Computable and Experimental Economics Laboratory

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# The elicitation of time preferences

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# The elicitation of time preferences

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**Abstract**

We compare three methods for the elicitation of time preferences in an experimental setting: the Becker-DeGroot-Marschak (BDM) procedure (BDM), a second price auction and the multiple price list format. The first two methods have been used rarely to elicit time preferences. Although all methods used are broadly strategically equivalent, and should induce the same 'truthful' revelation, we find that the methods do differ: the money discount rates elicited with the multiple price list tend to be higher than those elicited with the other two methods. Furthermore, there are no significant differences between the rates elicited with the BDM and the auction elicitation procedure.

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# 1 Introduction

Most economic decisions have a time dimension (e.g. investments, pensions) and therefore it is important to develop accurate theoretical model models and reliable empirical methods to elicit the time preferences of individuals. Over the past twenty years or so a body of empirical evidence, mostly experimental, has emerged documenting systematic contradiction between actual behaviour and the predictions of the standard model. Various exponential discounting 'anomalies' have been identified, and various theories have been put forward to try and explain them (see e.g. Manzini and Mariotti [22]).

One of the more puzzling findings is that of widely varying ranges for discount factors estimates (see e.g. Table 1 in Frederick, Loewenstein and O'Donoghue [11]), highlighting the fact that assessing time preferences is a far from trivial matter. Studies in this vast literature do not proceed in a standard way, and many are the confounding factors from one study to another, which hamper systematic comparisons to determine to what extent these differences depend on the elicitation methods themselves as opposed to other differences in experimental design. In a nutshell, at the level of experimental design the main issues that emerge are the following:

- not all studies elicit time preferences in an incentive compatible way;[1]
- even when an incentive compatible mechanism is used, it may still suffer from not being sufficiently 'robust': as noted by Harrison [12], some elicitation methods suffer from serious incentive properties in the neighbourhood of the truth telling dominant strategy: Deviations may be 'cheap' enough that experimental subjects do not select the dominant strategy;[2]
- the above aside, some recent empirical advances[3] even put into serious question certain results of the 'traditional' evidence.

That is, on the one hand techniques for the *estimation* of discount factors starting from the monetary penalties that subjects in the lab are prepared to pay in order to anticipate receipt of monetary rewards are still being developed. On the other hand, backtracking

[1] This would require not just participants to be paid, but paid in such a way that their payoff depends on their answer, and induce the participant to state her or his 'true' valuations. As we find in our own work (see Manzini and Mariotti [21], and Manzini, Mariotti and Mittone [23]), these differences can be very substantial, and should be taken into account.

[2] See also other common elicitation pitfalls in Harrison, Lau and Rutstrom [14].

[3] See e.g. Anderson et al. [3], Cubitt and Read [9], and Noor [24]. We discuss them further below.

one step, these difficulties with estimation notwithstanding, the *elicitation* of lab money rates, the starting point for estimation, does not yet proceed along standardised tracks.

In this paper we compare three methods to elicit time preferences. For all of them, we focus on eliciting the maximum amount subjects are prepared to pay in order to anticipate receipt of a monetary reward ("speed up" condition). We investigate whether or not the various elicitation procedures yield consistently different results, considering the more widely used elicitation methods. The first is the Multiple Price List Method (henceforth 'Tables'), currently the most used method for preference elicitation in the time domain. In addition, the so called Becker-DeGroot-Marschak [5] (henceforth BDM) and the 'sealed bid auction' (henceforth 'Auction') are the most widely relied upon methods to elicit 'home-grown' values in the goods domain. As far as we are aware, the BDM has been used in the time domain only twice before[4], and in a paper and pencil settings as opposed to computerised sessions. Auctions too have been used very rarely in the past for the elicitation of time preferences, and anyhow prevalently in the psychology rather than the economics literature[5]. We implement original modified versions of each of these methods for the time domain.

As will be clearer once we dwell into the details of each method, the Multiple Price List method falls into the category of *choice tasks*: subjects are simply asked to choose between two different amounts available at different dates. On the other hand, the other two methods, Auction and BDM, can be classified as *matching tasks*: broadly speaking subjects have to specify what amount available earlier would be equivalent to a later, fixed reward. That pricing and matching tasks can give rise to different 'prices' has been known for a long time, but in situations not involving delayed rewards[6]. In the time domain, Read and Roelofsma [30] study whether differences might emerge, and although they do find some evidence for this (i.e. their subjects are less patient when answering choice rather

[4]See Manzini [20] and Benhabib, Bisin and Schotter [6].

[5]See e.g. Kirby and Marakovich [17], who compare real and hypotetical delayed rewards within a *first* price auction mechanism, in rather small samples (22 subjects in the reas reward treatment, and 20 in the hypotehtical treatment). Kirby [16] uses a second price sealed bid auction. Here, though, subjects had to use their own money to bid to have the right to receive a delayed reward (i.e. the question asked was "The item up for auction is \$$X$. The most I would be willing to pay for this item immediately is ...", where $X$ was a (varying) monetary amount, and subjects had to fill in the blank with their own bid. This experimental design is close in spirit to Horowitz [15], were subjects bids for bonds that matured with delay. In our own experimental design our objective is to elicit the bid that makes the subject indifferent between receiving a larger sum later (LL) or the (elicited) smaller reward sooner (SS). That is, we believe that our experimental design makes immediately clear what SS and LL are.

[6]The first paper to uncover such differences is Lichtenstein and Slovic [18].

than matching questions), their experiment was conducted using hypothetical payments, and the choice task did not use an incentive compatible mechanism[7].

In this paper we rely on real payments, and all the elicitation mechanism we use are, as we will see more in detail, incentive compatible, in that declaring one's true 'time preference' is a weakly dominant strategy. Furthermore, all these elicitation methods are broadly[8] 'strategically equivalent': from a decision theoretic point of view there is no difference between them[9], and a 'rational' decision maker is expected to behave in the same way in all of them, the differences being simply ones of framing of the problem. Contrary to this benchmark expectation, we find that the methods do differ. First of all, money discount factors elicited with the Tables method are smaller than those elicited with the other methods. Secondly, unlike previous evidence in domains different from time,[10] we find that the BDM and the Auction method provide similar elicited values. Finally, for all three methods there appears to be no significant effects of changes in the stake on the elicited money discount factors, i.e. we find no evidence of magnitude effects. The rest of the paper is organised as follows. We detail the elicitation methods used in the next section, where we also describe our experimental design. We discuss in detail the strategic equivalence between the three elicitation methods used in section 3. The results are reported in section 4, with further details confined to the Appendix, which also includes the experimental instructions. Discussion of our estimates to control for socio-economic variables and interaction effects are in section 5, while section 6 concludes.

# 2 Methods

In our experiment we consider three widely used elicitation methods. The Table method pioneered by Coller and Williams [8] is used regularly for preference elicitation in the

[7] A very recent study addressing the difference between matching and choice task is Tokarchuk [32], who (in samples with an average of 16 subjects per treatment) analyses differences between choice and matching tasks in a variety of different treatments. Here there were real incentives, but the elicitation mechanism used for the matching task is not incentive compatible. She find that subjects are more impatient with the matching task than with the choce task.

[8] More precisely, the three elicitation methods are almost everywhere strategically equivalent, in that exact equivalence breaks down at a single point in the continuum strategy space. At such point, if deviating from truthtelling, the payoff with the Auction method will generally lie above the corresponding payoff in the BDM and Tables method, but still be below the payoff in case of truthtelling. That is, the incentive is still for truthful revelation of one's valuation. See sections 2.1.2-3 for details.

[9] Albeit with a caveat in correspondence with specific values - we discuss this further in sections 2.1.2-3.

[10] See e.g. Elisabet Rutström [31] or more recently Noussair, Robin and Ruffieux [25].

time domain, while BDM and the sealed bid auction are widely relied upon for preference elicitation in the *goods domain*. Since these three methods are broadly strategically equivalent in theory, as we discuss further in section 3 below, it seems appropriate to test whether they all deliver the same results in an experimental setting. In order to have as large a sample as possible, we limit ourselves to the speed up frame only, which in pilots appeared to be easier for subjects to understand than the delay frame. Within each treatment, we investigated time preferences for different time horizons. The experimental design is detailed in the section below.

## 2.1 Experimental design

In our design we had 6 distinct groups by elicitation method and size of the monetary stake, as follows, where each cell reports the number of valid data collected fo each treatment (with a total of 377 subjects):

| | Low stakes (€20) | High stakes (€50) |
|---|---|---|
| Tables | 62 | 65 |
| BDM | 63 | 62 |
| Second price auction | 62 | 63 |

Table 1: The six treatments

We followed the literature (e.g. Harrison, Lau and Williams [13]) in eliciting, in each cell, time preferences over three different time horizons (1, 2 and 4 month horizons). For each treatment we implemented a speed-up frame with no interest rate indication (either of the prevalent market interest rate or of the interest rate implied by the choice). In all treatments we used real monetary rewards: in addition to the fixed participation fee, 50% of the subjects in each group were drawn at random to receive a payment consistent with their choices (we explain more precisely how for each of the three methods below).

Indication of implied interest rates is now common when using Tables for elicitation. This calls for further discussion of our decision not to report any interest rate. First of all, as we detail later, in our view interest rate indication limits the scope of the Tables method. In addition, in continental Europe in general, and in Italy in particular, there is much less awareness of interest rates as compared to Anglo-Saxon countries, so that we conjectured that at best this information would be ignored, and at worst it could confuse our participants. Indeed, unawareness of market rates is what emerged from the answer to financial questions put to our subjects in a questionnaire after the elicitation phase.[11] We were satisfied that by not stating implied interest rates we had not withheld from subjects a crucial piece of information.[12] Since many of the issues raised so far depend on the way the Tables method is implemented, below we analyse it in some depth.

### 2.1.1 Tables

This method, pioneered in its current form by Coller and Williams [8], consists in asking a decision-maker (DM) questions of the type "Do you prefer: A) $a$ today or B) $B$ at time $T$", where $a$ is some monetary amount which increases steadily (from a starting value of zero) as the subject considers the sequence of questions (for this reason the method is also dubbed 'multiple price list format', or MPL, in the literature). A rational decision maker would start switching from selecting A to selecting B from one specific value of $a$ onwards, so that one can infer the (money) discount factor. Although this method had been used previously for the elicitation of time preferences, Coller and Williams' innovation was to introduce two additional pieces of information: the annual discount/interest rate implied by each choice, and the prevalent market rate in the real economy (to avoid that subjects anchored their choices to their own experience, unknown to the experimenters). The problem with this method is that it does happen that subjects exhibit several switches between A and B. Because subjects are forced to choose only one option, either A or B, multiple switches had been sometimes interpreted as evidence of indifference of the experimental subject between the two options. More recently Andersen et al. [3], have addressed this issue, and have considered several alternative MPL implementations: one where they explicitly allow subjects to express indifference, one where subjects are required to state explicitly a switching point, and one where, once a switching point is identified, further questions are asked in order to narrow down further the range of discount factors

[11] Answers to the questionnaires following the elicitation phase show that the vast majority of the participants did not know what the interest rate was on either checking or savings accounts. Only 70 out of 376 respondents (i.e. 18.6%) stated that they had a current account. Of these 70, only 31 (just over a half) thought they knew the interest rate on their current account. As there were indications of 7%, 8%, 10% and even 12% rates, while we found no current accounts paying more than 4% on the market at the time, even if one were to take these rates as what subjects really thought they were getting, it is pretty clear to us that their level of financial competence when it comes to interest rates is less than expert (!).

[12] As for savings accounts, about a third of subjects - 118 - declared they had one. Of these, 49, i.e. around 40%, stated they knew what the interest rate they were getting was, but 14 of these – i.e. almost 30% - stated a rate of at most 1%, and a further 16 stated rates between 1 and 2%: if this is really what they were getting, it was not a good deal!

(i.e. a new 'sub-table' is presented with smaller increments in $a$). The authors find no appreciable differences in the results, so that we take this as evidence that any of these methods could be used. Our reservation, though, concerns a different issue, that of the size of the stakes considered in the literature. All the papers we have mentioned use very large monetary amounts, in the order of several hundreds of dollars, and - presumably for budget reasons - either only a small fraction of the participants is then drawn to be actually paid (e.g. 1 chance in 35 in the Coller and Williams [8], 1 in 10 in both Andersen et al. [3] and [4]), or the sample is small (e.g. only 10 subjects per treatment on average in Andersen et al. [3]), or both. We feared that this might induce subjects to exert less effort than one would have wished, perceiving only a small probability of being paid. In addition, the large monetary amounts usually used in the literature, to which our student population of subject would be likely unaccustomed to, together with low probabilities might enhance the *perception of these choices as gambling* (in natural parlance terms), and induce respondents to always opt for the larger option, no matter what. But why use such large monetary amounts as rewards? In these experiments the tables also report the rate of interest associated with each $a$, as well as the prevalent market interest rate. Consider for example Table 1 from Coller and Williams [8] (which uses a 'delay frame').

| | Payment option A (pays amount below in 1 month) | Payment option B (pays amount below in 3 months) | Annual interest rate (AR) | Annual Effective interest rate (AER) | Preferred option (circle A or B) | |
|---|---|---|---|---|---|---|
| 1 | \$500 | \$501.67 | 2.00% | 2.02% | A | B |
| 2 | \$500 | \$502.51 | 3.00% | 3.05% | A | B |
| 3 | \$500 | \$503.34 | 4.00% | 4.08% | A | B |
| 4 | \$500 | \$504.18 | 5.00% | 5.13% | A | B |
| 5 | \$500 | \$506.29 | 7.50% | 7.79% | A | B |
| 6 | \$500 | \$508.40 | 10.00% | 10.52% | A | B |
| 7 | \$500 | \$510.52 | 12.50% | 13.31% | A | B |
| 8 | \$500 | \$512.65 | 15.00% | 16.18% | A | B |
| 9 | \$500 | \$514.79 | 17.50% | 19.12% | A | B |
| 10 | \$500 | \$516.94 | 20.00% | 22.13% | A | B |
| 11 | \$500 | \$521.27 | 25.00% | 28.39% | A | B |
| 12 | \$500 | \$530.02 | 35.00% | 41.88% | A | B |
| 13 | \$500 | \$543.42 | 50.00% | 64.81% | A | B |
| 14 | \$500 | \$566.50 | 75.00% | 111.53% | A | B |
| 15 | \$500 | \$590.54 | 100.00% | 171.45% | A | B |

Table 2: Coller and Williams original table

For the money increment $a$ to be appreciable, or for the corresponding interest rate not to be ridiculously high, here the base amount must be large ($500 in this example).

For instance, consider a base of 50, which in either pounds, dollars or euros is a large amount for many undergraduate students. The table would change to Table 3 below. One can readily see that, even with interest rates as large as 50%, you are only looking at 4.12 monetary units extra as compensation to wait for two more months: we do not think one needs to run an experiment to verify that, *without interest rate indication*, virtually all respondents would opt for the smaller, sooner option **A** for at least the first ten rows, regardless of anchoring.[13] Although Coller and Williams [8] did find a difference between choices with and without indication of the interest rate, arguably this could simply be down to a pure framing effect that has nothing to do with real preferences. To put it crudely, a person may think that 1.67 extra monetary units are not worth the extra wait, but once she is told that amounts to 20% interest, she may feel foolish at choosing something different, simply because "20" sounds like a large number.

| | Payment option A (pays amount below in 1 month) | Payment option B (pays amount below in 3 months) | Annual interest rate (AR) | Annual Effective interest rate (AER) | Preferred option (circle A or B) | |
|---|---|---|---|---|---|---|
| 1 | 50 | 50.17 | 2.00% | 2.02% | A | B |
| 2 | 50 | 50.25 | 3.00% | 3.05% | A | B |
| 3 | 50 | 50.33 | 4.00% | 4.08% | A | B |
| 4 | 50 | 50.42 | 5.00% | 5.13% | A | B |
| 5 | 50 | 50.62 | 7.50% | 7.79% | A | B |
| 6 | 50 | 50.83 | 10.00% | 10.52% | A | B |
| 7 | 50 | 51.04 | 12.50% | 13.31% | A | B |
| 8 | 50 | 51.25 | 15.00% | 16.18% | A | B |
| 9 | 50 | 51.46 | 17.50% | 19.12% | A | B |
| 10 | 50 | 51.67 | 20.00% | 22.13% | A | B |
| 11 | 50 | 52.08 | 25.00% | 28.39% | A | B |
| 12 | 50 | 52.92 | 35.00% | 41.88% | A | B |
| 13 | 50 | 54.12 | 50.00% | 64.81% | A | B |
| 14 | 50 | 56.25 | 75.00% | 111.53% | A | B |
| 15 | 50 | 58.33 | 100.00% | 171.45% | A | B |

Table 3: Coller and Williams' type table with smaller stakes

[13] Indeed, this is along the lines of the results in Read, Airoldi and Loewe [29], who find that when only interest rates are indicated, median money discount rates are much lower than when both interest rates and the corresponding monetary value are indicated, which in turn are lower than when only monetary amounts are reported in the table.

Now put this point aside, and suppose one decides after all to insert the information regarding interest rates. As pointed out by Harrison [12], if the increments from one row to the next are not large enough, the cost of a 'mistake' to a subject in stating his true preference is small enough that elicitation cannot be relied upon. If, then, one were to consider larger increments, the corresponding interest rates would be ludicrously high. For instance, even with €0.50 increments, starting from €52 as option **B** in the first row, the table would look as Table 4.

| | Payment option A (pays amount below in 1 month) | Payment option B (pays amount below in 3 months) | Annual interest rate (AR) | Preferred option (circle A or B) | |
|---|---|---|---|---|---|
| 1 | €50 | €52.00 | 24.00% | A | B |
| 2 | €50 | €52.50 | 30.00% | A | B |
| 3 | €50 | €53.00 | 36.00% | A | B |
| 4 | €50 | €53.50 | 42.00% | A | B |
| 5 | €50 | €54.00 | 48.00% | A | B |
| 6 | €50 | €54.50 | 54.00% | A | B |
| 7 | €50 | €55.00 | 60.00% | A | B |
| 8 | €50 | €55.50 | 66.00% | A | B |
| 9 | €50 | €56.00 | 72.00% | A | B |
| 10 | €50 | €56.50 | 78.00% | A | B |
| 11 | €50 | €57.00 | 84.00% | A | B |
| 12 | €50 | €57.50 | 90.00% | A | B |
| 13 | €50 | €58.00 | 96.00% | A | B |
| 14 | €50 | €58.50 | 102.00% | A | B |
| 15 | €50 | €59.00 | 108.00% | A | B |

Table 4: Coller and Williams' type table with 0.50 Euro increments

We get up pretty quickly to ridiculously high interest rates, not to mention wide intervals as a basis to estimate discount factors.

On the other hand, we do not believe one should give up trying to ascertain time preferences for monetary rewards which are still sizeable although not extraordinary, i.e. the kind of monetary amounts one generally deals with everyday. For this reasons we considered an experimental design with no mention of interest rates, and two different monetary amounts, €20 and €50 (which in Italy, where we carried out our experiment, go a little further than £20 and £50 would in the UK[14]).

[14] E.g. the price of a coffee in Trento is around €0.85 against 1.30 British Pounds, equivalent to approximately €2.

Note that we are interested in comparing elicitation methods, *not in obtaining an estimate of the implied discount factors*. Consequently, provided we keep similar 'error margins' across methods (i.e. the unit of measurement of monetary discounts), we are not worried by the fact that implied discount factors might be large or small. Further, we omit indication of the interest rates corresponding to a subject's choice from *all* estimation methods: a priori there is no reason why this omission should have a differential effect on the elicitation methods we employ.

Besides the interest rates/stake magnitude issues that we have discussed so far, one additional matter is how to implement a single switching point and a fine enough mesh of increments within a table that is still manageable in size, and useful for subjects to move about. Below we show in figures 1-3 a sequence of three sample screenshot (where, recall, we use a 'speed up frame'). The first one illustrates the initial 'blank' form presented to each participant.

The second screenshot shows a hypothetical choice in the top part: note that numbers now appear in the bottom part of the screen.

Finally the third screenshot shows a completed table. In the top part of the table, increments in option **A** from one line to the next are much larger than those in the bottom part (in this example the increments in the top are in €5 steps). The bottom part "expands" the two lines at which the subject switches between option **A** and option **B**. This allowed us to consider small increments within a single screen. For the reasons discussed in Harrison [12] we kept to a minimum increment of €0.50. In addition, our software implemented a single switch table, as positioning the cursor would select option **B** in all the rows lying below the cursor, and option **A** in all other rows.

Each subject was presented with 3 different tables, corresponding to 3 different time horizons (1, 2 and 4 months delay), appearing in random order. The same questions were asked for the other two elicitation methods. As for payment, at the end of the experiment we drew from a uniform distribution which 8 subjects (out of 16 participants in each computerised session) would receive a payment in addition to the show up fee; which screen (1 month, 2 months or 4 month delay) would 'count', and which row in that screen (the payment corresponding to the option, **A** or **B**, chosen in that row).

### 2.1.2 Auctions

We implemented a sealed-bid second-price auction to make it as similar as possible to the setups in the other two elicitation methods. When auctioning a good, it is pretty clear to participants that what they are offering is a price to obtain the good. In our case the


Figure 1: Initial sample screen for the Table elicitation method


Figure 2: Sample screen after a first choice has been indicated in the top panel

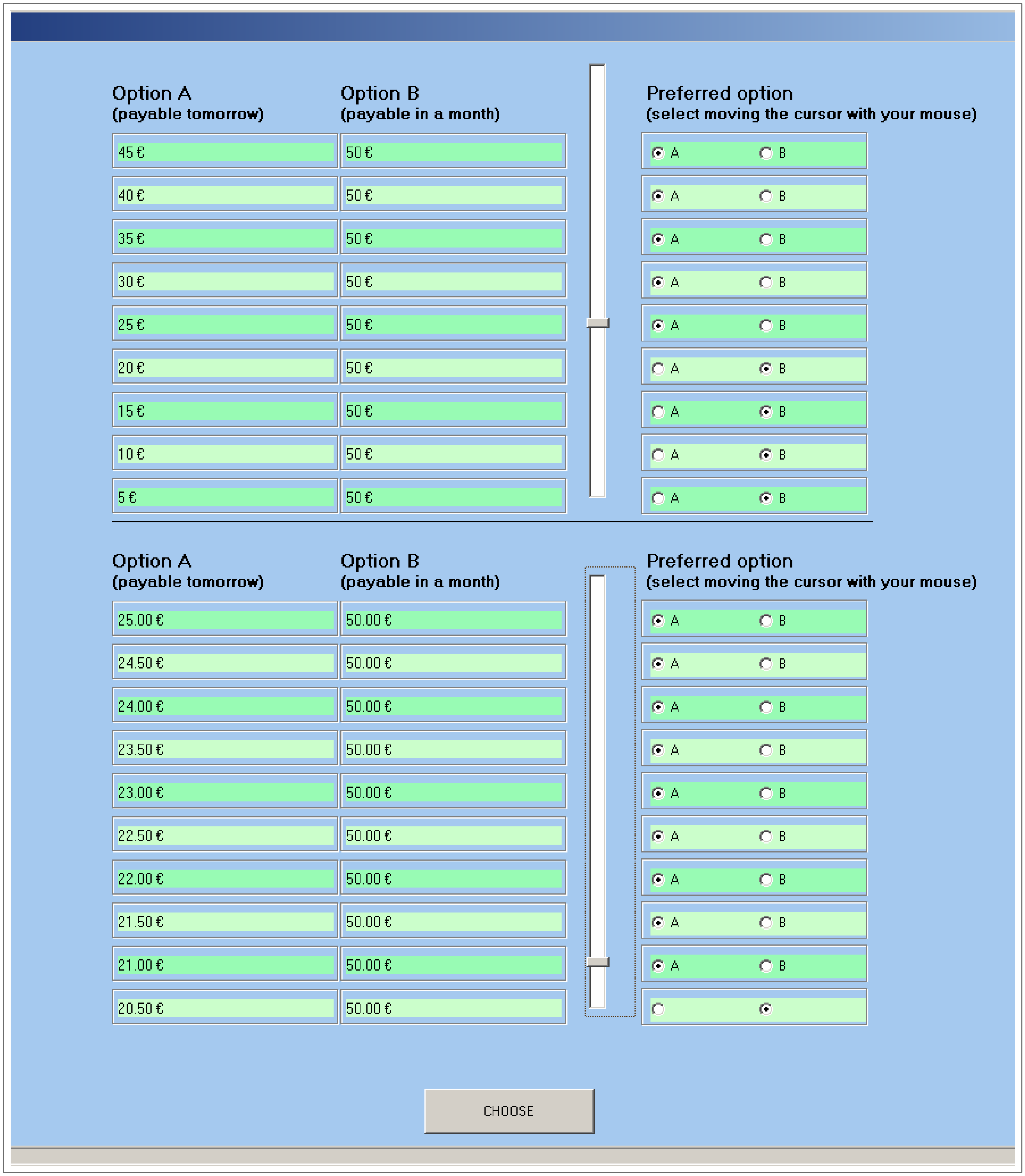


Figure 3: Sample screenshot for a completed decision (in both top and bottom panel)

good in question is time: so subjects were asked to state the minimum amount they would accept in order to anticipate receipt of a given amount (either €20 or €50, depending on the treatment). We believe this is a more direct way to frame the problem which is easier for participants to understand, as opposed to asking them to state how much they would be prepared to pay in order to anticipate receipt, and then work out by themselves how much money they would actually receive. The participant stating the lowest amount would win the right to anticipate the payment, and he would obtain the second to highest amount. A sample screenshot is visualised in figure 4.

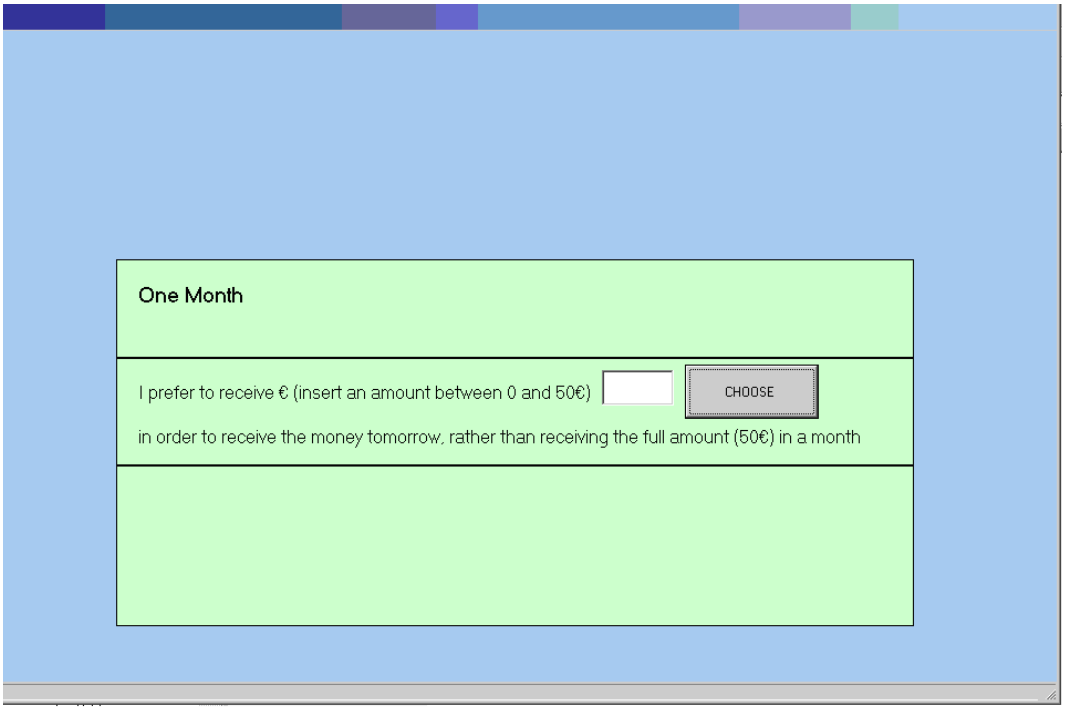


Figure 4: Sample screenshot with the elicitation question for the auction method

The outcome of each auction was not revealed before the next auction was played, to keep the three decision problems as distinct as possible. At the end of the each session 8 out of the 16 participants were drawn at random for payment, and one screen at random was also drawn. Selected participants received payment based on the outcome of the auction (if drawn, the winner of the auction received the second lowest amount the following day; all losers, if drawn, would receive the full amount with a delay depending on which screen had been drawn).

As is well known, in a second price auction truthful revelation of one's (perceived) true valuation is a weakly dominant strategy. A strategy for the subject consists in stating an amount $a$. Under truthful revelation, the subject should declare the amount $a^*$ that makes the agent indifferent between receiving $a^*$ sooner (denote this option by $(a^*, 0)$) and the full amount $B$ later (which we denote by $(B, 1)$). The payoff in case of truthtelling is depicted in figure 5, where on the horizontal axis we measure the minimum bid, and on the vertical axis the payoff accruing to the decision maker playing the auction.

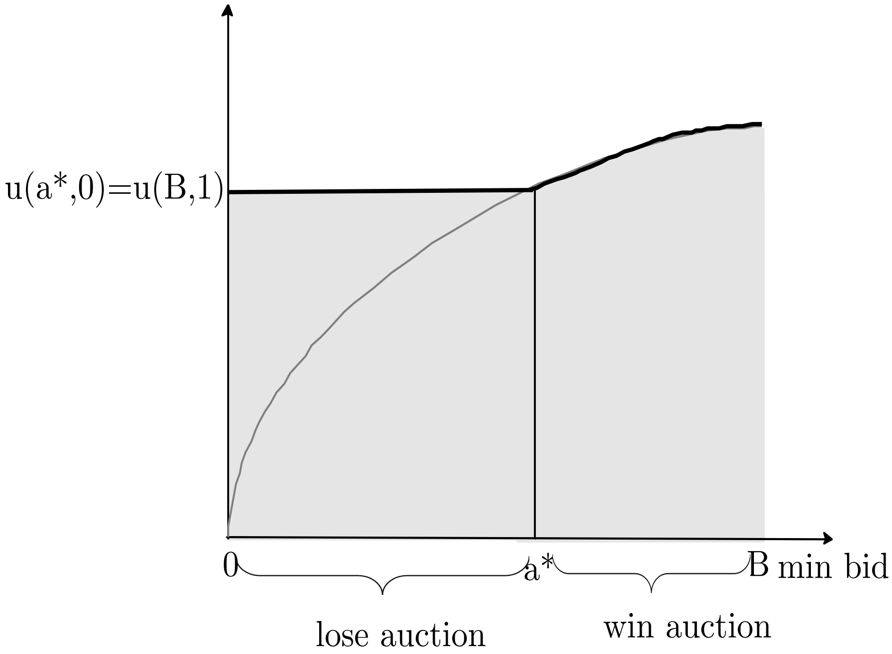


Figure 5: Truthtelling payoff with the second price auction mechanism

Assuming that the preferences are summarised by some utility function, truthful revelation that the payoff derived form the two indifferent outcomes $(a^*, 0)$ and $(B, 1)$ is the same, i.e. $u(a^*, 0) = u(B, 1)$. In figure 5 the light grey line represents the decision maker's utility for money. Consider the case when the agent bids his true valuation $a^*$. This means that if the minimum valuation is below $a^*$, the agent is going to lose the auction, and receive the full amount $B$ with delay, which explains the flat portion of his payoff function. If instead the minimum bid of the opponents is above $a^*$, then the agent is going to receive that amount earlier, explaining the increasing portion of his payoff function to the right of $a^*$. Finally, if some other bidder also bids $a^*$ and this is the minimum bid, then the agent will receive some convex combination of $a^*$ earlier and the full amount $B$ later. But since by construction $a^*$ is what makes the decision maker indifferent between these two outcomes, his payoff is exactly at $u(a^*, 0) = u(B, 1)$.

Consider now deviations from truthtelling, as depicted in figure 6. In the panel on the left, the solid black locus depicts the payoff in case of overstating one's true value, while the payoff when understating the true value is drawn as the solid black locus on the right.

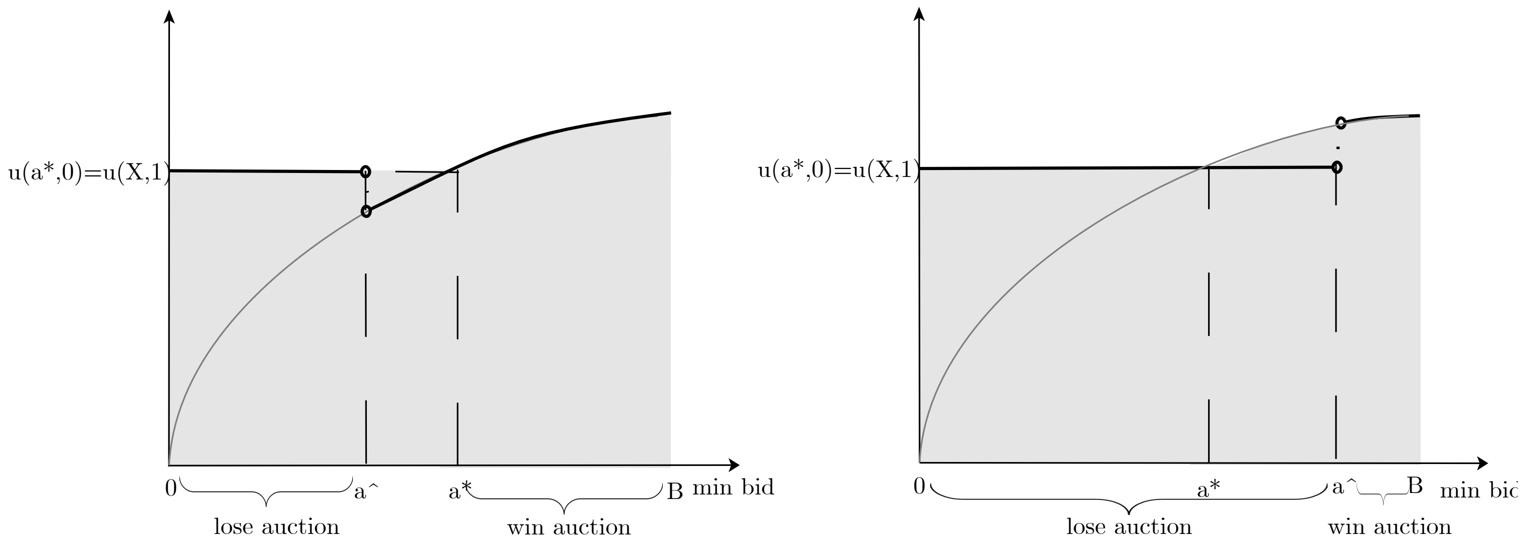


**Figure 6:** Payoffs in case of deviation from truthtelling in the Auction elicitation method

At a tie, the exact payoff depends on how many bids are tied, and lies somewhere between $u(B, 1)$ and $u(\text{min bid}, 0)$. In any event, the decision maker cannot profit from a deviation, as readily evident by comparing with the grey area, which represents the truthtelling payoff (as in figure 5). Note that one has to impose that if the minimum bid is $B$ no subject receives an anticipated payment: this rule results in a payoff function in case of overbidding that is a flat line at $u(B, 1)$, thereby ruling out any incentive to overbid one's true value.[15]

### 2.1.3 BDM

As explained above, the BDM has a very similar structure to the second price auction. Here, too, participants are asked to state the minimum amount they would be prepared to accept in order to anticipate receipt of their money, rather than wait. For each decision, if a subject input a value lower than a value drawn from a uniform distribution with support up to $B$, then he would receive the day after an amount of Euros equal to the number drawn. Otherwise he would get the full amount $B$ with delay. At the end of each session again we proceeded with a number of draws in order to determine, besides the result of

[15] Observe that *underbidding* one's true value is always suboptimal, even at 0. Also, if the true valuation were $a^* = 0$, i.e. if the decision maker were maximally impatient, the truthtelling payoff would coincide with the light grey line of figure 5 over the whole support, and it is easy to see that no deviation from truthtelling would be profitable.

the ‘auction’, which 8 subjects to be paid in each session, the screen that would ‘matter’. Payoffs would be determined based on the comparison between the number declared and the number drawn, as explained below. A sample screenshot is in figure 7.

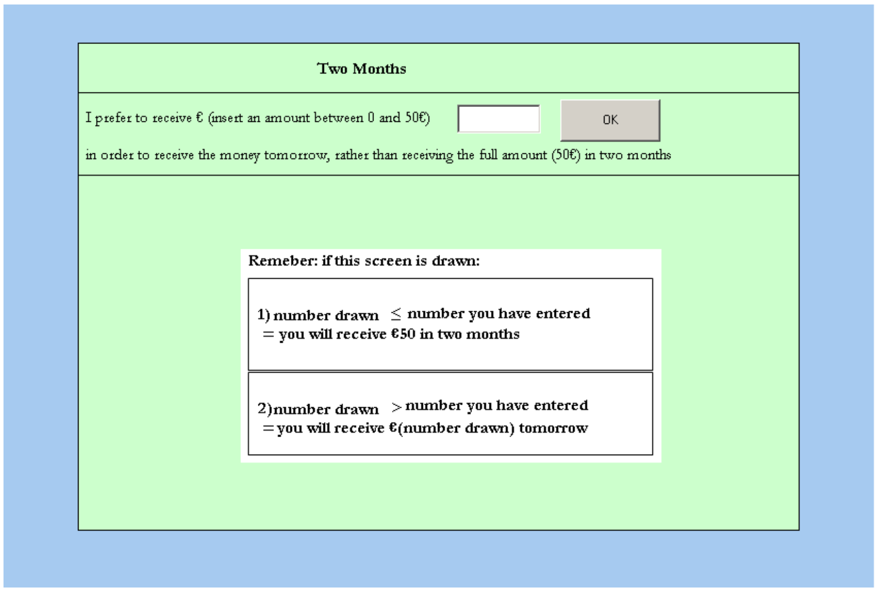


Figure 7: Sample screenshot for the BDM elicitation method, two month version

How ties between the number drawn and the value stated by each subject are dealt with is rather important to determine that truthtelling is indeed a (weakly) dominant strategy for a participant. To see this, consider the payoff for a decision maker who states truthfully his valuation at $a^*$, depicted in figure 8.

Similarly as for the Auctions, the vertical axis measures the utility of amount $x$ received early. As before, utility for money is represented by the light grey line, while the solid black line represents the payoff in case of truthtelling when the BDM mechanism is applied. If the decision maker truthfully states $a = a^*$, then for any number drawn which is not greater than $a^*$, the decision maker receives the full amount $B$ with delay, yielding utility $u(B, 1) = u(a^*, 0)$, corresponding to the flat part of the graph; if instead the number drawn is greater than $a^*$, then the subject receives an amount equal to the number drawn sooner, and his payoff now follows the dashed line. If we were to change the rules so that the full delayed amount is received when the number drawn is smaller (rather than smaller or equal), and the amount drawn when the latter is greater or equal than the one declared (rather than greater), the payoff would stay unchanged, as there are no discontinuities.

Consider now the payoffs in case the subject does *not* truthfully declare $a = a^*$ (figure 8). The panel on the right shows payoffs if $\underline{a} < a^*$ is declared, whereas payoffs if $\hat{a} > a^*$

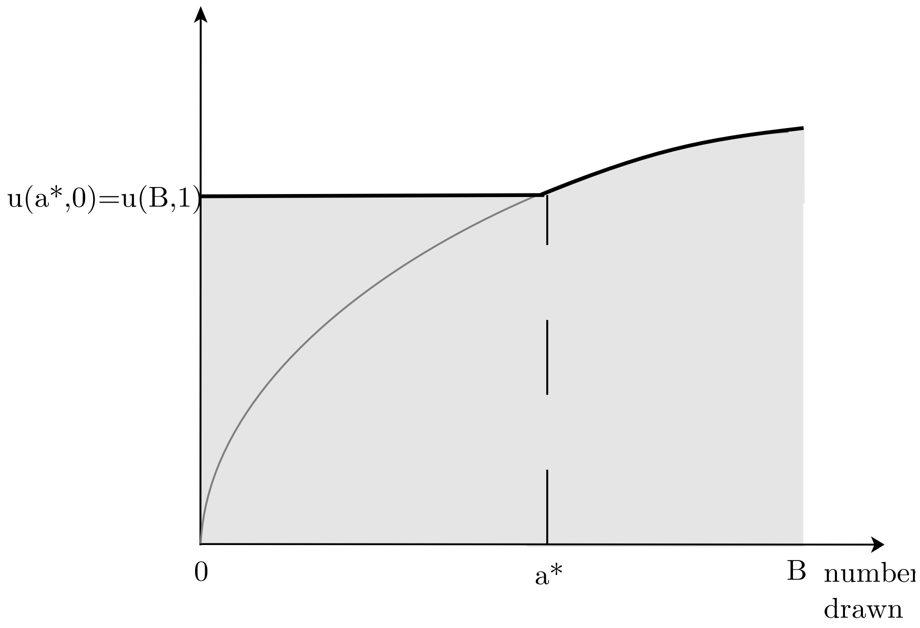


Figure 8: Truthtelling payoff with the BDM mechanism.

is declared are on the left, with the BDM mechanism implemented as in figure 7 (i.e.the full amount being corresponded with delay if and only if the number drawn is *smaller or equal* to the one declared - and the amount drawn sooner otherwise). For convenience we have kept the grey area below the truthtelling payoff. Now a discontinuity is introduced when the number drawn is the same as the one declared, as under this implementation of the BDM mechanism in that case the subject receives the delayed amount later.

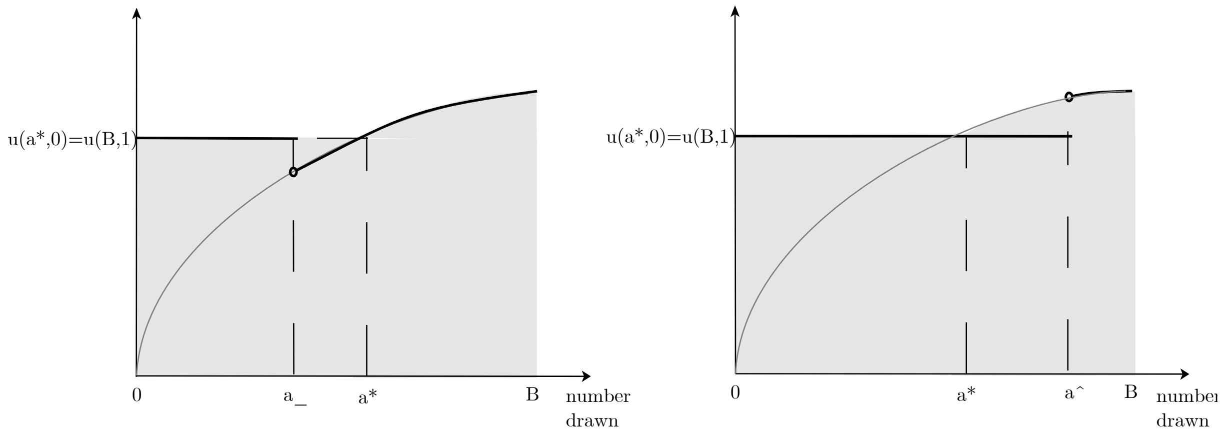


**Figure 8:** Payoffs in case of deviation from truthtelling when the number drawn is received if and only if it is **strictly greater** than the one declared

If instead we consider the alternative implementation in which the full amount being corresponded with delay if and only if the number drawn is *strictly smaller* than the one declared (and the amount drawn sooner otherwise), then the payoff discontinuity still exists, but is the opposite as in the previous case, as depicted in figure 9. In any event, regardless of which of the two complementary implementations is used, the fact remains that truth telling remains a weakly dominant strategy

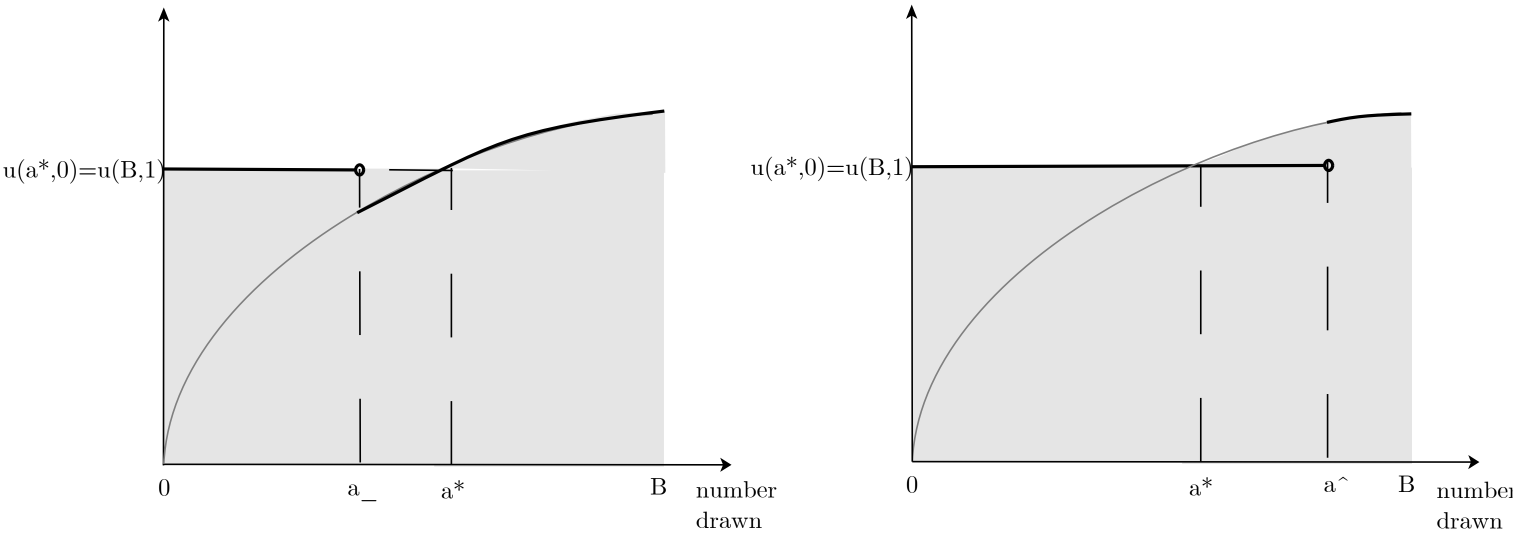


**Figure 9:** Payoffs in case of deviation from truthtelling when the number drawn is received if and only if it is **greater or equal** to than the one declared

However, consider the deviation $\hat{a} = B$. Under the BDM implementation we have followed, the corresponding payoff is that of receiving the full amount with delay, which cannot improve on the payoff in case of truthtelling. However, if the BDM implementation assumes that in case of equality the amount drawn is received earlier, then there is the possibility of a profitable deviation, since if exactly $B$ is drawn, the deviator who declared $\hat{a} = B > a^*$ would receive $B$ earlier, and since he is not perfectly patient, he would be strictly better off, as visualised in figure 10.[16]

[16] The informal discussion can be rendered a little more rigorous as follows. Denote by $n$ the number drawn from a uniform distribution over $[0, B]$. Recall the rules for this procedure: if the number drawn $n$ is larger than the declared amount $a$, the subject will receive $n$ earlier, while otherwise he will receive $B$ later. Fix $n$. Suppose $a^* \geq n$. The truth telling outcome would be that the subject gets $B$ with delay. Consider now possible deviations from truth telling. By declaring $a > a^* \geq n$ the outcome does not change while by deviating to $a < a^*$ there are two possibilities: if $a \geq n$, again the outcome does not change, whereas if $a < n$, the subject gets $n$ sooner: but if preferences are monotonic , the fact that $a^* \geq n$ implies that $(a^*, sooner) \succsim (n, sooner)$, where $\succsim$ indicates weak preference; and if preferences are transitive, then we also have $(B, later) \succsim (n, sooner)$ (since by construction $(B, later) \sim (a^*, sooner)$, where $\sim$ denotes indifference).

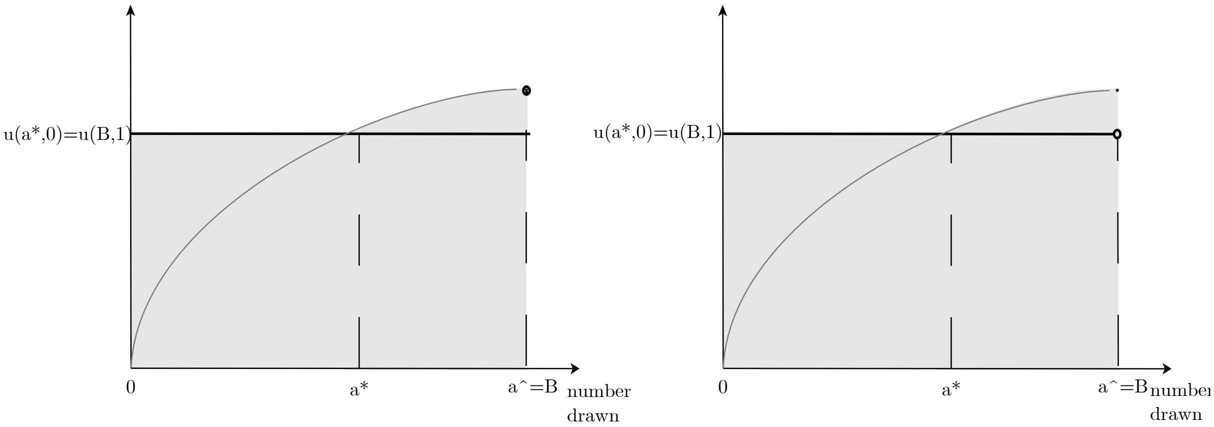


**Figure 10:** Incentive to overstate the true valuation when the number drawn is received also when it is **equal** to the one declared (right hand panel)

One final remark concerns the behaviour at 0. With our implementation, deviating to $a\_ = 0$ would create a discontinuity in the payoff, as *if* the number drawn were exactly 0, the subject would obtain $(B, 1)$, whereas with $a_{-}$ just above 0, that same draw of 0 would result in a payoff of $(a_{-}, 0)$, as depicted in figure 11. We thought this would be difficult to explain to subjects and generate confusion. Thus in order to make instructions clearer, we excluded zero from the support of the random draw, although not from the support of the values that the subject could declare (see instructions in the appendix).

# 3 Strategic equivalence

As we saw in the previous section, the BDM and the Auction mechanism are almost strategically equivalent, in the sense that each strategy employed by a decision maker when playing the BDM would generate the same distribution over outcomes and payoff as would the same strategy played in an Auction, the only exception being the payoff at a strategy off the equilibrium path in case of a tie between the stated value and the

Consider now the case $a^* < n$, so that by declaring the true valuation the subject would receive $n$ with delay. By deviating to $a \leq a^*$ the outcome does not change. By deviating to $a > a^*$, either the outcome does not change (if $a < n$); or it changes to $B$ with delay $(y, t') \sim (a^*, t) \prec (n, t)$, so that the subject is worse off.

This argument clarifies that truth telling is weakly dominant, of course in the event that the subject believes he will get the delayed amount $B$ for sure. What if this is not the case? If this is a situation of uncertainty, rather than risk, then the effort of succesful elicitation is most likely doomed. But even so, one could regard the delayed amount $B$ as the outcome of a simple lottery with prob $p$ (i.e. with prob $1 - p$ you will never see that money), while $a^*$ is the certainty equivalent of such lottery.

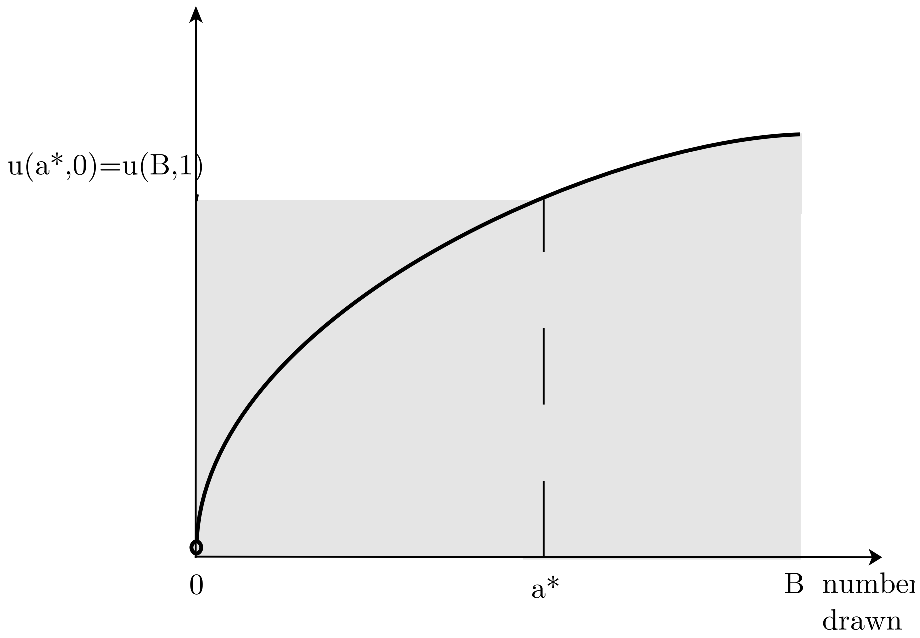


Figure 9: **Figure 11:** Truthtelling payoff with the BDM mechanism with a valuation $a^* = 0$

number drawn/minimum bid (depending on the mechanism, see figures 6 and 8). In both mechanisms, though, truthtelling is a weakly dominant strategy. So, bar for those cases, provided that the grid of allowable values that each decision maker is the same, each strategy (i.e. declared value) in one mechanism maps uniquely into (the same) strategy and payoff in the other setup. We also note that, in order to limit the effect framing might have on the declared values, we kept displays used when eliciting preferences with the two methods as similar as possible (see figures 7 and 4). We devote the rest of this section to showing the equivalence between these two elicitation method and the Tables method. In view of the (qualified) equivalence between the BDM and the Auction method, it is enough for us to focus on only one of them.

For definiteness, take the BDM method. In this case the decision maker has to select the (lowest) value he is prepared to accept in order to anticipate receipt of a monetary price, rather than obtaining a larger value $B$ (in our case either 20 or 50, depending on the treatment) with delay. As we saw earlier, a strategy for the subject is simply to state an amount $a$, and under truthful revelation, the subject will declare the amount $a^*$ that makes him indifferent between receiving this amount $a^*$ earlier or the full amount $B$ with delay.

On the other hand, in the case of the Table elicitation method with a single switching point, in practice a decision maker is asked to state the (minimum) value he would be prepared to accept in order to avoid the delay to obtain the full amount $B$. That is, in the Table method a subject is asked to state a switching value $s$, i.e. one such that he prefers to receive any amount equal or greater to $s$ at an earlier date rather than receive $B$ later. In this method, payment conforms to the choice made in the row which is drawn at the end of the experiment. Let $s^*$ be the true switching value, that is the true value that would make the subject indifferent between receiving $s^*$ earlier (option **A**) rather than having to wait longer for the full amount (option **B**). Correspondingly, such a subject would choose option **A** in all those rows such that $a > s^*$, and option **B** in all other rows. Thus we can number all the rows in the table progressively by the $a$ which makes up option **A** in that particular row, identifying the 'switching row' $a^*$ as the one such that the subject would chooses option **A** in all rows with $a > a^*$, and option **B** in all other rows such that $a < a^*$. Truthful declaration implies that if the row $a$ drawn is greater than $s^*$, then (having chosen option **A** in all such rows), the subject will receive the drawn amount $a$ sooner. To the contrary, if the drawn row $a$ is less than $a^*$, the decision maker is going to receive the full amount $B$ with delay. *At* the switching row, being indifferent the decision maker could state either option **A** or option **B**, as they are payoff equivalent. The payoff in case of truthtelling is depicted in figure 12.

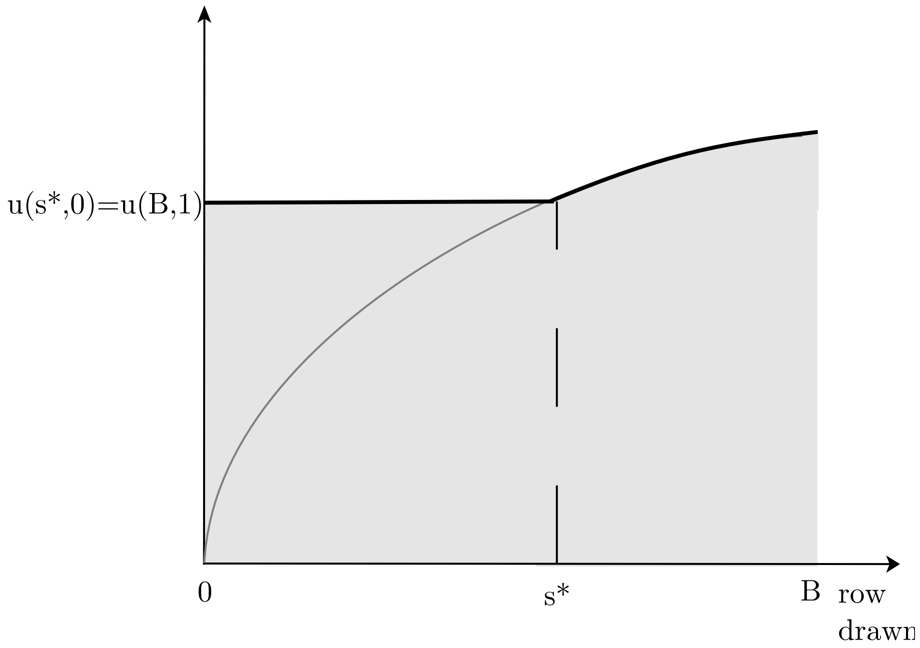


Figure 12: Truthtelling payoff in the Tables method

Based on this, it is easy to construct payoffs in case of deviations, depicted in figure 13.

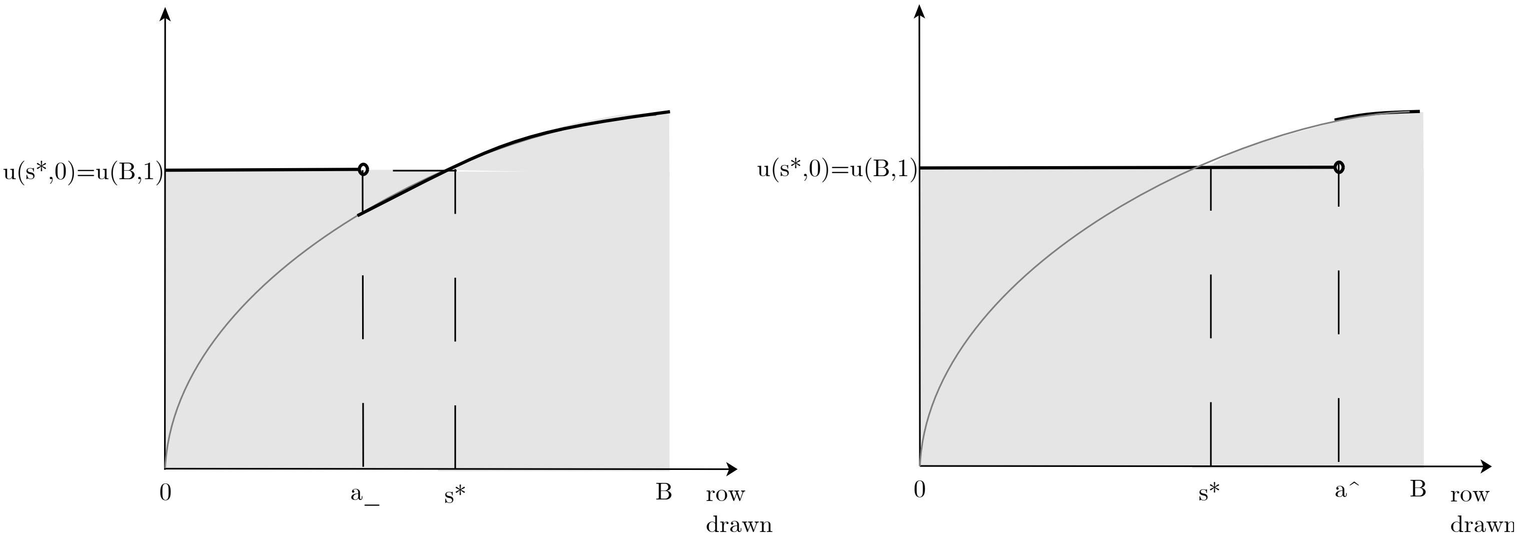


**Figure 13:** Payoffs in case of deviation from truthtelling in the Tables elicitation method

Observe that here the strategic equivalence between the Tables method and the BDM is complete. Of course, for the Tables method, too, truthtelling is a (weakly) dominant strategy. Arguably, though, the Tables method makes the optimality of truthtelling much easier for participants to realise[17].

# 4 Results

The bulk of our analysis revolves around money discount factors. As we explained above, in all elicitation methods subjects declared the amount $a$ that they were prepared to accept in order to avoid a longer wait for a larger sum $B$. Thus, money discount factors are calculated simply by dividing the declared values by the total delayed amount, that is as $\frac{a}{B}100$. In the BDM and Auction methods the unit of measurement was tenths of euros, which is also the inbuilt margin of error in the elicitation of the true value. That is, we can assume that any amount elicited through these methods was within 10 cents of the 'true' value $a^*$, which would lie within the range $[a - 0.10, a]$. Computing the money discount factors as the ratios between the elicited values and the total amounts changes

[17] Indeed, the convergence between 'willingness to pay' and 'willingness to accept' values elicited in Plott and Zeiler [27]was obtained with a BDM mechanism. The implementation of the mechanism, though, is very reminiscent of the multiple price list format: in practice they turn a matching task (the standard BDM mechanism) into a choice task. See especially Plott and Zeiler [28], p. 8 and following.

the range to $\left[\frac{a-0.1}{B}100, \frac{a}{B}100\right]$, which is at most 0.5% wide (in case $B = 20$). On the other hand, for the Tables methods, in view of the reasons we have already explained, we had instead larger increments, and the 'switching row' is a more imprecise indicator of the true switching value than in the case of the other two procedures, indeed five times larger. As a consequence, the range for the money discount factors is in this case $\left[\frac{a-0.5}{B}100, \frac{a}{B}100\right]$ is between $1-2.5\%$, depending on $B$. Since the incentives are for subjects to state the highest possible values that they are prepared to accept in order to avoid waiting, all the money discount factors we compute refer to the right boundary of these ranges. This means that we run the risk of overvaluing the money discount factors elicited with the Tables method more than those elicited with the other two methods. Because of this potential 'over-bias', this strengthens the significance of any evidence that money discount factors elicited in the Tables method are smaller than with the other two methods. Indeed, as we anticipated in the introduction this is precisely what we found.

Recall that, as we discussed in section 3, all three elicitation methods are broadly strategically equivalent: the set of actions available to a subject in one treatment can be mapped into strategies in the other treatment yielding exactly the same payoff. Consequently, each method should yield the same results. In fact, what we find that the methods do differ. We can summarise our results as follows:

1. median money discount factors elicited with the Tables method are smaller than those elicited with the BDM or Auction method; on the contrary, discount factors elicited with the latter two methods do not differ in a statistically significant way;

2. the distributions of discount factors elicited with the Tables method differ from those elicited with the other two procedures. In particular, money discount factors elicited with the Tables method first order stochastically dominate those elicited with either the BDM or the Auction methods.

The rest of this section is devoted to detailing these results.

## 4.1 Differences in central location

Descriptive statistics are summarised in Table 5 and depicted in figure 13.

In the figure, the boxplots are in groups of three, with the leftmost in each group referring to the one month delay horizon, the middle one for the two months delay, and the rightmost one for the four month delay. In each box, the thick line identifies the median money discount factors, the box itself covering the interquartile range. The three time horizons are grouped by treatment, those with the small stakes (€20) in the left part of the figure, and those with the large stake (€50) on the right, with $T\_i$, $A\_i$ and $B\_i$ referring to the Table, Auction and BDM, respectively, with stake $i \in \{€20, €50\}$.

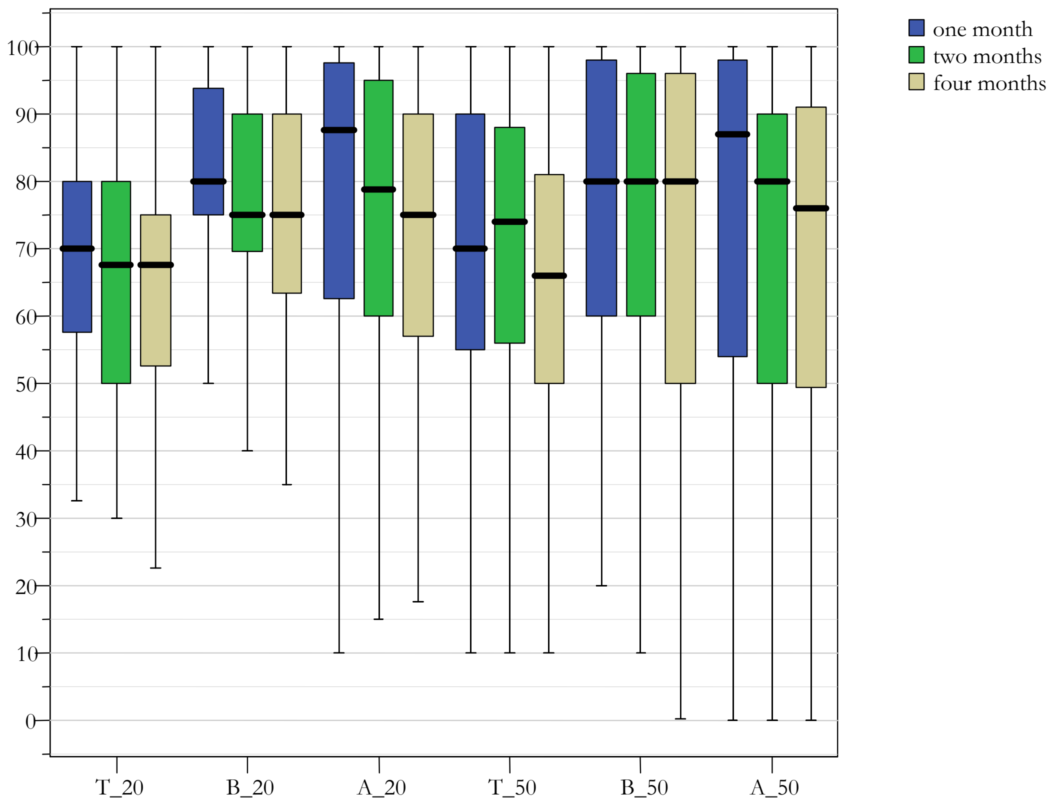


Figure 13: Distribution of money discount factors, all data

Figure 13 makes clear how that *the median money discount rate elicited with the Tables method is always smaller than those elicited with the other methods*, regardless of whether all data or only first observations are considered. Figure 13 refers to all data. However, each participant answered questions relating to three different time horizons. The order in which the questions were presented was randomised, and payoffs were determined only after all questions had been answered, so that we can rule out that the consequences of a previous answer influenced a subsequent answer. At any rate, considering only the first answer is a 'purist' way to ensure that the observations are independent.[18]

[18]This is mostly a robustness check (the first datum is the only unassailably independent piece of data), since there was no learning in our model. However we cannot exclude that subjects might anchor later

| Time horizon | stake | treatment | mean | st. error | median | st. dev. | skewness | count |
|---|---|---|---|---|---|---|---|---|
| **One month** | **20** | **T** | 68.06 | 2.59 | 70 | 20.43 | -0.563 | 62 |
| | **20** | **B** | 77.75 | 2.53 | 80 | 20.05 | -1.170 | 63 |
| | **20** | **A** | 76.88 | 3.44 | 87.5 | 27.06 | -1.476 | 62 |
| | **50** | **T** | 70.12 | 2.94 | 70 | 23.73 | -0.506 | 65 |
| | **50** | **B** | 77.25 | 3.19 | 80 | 25.12 | -1.105 | 62 |
| | **50** | **A** | 71.46 | 4.13 | 87 | 32.75 | -1.078 | 63 |
| **Two months** | **20** | **T** | 64.84 | 2.38 | 67.5 | 18.76 | -0.727 | 62 |
| | **20** | **B** | 75.44 | 2.51 | 75 | 19.93 | -1.036 | 63 |
| | **20** | **A** | 74.62 | 3.15 | 78.75 | 24.80 | -1.217 | 62 |
| | **50** | **T** | 68.26 | 3.00 | 74 | 24.15 | -0.906 | 65 |
| | **50** | **B** | 75.55 | 2.71 | 80 | 21.36 | -0.790 | 62 |
| | **50** | **A** | 69.54 | 3.51 | 80 | 27.89 | -0.839 | 63 |
| **Four months** | **20** | **T** | 62.38 | 2.57 | 67.5 | 20.23 | -0.817 | 62 |
| | **20** | **B** | 73.59 | 2.79 | 75 | 22.12 | -1.278 | 63 |
| | **20** | **A** | 69.68 | 3.14 | 75 | 24.76 | -0.878 | 62 |
| | **50** | **T** | 63.55 | 2.82 | 66 | 22.76 | -0.436 | 65 |
| | **50** | **B** | 71.45 | 3.37 | 80 | 26.54 | -0.696 | 62 |
| | **50** | **A** | 67.85 | 3.74 | 76 | 29.73 | -0.811 | 63 |

Table 5: Descriptive statistics, all data

Statistics referring to these first observations only (first datum) are reported in Table 6 and visualised in Figure 14, and they are in line with those for all data.

The differences in median money discount rates when elicited with the Tables method and with the other methods are statistically significant for the case of small stakes, regardless of whether one uses all data or only the first datum. For large stakes, the differences between the Tables and the BDM method are significant for all time horizons if all data are considered, whereas they are significant only for the longest delay when considering the first datum only. As for the differences between the Tables and the Auction methods, these are statistically significant only for the longest delay, but this holds true regardless of whether the first datum only or all responses are considered in the analysis, as reported in Table 7.

In the table, the column heading "$i$ vs. $j$" refers to a null hypothesis of equality of central location being tested against the alternative hypothesis that method $i$ elicits smaller values than method $j$, where $i, j \in T, B, A$ and obviously $i \neq j$. Dark grey cells

answers to previous answers.

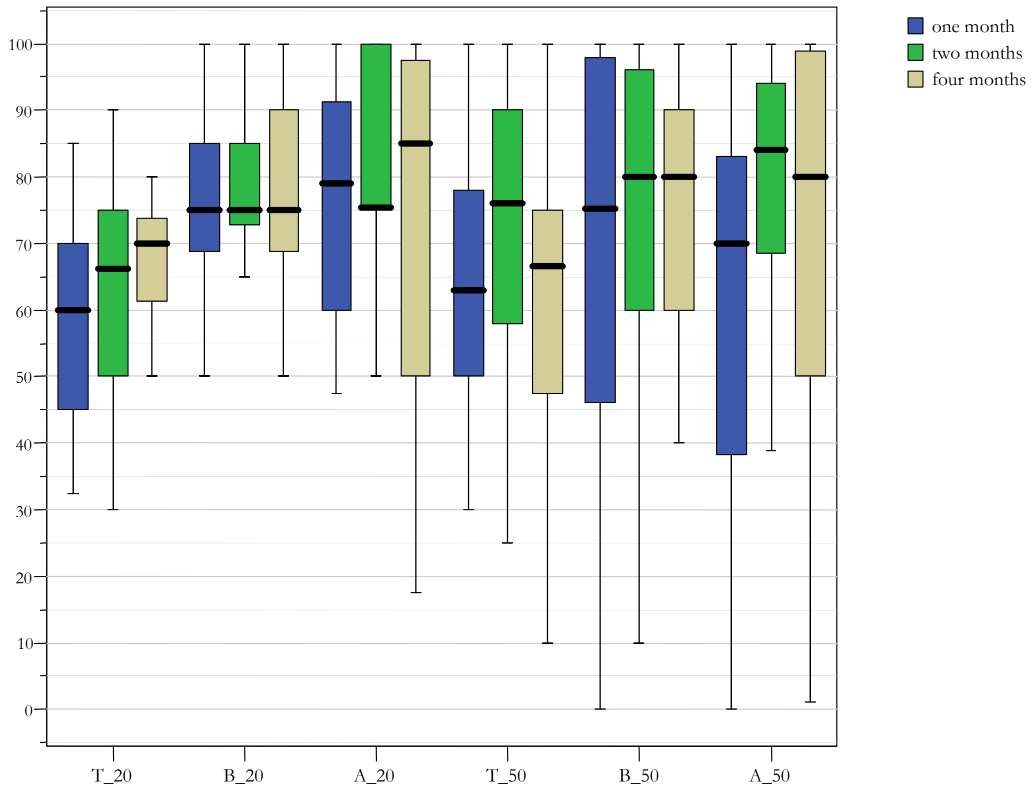


Figure 14: Distribution of money discount factors, first datum only

| Time horizon | stake | treatment | mean | st. error | median | st. dev. | skewness | count |
|---|---|---|---|---|---|---|---|---|
| **One month** | 20 | T | 57.80 | 2.99 | 60 | 14.97 | 0.126 | 25 |
| | 20 | B | 73.92 | 4.35 | 75 | 18.95 | -0.709 | 19 |
| | 20 | A | 69.90 | 5.73 | 79 | 29.79 | -1.223 | 27 |
| | 50 | T | 63.18 | 3.94 | 63 | 18.47 | -0.027 | 22 |
| | 50 | B | 67.88 | 6.40 | 75.2 | 29.33 | -0.683 | 21 |
| | 50 | A | 59.65 | 7.52 | 70 | 32.78 | -0.640 | 19 |
| **Two months** | 20 | T | 62.02 | 3.98 | 66.25 | 20.27 | -0.922 | 26 |
| | 20 | B | 75.89 | 3.44 | 75 | 18.22 | -0.931 | 28 |
| | 20 | A | 82.91 | 4.22 | 75.4 | 15.78 | -0.332 | 14 |
| | 50 | T | 70.26 | 5.29 | 76 | 25.36 | -0.967 | 23 |
| | 50 | B | 74.83 | 4.95 | 80 | 24.27 | -1.148 | 24 |
| | 50 | A | 76.23 | 5.57 | 84 | 24.29 | -1.056 | 19 |
| **Four months** | 20 | T | 67.05 | 2.86 | 70 | 9.47 | -0.384 | 11 |
| | 20 | B | 74.33 | 5.94 | 75 | 23.76 | -1.968 | 16 |
| | 20 | A | 73.43 | 5.99 | 85 | 27.46 | -0.800 | 21 |
| | 50 | T | 60.3 | 5.47 | 66.5 | 24.45 | -0.530 | 20 |
| | 50 | B | 77.88 | 4.12 | 80 | 17.00 | -0.585 | 17 |
| | 50 | A | 71.44 | 5.75 | 80 | 28.75 | -0.806 | 25 |

Table 6: Descriptive statistics, first datum only

refer to the null being rejected at 5%; light grey cells refer to the null being rejected at 10% confidence level.

| **Wilcoxon-Mann-Whitney test (exact p-values)** | | **Small stakes (€20)** | | | **Large stakes (€50)** | | |
|---|---|---|---|---|---|---|---|
| | | **T vs. B** | **T vs. A** | **B vs. A** | **T vs. B** | **T vs. A** | **B vs. A** |
| **All data** | **1 month** | 0.0012 | 0.0009 | 0.2290 | 0.0229 | 0.1162 | 0.2799 |
| | **2 months** | 0.0004 | 0.0005 | 0.3133 | 0.0481 | 0.1980 | 0.2180 |
| | **4 months** | 0.0003 | 0.0144 | 0.2309 | 0.0154 | 0.0513 | 0.3062 |
| **First observation only** | **1 month** | 0.0010 | 0.0030 | 0.4536 | 0.1368 | 0.4003 | 0.2151 |
| | **2 months** | 0.0026 | 0.0003 | 0.1327 | 0.2117 | 0.1913 | 0.3875 |
| | **4 months** | 0.0253 | 0.0787 | 0.3602 | 0.0136 | 0.0357 | 0.4220 |

Table 7: Wilcoxon-Mann-Whitney test

The results above also highlight how our implementation of the multiple price list formats does away with a framing effect that has been observed in various multiple price list studies (e.g. Coller and Williams [8], Harrison, Lau and Williams [13], and Read, Airoldi and Loewe [29]), whereby subjects' switching choices tend to concentrate on rows towards the middle of the table.

For the Tables treatment with large stakes, both top and bottom parts of the table were made up of nine rows each (see figure 1). For the small stakes treatment instead the top part of the table consisted of 9 rows, while the bottom part consisted of four rows. Yet, the median choices do not correspond to the middle rows in both parts. For the small stakes treatment, the median choice was €14 for the one month horizon, and €13.5 for both the two month and the four month horizon. That is, counting from the top of the table, the median subject picked the third row in the top part of the table, and then either the first or second row in the bottom part of the table, depending on whether the median is €14 or €13.50. For the case of large stakes, too, subjects do not seem attracted to the middle of the table, as median choices for one month horizon was €35, corresponding to the third and first line in the top and bottom part of the table, respectively; for two month horizon was €37, corresponding to the second (top) and seventh (bottom) line; and for four month horizon was €30, that is third and last lines in the top and bottom parts of the table, respectively.

Similar lack of "mid-page attraction" is observed when focussing on modal choices.[19] Thus, arguably because of the division of each table in two parts, our implementation of the multiple price list seems immune to such type of framing effects.

## 4.2 Other differences in distribution

In addition to the differences in central tendency, Tables 5 and 6, and Figures 15-17 also evidence marked differences in the *distributions* of the money discount factors elicited with the three methods. In these figures, the top two rows of panels refer to all the data, whereas the panels in the two bottom rows refer to money discount factors that were elicited as first question to the participant (i.e. no order effects).

When, as evidenced in Figures 15-17, distributions may differ in scale, as well as in location, the Kolmogorov-Smirnov test is more efficient than the Wilcoxon-Mann-Whitney test in detecting differences in distribution. Thus in Table 8 we report the Kolmogorov Smirnov test results. The column heading "$i$ vs. $j$" indicates that the comparison is

[19] For the case of small stakes, modal choices are €15, €16 and €14 for one, two and four month horizon, respectively, corresponding to: second row chosen in the top part of the table, and third row chosen in the bottom part (€15); second in the top part and first in the bottom (€16); and third row in the top and first row in the bottom (€14). For the case of large stakes, modal choices are €45, €40 and €34 for one, two and four month horizon, respectively, corresponding to: first rows in both top and bottom (€45), second row in the top and first row in the bottom (€40), and third row in both top and bottom (€34).

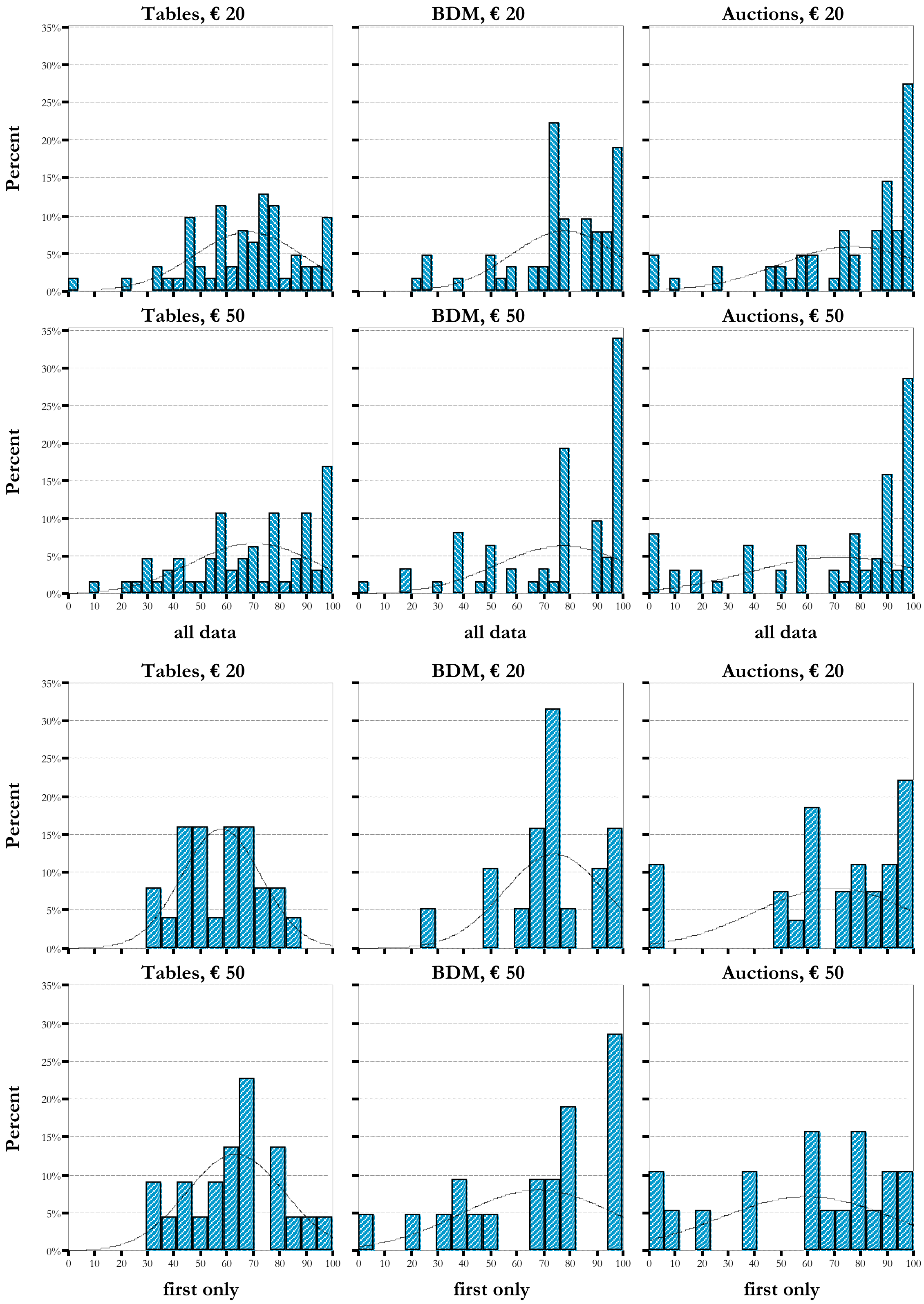


Figure 15: Frequency distributions of money discount factors, *one month* horizon

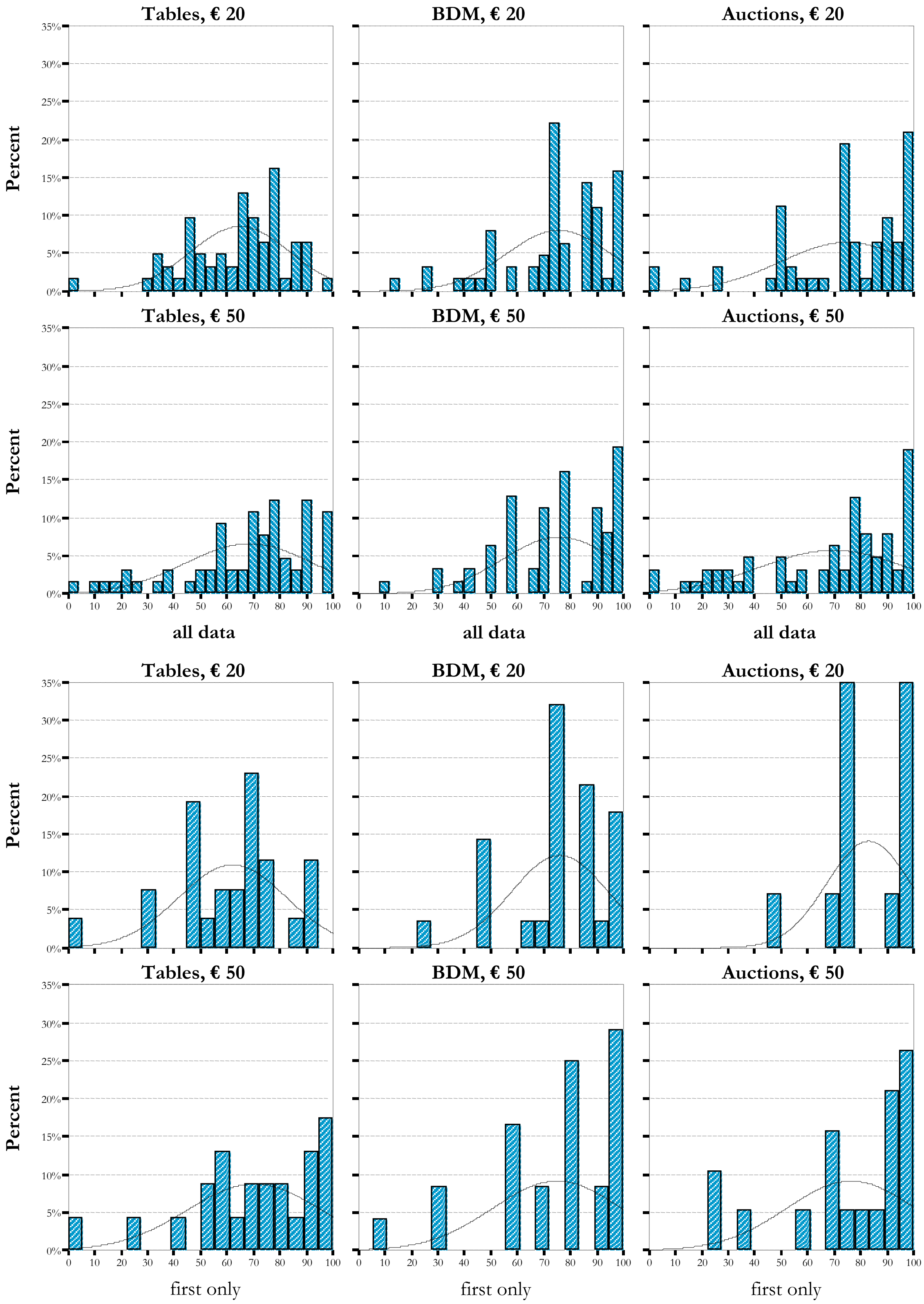


Figure 16: Frequency distributions of money discount factors, *two month* horizon

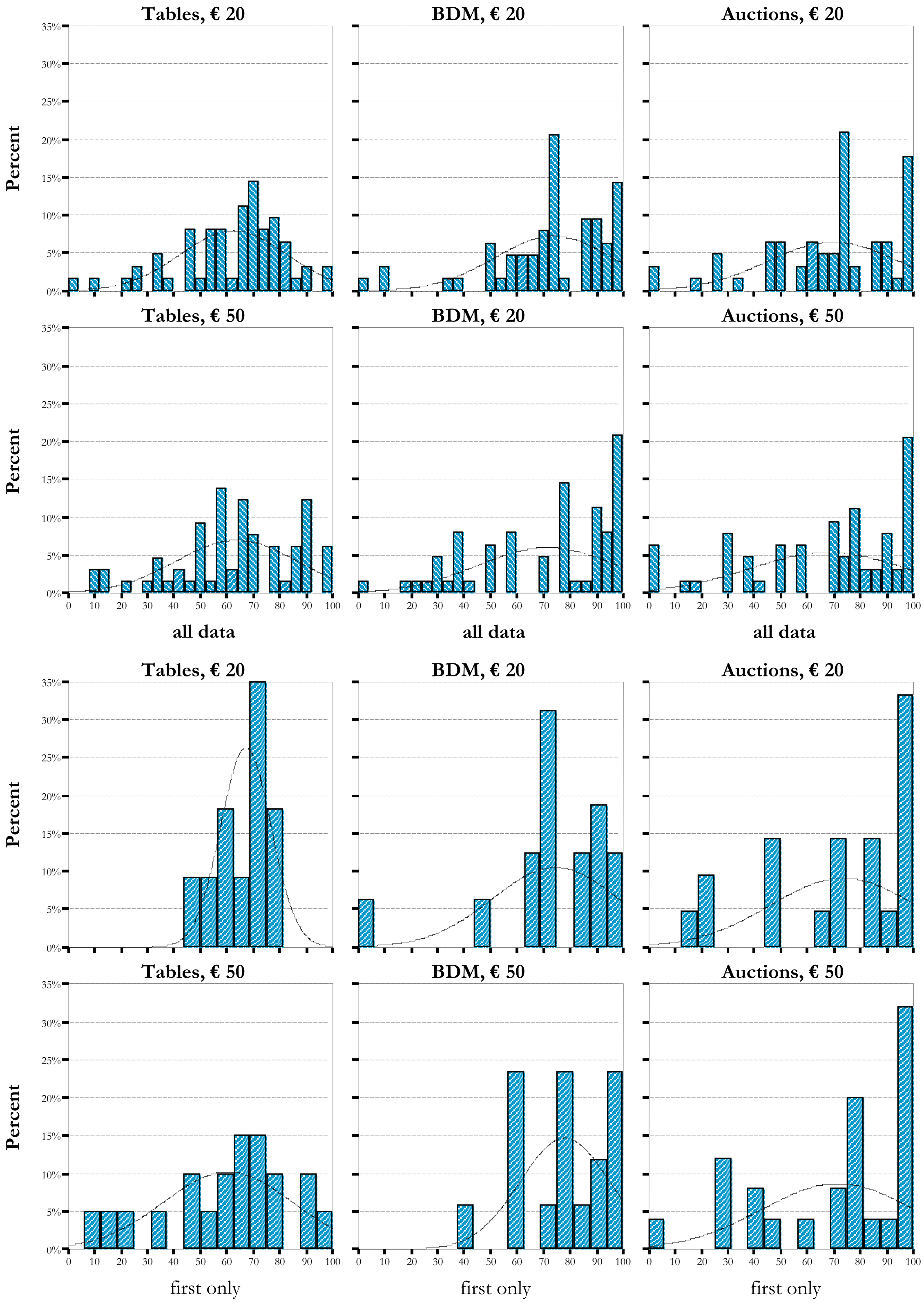


Figure 17: Frequency distributions of money discount factors, *four month* horizon

between the cumulative distribution $F_i$ of money discount factors elicited with method $i$ and the cumulative distribution $F_j$ of money discount rates elicited with method $j$, where $i, j \in \{T, B, A\}$ and obviously $i \neq j$. On the third line, $|\Delta|$ is a shorthand for the two sided test of equality between distribution functions, while $i > j$ is a shorthand for the one sided test of equality of the two distribution functions against the alternative that $F_i$ first order stochastically dominates $F_j$. Dark grey cells refers to the null being rejected at 5%, while lighter gray cells refer to the null being rejected at 10% confidence level.

Inspection of Table 8 confirms what emerges quite clearly at first sight from the empirical cumulative distribution functions: the cumulative distribution of money discount rates elicited with the Tables method first order stochastic dominates those elicited with other methods, especially for the case of low stakes.

For instance, figures 18-21 refer to the case of the shortest time horizon, with small and large stakes, respectively.[20] In all of these pictures, the black, dark grey and light grey lines refer to the Auction, BDM, and Tables elicitation methods, respectively.

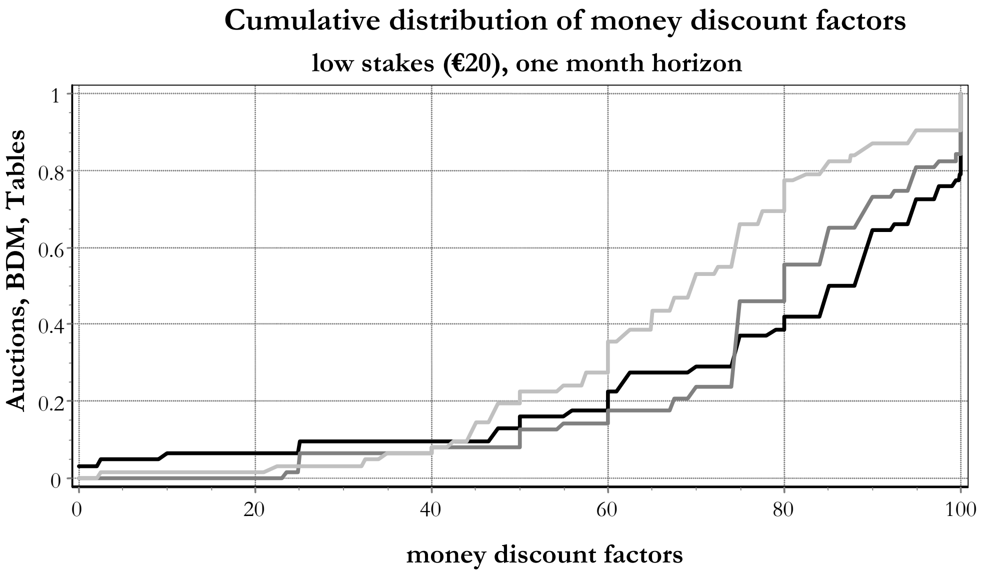


Figure 18: Distributions based on all observations

In the case of small stakes these differences are statistically significant, regardless of whether we consider the whole sample or only the first, order independent datum. For

[20] Please see the appendix for figures of the cumulative distributions referring to different combinations of time horizon, stake and whether all observations or only the first datum has been used.

| | | €20 | | | | | | | | | €50 | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | **T vs. B** | | | **T vs. A** | | | **B vs. A** | | | **T vs. B** | | | **T vs. A** | | | **B vs. A** | | |
| | **H** | $\lvert\Delta\rvert$ | T>B | B>T | $\lvert\Delta\rvert$ | T>A | A>T | $\lvert\Delta\rvert$ | B>A | A>B | $\lvert\Delta\rvert$ | T>B | B>T | $\lvert\Delta\rvert$ | T>A | A>T | $\lvert\Delta\rvert$ | B>A | A>B |
| **all** | **1** | 0.002 | 0.001 | 0.848 | 0.000 | 0.000 | 0.695 | 0.287 | 0.144 | 0.394 | 0.024 | 0.012 | 0.848 | 0.092 | 0.047 | 0.277 | 0.373 | 0.585 | 0.187 |
| | **2** | 0.000 | 0.000 | 0.8589 | 0.002 | 0.001 | 0.711 | 0.712 | 0.385 | 0.592 | 0.093 | 0.048 | 1.000 | 0.214 | 0.108 | 0.423 | 0.267 | 0.885 | 0.132 |
| | **4** | 0.001 | 0.001 | 0.947 | 0.017 | 0.009 | 0.887 | 0.739 | 0.722 | 0.391 | 0.010 | 0.005 | 0.720 | 0.014 | 0.007 | 0.535 | 0.610 | 0.476 | 0.321 |
| **1st only** | **1** | 0.007 | 0.003 | 0.880 | 0.015 | 0.008 | 0.614 | 0.593 | 0.311 | 0.324 | 0.211 | 0.108 | 0.668 | 0.464 | 0.225 | 0.263 | 0.766 | 0.907 | 0.420 |
| | **2** | 0.000 | 0.000 | 1.000 | 0.001 | 0.001 | 1.000 | 0.274 | 0.134 | 0.894 | 0.354 | 0.177 | 0.829 | 0.691 | 0.349 | 0.772 | 0.842 | 0.467 | 0.821 |
| | **4** | 0.069 | 0.032 | 0.895 | 0.0220 | 0.011 | 0.352 | 0.606 | 0.305 | 0.411 | 0.049 | 0.029 | 1.000 | 0.022 | 0.012 | 0.910 | 0.374 | 0.294 | 0.183 |

Table 8: Kolmogorov-Smirnov test. The table reports exact p-values. The column H differentiates horizons (one month, two months and four months).

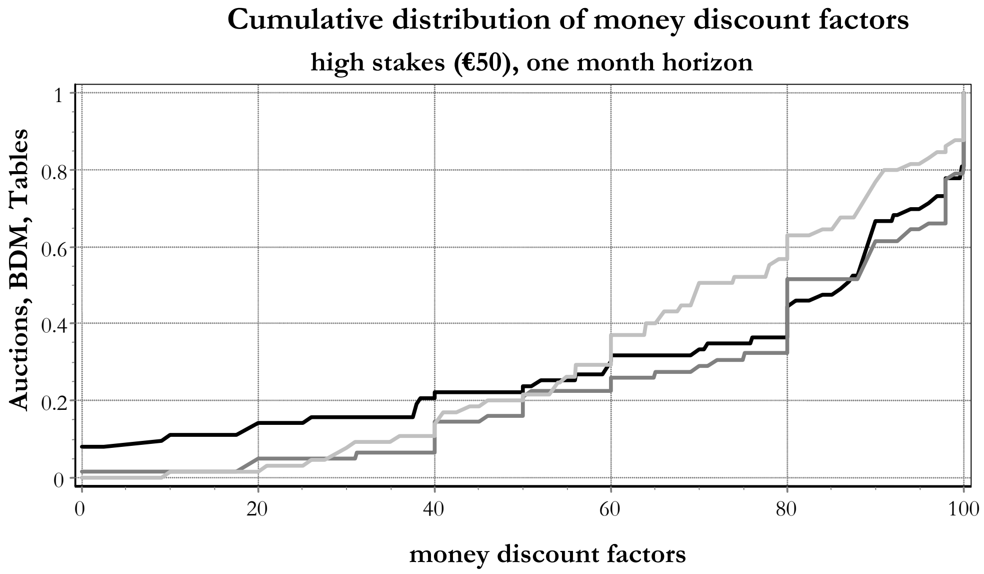


Figure 19: Distributions based on all observations

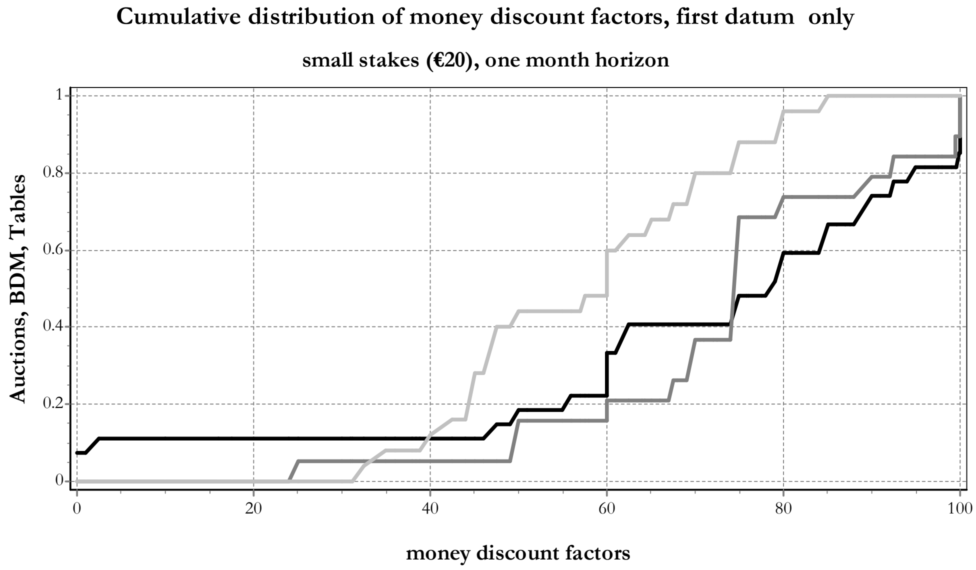


Figure 20: Distributions based on the first response only

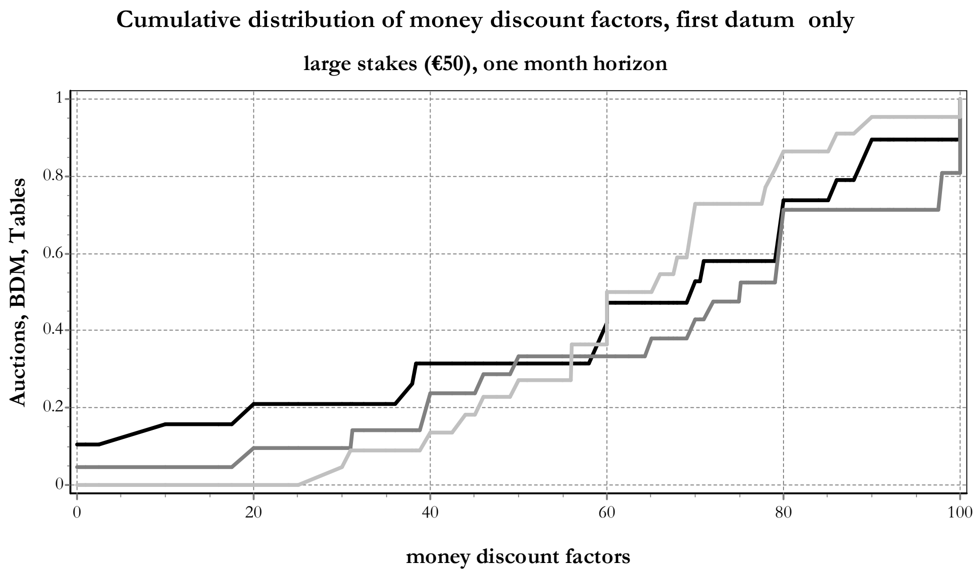


Figure 21: Distributions based on the first response only

larger stakes the effect is still significant for the whole sample, less so for first data only.

These results suggest that changing the stakes does not have an effect. As perhaps is more evident from figures Figure 13 and 14, for the Tables treatment the effect of an increase in the stake is to increase the median money discount factor only in the case of the intermediate delay horizon, where the increase is even more significant if only the first datum (i.e. no order effect) is considered.[21] For the other two elicitation methods too the effect is either absent or weak:[22] in other words, even more than doubling the stake produces no appreciable magnitude effect.

As a last consideration, our implementation of the Auction and BDM elicitation methods relies on instructions and layout as close as possible to one another. While for these we observed distributions of money discount factors which are more skewed towards high values, with the Tables elicitation method money discount factors are consistently lower. If one were to argue that this is due to the subjects not understanding the BDM elicitation mechanism, one should similarly argue that the Auction mechanism, too, is difficult for subjects to understand, as both mechanisms produce similar distributions (across stake

[21] See Table 1 in the appendix.

[22] For the Auction method the increase in the money discount rates is significant at 10% only for the intermediate horizon when all data are considered, while for the BDM none is significant, see tables 1-3 in the appendix.

magnitudes, and across time horizons). On the other hand, it can hardly be argued that it is the Auction experiment implementation that was hard for subjects to understand, as exactly the same design was used for small stakes auctions. But using the percentage of 'zero' values as a proxy for participants having not understood the problem, the proportion of subjects with a zero discount factor in small stakes auctions is in line with those with all other methods. In fact, we cannot conclude that those subjects with a zero discount factor did not understand the instructions, as we have a positive percentage of people with such impossibly low discount rates also when the Tables method is used, which is the most straightforward to understand.

# 5 Estimation

In order to be able to take into account both distribution and central tendency, and to be able to control for the potential effects of the socio-economic data on participants that we can use as controls, in this section we test a standard exponential utility discounting model using a Tobit random effect estimator regression. The model tested takes the form:

$$u\left(m_t\right) = u\left(m_{t+\tau}\right) e^{-\rho\tau}$$

where:

$m_{t+\tau}$ is the amount of money at time $t+\tau$

$m_t$ is the amount of money at time $t$

$\rho$ is the subjective discount rate

$u\left(\cdot\right)$ is the utility function of money.

Assuming risk neutrality, the above can be rewritten as:

$$m_t = m_{t+\tau} e^{-\rho\tau}$$

For the econometric analysis we assume that the subjective discount *rate* $\rho$ is a linear function of the elicitation method used, of some socio-demographic characteristics of the subject, of the temporal horizon considered in the experiment and of the amount of the endowment assigned to the participants.

In the first regression (model 1) we estimate the following model for $\rho$:

$$\rho = \alpha + \beta x + \gamma z$$

given choice data $m_t$ and $m_{t+\tau}$, $\rho$ is equal to $\left(\ln m_{t+\tau} - \ln m_t\right)/\tau$; $x$ is the vector of explanatory variables, while $z$ is the vector of control variables, namely, for $x$:

- auction.treatm = dummy variable which identifies the auction treatment[23];
- bdm.treatm = dummy variable which identifies the BDM treatment;
- amount = the money endowment used in the treatments ($m_{t+\tau}$);
- time = time delay ($\tau$);

while for $z$:

- student = is a dummy which identifies whether the participant is a student or not;
- male = is a dummy which identifies whether the participant is a male
- age = is the age in years of the participants;
- account = is a dummy which identifies whether the participant has a current account or not;
- card = is a dummy which identifies whether the participant has a credit card or not;
- house = is a dummy which identifies whether the family of the participant own or not its family house;
- income.[1-9] = dummy variables that controls for different levels of family income

The use of a Tobit regression is appropriate here given that our dependent variable, due to our experimental design, is censored at the value of zero. Moreover we included a random effect to control for idiosyncratic effects in the repeated choices framework used in the experiment. The results are reported in table 9

Looking at control variables first, only "house" and "age" are statistically significant at 5%. For the former, this means that owning the family house decreases the subjective discount rate, i.e. the subject is more patient. If one were to consider ownership of the family house as a proxy for the economic status of the subject, the significance of this coefficient points towards an interaction between wealth and intertemporal utility from money. This of course is compatible with the standard assumption of concave utility, and goes contrary to our assumption of linearity.

[23] In the regression we use the Tables method as baseline.

| **Variables** | **Coefficient** | **Std. Err.** | pvalue |
|---|---|---|---|
| bdm.treatm | -.1039614 | .042685 | 0.015 |
| auction.treatm | -.0983996 | .0424037 | 0.020 |
| amount | .000286 | .0011395 | 0.802 |
| Time | -.0763695 | .0064374 | 0.000 |
| Male | -.0602482 | .0346045 | 0.082 |
| Age | -.0147955 | .0072182 | 0.040 |
| House | -.0999612 | .0352782 | 0.005 |
| student | .0571312 | .0769332 | 0.458 |
| Card | .0056408 | .0470839 | 0.905 |
| account | -.0112869 | .0429361 | 0.793 |
| income.2 | .0523776 | .0524279 | 0.318 |
| income.3 | -.0754744 | .0991247 | 0.446 |
| income.4 | .049895 | .0824087 | 0.545 |
| income.5 | -.1360989 | .1120139 | 0.224 |
| income.6 | .0221209 | .1707654 | 0.897 |
| income.7 | .1878956 | .2092934 | 0.369 |
| income.8 | -.0359702 | .1510316 | 0.812 |
| income.9 | -.1007106 | .3497364 | 0.773 |
| cons | .7882806 | .1922619 | 0.000 |
| $\sigma_u$ | .2559586 | .0140142 | 0.000 |
| $\sigma_\varepsilon$ | .2367101 | .0071413 | 0.000 |
| $\rho$ | .5390104 | .0325283 | |

Number of obs = 931 [Left Censored = 113]
Log likelihood = -275.60062; Wald$\chi^2$ (18) = 172.16; p-value = 0.0000

Table 9: Random Effect Tobit Regression (model 1, no interactions)

As for “age", it is negatively related to subjective discount rate, pointing towards an increase of patience with age. Further, note that professional status (student versus non-student) does matter in determining the subjective discount rate, while sex is significant only at 10% level, indicating that males are more impatient than female participants.

Turning next to the explanatory variables, there is a clear treatment effect: the baseline elicitation treatment, i.e. tables, seems to induce a higher subjective discount rate as compared to that elicited by either BDM or auction, confirming the results already noted in the previous sections. Similarly, the elicited discount rates obtained by using BDM or auction are not significantly different ($\chi^2(1) = 0.02$; $Prob > \chi^2 = 0.8965$). Time delays too have a significant effect on the subjective discount rate, as an increase in the time delay, other things being equal, reduces the elicited discount rate: patience increases with time delay.

In order to test whether there were any significant interactions between our explanatory variables, we run a second regression (model 2) using the same set of control variables and the same set of explanatory variables adding to this second set the following variables:

- amount.bdm = encoded as “amount" (money endowment) times the “bdm.treatm" dummy, in order to capture a potential differential effect of the size of the stake across the BDM and the baseline treatment (tables);

- amount.auction = encoded as “amount" times the “auction.treatm" dummy, similar to “amount.bdm" above but comparing the Auction elicitation method to the baseline treatment;

- time.bdm = encoded as “time" (i.e. time delay) times the “bdm.treatm" dummy, in order to capture a potential differential effect generated of the time delay across the BDM and baseline treatments;

- time.auction = encoded as “time" times the “auction.treatm" dummy, similar to “time.bdm" above, but comparing the Auction elicitation method to the baseline treatment.

The results from the second regression are reported in table 10.

Inspection of table 10 reveals that in the mains, introducing the interactions makes any treatment effect lose significance (albeit "auction.treat" is significant at 10%), whereas the ‘time’ variable is still statistically different from zero. Moreover, of the interaction terms the only one that is significant at 10% is “time.bdm" (p-value 0.053). The implication is means that the effect of the different eliciting procedures is not constant as the time delay

| **Variables** | **Coefficient** | **Std. Err.** | pvalue |
|---|---|---|---|
| bdm.treatm | -.2088752 | .1114233 | 0.061 |
| auction.treatm | -.1699044 | .1105664 | 0.124 |
| amount | -.000154 | .0018731 | 0.934 |
| time | -.0938403 | .0105664 | 0.000 |
| male | -.0591409 | .0346148 | 0.088 |
| age | -.0148761 | .0072199 | 0.039 |
| house | -.0995384 | .0352275 | 0.005 |
| student | .0570137 | .0772268 | 0.460 |
| card | .0046395 | .0470929 | 0.922 |
| account | -.0120373 | .0429339 | 0.779 |
| income.2 | .0536162 | .052676 | 0.309 |
| income.3 | -.0768101 | .0990992 | 0.438 |
| income.4 | .0506692 | .0827245 | 0.540 |
| income.5 | -.1380663 | .1122902 | 0.219 |
| income.6 | .0212812 | .171007 | 0.901 |
| income.7 | .1888296 | .209301 | 0.367 |
| income.8 | -.0334573 | .1504268 | 0.824 |
| income.9 | -.0987755 | .3543114 | 0.780 |
| amount.bdm | .0009834 | .0027278 | 0.718 |
| amount.auction | .0003567 | .0027312 | 0.896 |
| time.bdm | .0299807 | .0154969 | 0.053 |
| time.auction | .0251368 | .0155866 | 0.107 |
| cons | .8465082 | .2021166 | 0.000 |
| $\sigma_u$ | .2562618 | .0140389 | 0.000 |
| $\sigma_\varepsilon$ | .235753 | .0071189 | 0.000 |
| $\rho$ | .541611 | .0325441 | |
| Number of obs = 931 [Left Censored = 113] | | | |
| Log likelihood = -273.31857; Wald$\chi^2$ (22) = 178.20; p-value = 0.0000 | | | |

Table 10: Random Effect Tobit Regression (model 2, allowing for interactions)

is varied. Indeed, one might argue that the already observed reduction in the elicited discount rate produced by an increase in the time delay is weaker in the BDM treatment than in either in the auction and in the tables treatments. In other words, participants' patience raises by increasing the time delay for all the elicitation procedures but in the case of BDM the rate of increase is slower.

We conclude by stressing once again that our objective here was to assess the impact of elicitation method on elicited value. For this reason the failure of risk neutrality[24], suggested by the above results, is not an issue for us (unless one has good reason to expect that risk aversion would affect one elicitation institution more than another). In this paper we addresses the issue of reliable elicitation, as a step that *precedes*, and does not mean to substitute for, correct estimation, which would have to take into account a wealth of additional issues not tackled here.

# 6 Concluding remarks

The general conclusion we draw from the experimental results is that in 'competitive' situations (either against Nature, as in the BDM mechanism, or against other human players, as in an auction), subjects behave differently than when compiling a table, although

[24] Experimental subjects might be risk averse, and because of this value less outcomes available in an uncertain future. Once this potential confound is taken into account, Anderson et al. [3] show in their pathbreaking contribution that the implausibly high previous estimates of discount factors fall substantially once both risk and time preferences are elicited (in their study the point estimate of the yearly discount factor is roughly 25%, falling falls six-fold to about 4% once risk aversion is accounted for) – However, recently Cubitt and Read [9] have highlited a connected issue, namely that all choice experiments involving questions about money-date pairs can be used only to reveal discount factors for *money*. Nevertheless, it is often implicitly assumed that the discount factor for consumption can also be assessed (which is what is required to estimate utility functions a la Andersen et al [3] above). The problem with this interpretation is that in turn it requires that the money offered in the experiment is consumed immediately, and not e.g. saved or invested (note that in some of these experiments the amounts paid are very substantial, in the order of several hundreds of dollars). This assumption does have implications: Cubitt and Read [9] study this problem in detail, and highlight that the choice between two money-date pairs in the presence of capital markets is not really the choice between two money bundles available at different points in time, rather it is the choice between two whole consumption frontiers, including all the tradeoffs between each amount of money and other goods at each date. Without being too technical, it should be intuitively clear that this fact greatly reduces the possibility of inference about discount factors for consumption.

decision-theoretically all situations are equivalent[25]. Perhaps contrary to expectations, subjects are more impatient in non-competitive situations. However, these results can also be couched in the context of differences between choice and matching tasks. As discussed in the introduction, such differences have been uncovered in various domains[26]: possible explanations go from emotional distress[27] to the fact that, when attributes of the objects being evaluated are clearly recognizable, choice tasks attribute more weight to the more important attributes that do matching task (prominence effect)[28]. Note, though, that these are all within subject designs, i.e. the same subject is confronted with both choice and matching tasks. Once the connection between the two is made less clear, the choice-matching discrepancy should disappear, as shown in e.g. Fischer et al [10]. Thus with a between subject design like the one we have employed, on should expect to observe no significant differences across elicitation method (provided, of course, that the subject in each treatment have been drawn form the same subject pool). Moreover, our finding is in the context of incentive compatible, real reward choices , not hypothetical ones. Yet we do find these differences, and the next question to address is of course why they occur[29]. Alas, with our current design we are unable to assess the relative validity of competing explanations for the choice-matching differences that we observe.

Yet, we do provide an exploratory study were the impact of elicitation institution on elicited value is assessed in as rigorous as possible a setting. Since the estimation of discount factors depends on the reliability of the time preference elicitation method, we hope that our contribution will open up further lines of research in the investigation of the relative merits of elicitation techniques other than the (so far most widely used) multiple price list format.

[25] Interestingly, for the BDM procedure, Ariely et al. [1] find differences in elicited values when varying the shape of the distribution from which the random values are drawn.

[26] See e.g. the seminal paper by Tversky, Sattath and Slovic [33].

[27] See Luce, Payne and Bettman [19].

[28] See e.g. Fischer et al. [10].

[29] An additional issue is whether these differences disappear with repetition. Plott [26] put forward the so called "discovered preference hypothesis", according to which as subjects in an unfamiliar experimental setting get more and more confortable with the setup, the choice-matching discrepancies disappear. See e.g. Braga and Starmer [7] for the discussion of this and related issues.

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