation corroborates the finding of Koch and Norman (2008). Like us, in a double-blind design they find no statistically significant difference in transfers depending on whether the recipient is being informed about the experimental condition or not. Of course, this does not prove that the effect is exactly zero, only that it is small: Their point estimate of the effect is about ten percent and ours is closer to fifteen percent. In the single-blind study of Dana et al. (2006) (their Study 2), the point estimate of the recipient knowledge effect

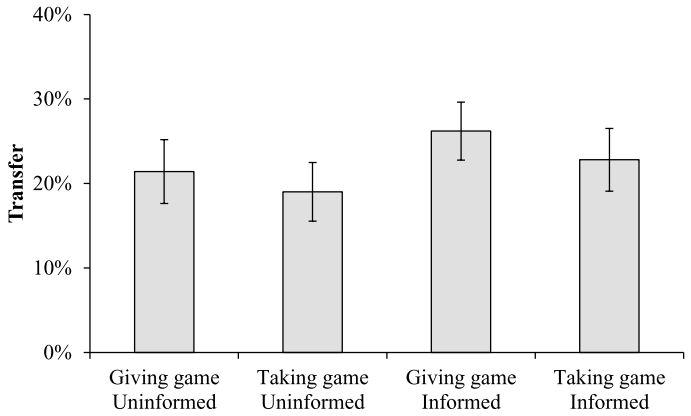

Fig. 1 Average dictator game transfer in Study 1 (out of $10), by framing of the game (giving versus taking) and recipient information (uninformed versus informed)

[13] Instead using Tobit regression with robust standard errors, to account for the minimum (0) and maximum (100) transfer percentages, gives qualitatively equivalent results. We choose to report OLS results so as to have directly interpretable coefficients.

[14] We consistently find a significant positive effect of female gender on dictator transfer.

[15] ANOVA finds no significant effect of the Game name ($p = 0.421$) or of Recipient information ($p = 0.233$), and no significant interaction between the two ($p = 0.890$).

Table 1 The effect on transfers of game name and recipient information in Study 1

	(1)	(2)	(3)	(4)
Game name (0 = Taking, 1 = Giving)	2.900	3.178	2.400	1.454
	(3.589)	(3.546)	(5.129)	(5.130)
Recipient informed (0 = No, 1 = Yes)	4.300	4.143	3.800	2.427
	(3.589)	(3.624)	(5.073)	(5.183)
Gender (0 = M, 1 = F)		7.797[b]		8.064[b]
		(3.741)		(3.763)
Age		−0.0765		−0.0653
		(0.235)		(0.241)
Game name X Recipient informed			1	3.450
			(7.195)	(7.483)
Constant	18.75[a]	16.67[a]	19[a]	17.15[a]
	(3.033)	(5.826)	(3.467)	(5.720)
Observations	200	200	200	200
R-squared	0.010	0.034	0.011	0.035

Robust standard errors in parentheses
[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.1$

is larger, but the number of subjects is too small to make the difference statistically significant.[16]

4 The second study

A possible objection to the first study is that the "taking" label is ambiguous. Perhaps some subjects interpret the label as a license to take rather than a reminder of the immorality of moving money from the other's envelope? In that case, the unselfishness induced by putting money in the "other" envelope will be counteracted, rather than magnified, by the taking label.

In the second study, we therefore return to a more standard set-up. The endowment always originally belongs to the dictator, and the names of the game and the strategies are chosen to have perfectly unambiguous normative implications: The Giving Game is contrasted with the Keeping Game.

To obtain additional information about the impact of social frames, we furthermore decided to elicit recipient beliefs. Previous work by Liberman et al. (2004) finds that beliefs of spectators are less responsive to frames than decision makers' actions are. Since the recipient has a stake, and is not merely a spectator, we were curious to see whether the reverse would be true here. (We did not consider this issue before conducting Study 1, and so do not have any belief data for that study.) For simplicity,

[16] As mentioned in the Introduction, Dana et al. (2006) do find a significant effect of recipient knowledge on dictator exit behavior.

like Liberman et al., we only asked for a straight guess with no payment for close guesses.[17]

We recruited 1586 subjects from MTurk in March and April 2010. Subjects responded to a job posting containing an html hyperlink leading to an external survey website containing the game instructions.[18] After reading the instructions, indicating their decision and completing a brief post-experimental questionnaire, subjects were given a completion code, which they entered back into Amazon Mechanical Turk. Once all responses had been collected, dictators and recipients were randomly paired and earnings were calculated, and each subject was paid accordingly through the Amazon Mechanical Turk website. Each subject was also sent a message explaining the decision of the dictator and the resulting payoff of both parties.

The subjects were Amazon Mechanical Turk workers from around the world. The average age was 29.9 years (min 13 years, max 75 years) and 45.8 % of the subjects were female. In terms of nationality, 49.2 % of subjects resided in the United States, 34.1 % resided in India, and 16.7 % resided elsewhere.

Subjects were randomly allocated to be either dictator or recipient in one of four different treatments. All subjects were paid a show-up fee of 20 cents. The study took on average 3 minutes to complete.[19]

In our 2X2 design, (i) the game is framed either in terms of giving or keeping, and (ii) the action label is either neutral or active. In all four treatments, dictators receive 100 cents and choose how to allocate those 100 cents between themselves and another anonymous subject (the recipient).

[17] The benefit of incentives is to reduce noise, whereas the main cost is to introduce bias through hedging. With our large sample small payoffs we think that both problems are small. For recent discussions of the costs and benefits of incentivizing belief elicitation, see Armantier and Treich (2010) and Blanco et al. (2010).

[18] The job was titled "Participate in a brief decision-making study" and the advertisement read "Participate in a brief study. In this HIT, you will be asked to participate in a short decision-making study followed by a brief survey. In addition to your initial payment of $0.20, you will have the opportunity to earn a bonus of up to $1.00, for a total payment of up to $1.20. To begin, please follow the study instructions here (the link will open in a new browser window). At the end of the study, you will be given a unique completion code. Paste the completion code below, and click submit. YOU MUST PASTE THE COMPLETION CODE BELOW TO RECEIVE YOUR BONUS."

[19] Online labor markets in general, and MTurk in particular, have recently received considerable attention as powerful platforms for performing incentive-compatible experiments. On MTurk, employers hire workers from around the world to complete short tasks for small amounts of money (usually less than $1). This allows researchers to recruit large number of subjects quickly with little effort or expense. Stakes are generally much lower in MTurk experiments than in physical lab experiments, a feature that is partly justified by the much smaller time costs associated with participating. Online experiments also necessarily permit less control over subjects during the study. To address these and other concerns regarding the validity of experiments run on MTurk, a number of replication studies have been undertaken. Most relevant for the present study, the effect of $1 stakes in dictator games on MTurk has been shown to be similar to that in the physical laboratory (Amir et al. 2012), and the same study finds that the average donation is very similar to the most common average transfer across many studies in the recent meta-analysis of Engel 2011. Furthermore, quantitative agreement between behavior on MTurk and in the physical lab has been demonstrated in a one-shot prisoners' dilemma (Horton et al. 2011) and a repeated 4-player public goods game (Suri and Watts 2011), and it has been shown that subjects on MTurk respond to framing manipulations (Paolacci et al. 2010; Horton et al. 2011).

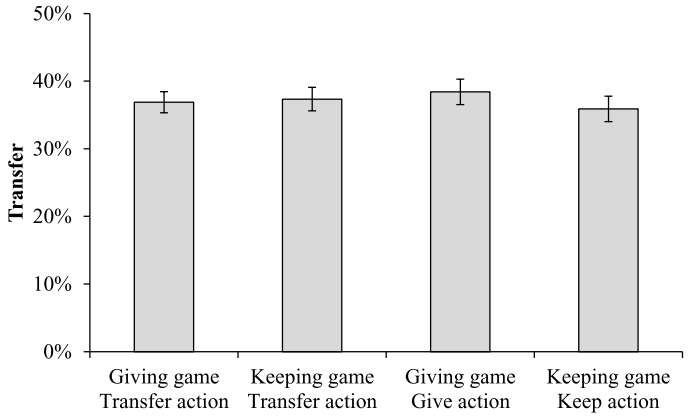

Fig. 2 Average dictator game transfer (out of 100 cents) in Study 2, by framing of the game (giving versus keeping) and labeling of the action (transfer versus give/keep)

We frame the game by informing subjects, in the written instructions, that they are playing either "the giving game" or "the keeping game."

The action label is either neutral ("Transfer") or active ("Give" in the giving game and "Keep" in the keeping game). In the treatments with the neutral action label (Giving-Transfer and Keeping-Transfer) and the 'Give' action label (Giving-Give), dictators use a sliding bar to indicate the number of cents (out of 100) they want to recipient to receive. In the treatment with the 'Keep' action label (Keeping-Keep) dictators instead indicate the number of cents they want to receive themselves (again using a 0-to-100 sliding bar).

Once a dictator has made her decision, she fills out a questionnaire on demographics.

To explore the effect of framing on beliefs, recipients are asked to indicate how much of 100 cents they expect to receive from their dictator. Recipients then fill out the same questionnaire. In all treatments, dictators and recipients get the same instructions about the setup.

4.1 Results

We begin by considering dictator transfers. As in Study 1, the average transfer is very similar across the four treatments of Study 2 (Fig. 2). To test for effects of the game name (giving versus keeping) and the action label (transfer versus give/keep), we use a regression analysis similar to that of Study 1. All analyses use ordinary least squares (OLS) regression with robust standard errors,[20] taking percentage transferred as the dependent variable, and including controls for gender, age and country of residence (Table 2).

We find no significant effect of a Game-name dummy (coeff $= 0.740$, $p = 0.692$) or an Action-label dummy (coeff $= 0.956$, $p = 0.609$). We also find no significant interaction between the two dummies (coeff $= 2.321$, $p = 0.535$). Thus, as in Study 1,

[20] As in Study 1, instead using Tobit regression gives qualitatively equivalent results.

Table 2 The effect on transfers of game name and action label in Study 2

	(1)	(2)	(3)	(4)
Game name (0 = Keeping, 1 = Giving)	0.920	0.740	−0.451	−0.338
	(1.755)	(1.867)	(2.342)	(2.469)
Action label (0 = Transfer, 1 = Give/Keep)	0.108	0.956	−1.441	−0.257
	(1.765)	(1.869)	(2.556)	(2.694)
Gender (0 = M, 1 = F)		−0.881		−0.903
		(2.000)		(2.001)
Age		0.0463		0.0436
		(0.0904)		(0.0903)
Game name X Action label			2.971	2.321
			(3.534)	(3.736)
Country of residence dummies	No	Yes	No	Yes
Constant	36.57[a]	97.27[a]	37.34[a]	98.46[a]
	(1.516)	(4.373)	(1.741)	(4.708)
Observations	793	790	793	790
R-squared	0.000	0.065	0.001	0.065

Robust standard errors in parentheses
[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.1$

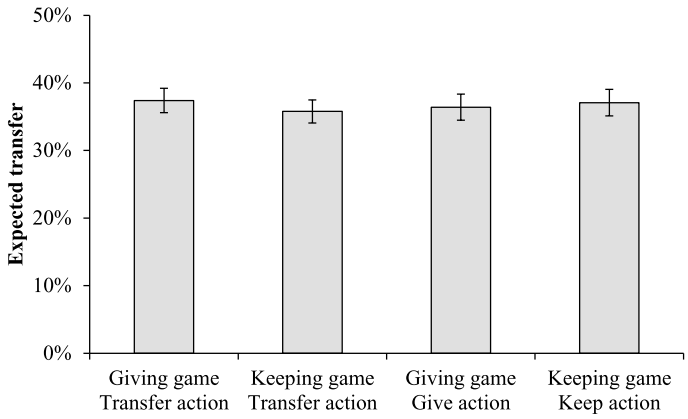

Fig. 3 Average dictator game transfer expected by recipients (out of 100 cents) in Study 2, by framing of the game (giving versus keeping) and labeling of the action (transfer versus give/keep)

we find no significant framing effects. Neither the name of the game nor the action label significantly changes the average transfer.[21]

We next turn to recipient beliefs about dictator transfers. Recipients are quite accurate in their predictions, and we find no significant difference between expected and actual transfers overall, or within each treatment (Rank-sum, $p > 0.10$ for all

[21] ANOVA finds no significant effect of Game name ($p = 0.570$) or of Action label ($p = 0.934$), and no significant interaction between the two ($p = 0.913$).

Table 3 The effect on recipients' expected transfers of game name and action label in Study 2

	(1)	(2)	(3)	(4)
Game name (0 = Keeping, 1 = Giving)	0.499	−0.438	1.613	−0.00253
	(1.849)	(1.841)	(2.491)	(2.450)
Action label (0 = Transfer, 1 = Give/Keep)	0.148	0.739	1.295	1.188
	(1.853)	(1.835)	(2.596)	(2.522)
Gender (0 = M, 1 = F)		−1.028		−1.045
		(1.949)		(1.951)
Age		0.0401		0.0401
		(0.0834)		(0.0836)
Game name X Action label			−2.282	−0.897
			(3.706)	(3.711)
Country of residence dummies	No	Yes	No	Yes
Constant	36.35[a]	45.38[a]	35.78[a]	45.39[a]
	(1.522)	(5.508)	(1.709)	(5.517)
Observations	799	799	799	799
R-squared	0.000	0.160	0.001	0.160

Robust standard errors in parentheses
[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.1$

comparisons).[22] Therefore, as can be seen in Fig. 3, expected transfers also do not vary across treatments. We perform the same set of regressions used above to test for effects of game name and action label, and again find no significant effects (Table 3). Thus we find no evidence that framing changed the beliefs of the recipients about how the dictators would behave.

5 The third study

In order to make sure that MTurk and laboratory results are compatible, we finally recruited 1516 subjects from MTurk in April 2012 to play a modified one-shot dictator game with similar instructions to Study 1. Subjects responded to a job posting containing an html hyperlink leading to an external survey website containing the game instructions.[23] After reading the instructions, indicating their decision and completing a brief post-experimental questionnaire, subjects were given a completion code, which they entered back into Amazon Mechanical Turk. Once all responses had been

[22] The difference between expected and actual transfers remains non-significant when including only attentive subjects (Rank-sum, $p > 0.10$ for all comparisons).

[23] The job was titled "Short academic study" and the advertisement read "Participate in a short decision-making study. You will make several decisions and answer a very short survey in this study. In total it will take less than 7 minutes. For your participation, you will receive $0.50 plus a bonus of up to $1.00. To begin, please follow the study instructions here (the link will open in a new browser window). At the end of the study, you will be given a unique completion code. Paste the completion code below, and click submit. YOU MUST PASTE THE COMPLETION CODE BELOW FOR YOUR HIT TO BE ACCEPTED."

collected, dictators and recipients were randomly paired and earnings were calculated, and each subject was paid accordingly through the Amazon Mechanical Turk website (for more information on this procedure, see Rand 2012). Each subject was also sent a message explaining the decision of the dictator and the resulting payoff of both parties, except for the recipients in the no-information conditions, who received a message stating that their payment was for their participation in the survey (but not specifying why the particular amount was paid).

The subjects were Amazon Mechanical Turk workers from around the world. The average age was 30.7 years (min 16 years, max 82 years) and 41.7 % of the subjects were female. In terms of nationality, 67.7 % of subjects resided in the United States, 22.8 % resided in India, and 9.5 % resided elsewhere.[24]

In total, 758 subjects were randomly allocated to be dictators and 758 to be recipients. Subjects were randomly allocated to be either dictator or recipient in one of four different treatments. All subjects were paid a show-up fee of 50 cents. At the outset of the study, all subjects were asked to transcribe the same paragraph of handwritten text[25] in order to discourage MTurk workers that would not have the attention or inclination to carefully read the subsequent instructions. The study took on average 6 minutes to complete.

As in Study 1, in our 2X2 design the game is (i) framed either in terms of giving or taking, and (ii) recipients are either uninformed or informed about the game. In each of the four treatments, dictators make a choice about how to divide 100 cents between themselves and another anonymous subject (the recipient).

We frame the game by informing subjects, in the written instructions, that they are playing either "the giving game" or "the taking game." In the giving game treatments, dictators use a sliding bar to indicate the number of cents (out of 100) they want the recipient to receive. In the taking game treatments, dictators instead indicate the number of cents they want to take from the recipient (again using a 0-to-100 sliding bar).

We vary recipient information as follows. When uninformed, they do not know they are recipients in a Dictator game. Instead, they complete a questionnaire and are told they may receipt a bonus payment for completing the survey, but are given no reason for the particular bonus amount they receive. When informed, they are given the same instructions as the dictators in their treatment, and then complete the same questionnaire as the uninformed recipients (and the Dictators). Dictators know whether their recipient is uninformed or informed, but are unaware of the existence of any of the treatments other than the one they are participating in. Once a dictator has made her decision, she fills out the same questionnaire as the recipients.

[24] While demographic information on MTurk is self-reported, it has been shown that country of residence reporting is reliable based on comparison with IP address (Rand 2012), and that there is a high degree of test-retest reliability for other demographic variables.

[25] The text was as follows: "regular old put-a-stamp-on-it mail. If you can't be there in person, send a letter. And if you have trouble finding the time, let us help you!"

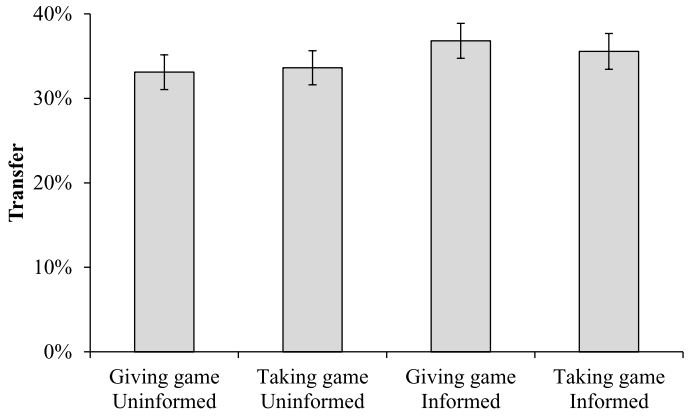

Fig. 4 Average dictator game transfer in Study 3 (out of 100 cents), by framing of the game (giving versus taking) and recipient information (uninformed versus informed)

Table 4 The effect on transfers of game name and recipient information in Study 3

	(1)	(2)	(3)	(4)
Game name (0 = Taking, 1 = Giving)	0.365	0.382	−0.515	−0.339
	(2.064)	(2.061)	(2.870)	(2.886)
Recipient informed (0 = No, 1 = Yes)	2.816	2.703	1.952	1.995
	(2.064)	(2.073)	(2.928)	(2.947)
Gender (0 = M, 1 = F)		−0.854		−0.854
		(2.170)		(2.173)
Age		0.158		0.156
		(0.0986)		(0.0990)
Game name X Recipient informed			1.761	1.438
			(4.130)	(4.154)
Country of residence dummies	No	Yes	No	Yes
Constant	33.18[a]	29.77[a]	33.61[a]	30.21[a]
	(1.758)	(3.629)	(2.015)	(3.897)
Observations	758	758	758	758
R-squared	0.002	0.009	0.003	0.009

Robust standard errors in parentheses
[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.1$

5.1 Results

We begin by considering dictator transfers. As in Study 1, the average transfer is very similar across the four treatments of Study 3 (Fig. 4). To test for effects of the frame (giving versus taking) and recipient information (uninformed versus informed), we use a regression analysis similar to that of Study 1. All analyses use ordinary least squares (OLS) regression with robust standard errors, taking percentage transferred as the dependent variable, and including controls for gender, age and country of residence (Table 4). As in Study 1, we find no significant effect of Game name (coeff = 0.382, $p = 0.853$) or Recipient information (coeff = 2.70, $p = 0.193$), and no significant interaction between the two (coeff = 1.438, $p = 0.729$).

We conclude that the taking/giving manipulation noticeably affects behavior neither in the lab nor in the field.[26] The only difference between Study 3 and Study 1 is the overall level of transfers. The donation level is substantially higher in Study 3. In fact, donation levels are similar in the two Mturk studies (Studies 2 and 3). One possible explanation is that the stakes are smaller in these studies; another is that the subject pools differ.

6 Discussion

As noted in the Introduction, there is a widespread concern that people's conception of situations and the norms that govern them are easily malleable—that minor changes in how the situation is described can affect people's preferences. However, our experimental evidence rejects the hypothesis that behavior in a Dictator game is sensitive to the naming of the game or of the strategies. If there is such an effect at all in our experiment, we have enough statistical power to say that it is small.

After completing our work, we became aware of Suvoy (2003), an unpublished Honors thesis reporting evidence from an experiment quite similar to our first study—a dictator game framed as either a taking game or a giving game, with associated entitlements. Like us, Suvoy failed to identify any significant effect of the frame on behavior, rejecting his (and our) hypothesis that the dictator would be less greedy in the take frame. However, we think that our rejection is even stronger, since we expose each subject to one frame only; Suvoy let each subject make decisions under both frames.

Our work is also closely related to Leliveld et al. (2008), who investigate framing effects on proposer behavior in an Ultimatum game. They consider three frames: "giving," "splitting," and "taking." They find that the proposals are more generous in the "taking" treatment (the opponent has the entitlement) than in the "giving" treatment in (the proposer has the entitlement). These findings are perfectly in line with our initial expectations, and the effect sizes are considerable. Why, then, is it that there is a framing effect in this study but not in ours? At first sight, one reason would seem be that the responder can reject in the Ultimatum game, and that proposers anticipate more aggressive rejections under the taking frame. But Leliveld et al. (2008) reject this explanation on the basis of a "generalized Ultimatum game" control treatment in which they vary the impact of rejection on the proposer's payoff. Formally, the generalized Ultimatum game is constructed as follows:[27] If the proposer suggests the payoff division $(1-s, s)$, a rejection entails the payoffs $(\delta(1-s), \delta s)$. Note that when $\delta = 0$, payoffs are as is the standard Ultimatum game, and when $\delta = 1$, payoffs are as in the standard Dictator game. In the low-dependency (high δ) condition of Leliveld, van Dijk, and van Beest's Experiment 3, $\delta = 0.9$, but framing effects are almost as large as in the high-dependency condition $\delta = 0.1$.

[26] ANOVA finds no significant effect of Game name ($p = 0.860$) or Recipient information ($p = 0.171$), and no significant interaction between the two ($p = 0.670$).

[27] To the best of our knowledge, this game was first studied by Suleiman (1996).

Clearly, these findings from the generalized Ultimatum game are compatible with our Dictator game findings only if people are more sensitive to the frame in the Ultimatum game than in the Dictator game. With the benefit of hindsight (and the helpful guidance of an anonymous referee), we now think that this is a plausible interpretation. In a particularly relevant study, Handgraaf et al. (2008) show that the generalized Ultimatum game with $\delta = 0.9$, which is "close to" the Dictator game in payoff terms, induces significantly more selfish behavior by the proposer than the case $\delta = 1$. Indeed, the selfishness of proposals, $1 - s$, is non-monotonic in δ. When δ is low, selfishness is constrained by the fear of rejection, and $1 - s$ thus increases in δ. However, as the responder's power is eliminated ($\delta = 1$), the proposer on average becomes less selfish again. Essentially, Handgraaf et al.'s explanation is that the norm of social responsibility kicks in more strongly in extreme case of a completely powerless opponent; whenever the responder has some power, however small, proposers tend to feel more competitive and less socially responsible. We are thus left with the following reconciliation of the two sets of evidence: There is less ambiguity about the social norm in the Dictator game than in the Ultimatum game, and accordingly framing matters less.[28] In other words, our current view is the polar opposite of Fehr and Schmidt's (2006) hypothesis that Dictator games are "more fragile" than Ultimatum games (see Introduction).

A related but distinct reconciliation of our evidence with that of Leliveld et al. (2008) is that the low dependency Ultimatum game, while similar to the Dictator game in material terms, is still rather different in social terms, since rejection allows the responder to express a negative emotion or opinion about the fairness of the proposal. As shown by Ellingsen and Johannesson (2008) and Houser and Xiao (2009), even an anticipated verbal feedback message from the recipient can have a sizeable impact on allocations in a Dictator game. And as shown by Xiao and Houser (2005), rejections and verbal feedback are similar enough to act as substitute behaviors for responders in the Ultimatum game. It is quite possible that communication, either in the form of rejections or in the form of messages, serve to accentuate social norms in the dictator's mind (perhaps by reducing the dictator's experienced social distance or trigger instinctive fears of retribution), and with more accentuated norms, the labels should start to matter more. This argument too, at least partially, reconciles our null results in Studies 1 and 3 with the differences documented by Leliveld et al. A natural future test of this hypothesis is to investigate the role of social labels in a Dictator game with verbal feedback.[29] As always, it is also possible that the differences between the two sets of findings are caused by other differences in experimental design or subject pools, or even that one or both sets of findings are due to chance and will not survive replication.

[28] Note that in terms of modeling, norm ambiguity is better captured by the proposed generalization of (2) than by (2) itself.

[29] Another explanation is that procedures differ in the two studies. We are particularly concerned that subjects in a study conducted by psychologists are suspicious that they are being deceived or that payments are not real. (As it happens, Leliveld, van Dijk, and van Beest deceived their subjects, because they were not playing against another subject at all, and ultimate payments didn't depend on their behavior, despite instructions to the contrary.) In this case, some subjects may simply decide to neglect the monetary incentives and instead behave either according to some moral norm or as they think that the experimenters desire.

We also fail to detect a significant impact on behavior of recipient knowledge about the game. The latter finding is consistent with previous work by Koch and Norman (2008), as well as with the lack of a framing effect in Study 2 of Ellingsen et al. (2012), but raises questions about the interpretation of exit behavior in dictator games. Since the studies of Dana et al. (2006), we have thought that recipient awareness is crucial for exit decisions. But in view of the present results, it is natural to ask whether recipient awareness interacts with other factors that differ between their setting and ours, such as experimenter presence and social distance.

Our main conclusion is that preferences in Dictator games are less sensitive to social framing effects than we and many others have previously thought. It is straightforward to reconcile this finding with recent evidence on framing effects in Prisoners' dilemmas, as Ellingsen et al. (2012) suggest that the latter effects are mediated by beliefs about opponent behavior rather than by preferences. We also propose explanations that reconcile the absence of framing effects in Dictator games with the presence of framing effects in Ultimatum games. Evaluating these explanations experimentally is a natural avenue for further research on social framing effects.

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Appendix: Framing in a one-shot prisoners' dilemma on MTurk

To demonstrate the presence of framing effects on MTurk in games where both subjects make a decision, we recruited 400 subjects from MTurk in February 2011 to play a modified one-shot Prisoners' dilemma game. Subjects were randomly allocated to one of two framing treatments. All subjects were paid a show-up fee of 25 cents. The study took on average 4 minutes to complete.

Subjects were informed that they were playing either the 'Community game' or the 'Profit game.'[30] Then they read identical instructions for the following Prisoners' dilemma, indicated their decision (A or B), and completed a post-experimental questionnaire.

	A	B
A	\$0.15, \$0.15	\$0, \$0.20
B	\$0.20, \$0	\$0.05, \$0.05

Unlike in our Dictator game experiments, we do find substantial variation in Prisoners' dilemma cooperation across treatments (Community game: 65 % cooperation, Profit game: 58 % cooperation). To test for effects of the game name, we use logistic

[30] It has been previously demonstrated in the traditional (offline) laboratory that subjects cooperation more when the PD is called the 'Community game' relative to the 'Wall Street game' (Liberman et al. 2004). In this replication, we used the term 'Profit game' instead of 'Wall Street game' because of the particular negative valence attached to Wall Street in the mind of (at least) the American public at the time of the experiment.

Table 5 The effect of game name in a Prisoner's dilemma

	Community game vs profit game	
Profit Game	−0.306	−0.446[b]
	(0.207)	(0.221)
Age		0.00844
		(0.00986)
Female		0.327
		(0.226)
Constant	0.626[a]	−18.53[a]
	(0.149)	(1.403)
Country of residence dummies	No	Yes
Observations	400	378

Robust standard errors in parentheses
[a] $p < 0.01$, [b] $p < 0.05$, [c] $p < 0.1$

regression with robust standard errors, taking decision to cooperate (i.e. choose option 'A') as the dependent variable, and including controls for gender, age and country of residence. We find a negative effect of the 'Profit game' frame on cooperation (coeff $= -0.446$, $p = 0.043$; Table 5).

Thus we show that framing effects do occur on MTurk in games where both players make a decision. This supports our conclusion that the lack of framing effects in our Dictator games is the result of the unilateral nature of the dictator game decision setting.

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