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Delaying and motivating decisions in the (Bully) dictator game

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ABSTRACT

We investigate experimentally how decisions in the Dictator Game are affected by cognitive manipulations aimed at promoting greater reliance on deliberation. Specifically, we run an online experiment where we have 6 distinct experimental conditions resulting from the combination of 2 conditions for the Dictator Game (*non-bully*: the dictator is initially endowed with all the money; *bully*: the initial endowment is equally split), and 3 conditions for the cognitive manipulations (*time delay*: decisions are delayed; *motivated delay*: decisions are delayed and a written motivation is required; *control*: no manipulation). We find that the equal initial endowment leads the dictator to get less, confirming in the online setting previous evidence from the lab. Further, our findings suggest that the request to write a motivation makes subjects take less for themselves with respect to the mere request to wait some time before choosing.

1. Introduction

Manipulation of cognition has been widely applied to explore how different modes of cognition affect altruistic and prosocial behavior (Capraro, 2019; Fromell, Nosenzo, & Owens, 2020; Kvarven et al., 2020; Rand, 2019). The literature has mostly focused on the distinction between intuition and deliberation (Kahneman & Egan, 2011), rooted in the dual process theory of cognition (Evans & Stanovich, 2013). An array of experimental conditions has been designed to induce greater reliance on intuition or deliberation. Some conditions use time-related manipulations, others rely on priming or involve tasks that require cognitive effort. However, it is unclear to what extent the impact on behavior is sensible to the specific features of these conditions, i.e., the specific mechanism through which they affect prosocial behavior (Chen & Krajbich, 2018; Crosetto & Güth, 2021). Regarding the conditions aimed at promoting reliance on intuition, although there is no systematic comparative study on their effects, some evidence has been accumulated which suggests that relying on time pressure (Alós-Ferrer & Garagnani, 2020a; Merkel & Lohse, 2019; Teoh, Yao, Cunningham, & Hutcherson, 2020), cognitive load (Achtziger, Alós-Ferrer, & Ritschel, 2020; Deck & Jahedi, 2015; Schulz, Fischbacher, Thoni, & Utikal, 2014), or ego depletion (Alós-Ferrer, Ritschel, García-Segarra, & Achtziger, 2019; Baumeister, 2019; Baumeister, Bratslavsky, Muraven, & Tice, 1998) may not be neutral (Cappelletti, Güth, & Ploner, 2011; Capraro & Cococcioni, 2016; Evans & Rand, 2019; Grossmann, Brienza, & Bobocel, 2017; Tinghög et al., 2016). In contrast, no substantial evidence has been produced regarding the conditions aimed at promoting reliance on deliberation.

It is widely believed that requiring decision-makers to wait before actually making a decision induces, on average, greater reliance on deliberation (Horstmann, Hausmann, & Ryf, 2009). More recently a new method, where experimental subjects are required to motivate their decision with a written text, has been applied with the aim of inducing greater reliance on deliberation (Bilancini, Boncinelli, & Celadin, 2022; Bilancini, Boncinelli, & Luini, 2019; Bilancini, Boncinelli, & Spadoni, 2021). This new method yields a delay in decision times, since writing down a motivation requires time, but it can also trigger a specific form of reflection aimed at finding a motivation. Therefore, the time delay condition and the motivated delay condition, while both hinging upon time delay, differ one from the other because the latter also requires actively reflecting on one's own motivations while the former typically

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does not. While in many situations this difference may not play any substantial role, we conjecture that it does play a role in social contexts where moral judgment and social norms are relevant. The reason is that actively thinking about a motivation for one's own decision may affect the salience and relevance of moral judgments and social norms. For instance, by asking a decision-maker to motivate why she wants to allocate money between herself and another person in a certain way, we can expect to make the decision-maker reflect upon what she, her peers, or the society, think to be the right thing to do.

The Dictator Game (Engel, 2011; Forsythe, Horowitz, Savin, & Sefton, 1994) is a simple game where the decision-maker is called to allocate money between herself and another recipient. Decision-making in dictator games are likely to involve both moral judgment and social norms. More precisely, we employ the two variants of the Dictator Game known as "bully" and "non-bully" (Krupka & Weber, 2013). In the bully variant the initial endowment of money is equally distributed between the decision-maker and the other recipient, while in the non-bully variant the decision-maker is initially endowed with all the money. This difference in initial endowments is likely to trigger different judgments of what is fair or acceptable, individually or socially. The reason for this is that the fifty/fifty initial endowment is likely to trigger a stronger deontological resistance as a consequence of the fact that taking from others – e.g., getting some extra payoffs in the bully version – might be perceived as more normatively inappropriate than not giving – in the non-bully version – as found by Krupka and Weber (2013). Accordingly, manipulating initial endowments is likely to induce subjects to focus on the normative appropriateness of actions *per se*, rather than on the consequences of decisions on final allocations, thus making deontological considerations more salient than utilitarian (Baron & Goodwin, 2020, 2021; Gawronski, Armstrong, Conway, Friesdorf, & Hütter, 2017; Gawronski et al., 2020).

In this paper we run an experiment to compare the effects of a time delay condition and a motivated delay condition (with respect to a baseline condition) on allocation decisions in the Dictator Game, for both the bully and non-bully variants. This is meant to address our primary question on potential differences between the time delay condition and the motivated delay condition in a setting that is prone to moral judgments and where social norms are likely to play a role. Thus, we aim at contributing to the mixed and non-conclusive evidence produced in the literature on the effect of deliberation in the dictator game (Andersen, Gneezy, Kajackaite, & Marx, 2018; Carlson, Aknin, & Liotti, 2016; Chuan, Kessler, & Milkman, 2018; Grolleau, Sutan, El Harbi, & Jedidi, 2018; Mrkva, 2017; Tinghög et al., 2016; see Capraro, 2019 for a review), while further exploring the role of normative and moral orientations in such decision context (Brañas-Garza, 2007; Capraro, Jagfeld, Klein, Mul, & de Pol, 2019; Capraro & Vanzo, 2019; Chang, Chen, & Krupka, 2019; Hillenbrand & Verrina, 2022; Krupka & Weber, 2013; see Capraro, Halpern, & Perc, 2022 for a review). To this purpose, we also elicit subjects' empirical expectations, normative beliefs and normative expectations (Bicchieri, 2005, 2016; Bicchieri & Xiao, 2009). Empirical expectations are what we expect others to do. Normative beliefs are our beliefs about what action or behavior one ought to undertake. Normative expectations are our expectations about what others think we should do. We attempt to assess the role of empirical expectations, normative beliefs and normative expectations in allocation choices.

The experimental evidence that we collect provides a number of findings. First, the dictator gets less in the bully variant, confirming in the online setting what has been found in a lab setting (Krupka & Weber, 2013). Second, in the motivated delay condition the dictator gets less with respect to the baseline condition, while this is not the case for the time delay condition. Third, differently from what has been found in a lab setting (Krupka & Weber, 2013), the norms elicited for the bully Dictator Game are not significantly different from those elicited for the non-bully Dictator Game. This might be due to the different within-subject methodology adopted for eliciting empirical expectations, normative beliefs and normative expectations. Notwithstanding, this methodology let us single out the role of personal normative beliefs as a potential channel for the effect of motivated delay on decisions.

Overall, our findings suggest that requesting decision-makers to motivate their decisions may affect behavior beyond the mere fact that providing a motivation requires time, at least in the context like the Dictator Game. In particular, since asking to provide a motivation leads dictators to get less and be closer to the fifty-fifty split, we can conjecture that asking people to motivate their decisions is an effective tool to promote decisions which are more in line with moral preferences and norms.

2. Methods

The experiment was implemented in oTree (Chen, Schonger, & Wickens, 2016) and conducted on the online platform Prolific.co (Palan & Schitter, 2018) in July 2020. An initial pilot with 180 participants overall was conducted for exploratory purposes, but the data gathered from the pilot was not utilized in the present study. The experiment involved 1022 participants living in the US at the time of the experiment and lasted on average 6 min. Participants received a participation fee of 0.32 USD and an average payoff of 0.34 USD. The resulting overall payment is compliant with the prescriptions for online experiments on Prolific.co. The design and the analyses were pre-registered on AsPredicted.org (pre-registration number 44855).

2.1. Experimental conditions

Cognition was manipulated by means of two conditions: time delay and motivated delay. In the Time Delay (TD) treatment participants were asked to wait 40 s before making their decision; in the Motivated Delay (MD) treatment participants were asked to write a short text (of at least 40 characters) providing motivation for their choice while also waiting 40 s before making their decision; lastly, in a Control (C) treatment participants were asked to make their decision without any time constraint or request of motivations.

Following Krupka and Weber (2013), we attempted to manipulate perceived norms by presenting subjects either with a standard Dictator Game (DG) or a Bully Dictator Game (BDG). In the DG the dictator decided how to allocate 0.60 USD between herself and the receiver, starting from an initial allocation of 0.60 USD for the dictator and 0 for the receiver. In the BDG, the dictator decided how to allocate the same total amount, but starting from the equal split of 0.30 USD for the dictator and 0.30 USD for the receiver.

In order to use individual-level data regarding perceived norms, we elicited in each condition participants' personal normative orientations and social expectations referred to the decision task (Bicchieri & Chavez, 2010; Bicchieri & Xiao, 2009), providing material incentives for empirical and normative expectations, not for normative beliefs. We also administered a variant of the Cognitive Reflection Test (Frederick, 2005; Primi, Morsanyi, Chiesi, Donati, & Hamilton, 2016; see Brañas-Garza, Kujal, & Lenkei, 2019 for a meta-study) and, lastly, a self-reported measure of how much the participants reflected in the main task. This latter measure was added in order to have an alternative way to evaluate the effectiveness of cognitive manipulations to induce reflection.

The 3 × 2 scheme in Table 1 summarizes the six conditions to which participants were randomly assigned and reports the number of subjects in each of them. The slight unbalance between treatments and, in particular, the decrease in the number of observations in the time delay treatment are not unexpected. The occurrence of dropouts is one of the limitations of the online experimental setting. However, this occurrence is rare in our experiment (3.13 percent drop out from the experiment overall and only 1.80 percent during the task) and, at least to some extent, it can be ascribed to exogenous factors. For instance,

Table 1
Number of participants for each of the 3 × 2 experimental conditions.

	Control	Time Delay	Motivated Delay
Standard Dictator Game	DG-C (N = 174)	DG-TD (N = 169)	DG-MD (N = 166)
Bully Dictator Game	BDG-C (N = 176)	BDG-TD (N = 174)	DG-MD (N = 163)

Note. *Control*: decision without any time constraint or request of motivations. *Time Delay*: decision delayed 40 s. *Motivated Delay*: decision with a written motivation and delayed 40 s. *Standard Dictator Game*: Dictator initially endowed with 0.60 USD who decides how much to give to the receiver. *Bully Dictator Game*: Dictator initially endowed with 0.30 USD who decides how much to give or take from the receiver initially endowed with 0.30 USD.

Table 2
Differences across treatments of socio-demographic characteristics.

Variable	Test	p (by treatment)	p (by cogn. manip.)
CRT right	Kruskal–Wallis	0.272	0.378
CRT int	Kruskal–Wallis	0.221	0.280
Female	χ^2	0.527	0.488
Age	Kruskal–Wallis	0.510	0.545
Household income	Kruskal–Wallis	0.286	0.404
Children	χ^2	0.821	0.669
Full time job	χ^2	0.171	0.688
Christian	χ^2	0.339	0.518
Student	χ^2	0.994	0.913
Democrat	χ^2	0.549	0.398

Note. *CRT right* (cognitive reflection test with "right" as number of correct answers): integer from 1 to 6. *CRT int* (number of intuitive answers): integer from 1 to 6. *Female*: 1 if female; 0 if male. *Age*: integer number *Household income*: variable ranging 0, 1, … , 12 where 0 is household income < 10 000 USD and 12 is household income > 150 000 USD. *Children*: 1 if household with at least one child; 0 if not. *Full time job*: 1 if full time employment; 0 if not *Christian*: 1 if Christian; 0 if not. *Student*: 1 if student; 0 if not *Democrat*: 1 if Democrat; 0 if not. The third and the fourth column list respectively the probabilities, *p*, of having a different distribution across the 6 experimental conditions and across the 3 cognitive manipulations with respect to the tests indicated in the second column.

the time spent on the main task is correlated with the probability of unintentional dropout, and participants spent on average more time in the treatments where we had more dropouts (see Section 3). Importantly, the occurrence of these events (such as disconnecting from the internet) is fully random and hence does not introduce any bias. On the other hand, we cannot in principle exclude that sample selection is endogenous to treatments. To check for this, we can look at the composition of samples by version of the dictator game and cognitive manipulation with respect to the CRT. For the CRT we build two distinct variables, one counting the number of correct answers ("CRT right", ranging from 0 to 6), which measures the inclination to reflect, and one variable counting the number of intuitive wrong answers ("CRT int", ranging from 0 to 6). Although CRT right is a widely used and reliable measure of cognitive reflection (Jimenez, Rodriguez-Lara, Tyran, & Wengström, 2018), CRT int is considered to be less reliable due to its weak correlation with other measures of reliance on intuition (Pennycook, Cheyne, Koehler, & Fugelsang, 2016). However, Jimenez et al. (2018) argue that distinguishing intuitive answers from generally incorrect responses is a best practice for measuring intuitive thinking. The estimates for the CRT are not necessarily in contrast with the estimated treatment effects, since the CRT measures the "dispositional reflection" while our cognitive manipulations induce "situational reflection". The third and the fourth column of Table 2 respectively show that the composition of samples by version of the dictator game and by cognitive manipulation are not heterogeneous with respect to CRT and several socio-demographic characteristics.

2.2. Experimental protocol

The online setting requires simple and readable instructions to avoid the risk that contextual conditions may distract participants and drive their attention away from the experimental tasks. To this purpose, we provided very succinct and plain instructions minimizing the cognitive load and reading time, using graphical elements and user-friendly tools. All screens shown to participants during the experiment can be found in the Section A of the Supplementary Material.

After giving their informed consent, participants were presented with the task description explaining that each participant would have been paid as dictators or receivers of other dictators' decisions with a 50% probability. In the same screen participants were told what task they would have to go through, depending on the treatment: for C simply make a decision, for TD wait for forty seconds before making a decision, and for MD wait for forty seconds and write a motivation before making a decision.

Both the DG and the BDG were presented on the next screen, in the way shown in Fig. 1. As can be noted, the difference between the DG and the BDG lies in the initial allocation of the endowments, which was highlighted by means of a graphical distribution of six ten-cent coins. In this way we could provide participants with a simple and clear picture of initial endowments without having them to read and think too much about. Subsequently, participants could alter the initial distribution by operating the slider below the cells, moving one by one the coins from one box to the other and then, when satisfied with their choice, implement their decisions by clicking on the "confirm" button. By devising the decision task as a concrete manual operation causing a perceivable change in the representation of the coin allocation – rather than for example a merely numerical list of distributions in a radio button – we aimed at reproducing online the setting of the decision task in Krupka and Weber (2013) (i.e., the concrete operation of moving money from two envelops respectively addressed to the dictator and the receiver) making both the initial allocation and the decision to change it more salient and tangible to participants.

The actual screen of the decision task varied depending on the treatments. In the C treatment participants were immediately allowed for operating the slider and click the confirmation button. In the TD treatment participant could not operate the slider and confirm the chosen allocation for the first 40 s, with both the slider and the button kept invisible during this time. The same happened in the MD treatment with the only difference that during the first 40 s participants were asked to write a motivation long at least 40 characters; in particular, the slider and the confirmation button became clickable only when the participants had typed at least 40 characters.

Two reasons motivated our decision to hide the slider and the confirmation button for the initial 40 s in TD and MD. In the first place, by making the initial allocation not modifiable for 40 s we further highlighted endowments. In the second place, by keeping the same protocol we maximized the comparability of the TD and MD treatments. This would not have been the case if operating the slider was made available: moving coins would have become an action in alternative to writing the motivation.

The task was immediately followed by the elicitation of norms. We followed Bicchieri and Xiao (2009) and Bicchieri and Chavez (2010) and measured empirical expectations (EE), personal normative beliefs (NB) and normative expectations (NE). Accordingly, in each experimental treatment, we asked subjects the following set of questions to elicit EE, NB and NE, respectively:

EE Consider the allocation task you faced.
Which allocation do you believe is chosen mostly by the other participants?

NB Consider again the allocation task you faced.
Which allocation do you believe ought to be chosen?

NE Which allocation do you believe is chosen mostly by the other participants in the above question?

We separated in two different screens the question regarding EE from the questions regarding NB and NE in order to minimize cross-contamination of responses. Only EE and NE were incentivized,

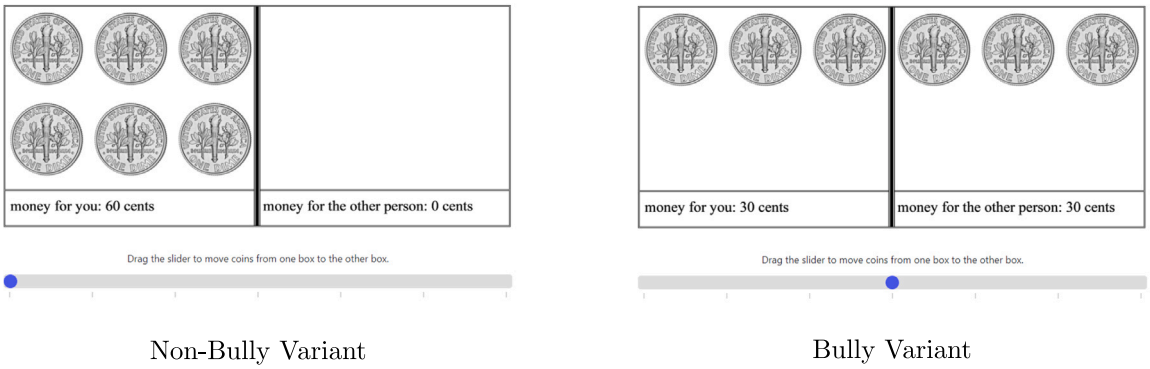

Fig. 1. Task screens of the Standard Dictator Game and the Bully Dictator Game. The dictator changes the initial allocation by operating the slider below the boxes. As the position of the slider changes, the coins move between the two boxes representing the dictator's account (left) and the receiver's account (right), respectively.

specifically by paying 0.10 USD for each correct answer. The very small magnitude of the incentives and the lack of transparency of the potential for hedging risks (by coordinating answers to these questions and the choice in the task) suggest that the risk of behavior being influenced by hedging motives is minimal (Blanco, Engelmann, Koch, & Normann, 2010).

Subsequently, a modified 6-item Cognitive Reflection Test (CRT) (Primi et al., 2016) task was administered, where we slightly reworded the first three well-known items taken from Frederick (2005) in order to avoid the recognition of these tasks by subjects.

Lastly, we asked participants to report how much they carefully reflected on the main task. Specifically, we asked participants how much they agreed (measured on a 0-to-10 Likert scale) on a sentence stating that they carefully reflected on the allocation task.

2.3. Research questions and behavioral hypotheses

The described experimental setup was designed in order to answer the following research questions, that we preregistered on AsPredicted.org (pre-registration number 44855):

RQ.1 Are allocation choices in DG different under TD, MD and C? If this is the case, in which treatment the dictator gets a larger share?

RQ.2 Are allocation choices in the BDG different under TD, MD and C? If this is the case, in which treatment the dictator gets a larger share?

RQ.3 Do allocation choices under TD, MD and C differ between DG and BDG? If this is the case, in which variant of the game the dictator gets a larger share?

Based on the related literature, we can make, for each of these questions, a hypothesis concerning the expected effects of our experimental conditions. It is not the first time that the dictator game is used online to measure altruism in relation to expectations. Despite substantial differences between men and women, the standard lab evidence has been replicated (Brañas-Garza, Capraro, & Rascon-Ramirez, 2018) and we consequently posit positive average amounts allocated to recipients as a benchmark prediction. However, the use of cognitive manipulations and their interaction with different versions of the dictator game is new in an online setting. Consequently, it is important to note that our behavioral hypotheses – which were not preregistered – are intended to situate our contribution within the existing literature of laboratory experiments.

As concerning RQ.1, cognitive manipulation has been widely applied to the standard dictator game design, but the results have been mixed (see Capraro, 2019 for a review) and indicate that cognitive manipulations have heterogeneous effects on altruism depending on characteristics of subjects and decision contexts. Rand, Brescoll, Everett, Capraro, and Barcelo (2016) run a meta-analysis of 22 experiments applying cognitive-processing manipulations to the dictator game, finding no effect except for the fact that intuition increases giving among women. Fromell et al. (2020) complement this meta-analysis by adding other 42 experiments – designed with a passive receiver of a decision maker's choice but either framed as a dictator game (two-thirds of the analyzed experiments) or as a charitable donation – finding no overall differences in decisions due to the promotion of intuition *versus* deliberation and only a mildly significant negative effect of intuition on men. Moreover, underlying prosocial attitudes moderate the effect of manipulation of response times, which produces, in the case of time delay, a negative effect for the altruists and a positive for the selfish (and the opposite in the case of time pressure) in a series of mini-dictator games (Chen & Krajbich, 2018). A substantial portion of the literature specifically focusing on time delay finds no effect (Andersen et al., 2018; Carlson et al., 2016; Tinghög et al., 2016), while there are studies where delaying the dictator's decision results either in more giving (Mrkva, 2017), or less (Chuan et al., 2018; Grolleau et al., 2018). Overall, this evidence makes us hypothesize that time delay *per se* has a mildly or not significant effect on allocation decisions in our standard dictator game (with the direction of this potential effect dependent on sample characteristics).[1]

To the best of our knowledge, the impact of the motivated delay manipulation in terms of fairness or altruism has never been measured in the context of the standard dictator game. However, its application to the public good game resulted in increased pro-social decisions (Bilancini, Boncinelli, Capraro, & Celadin, 2020; Bilancini et al., 2022) and a similar treatment based on providing motivations to other dictators resulted in an increase in fairness in a risky dictator game (Feri, Giannetti, & Guarnieri, 2023). This evidence appears consistent with the idea of "language-based preferences" referred to theorize that "what matters is not just the monetary payoffs associated with actions, but also how these actions are described" by means of language (Capraro

[1] This hypothesis gains further support from the fact that we did not impose an upper bound on the time allowed for decision-making in our benchmark treatment (C). This circumstance could mitigate the potential effect size of time delay, which is commonly measured in comparison to time pressure. The sterilization of pressure due to time constraints in C was implemented intentionally to increase the comparability with both MD and TD and to make the difference between TD and MD cleanly emerge. Moreover, the absence of a time limit in C avoids implicit self-selection of our sample based on factors that might bias the benchmark measure of fairness in our setting – e.g., the difficulty of the task (Merkel & Lohse, 2019) or prior subjects' predispositions towards being intuitively more prosocial/egoist under time pressure (Alós-Ferrer & Garagnani, 2020b; Chen & Krajbich, 2018).

et al., 2022). This notion appears useful to make sense of the experimental evidence highlighting, particularly in dictator game settings, how the framing of decisions by means of normative or moral interpretations and narratives (usually provided with the task instructions) can sustain altruism and fairness (Brañas-Garza, 2007; Capraro et al., 2019; Capraro & Vanzo, 2019; Chang et al., 2019; Hillenbrand & Verrina, 2022). In our MD treatment subjects, are not provided with specific normative meaning interpreting certain outcomes as more fair/unfair than others. Notwithstanding, we can conjecture that just asking people to motivate their decision could potentially activate a normative framing of the decision at stake. Consequently, subjects would be more prone to base their decisions on personal and social norms that are likely oriented towards giving. Accordingly, we expect that MD could decrease on average the amount got by dictators both with respect to C and TD.

As concerning RQ.2, the literature gives no reason for hypothesizing that the relative ordering of the expected effects of the cognitive manipulations changes between DG and BDG. Accordingly, also in the BDG conditions we expect no significant difference in the average allocations between C and TD, but a potential decrease in the amount got by the dictators due to MD. However, this relative difference might change in magnitude and statistical significance due to the increased salience of normative features of the allocation decision due to the BDG setting, which makes the fair split more salient and activate a deontological resistance to take from the recipient's endowment. This intuition is based on the evidence produced by Krupka and Weber (2013) that shows that dictators' decisions to keep money from the initial endowment are judged as less socially appropriate in the bully version than in the standard one, thus decreasing the average amount got by the dictators. Based on this evidence, we predict for RQ.3 that in each of the cognitive manipulation treatments the dictators get a larger share in the DG than in the BDG treatment.

However, two clarifications are useful to better characterize this prediction. The first one concerns the specific structure of the two versions of the dictator game we run. Our design is isomorphic with the one implemented in Krupka and Weber (2013), which in turn differs from the one which is mainly applied in the stream of literature aimed at testing the impact of the introduction of a taking option on dictators' giving (Bardsley, 2008; List, 2007). This literature is based on a design where the introduction of the taking option enlarges the choice set and results in less giving (see Zhang & Ortmann, 2014 for a meta-study). On the contrary, by keeping constant the choice set, we only rely on a framing effect that assigns a different normative meaning to the same allocation options between the two versions of the dictator game (and not new ones). Specifically, based on the evidence produced by Krupka and Weber (2013), we expect dictators to resist taking from the recipients and changing the initial fair allocation in the BDG out of normative considerations about social or moral acceptability.

The second clarification concerns the potential impact on perceived social norms consequent to the framing effect induced by the BDG treatment. Following Krupka and Weber (2013), we would expect that the perceived norm changes between the DG and BDG treatments. However, it must be underlined that our elicitation methodology differs from the one they apply. Indeed, we elicit the perceived norms from the subjects who perform the dictator task and not in an independent sample. Furthermore, we elicit social expectations – i.e., empirical and normative expectations (Bicchieri & Chavez, 2010; Bicchieri & Xiao, 2009) – instead of the individual perception of social appropriateness. These differences were purposely set to obtain a within-subject design able to connect a detailed analysis of subjective perceptions of descriptive and social norms to cognitive manipulations at the individual level. However, these features introduce elements of novelty that need to be empirically explored to understand how the elicited norms impact decisions in the two dictator game settings.

3. Results

In this section, we attempt to provide answers to the research questions by relying on the method of analysis that we pre-registered alongside an *a priori* power analysis for our sample size. Given the novelties introduced with our treatments, we took a conservative approach and posited an effect size d = 0.13 – defined as the ratio between population means and the overall population standard deviation (Cohen, 2013) – in the range of what is considered small in the literature (Cohen, 2013; Lakens, 2013; Thompson, 2007). We then computed our sample size by setting an expected statistical power of 0.8 for a significance level of 5%, which required 164 subjects per treatment (rounded up to 170). Upon conducting a *post hoc* analysis, we found that our study has a statistical power of above 0.95.

Some preliminary remarks about the effectiveness of our cognitive manipulations are useful to prepare the discussion of our main results. The first remark concerns subjects' feeling of being engaged in reflection which is self-reported as higher in MD and TD. The second remark is relative to the time actually spent on the main task. TD and MD treatments led participants to spend substantially more time on the screen of the main task than the C treatment, suggesting that the time constraint was effectively delaying decisions. Moreover, only a few subjects spent more than forty seconds in the C condition (8.29 percent), confirming that for a large majority TD and MD treatments are actually postponing decisions and not causing uncontrolled sample selection. We also see that it took a longer time in the MD treatment than in the TD treatment, highlighting a difference in participants behavior when involved in writing with respect to just waiting. The tests and figures about the effectiveness of cognitive manipulations are shown in the Section B.1 of the Supplementary Material.

3.1. Main analysis

To answer RQ.1 and RQ.2 we test if allocation choices in the DG and in the BDG are different under the TD treatment, the MD treatment, and the C treatment, and, in the case that a difference is detected, we check in which treatment the dictator gets a larger share. Henceforth, we use "Got" to indicate the variable that records the amount of money that the dictators decide to get for their own benefit. The evidence shows similar patterns for allocation choices across the two versions of the DG, with dictators consistently getting more in C and less in TD. Fig. 2 reports average values of Got in the DG, which appear not to be statistically different across the TD, MD, and C treatments (Kruskal–Wallis, p = 0.5204). Fig. 3 reports average values of Got in the BDG, which instead appear to be statistically different across the TD, MD, and C treatments (Kruskal–Wallis, p = 0.0145). Pairwise comparisons to test differences between treatments show that dictators on average got less for themselves in the MD treatment with respect to both the TD treatment (Mann–Whitney test, z = 2.467, p = 0.0136) and the C treatment (Mann–Whitney test, z = 2.659, p = 0.0078), whereas no statistically significant difference appears to emerge when comparing the TD and C treatments (Mann–Whitney test, z = 0.289, p = 0.7723).

To answer RQ.3 we test if, for each of the TD, MD and C treatments, Got differs between the DG and BDG conditions, and in the case that a difference is detected, we check under which condition the dictator gets a larger share. Fig. 4 reports average values of Got for the DG and BDG conditions. We see that Got, on average, is smaller in BDG than in DG in all cases, but only in the MD treatment the difference between the two conditions is statistically significant (Mann–Whitney test, z = 2.595, p = 0.0095).[2]

[2] Multiple hypothesis testing can be a concern since we are conducting a total of nine hypothesis tests (see List, Shaikh, Vayalinkal, et al., 2021). To control for multiple hypothesis testing we apply the false discovery rate procedure (Benjamini & Hochberg, 1995; Simes, 1986) and adjust the p-values accordingly. Our results are found to be robust to this correction at a significance level of q = 0.05, as shown in the Section B.2 of the Supplementary Material.

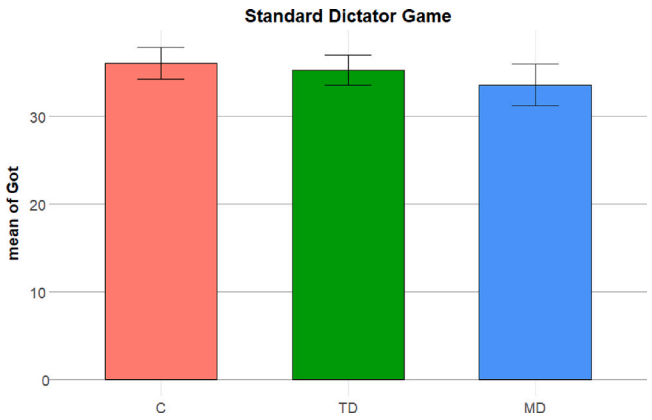

Fig. 2. In the Standard Dictator Game, the amount got by the dictators does not differ significantly under the different cognitive manipulations and the control treatment. There is no statistically significant difference among the average of money got by the dictators in the Control (M = 36.09, SD = 12.01), the Time Delay (M = 35.32, SD = 11.23), and the Motivated Delay (M = 33.61, SD = 15.45) treatments (Kruskal–Wallis, p = 0.5204). Note. Confidence intervals on means are at 95%.

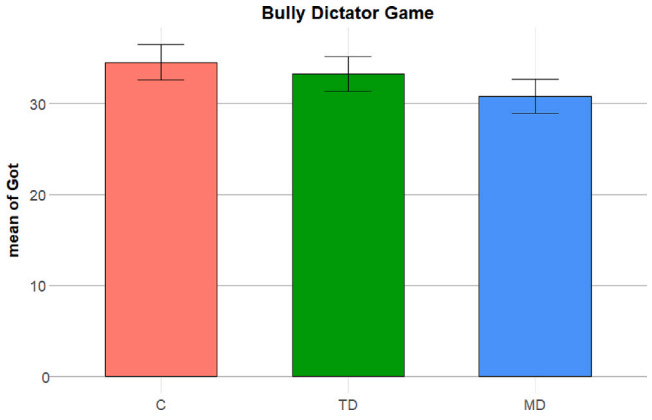

Fig. 3. In the Bully Dictator Game, the dictators get the lowest amount under motivated delay. The average amount of money the dictators got in the Motivated delay treatment (M = 30.80, SD = 12.22) is lower than in the Time Delay treatment (M = 33.28, SD = 12.91), and lower than the amount of money got by the dictators in the Control treatment (M = 34.54, SD = 13.17). Statistically significant differences are observed among the TD, MD, and C treatments (Kruskal–Wallis, p = 0.0145). Pairwise comparisons reveal that the MD treatment has a significantly lower average Got level compared to both TD (Mann–Whitney test, z = 2.467, p = 0.0136) and C treatments (Mann–Whitney test, z = 2.659, p = 0.0078), while no significant difference is found between the TD and C treatments (Mann–Whitney test, z = 0.289, p = 0.7723). Note. Confidence intervals on means are at 95%.

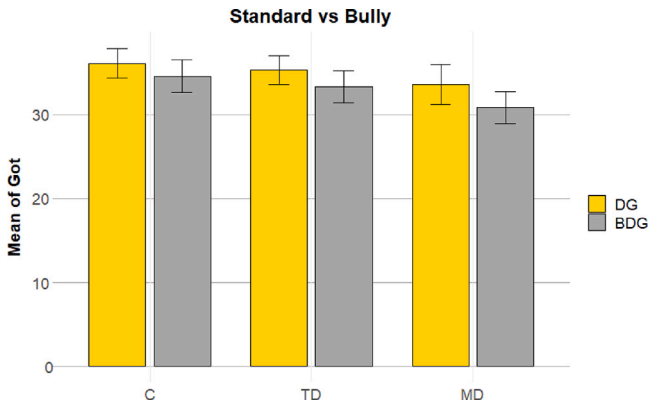

Fig. 4. The amount got by the dictator in the Bully Dictator game is consistently lower than that in the standard Dictator game, but only under Motivated Delay the difference is statistically significant. The average amount the dictator got under Motivated Delay is lower in the Bully variant of the dictator game (M = 30.80, SD = 12.22) than in the standard one (M = 33.61, SD = 15.46). The average amount the dictator got under Time Delay is lower in the Bully variant of the Dictator game (M = 33.28, SD = 12.9) than in the standard one (M = 35.33, SD = 11.24). The average amount the dictator got in the Control treatment is lower in the Bully variant of the dictator game (M = 34.54, SD = 13.17) than in the standard one (M = 36.09, SD = 12.01). However, the difference between the two variants of the dictator game is statistically significant only in the case of Motivated Delay (Mann–Whitney test, z = 2.595, p = 0.0095) Note. In the figure, *KW* stands for the Kruskal–Wallis equality-of-populations rank test and *MW* for the Wilcoxon rank-sum (Mann–Whitney) test. Confidence intervals on means are at 95%.

Table 3
Tobit regressions of elicited norms on experimental conditions.

	(1) EE	(2) NB	(3) NE
BDG	0.0693 (0.925)[0.940]	−0.399 (0.716)[0.578]	−0.0731 (0.757)[0.923]
TD	−0.256 (1.106)[0.817]	0.179 (0.844)[0.832]	−0.267 (0.898)[0.766]
MD	−1.514 (1.162)[0.766]	−1.334 (0.886)[0.133]	−0.116 (0.943)[0.902]
log likelihood	−3718.3	−3712.3	−3736.7
N	1022	1022	1022

Note. Dependent variables. *EE* (empirical expectations): variable ranging 0.10, 0.20, … 0.60. *NB* (normative beliefs): variable ranging 0.10, 0.20, … 0.60. *NE* (normative expectations): variable ranging 0.10, 0.20, … 0.60.
Regressors. *BDG*: 1 if Bully Dictator Game and 0 if standard Dictator Game. *TD*, 1 if time delay treatment and 0 otherwise; *MD*: 1 if Motivated Delay treatment and 0 otherwise.
Standard errors are in parentheses, p-values are in brackets.
Significance of coefficients: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

3.2. Secondary analysis

As preregistered, we further investigate whether the findings of our main analysis are robust to an array of controls. Specifically, we check if, once we control for EE, NB, and NE, the experimental conditions still affect the dictators' decision. Furthermore, we also control for CRT, sex and income. In the following analysis we use Tobit regressions since the main dependent variable, Got, is bounded on both sides and has a non-negligible number of extreme values.

No causal claim can be made regarding the impact of EE, NB and NE on Got, since EE, NB and NE are not necessarily exogenous to Got. In addition, EE, NB, and NE might be affected by the experimental conditions. However, while we cannot test the exogeneity of EE, NB, and NE to Got, we can check if they are affected by the experimental conditions. In Fig. 5 we report the means of the three norms distinguishing by cognitive manipulation and by version of Dictator Game. The non-parametric analysis reported in caption suggests the absence of any effect on the three norms for our treatment variables, although we register a mild effect on NB for cognitive manipulation. Table 3 reports the estimates for three Tobit regressions where, respectively, EE, NB, and NE are regressed on dummies for the TD, MD and BDG conditions (omitted category is the case of the DG and C conditions). No estimated coefficient of these regressors is significantly different from zero, confirming that EE, NB, and NE are not affected by our experimental conditions.

We can hence look at how Got is affected by our treatment variables controlling for EE, NB, and NE, and, in addition, for CRT and demographics. Table 4 reports the results of four Tobit regressions.[3] In model (1), and consistently in the other three models, we find a negative and significant effect of BDG and MD on Got. In model (2), both EE and

[3] In Section B.3 of the Supplementary Material, we provide regressions with the self-reported measure of attention in the controls, for which we do not report any significant impact.

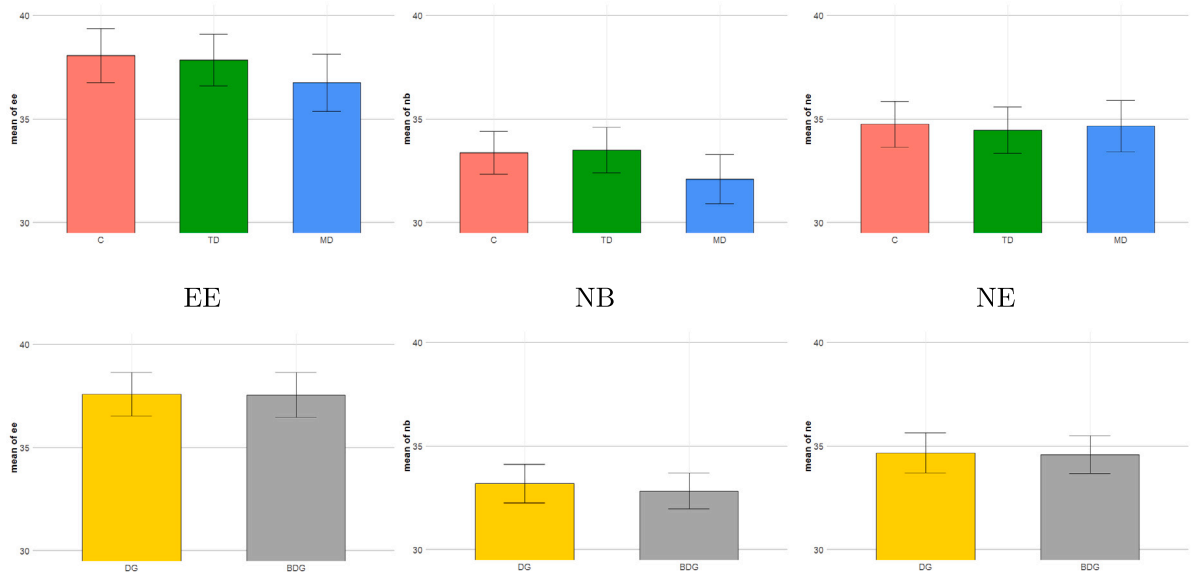

Fig. 5. The elicited norms (a) do not differ under the different cognitive manipulations and the control, (b) do not differ between the standard and the Bully Dictator Game The difference in the means of Empirical Expectations (EE), Normative Beliefs (NB) and Normative Expectations (NE) are not statistically significant neither with respect to cognitive manipulations nor to variants of the dictator game. We only note a mild effect of the cognitive manipulation on NB. EE: C vs TD vs MD, Kruskal–Wallis, p = 0.3995; DG vs BDG: Mann–Whitney test, p = 0.9215. NB: C vs TD vs MD, Kruskal–Wallis, p = 0.0789; DG vs BDG: Mann–Whitney test, p = 0.4578. NB: C vs TD vs MD, Kruskal–Wallis, p = 0.7893; DG vs BDG: Mann–Whitney test, p = 0.9358. Note. Confidence intervals on means are at 95%.

Table 4
Tobit regressions of Got on experimental conditions and controls.

Got	(1) Baseline	(2) Norms	(3) Right	(4) Intuitive
BDG	−2.302** (.952)[0.016]	−2.023*** (0.733)[0.006]	−2.035*** (0.737)[0.006]	−2.027*** (0.737)[0.006]
TD	−1.368 (1.110)[0.218]	−1.262 (0.886)[0.155]	−1.126 (0.878)[0.200]	−1.116 (0.877)[0.204]
MD	−3.626*** (1.219)[0.003]	−2.075** (0.914)[0.023]	−2.005** (0.915)[0.029]	−2.038** (0.915)[0.026]
EE		0.371*** (0.050)[<0.001]	0.358*** (0.049)[<0.001]	0.357*** (0.050)[<0.001]
NB		0.776*** (0.068)[<0.001]	0.785*** (0.068)[<0.001]	0.781*** (0.068)[<0.001]
NE		−0.075 (0.053)[0.155]	−0.070 (0.053)[0.191]	−0.067 (0.054)[0.208]
CRT right/int			0.418** (0.191)[0.029]	−0.540** (0.213)[0.011]
Female			0.007 (0.733)[0.993]	0.075 (0.733)[0.919]
Household income			−0.010 (0.100)[0.919]	−0.001 (0.099)[0.991]
log likelihood	−3795.5	−3521.9	−3507.9	−3507.1
N	1022	1022	1019	1019

Note. Dependent variable. *Got* (dictator's share): variable ranging 0.10, 0.20, … 0.60.
Regressors. *BDG*: 1 if Bully Dictator Game and 0 if standard Dictator Game. *TD*, 1 if time delay treatment and 0 otherwise; *MD*: 1 if Motivated Delay treatment and 0 otherwise. *EE* (empirical expectations): variable ranging 0.10, 0.20, … 0.60. *NB* (normative beliefs): variable ranging 0.10, 0.20, … 0.60. *NE* (normative expectations): variable ranging 0.10, 0.20, … 0.60. *CRT right* (cognitive reflection test with "right" as number of correct answers) integer from 1 to 6 (model 3). *CRT int* (number of intuitive answers) integer from 1 to 6 (model 4).
Standard errors are in parentheses, p-values are in brackets.
Significance of coefficients: * $p < 0.001$, ** $p < 0.05$, *** $p < 0.01$.

NB have a positive and statistically significant coefficient, while both TD and NE have a negative and not statistically significant coefficient. Note that EE, NB, and NE are elicited at the individual level, so that the corresponding coefficients indicate the partial correlation between one's own decisions Got and elicited norm.

So, while the findings of the main analysis regarding the effects of the experimental conditions on Got are confirmed by the Tobit regression, we also see that such findings are not washed out by controlling for elicited norms, which in turn appear not to be affected by the experimental conditions. Together these findings suggest that

the experimental conditions do not affect Got through norms, with the only potential exception of NB.

In models (3) and (4) we add controls for CRT and demographics. Nevertheless, model (3) and (4) provide a consistent picture regarding the CRT, in line with what has been found by Arechar, Kraft-Todd, and Rand (2017). In model (3), the estimated coefficient of CRT right is positive and statistically significant, suggesting that dictators who are more inclined to reflection tend to get more. In model (4), the coefficient CRT int is negative and statistically significant, suggesting that dictators who are less inclined to reflection tend to get less. This evidence aligns with the findings of Cueva et al. (2016), which shows that intuitive individuals tend to be more averse to inequality.[4] Lastly, socio-demographic characteristics (sex and household income) do not seem to affect the dictator's decision in either model.[5]

4. Discussion

We have investigated how decisions in a Dictator Game played online are affected by two distinct cognitive manipulations aimed at promoting greater reliance on deliberation, using two distinct initial allocations of endowments with the purpose of influencing moral judgment and norm compliance. We have found that a more equal initial endowment leads the dictator to get less, confirming in the online setting previous evidence from the lab (Krupka & Weber, 2013). Moreover, our findings suggest that the request to write a motivation for the allocation decision reduces the amount of money that the dictator gets, while the mere request to wait some time before choosing does not have appreciable behavioral effects. One explanation posits that our results are produced by the interaction of a cognitive manipulation that primes a normative framing (the motivation condition) with a decision involving moral judgment and norm compliance (the bully *versus* non-bully variants of the Dictator Game).

This explanation is consistent with the notion of "language-based preferences" (Capraro et al., 2022) advanced in the stream of literature engaged in showing that moral or normative interpretations of outcomes have the power to condition preferences by means of a framing effect (Brañas-Garza, 2007; Capraro et al., 2019; Capraro & Vanzo, 2019; Chang et al., 2019; Hillenbrand & Verrina, 2022). The initial allocation of endowments in the bully dictator game may make the normative and moral implications of dictators' options and outcomes more salient with respect to the standard dictator game. Then, in the motivated delay treatment, because a justification for behavior is requested, decision-makers may be pushed to rely more on personal normative considerations. Indeed, in our data personal normative beliefs appear to have a non-negligible partial correlation with decisions even when we control for empirical or normative expectations, as defined by Bicchieri (2005, 2016). Thus, participants in the motivated delay treatment might tend to reflect more on what they personally believe is "the right thing to do" and then they might rely more on such reflections while making their decision. In other words, by favoring the scrutiny of motivations, the motivated delay treatment might actually lead to think more about personal norms, rather than social, generating behavior that is consistent with recent investigations on the role of personal norms (Bašić & Verrina, 2021; Capraro & Rand, 2018; Catola, D'Alessandro, Guarnieri, & Pizziol, 2021).

However, we must acknowledge that this interpretation of our results is a conjecture whose validity would require accurate tests that we cannot run with our experimental data. Indeed, it would be useful to run a similar experiment setting up treatments devoid of moral considerations. This would allow rejecting alternative explanations, like the one based on the idea that, in online settings, attention gets reduced in case of time delay, while this is not so in case of motivated delay, with the latter only increasing deliberation due to the switch to the engaging activity of writing a motivation (Bohannon, 2016; Chandler, Mueller, & Paolacci, 2014). Moreover, further investigation of the channels of transmission of our main effect would be necessary to connect our explanation to related streams of literature, as for instance those dealing with accountability (Brandts & Garofalo, 2012; Lerner & Tetlock, 1999; Xiao, 2017) or with the "focusing" effect (Krupka & Weber, 2009). As refraining from taking money from the other player in the bully Dictator Game does not necessarily imply non-selfish behavior, it would also be important to better understand the differences in actions and motives underlying dictators' decisions in the bully and non-bully versions of the Dictator Game (see, e.g., Cappelen, Nielsen, Sørensen, Tungodden, & Tyran, 2013; Korenok, Millner, & Razzolini, 2014). To conclude, we discuss a number of methodological issues that we had to deal with during the design of the experiment, which admittedly may be tackled in different ways, generating outlines for future research.

Norms are often used as explanatory variables of behavior, being formed in daily life experience and then taken by the experimental subjects into the lab setting. Coherently with this view, norms should be elicited independently from the experimental task, so as not to incur the risk of declarations that are affected by own behavior. Krupka and Weber (2013) relied on a clever design to address this issue, eliciting norms from a sample of individuals which is different from the one of decision-makers, by asking them to guess social norms in an incentive-compatible way. In our setting we might have relied on a similar procedure for the six cases: the two variants of the Dictator Game (bully and non-bully) combined with the three conditions regarding cognitive manipulations (control, time delay, motivated delay). We opted not to do so because we believe that stepping into the shoes of participants who experience unfamiliar cognitive manipulations, without direct experience of the manipulation itself, may require too much of imagination and possibly lead to unreliable elicitation of norms. Rather, we have preferred to ask subjects soon after experiencing the experimental condition, which also allows us to assess whether elicited norms are affected by cognitive manipulations. In this way, we have given up the possibility to inquire about the causal relationship that goes from norms to behavior, but we have a potentially superior control at the individual level for analyzing the causal relation that goes from our experimental conditions to both behaviors and norms.

Another issue concerns our choice to work with the bully and non-bully variants of the Dictator Game. A prominent alternative is represented by the Social Value Orientation (SVO), which is widely used in the literature to measure altruism and prosociality (Dolton, Tol, et al., 2019; Murphy & Ackermann, 2014; Murphy, Ackermann, & Handgraaf, 2011; Van Lange, De Bruin, Otten, & Joireman, 1997). We note that cognitive manipulations have more clearly identifiable effects in single decisions, while the SVO requires at least six distinct decisions to be taken. In settings where multiple decisions have to be made, two main alternatives are possible: cognition can be manipulated just for the first decision, or for all decisions. Whatever the choice, biased effects may show up in later decisions, due to the combination of the effects of one or more manipulations with those of prior decisions taken. A solution may be to restrict the analysis on the first decision only, but this would largely reduce the desirability of additional decisions after the first – which adds burden on experimental subjects. Hence, we preferred to present just a single decision to be made. Further, the differentiation of

[4] In the Section B.6 of the Supplementary Material, we replicated the methodology used by Cueva et al. (2016) by dividing our subject pool into three groups based on their answers to the CRT items. Our findings are consistent with those of Cueva et al. (2016), as reflective individuals tend to allocate more for themselves.

[5] To offer an alternative perspective on our results, we present in section B.5 of the Supplementary Material a panel of figures representing the distribution of amounts kept by dictators in each condition and Hurdle model regressions (see, for instance, Brañas-Garza, Rodríguez-Lara, & Sánchez, 2017; Engel, 2011). These regressions reveal that while female dictators are less likely to provide any allocation to recipients, they tend to give more when they do allocate something.

the initial endowments, as done in the bully and non-bully variants that we have employed, are in our opinion more likely to trigger reliance on well-formed norms (based on property rights) for intuitive decision-making, with respect to the questions used to measure SVO which are more unusual and abstract and which make calculations reasonably more salient.

A final remark is about the cognitive mode which is most relied upon in the online setting. Online experiments are characterized by shorter procedures and lower stakes with respect to laboratory experiments, which reasonably increase the likelihood that participants make quicker and more intuitive decisions. This leads us to believe that, in online experiments, conditions increasing the reliance on deliberation may produce starker and hence more easily identifiable effects, if compared with treatments increasing the reliance on intuition. This observation has contributed to our choice to focus on cognitive manipulations aimed at promoting deliberative decisions. However, it must be acknowledged that the limitations due to the size of stakes (Brañas-Garza, Jorrat, Kovářík, & López, 2021; Hara et al., 2018; Larney, Rotella, & Barclay, 2019), or, in general, to the reliability of the data generated in online environments (Cuskley & Sulik, 2022; Webb & Tangney, 2022) are relevant, and would deserve robustness checks (in the laboratory), especially to verify that the null result we obtain in the case of the time delay treatment is evidence for an actual lack of effect of the designed condition.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data with the code for replicating the results of this paper are available at https://osf.io/euhct/?view_only=de730bd958ef4711819216d30361c8d8.

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MIUR was not involved in any stage of the study design, data collection, data analysis, and interpretation of the study results.

Use of human subjects

The authors declare that all procedures were performed in compliance with relevant laws and institutional guidelines. Informed consent was obtained from all subjects in the experiment.

Preregistration, data and reproducibility files repository

The preregistration document and the data with the code for replicating the results of this paper are available at https://osf.io/euhct.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.socec.2023.102106.

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