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On the redistribution of wealth in a developing country: Experimental evidence on stake and framing effects

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ABSTRACT

We experimentally study the effect of framing and size of large windfall gains on the redistribution of such gains. Randomly selected individuals from villages in Bangladesh were invited to take part in dictator experiments where they received endowments worth up to five months of average household income and were asked to distribute the endowment between themselves and other individuals. We manipulated whether dictators could GIVE to or TAKE from another individual (i.e. whether the endowment was allocated to the dictator or other individual) and whether the endowment was moderate (LOW) or very large (HIGH). We also provided dictators with the option to reconsider their original decision. We find that dictators allocate almost nine times more to other individuals under the TAKE than the GIVE frame when stakes are HIGH, even after they could reconsider their choices. In addition, we find that proportions allocated to other individuals dramatically drop when stakes increase under the GIVE but not the TAKE frame. The results provide novel evidence on the role of framing and stakes for pro-sociality.

1. Introduction

Large income inequalities are common in many developing countries and are often viewed as one of the major reasons behind political instability (Alesina and Perotti, 1996), crime (Kelly, 2000) and unhappiness (Alesina et al., 2009). In these environments where opportunities to strike it rich are concentrated in the hands of a few and governments are unable and often unwilling to redistribute wealth, poorer individuals are often dependent on the altruism of the lucky and rich. Experimental research on charitable giving has provided important insights into circumstances under which individuals help others and subsequently helped design mechanisms to increase giving[1].

Given the importance of pro-sociality in wealth redistribution in developing countries, it is crucial to examine what factors affect the extent of pro-sociality in such societies. One important general factor that has shown to affect pro-social behaviour is the "frame" of the decision environment. There is now substantial evidence that framing can significantly influence individual choices. In particular, there is evidence that framing affects voluntary contributions in the context of redistribution experiments suggesting that framing can be used to nudge lucky, richer individuals to redistribute some of

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[1] For excellent summaries see Andreoni (2006) and List (2012).

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their wealth to needy, poorer individuals[2]. Another important factor that seems to affect pro-social behaviour is the size of the endowment (or the windfall gain, which is to be redistributed). There is some evidence that individuals behave differently if they need to share a very large sum as opposed to a moderate sum (Engel, 2011; Andersen et al., 2011)[3].

In this paper we use an artefactual field experiment conducted in rural Bangladesh to investigate how individuals redistribute windfalls of money and whether framing and the size of this windfall affects their pro-sociality. We invited randomly selected individuals from a set of villages to participate in dictator experiments where we manipulated frames and stakes. In these experiments, participants were either assigned the role of dictators (the lucky and rich) or recipients (the poor). Each dictator was provided with a monetary endowment by the experimenter and had to decide on the allocation of her endowment between herself and another anonymous participant (the recipient) who could not make any decisions. We employed a 2×2 across subject design in which we varied both the frame and the endowment (stake). In our frame treatments we varied whether the endowment was allocated to the dictator (GIVE frame) from which the dictator could give any amount they wished to their matched recipient or to the recipient (TAKE frame) from which the dictator could take away any amount[4]. Our stakes treatment varied whether the windfall was moderate – representing a typical a day's worth of earnings (LOW stake) or very large – representing 5 months worth of average earnings (HIGH stake).

We find in our field settings that redistributions of wealth significantly depend on both frames and stakes. Similar to the findings from studies using students in developed countries (see Engel, 2011; for a review), we find that dictator transfers are non-negligible. Importantly, we find that the proportion of endowment allocated to recipients dramatically differ across our treatments. In the GIVE frame, dictators allocate 16 percentage points less than in the TAKE frame. Allocations by the dictator are also significantly lower when stakes are HIGH than when they are LOW (corroborating findings in Engel, 2011). Additionally, we report a pronounced interaction effect between frames and stakes: there is almost no redistribution of wealth when stakes are HIGH and individuals are asked to GIVE (the HIGH-G treatment). Here the proportion allocated drops below 4%. In contrast, allocations to recipients are significantly higher when individuals are asked to TAKE, even if stakes are HIGH (the HIGH-T treatment). In this case, 33% of the endowment is allocated to the recipients.

In addition, we allow dictators to *reconsider* their allocation decision (approximately 30 min after they had made the initial allocation). We find that 40% of the dictators reconsider their allocations and that the use of the reconsider option depends on the frame. Allocation decisions are less robust under the GIVE than under the TAKE frame, where individuals are approximately twice as likely to reconsider.

Our findings provide novel evidence that framing and stake size affect transfers in a simple redistribution of wealth experiment. Both the documented framing and stake size effects are difficult to reconcile with standard economic theory and challenge the assumption that individuals have simple stable preferences, such as self-interest, altruism (Andreoni, 1990), inequity-aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), or follow simple fairness norms in dictator experiments (Andreoni and Bernheim, 2009). The findings are, however, consistent with other models that predict that frames affect social decision-making (see e.g. Krupka and Weber, 2013) and that 'taking' (but not 'giving') activates a notion of property rights, which motivates individuals to respect other people's possessions (see Stake, 2004). Our findings related to stakes are consistent with the argument that adherence to social norms is a normal good, and decreases with costs (see for example, Levitt and List, 2007).

Our study contributes to existing research in several important ways. Our paper adds to the limited literature of the effect of framing on dictator behaviour and to the best of our knowledge, this is the first attempt to study the role of framing in social decisions involving very large stakes[5]. More generally, our study adds to the rather sparse literature that investigates social decisions when stakes are very large (Slonim and Roth, 1998; Cameron, 1999; Carpenter et al., 2005; List and Cherry, 2008; Andersen et al., 2011). A second novel feature of our experiment is the *reconsider* option, which could be regarded as a tool to assess the robustness of individual choices over short periods of time.

There are a number of other studies investigating the role frames and stakes in social decision environments, albeit not in the context of developing countries. Dreber et al. (2013) report no framing effect in the context of dictator games when comparing dictator behaviour under TAKE and GIVE frames over relatively small stake sizes. List (2007) and Bardsley (2008) also conduct dictator game experiments and show that increasing the action space of the dictator from only having the ability to GIVE versus the ability to GIVE as well as to TAKE leads to lower levels of actual giving. Cappelen et al. (2012) show that these findings are robust even if dictators have to earn their endowments. All of these studies investigate the effect of framing over comparatively low stakes (typical 1–2 hours student wages). Our results using low stakes are consistent with the results presented in Dreber et al. (2013), which concludes no effect of framing. The focus on low stakes may constitute an important limitation as there is evidence that behaviour in dictator games changes as stakes increase (List and Cherry, 2008). Indeed, we show that framing effects are significant when stakes are very large.

[2] See Andreoni (1995), Dufwenberg et al. (2011) and Ellingsen et al. (2012).

[3] However it is not the case that stakes always matter in such an environment. For example Carpenter et al. (2005) show that that stakes do not matter in dictator or ultimatum games.

[4] The dictators received two envelopes (yellow and white). The yellow envelope was labelled "YOUR", and the white envelope was labelled "OTHER PERSON'S". In the GIVE frame, the endowment was placed in the "YOUR" envelope and the dictators could transfer money to the "OTHER PERSON'S" envelope. In the TAKE frame, the endowment was placed in the "OTHER PERSON'S" envelope and participants could transfer money to their own envelope. We discuss the experimental procedure in greater detail in Section 2.

[5] Carpenter et al. (2005) and List and Cherry (2008) use stakes up to US $100 for students in the U.S., which roughly translates into 1–2 days of salary.

Table 1
Summary of treatments.

Treatment	Endowments		Permissible transfer range	Sample size	No. of sessions
	Dictator	Recipient			
LOW-G	100	0	[0;100]	24	3
LOW-T	0	100	[−100;0]	24	3
HIGH-G	10,000	0	[0;10,000]	21	2
HIGH-T	0	10,000	[−10,000;0]	21	2

Notes: In the TAKE frame (treatments LOW-T and HIGH-T), the permissible allocation range is coded as being negative. In all treatments, dictators can choose allocations at multiples of 10.

2. Experimental design and setting

We use a 2 × 2 experimental design in which we vary stake size (LOW or HIGH) and frame (GIVE (G) or TAKE (T)) in the context of a dictator game (Table 1). The dictator game is the simplest two-player social decision setting where only one player, the dictator, makes a decision: she determines how to allocate and endowment of money between herself and another anonymous player, the recipient. The recipient makes no decision and receives whatever amount has been allocated to her by the dictator.

We therefore have two treatments with moderate stakes (LOW-G and LOW-T) and two with very high stakes (HIGH-G and HIGH-T). In the moderate stake treatments LOW-G and LOW-T the endowment was roughly equal to the average daily wage (Tk 100)[6]. The endowment was split in ten bills of 10 Taka and dictators could allocate any amount divisible by ten (0, 10, . . ., 100) to the recipient. In the HIGH stake treatments (HIGH-G and HIGH-T) the endowment was multiplied by a factor of 100, which approximately translated to a typical 5-months average household income (Tk 10,000)[7]. In the HIGH stake treatments the endowment was split into 10 bills of Tk 10 (as in the LOW stake treatment), 9 Tk 100, 12 Tk 500 and 3 Tk 1000 bills. Dictators could therefore potentially make allocations in increments of Tk 10.

While we employed two different frames, the experimental procedure was the same in both treatments. After the instructions were read out, each of the dictators received two envelopes (one yellow and one white). The yellow envelope was clearly labelled "YOUR", and the white envelope was clearly labelled "OTHER PERSON'S" (see Fig. 1). In the GIVE frame, which is the standard frame used in most dictator games, the endowment (i.e. the actual different bills) was placed in the "YOUR" envelope and participants could transfer money from it to the "OTHER PERSON'S" envelope. In the TAKE frame, the endowment was placed in the "OTHER PERSON'S" envelope and participants could transfer money from this envelope to their own envelope. While recipients knew in which treatment their dictator was (i.e. stake and frame), dictators were not explicitly informed in any treatment that recipients knew the details of their decision environment. The experimenter was always available to explain in case of confusion, and each dictator decided on her allocation decision in private and placed both envelopes in the decision box. The English version of the oral instructions explicitly used the words "GIVE" and "TAKE" for the two frames and are reproduced in Appendix 2.

The experimental sessions were conducted in six villages that were randomly chosen from all villages in a subdistrict in Bangladesh (Dhamrai) that were accessible by car. All these villages were located 30–40 km from the outskirts of the capital city Dhaka. In each of these villages we randomly selected participants by going to every *n*th household[8]. At the time of recruitment potential participants were told that they are invited to take part in a study, were informed of the venue and time, duration of the session and the show up fee (Tk 100). Members from all selected households showed up to the experimental sessions.

Summary statistics for selected demographic and socio-economic characteristics for the full sample and by treatment are presented in Table 2. The mean age of the dictators is 31.5 years. They have on average 7.5 years of schooling. The average household size is close to 5 and the average monthly household income is Tk 9240 (this is actually less than the endowment in the HIGH stake treatment). A third of the participants were women, more than two-thirds of the participants were married and on average participants lived in households with 4.6 members. Table 2 also presents the results from a randomization check and shows that there are no systematic differences in terms demographic and socio-economic characteristics of subjects allocated to the different treatments.

Our design implements a new feature to test for the robustness of choices: the *reconsider* option. We provided participants with the option of revising their initial choices. It is possible that framing effects are fragile and disappear if individuals have more time to revise their choices. Participants were not aware of the reconsider option when making their initial decision. We implemented the reconsider option in the following manner. After all participants made their initial choices, we asked

[6] Taka (Tk) is the local currency of Bangladesh. When the experimental sessions were conducted (February 2013) 100 Taka ∼ USD 1.22.

[7] The average monthly household income for our sample participants was Tk 9230 and the average household size of 4.6. The average per capita income is calculated as Tk 2008 per month. Thus the endowment of Tk 10,000 in the HIGH stake treatments translates to almost 5 month per capita household income.

[8] This *n* was calculated by dividing the total number of households in the village by the total number of participants required from that village. The average number of households in the villages was 690.

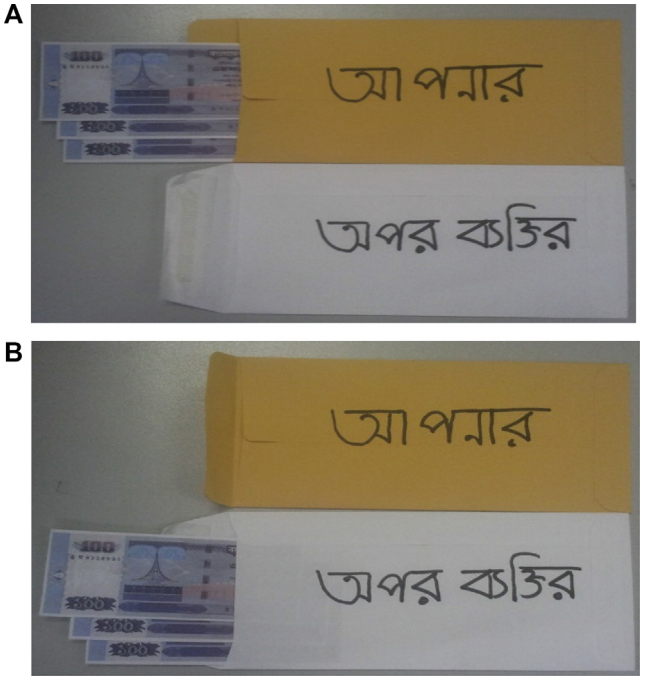

Fig. 1. Framing treatment through coloured envelopes. *Note*: On the yellow envelope the word written in Bengali (local language) translates to "Your". On the white envelope the word written in Bengali (local language) translates to "Other Person's".

participants to return to the decision booths where they received the two envelopes with the allocations that they made before. They were then told that *they can – but don't have to – change their allocation*. Every dictator, irrespective of whether or not they wanted to revise their allocation, had to go back to the decision booth. In addition, they were told that this was their final choice in this experiment. On average the lag between the initial decision and the final decision was for each individual approximately 30 min. During the waiting period participants were not allowed to chat or communicate with each other and this was strictly implemented. Note also that the recipients were not aware of the initial allocation made: they were only informed of the final allocation they received.

Our experimental procedure was designed to minimize anonymity concerns and those relating to close knit village networks—for example allocations might be higher if both the recipient and the dictator resided in the same village. We therefore conducted experimental sessions in a total of six villages and always simultaneously in two different villages of which one was assigned as the "dictator village" and the other as the "recipient village". Out of the six villages, three were assigned the role of the dictator village and three the role of the recipient village. The assignment into dictator and recipient village was determined through a lottery. In the dictator villages all participants were dictators, and in the recipient village

Table 2
Mean observable characteristics of sample participants across treatments.

Variable	Full sample	LOW-G	LOW-T	HIGH-G	HIGH-T	K–Wallis *p*-value
Age	31.53 (1.40)	37.58 (3.65)	30.50 (2.56)	29.10 (2.43)	28.24 (1.54)	0.41
Years of schooling	7.42 (0.54)	6.96 (0.97)	7.54 (1.03)	8.10 (1.04)	7.14 (1.33)	0.92
Income ('000s)	9.24 (0.73)	11.52 (1.71)	8.63 (1.26)	8.29 (1.10)	8.29 (1.58)	0.31
Marriage dummy	0.69 (0.05)	0.71 (0.09)	0.71 (0.09)	0.57 (0.11)	0.76 (0.10)	0.74
Female dummy	0.33 (0.05)	0.38 (0.10)	0.33 (0.10)	0.33 (0.11)	0.29 (0.10)	0.97
Household size	4.59 (0.19)	4.75 (0.35)	4.42 (0.38)	4.62 (0.38)	4.57 (0.40)	0.82

Notes: Standard errors are presented in parenthesis. Income denotes average income of the household in a month in thousand Tk (Taka).

Table 3
Aggregate behaviour across treatments.

Panel A: allocations by frame and stake					
	Pooled	GIVE	TAKE	LOW	HIGH
Allocation to recipient (Tk)	862.1	184.4	1539.8	27.1	1816.4
Allocation to recipient (%)	22.9	15.1	30.8	27.1	18.2
Allocation to recipient = 0	23.3	24.4	22.2	25	21.4
Allocation to recipient = 50% (equal split)	15.6	8.9	22.2	20.8	9.5
Allocation to recipient = 100% (all)	6.67	2.2	11.1	8.3	4.8
Panel B: allocations by treatments					
	Pooled	LOW-G	LOW-T	HIGH-G	HIGH-T
Allocation to recipient (Tk)	862.1	25	29.2	366.7	3266.2
Allocation to recipient (%)	22.9	25	29.2	3.67	32.7
Allocation to recipient = 0	23.3	12.5	37.5	38.1	4.7
Allocation to recipient = 50% (equal split)	15.6	16.7	25	0	19.4
Allocation to recipient = 100% (all)	6.67	4.1	12.5	0	9.5

Notes: Table reports means of the final choice.

all participants were recipients. The participants in the dictator village did not know from which village their recipient was (and vice versa)[9].

The experimenters communicated between the 'dictator' and 'recipient' villages using a mobile phone at the beginning and at the end of the sessions to ensure dictators could verify the existence of recipients[10]. Importantly, the procedure to increase credibility was identical across all treatments. Thus, treatment effects cannot be attributed to credibility issues. Each day we conducted three sessions in each of the two villages. Sessions lasted for approximately 90 min. Altogether 180 participants took part in our set of dictator experiments: we have 24 dictators in each of the LOW stake treatments and 21 dictators in each of the HIGH stake treatments.

The predictions using the most common economic theories in the different treatments are straightforward. If individuals are selfish, dictators will allocate the whole endowment to themselves and not allocate anything to the recipients, regardless of the treatment. If individuals have simple distributional preferences such as inequity-aversion they may allocate part of their endowments to recipients. However, the proportion allocated to the recipient should be the same across treatments. Thus, these models predict no treatment effects, i.e., irrespective of whether dictators are selfish or have simple distributional preferences, percentage allocated to the recipient should be invariant to stake and frame. In contrast, other models (Cialdini et al., 1990; Dreber et al., 2013; Krupka and Weber, 2013) predict that frames affect social decision-making. More precisely, these models argue that frames either increase the focus on a given norm or activate a different norm. One such norm could be a "property rights" norm (see Stake, 2004), which prescribes that individuals should respect other people's possessions. Assuming the TAKE frame activates such a norm, we should observe that allocations to recipients are larger in the TAKE compared to the GIVE frame.

3. Experimental results

Overall we observe that approximately 77% of the dictators allocate positive amounts and that they on average allocate 23% of their endowment. Panel A of Table 3 provides summary statistics pooling data either over the different frames or the different stakes. Framing affects dictator allocations (see Table 3, Panel A): in the GIVE frame dictators only allocate 15% whereas in the TAKE frame they allocate more than twice this fraction (31%) and this difference is statistically significant using a two sided Mann–Whitney U test (p-value = 0.009)[11]. There is also evidence that stakes affect overall allocations. In the two LOW stakes treatments dictators allocate 27% of their endowment whereas in the two HIGH stakes treatments they allocate only 18% though the difference is only marginally significant (p-value = 0.087; Mann–Whitney U test). However, while proportions allocated to recipients decline as stakes increase, the actual amount allocated still clearly increases from Tk 27 when stakes are LOW to Tk 1816 when stakes are HIGH. This is perhaps not a surprise as endowments increase by a factor of 100 when moving from the LOW to the HIGH stakes treatment.

[9] In Table A1 in the Appendix we compare the demographics of recipients and the dictators. While there are some differences in sample characteristics of the dictators and the recipients, only one is statistically significant at the 5% level: the years of schooling is significantly lower in the recipient villages. However, this is driven by participants in one recipient village, which has substantially lower level of educational attainment (3.07 years, compared to an average of 7.24 years for the other five villages).

[10] At the beginning of each session the experimenter in the dictator village made a phone call (in front of the dictators) to the experimenter in the recipient village to confirm that the total number of recipients matched the total numbers of dictators present in that session. At the end of the session, another such phone call was made where the amount allocated to the recipients were conveyed to the experimenter in the other village. This phone call was initiated in front of the dictators, but when the allocations were being conveyed the experimenter left the room to a safe distance where the dictators could not infer individual allocations from the session.

[11] All p-values reported are from two-sided tests.

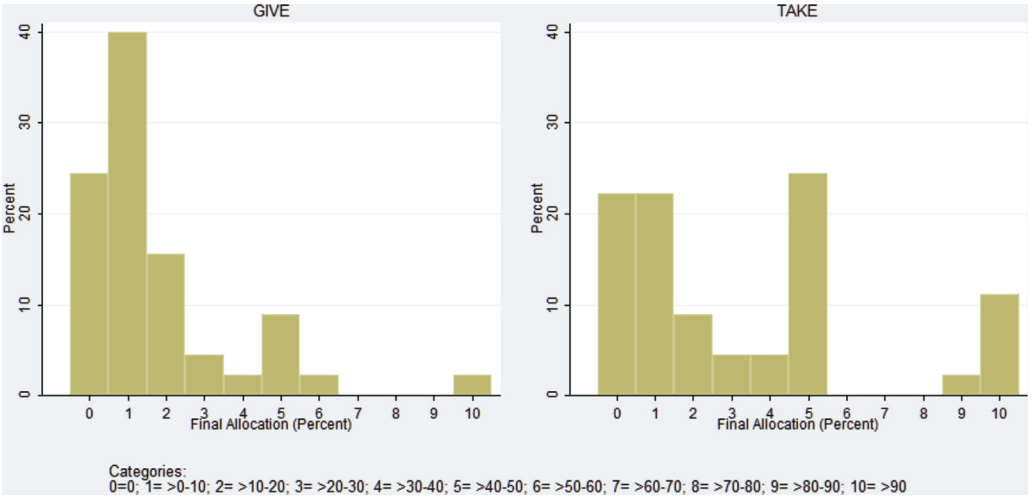

Fig. 2. Effect of framing on final allocations to recipient. *Notes*: In both GIVE and TAKE frame allocations have been pooled across stake size. The *X*-axis measures the percentage of endowment allocated to Recipient. The *Y*-axis measures the percentage of Dictators who chose to allocate that particular *X*-axis amount.

Further, we observe that while frames and stakes have no impact on the fraction of dictators who give nothing, both of our treatment dimensions matter for the proportion of dictators who choose equal split of endowments. While dictators choosing equal splits are rare in the GIVE frame (9%) they are more common in the TAKE frame (22%). Similarly when stakes are HIGH, 9.5% of dictators choose equal split, where as the corresponding proportion for the LOW stakes is 21. There is also a non-negligible fraction of dictators who allocate everything to the recipient only under the TAKE frame (11%) across both LOW (12.5%) and HIGH (9.5%) stakes (see Table 3, Panel B).

The corresponding distributions are presented in Figs. 2 and 3 where we vary the frame and the stake respectively. Not surprisingly the distributions are quite different both across frame (p-value = 0.054; Kolmogorov–Smirnov) and stake (p-value = 0.000; Kolmogorov–Smirnov).

3.1. Redistribution of wealth in each treatment

Fig. 4 presents histograms illustrating distributions of final allocations from the dictators to recipients across all of our treatments: LOW-G, LOW-T, HIGH-G and HIGH-T. Panel B of Table 3 provides additional summary statistics for all four treatments. Several insights follow. First, comparing LOW to HIGH stakes for each frame separately we find that stakes strongly affect distributions in the GIVE frame but not in the TAKE frame. More precisely, allocations equal or larger than 10% to recipients are very common in LOW-G (87.5%), but extremely uncommon in HIGH-G (less than 5%). The mode in

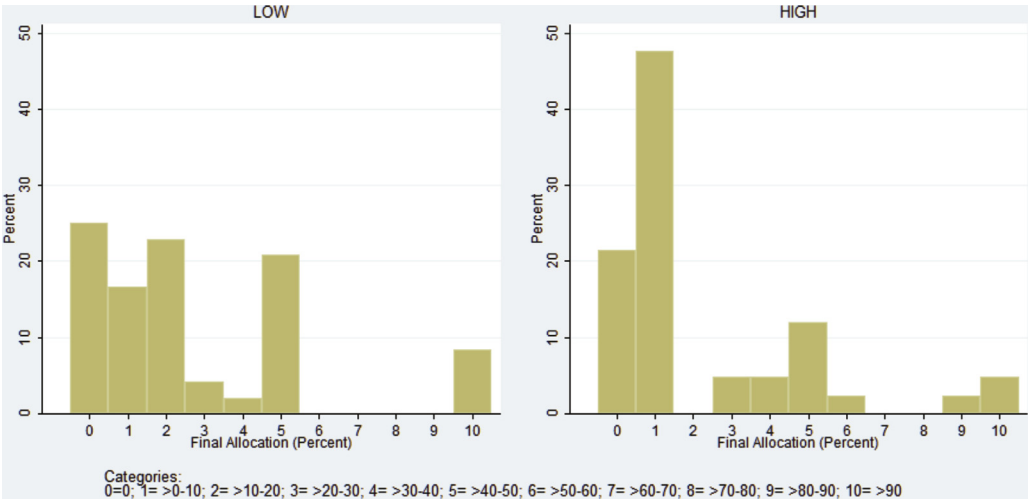

Fig. 3. Effect of stake size on final allocations to recipient. *Notes*: In both LOW and HIGH allocations have been pooled across frames. The *X*-axis measures the percentage of endowment allocated to Recipient. The *Y*-axis measures the percentage of Dictators who chose to allocate that particular *X*-axis amount.

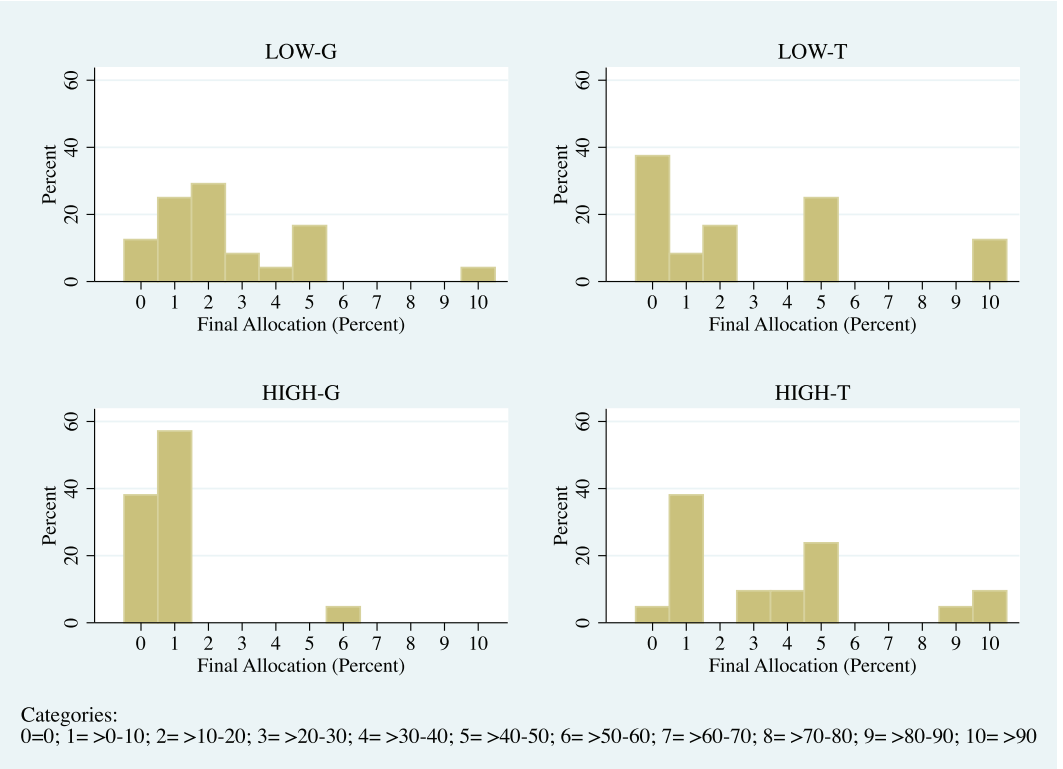

Fig. 4. Final percentage allocations to recipient by treatment. *Notes*: The *X*-axis measures the percentage of endowment allocated to recipient. The *Y*-axis measures the percentage of Dictators who chose to allocate that particular *X*-axis amount.

LOW-G are allocations in the 20–30% range, whereas in HIGH-G more than 95% allocate in the 0-10% range. On average, dictators allocate 25% in LOW-G but only 3.7% in HIGH-G, a highly significant difference (p-value < 0.001; Mann–Whitney). In fact, in HIGH-G, 81% of the dictators do not allocate more than 1% and only one dictator allocated more than 5%. Excluding this one participant from the analysis leads to an average allocation of only 0.85% in HIGH-G. In contrast, in the TAKE frame, stakes appear to not affect the distribution of allocations (difference between LOW-T and HIGH-T is not statistically significant, p-value = 0.375; Mann–Whitney). Across both stakes the mode is to allocate between 0 and 10% (46% in LOW-T versus 43% in HIGH-T). Additionally, a large fraction of participants allocate between 40 and 50% (25% in LOW-T versus 29% in HIGH-T). The distribution of allocation across the stakes treatments in the TAKE frame are not statistically different (p-value = 0.119; Kolmogorov–Smirnov).

Second, comparing across frames conditioning on stake size, we find that frames do not affect mean and distribution of allocations when stakes are LOW but do so when stakes are HIGH. On average, dictators allocate 33% in HIGH-T, compared to less than 4% in HIGH-G—a statistically significant difference (p-value < 0.001; Mann–Whitney). The distribution of allocations across HIGH-G and HIGH-T are also significantly different (p-value = 0.001; Kolmogorov–Smirnov). In contrast, when stakes are LOW, the difference in average allocation to recipients between the two frames is not statistically significant (25% in LOW-G versus 29% in LOW-T, p-value = 0.736; Mann–Whitney). The distributions of allocations are not different across the frames (p-value = 0.342; Kolmogorov–Smirnov).

Table 4 presents results from three OLS regressions, which shows that the previous findings are robust after clustering standard errors at the session level to account for potential within session correlations (all models), accounting for village level fixed effects (columns 2 and 3) and controlling for a set of individual characteristics variables (age, education, income, gender, household size, and marital status; column 3)[12]. From Panel B, which presents the mean difference between our treatments, we can observe that the average allocation to recipients in HIGH-G is significantly lower (p-value < 0.05) than that in LOW-G in all cases. The framing effect over high stakes is also validated: average proportion allocated to recipients in HIGH-T is significantly higher (p-value < 0.05) than that in HIGH-G. With regards to the individual characteristics, we find none statistically significant with p-value < 0.05[13].

[12] The reference category in each of the regressions is LOW-G. Findings using Tobit regressions are very similar and are available upon request.

[13] We do not find heterogeneous effects of our treatment across gender and wealth. However, women and the poor in our sample on average allocated lower amounts compared to men and the rich (Table A2).

Table 4
Regression results for final allocations to recipients across treatments.

	(1)	(2)	(3)
Panel A: regression estimates			
LOW-T	−4.167	−4.167	−4.13
	(8.419)	(8.581)	(8.79)
HIGH-G	−21.333**	−16.79**	−17.87**
	(7.557)	(5.612)	(5.71)
HIGH-T	7.662	3.115	4.82
	(12.366)	(7.566)	(7.46)
Age			0.16
			(0.17)
Years of schooling			0.52
			(0.89)
Income ('000)			−0.05
			(0.34)
Female dummy			2.10
			(6.09)
Household size			−2.48*
			(1.26)
Married			−9.23
			(12.76)
Constant	25.000***	25.00***	33.35*
	(7.332)	(4.017)	(15.41)
Panel B: difference estimate			
HIGH-G–LOW-G	−21.33**	−16.78**	−17.8**
LOW-T–HIGH-T	−3.49	1.05	−0.7
LOW-T–LOW-G	−4.16	−4.17	−4.12
HIGH-G–HIGH-T	−28.99**	−19.9**	−22.69**
Village fixed effects	No	Yes	Yes
Observations	90	90	90

Notes: Dependent variable: percentage of the endowment allocated to the recipient; OLS regressions; omitted category: LOW-G. Cluster robust standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Models 2 and 3 control for village fixed effects.

3.2. Reconsiderations of redistributions

There is no apparent reason to expect that dictators will make use of the reconsider option assuming that they have stable preferences and have not made mistakes. Yet, we find that 40% of the dictators use the reconsider option and revise their allocations. Interestingly, dictators tend to use this option more often to reduce than to increase initial allocations to recipients: 61% decrease whereas 39% increase initial allocations, conditional on choosing to reconsider (p-value = 0.099; Fisher exact): i.e., giving participants the chance to revise their choices leads to more selfish behaviour. This is consistent with the negative mean percentage change in the allocation to the recipients in the four treatments as presented in Fig. 5. Moreover, as we see from Panel A in Table 5, the reconsider option is twice as often used in the GIVE frame than in the TAKE frame (53.4% versus 26.7%, p-value = 0.010; Mann–Whitney), suggesting that behaviour in the GIVE frame is more

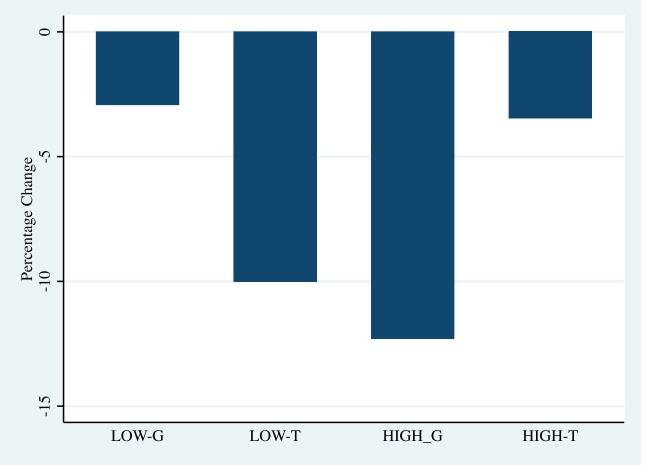

Fig. 5. Mean percentage change after reconsider by treatment. *Notes*: The *Y*-axis measures the difference (in percentage terms) between final allocation and initial allocation made to recipient.

Table 5
Usage of and revisions under the reconsider option across treatments.

	Pooled	GIVE	TAKE	LOW	HIGH
Panel A: allocations by frame and stake					
% of dictators using reconsider option	40.0	53.3	26.7	35.4	45.3
% of dictators making generous revisions	15.6	20.0	11.1	12.5	19.0
% of dictators making selfish revisions	24.4	33.3	15.6	22.9	26.2
Panel B: allocations by treatments					
	Pooled	LOW-G	LOW-T	HIGH-G	HIGH-T
% of dictators using reconsider option	40.0	50.0	20.8	57.2	33.3
% of dictators making generous revisions	15.6	25.0	0.0	14.3	23.8
% of dictators making selfish revisions	24.4	25.0	20.8	42.9	9.5

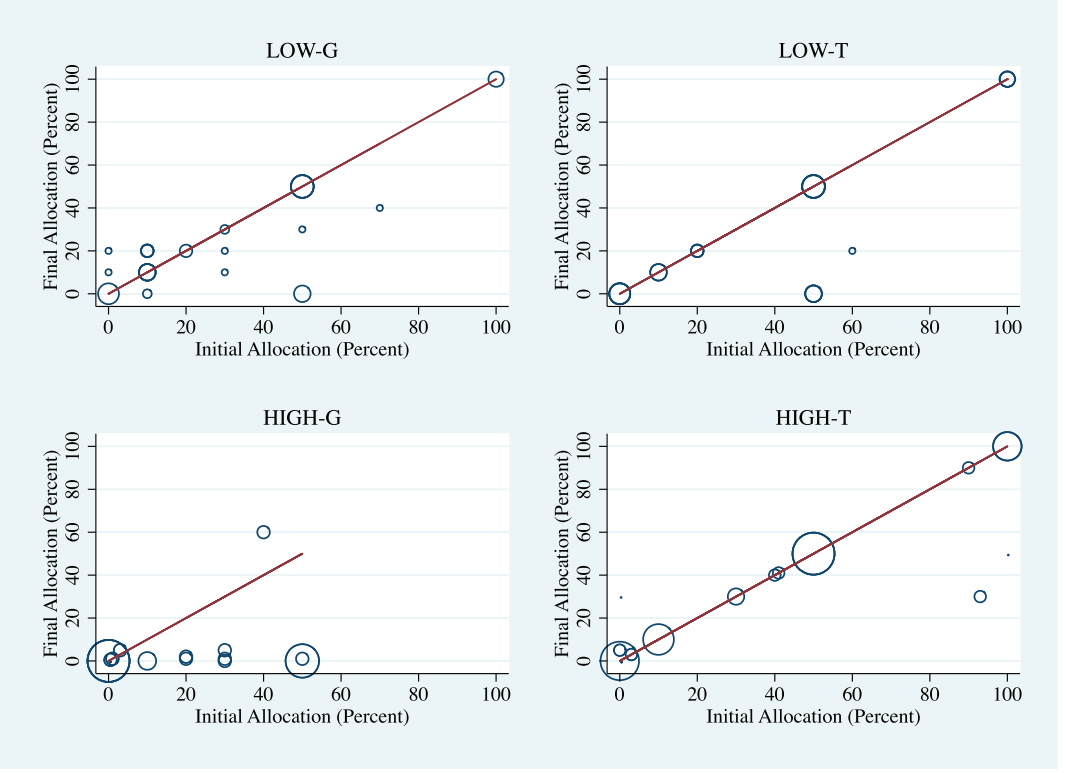

Fig. 6. Scatter plot for initial versus final allocation to recipients. *Notes*: The *X*-axis presents the initial allocations (%) made to recipient. The *Y*-axis presents the final allocations (%) made to recipient. The size of the scatters weighted by the frequency of the combination.

'fragile' than in the TAKE frame. In contrast, the difference in reconsiderations across stakes is not statistically significant (35.4% in the LOW stakes compared to 45.3% in the HIGH stakes treatments; p-value = 0.345; Mann–Whitney), suggesting that reconsideration decisions are largely independent of stakes.

Fig. 6 presents a scatter plot depicting the use of the reconsider option across our treatments and illustrates revisions in both dimensions (increase or decrease of initial allocations) in all treatments. In general, and this is true for all treatments, allocations become more selfish post reconsideration—the majority of scatters are below the 45 degree line (in the event of a change). This is particularly stark in the HIGH-G treatment where these reconsiderations contribute to the low final allocations in HIGH-G and the stark framing effect between HIGH-G and HIGH-T. Table 5, Panel B provides the corresponding summary statistics across the different treatments[14].

Columns 1 and 2 in Table 6 present the results from a hurdle regression where column 1 presents both the coefficient estimates and the marginal effects from a probit regression of choosing to reconsider and column 2 the OLS results of the

[14] Of the 36 individuals who chose to reconsider, 14 (39%) chose to revise towards equality. However at the same time, 22 (61%) individuals made selfish revisions and 14 (39%) made altruistic revisions. Interestingly, while 79% (11 out of the 14) of altruistic revisions were in fact revisions towards equality, this was only 13.6% (3 out of the 22) in the case of selfish revisions. These results suggest that people were more likely to make selfish revisions after reconsideration and most of these revisions were not towards equality.

Table 6
Regression results for usage of and revisions under the reconsider option.

	(1) Use of reconsider option	(2) Proportion change (conditional on choosing reconsider option)	(3) Proportion change
Panel A: regression estimates			
Initial % allocated to recipient	0.00	−0.74***	−0.26***
	(0.007) [0.00]	(0.101)	(0.061)
LOW-T	−0.98*	−25.69**	−4.36
	(0.517) [−0.32**]	(9.536)	(4.906)
HIGH-G	−0.12	−6.40	−11.00**
	(0.430) [−0.04]	(7.157)	(5.466)
HIGH-T	−0.39	−3.67	1.47
	(0.239) [−0.14*]	(8.655)	(5.628)
Age	−0.02	0.30	0.24
	(0.012) [−0.01*]	(0.231)	(0.174)
Years of schooling	−0.11**	0.76	0.56
	(0.053) [−0.04**]	(1.010)	(0.494)
Household income (Tk '000)	0.01	0.03	−0.26
	(0.028) [0.00]	(0.421)	(0.287)
Female	−0.49	9.26	4.04
	(0.476) [−0.18]	(7.100)	(5.828)
Household size	0.15	−1.15	−3.04***
	(0.100) [0.06]	(1.930)	(1.070)
Married	−0.68	−10.36	0.21
	(0.434) [−0.26]	(7.640)	(5.450)
Constant	1.62	−2.59	6.41
	(1.318)	(17.222)	(11.309)
Panel B: difference estimates			
LOW-G–HIGH-G	−0.12	11.6	11.0*
LOW-T–HIGH-T	−0.58	−22.01**	−6.9
LOW-G–LOW-T	0.98**	25.69**	4.4
HIGH-G–HIGH-T	0.27	−14.5*	−12.5*
Sample size	90	36	90

Notes: Column (1) presents the coefficients from probit regression on whether the dictator chose to use the reconsider option ($y = 1$ if dictator chooses to use the reconsider option and $y = 0$ otherwise). The marginal effects are presented in square brackets [x]. Columns (2) and (3) present the percentage change. Sample in column (2) conditional on choosing the reconsider option. In column (3) percentage change is 0 for subjects not choosing the reconsider option. All models control for village fixed effects. Cluster robust standard errors in parentheses. Omitted category: LOW-G.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The difference estimates give the differences between the estimated coefficients.

magnitude of change should the subject choose to reconsider[15]. We find that the usage of the reconsider option is less likely in LOW-T compared to that in LOW-G (p-value = 0.024). We however do not find a difference across frames over HIGH stakes. The magnitude of the selfish revisions (conditional on making revisions) is higher in LOW-T compared to LOW-G treatments. We also observe that the magnitude of the selfish revision is greater in LOW-T compared to HIGH-T. If we focus on unconditional revisions, we observe that the magnitude of the selfish revisions is greater in HIGH-G compared to LOW-G and greater in HIGH-G compared to HIGH-T. We also find that while the initial allocation does not affect the use of the reconsider option, it does affect the magnitude of the change: 1% increase in initial allocation is associated with a 0.73% decrease in the allocation post reconsideration[16]. This is perhaps not surprising that people who chose to revise downwards are more likely to have made higher initial allocation. We also observe that educated participants have a lower likelihood of using the reconsider option (Column 1), while participants from smaller households make selfish revisions (Column 3).

4. Discussion

Altruism and redistribution is of particular importance in environments where many individuals are in need and governmental support is lacking. Our study takes place in Bangladesh, a major developing country, where such conditions are present and where we investigate the redistribution of large windfall gains using an artefactual field experiment. We randomly invited inhabitants from a set of villages and asked them to allocate up to 5-month wages between themselves and an anonymous participant from another village. Besides manipulating the size of the stakes, we also manipulated the framing of the decision environment. We find that we can increase average allocations to other participants by almost a one-month salary when changing the frame of the decision environment from 'giving to another participant' (GIVE) to 'taking from

[15] The magnitude of unconditional changes is presented in column 3.
[16] There is however no evidence of non-linearity in this relationship.

another participant' (TAKE). In addition, we find that the proportions allocated to others dramatically drop when stakes increase and allocations are framed as 'giving'.

We allow dictators to reconsider their choices, half an hour after they had made their initial choices, and almost half actually do so. We find that reconsiderations are much more common when using the GIVE frame instead of the TAKE frame and that reconsiderations lead to less altruistic behaviour if stakes are very large (HIGH). These are important findings in light of the fact that almost all dictator experiments are conducted using the GIVE frame. It suggests that the behaviour observed in the standard dictator game is fragile and cannot be readily extrapolated to environments in which individuals have the chance to revise their choices. On the other hand, the level of altruism in our dictator experiment with HIGH stakes using the GIVE frame after the reconsiderations is much more in line with actual levels of charitable donations than standard dictator experiment finding, suggesting that observations in dictator experiments may translate well to out-of-lab giving[17].

More generally, multiple studies have shown that allowing individuals to "cool off" affects decision-making. For example, Oechssler et al. (2009) show that allowing participants to cool off led to lower rates of rejections of unfair offers, particularly when stakes are large. Similarly, other studies show that already 10 additional minutes time to think about proposals in the ultimatum game reduces the willingness to reject unfair offers (Cappelletti et al., 2011; Grimm and Mengel, 2011; Rand et al., 2012; Neo et al., 2013). These findings show that giving individuals time to think about their choices systematically suppresses the level of altruism (and altruistic punishment) suggesting that individuals act more in line with standard economic theory, perhaps because they have overcome their first initial instincts to give (and redistribute) and thus are better able to rationalize their selfish behaviour.

Our findings show the power of frames for altruism and that framing effects can be stark even when stakes are very large. In fact, we show that framing effects are more pronounced when stakes are HIGH than when they are LOW. Our findings are consistent with the idea that frames affect decisions because they induce social norms. More precisely, in our TAKE frame it seems possible that we induced a property norm that prevents individuals from taking wealth from others even if they can. The apparent respect of property in poor regions even when the property is unprotected and the result of large windfalls can be of great importance for policy makers and managers. Many developing countries have extensive natural resources and property rights are not well defined. Perhaps, assigning some sort of property rights (even if community property rights) instead of leaving property rights unspecified for natural resources may prevent individuals from grabbing and exploiting them at the expense of others.

Our study suggests that framing may be a powerful tool to increase the redistribution of wealth, from the rich to the poor. One limitation of our study is, however, that we study only wealth caused from windfall gains. It may very well be that framing is less effective when wealth is the result from hard work. Cappelen et al. (2012) suggest that the effect of framing is similar across income earned from both windfalls and real effort tasks when stakes are relatively low. Future research may address whether framing effects are still present when stakes are large and wealth is not the result of windfall gains.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jebo.2015.02.015.

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[17] We find that individuals on average give 3.7% of their endowment to others in treatment HIGH-G as compared to typical mean allocations larger than 20% (see e.g. Engel, 2011). 3.7% is closer to typical rates of charitable giving. For example, in Bangladesh only 17% of the population gave money to charity at all (see Charities, 2013).

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