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Social motives *vs* social influence: An experiment on interdependent time preferences ☆

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ABSTRACT

We design an intertemporal Dictator Game to test whether Dictators modify their discounting behavior when their own decision is imposed on their matched Recipients. We run four different treatments to identify the effect of payoffs externalities from those related to information and beliefs. Our descriptive statistics show that Dictators display a marked propensity to account for the intertemporal preferences of Recipients, both in the presence of externalities (social motives) and/or when they know about the decisions of their matched partners (social influence). We also perform a structural estimation exercise to control for heterogeneity in risk attitudes. As for individual behavior, our estimates confirm previous studies in that high risk aversion is associated with low discounting. As for social behavior, we find that social motives outweigh social influence, especially when we restrict our sample to pairs of Dictators and Recipients who satisfy minimal consistency conditions.

"At the first time of sexual union the passion of the male is intense, and his time is short [...]
With the female, however, it is the contrary, for at the first time her passion is weak, and then her time long [...]
If a male be a long-timed, the female loves him the more, but if he be short-timed, she is dissatisfied with him."
["The Kama Sutra of Vatsyayana" – Burton et al. (2009)]

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1. Introduction

We often show concerns for others by changing the timing of a specific course of action. This happens routinely in household key decisions such as selling a house, investing in a pension plan or even getting divorced. The empirical literature on health economics has extensively studied the relationship between time preferences for one's own private health and for others' health (see Lazaro et al., 2001, 2002 or Robberstad, 2005 for earlier contributions on this area and Mahboub-Ahari et al., 2014 for a recent meta-analysis). It is also well documented (see, e.g., Abdellaoui et al., 2013; Browning, 2000; Mazzocco, 2004, 2007, among others) that multi-person household saving and consumption patterns may strongly differ from those of single-person households, even after controlling for individual characteristics (e.g., own risk aversion, or discounting) of each household component. As all these examples illustrate, *social* (i.e., interdependent) concerns may affect the timing of choices: decision makers may try to accommodate others' intertemporal concerns, when decisions affect the latter's prospects and welfare.

This paper aims at providing evidence on *the effects of social preferences on intertemporal decisions*. More in detail, we are interested in better understanding how much – and in which direction – individuals' preferences for anticipating or delaying an action can be affected by the presence of payoff externalities. Clearly, our motivating examples lead to a broader concept of social preferences, compared with its current usage in the flourishing – mainly experimental – literature on these matters, where social preferences are usually restricted to people's interest on "*the fairness of their own material payoff relative to the payoff of others*..." (Fehr and Schmidt, 1999, p. 819). In contrast with this literature, in this paper concerns for others may not only involve others' *material consequences* (e.g., monetary outcomes, consumption bundles), but also others' *concerns and inclinations*, such as risk aversion or discounting. This, in turn, calls for modeling social preferences *as mapped directly on others' individual utilities* (Harrison et al., 2014).

This modeling approach basically frames subjects' behavior as maximizing a social welfare function, which requires an operational solution of the delicate issue of interpersonal comparison of utilities (which is, probably, the reason why the mainstream literature has always preferred to restrict the domain of social preferences to the physical outcome space). By contrast, the empirical literature we just cited – take, e.g. Mazzocco, 2007, eq. (3) – posits that households maximize a convex linear combination of the individual ("selfish") utilities of their members, which are assumed to be derived as different parametrizations – depending on individual characteristics – of the same functional, with weights being interpreted as proxies of each member's bargaining power within the household. This is going to be the modeling approach we use in this paper.

Our empirical evidence comes from a multi-stage laboratory experiment where we investigate on the link between time and social preferences by way of Multiple Price Lists (MPLs, Holt and Laury, 2002, 2005). Since time and risk preferences are interwined, we follow Andersen et al. (2008) by eliciting (own) risk and time preferences by way of separate tasks in the first two stages of the experiment (see also Andersen et al., 2014b; Harrison et al., 2013a, 2005 or Sutter et al., 2013 for applications of similar methods). Thus, we use MPLs to elicit risk preferences and control for the curvature of subjects' utility function when estimating time preferences by way of another sequence of ten MPLs in which subjects are asked to choose between an immediate smaller reward and an increasingly larger later reward. The novelty of our approach relies on incorporating a *social dimension* to this protocol. Thus, once subjects have completed the first two stages, we match them in pairs and assign the roles of *Dictators* and *Recipients*. Then, Dictators go through, once again, the same sequence of intertemporal decisions knowing that – this time – their choices may also be implemented for their assigned Recipient. Subjects' information on others' risk and time decisions and the presence of payoff externalities defines our treatment conditions:

1. in the baseline treatment (T_0, INFO-SOCIAL), Dictators make their intertemporal choices after being informed of what their assigned Recipient had chosen in the first two stages of the experiment;
2. in the BELIEF-SOCIAL treatment (T_1), before deciding for the pair, Dictators go through an additional stage in which we elicit their beliefs on risk and time concerns of their assigned Recipient;
3. in the INFO-PRIVATE treatment (T_2), subjects receive (exactly as in the baseline) information on risk/time individual choices of their groupmate, but no payoff externalities are imposed on others;
4. in the NO INFO-SOCIAL treatment (T_3), Dictators make their intertemporal decisions for the pair without prior knowledge (or elicited belief) of the Recipient's risk/time decisions.

Our design strategy allows to tease apart *social motives* from *social influence*. The comparison between the INFO-SOCIAL and the INFO-PRIVATE treatments allows us to determine whether informed Dictators change their decision more often when they act on behalf of the pair (social motives) compared with the situation in which – whatever the reason – they can just mimic the behavior of their assigned Recipient (social influence), without imposing any payoff consequence on the latter.[1] Along similar lines, we can also compare the behavior of uninformed Dictators in the BELIEF-SOCIAL and the NO

[1] The role of social influence was first studied in Psychology by Sherif (1937) and Asch (1955). The economic literature on this topic includes papers on *informational influence*, that is, herding or observational cascades (see, e.g., Banerjee, 1992; Bikhchandani et al., 1992 or Feri et al., 2011) and *normative influence*, that is, imitation based on moral judgment (see, e.g., Hung and Plott, 2001 or Moreno and Ramos-Sosa, 2017). The interested reader on the comparison between these two behavioral phenomena can consult Goeree and Yariv (2015) and the references therein.

INFO-SOCIAL treatments so as to measure the impact of belief elicitation in the emergence of social (time) preferences. This is what Krupka and Weber (2009) label as the effect of *focusing* on social preferences: guessing and thinking about the actions of others leads – in standard Dictator games – to focus more on the social norm and, as a result, more generosity is observed.

Following Rodriguez-Lara (2010), our experimental design is built around the structural estimation exercise of Section 4.2, in which subjects' behavior is framed by way of a convex linear combination between the ("selfish") utilities of Dictator and Recipient. By contrast with the literature cited earlier, here weights reflect the Dictator's concerns about the Recipient's risk aversion and discounting. In this respect, our identification strategy crucially relies on the experimental design by manipulating subjects' incentives and information in the various stages of the experiment.

The remainder of the paper is arranged as follows. Section 2 reviews the relevant literature on these matters. In Section 3 we lay out our experimental design, whereas Section 4 reports our results. First, Section 4.1 reports some descriptive statistics on subjects' behavior in the various stages of the experiment. Here we show that *i*) Dictators' choices significantly move in the direction of their matched Recipients in our baseline treatment; *ii*) social influence is another important factor in explaining choices, in that Dictators tend to move in the direction of Recipients also in the INFO-PRIVATE treatment and *iii*) Krupka and Weber's 2009 *focusing* effect is also relevant in the absence of information in that eliciting beliefs seems to trigger social preferences in the BELIEF-SOCIAL, compared with the NO INFO-SOCIAL treatment.

Section 4.2 tests the robustness of our preliminary findings by way of a structural model in which we frame subjects' choices within the realm of a random utility maximization problem, by which we can control for subjects' heterogeneity in risk preferences. We look both at subjects' *i)* individual decisions (and elicited beliefs) in Stages 1 to 3, as well as *ii)* Dictators' intertemporal choices in Stage 4. As for the former, our evidence is consistent with previous findings in that our subjects exhibit, on average, (Constant Relative) Risk Aversion (CRRA, Hey and Orme, 1994; Holt and Laury, 2002; Harrison and Rutström, 2008) and non-exponential discounting (Coller et al., 2012; Benhabib et al., 2010; Andersen et al., 2008). In addition, we also find (consistently with Sutter et al., 2013 and Dean and Ortoleva, 2012), that individual (own) risk and time preferences are strongly correlated, in that risk averse subjects are also more patient. Finally, we see that – once we control for risk aversion in our structural estimation – social motives outweigh both social influence and focusing, in that the estimated weight of the Recipient's utility is positive and highly significant only when externalities and information are *both* present (i.e., in our baseline treatment). By contrast, the "social influence" conjecture (proxied by the estimated weight for the INFO-PRIVATE treatment) is only partially validated, since the estimated coefficient remains positive, but is only significant at 10% confidence, and only when we do not restrict our sample to pairs of Dictators and Recipients who satisfy minimal *consistency* conditions (see Section 4.1.3). Also for the focusing hypothesis, the estimated weight in the BELIEF-SOCIAL treatment is positive, but not significant.

Finally, Section 5 concludes, followed by Appendices containing information on the identification strategy, the experimental instructions, the debriefing questionnaire and supplementary experimental evidence.

2. Related literature

Notwithstanding our "social twist", this paper sits squarely in the emerging literature that applies experimental methods to study the link between individual risk and time preferences (see, among others, Andreoni and Sprenger, 2012a, 2012b; Halevy, 2008; Laury et al., 2012). In this respect, we borrow the methodology put forward by Andersen et al. (2008) and control for the curvature of the utility function when eliciting discount rates. To this aim, a double MPL is employed to elicit risk and time preferences *independently*, that is, with two separate tasks: one MPL over lotteries paid off at the time of the experiment (Stage 1), another intertemporal MPL of certain monetary payoffs paid off at different times (Stage 2). Similar methods have been employed by Andersen et al. (2006, 2014b), Harrison et al. (2005), Cheung (2015), Sutter et al. (2013) or Frederick et al. (2002), among others.

Andreoni and Sprenger (2012b), instead, apply an identification strategy between risk and time preferences in which subjects allocate a budget of tokens between risky prospects that reward at different points in time (see also Miao and Zhong, 2015). With this design, the null hypothesis of risk neutrality is also rejected. Other methods to elicit time preferences are those of Benhabib et al. (2010), where subjects are asked to elicit *intertemporal equivalents*, i.e., the amount of money that received today (in the future) that would make them indifferent to some amount paid in the future (today) or Laury et al. (2012), where the elicitation of risk preferences does not require any assumption on the form of the utility function.[2]

Along similar lines, we here mention an emerging literature that, by way of joint elicitation of risk and social preferences, claims that the empirical content of the latter may be severely reduced by the presence of some – strategic, or environmental – uncertainty (Winter, 2004; Winter et al., 2008; Cabrales et al., 2008; Frignani and Ponti, 2012).[3]

To the best of our knowledge, this is the first paper that elicits discount rates in a model of social preferences. The only related precedents we are aware of are the papers of Phelphs and Pollak (1968) and Kovarik (2009). Phelphs and Pollak (1968) propose an intergenerational model in which each generation cares about the consumption of future generations,

[2] See also Andreoni et al. (2015) or Harrison et al. (2013a) for a discussion on the different elicitation methods.

[3] Andersson et al. (2016), Chakravarty et al. (2011) and Harrison et al. (2013b) are further examples of the investigation of social preferences in the risk dimension.

which is discounted in a non-linear manner. Kovarik (2009) collects evidence on the relationship between altruism and discounting, showing that donations in a Dictator Game decrease as the moment for receiving payments is delayed. This contradicts standard theories of time preferences, including exponential and hyperbolic discounting.

Last, but not least, given that our estimation strategy involves the joint elicitation of risk, time and social preferences by way of separate experimental tasks, our findings are to be compared with those of some recent papers that establish empirical correlations among these behavioral traits. In this respect, our finding are consistent with those of Sutter et al. (2013), in that *subjects with a comparatively lower degree of risk aversion discount the future significantly more* (see also Burks et al., 2009 and Dean and Ortoleva, 2012). Because our debriefing questionnaire collects a wide variety of individual characteristics, we can also establish a positive correlation between the willingness to accept the delayed payment and the score in the cognitive skills, also reported by Anderson et al. (2011) or Burks et al. (2009).[4]

3. Experimental design

3.1. Sessions

Thirteen experimental sessions were run at the Laboratory for Research in Experimental Economics (LINEEX), at the Universidad de Valencia. A total of 624 subjects (48 per session) were recruited within the undergraduate population of the University. The experimental sessions were computerized. Instructions were read aloud and we let subjects ask about any doubt they may have had. All sessions ended with a debriefing questionnaire to distill subjects' individual socio-demographics and social attitudes.[5] Each session lasted, on average, 1 hour and 40 minutes.

3.2. Stages

Each subject participates to one of the four treatment conditions (T_0 to T_3). Stages 1 and 2 (common to all treatments) are used to elicit individual (own) risk and time preferences, respectively. After Stage 2, participants are matched in pairs. In one of the treatments (T_1, labeled as BELIEF-SOCIAL), subjects face an additional stage (Stage 3) in which we elicit their beliefs on (own) risk and time preferences of their assigned groupmate. Then, in Stage 4 (common to all treatments), we assign subjects the role of Dictator and Recipient, and all subjects go again through the same sequence of decisions of Stage 2. Our treatment conditions (Section 3.6) determine whether the Dictators' decision in Stage 4 is binding for the Recipient, and the information Dictators receive (if any) about the Recipients' decisions in Stages 1 and 2.

3.3. Stage 1. Own risk preference elicitation

We elicit subjects' individual risk preferences by way of a MPL in which subjects face the ordered array of binary lotteries of Fig. 1. As Fig. 1 shows, subjects face a sequence of 11 binary lotteries, one for each row. The entire sequence is characterized by the fact that the "risky" option (B) is increasingly more profitable, as the probability of the highest prize (€ 190, in our parametrization) grows in probability, and so is falling the expected payoff difference between options A and B. In decision 1 (11) lotteries are degenerate, giving probability 1 to the lower (larger) prize for lottery A and B, respectively. A risk-neutral subject should be switching from option A to B in decision 6, when the expected payoff difference between option A and option B goes negative. The higher the switching point, the more risk averse the subject is.

3.4. Stage 2. Individual time preference elicitation

MPLs are also used to elicit time preferences. In Stage 2 subjects go through 10 decision rounds, each of which is characterized by a specific time delay, τ, ranging from 1 to 180 days. For each MPL, τ, subjects face 20 binary choices, k, and choose between receiving € 100 in the day of the experiment (hereafter "today") and € $100\left((1+\frac{i_k}{365})^{\tau}\right)$ in τ days, where the sequence of Annual Interest Rates (AIR), i_k, constant across rounds, τ, varies from 2% to 300%. Delays correspond to $\tau = 1, 3, 5, 7, 15, 30, 60, 90, 120$ and 180 days. Fig. 2 reports the user interface of the MPL corresponding to a delay of 100 days, the same used in the experimental instructions.[6] Contrary to other studies (e.g., Andersen et al., 2008, 2014b; Coller and Williams, 1999; Coller et al., 2012) the AIR is not shown to subjects in the user interface. Another important difference with respect to these papers is that subjects do not make a unique intertemporal decision (with different delays distributed between subjects). Instead, *all* subjects go through *all* intertemporal MPLs.[7]

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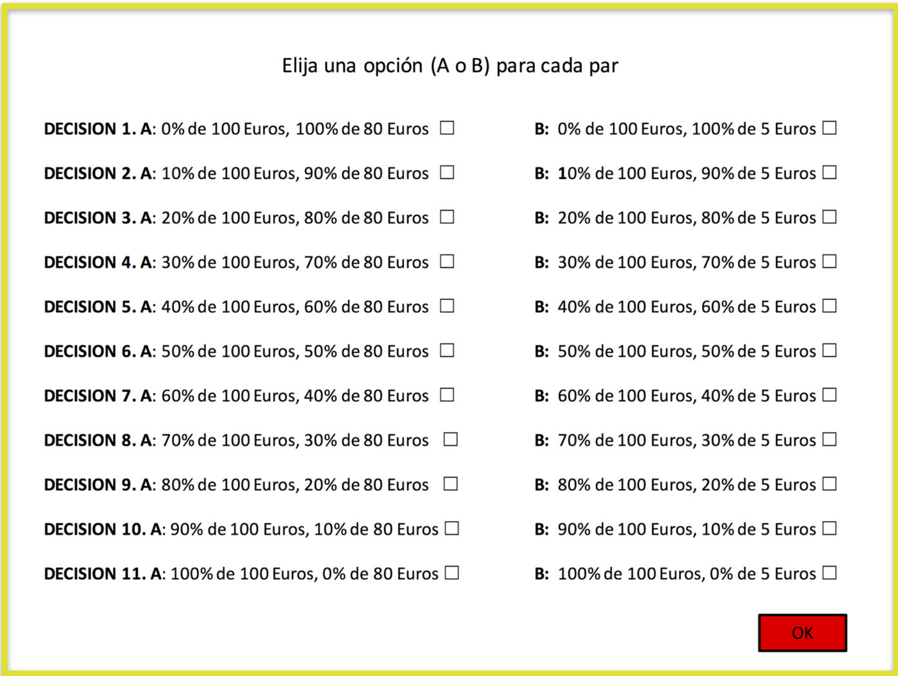
4 See Appendix D2.

5 The experiment was programmed and conducted with the software *z-tree* (Fischbacher, 2007). Translated versions of the instructions and the debriefing questionnaire can be found in Appendix B.

6 The interested reader can see the full set of MPLs of Stage 2 in Appendix C (Table C1).

7 This within-subject design has been also used by Tanaka et al. (2010), Cheung (2015) and Sutter et al. (2013).

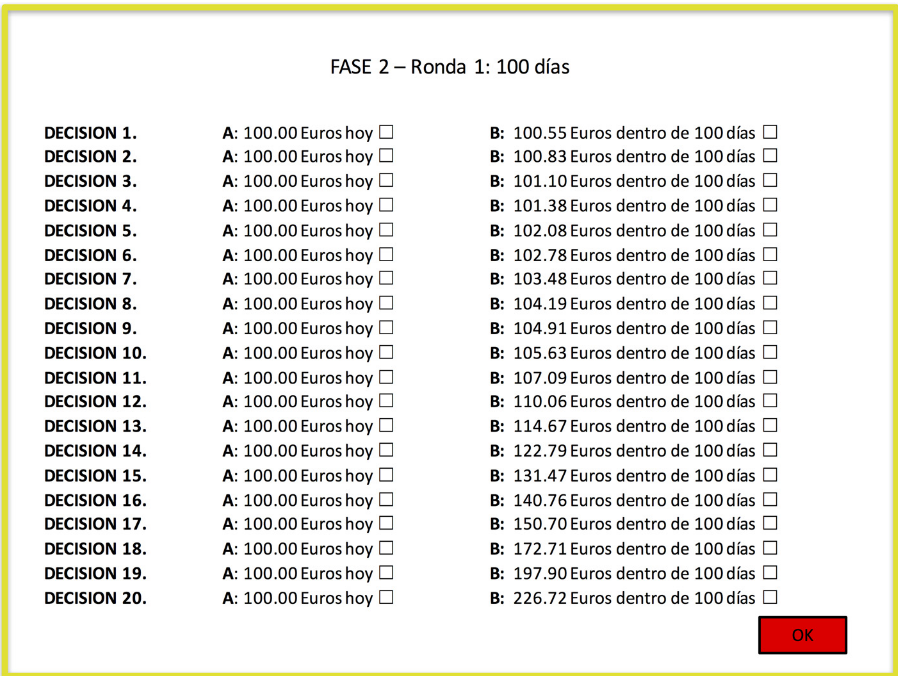


Elija una opción (A o B) para cada par

DECISION 1. A: 0% de 100 Euros, 100% de 80 Euros ☐	**B:** 0% de 100 Euros, 100% de 5 Euros ☐
DECISION 2. A: 10% de 100 Euros, 90% de 80 Euros ☐	**B:** 10% de 100 Euros, 90% de 5 Euros ☐
DECISION 3. A: 20% de 100 Euros, 80% de 80 Euros ☐	**B:** 20% de 100 Euros, 80% de 5 Euros ☐
DECISION 4. A: 30% de 100 Euros, 70% de 80 Euros ☐	**B:** 30% de 100 Euros, 70% de 5 Euros ☐
DECISION 5. A: 40% de 100 Euros, 60% de 80 Euros ☐	**B:** 40% de 100 Euros, 60% de 5 Euros ☐
DECISION 6. A: 50% de 100 Euros, 50% de 80 Euros ☐	**B:** 50% de 100 Euros, 50% de 5 Euros ☐
DECISION 7. A: 60% de 100 Euros, 40% de 80 Euros ☐	**B:** 60% de 100 Euros, 40% de 5 Euros ☐
DECISION 8. A: 70% de 100 Euros, 30% de 80 Euros ☐	**B:** 70% de 100 Euros, 30% de 5 Euros ☐
DECISION 9. A: 80% de 100 Euros, 20% de 80 Euros ☐	**B:** 80% de 100 Euros, 20% de 5 Euros ☐
DECISION 10. A: 90% de 100 Euros, 10% de 80 Euros ☐	**B:** 90% de 100 Euros, 10% de 5 Euros ☐
DECISION 11. A: 100% de 100 Euros, 0% de 80 Euros ☐	**B:** 100% de 100 Euros, 0% de 5 Euros ☐

OK

Fig. 1. Stage 1 user interface.



FASE 2 – Ronda 1: 100 días

DECISION 1.	**A:** 100.00 Euros hoy ☐	**B:** 100.55 Euros dentro de 100 días ☐
DECISION 2.	**A:** 100.00 Euros hoy ☐	**B:** 100.83 Euros dentro de 100 días ☐
DECISION 3.	**A:** 100.00 Euros hoy ☐	**B:** 101.10 Euros dentro de 100 días ☐
DECISION 4.	**A:** 100.00 Euros hoy ☐	**B:** 101.38 Euros dentro de 100 días ☐
DECISION 5.	**A:** 100.00 Euros hoy ☐	**B:** 102.08 Euros dentro de 100 días ☐
DECISION 6.	**A:** 100.00 Euros hoy ☐	**B:** 102.78 Euros dentro de 100 días ☐
DECISION 7.	**A:** 100.00 Euros hoy ☐	**B:** 103.48 Euros dentro de 100 días ☐
DECISION 8.	**A:** 100.00 Euros hoy ☐	**B:** 104.19 Euros dentro de 100 días ☐
DECISION 9.	**A:** 100.00 Euros hoy ☐	**B:** 104.91 Euros dentro de 100 días ☐
DECISION 10.	**A:** 100.00 Euros hoy ☐	**B:** 105.63 Euros dentro de 100 días ☐
DECISION 11.	**A:** 100.00 Euros hoy ☐	**B:** 107.09 Euros dentro de 100 días ☐
DECISION 12.	**A:** 100.00 Euros hoy ☐	**B:** 110.06 Euros dentro de 100 días ☐
DECISION 13.	**A:** 100.00 Euros hoy ☐	**B:** 114.67 Euros dentro de 100 días ☐
DECISION 14.	**A:** 100.00 Euros hoy ☐	**B:** 122.79 Euros dentro de 100 días ☐
DECISION 15.	**A:** 100.00 Euros hoy ☐	**B:** 131.47 Euros dentro de 100 días ☐
DECISION 16.	**A:** 100.00 Euros hoy ☐	**B:** 140.76 Euros dentro de 100 días ☐
DECISION 17.	**A:** 100.00 Euros hoy ☐	**B:** 150.70 Euros dentro de 100 días ☐
DECISION 18.	**A:** 100.00 Euros hoy ☐	**B:** 172.71 Euros dentro de 100 días ☐
DECISION 19.	**A:** 100.00 Euros hoy ☐	**B:** 197.90 Euros dentro de 100 días ☐
DECISION 20.	**A:** 100.00 Euros hoy ☐	**B:** 226.72 Euros dentro de 100 días ☐

OK

Fig. 2. Stage 2 user interface.

Contrary to what happens in Stage 1, subjects make only one decision for MPL, in that they are simply asked to indicate their "switching point" (if any) from option A (€ 100 "today") to option B (€ $100\left((1+y_k)^{\frac{\tau}{365}}\right)$ in τ days). Thus, "time consistency" (see Section 4.1.3) within each MPL – but not across MPLs – is artificially imposed.

3.5. Stage 3. Belief elicitation (only for T_1)

As we explained earlier, at the end of Stage 2 subjects are matched in pairs. In one of our treatments, (T_1, BELIEF-SOCIAL) subjects are asked to predict their matched partner's decisions in Stages 1 and 2. Predictions are incentivized, as detailed in Section 3.8.

3.6. Stage 4. Social time preference elicitation

In Stage 4, for each matched pair, subjects are assigned the role of Dictator or Recipient (with the exception of T_2, where all subjects can be considered as "Dictators" of their own fate). All subjects are reminded about their own choices in Stages 1 and 2. Then, both Dictators and Recipients go through – once again – the same sequence of MPLs as in Stage 2.[8] Subjects' information on others' risk and time preferences – together with the presence of payoff externalities – define our treatment conditions, as follows:

- In our baseline treatment (T_0, INFO-SOCIAL: 6 sessions, 288 subjects), Dictators are informed about their partner's choices in Stages 1 and 2 before making their decision for the pair.
- In the BELIEF-SOCIAL treatment (T_1: 2 sessions, 96 subjects) Dictators are reminded about their own predictions of Stage 3 before making their decision for the pair. As in T_0, the Dictator's decision has payoff consequences for the pair in that the Dictator's choice imposes a payoff externality on the Recipient.
- In the INFO-PRIVATE treatment (T_2: 3 sessions, 144 subjects), all subjects receive information about the decision of their matched partner in Stages 1 and 2 exactly as in T_0, but no payoff externalities are imposed. Thus, all subjects choose again across all 10 decision rounds, τ, the payoff they would like to receive for themselves (as in Stage 2).
- In the NO-INFO-SOCIAL treatment (T_3: 2 sessions, 96 subjects), neither Dictators receive information about the Recipients' previous decisions, nor we elicit the Dictators' beliefs about Recipients' behavior in Stages 1 and 2. By analogy with treatments T_0 and T_1, Dictators' decisions have payoff consequences for their matched Recipients.

Fig. 3 reports the Stage 4 user interface for our baseline treatment (INFO-SOCIAL). As Fig. 3 shows, the top (bottom) screen provides information about the lottery (intertemporal) choices of Stage 1 and 2, for both the deciding subject (Player A) and her assigned partner (Player B), where the information about the latter refers to the Recipient's actual choice (or the Dictator's elicited belief) depending on the treatment condition. In the NO INFO-SOCIAL (T_3) treatment the Player B column is hidden, in that Dictators make a decision for the pair without any information on the Recipients' decisions in Stages 1 and 2.

3.7. Matching

Along the development of this research project, three are the matching protocols that have been used.[9]

1. *Random Matching (RM)*. In this case, Dictators and Recipients are randomly matched, with no further restriction.
2. *Dissortative Matching (DM)*. In this case, we use data from Stage 2 to compute the average switching point per subject across all 10 decision rounds, τ, where average switching point is taken as a proxy of individual discounting (the higher the switching point, the lower the discounting). We then match the most patient Dictator with the most impatient Recipient, the second most patient Dictator with the second most impatient Recipient, and so on. This design feature makes that Dictators are the most patient subjects in half of the couples, to provide sufficient dispersion/variability in the data minimize the possibility of matchings between subjects with very similar time preferences, thus making social preferences very difficult to identify.
3. *Efficient Random Matching (ERM)*. In this case, we impose that consistent Dictators are randomly matched with consistent Recipients whenever possible. This design enhances efficiency of our structural estimation exercise (see Section 4.1.3).

Table 1 summarizes our treatment layout, including information on the number of sessions (by matching protocol) and the number of subjects (Dictators) in each of the treatments.

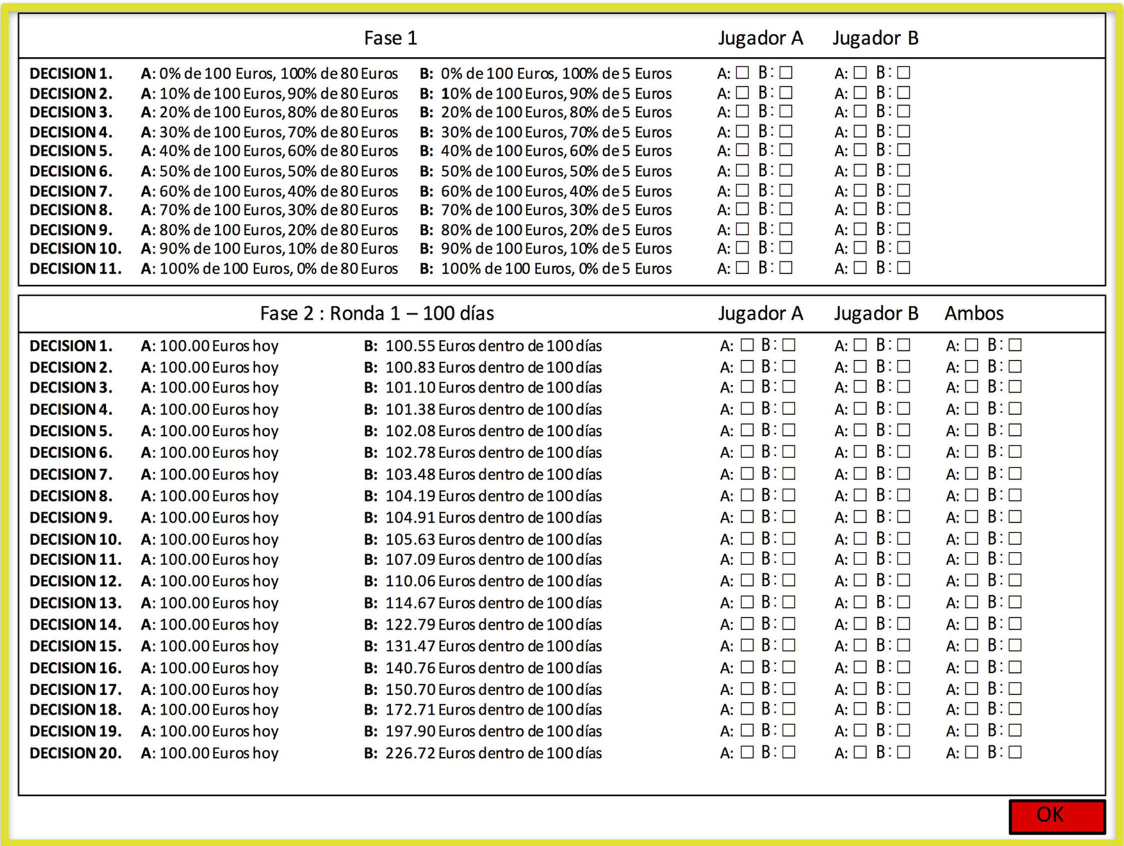
3.8. Financial rewards

All subjects receive € 10 just to show up. For the payment of Stages 1 and 2, we select at random one subject and one decision per session for payoff. By analogy, in Stage 3 we randomly pick one subject and one Stage 1 *or* Stage 2 prediction. A prize of € 100 is paid in case of a correct guess.[10] As for Stage 4, we follow the same payment protocol as in Stage 2:

[8] Also Recipients go through the same sequence of decisions, although it is made clear in the instructions that Recipients' decisions have no monetary consequences on either party.

[9] We are grateful to two anonymous referees for expanding the scope of the paper and considering alternative matching protocols.

[10] This, in turn, implies that our belief elicitation protocol is neutral to subjects' degree of risk aversion (see Andersen et al., 2014a).



Fase 1			Jugador A	Jugador B
DECISION 1.	A: 0% de 100 Euros, 100% de 80 Euros	B: 0% de 100 Euros, 100% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 2.	A: 10% de 100 Euros, 90% de 80 Euros	B: 10% de 100 Euros, 90% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 3.	A: 20% de 100 Euros, 80% de 80 Euros	B: 20% de 100 Euros, 80% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 4.	A: 30% de 100 Euros, 70% de 80 Euros	B: 30% de 100 Euros, 70% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 5.	A: 40% de 100 Euros, 60% de 80 Euros	B: 40% de 100 Euros, 60% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 6.	A: 50% de 100 Euros, 50% de 80 Euros	B: 50% de 100 Euros, 50% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 7.	A: 60% de 100 Euros, 40% de 80 Euros	B: 60% de 100 Euros, 40% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 8.	A: 70% de 100 Euros, 30% de 80 Euros	B: 70% de 100 Euros, 30% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 9.	A: 80% de 100 Euros, 20% de 80 Euros	B: 80% de 100 Euros, 20% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 10.	A: 90% de 100 Euros, 10% de 80 Euros	B: 90% de 100 Euros, 10% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 11.	A: 100% de 100 Euros, 0% de 80 Euros	B: 100% de 100 Euros, 0% de 5 Euros	A: ☐ B: ☐	A: ☐ B: ☐

Fase 2 : Ronda 1 – 100 días			Jugador A	Jugador B	Ambos
DECISION 1.	A: 100.00 Euros hoy	B: 100.55 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 2.	A: 100.00 Euros hoy	B: 100.83 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 3.	A: 100.00 Euros hoy	B: 101.10 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 4.	A: 100.00 Euros hoy	B: 101.38 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 5.	A: 100.00 Euros hoy	B: 102.08 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 6.	A: 100.00 Euros hoy	B: 102.78 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 7.	A: 100.00 Euros hoy	B: 103.48 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 8.	A: 100.00 Euros hoy	B: 104.19 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 9.	A: 100.00 Euros hoy	B: 104.91 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 10.	A: 100.00 Euros hoy	B: 105.63 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 11.	A: 100.00 Euros hoy	B: 107.09 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 12.	A: 100.00 Euros hoy	B: 110.06 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 13.	A: 100.00 Euros hoy	B: 114.67 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 14.	A: 100.00 Euros hoy	B: 122.79 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 15.	A: 100.00 Euros hoy	B: 131.47 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 16.	A: 100.00 Euros hoy	B: 140.76 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 17.	A: 100.00 Euros hoy	B: 150.70 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 18.	A: 100.00 Euros hoy	B: 172.71 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 19.	A: 100.00 Euros hoy	B: 197.90 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐
DECISION 20.	A: 100.00 Euros hoy	B: 226.72 Euros dentro de 100 días	A: ☐ B: ☐	A: ☐ B: ☐	A: ☐ B: ☐

OK

Fig. 3. Stage 4 user interface (T_0 to T_2).

Table 1
Treatment conditions.

Cod.	Treat.	Info	Pay. ext.	#Sessions	(RM/DM/ERM)	#Subj. (dict.)
T_0	INFO-SOCIAL	Yes	Yes	6	(1/2/3)	288 (144)
T_1	BELIEF-SOCIAL	Beliefs	Yes	2	(2/0/0)	96 (48)
T_2	INFO-PRIVATE	Yes	No	3	(1/1/1)	144 (144)
T_3	NO INFO-SOCIAL	No	Yes	2	(2/0/0)	96 (48)
			Total	13	(6/3/4)	624 (384)

one matched pair and one decision is selected at random and both, the Dictator and the Recipient, are paid according to the Dictator's choice.

All choices are paid at the end of the experiment, when we randomly select 2 subjects per stage for the payment of a randomly selected decision.[11] The show-up fee and the decisions for Stages 1 and 3 are paid in cash on the same day of the experiment. By contrast, we take extreme care with the payment of Stages 2 and 4, as we are concerned with the transaction costs associated with receiving delayed payments (including physical costs and payment risk). To make all choices equivalent except for the timing dimension, all payments are made by way of a bank transfer to the subjects' account. This is to minimize transaction costs and equalize them across periods, including payments for subjects who opt for the payment "today".[12] The dates of all delayed payments were set to avoid public holidays and weekends.

[11] Although this method yields a compound lottery over the various stage decisions, there exists substantial evidence showing that this does not create a response bias (see, among others, Starmer and Sugden, 1991; Cubitt et al., 1998 and Hey and Lee, 2005).

[12] We run all sessions at 10 a.m. to ensure that subjects would receive the bank transfer the same day of the experiment if this was selected for payment. To control for credibility in the payment method, we add a formal legal contract between the legal representative of the laboratory (LINEEX) and the subjects who were selected for payment. This contract is privately received by the subjects in an envelop and includes a formal statement on a 20% compensation if payments do not take place at the stated date.

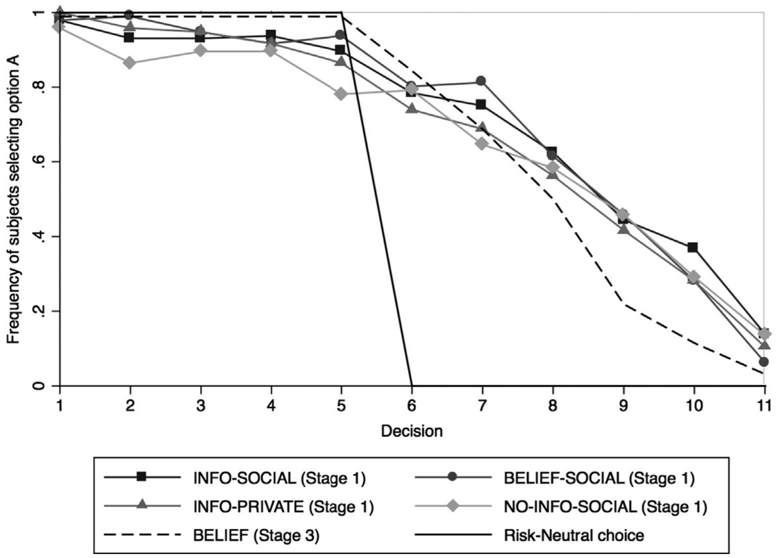


Fig. 4. Aggregate behavior in the lottery tasks.

3.9. Debriefing

All sessions end with a (computerized) debriefing questionnaire including, among others,

1. *standard socio-demographics*, such as gender, that we code using the dummy variable **Female** that takes value 1 for females (it is 0 otherwise); the rooms/household size ratio, **RSR**, a standard proxy of the household wealth, together with the self-reported weekly budget, **WB**;
2. *proxies of cognitive ability*, such as Frederick's (2005) Cognitive Reflection Test (**CRT**), a 3-item task of quantitative nature designed to measure the tendency to override an intuitive and spontaneous response alternative that is incorrect and to engage in further reflection that leads to the correct response;
3. *proxies of social capital* drawn from the *World Values Survey*, such as self-reported measures of individual **happy**ness, or personal inclinations toward **trust** (see Glaeser et al., 2000) and **inequality**.

4. Results

Section 4.1 provides summary statistics of our behavioral data, stage by stage, while in Section 4.2 we perform a structural estimation exercise, where subjects' risk, time and social preferences are framed within the realm of a parametric welfare function consisting in a convex linear combination between the Dictator's and the Recipient's "selfish" utilities. Section 4.1.3 includes a discussion on the consistency of choices and how it affects our structural estimations.

4.1. Descriptive statistics

4.1.1. Stages 1 and 3: risk preferences

Fig. 4 plots the relative frequencies of subjects selecting the "safe" option (A) across all 11 lotteries in Stage 1 (all treatments) and Stage 3 (treatment BELIEF-SOCIAL). Fig. 4 also reports optimal choices under Risk Neutrality (RN), which correspond to the lottery with the highest expected value (i.e., Option A in the first 5 decisions and Option B thereafter).[13]

As Fig. 4 shows, subjects display aggregate risk aversion, in that switching to Option B occurs at a slower pace, compared with the RN benchmark ($p < 0.001$) (e.g., Holt and Laury, 2002; Harrison and Rutström, 2008).[14] As expected, we do not detect any significant treatment conditions using the Krusall–Wallis test ($p = 0.148$).[15]

4.1.2. Stages 2 and 3: individual time preferences

Remember that, for each of the 10 delays, τ, subjects must identify the minimum amount of money (if any) they would need to receive in the future against the immediate bank transfer of € 100. Fig. 5 summarizes subjects' behavior in stages

13 Figure D1 in Appendix D reports the same information by matching protocol.

14 Unless otherwise stated, all reported p-values are derived from a (two-tail) Wilcoxon–Mann Whitney test between-subject and a Wilcoxon signed-rank test within-subject.

15 See Eckel and Grossman (2008).

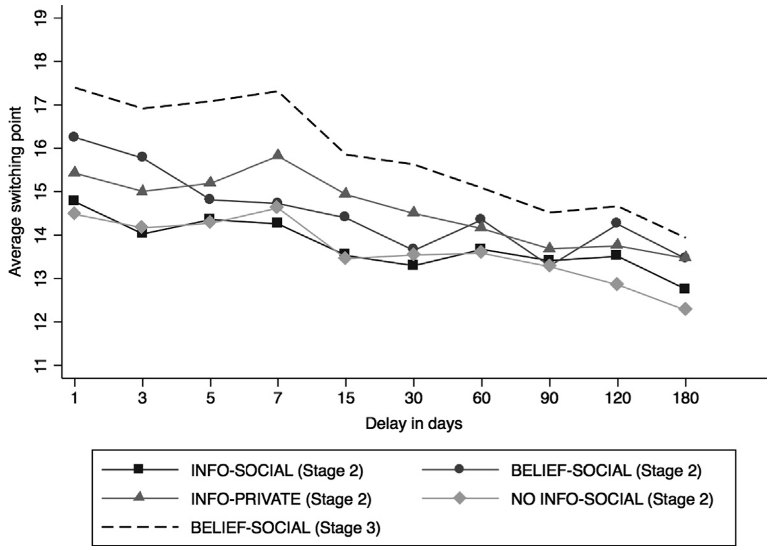


Fig. 5. Aggregate behavior in the intertemporal task.

2 (all treatments) and 3 (treatment T_1), with the vertical axis representing the distribution of "average switching points", that is, the first decision (out of a sequence of 20) for which subjects express their preference for the delayed payment.[16]

As Fig. 5 shows, average switching points decrease with delay (i.e., for increasing delays, subjects' indifference interest rate goes down). This evidence contradicts (is consistent with) exponential (hyperbolic) discounting, respectively.

4.1.3. Inconsistent behavior

To the extent to which, in the structural estimations of Section 4.2, we frame subjects' behavior within the realm of specific parametric models (along with all the implicit auxiliary assumptions that come with them), we are interested in a prior check on whether observed behavior satisfies basic *consistency* conditions compatible with our postulated theoretical setup.

In the MPL of Stage 1, standard behavioral restrictions (namely, monotonicity, first-order stochastic dominance and transitivity) require that subjects who face the MPL of Stage 1 satisfy the following

Condition 1. A subject should choose option A in the first row, option B in the last row, and switch from option A to B once – and once only – along the sequence.

We also look along similar lines at the intertemporal decisions of Stage 2. Remember that we force subjects to switch at most once within each MPL, i.e., consistency is artificially imposed *within* delays, τ, by the same experimental design. No further restriction is imposed by the experimental protocol when comparing choices *across* MPLs. In this respect, a natural requirement is contained in the following

Condition 2. If a subject prefers € 100 today against any higher amount € x at some point τ in the future, then, for all $\tau' > \tau$, she should never prefer €$x' < x$ against € 100 today.[17]

Fig. 6 reports an overview of our data with regard to inconsistent behavior, as defined by both conditions 1 and 2. We consider four different categories, depending on whether subjects are in/consistent in the risk (Condition 1) and/or the time preference (Condition 2) task. As Fig. 6 shows, roughly 60% of our pool (352 subjects out of 624) passes both our consistency tests, and we cannot reject the null that the distribution of in/consistent subjects is the same across treatments (Krusall–Wallis test, two-tail: $p = 0.98$).

Motivated by the evidence of Fig. 6, we are interested in characterizing subjects' inconsistency by way of the observable heterogeneity that can be inferred by the debriefing questionnaire. To this aim, we first partition our subject pool in four groups, depending on their risk (intertemporal) in/consistency, respectively. Since our two proxies of consistency are strongly correlated (Spearman Beta = 0.17, $p < 0.01$), Table 2 reports the estimates of *i)* the probability of being inconsistent in either task by way of a bivariate probit regression, where the set of covariates includes proxies from the questionnaire and

[16] If a subject always prefers the immediate payment, we assign this choice with "option 21", which is also averaged out in Fig. 5. Appendix D2 reports the relative frequency of choices in favor of the immediate payment for each possible delay.

[17] Condition 2 is akin to what Tanaka et al. (2010) define as "time-inconsistent behavior".

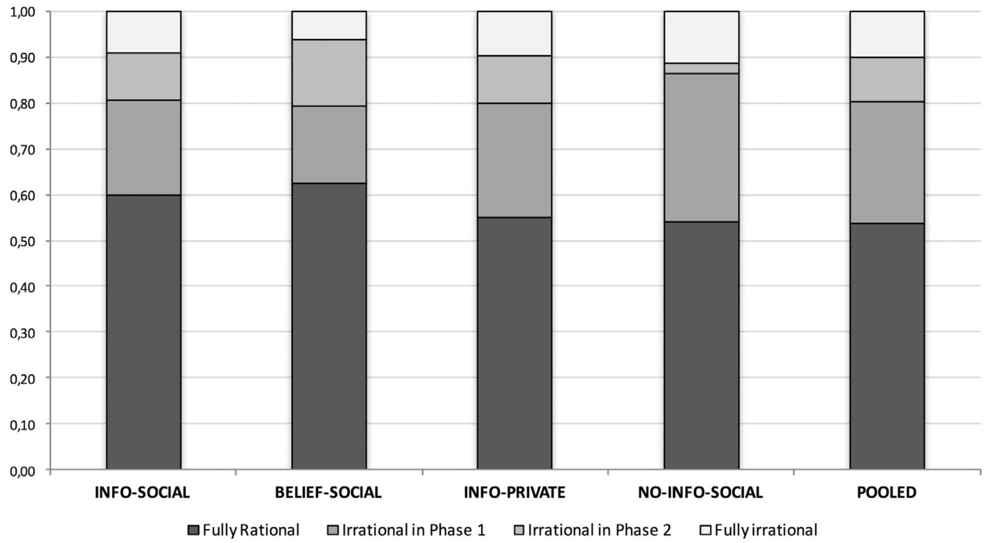


Fig. 6. In/consistent behavior in Stages 1 and 2.

ii) the probability of failing at least one of our consistency tests (**incDUMMY**) against the same set of covariates by way of a standard logit regression.[18] As it turns out, both gender and cognitive reflection play a key role in our estimations. This is why we include in the regressions an interaction term, and also report our estimations by males and females.

Our findings suggest a positive (negative) significant effect of **Female** (**CRT**) on the likelihood of inconsistent behavior in any of the two stages, as both marginal effects are highly significant. When we condition our estimates on gender, we observe that **CRT** has a significant (negative) effect, but only for females. By contrast, socio-demographics or social capital proxies have only a marginal impact in all regressions. This is consistent with previous results in the literature (take, e.g., Frederick, 2005 and Cueva et al., 2016).[19]

Once we have acquired a better grasp on the main determinants of inconsistent behavior, the next – rather delicate – question is what to do with those subjects who do not pass our consistency tests. This is because our behavioral paradigm – with specific reference to the structural estimations of Section 4.2 – imposes much stronger consistency conditions, whose violation may affect our numerical exercise in unexpected directions, whose interpretation goes well beyond the scope of this paper. In this respect, our analysis of individual behavior in Stages 1 to 3 (sections 4.1.4 and 4.2.1) follows the approach in Dean and Ortoleva (2012) or Sutter et al. (2013) in that we discard all observations from inconsistent subjects. This reduces our database to 352 subjects out of 624 (56%). As for the analysis of Dictators' choices in Stage 4 (sections 4.1.4 and 4.2.2), we focus the analysis on those consistent Dictators who satisfy Conditions 1 and 2. This reduces our database to 210 subjects out of 384 (53%). As some of these consistent Dictators might have received information about inconsistent Recipients in the INFO-SOCIAL and the INFO-PRIVATE treatments, we control for the inconsistency of Recipients in the regressions of Table 6. Along similar lines, in our structural estimations we also check whether Dictators' beliefs in treatment T_1 satisfy the same consistency conditions. Table 3 summarizes the number of in/consistent pairs in each treatment. This includes information about consistent beliefs in treatment T_1 and consistent Dictators in the NO INFO-SOCIAL treatment, where Dictators received no information about their matched Recipient.

As Table 3 shows, not only the majority of the pairs (210 out of 384, 55%) is characterized by a consistent Dictator – something we already know from Fig. 6 – but also that pairs with both consistent Dictators *and* Recipients are the majority within this subgroup (139 pairs out of 210: 66%). In treatments T_0 and T_2 this is partially due to the use of our *ERM* matching protocol in some of the sessions (see Table 1).

4.1.4. Social motives vs. social influence. Some preliminary evidence

We begin our descriptive analysis of Stage 4 by looking at the difference between the intertemporal choices in Stage 2 and 4, to be interpreted as a necessary condition for the existence of social motives/influence. Panel (a) of Fig. 7 reports the relative frequency of rounds where the decisions of consistent Dictators in Stages 4 differ from those in Stage 2. To assess the relative importance of social motives/influence, Panel (b) displays, for each time delay, the relative frequency of informed Dictators who change their choices in T_0 (INFO-SOCIAL) *vs.* T_2 (INFO-PRIVATE). Panel (c) looks directly at the focusing effect by showing the behavior in treatments with payoff externalities and no information (T_1: BELIEF-SOCIAL *vs.*

[18] The reported marginal effects follow the approach put forward by Ai and Norton (2003) and Karaca-Mandic et al. (2012) in the estimation of marginal effects in nonlinear models that include interaction terms. We also run a probit regression (not reported here) with qualitatively similar results.

[19] The interested reader on the effects of cognitive reflection and gender in intertemporal preferences can look, among others, at Benjamin et al. (2013) and Coller and Williams (1999). Ponti and Rodriguez-Lara (2015) and Cueva et al. (2016) investigate on the link between CRT and social preferences.

Table 2
In/consistent behavior: regression results.

	Inconsistent (pooled data)			Inconsistent (males)			Inconsistent (females)		
	Stage 1	Stage 2	incDUMMY	Stage 1	Stage 2	incDUMMY	Stage 1	Stage 2	incDUMMY
Logit estimates									
Female	0.602***	0.313**	0.905***						
	(0.137)	(0.148)	(0.215)						
Cognitive reflection (CRT)	−1.022***	−0.033	−0.901*	−1.015***	0.009	−0.903*	−1.719***	−1.509***	−3.043***
	(0.343)	(0.322)	(0.486)	(0.341)	(0.316)	(0.488)	(0.358)	(0.407)	(0.594)
Interaction (Gender × CRT)	−0.654	−1.430***	−2.134***						
	(0.490)	(0.522)	(0.767)						
Weekly budget	0.002*	0.002*	0.004**	0.002	−0.001	0.004	0.002	0.005***	0.004
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Room size ratio	0.158	0.175*	0.288*	0.102	0.151	0.322	0.194	0.226*	0.247
	(0.098)	(0.093)	(0.171)	(0.148)	(0.145)	(0.264)	(0.139)	(0.122)	(0.222)
Happiness	−0.834***	−0.159	−1.013***	−0.640**	−0.407	−0.907*	−0.964***	0.015	−1.088**
	(0.218)	(0.216)	(0.339)	(0.323)	(0.329)	(0.486)	(0.293)	(0.294)	(0.478)
Trust (general social survey)	−0.494**	0.047	−0.339	−0.176	0.325	−0.069	−0.696**	−0.065	−0.555
	(0.236)	(0.274)	(0.375)	(0.368)	(0.438)	(0.567)	(0.309)	(0.361)	(0.507)
Inequality loving	−0.861**	−0.041	−0.844	−0.318	0.410	−0.262	−1.198**	−0.240	−1.288
	(0.415)	(0.484)	(0.663)	(0.663)	(0.815)	(1.051)	(0.537)	(0.607)	(0.873)
Constant	0.410	−1.174**	0.263	−0.141	−1.261*	−0.317	1.367***	−0.964	1.637**
	(0.388)	(0.475)	(0.636)	(0.580)	(0.761)	(0.951)	(0.509)	(0.590)	(0.833)
Marginal effects									
Female	0.392***	0.184***	0.470***						
	(0.028)	(0.024)	(0.029)						
Cognitive reflection (CRT)	−0.466***	−0.241***	−0.495***						
	(0.079)	(0.083)	(0.077)						
Obs.	624	624	624	277	277	277	347	347	347

Notes. Standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3
Number of in/consistent pairs in each treatment.

	T_0 INFO-SOCIAL	T_1 BELIEF-SOCIAL	T_2 INFO-PRIVATE	T_3 NO INFO-SOCIAL	POOL DATA
Consistent Dictators	80 (0.55)	30 (0.62)	79 (0.54)	21 (0.43)	210 (0.55)
Consistent pairs (Dictators *and* Recipients)	58 (0.40)	27 (0.56)	54 (0.37)	.	.
Consistent Dictators, inconsistent Recipients	22 (0.15)	3 (0.06)	25 (0.17)	.	.
Inconsistent Dictators (*or* Both Inconsistent)	64 (0.45)	18 (0.37)	65 (0.45)	27 (0.56)	174 (0.45)
Number of Dictators	144	48	144	48	384

Note. There is no information about the Recipient in T_1 (BELIEF-SOCIAL) and T_3 (NO-INFO-SOCIAL), but we report in the table the number of pairs in which consistent Dictators have a consistent/inconsistent belief in the former treatment.

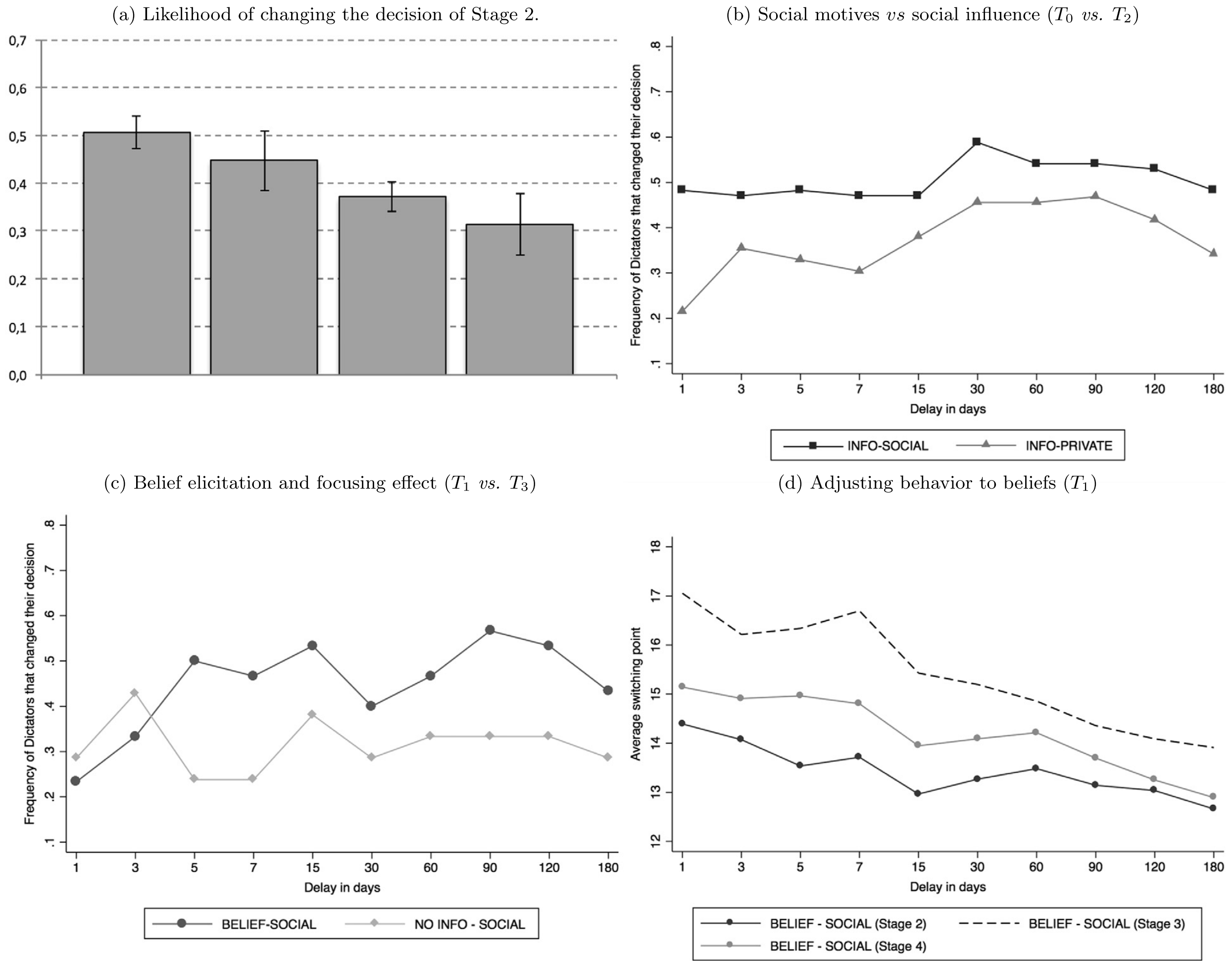


Fig. 7. Dictators' decisions in Stage 4.

T_3: NO INFO-SOCIAL). Further evidence on the effect of eliciting beliefs is presented in Panel (d), where we show how consistent Dictators move their switching point into the direction of their beliefs in Stages 2 to 4 of treatment T_1.

As Panel (a) shows, when Dictators are provided with information about the Recipients' decisions, changes in behavior are more likely in the presence of payoff externalities (INFO-SOCIAL: 50.6% *vs* INFO-PRIVATE: 37.1%) while, in the absence of information, Dictators tend to change their behavior more frequently if beliefs are elicited (BELIEF-SOCIAL: 44.7% *vs* NO INFO-SOCIAL: 31.4%).[20] Panels (b) and (c) confirm this preliminary evidence disaggregating our observations by time delay. As Panel (b) shows, informed Dictators are more likely to change their decision when the latter has payoff consequences for the Recipients. Therefore *social motives* seem stronger than *social influence* ($p < 0.004$). On the other hand, Panel (c)

[20] Table D3 in Appendix D reports the estimated coefficients of a probit regression on the likelihood of changing the decision in Stage 4 controlling for Dictactors' individual characteristics.

Table 4
Choices of Dictators in Stage 4 compared with those of Recipients in Stage 2.

	T_0 INFO-SOCIAL	T_1 BELIEF-SOCIAL	T_2 INFO-PRIVATE	T_3 NO INFO-SOCIAL	POOL DATA
(a) Frequency of Recipients' choices matched by Dictators who moved their choices in Stage 4					
Choices move towards the Recipients' choices	0.67	0.56	0.56	0.29	0.29
Recipients' choices are perfectly matched	0.15	0.09	0.14	0.06	0.06
Choices move against the Recipients' choices	0.18	0.36	0.30	0.65	0.65
(b) Tobit regression on the switching point in Stage 4: $\varphi^{\tau}_{P4} = (1-\alpha)\,\varphi^{\tau}_{P2} + \alpha\ \varphi^{\tau}_{P3}$					
Estimates of alpha	0.262***	0.481***	0.116***	0.050	0.186***
	(0.049)	(0.146)	(0.027)	(0.041)	(0.028)

Notes. In the Tobit regression, φ^{τ}_{Pk} corresponds to the switching point of Dictators in Stage $k = \{2, 4\}$ when the future payment is delayed τ days. The value of φ^{τ}_{P3} denotes the switching of the matched Recipient or the Dictator's elicited belief. Robust standard errors (clustered at the individual level) in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

shows that, without information, eliciting beliefs seem to trigger social preferences, in that Dictators are more likely to change their choices in the BELIEF-SOCIAL compared with the NO-INFO-SOCIAL treatment ($p < 0.096$).[21] This latter evidence is in line with the idea of "focusing" put forward by Krupka and Weber (2009), where belief elicitation has a positive effect on pro-social behavior. Finally, Panel (d) shows that Dictators believe that Recipients are more impatient than they are. Interestingly, Dictators in the BELIEF-SOCIAL treatment seem to weight their own preferences and beliefs about the Recipients' preferences and choose a switching point in Stage 4 that is roughly between the two. This evidence is perfectly in line with our treatment of social time preferences.

In our paper, we are not only interested in detecting a change of behavior between stages 2 and 4, but also the *direction* of such changes. One question to be addressed is then under which treatment the behavior of the Recipients is better matched by their assigned Dictator. In Panel (a) of Table 4 we disaggregate the evidence of Fig. 7(a) by looking, by treatment, at the relative frequency of Dictators' choices that *i*) move toward, *ii*) perfectly match or *iii*) move against the Recipients' choices (belief) of Stage 2 (3), respectively. As Table 4 shows, with the exception of T_3, a clear majority of choices in Stage 4 has changed with respect to Stage 2 in the direction of the Recipients' preferences.[22]

Panel (b) of Table 4 provides a quantitative assessment on the statistical significance of such changes by estimating a double censored tobit model (clustered for subjects) by which the switching point of Dictators in Stage 4, $\varphi^{\tau}_{S4} \in \{1, \ldots, 21\}$, is calculated as a convex linear combination between own choices in Stage 2 (φ^{τ}_{S2}) and the information received or the Dictator's elicited beliefs, (φ^{τ}_{S3}):

$$\varphi^{\tau}_{S4} = (1-\alpha)\varphi^{\tau}_{S2} + \alpha\varphi^{\tau}_{S3} + \varepsilon_{\tau}.$$

As Panel (b) shows, α is positive and highly significant in all treatments with the exception of T_3 (NO INFO-SOCIAL), this confirming that Dictators' thresholds move in the direction of those of their groupmates, once they know (or they reflect upon) the others' time concerns.[23] This effect seems stronger in the presence of payoff externalities (where social motives apply), but does not vanish without them, as a further sign of the empirical content of social influence, too. Consistently with our finding in Fig. 7, the estimated α is the highest (lowest) in the BELIEF-SOCIAL (NO INFO-SOCIAL) treatments, respectively. In this respect, framing the decision of the Dictator *as an explicit choice between two selves* – whether actual or simply fictitious – seems to work as a necessary condition for a detectable change in behavior in the direction of the other's decision. When we look at the intertemporal choices of Stage 4, we indeed find that Dictators are more likely to follow their own choices of Stage 2 in the INFO-PRIVATE compared with the INFO-SOCIAL ($p = 0.019$) treatment. Similarly, Dictators are more likely to follow their own choices in the NO-INFO-SOCIAL compared with the BELIEF-SOCIAL treatment ($p < 0.037$) (see Appendix D3).

4.2. Structural estimations

The estimates of Table 4 show a significant shift in the direction of the Recipient's decision, conditional upon *i*) the provision of some explicit information (or belief) about the latter's decision, and/or *ii*) a modification of the incentive structure to experimentally induce payoff externalities. These considerations notwithstanding, the estimates of Table 4 look at our behavioral evidence on intertemporal decisions *only*, disregarding the information on individual risk preferences collected in Stage 1. As we already discussed in Section 2, this may introduce a confound – namely, Dictators' heterogeneity in own risk concerns – that our own experimental design can, indeed, control for. This is the reason why we test the

[21] When doing pairwise comparisons, we compute, for each Dictator, the frequency of rounds in which the decision was changed and treat each Dictator as an independent observation. All our findings are robust if we restrict our sample to consistent pairs instead of consistent Dictators.

[22] See Figure D3 in Appendix D for the same evidence disaggregated by time delays.

[23] The estimated coefficient for the NO INFO-SOCIAL treatment makes sense only within the realm of some "rational expectation" hypothesis, since Dictators are never informed about their matched Recipient's decision. Nevertheless, we report the T_3 estimated coefficient for the sake of completeness, and also for a direct comparison with that of T_1, where also Dictators are not informed, but their beliefs about the Recipients' decisions are elicited.

Table 5
Risk and time preferences: structural models of individual behavior (Stages 1 to 3).

	(1) Exponential discounting		(2) Hyperbolic discounting			(3) Mixture model			
	Risk (ρ)	Time (δ)	Risk (ρ)	Time (δ)	Time (β)	Risk (ρ)	Time (δ)	Time (β)	π
Private "own" decisions (Stages 1 and 2)									
Pooled data	0.853***	0.898***	0.858***	0.261***	0.848***	0.825***	0.992***	0.690***	0.231***
	(0.008)	(0.099)	(0.008)	(0.026)	(0.007)	(0.009)	(0.025)	(0.014)	(0.019)
INFO-SOCIAL (T_0)	0.874***	1.133***	0.878***	0.195***	0.860***	0.848***	0.837***	0.725***	0.244***
	(0.012)	(0.215)	(0.012)	(0.033)	(0.011)	(0.014)	(0.028)	(0.026)	(0.035)
BELIEF-SOCIAL (T_1)	0.859***	1.133***	0.864***	0.219***	0.836***	0.837***	0.927***	0.634***	0.246***
	(0.02)	(0.214)	(0.020)	(0.069)	(0.022)	(0.024)	(0.065)	(0.025)	(0.063)
INFO-PRIVATE (T_2)	0.829***	1.179***	0.833***	0.382***	0.861***	0.791***	1.151***	0.674***	0.173***
	(0.015)	(0.132)	(0.015)	(0.062)	(0.013)	(0.019)	(0.050)	(0.023)	(0.032)
NO INFO-SOCIAL (T_3)	0.825***	1.682***	0.831***	0.335***	0.808***	0.799***	1.352***	0.687***	0.323***
	(0.021)	(0.231)	(0.021)	(0.072)	(0.016)	(0.026)	(0.088)	(0.023)	(0.048)
Beliefs about Recipients (Stage 3)									
BELIEF-SOCIAL (T_1)	0.809***	1.259***	0.812***	0.495***	0.872***	0.766***	1.368***	0.742***	0.172***
	(0.020)	(0.163)	(0.021)	(0.103)	(0.021)	(0.026)	(0.061)	(0.048)	(0.063)

Notes. Robust standard errors in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

robustness of our previous findings by means of some structural estimations in which we frame (consistent) Dictators' behavior as maximizing various parametric random utility functions, some related with the individual decisions of stages 1 to 3, others which include both the individual ("selfish") utilities of the Dictator and the Recipient as a result of some social preference – or social influence – process of joint utility maximization, depending on the treatment.

To this aim, we follow Andersen et al. (2006) by conditioning our estimations upon the following stationarity condition:

$$u_i(M_0) = \Delta_i(\tau) u_i(M_\tau), \tag{1}$$

where $u_i(x) = x^{1-\rho_i}/(1-\rho_i)$ is a standard (time independent) CRRA utility function and $\rho_i \neq 1$ is the risk aversion coefficient. With this parametrization, $\rho_i = 0$ identifies risk neutrality, with $\rho_i > 0$ ($\rho_i < 0$) identifying risk aversion (risk loving) behavior, respectively. As in Coller et al. (2012), the discount factor is assumed to be $\Delta_i(\tau) = \beta_i/(1+\delta_i)^\tau$, with $\beta_i = 1$ ($\beta_i < 1$) in the case of exponential (hyperbolic) discounting, respectively. The estimations we report in the remainder of this paper follow a standard "maximum likelihood" approach, by which the estimated parameters (jointly) maximize the likelihood of observed choices in the different stages of the experiment, conditional on the structural parametrization (1) and the auxiliary assumption that choices made by the same subject across different stages are statistically independent.[24]

In Section 4.2.1 we collect pool estimates of (own) risk (ρ) and intertemporal preferences (β and δ) using the evidence from Stages 1 to 3. As for social time preferences/influence, Section 4.2.2 estimates the weights of a social welfare function where individual (own) risk and discounting parameters are estimated separately for each subject participating to the experiment.

4.2.1. Stages 1–3: individual choices

Table 5 replicates Table 2 in Coller et al. (2012) by estimating pool parameters of our structural model (1) using observation from stages 1 to 3. Model 1 imposes $\beta = 1$; i.e., it assumes exponential discounting for all observations. We remove this assumption in Model 2, which allows for hyperbolic discounting. Finally, we consider in Model 3 a "binary mixture model" that estimates – jointly with the other behavioral parameters, ρ, δ and β – the ex-ante probabilities, denoted by π ($1-\pi$), that each individual observation is an independent draw from Model 2 (Model 1), respectively. The last line of Table 5 replicates our structural estimations using the evidence from Stage 3 of the BELIEF-SOCIAL treatment.[25]

We look first at the pool estimations (first row of Table 5). Our estimates for Model 1 qualitatively confirm those of Coller et al. (2012) in that our (consistent) subjects exhibit significant CRRA and discounting. Similar considerations hold for Model 2: β is significantly smaller than 1, thus providing empirical content to the hyperbolic discounting hypothesis. By the same token, the estimated value of δ significantly drops with respect to the estimate of Model 1, as it occurs in Coller et al. (2012). When we consider the mixture Model 3, defined as the probability-weighted average of exponential and hyperbolic discounting, we find that the probability of the latter model being the correct one, π, is estimated to be around 23% and highly significant. In this sense, we reject the null by which choices can be explained by exponential discounting only.[26] The estimates of the three models disaggregated by treatment suggest little variability across treatments, since we

[24] A detailed description of our identification strategy is presented in Appendix A.

[25] As we explained in Section 3, our belief scoring rule is neutral to the (CRRA) risk aversion parametrization, since subjects either win the price when they guess correctly, otherwise they get nothing. As a consequence, maximizing expected payoffs is equivalent to maximizing winning probabilities (i.e., our scoring rule only serves the purpose of eliciting the mode of subjects' belief distribution). Under the assumption that subjects formulate their beliefs using the behavioral model in equation (1) that we use to frame their own behavior, we can map subjects' beliefs into the same (ρ, δ) behavioral space.

[26] We also note that our estimate for π is significantly lower than the one reported by Coller et al. (2012) (point estimate: 0.59; std. err. 0.07; 95% confidence interval (0.45, 0.74)). This, in turn, seems to be in line with Andersen et al. (2014b), where it is suggested that present-bias preferences may

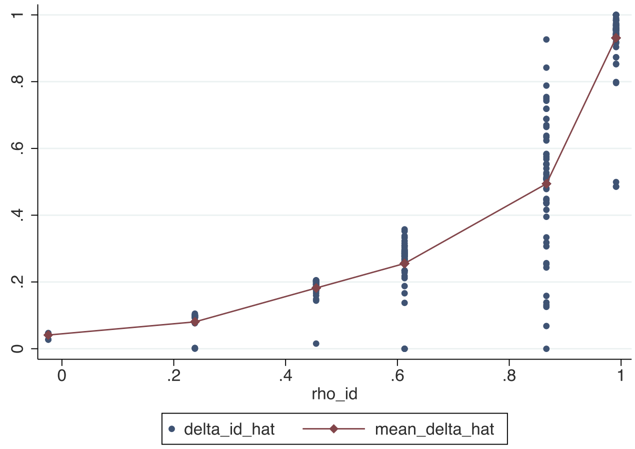


Fig. 8. Individual parameter distribution: $\hat{\delta}$ *vs.* ρ.

can never reject the null of joint equality of the estimated coefficients across treatments. Our estimates for Stage 3 in T_1 show that ρ (δ) is not as high (low) as the corresponding estimate for Stage 2, which is in line with the descriptive statistics of Section 4.1: subjects believe that their groupmates are less risk averse and more impatient (for all models, the corresponding differences are always significant at at least 5 % confidence). Interestingly enough, our mixture model estimates reveal that subjects' beliefs underestimate the relevance of hyperbolic discounting in their groupmates' decisions, as the estimated value for β is significantly smaller.

We now move to between-subject heterogeneity, which we study by estimating our equation (1) for each consistent subject. Due to lack of observations at the individual level, we can only get estimates for Model 1, where we impose exponential discounting. Let $\hat{\delta} = \frac{1}{1+\delta}$ denote the individual discount factor. Fig. 8 reports the scatter diagram of the estimated $(\hat{\delta}, \rho)$ pairs characterizing each consistent subject participating to the experiment.

As Fig. 8 shows, risk (ρ) and time ($\hat{\delta}$) preferences are strongly correlated: *more risk averse subjects turn out to be also more patient*. If we calculate the Spearman correlation coefficient between $\hat{\delta}$ and ρ we get a value of 0.78 ($p < 0.0001$). In this respect, our evidence is consistent with that of Dean and Ortoleva (2012), Burks et al. (2009) and Epper et al. (2011).

4.2.2. Stage 4: social motives vs. social influence

Dictators' choices in Stage 4 are framed as maximizing a welfare function consisting of a convex linear combination between their own and their assigned Recipient's risk and intertemporal concerns (precisely, the individual specific parameters reported in Fig. 8):

$$v_i^k(\tau) = (1-\alpha_i)\Delta_i(\tau)\left(\frac{x(\tau)^{1-\rho_i}}{1-\rho_i}\right) + \alpha_i \Delta_j(\tau)\left(\frac{x(\tau)^{1-\rho_j}}{1-\rho_j}\right), \tag{2}$$

where ρ_j and $\Delta_j(\tau)$ correspond to the risk and discount individual parameters of i's assigned Recipient, j. In this respect, our estimation strategy consists in two steps. We first estimate, the maximum-likelihood individual parameter profile (ρ_i, δ_i) estimated using data from Stages 1 and 2, together with the elicited beliefs in Stage 3 for those subjects participating in the BELIEF-SOCIAL treatment. Once we have obtained individual estimates for each subject, we estimate the probabilities that any given consistent Dictator i in Stage 4 resolves the same sequence of intertemporal decisions assuming that i is maximizing the welfare function (2), derived as the convex linear combination between the utilities of Dictator i and Recipient j, whether using directly j's estimated parameters (treatments INFO-SOCIAL and INFO-PRIVATE), or the elicited parameters of Stage 3 (treatment BELIEF-SOCIAL).[27]

Table 6 reports our estimation results. Panel (a) does not condition on whether a consistent Dictator is matched with an in/consistent Recipient. Panel (b) estimates a constant and a coefficient associated with a dummy which is positive for matchings between consistent Dictators and inconsistent Recipients. In other words, the constant identifies the value of α resulting from matchings between consistent Dictators and Recipients, while the dummy measures the effect, for a consistent Dictator, to be matched with an inconsistent Recipient.

We look at Model (a) first. Here we see that – somewhat in line with the descriptive statistics of Section 4.1.4 – the estimated value of α is positive in all cases. However, it is significant (at 1% confidence) for treatment T_0 (INFO-SOCIAL) and

be less prominent than previously suggested by the literature. One possible factor driving our results is that we exclude inconsistent Dictators from the analysis, what may indeed affect our estimates of hyperbolic discounting (see Meier and Sprenger, 2015 for a discussion along these lines).

[27] Dictators do not receive any information (nor we elicit their beliefs) in the NO INFO-SOCIAL treatment, therefore equation (2) cannot be identified in T_3.

Table 6
Structural model.

	TR_0 INFO-SOCIAL	TR_1 BELIEF-SOCIAL	TR_2 INFO-PRIVATE	POOL
(a) Estimates of α for Consistent Dictators (CD)				
Const.	0.739***	0.612	0.394*	0.527
	(0.214)	(0.999)	(0.204)	(0.413)
(b) Estimates of α for Consistent Dictators, conditioned on (In)Consistent Recipients				
Const.	0.758***	0.612	0.101	0.245
	(0.270)	(0.999)	(0.102)	(0.457)
Inc. Rec.	−0.070	N/A	0.769***	0.589
	(0.292)		(0.113)	(0.469)

Notes. Robust standard errors (clustered at the individual level) in parentheses: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

significant only at 10% confidence in the case of T_2 (INFO-PRIVATE). Things are different when we condition our estimates to the consistency of the Recipient. As Panel (b) shows, the estimate of the constant in T_0 remains positive and highly significant. The same does not happen in T_2, where we cannot reject the null of absence of social influence in our data as for the matchings between consistent Dictators and Recipients are concerned. By contrast, the effect of being matched with an inconsistent Recipient is negligible (highly significant) in T_0 (T_2), respectively. This suggests that social motives are more important than social influence when we only consider pairs composed by consistent Dictators *and* Recipients. Along these lines, we find that the effect of being matched with an inconsistent Recipient is only significant for T_2, which explains why the unconditional estimate of Model (a) for T_2 is significant. By contrast, we cannot reject the null for the INFO-SOCIAL data, which indicates that behavior of consistent Dictators in T_0 does not seem to vary significantly depending on the consistency of their assigned Recipient.

To summarize, once we control for the Dictators' and Recipients' estimated risk aversion, both the effects of the social influence and focusing seem to lose force, compared with our social motives motivating theme. In particular, the estimated parameter in T_1 is never significant, while in T_2 is (marginally) significant only when we do not condition on the consistency of the Recipient.

5. Conclusion

Using evidence from a laboratory experiment, we have tested several complementary working conjectures on the influence of others in individual intertemporal decisions. According to the "social influence" conjecture, being simply acknowledged of the choices of someone else is sufficient to trigger a change in behavior; according to the "focusing" conjecture, forcing subjects to form beliefs over the time preferences of others is sufficient to move behavior in the direction of beliefs. Our motivating conjecture, instead, calls for social time preferences as a result of some conscious deliberation in which the others' intertemporal concerns are explicitly taken into account when decisions yield payoff externalities.

To different degrees, the descriptive analysis of Section 4.1.4 supports all these working conjectures, as changes in behavior (in the direction of the Recipient) are more likely in the presence of *i*) information about others' decisions (even in absence of any payoff externality), *ii*) belief elicitation (even in absence of any information about others' decisions) and *iii*) payoff externalities (especially in conjunction with information about others' decisions). The structural estimations of Section 4.2, however, favor social motives with respect to the other competing conjectures, especially when we restrict our attention to consistent pairs of Dictators and Recipients, that is, an environment more in tune with a well defined process of conscious deliberation. However, it should be noticed that *i*) this result has been obtained at the cost of a drastic reduction in the sample size, leaving unexplained the behavior of significant fraction of our subject pool and *ii*) since our structural model has been tailored around our social time preference working hypothesis, it is far from straightforward how to interpret equation (2) within the realm of a model of "social cues" ("focusing"), in absence of a compelling structural link between the motives (the actions) of Dictators and Recipients, respectively.

As for avenues for future research, we consider our results as groundwork for exploring *endogenous* matching processes, where people – when considering intertemporal decisions with payoff externalities – may cluster or delegate time decisions depending on others' (risk and) time preferences.

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