Similarity and social discounting*

Benjamin Beranek[†] Geoffrey Castillo[‡]

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Social discounting refers to the idea that decision-makers discount payoffs as a function of social distance. We introduce a method to measure social distance using interpersonal similarity; that is, how similar or different others are to the decision-maker. We use data from our own preregistered experiments as well from an existing, independently conducted, lab-in-the-field experiment to estimate the structural parameters of social discounting and find evidence for it. Our experiments control for competing explanations to isolate the effect of similarity and thus show that people have a preference for more similar others. Our estimates imply that in order for a decision-maker to willingly forgo \$1 and have it instead benefit a dissimilar other, then it would need to increase to at least \$1.25. We also find evidence for quasi-hyperbolic social discounting.

Keywords: similarity, social distance, social discounting, choice experiment, Inclusion of Other in the Self
JEL Codes: C91, D01, D64, D90

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[†]Department of Finance, Economics and Risk Management, Missouri State University. `benjaminberanek@missouristate.edu`

[‡]Centre for Behavioural Sciences, Department of Economics, Nottingham Business School, Nottingham Trent University. `geoffrey.castillo@ntu.ac.uk`

1 Introduction

Social distance influences how we perceive the outcomes of others. More precisely, we tend to prefer payoffs that go to those who are socially close to us, and discount payoffs as social distance increases. This idea, referred to as 'social discounting', has a long tradition in economics. It was already present in the writings of Smith (1759), Edgeworth (1881) and Marshall (1975), as highlighted by Collard (1975) and Sally (2001). For example, Edgeworth (1881) wrote:

> ... between the frozen pole of egoism and the tropical expanse of utilitarianism, there [is a] position of one for whom in a calm moment his neighbour's happiness as compared with his own neither counts for nothing, nor yet 'counts for one,' but counts for a fraction. We must modify the utilitarian integral ... by multiplying each pleasure, except the pleasures of the agent himself, by a fraction—a factor doubtless diminishing with what may be called the social distance between the individual agent and those of whose pleasures he takes account.

Since then social discounting has been somewhat lost, with a few exceptions in the theoretical literature (Sally, 2001; Tabellini, 2008). Its absence can be attributed to the challenge of measuring social distance. Indeed, social distance is a multi-faceted concept with many, sometimes contradictory, components (Karakayali, 2009; Rummel, 1976). And without a good measure of social distance, one cannot estimate social discounting.

In this paper, we provide the missing tools to measure social distance and estimate social discounting. We measure social distance via interpersonal similarity; that is, how similar or different others are to the decision-maker. Similarity is an integral component of social distance (Liviatan et al., 2008, and the references therein) which, importantly for our purposes here, has the advantage of being objectively quantifiable. Similarity is by itself an important concept, and has been linked to positive outcomes in many domains, for instance in the labour market, health, trade, and education.[1] In two preregistered experiments, we measure the extent to which people prefer more similar others while controlling for confounding factors. We

[1] See respectively Behncke et al. (2010), Alsan et al. (2019), Guiso et al. (2009), and Gershenson et al. (2022). We come back to this literature at the end of the introduction.

confirm that subjects take similarity into account when making choices and directly estimate the structural parameters of social discounting. Finally, we re-analyse data from an existing, independently conducted, lab-in-the-field experiment, and replicate our results. Observing the same results in both settings increases their external validity and shows that they are not dependent on our design.

Our contributions are thus three-fold. First, we offer a method to measure social distance in experiments via interpersonal similarity. This method does not rely on real relationships—with their associated confounds—yet it remains incentive compatible. It can be easily implemented to test existing and new theories that use social distance. Second, we show how social discounting can be tested and its primitives estimated. This allows for the implications of social discounting to be investigated; for example, we can examine how the level of support people have for redistributive policies relates to the social distance between themselves and those benefiting from the policies. Third, our results show that indeed people have a preference for more similar others. Our experiments rule out competing explanations, such as the anticipation of future interactions.

In Section 2, we start by formalising social discounting. We consider a decision-maker who faces options characterised by two attributes: attribute x is an amount of money allocated to a recipient while attribute s is the similarity between the decision-maker and the recipient. Social discounting suggests that the preferences of the decision-maker over these options can be described by the utility function $U(x, s) = D(s)\, u(x)$, where $D(s)$ is the social discount function and $u(x)$ is the utility of money. The social discount function $D(s)$ captures the decision-maker's preferences for similarity. The main question of this paper relates to the shape of $D(s)$. If $D(s)$ is flat, the decision-maker discounts money received by all recipients in the same way, irrespective of the similarity between the decision-maker and the recipients. Instead, social discounting predicts that $D(s)$ is downward-sloping: the decision-maker increasingly discounts money received by recipients as similarity decreases.

We design an experiment with the aim of estimating the social discount function $D(s)$. Our main ingredient is a choice task: subjects make a series of binary choices between options (x, s) in which we systematically vary both the amount of money x and similarity s. For this, we first need to measure and create variation in similarity. Our experimental design, which we present in Section 3, accomplishes this in two steps. First, we survey members of the US population to create a set of passive subjects. We ask them 40 questions adapted from existing sources such as the American Census, the American Community Survey, the General Social Survey, and the World Value Survey. Then, we invite a second set of subjects and ask them the same questions. As soon as they finish answering the 40 questions, we compare their answers from some of these questions to the answers of the passive subjects. Independently for each of them, we then select a series of passive subjects depending on the number of answers they have in common. The selection ensures that a given subject is very similar to some of their selected passive subjects and very dissimilar to others.

To properly measure social discounting, we carefully designed our experiment such that subjects have no reason to act other than money x and similarity s. We control for selfishness by having the options never benefit the subjects themselves. We control for reciprocity by having the options only benefit the passive subjects and not the subjects who actually take part in our experiment. We control for reputation concerns by keeping anonymity between subjects and passive subjects. Even more, the passive subjects do not know why they receive money or that this experiment even took place.

We present our results in Section 4. Using a popular measure from social psychology—the Inclusion of Other in the Self scale (Aron et al., 1992)—we confirm that subjects feel more connected to more similar others. We then directly estimate a model of social discounting. We use an exponential social discount function $D(s) = \delta^s$ where δ is the social discount factor. To account for heterogeneity, we use a random coefficient framework and estimate the parameters of the model using the method of maximum simulated likelihood. We find an average social discount factor $\delta = 0.9$, which means that the social discount function $D(s)$ is indeed downward-sloping. Our estimates imply that the decision-maker would be indifferent between themselves receiving $1 or the average least similar other we observe in our experiment receiving $1.25. While most subjects can thus be described as social discounters, we also observe about 25% of 'social magnifiers' who prefer options that benefit not more similar others but rather more dissimilar others.

In order to test for hyperbolic discounting, we design an extension of our experiment. We do so by including the self as a potential recipient when generating the options among which subjects choose. We also increase the number of passive recipients a subject faces along with the variation in similarity. We conduct this extension on a separate sample and estimate a quasi-hyperbolic social discount function $D(s) = \beta\,\delta^s$, which parallels the quasi-hyperbolic model in intertemporal choice (Laibson, 1997). In addition to replicating our original results, we find evidence for quasi-hyperbolic social discounting, meaning that subjects give themselves a premium compared to even very similar recipients.

In Section 5, we exploit the richness of our dataset to perform a number of exploratory analyses. We first look at which elements of similarity matter the most for subjects. We find that the most important elements of similarity are age, religion, and the place where one grew up. Contrary to our expectations, however, subjects are on average *more* likely to give to those who are of a different race. Upon closer inspection, we see that this result is primarily driven by white subjects who make up a majority of our sample, whereas non-white subjects are *less* likely to give to those who are of a different race. Second, we look at which demographic characteristics best predict being a social magnifier. We find that social magnifiers are more likely to be male and less likely to be Roman Catholic or Republican. We also find that the likelihood of being a social magnifier decreases as household income increases.

Our experimental design choices were made with an aim to isolate the effect of similarity and remove competing explanations. However, one might be concerned that we rendered similarity too salient and thus exposed ourselves to experimenter demand effects. In Section 6, we address these concerns by re-analysing data from an existing lab-in-the-field experiment that uses a completely different design and showing that our results replicate. The data comes from Robson (2021), who conducted a series of experiments in Uganda. In these experiments, subjects allocate a budget between themselves and two other subjects, in a manner similar to Andreoni and Miller (2002) and Fisman et al. (2007). As in our experiments, demographic characteristics of subjects are measured which allows us to compute the index of similarity. A main difference from our experiments is that all subjects sit in the same room, and the only way for allocators to infer similarity is by visually identifying the recipients. Overall, we replicate our results, which shows that they are not dependent on our particular design.

We close the paper in Section 7 with a discussion of our results.

Related literature. First, we contribute to a large literature showing that interpersonal similarity shapes a range of outcomes by highlighting the existence of a preference channel. This literature spans several fields and focuses on different settings. For example, Behncke et al. (2010) show that unemployed individuals are more likely to find and retain a job when they are matched with caseworkers who are of the same age, gender, education, and nationality. Alsan et al. (2019) show that Black patients are more likely to choose preventive health interventions when they are treated by Black doctors. Additional examples are easy to find, for example with respect to the perception of migrants, education, international trade, development, and sports.[2] Similarity can drive these results through a variety of channels. For example, Behncke et al. (2010) find that people trust each other more as their similarity increases, and Alsan et al. (2019) find evidence that similarity leads to better communication. Further, the homophily literature finds that people who are more similar to each other interact more often with each other (McPherson et al., 2001). We show that even after removing these competing channels—trust, communication, more frequent interactions, and so on—people still prefer more similar others.

Our second contribution is to offer a method to study social distance that sits between two opposites. To study social distance, one possibility is to use existing relationships, for example classmates (Goeree et al., 2010) or friends (Gächter et al., forthcoming). This method adds realism and external validity, but comes with social image concerns and the anticipation of future interactions (Leider et al., 2009). On the other side of the spectrum, another possibility is to use hypothetical relationships and thus hypothetical questions (Enke et al., 2022; Jones and Rachlin, 2006). This method removes concerns about repeated game effects, but it also removes incentives altogether. Our method preserves incentives

[2] In the early 20$^{\text{th}}$ century, people born in the US were more likely to express political discontent against immigration when the cultural difference between themselves and immigrants was higher (Tabellini, 2020). Having a more similar teacher leads to positive educational outcomes (Dee, 2004, 2005; Gershenson et al., 2022). Cultural distance affects trust and thus trade between countries (Guiso et al., 2009). More diversity—and thus less similarity—among residents in communities decreases the provision of public goods (Alesina et al., 1999), the formation of social capital (Alesina and La Ferrara, 2000), and trust (Alesina and La Ferrara, 2002; see also Alesina and La Ferrara, 2005 and Alesina and Giuliano, 2015 for reviews). Football players from a more similar cultural background are more likely to pass the ball to each other (Békés and Ottaviano, forthcoming).

while still maintaining complete anonymity and removing any kind of interaction between players. We thus strengthen the results of these studies by showing that their results—that people prefer closer others, being friends or other hypothetical persons—hold even after removing confounding factors. In other words, people have a preference for socially close others, controlling for other possible explanations.

Our third contribution is to generalise the results from the social identity literature (Akerlof and Kranton, 2000; Chen and Li, 2009).[3] This literature shows that people favour those who belong to their same ingroup over those who belong to an outgroup. We depart from this binary setting by recognising that people belong to multiple groups at the same time. Some group memberships we consider in this study are those based on gender, age, race, ethnicity, place of residence, and so on. Our measure of similarity accounts for the fact that, when we consider others, some of our group memberships overlap and others do not. Therefore, our continuous measure of similarity is broader than a binary case of social identity as it considers multiple characteristics all at once. Our approach can thus more easily be used in settings where individuals share multiple, partially overlapping memberships in various ingroups and outgroups.

Finally, note that social discounting is not exclusive to economics. It has resurfaced in psychology with Jones and Rachlin (2006), and Tiokhin et al. (2019) count more than 50 recent studies on social discounting.[4] The psychology literature generally relies on hypothetical social distances and thus hypothetical options to test social discounting. In contrast, and as we have already argued, we use objective, pre-existing, and measurable social distances—similarity—while still maintaining anonymity.

2 Social discounting

Consider a decision-maker i who faces options characterised by two attributes, x_j and s_{ij}. Attribute x_j is an amount of money received by recipient j while attribute s_{ij} is the social distance between i and j. We proxy s_{ij} with similarity, one of the

[3]See also Balliet et al. (2014) and Lane (2016) for reviews.

[4]Social discounting has been applied to several topics, for example to explain contributions in the public goods game (Jones and Rachlin, 2009), cigarette smoking during pregnancy (Bradstreet et al., 2012), externalising behaviour problems (Sharp et al., 2012), and organ donation (Vekaria et al., 2017). See Jones (2022) for a recent review.

primary components of the broader concept of social distance (Karakayali, 2009; Liviatan et al., 2008; Rummel, 1976). Hence, $s_{ij} \in [0, 1]$ measures the similarity between i and j. If $s_{ij} = 0$, i and j are identical; if $s_{ij} = 1$, i and j are as dissimilar as possible.

According to social discounting, preferences over these options (x_j, s_{ij}) can be captured by the multiplicative utility function $U_i(x_j, s_{ij}) = D_i(s_{ij})\, u_i(x_j)$, where $D_i : [0, 1] \to \mathbb{R}_+$ is the social discount function and $u_i : \mathbb{R} \to \mathbb{R}$ is the utility of money. Since the decision-maker is identical to themselves, $s_{ii} = 0$, we follow Edgeworth (1881) and Sally (2001) and assume that $D_i(0) = 1$: the decision-maker does not discount money they receive.

Note that u_i only depends on x_j, and U_i, only on D_i and u_i. In other words, the decision-maker does not exhibit inequality aversion, and they always judge x_j using their own preferences and not j's preferences.[5] We will keep these assumptions throughout and thus write more succinctly $U(x, s) = D(s)\, u(x)$.

Our main objective is to identify the shape of the social discount function $D(s)$. Figure 1 represents different possible shapes for $D(s)$. To begin with, if $D(s)$ is flat, then the decision-maker does not discount money to others as a function of similarity. The decision-maker always chooses the option that provides the largest amount of money to any recipient, regardless of similarity. We thus have our first hypothesis:

Hypothesis 1. $D(s) = c$ for some constant $c \in [0, 1]$ and for all $s \in]0, 1]$.

Hypothesis 1 implies that the first derivative of $D(s)$ is null, $D'(s) = 0$. Observing that $D'(s) < 0$ and rejecting Hypothesis 1 would provide evidence for social discounting.

We will consider two special cases of Hypothesis 1. If $c = 0$, the decision-maker strictly prefers money for themselves and similarity plays no role. On the other hand, if $c = 1$, the decision-maker treats money to others exactly the same as they treat money for themselves and does not discount at all. We will come back to these cases and make them explicit when we introduce a functional form for $D(s)$.

[5] Both of these assumptions could be relaxed to augment social discounting in subsequent work. An example of including inequality aversion would be to use Fehr and Schmidt (1999) preferences with $u_i(x_i, x_j) = x_i - \alpha_i \max(x_j - x_i, 0) - \beta_i \max(x_i - x_j, 0)$. One could further assume that the parameters that govern inequality aversion also depend on s_{ij} and write $\alpha_i(s_{ij})$.

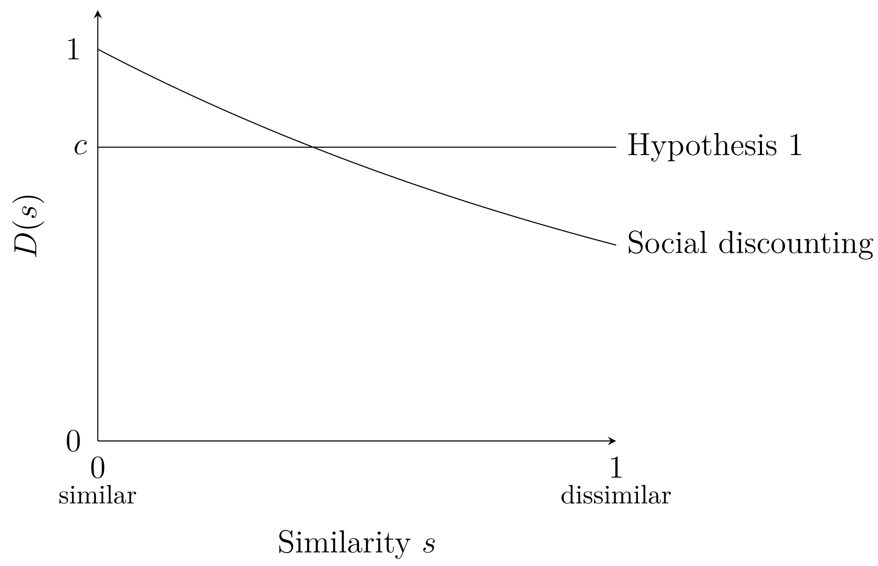


Figure 1: The social discount function $D(s)$ and Hypothesis 1.

3 Experimental design

We design an experiment with the objective of estimating the social discount function $D(s)$. As we have seen, if the social discount function $D(s)$ is downward-sloping, then a decision-maker discounts money received by others as a function of similarity. If instead $D(s)$ is flat, then the decision-maker considers all others in the same way.

To estimate the social discount function, we will present subjects with options that vary amounts of money and similarity and observe their choices. For this, we first need a way to measure interpersonal similarity and generate variation in it.

3.1 Measuring and varying interpersonal similarity

We measure and vary interpersonal similarity using a two-step process.

Step 1: Gathering passive subjects. We first surveyed members of the US population to create a set of passive subjects. We asked them 40 questions adapted from existing sources such as the American Census, the American Community Survey, the General Social Survey, and the World Value Survey. The questions are a mix of standard demographic questions—sex, age, location, race, ethnicity, religion, etc.—and questions about one's opinion on different topics—confidence in several institutions, trust in others, etc. Appendix A provides the full list of questions.

Step 2: Generating variation in similarity. We then invited a second set of subjects and asked them to answer the same questions. As soon as each subject in Step 2 submitted their answers, we looked at the difference between their answers and the answers from all the passive subjects in Step 1. For example, if a subject in Step 2 answered 'white' to the question in which we asked their race, the difference for this variable would be 0 when compared to a passive subject in Step 1 who also answered 'white' and 1 when compared to a passive subject who answered something different. As such, the difference for categorical variables can only be 0 and 1, while we normalise the difference for ordinal or continuous variables—location, age—to be between and 0 and 1.

In principle, we could compute these differences for any of the answers elicited from the survey. We chose the answers to the following subset of the questions: sex, date of birth, zip code, race, ethnicity, religion, and the place where one grew up. Our choice was driven by the fact that we needed questions whose answers could be succinctly conveyed to subjects. Further, we wanted to avoid questions that would immediately trigger equity concerns, such as household income or employment status. We also wanted to avoid loaded questions, such as support for the right to an abortion, the death penalty, or affirmative action. Finally, we avoided questions related to politics to avoid triggering partisanship, especially since we conducted our experiment close to the 2020 US elections.

After having computed the difference for each of these variables, we took the unweighted average of these differences to create an *index of similarity*. The index measures how similar the answers of a subject in Step 2 are compared to a given passive subject from Step 1 and ranges between 0 and 1. An index of 0 describes a passive subject who has given exactly the same answers to every question while an index of 1 describes a passive subject who has given answers as different as possible. We use this index of similarity to proxy for attribute s which we introduced in the previous Section.

Independently for each Step 2 subject, we computed the index of similarity

Figure 2: Card display used in the experiment.

towards all Step 1 passive subjects. Again independently for each Step 2 subject, we then order all Step 1 passive subjects from most similar to least similar based on the values of the index. To generate variation in similarity, we selected three subjects: the most similar, with the smallest index of similarity; the least similar, with the largest index; and one in the middle, with an intermediate index.

In this paper, we refer to these selected subjects as 'matches', and use the shorthands 'similar match', 'intermediate match', and 'dissimilar match'. Throughout the experiment itself, we randomly associated each match with one of the four suits from a deck of playing cards (♣ ♦ ♠ ♥) and referred to them as such to the Step 2 subjects using the card display shown in Figure 2. This way, we avoided using labels that are obviously ranked, such as $(1, 2, 3)$ or (A, B, C), when referring to the matches in the experiment itself.

The variables shown on the card display are the ones we used to compute the index of similarity. However, we did not tell subjects about the index of similarity. If we had, subjects might mechanically have chosen options that benefit matches with low indices of similarity. For similar reasons, we did not tell subjects how these matches were selected.

We revealed to subjects their three matches all at once in a randomised order as can be seen in Figure 2. Then, to make sure that subjects engaged with the specific characteristics of each of their matches, we presented the matches one by one in a randomised order and asked them to write in at least 25 characters the first things that came to mind when they read the card.

3.2 Inclusion of Other in the Self task as a manipulation check

To make sure our manipulation was effective, we used the Continuous Inclusion of Other in the Self (IOS) scale (Beranek and Castillo, 2024). The IOS scale is a popular tool to measure interpersonal closeness that is increasingly being used in economics.[6] Compared to the standard IOS scale (Aron et al., 1992), the Continuous IOS scale provides a finer measurement and, as we show in Beranek and Castillo (2024), it solves a bias of the original IOS scale whereby subjects avoid selecting low levels of closeness.[7]

Subjects evaluated each of their matches, one after the other and in a randomised order, using the Continuous IOS scale. Figure 3a shows an example. The measure obtained from the Continuous IOS scale is the degree of overlap between the two circles which represents the extent to which subjects feel connected to one of their matches. An overlap of 0 means that subjects do not feel connected, while an overlap of 1 means they feel very connected.

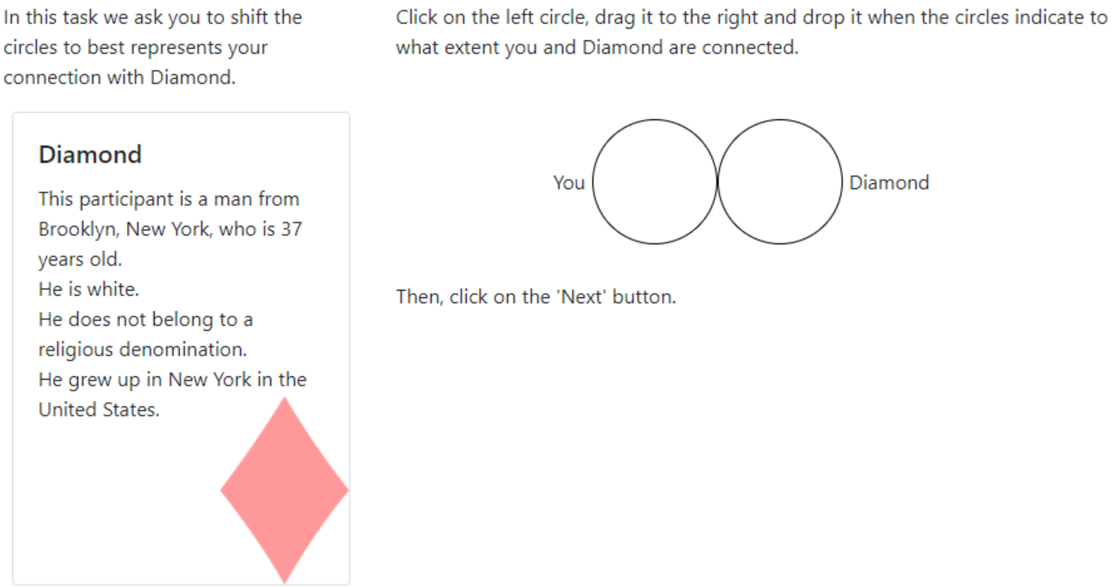
3.3 Choice task

Finally, we explain the main ingredient of our experiment: the choice task. In the choice task, subjects made a series of choices between two options. Each option features a specific amount of money going to one of their matches. Subjects could also report explicit indifference. Figure 3b provides an example.

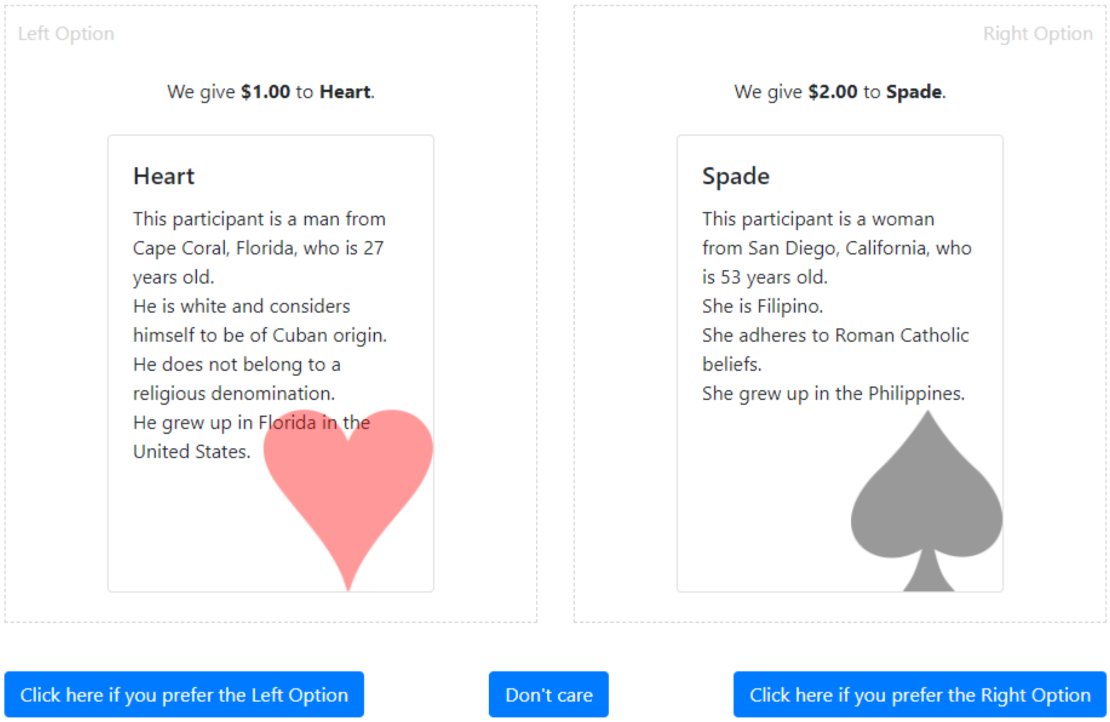
We chose three amounts of money: \$1, \$2, and \$5. With three matches and three amounts of money, that makes 9 possible options. We generate all possible binary choices among these 9 options, remove choices where there is dominance in money (e.g., \$1 for the closest match vs \$2 for the closest match) but keep choices where there is dominance in interpersonal similarity (e.g., \$1 for the similar match vs \$1 for the dissimilar match). That leaves us with 27 choices.

[6] See Aron et al. (2013) and Branand et al. (2019) for reviews of the IOS scale, and Gächter et al. (2015) for a recent validation of it. On its usage in economics, see Goette and Tripodi (2021), Robson (2021), Hofmann et al. (2021), Castillo (2021), Dimant (2024) and Gächter et al. (forthcoming) for recent examples.

[7] A demo of the Continuous IOS scale can be found on `https://geoffreycastillo.com/ios-js-demo/`.



(a) Continuous IOS task.



(b) Choice task.

Figure 3: Screenshots of the tasks used in the experiment.

To control for order effects within the choice task—e.g., seeing options that benefit the similar match before seeing options that benefit the dissimilar match—subjects saw these 27 choices in an order randomised independently for each subject. To control for order effects between tasks, the order of the IOS task and the choice task was also randomised. We show in Appendix C that there are no order effect between tasks, so we pool all the data in our analysis.

Our design has a number of additional features that allow us to isolate the effect of similarity. First, we control for selfish motives by not having options that favour the Step 2 subjects themselves. The subjects do not split a pie between themselves and their matches; instead, they face a series a binary dictator games with only other-other allocations. Second, we control for reciprocity by having options that benefit a pool of passive subjects different from those who are actively taking decisions. The Step 2 subjects knew that the targets of the options they were considering were passive subjects who had completed the survey long ago, and that the money they themselves received for their participation was not determined by the choice of another subject. Third, we control for reputation concerns by not revealing the characteristics of the Step 2 subjects to the passive subjects when making the payments. Otherwise, the Step 2 subjects might choose options based on what they think is expected of them—e.g., they might think that residents of New Jersey have to choose residents of New Jersey. We go one step further by not revealing to the passive subjects that an experiment even took place. The money the passive subjects received came directly from us without mention of any experiment, which we told the Step 2 subjects in our instructions. Therefore, it is not possible for the Step 2 subjects to choose a particular option to let a passive subject know that someone similar to them decided to send them money.

At the end of the experiment, we selected one of the 27 choices at random (also independently for each subject) and implemented the option selected. If subjects reported indifference, we selected one of the two options at random as was explained in the instructions.

We took a number of steps to convince subjects that their choices had real consequences. Some of them are standard: we used an account that has been rated positively by previous subjects in other, unrelated experiments; our IRB approval was prominently featured; and we made clear in the instructions and the control questions our intention to implement the choices. We went one step further by posting on our website the receipts of all payments we made.[8] Subjects were invited to visit our website to check the receipts and convince themselves we actually pay. So that even the first subjects were convinced too, we first ran a few pilot sessions before the actual sessions and reported those payments. Finally, we told subjects we would (and then we did) send them a notification once their payment was sent with a link to our website where they could literally check the receipts.

3.4 Implementation details

We conducted the experiment in December 2020 on Amazon MTurk. A total of 500 subjects participated in the experiment. We preregistered a data-cleaning protocol to remove from the sample subjects who submitted implausible or suspicious answers. After implementing this protocol, 355 subjects remain in the pool for analysis.[9] These subjects were all paid a flat fee of \$3.00. We implemented one of the choices each of these subjects made and paid \$3.24, on average, to their matches.

The experiment was programmed using oTree (Chen et al., 2016). The full instructions can be found in Appendix B. Our design, sampling, and analyses were preregistered on the OSF.[10]

4 Results

4.1 Similarity and Inclusion of Other in the Self scale

Before getting into our main analysis, we perform a series of simple checks. We first look at the index of similarity. Remember that for each subject we selected three matches based on their computed index of similarity towards the passive

[8] `https://geoffreycastillo.com/mturk/`

[9] We removed 87 obvious bots who, for example, copied-and pasted the instruction text or random text found online into any of their free text responses. We also removed 56 subjects who exhibited at least two instances of suspicious behaviour; for example, completing the entire survey in less than 3 minutes or completed the experiment from the same IP address as another subject. All of these exclusion criteria were preregistered. We also exclude two subjects who managed to report a proportion of overlap greater than 1 in the Continuous IOS scale.

[10] See `https://osf.io/8gtnd` for the preregistration for the set of passive subjects and `https://osf.io/wcsaz` for the preregistration for the actual experiment.

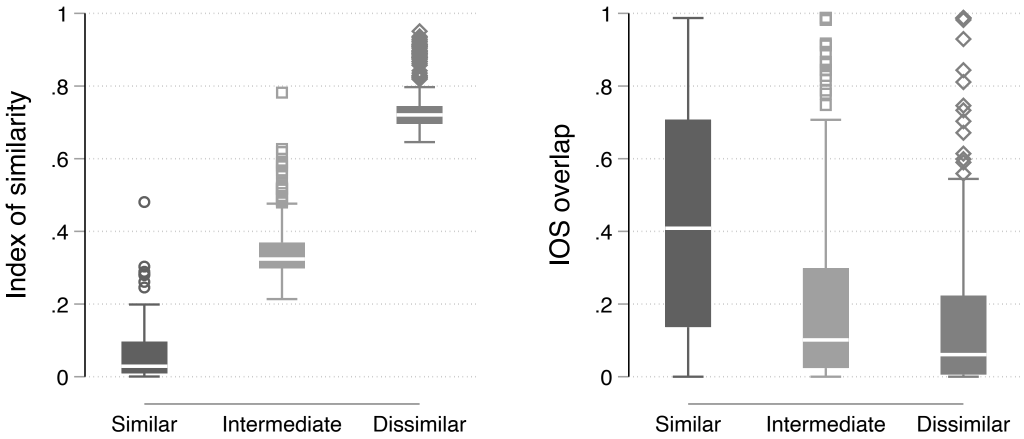


Figure 4: Box plots of the index of similarity and of the degree of overlap in the Continuous IOS scale for each match

subjects. The left panel in Figure 4 reports a box plot of the index of similarity for each match. We see that, by construction, different matches have different levels of similarity. The average index of similarity for similar, intermediate, and dissimilar matches is 0.056, 0.337 and 0.730. These values can be interpreted as the average proportion of demographic characteristics in common between subject and match on the variables shown in Figure 2—sex, date of birth, zip code, race, ethnicity, religion, and the place where one grew up.

Second, we check whether what we call similar, intermediate and dissimilar matches are perceived as such by subjects. To do that, the right panel in Figure 4 shows box plots of the Continuous IOS overlaps reported by subjects for each match. We see that subjects report a greater degree of overlap with more similar matches, which indicates a greater connection in the context of the IOS scale. We confirm this using Page's (1963) trend test (Page's L statistic $= 4660$, $p < 0.01$).

Finally, we provide a snapshot of the data in Table 1 to get a sense of the main patterns of behaviour. In this table we report the proportion of choices for the most similar match for different combinations of attributes. For example, the number in the first cell, 0.291, means that 29.1% of subjects chose the option giving the similar match a smaller amount of money over the option giving the intermediate match a larger amount of money for all choices involving this combination of attributes.

Table 1: Proportion of choices where the option with the most similar match is selected for different matches and amounts of money.

	(\$ small, \$ large)	(\$ large, \$ small)	(\$ same, \$ same)	Overall
(Similar, intermediate)	0.291	0.853	0.576	0.573
(Similar, dissimilar)	0.292	0.828	0.528	0.549
(Intermediate, dissimilar)	0.186	0.739	0.336	0.420
Number of choices	1065	1065	1065	3195

The first observation is that, if subjects always picked the most similar match irrespective of the amounts of money, we would expect proportions in every row and column to be 1. On the other hand, if subjects always picked the largest amount of money irrespective of the match, we would expect the proportions to be 0 in the first column and 1 in the second. Instead we observe proportions clearly between 0 and 1 throughout the table. Therefore, subjects trade-off similarity and amounts of money, which is what social discounting predicts.

The second observation is that, when subjects have to choose between a small amount of money for their similar match or a large amount of money for their dissimilar one, there are still about 30% of subjects who choose money for the similar match. This is true even when the amounts of money are \$1 for the similar match and \$5 for the dissimilar one. Therefore, social discounting is sufficiently strong for some subjects to lead them to prefer their similar match even when they could have provided five times as much money to a more dissimilar match.

Finally, when comparing cases where subjects choose between similar and intermediate matches to cases where they choose between intermediate and dissimilar matches, we see that the proportion of choices for the similar matches actually decreases. This suggests that at least some subjects may prefer less similar matches and behave opposite what social discounting predicts. Therefore, our estimation procedure needs to be sufficiently flexible to accommodate these cases.

4.2 Maximum likelihood estimations

We now move to the core of our analysis, the maximum likelihood estimation of the structural parameters of social discounting. As we have just seen, it is important to be able to explain the heterogeneity in our data. To account for this heterogeneity, we adopt a random coefficient framework and estimate the model using the method of maximum simulated likelihood, in the manner of Conte et al. (2011) and von Gaudecker et al. (2011).

4.2.1 Structural model and hypotheses

Recall from Section 2 that we have $U(x, s) = D(s)\, u(x)$, where $u(x)$ is the utility of money, and $D(s)$, the social discount function. To estimate the model, we assume functional forms for $u(x)$ and $D(s)$. We assume that $u(x)$ is the exponential utility function[11]

$$u(x, \alpha) = \begin{cases} 1 - \exp(-\alpha x) \text{ if } \alpha > 0, \\ x \text{ if } \alpha = 0, \\ \exp(-\alpha x) - 1 \text{ if } \alpha < 0, \end{cases} \tag{1}$$

where α captures the curvature of the utility of money.

For $D(s)$ we choose the exponential discount function $D(s) = \delta^s$ where $\delta \geq 0$ is the social discount factor. This particular form mirrors the classic exponential discount function of the Discounted Utility model in intertemporal choice (Koopmans, 1960; Samuelson, 1937). In the context of social discounting, Marshall (1975) and Tabellini (2008) use the functionally equivalent form $D(s) = \exp(-\delta s)$.

This particular functional form for $D(s)$ lends itself to two special cases of Hypothesis 1:

Hypothesis 2. $\delta = 0$,

Hypothesis 3. $\delta = 1$.

If we observe $\delta \in [0, 1]$ and reject Hypotheses 2 and 3, we know that $\delta \in\,]0, 1[$. The first derivative of $D(s) = \delta^s$ being $D'(s) = \delta^s \ln \delta$, $\delta \in\,]0, 1[$ implies that $D'(s) < 0$. Therefore, we would automatically reject Hypothesis 1 that the social discount function $D(s)$ is flat.

[11]We preregistered the power utility function $u(x, \alpha) = x^\alpha$ based on pilot data, but the model does not converge with this functional form when applied to the final sample.

4.2.2 Stochastic assumptions and likelihood

In the experiment, subject $i \in \{1, \ldots, I\}$ faces choice tasks $t \in \{1, \ldots, 27\}$. In each choice task (described in Subsection 3.3), subjects choose between a left and a right option, which we denote by ω_t^L and ω_t^R. Subjects can also report indifference, which we write as ι_t. Denote the utility difference between ω_t^L and ω_t^R by

$$\Delta U_t(\alpha_i, \delta_i) = U\left(\omega_t^L; \alpha_i, \delta_i\right) - U\left(\omega_t^R; \alpha_i, \delta_i\right).$$

To take the model to the data, we use the Fechner model (Hey and Orme, 1994): subjects make stochastic errors $\epsilon \sim \mathcal{N}(0, \sigma^2)$ when judging the utility difference such that ω_t^L is chosen when $\Delta U_t(\alpha_i, \delta_i) + \epsilon > 0$. The noise parameter ϵ is assumed to be independently and identically distributed across subjects and choices.

Denote the choice made by a subject by $c(\omega_t^L, \omega_t^R, \iota_t)$. The probability of observing subject i in choice t choosing ω_t^L is

$$\Pr\left(c(\omega_t^L, \omega_t^R, \iota_t) = \omega_t^L; \alpha_i, \delta_i\right) = \Pr\Big(\Delta U_t(\alpha_i, \delta_i) + \epsilon > 0\Big) = \Phi\left(\frac{\Delta U_t(\alpha_i, \delta_i)}{\sigma}\right),$$

where Φ is the cumulative distribution function of the standard normal distribution. The probability of choosing ω_t^R is similarly derived. In our experiment, subjects can also express indifference; in which case, we have

$$\Pr\left(c(\omega_t^L, \omega_t^R, \iota_t) = \iota_t; \alpha_i, \delta_i\right) = 0.5 \cdot \Phi\left(\frac{\Delta U_t(\alpha_i, \delta_i)}{\sigma}\right) + 0.5 \cdot \Phi\left(-\frac{\Delta U_t(\alpha_i, \delta_i)}{\sigma}\right).$$

To account for heterogeneity, we assume that the parameters are distributed according to some distribution and estimate the parameters of these distributions. We assume that α is normally distributed, $\alpha \sim \mathcal{N}(\mu_\alpha, \sigma_\alpha^2)$. Since we have the requirement $\delta \geq 0$, we assume that δ is log-normally distributed, $\ln \delta \sim \mathcal{N}(\mu_\delta, \sigma_\delta^2)$. Note that we do not restrict δ to be between 0 and 1. As we have seen before, some subjects seem to prefer less similar matches, which requires $D'(s) > 0$ and is captured by $\delta > 1$.

The contribution to the likelihood of the choices t of subject i is then

$$L_i = \iint\limits_{\mathbb{R}} \prod_{t=1}^{27} \Pr\left(c(\omega_t^L, \omega_t^R, \iota_t); \alpha_i, \delta_i, \sigma\right) f(\alpha, \delta; \mu_\alpha, \sigma_\alpha, \mu_\delta, \sigma_\delta) \,\mathrm{d}\alpha \,\mathrm{d}\delta$$

where f is the joint density of α and δ. Finally, the sample log-likelihood is $\ln L = \sum_{i=1}^{I} \ln L_i$.

We estimate $(\mu_\alpha, \sigma_\alpha, \mu_\delta, \sigma_\delta, \sigma)$ via maximum simulated likelihood.[12]

4.2.3 Estimation results

Table 2 shows the estimation results. Since δ is log-normally distributed, it is hard to infer the shape of its distribution from these estimates alone. Therefore, we also plot the estimated distribution of the parameters in Figure 5. As we can see, most of the distribution of δ is between 0 and 1, which confirms social discounting.

More formally, from $\ln \delta \sim \mathcal{N}(\mu_\delta, \sigma_\delta^2)$ we obtain $\mathbb{E}(\delta) = 0.90$ and $\mathbb{V}\mathrm{ar}(\delta) = 0.05$.[13] We reject Hypothesis 2 that $\delta = 0$ (Wald's $\chi^2 = 15405$, $p < 0.01$), leading to our first result:

Result 1. $\delta \neq 0$: subjects are not indifferent between all options; instead, they take into account money received by their matches.

Similarly, we reject Hypothesis 3 that $\delta = 1$ (Wald's $\chi^2 = 198.03$, $p < 0.01$), leading to our second result:

Result 2. $\delta \neq 1$: subjects do not disregard similarity and do not always choose the option with the largest amount of money.

The combination of Results 1 and 2, together with $\mathbb{E}(\delta) = 0.90$, leads us to our third result:

Result 3. $\delta \in \,]0, 1[$: subjects discount money received by others as a function of the similarity. In other words, they exhibit social discounting.

Therefore, we reject Hypothesis 1: the social discount function $D(s)$ is not flat but downward-sloping.

In order to individually classify subjects, we recover a posterior estimate of α_i and δ_i for each subject conditional on their 27 choices (see Conte et al., 2011, for a similar procedure). Figure 6 shows kernel density plots of these posterior estimates.

[12] We use **R** (R Core Team, 2023) with the `maxLik` package (Henningsen and Toomet, 2011). We rely on the BFGS algorithm with numerical derivatives. We use Halton sequences of length 100 per subject.

[13] Using $\mathbb{E}(\delta) = \exp\left(\mu_\delta + \frac{\sigma_\delta^2}{2}\right)$ and $\mathbb{V}\mathrm{ar}(\delta) = \left(\exp(\sigma_\delta^2) - 1\right) \times \exp(2\mu_\delta + \sigma_\delta^2)$.

Table 2: Maximum simulated likelihood estimates.

	$u(x)$ exponential, $D(s)$ power
μ_α	1.65^{***}
	(0.11)
σ_α	2.62^{***}
	(0.17)
μ_δ	-0.14^{***}
	(0.01)
σ_δ	0.24^{***}
	(0.02)
σ	0.10^{***}
	(0.01)
Log Likelihood	-4283.94

Notes. α normally distributed, δ log-normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

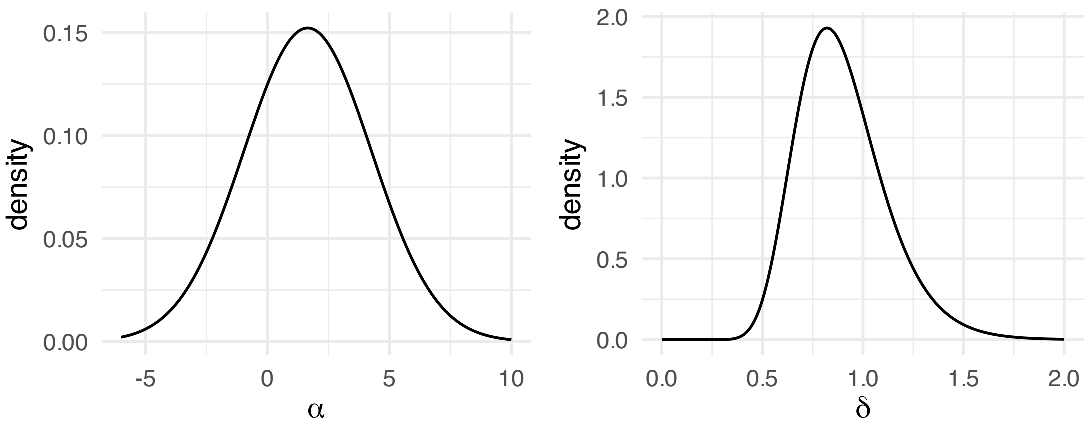


Figure 5: Plots of the distributions estimated in Table 2 of α (utility of money parameter) and δ (social discount factor).

Then, we classify subjects depending on the values of α_i and δ_i. The utility of money is concave if $\alpha_i > 0$ and convex if $\alpha_i < 0$. The shape of the social discount function is captured by δ. If $0 < \delta_i < 1$ the social discount function is downward-sloping and, as a result, subjects prefer to give money to those more similar to themselves. We classify these subjects as social discounters. If $\delta_i > 1$, the social discount function is instead upward-sloping and, as a result, subjects prefer to give money to those less similar. In other words, the utility of these decision makers is magnified as the dissimilarity between the decision maker and the recipient increases. We thus call these subjects 'social magnifiers'.

Table 3 shows the result of this classification. Focusing on δ_i, we see that 75% of subjects are social discounters. The rest are social magnifiers. Focusing on α_i, almost 75% of subjects have a concave utility function. Combining both classifications, we find that overall 55% of subjects are social discounters with a concave utility function.

4.3 Extension: quasi-hyperbolic social discounting

We have found strong evidence for social discounting using the exponential social discount function $D(s) = \delta^s$. A natural follow-up question is whether other discount functions would fit the data well. In particular, the social discounting literature in psychology finds that hyperbolic social discount functions fit the data better than exponential ones (Jones and Rachlin, 2006; Rachlin and Jones, 2008) This conclusion is also reached in the intertemporal choice literature (Cohen et al., 2020; Frederick et al., 2002) from which we took inspiration for our choice of $D(s)$.

However, because our original experiment does not include money to the self in the list of options considered by subjects, the data from it does not allow us to properly identify hyperbolic discounting. The key insight provided by hyperbolic discounting in our context is that trade-offs between self ($s = 0$) and a recipient at, say, $s = 0.1$ are different from the trade-offs between a recipient at $s = 0.3$ and a recipient at $s = 0.4$. This implication mirrors intertemporal choice, where hyperbolic discounting implies that the decision-maker makes different trade-offs between today and tomorrow and between a year in the future and a year in the future plus one day. Therefore, to identify hyperbolic social discounting, we need to include options that provide an amount of money to the self in the choice task.

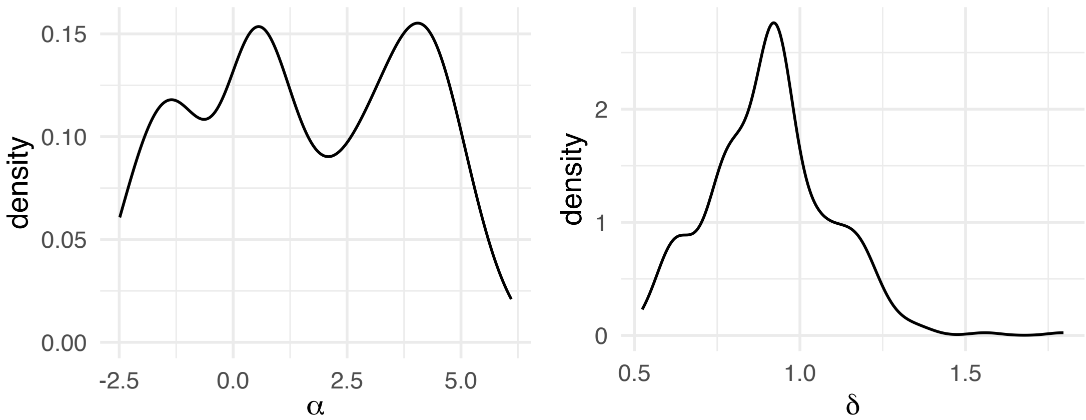


Figure 6: Kernel density plots of the posterior estimates of α (utility of money parameter) and δ (social discount factor).

Table 3: Classification of subjects based on their recovered posterior estimates.

$D(s)$ type	$u(x)$ type	n	%
Social discounters	Concave	196	55.1%
	Convex	72	20.2%
Social magnifiers	Concave	69	19.4%
	Convex	19	5.3%

Notes. Social discounters: $\delta_i < 1$; social magnifiers: $\delta_i > 1$. Concave: $\alpha_i > 0$; convex: $\alpha_i < 0$.

Further, in the initial experiment we considered special matches: the similar and dissimilar matches were the *most* and the *least* similar subjects in the set of passive subjects ordered by similarity. We also want to see whether our results hold when we consider less extreme matches.

Therefore, we designed and preregistered an extension of our experiment with two key changes. First, for each subject we collect decisions that relate to 6 matches, twice as many matches as in the previous experiment. We do so by inviting the same subjects to two sessions. The first session is in all respects identical to the previous experiment: we select from the ordered passive subjects the most similar passive subject, the least similar passive subject, and the one in the middle who is 50% of the way to the least similar match. In the second session, we instead select the second-most similar passive subject, the passive subject 25% of the way to the least similar match, and the passive subject 75% of the way.

Second, we also include the subjects themselves as possible recipients in the second session. As a result, in some choices, subjects now choose between money for themselves or money for one of their matches. This inclusion allows us to investigate social discounting for similarities close to 0 and thus directly test for hyperbolic discounting. As before, we create all possible options and all possible choices between these options which doubles the number of choices to 54.

This extension was conducted in March 2021. In total 127 subjects participated to both sessions. After implementing the same preregistered data-cleaning protocol as for our original experiment, we are left with 109 subjects whose decisions we analyse.[14] As before, we implemented one randomly selected choice for each subject and paid \$3.25, on average, to their matches. Subjects themselves were paid a flat fee of \$3.00 for each session they completed. This extension was also preregistered.[15]

We observe 27 choices in one session and 54 choices in another session, so we have 81 choices overall per subject to analyse. To capture the effect of introducing the self in the list of potential recipients, we use the quasi-hyperbolic social discount

[14] In total, 219 subjects participated in at least one of the two sessions. When we implement the protocol on the 219 subjects, we remove 31 obvious bots and 12 subjects who exhibited at least two instances of suspicious behavior, leaving 176 subjects. Among them, 109 (61.9%) completed both sessions.

[15] See `https://osf.io/u4jkh` for the preregistration.

function

$$D(s) = \begin{cases} 1 \text{ if } s = 0, \\ \beta\,\delta^s \text{ if } s > 0. \end{cases} \tag{2}$$

This function is mirrors the quasi-hyperbolic discount function used in intertemporal choice (Laibson, 1997). $s = 0$ represents the self, while $s > 0$ represents others. The term β represents the premium one places on oneself. If $\beta = 1$ we go back to the social discount function we have already used. If instead $\beta < 1$, there is a discontinuity at $s = 0$, which means the self is given a premium.

We assume that β is normally distributed, $\beta \sim \mathcal{N}(\mu_\beta, \sigma_\beta^2)$, and we estimate $(\mu_\beta, \sigma_\beta)$ in addition to the parameters we previously estimated. All other aspects of the estimation remain the same as before.

Table 4 reports the estimation results, and Figure 7, plots of the estimated distributions. We observe a slightly lower δ, with $\mathbb{E}(\delta) = 0.83$ and $\mathbb{V}\text{ar}(\delta) = 0.07$. We replicate Result 1 that subjects consider money received by their matches (Wald's $\chi^2 = 4903.6$, $p < 0.01$) and Result 2 that subjects do not consider all their matches equally (Wald's $\chi^2 = 214.27$, $p < 0.01$). We thus replicate Result 3. Further, the mean of β is 0.68 and is statistically significantly different from 1 (Wald's $\chi^2 = 312.8$, $p < 0.01$), which validates quasi-hyperbolic social discounting and gives our fourth result:

Result 4. $\beta \neq 1$: subjects give self a premium and follow quasi-hyperbolic social discounting.

As we did previously, we recover posterior estimates of the parameters of each subject conditional on their 81 choices. Figure 8 shows the corresponding kernel density plots. We then carry out the same classification exercise and show the results in Table 5. In addition to the classes we have already defined for α_i and δ_i, we also classify subjects for β_i. Subjects with $\beta_i < 1$ give themselves a premium compared to very similar others, while those with $\beta_i > 1$ give others a premium. Most subjects, 72.5%, give themselves a premium and are social discounters with a concave utility function of money.

Table 4: Maximum simulated likelihood estimates, extension.

	$u(x)$ exponential, $D(s)$ quasi-hyperbolic
μ_α	1.37^{***}
	(0.10)
σ_α	1.42^{***}
	(0.10)
μ_β	0.68^{***}
	(0.02)
σ_β	0.40^{***}
	(0.02)
μ_δ	-0.24^{***}
	(0.02)
σ_δ	0.32^{***}
	(0.02)
σ	0.06^{***}
	(0.00)
Log Likelihood	-3058.02

Notes. α and β normally distributed, δ log-normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

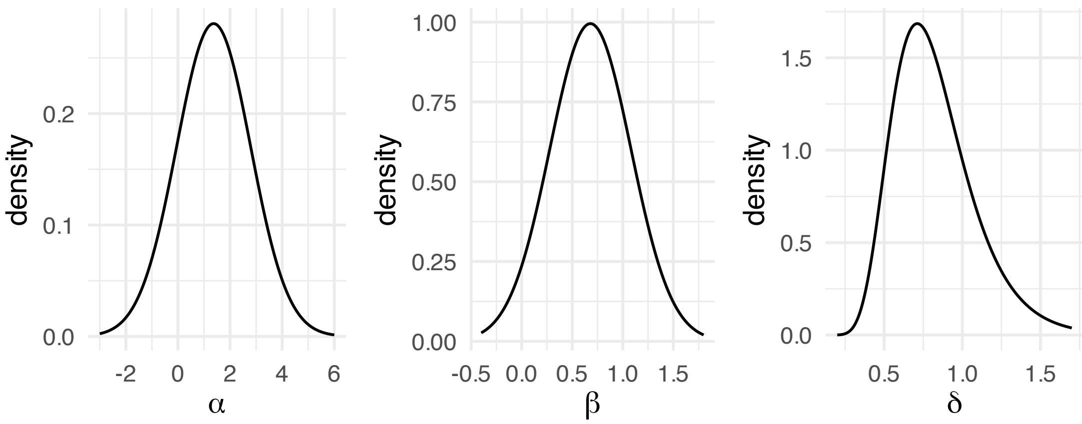


Figure 7: Plots of the distributions estimated in Table 4 of α (utility of money parameter), β (premium to self parameter), and δ (social discount factor), extension.

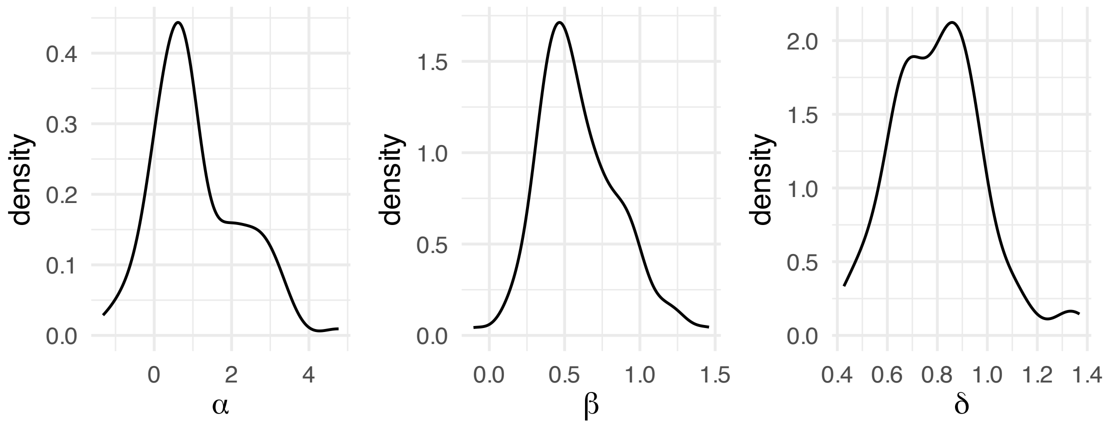


Figure 8: Kernel density plots of the posterior estimates of α (utility of money parameter), β (premium to self parameter), and δ (social discount factor), extension.

Table 5: Classification of subjects based on their recovered posterior estimates, extension.

$D(s)$ type		$u(x)$ type	n	%
Self premium	Social discounters	Concave	79	72.5%
		Convex	12	11%
	Social magnifiers	Concave	10	9.2%
Other premium	Social discounters	Concave	6	5.5%
		Convex	1	0.9%
	Social magnifiers	Convex	1	0.9%

Notes. Self premium: $\beta_i < 1$; other premium $\beta_i > 1$.
Social discounters: $\delta_i < 1$; social magnifiers: $\delta_i > 1$.
Concave: $\alpha_i > 0$; convex: $\alpha_i < 0$.

5 Exploratory analyses

Given that all subjects completed the 40 question survey presented in Subsection 3.1, we have extensive demographic information about them. We will now use this data for a number of exploratory analyses.

We first pool the data from the original experiment and the extension. We include subjects we were unable to include in the extension reported in Subsection 4.3 because they only completed the first part of the experiment and for whom we have 27 observations instead of the full 81. In Appendix D we show that we replicate our results using this pooled data.

5.1 Which dimensions of similarity are the most important?

To start with, we look at which dimensions of similarity best explain the choices subjects made in the experiment. To do so, instead of using the index of similarity in the estimations, we now use the difference on each of the variables that enter the computation of the index: sex, date of birth, zip code, race, ethnicity, religion, and the place where one grew up. Due to the large number of independent variables, we switch to a simpler, panel-data mixed logit model.[16] In Appendix E, we show that this model gives the same qualitative results as the structural model used so far when using only amount of money and index of similarity.

We assume that only the coefficient placed on the amount of money is normally distributed, and that the coefficients placed on the differences for each variable do not vary between subjects. The reason for this assumption is that we do not always have variation across matches on some of these differences for some subjects. For example, it is possible that all the passive matches were male for a given Step 2 subject. For this subject, we would not be able to identify the effect of the difference on sex.

The first column of Table 6 reports the estimation results. We find that most of the coefficients placed on difference variables are negative, so any difference on these variables would make subjects less likely to choose a particular match. Among these variables, the differences on age, religion, and the place where one

[16]We use the Stata (StataCorp, 2023) command `cmxtmixlogit` with Halton sequences of length 1000 per subject.

grew up are the ones that best explain choice.

However, the coefficients placed on ethnicity and on race are positive. Taken at face value, this result means that not being of the same race or not belonging to the same ethnicity actually increases the probability of choosing the corresponding match. To better understand this result, we re-run the regression but separately for white and non-white subjects.

The second and third columns of Table 6 report the results. We find that most coefficients are similar between white and non-white subjects. However, there are two main differences. While white subjects disfavour others located at a farther geographic distance, non-white subjects reward geographic distance. Further, while white subjects are more likely to select options that benefit matches who are not white, non-white subjects do the opposite and thus disfavour matches who are of a different race. These results suggest that some components of similarity do not necessarily influence the preferences of everyone in the same way.

5.2 Who are social magnifiers?

Next, we investigate the demographic characteristics of social magnifiers. Remember that social magnifiers are subjects who behave opposite to social discounters: they prefer rewards for less similar others.

After estimating the structural model on the pooled data, we recover posterior estimates in the same way as before. Then, we define an indicator variable for social magnifiers, equal to 1 if $\delta_i > 1$. Finally, we run a logistic regression with this indicator variable as the dependent variable and the demographic characteristics as independent variables.[17]

The results are reported in Table 7. We find that social magnifiers are more likely to be male and less likely to be Roman Catholic or Republican. Moreover, the likelihood of being a social magnifier decreases as household income increases.

[17]In Appendix F, we present an alternative regression—a linear regression, instead of a logistic regression, using the value of δ_i as opposed to the indicator variable—and find the same qualitative results.

Table 6: Decomposition of similarity. Panel-data mixed logit model, pooled data.

	(1) All	(2) Whites	(3) Non-Whites
Sex	-0.124^{***}	-0.140^{***}	-0.208^{**}
	(0.004)	(0.005)	(0.037)
Date of birth	-0.376^{***}	-0.284^{***}	-0.660^{***}
	(0.000)	(0.001)	(0.001)
Miles	-0.088	-0.207^{*}	1.135^{***}
	(0.353)	(0.051)	(0.000)
Ethnicity	0.274^{***}	0.063	0.500^{***}
	(0.000)	(0.314)	(0.000)
Ethnicity=0 × Ethnicity choice=1	0.698^{**}	0.782	0.615
	(0.040)	(0.433)	(0.101)
Race	0.191^{***}	0.554^{***}	-0.994^{***}
	(0.000)	(0.000)	(0.000)
Religious	-0.651^{***}	-0.699^{***}	-0.383^{***}
	(0.000)	(0.000)	(0.000)
Religious=0 × Religion denomination=1	-0.373^{***}	-0.247^{***}	-0.628^{***}
	(0.000)	(0.002)	(0.000)
Where grown up	-0.340^{***}	-0.405^{***}	-0.955^{***}
	(0.000)	(0.000)	(0.000)
Where grown up=0 × US state grown up=1	-0.468^{***}	-0.414^{***}	-0.843^{***}
	(0.000)	(0.000)	(0.000)
Where grown up=0 × Country grown up=1	-0.256	36.169	-0.907
	(0.669)	(0.995)	(0.168)
Money amount	1.408^{***}	1.476^{***}	1.299^{***}
	(0.000)	(0.000)	(0.000)
Money amount standard deviation	1.438^{***}	1.481^{***}	1.434^{***}
	(0.000)	(0.000)	(0.000)
Log Likelihood	-6794.257	-5336.549	-1316.108
Number of subjects	523	418	105
Number of choices	15089	12000	3089
Number of observations	30178	24000	6178

Notes. Coefficient placed on amount of money assumed to be normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Who are social magnifiers? Logit model, pooled data.

	Social magnifier = 1	
	Coef.	SE
Age	0.010	(0.011)
Gender (ref.: Female)		
Male	0.660^{**}	(0.247)
Race (ref.: White)		
Asian Indian	−0.271	(0.906)
Black	−0.857	(0.443)
Chinese	0.576	(0.690)
Korean	0.167	(0.889)
Other	−1.197	(0.773)
Ethnicity (ref.: None)		
Mexican	−0.759	(0.657)
Other	−0.515	(0.835)
Religion (ref.: None)		
Jewish	0.111	(1.003)
Other	0.407	(0.567)
Protestant	0.305	(0.329)
Roman Catholic	-0.836^{*}	(0.347)
Political party (ref.: Democrat)		
Independent	−0.435	(0.281)
No preference	−0.492	(0.746)
Other party	−1.018	(0.886)
Republican	-1.026^{**}	(0.327)
Marital status (ref.: Single)		
Divorced	−0.434	(0.579)
Married	0.490	(0.301)
Separated or widowed	0.258	(0.866)
Social class (ref.: Middle class)		
Lower class	0.546	(0.449)
Upper class	−0.206	(0.887)
Working class	−0.240	(0.269)
Work last week (ref.: Full time work)		
Housework	0.193	(0.377)
Part time work	−0.134	(0.378)
School	0.383	(0.666)

Place growing up (ref.: Small town)		
Farm	−0.635	(0.730)
Large city	0.134	(0.340)
Medium city	−0.245	(0.318)
Open country	0.120	(0.596)
Suburb	0.135	(0.314)
Highest degree (ref.: College or some college)		
12th grade no degree and less	0.723	(0.870)
Beyond bachelors	0.209	(0.333)
High school graduate	−0.138	(0.333)
Number of children	−0.056	(0.109)
Household income	−1.212*	(0.608)
Constant	−0.723	(0.678)
Observations	531	
Log-likelihood	−260.489	
Pseudo R^2	0.097	
Wald χ^2	56.215	
Prob > χ^2	0.017	

Notes. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Social discounting in the field: a re-analysis of Robson (2011)

In the previous Sections, we show evidence in favour of social discounting. However, one might be concerned that providing demographic characteristics to subjects using the card display presented in Figure 2 is susceptible to experimenter demand effects. To address this concern, we re-analyse the data of Robson (2021)—a lab-in-the-field experiment conducted in Uganda that does not share this feature—and show that our results replicate.

6.1 Design of Robson (2011)

In Robson (2021), subjects participate in 27 rounds of a three-person dictator game. In each round, they divide a budget of 60,000 Ugandan schillings between themselves and two other subjects. Between each round, the price of giving changes in a manner similar to Andreoni and Miller (2002) and Fisman et al. (2007). Crucially, in one of the two treatments considered by Robson (2021)—the 'Known' treatment—subjects could see each other. As a result, those making the dictator decisions could visually identify the recipients while making their choices, but they were not provided written demographic information in contrast to our experiments. This treatment thus allows us to test whether social discounting holds even when people can only visually observe demographic characteristics and infer similarity themselves.

The demographic characteristics measured by Robson (2021) are: gender, age, education, religion, and tribe. We use these five variables to compute an index of similarity like we did in our experiments. To be able to compare estimates with those we obtained from our experiments, we convert the payoffs into US dollars using the exchange rate at the time of the experiment (Robson, 2021, footnote 8). As a result of this conversion, the total amount to divide becomes \$18. If subjects split the money equally, each recipient would receive \$6, which puts the payoffs in the same range as in our experiments.

To get a sense for the data, we estimate a simple random effects model with the amount of money allocated as the dependent variable and the index of similarity and the target of the allocation as the independent variables. In a second model,

Table 8: Effect of similarity on allocations in Robson (2021), random effects model.

	(1)	(2)
Intercept	11.81^{***}	12.02^{***}
	(0.11)	(0.28)
Index of similarity	-0.36^{*}	-0.47^{*}
	(0.18)	(0.20)
Multiplier	-3.35^{***}	-3.35^{***}
	(0.10)	(0.10)
Round	-0.01^{***}	-0.01^{***}
	(0.00)	(0.00)
Target (ref.: P1, self)		
P2	-4.74^{***}	-4.69^{***}
	(0.11)	(0.12)
P3	-4.83^{***}	-4.77^{***}
	(0.11)	(0.12)
Controls	✗	✓
R^2	0.39	0.39
Adjusted R^2	0.39	0.39
Number of subjects	147	147
Number of observations	11497	11497

Notes. Standard errors in parentheses.
Controls as in Robson (2021): session time (AM/PM), enumerator foreign status, number of questions answered correctly, student status, gender, highest education level, household size, Christian religion dummy, religious participation, tribe dummy, wealth index, and extended multidimensional poverty index.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

we also add the same controls as Robson (2021). We report the results in Table 8. In both models the coefficient on the index of similarity is negative and statistically significant at the 5% level: as similarity decreases, subjects give less money to the recipient. We will see whether the model of social discounting we have used so far is able to capture this pattern.

6.2 Econometric specification

We assume the same functional forms as in our extension (Subsection 4.3) meaning that the utility function of money $u(x)$ has the exponential form (1) and that the social discount function $D(s)$ has the quasi-hyperbolic form (2).

Denote by x_{ij}^* the optimal allocation of subject i to recipient j that we obtain by solving the maximisation problem of a subject allocating a budget $m = \sum_{j=1}^{3} x_{ij}$ given prices $\{p_j\}_{j=1}^{3}$. We have

$$x_{ij}^*(\alpha_i, \beta_i, \delta_i) = \frac{m + \sum\limits_{k \neq j} \mathbb{1}_{\{x_{ik}>0\}} \frac{1}{\alpha_i p_k} \ln \left(\frac{D(s_{ij}; \beta_i, \delta_i)}{D(s_{ik}; \beta_i, \delta_i)} \frac{p_j}{p_k} \right)}{1 + \sum\limits_{k \neq j} \mathbb{1}_{\{x_{ik}>0\}} \frac{p_j}{p_k}} \tag{3}$$

where $\mathbb{1}_{x_{ik}>0}$ is an indicator function that is equal to 1 if i gives money to k, and 0 otherwise. Recall that $s_{ii} = 0$ and $D(0) = 1$.[18]

To take the model to the data, we assume that the observed allocations are given by $x_{ij} = x_{ij}^* + \epsilon_{ij}$, where $\epsilon \sim \mathcal{N}(0, \sigma^2)$. In other words, subjects make mean-zero, normally distributed random errors when allocating the budget. Given this assumption, the likelihood contribution of subject i in round t, conditional on the parameters, is

$$\prod_{j=1}^{3} \frac{1}{\sigma} \phi \left(\frac{x_{ij} - x_{ij}^*(\alpha_i, \beta_i, \delta_i)}{\sigma} \right),$$

where ϕ is the standard normal density function.

The fact that β_i enters the logarithms in (3) puts the restriction that $\beta_i > 0$, so this time we assume that β is also log-normally distributed, $\ln \beta \sim \mathcal{N}(\mu_\beta, \sigma_\beta^2)$. All other aspects of the estimation remain the same and, once again, we rely on the method of maximum simulated likelihood to estimate the parameters.

6.3 Estimation results

Table 9 reports the estimation results. As before, we also plot the estimated distributions in Figure 9. We obtain $\mathbb{E}(\delta) = 1.19$, which is statistically different

[18] A similar solution resulting from an exponential utility function can be found in Robson et al. (2024).

Table 9: Maximum simulated likelihood estimates, data from Robson (2021).

	$u(x)$ exponential, $D(s)$ quasi-hyperbolic
μ_α	0.53^{***}
	(0.01)
σ_α	0.57^{***}
	(0.01)
μ_β	-1.01^{***}
	(0.03)
σ_β	0.97^{***}
	(0.02)
μ_δ	-0.08
	(0.04)
σ_δ	0.73^{***}
	(0.04)
σ	1.94^{***}
	(0.01)
Log Likelihood	-22455.45

Notes. α normally distributed, β and δ log-normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

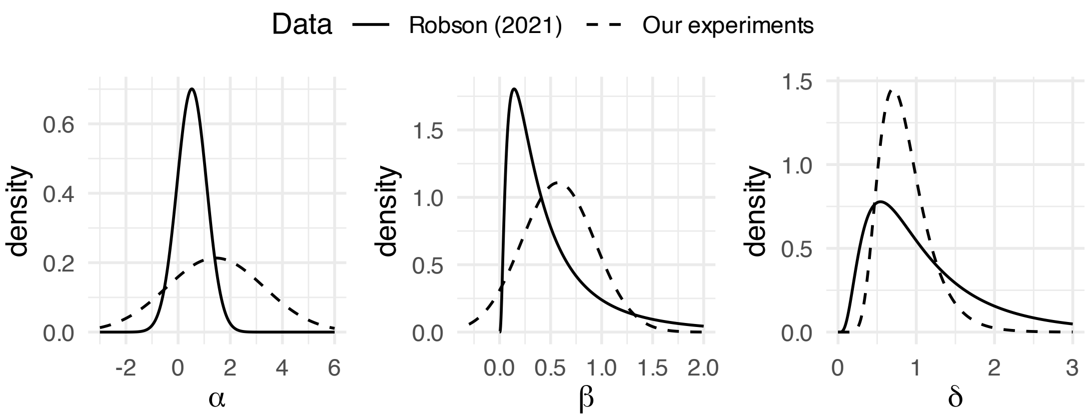


Figure 9: Comparisons of the estimated distributions of α (utility of money parameter), β (premium to self parameter), and δ (social discount factor), between Robson (2021) (Table 9) and our experiments (Appendix D).

from 1 (Wald's $\chi^2 = 9.48$, $p < 0.05$). This result would indicate that subjects are predominantly social magnifiers. However, as we can see in Figure 9, the estimated distributions for δ and β are rightly skewed and so the means do not indicate where most of the distribution is. Indeed, we get $\mathbb{M}\text{edian}(\delta) = 0.92$ and $\mathbb{M}\text{ode}(\delta) = 0.54$.[19] Both are statistically significantly different from 1 (Wald's χ^2, respectively 3.97 and 135.06, all $p < 0.05$). For the premium to self β, we find $\mathbb{E}(\beta) = 0.58$, $\mathbb{M}\text{edian}(\beta) = 0.36$ and $\mathbb{M}\text{ode}(\beta) = 0.14$, all also statistically different from 1 (Wald's χ^2, respectively, 851.33, 4646.6 and 14251, all $p < 0.001$). Therefore, all the results from our experiments hold, which gives our last result:

Result 5. We replicate our results using the data from the lab-in-the-field experiment of Robson (2021).

As we did for our experiments, we recover the posterior estimates of the parameters for each subject conditional on their allocations. We report the kernel density plots in Figure 10 and classify subjects in Table 10. We observe more social magnifiers compared to our experiments and no subjects with a convex utility of money.

In Figures 9 and 10 we also include the results obtained in our experiments (shown using dashed lines) in order to more easily compare them with those from Robson (2021). We use the pooled data from the original experiment and from the extension as we did in Section 5. We see that the distribution of β in Robson (2021) is more skewed towards 0 which indicates that subjects more markedly made a difference between money for themselves and money for others. We also see that the distribution of δ is more spread out which may reflect that subjects were sampled from a more diverse population. Further, when comparing the estimated distributions in Figure 9—which are essentially priors—to the distributions of the posteriors in Figure 10, we see that the distribution of δ in Robson (2021) shifts more to the right until it is almost centered on 1. This shift may reflect the fact that some subjects require large, or perhaps even implausible, values of δ to explain their choices. Indeed, we can recover estimates for only 106 out of 147 subjects when we perform the estimation subject-by-subject. Among these subjects, δ ranges from 0 to 1304, with a mean of 15.1 and a standard deviation of 127. The whole

[19]Using $\mathbb{M}\text{edian}(\delta) = \exp(\mu_\delta)$ and $\mathbb{M}\text{ode}(\delta) = \exp(\mu_\delta - \sigma_\delta^2)$.

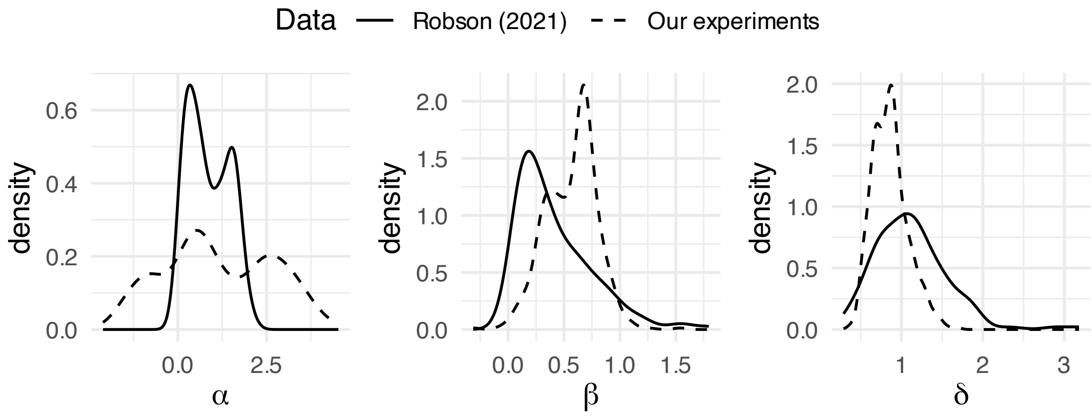


Figure 10: Comparison of kernel density plots of the posterior estimates of α (utility of money parameter), β (premium to self parameter), and δ (social discount factor), between Robson (2021) and our experiments.

Table 10: Classification of subjects based on their recovered posterior estimates, data from Robson (2021).

$D(s)$ type		$u(x)$ type	n	%
Self premium	Social discounters	Concave	60	40.1%
	Social magnifiers	Concave	78	53.1%
Other premium	Social discounters	Concave	2	1.4%
	Social magnifiers	Concave	7	4.8%

Notes. Self premium: $\beta_i < 1$; other premium $\beta_i > 1$.
Social discounters: $\delta_i < 1$; social magnifiers: $\delta_i > 1$.
Concave: $\alpha_i > 0$; convex: $\alpha_i < 0$.

distribution shifts to the right in order to capture subjects with such extreme values for δ.

7 Discussion and conclusion

In this paper, we have shown how to measure social distance and study social discounting. We capture social distance through interpersonal similarity; that is, how similar or different people are one to another. We estimate the structural parameters of social discounting in our experiments and in the lab-in-the-field experiment of Robson (2021).

We chose to focus on similarity based on sex, date of birth, zip code, race, ethnicity, religion, and the place where one grew up. Subsequent studies may look at the effect of other characteristics on social discounting. For example, due to the timing and location of our main experiment—November 2020 in the US—we shied away from using variables that relate to political preferences. We expect that the inclusion of characteristics that relate to political preferences would allow the study of social discounting in relation to rising political polarisation. Further, variables that make it easier to relate to others, such as one's favourite colour, liking dogs, and enjoying movies (Eckel and Wilson, 2004), or watching reality TV (Liviatan et al., 2008) might lead to even steeper social discounting.

Our exploratory analyses have generated a number of results that warrant further study. As an example, we found that only non-white subjects discount rewards based on race. A possible explanation is that, on average, minorities have a lower income in the US. We would observe exactly this result if subjects in our experiment took non-white status as a proxy for income and favoured redistribution in favour of equality. To test this explanation, we could gather more data on non-white subjects or, for example, reveal the income of the matches.

Overall, we find strong evidence for social discounting. Whenever social distance exists between people, social discounting becomes relevant. As such, social discounting is relevant for settings beyond the ones we have studied here. The methods we have developed allow others to incorporate social discounting into new settings and formulate and test new predictions.

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Appendices

Appendix A Survey questions

We ask the following questions in the survey. We include each question as written followed immediately by the answer type in italics and where appropriate the options from which subjects selected. This list already appeared in the Appendix of Beranek and Castillo (2024).

1. What is your sex? *Multiple choice*
    - Male
    - Female
2. What is your date of birth? *Calendar date entry*
    - Date
3. What is your marital status? *Multiple choice*
    - Now married
    - Widowed
    - Divorced
    - Separated
    - Never married
4. Have you had any children? *Multiple choice*
    - Yes (*Selecting this answer led to parts a and b below*)
    - No
    a) How many children have you had? Please count all that were born alive at any time (including any you had from a previous relationship). *Numerical entry*
        - Number
    b) How old were you when your first child was born? *Numerical entry*
        - Number
5. What language do you normally speak at home? *Multiple choice*
    - English
    - Spanish
    - Chinese (including Mandarin and Cantonese)
    - Tagalog (including Filipino)
    - Vietnamese
    - Arabic
    - French
    - Korean
    - Russian
    - German
    - Other

    - *Please enter the language:*
6. What is the ZIP code in which you reside? Please only enter the first 5 numbers. *Numerical entry restricted to existing zip codes*
    - Number
7. Are you of Hispanic, Latino, or Spanish origin? *Multiple choice*
    - Yes (*Selecting this answer led to part a below*)
    - No
    a) If yes, which one? *Multiple choice*
        - Mexican, Mexican American, Chicano
        - Puerto Rican
        - Cuban
        - Another Hispanic, Latino, or Spanish origin
            - *Please enter your origin (for example, Argentinean, Colombian, Dominican, Nicaraguan, Salvadoran, Spaniard, and so on):*
8. What race do you consider yourself? *Multiple choice*
    - White
    - Black or African American
    - American Indian or Alaska Native
    - Asian Indian
    - Chinese
    - Filipino
    - Other Asian (including Cambodian, Laotian etc.)
        - *Please enter your race (for example, Hmong, Laotian, Thai, Pakistani, Cambodian, and so on):*
    - Japanese
    - Korean
    - Vietnamese
    - Native Hawaiian
    - Guamanian or Chamorro
    - Samoan
    - Other Pacific Islander:
        - *Please enter your race (for example, Fijian, Tongan, and so on):*
    - Other (including Two or more races such as Biracial, Multiracial, etc.)
        - *Please enter your race:*
9. Do you belong to a religious denomination? *Multiple choice*
    - Yes (*Selecting this answer led to part a below*)
    - No
    a) Which religious denomination do you belong to? *Multiple choice*
        - Roman Catholic
        - Protestant
        - Mormon

    - Orthodox (Russian/Greek/etc.)
    - Jew
    - Muslim
    - Hindu
    - Buddhist
    - Other denomination
        - *Please enter your religious denomination:*
10. Where did you grow up? *Multiple choice*
    - In the the United States (*Selecting this answer led to part a below*)
    - In another country (*Selecting this answer led to part b below*)
    a) Please select the State or Territory in which you grew up: *Dropdown list of states and territories*
        - State or Territory
    b) Please select the country in which you grew up: *Dropdown list of countries*
        - Country
11. Are you a citizen of the United States? *Multiple choice*
    - Yes (*Selecting this answer led to part a below*)
    - No
    a) Were you: *Multiple choice*
        - Born in the United States
        - Born in American Samoa, Guam, the Northern Mariana Islands, Puerto Rico, or the Virgin Islands
        - Born abroad of United States citizen parent or parents
        - United States citizen by naturalization
12. Which best describes the building where you live? *Multiple choice*
    - A mobile home
    - A one-family house detached from any other house
    - A one-family house attached to one or more houses
    - A building with less than 5 apartments
    - A building with 5 or more apartments
    - A dormitory or hall of residence
    - Boat, RV, van etc.
    - Other
        - *Please enter the type of building you live in:*
13. Do you happen to have in your home any guns or revolvers? *Multiple choice*
    - Yes
    - No
14. Which of the categories comes closest to the type of place you were living in when you were 16 years old? *Multiple choice*
    - In open country but not on a farm
    - On a farm

- In a small city or town (under 50,000)
- In a medium-size city (50,000–250,000)
- In a suburb near a large city
- In a large city (over 250,000)

15. What is the highest degree or level of school you have COMPLETED? If currently enrolled, mark the previous grade or highest degree received. *Multiple choice*
    - No schooling completed
    - Nursery or preschool through grade 12
      - Nursery school
      - Kindergarten
      - Grade 1 through 11
        - *Specify the highest grade completed:*
    - 12th grade – no diploma
    - High school graduate
      - Regular high school diploma
      - GED or alternative credential
    - College or some college (*Selecting this answer led to part a below*)
      - Some college credit, but less than 1 year of college credit
      - 1 or more years of college credit, no degree
      - Associate's degree (for example: AA, AS)
      - Bachelor's degree (for example: BA, BS)
    - Postgraduate education (*Selecting this answer led to part b below*)
      - Master's degree (for example: MA, MS, MEng,MEd, MSW, MBA)
      - Professional degree beyond a bachelor's degree (for example: MD, DDS, DVM, LLB, JD)
      - Doctorate Degree (for example: PhD, EdD)

    a) What has been your main area of study? (For example a major like chemical engineering, elementary education, nursing, or organizational psychology): *Free text response*
       - Main area of study

    b) What has been your main area of study? (For example a major like chemical engineering, elementary education, nursing, or organizational psychology): *Free text response*
       - Main area of study

16. What were you doing for the majority of last week? *Multiple choice*
    - Working full time (*Selecting this answer led to parts a, b, and c below*)
    - Working part time (*Selecting this answer led to parts a, b, and c below*)
    - Studying
    - Keeping house

    a) What do you do for work? Please write your profession: *Free text response*

        - Profession
    b) What category best describes your employer? *Multiple choice*
        - Government or public institution
        - Private business or industry
        - Private non-profit organization
        - I am self employed
    c) Are the tasks you do at work mostly manual or mostly intellectual? Please select the number where 1 means "mostly manual tasks" and 10 means "mostly intellectual tasks": *Multiple choice*
        - Ranging from 1 to 10
17. At any time during the last ten years, have you been unemployed and looking for work for as long as a month? *Multiple choice*
    - Yes
    - No
18. Do you belong to a labor union? *Multiple choice*
    - Yes
    - No
19. To the best of your knowledge, in which of these groups did your total household income from all sources (before taxes) fall last year? (If you are a student who is supported financially by your family, please include their income in your calculation of household income.): *Multiple choice*
    - Under $1,000
    - $1,000 to $2,999
    - $3,000 to $3,999
    - $4,000 to $4,999
    - $5,000 to $5,999
    - $6,000 to $6,999
    - $7,000 to $7,999
    - $8,000 to $9,999
    - $10,000 to $12,499
    - $12,500 to $14,999
    - $15,000 to $17,499
    - $17,500 to $19,999
    - $20,000 to $22,499
    - $22,500 to $24,999
    - $25,000 to $29,999
    - $30,000 to $34,999
    - $35,000 to $39,999
    - $40,000 to $49,999
    - $50,000 to $59,999
    - $60,000 to $74,999

- $75,000 to $89,999
- $90,000 to $109,999
- $110,000 to $129,999
- $130,000 to $149,999
- $150,000 or over

20. Imagine an income scale from 1 to 10 where 1 indicates the lowest income group in America and 10 indicates the highest income group. Counting all wages, salaries, pensions and other incomes that come in, please specify what income group your household is in: *Multiple choice*
    - Ranging from 1 to 10
21. Thinking about the time when you were 16 years old, compared with families in general then, where would you say your family income was? *Multiple choice*
    - Far below average
    - Below average
    - Average
    - Above average
    - Far above average
22. Compared to your parents when they were the age you are now, do you think your own standard of living now is: *Multiple choice*
    - Much better
    - Somewhat better
    - About the same
    - Somewhat worse
    - Much worse
23. If you were asked to use one of four names for your social class, which would you say you belong in? *Multiple choice*
    - the Lower Class
    - the Working Class
    - the Middle Class
    - the Upper Class
24. How satisfied are you with the present financial situation of you and your family? *Multiple choice*
    - Pretty well satisfied with my present financial situation
    - More or less satisfied with my present financial situation
    - Not satisfied at all with my present financial situation
25. How important is it for you to live in a country that is governed democratically? Please indicate the importance on a scale where 1 means it is "not at all important" and 10 means "absolutely important". *Multiple choice*
    - Ranging from 1 to 10
26. How proud are you to live in the United States? *Multiple choice*
    - Very proud

- Quite proud
- Not very proud
- Not at all proud

27. Generally speaking, do you usually think of yourself as a Democrat, a Republican, an Independent, or what? *Multiple choice*
    - Democrat
    - Republican
    - Independent
    - Other party
        - *What other political party do you identify with:*
    - No preference
28. We hear a lot of talk these days about liberals and conservatives. Here is a seven-point scale on which the political views that people might hold are arranged from extremely liberal to extremely conservative. Where would you place YOURSELF on this scale? *Multiple choice*
    - Extremely liberal
    - Liberal
    - Slightly liberal
    - Moderate; middle of the road
    - Slightly conservative
    - Conservative
    - Extremely conservative
29. The table below lists some institutions in this country. As far as the people running these institutions are concerned, would you say you currently have a great deal of confidence, only some confidence, or hardly any confidence at all in them?
    a) Executive Branch of the Federal Government *Multiple choice*
        - Hardly any confidence at all
        - Only some confidence
        - A great deal of confidence
    b) Congress *Multiple choice*
        - Hardly any confidence at all
        - Only some confidence
        - A great deal of confidence
    c) The Supreme Court *Multiple choice*
        - Hardly any confidence at all
        - Only some confidence
        - A great deal of confidence
    d) The Military *Multiple choice*
        - Hardly any confidence at all
        - Only some confidence

    - A great deal of confidence
   e) The Police *Multiple choice*
    - Hardly any confidence at all
    - Only some confidence
    - A great deal of confidence
   f) Banks and Financial Institutions *Multiple choice*
    - Hardly any confidence at all
    - Only some confidence
    - A great deal of confidence
   g) Organized Labor (or Unions) *Multiple choice*
    - Hardly any confidence at all
    - Only some confidence
    - A great deal of confidence
   h) Public Education *Multiple choice*
    - Hardly any confidence at all
    - Only some confidence
    - A great deal of confidence
   i) The Press *Multiple choice*
    - Hardly any confidence at all
    - Only some confidence
    - A great deal of confidence
30. We are faced with many problems in this country. For those listed in the table below, do you think that we are spending too much, too little, or about the right amount on them?
   a) Improving the conditions of African Americans *Multiple choice*
    - Spending too much
    - Spending the right amount
    - Spending too little
   b) Improving the conditions of those living in Foreign Countries *Multiple choice*
    - Spending too much
    - Spending the right amount
    - Spending too little
   c) Improving and protecting the Environment *Multiple choice*
    - Spending too much
    - Spending the right amount
    - Spending too little
31. On a seven-point scale, where 1 means very important and 7 means not important at all, how important do you think it is for the government in Washington to reduce the differences in income between the rich and the poor? *Multiple choice*

- Ranging from 1 to 7

32. Do you consider the amount of federal income tax we pay as too high, about right, or too low? *Multiple choice*
    - The federal income tax I pay is too high
    - The federal income tax I pay is about right
    - The federal income tax I pay is too low
33. Do you favor or oppose the death penalty for persons convicted of murder? *Multiple choice*
    - I favor the death penalty for persons convicted of murder
    - I oppose the death penalty for persons convicted of murder
34. Are you for preferential hiring and promotion of African Americans or are you against it? Common considerations when evaluating this policy include the past discrimination of African Americans as well as the discriminatory impact of this policy on others. *Multiple choice*
    - Strongly opposed to giving preference to African Americans in hiring and promotion
    - Somewhat opposed to giving preference to African Americans in hiring and promotion
    - Somewhat in favor of giving preference to African Americans in hiring and promotion
    - Strongly in favor of giving preference to African Americans in hiring and promotion
35. In your opinion, if two consensual adults have sexual relations before marriage, do you think it is: *Multiple choice*
    - Always wrong
    - Almost always wrong
    - Wrong only sometimes
    - Not wrong at all
36. Similarly, if two consensual adults of the same sex have sexual relations, do you think it is: *Multiple choice*
    - Always wrong
    - Almost always wrong
    - Wrong only sometimes
    - Not wrong at all
37. Do you think it should be possible for a pregnant woman to obtain a legal abortion if the woman wants one for any reason? *Multiple choice*
    - Yes, it should be possible
    - No, it should not be possible
38. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves? *Multiple choice*
    - Most of the time people try to be helpful

- People are mostly just looking out for themselves

39. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? *Multiple choice*
    - Most people would try to take advantage of you if they got a chance
    - Most people would try to be fair
40. Generally speaking, would you say that most people can be trusted or that you can not be too careful in dealing with people? *Multiple choice*
    - Most people can be trusted
    - You cannot be too careful in dealing with people

Appendix B Instructions

This Appendix reports the instructions that subjects saw in the experiment.

When they joined the experiment, subjects started by completing a forty question survey. Appendix A contains a full list of the questions from the survey.

Then, they went through the following tasks. The section titles were not shown to subjects.

B.1 Matches display

Previously, we invited several hundred participants to complete the same survey as you. Out of that group, we selected 3 participants and display some of their responses here.

In order to more easily refer to them we have randomly associated each with one of the four suits from a deck of playing cards: Spade (♣), Club (♠), Heart (♥), and Diamond (♦).

We will represent these participants and some of their answers to the survey with the following cards:

[Figure B1]

For the rest of the study, you will complete tasks involving these 3 participants and they will remain the same throughout.

In the next pages we will ask you to consider these participants one by one and write a few words about them.

B.2 Continuous Inclusion of Other in the Self scale

We will now ask you to consider how connected you feel towards the participants.

More specifically, we will show you the following circles:

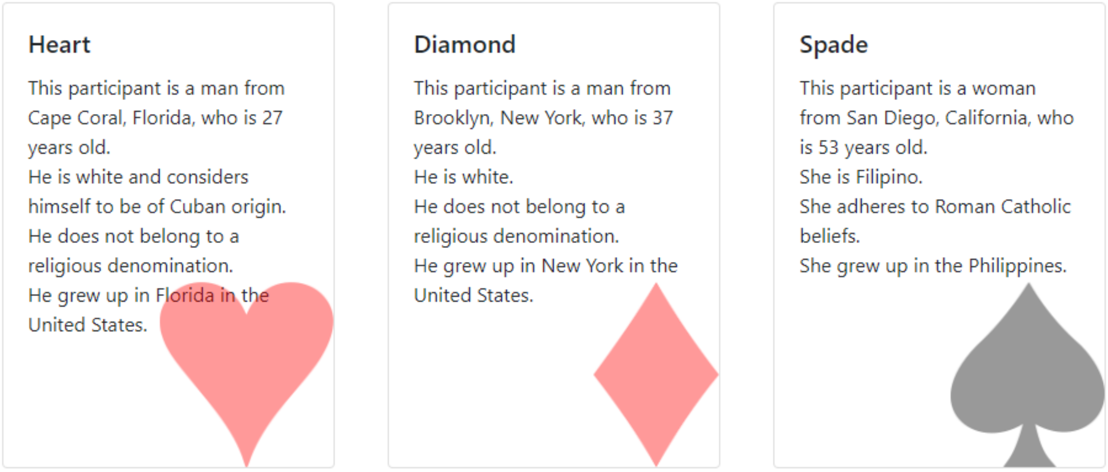

Figure B1: Cards presented in the display of the matches.

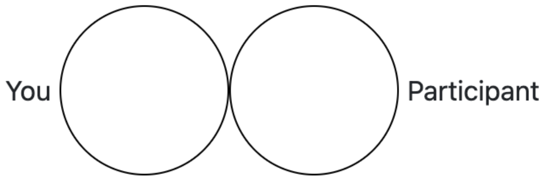

Figure B2: Continuous IOS scale.

[Figure B2]

You can manipulate the circles by clicking on the left circle, then drag-and-dropping it.

The overlap of the circles represents the connection you feel towards the other participant. For example, a stronger connection between you and the other participant would be represented by a greater overlap of the circles.

We will ask you to shift the circles until you find a configuration that best represents the connection you feel toward each of the 3 participants whose survey responses you just read.

You can practice on this page as much as you want.

To go to the next page, please change the circles so that they're overlapping as much as possible. Once you've done so the 'Next' button will appear.

B.3 Choice task

B.3.1 Instructions

According to your decisions in this task, we will send money to one of the participants whose survey answers you just read. You are literally picking who receives money from us.

Recall that these participants were selected from several hundred MTurk Workers who previously completed the same survey as you.

The amount of money the MTurker receives will be $1.00, $2.00, or $5.00, and they will receive it in the form of an MTurk bonus payment. This MTurk bonus payment will come from us without explanation. This money is completely independent of the $3.00 reward you will receive for completing this HIT.

It is therefore important that you read these instructions carefully, because your decisions will determine the bonus payment of one of these MTurk workers.

Outline of this task You will be asked 27 questions of the same type. In each question you will be presented with two options and you will be asked to pick the one you prefer.

After you have answered all 27 questions, one of them will be selected at random and the option that you picked will be implemented: if, for example, you picked an option that directed us to give $1.00 to a particular MTurk worker, we will pay this MTurk worker $1.00.

Therefore, you should answer each question as if it will be the question that is selected at the end of the experiment to determine another MTurk worker's payment.

The questions A sample question is shown below:

[Figure B3]

You can see that there are two options—one on the left and one on the right. The option on the left would lead us to give $2.00 to Club. The option on the right would lead us to give $1.00 to Diamond.

You have to decide for each question whether you prefer the option on the left or the option on the right. You indicate your choice by clicking on the button below your preferred option. If you feel indifferent between the two options you will also be able to click the button labelled 'Don't care'.

The buttons will appear after 4 seconds and you can spend as much time as you want on each choice.

Since your decisions will affect another MTurk worker you should be as careful as possible when answering the questions.

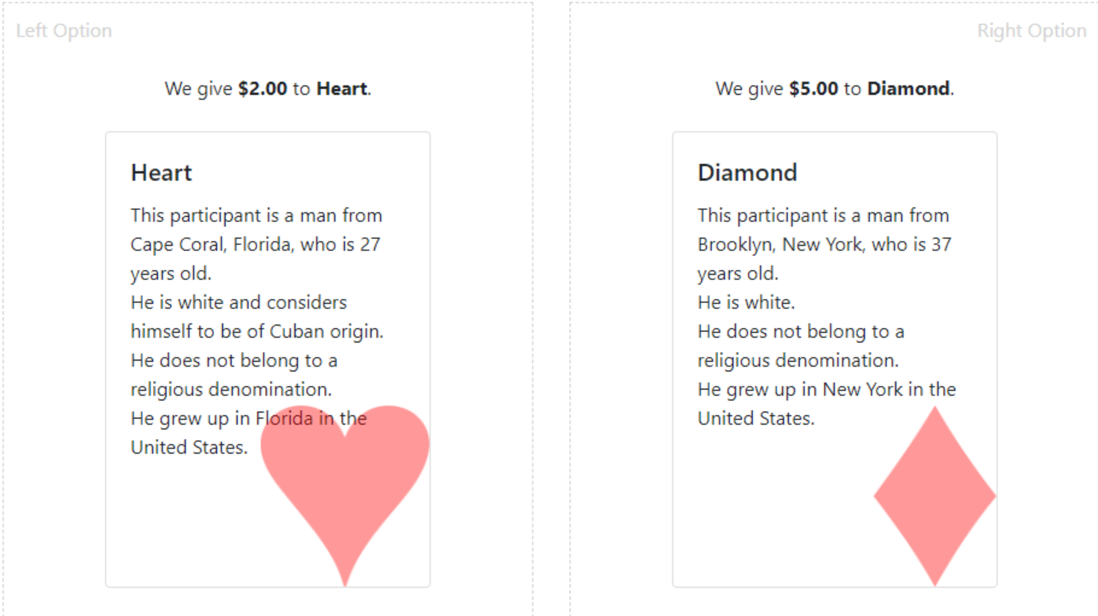

Figure B3: Choice task example in the instructions.

The end of the task After you have answered all 27 questions we will select one at random and will implement what you chose.

For example, if the question above was selected at the end of the experiment and if you had chosen the Left Option, then we would send a bonus payment of $2.00 to Club. If, on the other hand, you had chosen the Right Option, then we would send a bonus payment of $1.00 to Diamond. Finally, if you had chosen 'Don't care', we will flip a coin to randomly select one MTurk worker to receive all of their allocation.

Therefore, as you go through the questions, remember that each question could be used to determine a real cash payment to another MTurk worker like yourself.

We will maintain your anonymity throughout and the MTurk bonus payment will come from us without explanation.

Transparency We are committed to transparency in making these payments and so we will post records of these transactions on our website: `https://geoffreycastillo.com/mturk`.

Three days after the experiment, you will find two documents:

- A Record of the Relevant Decisions from the Experiment that includes
  - the decision situation that was randomly selected
  - the option you chose in that decision situation
  - the partially redacted MTurk worker ID of the participant who we sent the money to

    - your partially redacted MTurk worker ID so you can find this information easily
- A copy of the our Complete Transaction History showing payment to the relevant MTurk worker (again with their MTurk worker ID partially redacted)

A copy of the our Complete Transaction History showing payment to the relevant MTurk worker (again with their MTurk worker ID partially redacted) After we give the money to this MTurk worker, we will send you a bonus of $0.01 with a message reminding you of this procedure and directing you to our website so that you can verify these transactions (remember that this bonus is in addition to your $3.00 reward for completion of the HIT). You can see an example of these records on our website: `https://geoffreycastillo.com/mturk`.

B.4 Control questions

To test your understanding we have devised the following control questions.

If you want to take another look at the instructions, click the following button:

[button to reopen instructions]

When I decide an allocation between two participants, those participants are: *[single choice]*

- Hypothetical people who do not exist in real life
- Real MTurk workers who completed the same survey as me, but they won't actually receive any payments
- Real MTurk workers who completed the same survey as me and they will receive payments according to my decisions

When participants receive money as a consequence of my decisions, what will they know? *[single choice]*

- They will know that I made the choice
- They will know that another MTurk worker made the choice, but they won't know it was me in particular
- They won't know anything, as the money will come directly from the researchers and no mention of the present study will be made

True or False: The payment to the participants will come from my own reward. *[single choice]*

- True: The researchers will deduct the payment to the chosen participant from my reward
- False: My reward of $3.00 is independent of the payment to the chosen participant

True or False: Even if I choose 'Don't care' only one participant will receive a payment: *[single choice]*

- True: Choosing 'Don't care' means that one participant is randomly selected to receive all of their allocation
- False: Choosing 'Don't care' means that both participants receive a portion of the allocation

How many of my decisions will be implemented? *[Integer input]*

True or False: I will not be able to verify that any payments have been made: *[single choice]*

- True: I just have to trust the MTurk Requesters will implement my decision
- False: I can verify that payments have been made by visiting `https://geoffreycastillo.com/mturk` three days after the experiment

Appendix C Order effects

In our experiment, half of the subjects faced the Continuous IOS scale task before the choice task, and the other half faced the choice task before the Continuous IOS scale task. In this Appendix we show that the order of the tasks did not affect the subjects' choices.

In Table C1 we report the results of panel-data mixed logit models in which we explain choice as a function of the options' attributes, amount of money and dissimilarity. In the first model we restrict to subjects who faced the choice task first, and in the second model we restrict to subjects who faced the Continuous IOS scale task first. In the third model, we combine both orders, identify subjects who did the IOS scale task first, and add interaction effects. As can be seen, we do not find evidence that the order of the tasks affected subjects' choices.

Appendix D Structural model and maximum simulated likelihood estimation on the pooled data

In the main text we estimated the structural model twice: once with the original dataset (Section 4.2.3) and again with the extension dataset (Section 4.3). Here we do the exact same estimation but with all the data pooled.

Table D1 report the estimation results and Figure D1 plots the estimated distributions. The estimates are very similar to those obtained in the extension

Table C1: Panel-data mixed logit model, choice task first or Continuous IOS scale task first.

	Choice first	IOS first	Combined
Means:			
Money amount	1.627^{***}	1.527^{***}	1.618^{***}
	(0.000)	(0.000)	(0.000)
Dissimilarity	-1.571^{***}	-1.605^{***}	-1.466^{***}
	(0.000)	(0.000)	(0.000)
IOS first=1 × Money amount			−0.001
			(0.995)
IOS first=1 × Dissimilarity			−0.114
			(0.789)
Standard deviations:			
Money amount	1.789^{***}	1.576^{***}	1.676^{***}
	(0.000)	(0.000)	(0.000)
Dissimilarity	3.494^{***}	3.433^{***}	3.412^{***}
	(0.000)	(0.000)	(0.000)
Log Likelihood	−1832.249	−1802.965	−3635.782
Number of subjects	175	174	349
Number of choices	4283	4169	8452
Number of observations	8566	8338	16904

Notes. All coefficients assumed to be normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table D1: Maximum simulated likelihood estimates, pooled data.

	$u(x)$ exponential, $D(s)$ quasi-hyperbolic
μ_α	1.43^{***}
	(0.03)
σ_α	1.87^{***}
	(0.04)
μ_β	0.57^{***}
	(0.01)
σ_β	0.36^{***}
	(0.00)
μ_δ	-0.22^{***}
	(0.01)
σ_δ	0.37^{***}
	(0.02)
σ	0.06^{***}
	(0.00)
Log Likelihood	-8259.96

Notes. α and β normally distributed, δ log-normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

(Table 4 in the main text). We have $\mathbb{E}(\delta) = 0.86$ and $\mathbb{V}\text{ar}(\delta) = 0.10$, and still reject $\delta = 0$ (Wald's $\chi^2 = 25536$, $p < 0.01$) and $\delta = 1$ (Wald's $\chi^2 = 675.08$, $p < 0.01$). We also reject $\beta = 1$ (Wald's $\chi^2 = 6092.0$, $p < 0.01$).

As in the main text, we also recover posterior estimates for each subject, conditional on their 27 choices (if they did the original study or only the first part of the extension) or their 81 choices (if they did both part of the extension). Figure D2 displays kernel density plots of such estimates. Finally, Table D2 shows the classification of subjects based on their recovered posterior estimates.

Appendix E Panel-data mixed logit model

In the main text we used a panel-data mixed logit model to explore which dimension of similarity mattered the most. Here we show that the same type of model gives the same qualitative results as the the structural model applied to the pooled data and reported in Appendix D.

Table E1 shows the estimation results. In model (1) we see that a larger amount of money or a smaller dissimilarity increases the probability to choose an option,

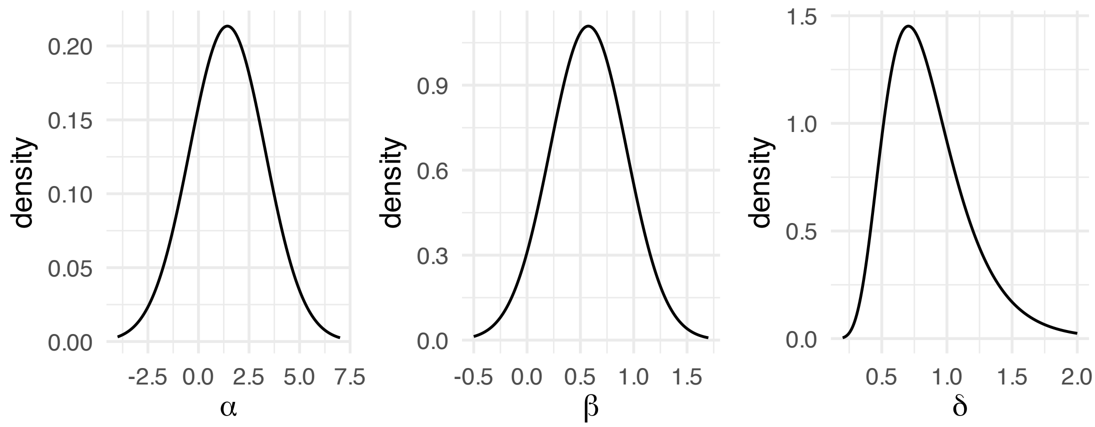

Figure D1: Plots of the distributions estimated in Table D1 of α (utility of money parameter), β (premium to self parameter), and δ (social discount factor), pooled data.

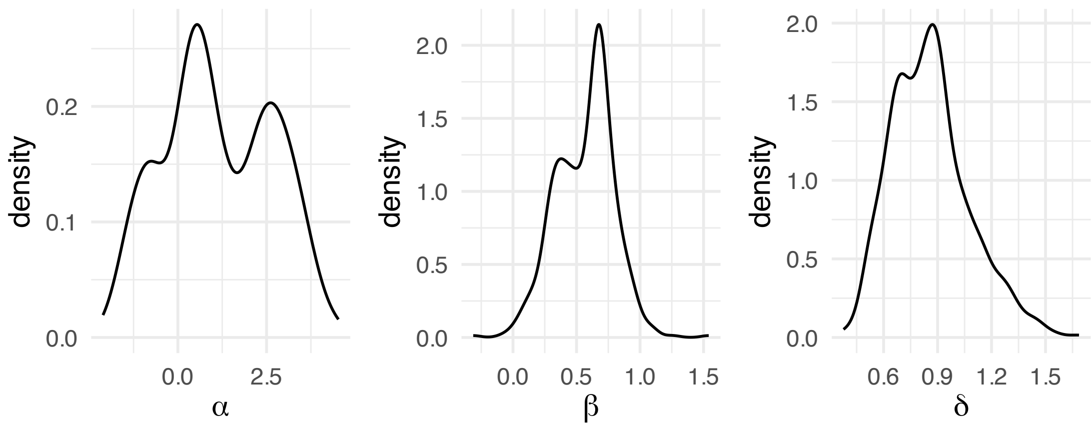

Figure D2: Kernel density plots of the posterior estimates of α (utility function of money parameter), β (premium to self parameter), and δ (social discount factor), pooled data.

Table D2: Classification of subjects based on their recovered posterior estimates, pooled data.

$D(s)$ type		$u(x)$ type	n	%
Self premium	Social discounters	Concave	310	58.2%
		Convex	96	18%
	Social magnifiers	Concave	94	17.6%
		Convex	25	4.7%
Other premium	Social discounters	Concave	3	0.6%
	Social magnifiers	Concave	4	0.7%
		Convex	1	0.2%

Notes. Self premium: $\beta_i < 1$; other premium $\beta_i > 1$.
Social discounters: $\delta_i < 1$; social magnifiers: $\delta_i > 1$.
Concave: $\alpha_i > 0$; convex: $\alpha_i < 0$.

in line with the results from the structural model.

In models (2) and (3) we look at whether the values obtained from the Continuous IOS scale perform better than the index of dissimilarity in explaining choice. The log-likelihoods show that indeed they do as they are all larger than in model (1). This result is perhaps not surprising: while the index of dissimilarity is computed by us ex-ante and is based only on the survey responses of the subject and their match, the Continuous IOS scale is reported by subjects for each of their match. Therefore, it makes sense that the Continuous IOS values would be more in line with what subjects actually think of their match.

Comparing models (2) and (3) allows us to understand better what subjects actually use when responding to the Continuous IOS scale. There are two values obtained from the Continuous IOS scale: the degree of overlap and the distance between the circles. In model (2) we first the former (reversed), and in model (3), the latter. Model (3) gives a slightly larger log-likelihood which suggests that subjects are primarily using the distance between the circles to respond to the IOS scale.

Conceptually, the degree is overlap is what subjects should have in mind as it represents how much one's sense of self overlaps with the other in the context of the IOS scale. The distance between the circles, however, is more intuitive and easier to report.

Table E1: Panel-data mixed logit model, full sample.

	(1)	(2)	(3)
Means:			
Money amount	1.325^{***}	1.585^{***}	1.567^{***}
	(0.000)	(0.000)	(0.000)
Dissimilarity	-2.052^{***}		
	(0.000)		
IOS overlap (reversed)		-5.898^{***}	
		(0.000)	
IOS distance			-5.408^{***}
			(0.000)
Standard deviations:			
Money amount	1.396^{***}	1.587^{***}	1.545^{***}
	(0.000)	(0.000)	(0.000)
Dissimilarity	3.420^{***}		
	(0.000)		
IOS overlap (reversed)		6.766^{***}	
		(0.000)	
IOS distance			6.058^{***}
			(0.000)
Log Likelihood	−8064.343	−6709.655	−6592.552
Number of subjects	525	525	525
Number of choices	18161	18161	18161
Number of observations	36322	36322	36322

Notes. All coefficients assumed to be normally distributed.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Appendix F Relation between structural parameters and demographic characteristics

In this Appendix we look at the relation between structural parameters and demographic characteristics. To do so, we estimate the structural model on the pooled data and regress the demographic characteristics on the recovered, posterior estimates.

We show the results in Table F1. We use a seemingly unrelated regression (SUR) framework, since the Breusch-Pagan test (also reported at the bottom of Table F1) shows that the residuals from independent OLS regressions are correlated. To interpret the results reported in the table, remember that subjects with a larger α have a more concave utility function; those with a larger δ exhibit less social discounting; and those with a larger β give a lower premium to the self compared to others.

Overall, the results we find here are in line with those reported in Section 5.2 of the main text where we looked at what predicts being a social magnifier. In addition, our analysis of the pooled data leads to some new findings. Starting with α, we find that Roman Catholics and those who grew up on a farm have a less concave utility function of money. On the other hand, those who report belonging to the lower class, work part-time, live in the suburbs, or those who completed postgraduate studies have a more concave utility function of money. In terms of β, we find that political Independents and those who live in the suburbs give a lower premium to the self; while those who are married, or separated or widowed, give a higher premium. For δ, the fact that Black subjects have a lower value and are thus less likely to be social magnifiers now becomes significant.

Table F1: Influence of demographic characteristics on structural parameters, SUR regression.

	α	δ	β
Age	0.008	0.001	0.001
	(0.005)	(0.001)	(0.001)
Gender (ref.: Female)			
Male	−0.187	0.076***	−0.034
	(0.095)	(0.020)	(0.028)
Race (ref.: White)			
Asian Indian	0.611	−0.106	−0.079
	(0.389)	(0.082)	(0.115)
Black	0.273	−0.082*	0.032
	(0.156)	(0.033)	(0.046)
Chinese	0.364	0.031	0.147
	(0.319)	(0.068)	(0.095)
Korean	0.464	0.106	0.013
	(0.392)	(0.083)	(0.116)
Other	0.135	−0.058	0.033
	(0.215)	(0.045)	(0.064)
Ethnicity (ref.: None)			
Mexican	0.068	−0.043	−0.031
	(0.207)	(0.044)	(0.061)
Other	−0.271	0.012	−0.019
	(0.278)	(0.059)	(0.082)
Religion (ref.: None)			
Jewish	0.084	0.054	0.121
	(0.465)	(0.098)	(0.138)
Other	−0.009	0.057	−0.056
	(0.241)	(0.051)	(0.071)
Protestant	0.212	−0.017	0.001
	(0.135)	(0.029)	(0.040)
Roman Catholic	0.321*	−0.060*	−0.067
	(0.127)	(0.027)	(0.038)
Political party (ref.: Democrat)			
Independent	0.040	−0.029	0.083*
	(0.116)	(0.024)	(0.034)
No preference	−0.334	−0.003	0.015
	(0.309)	(0.066)	(0.092)

Other party	−0.367	−0.109	0.126
	(0.355)	(0.075)	(0.105)
Republican	0.083	−0.074**	0.039
	(0.117)	(0.025)	(0.035)
Marital status (ref.: Single)			
Divorced	0.036	−0.018	−0.034
	(0.206)	(0.044)	(0.061)
Married	0.036	0.049	−0.107**
	(0.121)	(0.026)	(0.036)
Separated or widowed	0.225	0.008	−0.231*
	(0.339)	(0.072)	(0.100)
Social class (ref.: Middle class)			
Lower class	−0.386*	0.029	0.037
	(0.193)	(0.041)	(0.057)
Upper class	0.433	−0.018	−0.138
	(0.348)	(0.074)	(0.103)
Working class	0.061	−0.037	−0.029
	(0.105)	(0.022)	(0.031)
Work last week (ref.: Full time work)			
Housework	−0.262	0.016	0.087
	(0.157)	(0.033)	(0.046)
Part time work	−0.327*	−0.012	0.030
	(0.142)	(0.030)	(0.042)
School	0.078	−0.028	0.043
	(0.277)	(0.059)	(0.082)
Place growing up (ref.: Small town)			
Farm	0.585*	−0.027	0.000
	(0.287)	(0.061)	(0.085)
Large city	0.111	0.008	−0.040
	(0.137)	(0.029)	(0.041)
Medium city	0.099	−0.007	−0.018
	(0.123)	(0.026)	(0.037)
Open country	0.124	−0.067	−0.013
	(0.247)	(0.052)	(0.073)
Suburb	−0.293*	0.027	0.095*
	(0.129)	(0.027)	(0.038)
Highest degree (ref.: College or some college)			
12th grade no degree and less	0.390	0.087	0.020
	(0.401)	(0.085)	(0.119)

Beyond bachelors	-0.277^{*}	0.015	0.017
	(0.132)	(0.028)	(0.039)
High school graduate	-0.154	-0.033	0.043
	(0.133)	(0.028)	(0.039)
Number of children	-0.034	-0.015	0.022
	(0.042)	(0.009)	(0.012)
Household income	-0.455	-0.172^{***}	0.036
	(0.246)	(0.052)	(0.073)
Constant	1.111^{***}	0.975^{***}	0.619^{***}
	(0.278)	(0.059)	(0.082)
Observations	531		
R^2	0.114	0.128	0.122
Wald χ^2	68.598	77.798	73.639
Prob > χ^2	0.001	0.000	0.000
Breusch-Pagan χ^2	83.919		
Prob > Breusch-Pagan χ^2	0.000		

Notes. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.