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ABSTRACT

Framing Effects and Impatience: Evidence from a Large Scale Experiment*

We confront a representative sample of one 1,102 Dutch individuals with a series of incentivized investment decisions and also elicit their time preferences. There are two treatments that differ in the frequency at which individuals decide about the invested amount. The low frequency treatment stimulates decision makers to frame a sequence of risky decisions broadly rather than narrowly. We find that the framing effect is significantly larger for impatient than for patient individuals. This result is robust to controlling for various economic and demographic variables and for cognitive ability.

JEL Classification: C93, D03, D81

Keywords: framing, choice under risk, time preference, experiment

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1 Introduction

Minor variations in the framing of a choice problem are often found to have significant effects on people's decisions.[1] In this paper we investigate which demographic or socio-economic characteristics co-vary with such a framing effect and address questions such as: Are women more susceptible to framing effects than men? Does the sensitivity to frames fade with age? Are framing effects related to education? We are particularly interested in the relationship between impatience and the susceptibility to framing effects. Heavy discounting is often associated with problematic behaviors such as undersaving, loan defaulting, and an unhealthy life style.[2] Therefore, it is important to know if relatively mild policy interventions ('nudges') such a framing and changes in choice architecture are effective for impatient individuals, since these are the typical target group of potential interventions (Thaler and Sunstein 2008).

The CentERpanel, hosted by Tilburg University, provides a unique opportunity to run economic experiments on a broadly representative sample of the adult population. It also enables us to measure people's time preferences in an incentivized way. At the same time, a wealth of background information of the individual panel members is available. This allows us to investigate whether treatment effects are related to socio-demographic background variables such as gender, age, occupational status, education, and income.

The specific framing effect that we investigate stimulates decision makers to evaluate a sequence of risky decisions in combination rather than in isolation, and, thus, not to fall prey to *myopic loss aversion*. The design is based on Gneezy and Potters (1997). Two groups of participants are asked to make a sequence of three risky investment decisions. Participants in the first (high frequency) group can change their investment level from one decision to the next and are supplied with feedback about the outcome after each decision. Participants in the second (low frequency) group are restricted to choose the same investment level for all three rounds before the first lottery is played, and they receive feedback about the outcomes only at the end of the third round. Gneezy and Potters (1997) find that participants in the second group invest higher amounts

[1] Prominent examples include Madrian and Shea (2001) who illustrate that the default option has a major effect on the decision to enroll into a retirement savings plan, Ashraf, Karlan, and Yin (2006) who report that savings increase substantially when people are offered a commitment device, Brown et al. (2008) who find that the decision to annuitize retirement savings is affected by whether the problem is framed in consumption terms or in investment terms, Bertrand et al. (2010) who show that the take-up of loan offers is higher when the offer contains a single rather than multiple offers.

[2] See, e.g., Eckel, Johnson, and Montmarquette (2005), Chesson et al. (2006), Khwaja, Sloan, and Salm (2006), Chabris et al. (2008), Meier and Sprenger (2011), and Sutter et al. (2010).

on average than participants in the first group.[3] The reason is that, when induced to take a decision once-and-for-all (low frequency), individuals are stimulated to evaluate the risks in combination, and this pooling of the risks renders them more attractive.[4]

Our main result is that the treatment effect (high versus low frequency) on investment levels is significantly larger within the group of high discounters than within the group of low discounters. Impatient individuals are more sensitive to the framing of the decision problem than patient ones. The difference in the effect between patient and impatient individuals is sizeable: Being in the low-frequency treatment results in a 4 percentage point increase in the invested amount for patient individuals and a 12 percentage point increase for impatient individuals. This result is robust to controlling for various economic and sociodemographic variables. Moreover, almost none of these background variables co-varies significantly with the treatment effect. For example, the treatment is equally effective for men and women, for younger and older people, and for individuals with a high and a low level of education. Interestingly, we do not find that impatient individuals generally invest less than patient individuals, which is somewhat contrary to studies who find a positive correlation between risk aversion and discount rates (Anderhub et al., 2001, Burks et al., 2009, Eckel et a., 2005). The evidence on the relation between time and risk preferences is equivocal though (e.g., Booij and van Praag, 2009), which may be due to the fact that the preference elicitation tasks vary substantialy across studies.

Our paper contributes to an emerging literature relating behavioral preferences and anomalies - such as low-stakes risk aversion, short-term discounting, narrow framing, and social preferences - to each other and to covariates such as education, cognitive abilities, age, gender, income and wealth (Benjamin, Brown, and Shapiro, 2006, Burks et al., 2009, Dohmen et al., 2010, Frederick, 2005, Rabin and Weiszacker, 2009). This literature is predominantly empirical in nature and not based on a firmly grounded theoretical hypotheses. This may change, however, as soon as empirical patterns turn out to be robust.

Few studies have examined whether framing and treatment effects are related to background variables. Ashraf, Karlin, and Yin (2006) report that in their field experiment women with hyper-

[3] For similar results see Thaler et al. (1997), Benartzi and Thaler (1999), Read, Loewenstein, and Rabin (1999), Barron and Erev (2003), Gneezy, Kapteyn, and Potters (2003), Langer and Weber (2003), Haigh and List (2005), Bellemare et al. (2005), Sutter (2007), Hopfensitz and Wranik (2008), and Fellner and Sutter (2009).

[4] Similar manipulations have been applied outside the lab. Gneezy, Kapteyn and Potters (2003) report how an Israeli bank reduced the information that was released about investment performance in order to induce its clients to take a less myopic perspective. Kliger and Levit (2009) show how the Tel Aviv Stock Exchange affected traders' evaluation period by shifting the trading frequency of certain securities from daily to weekly. See Beshears et al. (2011) for an aggregation manipulation in a field experiment that did not affect participants' portfolio risk taking.

bolic, time-inconsistent preferences were more likely to pick up a commitment savings product than were men or women with time-consistent preferences. Guiso (2009) manipulates the accessibility of background income risk and finds that higher accessibility generally increases risk tolerance, particularly for those individuals who say that they base their decisions mostly on reasoning as compared to those who say they mostly rely on intuition. Interestingly, Steul (2006) reports more or less the opposite result for the impact of the framing of investment portfolios (aggregated vs. segregated) on risk taking. Subjects who said they engaged in explicit calculations of expected values were less affected by the framing manipulation than subjects who said they did not. We contribute to this literature by relating framing effects to time preference.

2 Experimental design and data collection

2.1 Experimental design

Our experiment had two parts. The first part was a risky investment decision task and the second part consisted of the elicitation of time preferences.

Part 1: Investment decision. In this part we employed the basic design of Gneezy and Potters (1997) involving three rounds of an investment task. In each round, subjects were endowed with €2 and had to decide how much of this amount they wanted to invest in a lottery in which there was a 2/3 chance to lose the invested amount and a 1/3 chance to win 2.5 times the invested amount. Hence, expected earnings in round t when investing an amount x_t (with $0 \leq x_t \leq 2$ and $t = 1, 2, 3$) were equal to $2 - (2/3)x_t + (1/3)2.5x_t = 2 + (1/6)x_t \geq 2$. The lotteries in each round were independent. Moreover, subjects could not invest money accumulated in previous rounds, that is, the maximum investment in each round was €2.

The central feature of the design was that were two treatments. In the high-frequency treatment (referred to as "HIGH"), subjects made the investment decisions round by round. At the beginning of round 1 they had to choose the amount x_1 of their endowment of €2 to invest in the lottery. Then they were informed of the result of the lottery in round 1. Thereafter, subjects decided on the part x_2 of their new endowment of €2 they wished to invest in round 2. Again, they were informed of the outcome of the round-2 lottery, and were finally asked to make their decision x_3 for round 3, with subsequent feedback about the outcome. In the low-frequency treatment (referred to as "LOW"), subjects made just one decision for all three rounds, which imposes the constraint $x_1 = x_2 = x_3$. Subjects in this treatment only received feedback about the combined result of

rounds 1, 2, and 3 at the end of the third round. That is, they were only informed whether they had won in no, one, two or all three rounds, but could not assign a gain or loss to any particular round.[5] In this part of the experiment all subjects were paid according to their decisions.

Part 2: Elicitation of time preferences. For this part of the experiment we followed Coller and Williams (1999) and confronted subjects with a set of 20 payoff alternatives, which we also list in the rows of Table 1. In principle, in each of the 20 rows subjects had to decide between option A and Option B. Option A always paid €300 in one month from the day of the experiment. Option B paid the amount of €300 + €X after seven months from the day of the experiment, where X varied from €3.80 to €79.70 (corresponding to annual interest rates varying from 2.5% to 50% of return on the amount of €300, compounded quarterly).[6] However, instead of asking the subjects to make a choice for each decision listed in the rows of Table 1, a subject's task was to choose the *minimum* X which would make her prefer Option B (performed by moving a slider on a row in Table 1 that represented a "switch point" from preferring Option A to preferring Option B). So, for instance, if a subject's preference was such that it would take an extra payment of at least €25 to wait seven months from the day of the experiment instead of receiving €300 in one month from the day of the experiment, this subject would select the row of decision alternative 7 in Table 1. By asking subjects to indicate the minimum amount of X to make it worth waiting for seven months, we forced subjects to switch from Option A to Option B at most once. In particular, we explicitly stated that there are, in principle, three choices available: a preference for Option A in all decision rows, a preference for Option B in all decision rows, and a preference for Option A for decision rows with a lower number, and Option B for decision rows with a higher number. The instructions explained what a subject needed to do in each of these cases (see the Appendix with the instructions).[7]

Subjects were informed that there was a 1 in 100 chance to be selected and paid in accordance with the stated preference.[8] We told them that for this purpose the computer would

[5] There was also a third treatment in which subjects first chose whether they wanted to make their investment decision(s) under the conditions of treatment LOW or treatment HIGH and only then were confronted with the decision task of the chosen treatment. However, we will not report on this treatment in this paper because differences in invested amounts between the two treatments do not have a causal interpretation, as there is selection into treatments.

[6] Note that the reward is always received with a delay. Using the CentERpanel (see below) forced us to have a front-end delay since it was impossible to pay the participants immediately after the experiment. This means that our time preference measure is not affected by present-bias.

[7] In order for a subject to select Option A in all decisions, in the experiment Table 1 contained an additional decision line 21 with the entry "Always €300" in column 2 labeled "Payment Option A," and no entry in all other columns. Hence, time preference choices ranged from 1 to 21.

[8] This is common practice in experiments with large samples, see e.g. Andersen et al. (2008) and von Gaudecker,

Decision	Payment Option A (pays amount below in 1 month)	Payment Option B (pays amount below in 7 months)	Preferred Payment Option	
1	€300	€303.80	A	B
2	€300	€307.50	A	B
3	€300	€311.40	A	B
4	€300	€315.20	A	B
5	€300	€319.00	A	B
6	€300	€322.90	A	B
7	€300	€326.80	A	B
8	€300	€330.80	A	B
9	€300	€334.70	A	B
10	€300	€338.70	A	B
11	€300	€342.70	A	B
12	€300	€346.70	A	B
13	€300	€350.70	A	B
14	€300	€354.80	A	B
15	€300	€358.90	A	B
16	€300	€363.00	A	B
17	€300	€367.10	A	B
18	€300	€371.30	A	B
19	€300	€375.50	A	B
20	€300	€379.70	A	B

Table 1: Table used for the elicitation of time preferences in part 2 of the experiment

randomly select a number between 1 and 100, independently for each subject. If the number was 100, the subject would receive an additional sum of money in this part of the experiment. The computer would then randomly select one of the decision lines in Table 1 and the subject would be paid according to the choice indicated in this decision line. To make sure that subjects received their money exactly in one or seven months from the day of the experiment, we made use of CentERdata's established and reliable payment system (see also below).

To summarize, our experiment had two parts. Treatments only differed in the first part of the experiment. In treatment HIGH, subjects made three investment decisions in part 1, and in treatment LOW they made only one investment decision. In the second part of the experiment we elicited subjects' time preferences.

van Soest, and Wengström (2011). Starmer and Sugden (1991) find in a different context that subjects' responses are not affected by this.

2.2 Data collection

The experiment was conducted by CentERdata, an institute for applied economic and survey research for the social sciences that is affiliated with Tilburg University in the Netherlands. CentERdata carries out its survey research mainly by using its own panel called CentERpanel. This panel is internet-based and consists of about 2,000 households in the Netherlands that form a representative sample of the Dutch population.[9] Panel members use their computers at home to participate in the panel questionnaires, and they complete a questionnaire on the internet every week.[10] A particular advantage of the CentERpanel is that for each panel member, researchers have access to regularly collected background information such as demographic and financial data.

After logging on to our experiment, panel members were randomly assigned to one of the treatments and were informed about the nature of the experiment. Then, subjects decided whether or not to participate—as is common with all modules of the panel. For participating subjects, the next screen then described the investment decision task. After making their decision (treatment LOW) or their decisions (treatment HIGH) in the first part of the experiment, time preferences were elicited. Subjects received their earnings by means of the payment and reimbursement system used by CentERdata. CentERdata reimburses the costs for internet access to panel members' private bank accounts four times a year. Whereas payments for earnings in Part 1 were made at the earliest scheduled normal date of payments, it was absolutely crucial for the payments in Part 2 of the experiment that subjects would receive their money either exactly one month or exactly seven months from the day of the experiment. Hence, subjects were told that they would, conditional on receiving a payment at all, receive it for the second part of the experiment also by means of CentERdata's reimbursement system, in accordance with their stated preference in the selected line. Since CentERdata makes reimbursement and other payments regularly and reliably, we can assume that subjects believed that payments for the time elicitation task would be paid according to the rules specified.

Prior to the panel experiment, we conducted a pilot experiment in the lab of Tilburg University with 92 student subjects in order to test whether instructions were clear and whether the procedures we designed to use in the main panel study actually worked. The lab experiment was conducted in exactly the same way as later in the CentERpanel. That is, student subjects

[9] For more information about the CentERpanel and the way it is administered see http://www.centerdata.nl/en/.

[10] Panel members without a computer answer questionnaires using a special device connected to their TV sets.

completed the experiment using a web browser (in the lab) and using the same screens as later the subjects in the panel (at home). As there were no problems with the lab pilot, we used the same procedures and programs later in the main panel study. We briefly summarize the results of the lab experiment in footnote 13 in the Results Section.

In total, 1,872 members of the CentERpanel logged on to our experiment. Of these subjects, 1,637 (87.4%) subjects decided to participate in our experiment, while 235 (12.6%) subjects decided not to participate. Of the 1,637 subjects participating, 1,102 subjects participated in the two randomly assigned treatments reported in this paper (while the remaining 535 subjects participated in another treatment referred to in footnote 5).

The column labeled "Participation" in Table 2 shows descriptive statistics for participating subjects in each of the two treatments, as well as statistics of subjects who chose not to participate in the experiment. The columns labeled "Investment (in %)" show statistics of investment decisions for participating subjects, which we analyze in Section 3.1.

The table is grouped according to a selection of various sociodemographic and socioeconomic characteristics.[11] Concentrating on descriptive statistics for participating subjects, we note that by and large the distribution of the covariates is balanced across the two treatments. We show below that our results are robust to controlling for all those covariates. A comparison of the descriptive statistics in the columns for participants with those of non-participants reveals no significant differences except for some of the age and children categories as well as for the occupation category, with retired and older individuals and those with no children being more reluctant to participate. To address this potential sample selection problems, for regressions reported below we ran Heckman (1976) selection models using the variable "Ratio" as one of the exclusion variables. The variable "Ratio" measures the proportion of questionnaires completed by panel members in the three months proceeding our experiment. This variable can be assumed to affect the participation decision but not the decisions taken in the experiment. For none of the regressions we found evidence for a selection bias.

[11] Next to the self-explanatory variables, Table 2 contains the following variables. Subjects in Education category "Low" have primary or lower secondary education, those in "Middle" have higher secondary or intermediate vocational training, and those in "High" have higher vocational training or a university education. The variable "Holds equity" is 1 if a subject currently holds equity and 0 otherwise.

		Participation			Investment (in %)		
		YES		NO			
		Low	High		Low	High	Difference
Gender	Female	47.2	46.1	50.2	51.7	43.1	8.6***
	Male	52.8	53.9	49.8	52.5	43.7	8.8***
Age	Age 18-24	5.08	3.81	5.11	50.5	44.8	5.7
	Age 25-34	19.4	20.3	7.2	58.1	43.5	14.6***
	Age 35-44	19.8	17.4	16.2	53.2	42.3	10.8***
	Age 45-54	19.8	27.0	18.3	48.6	44.1	4.5
	Age 55-64	18.0	16.9	24.3	47.8	41.1	6.7**
	Age 65+	18.0	14.5	28.9	53.0	45.8	7.2**
Education	Low	30.49	29.95	36.60	52.6	46.0	6.6**
	Middle	35.03	33.58	28.09	52.9	43.2	9.7***
	High	34.48	36.48	35.32	50.8	41.5	9.3***
Location	Rural	57.4	61.9	53.2	53.1	43.9	9.2***
	Urban	42.6	38.1	46.8	50.6	42.6	8.0***
Partner	No	23.1	23.6	26.0	50.3	39.8	10.5***
	Yes	76.9	76.4	74.0	52.6	44.5	8.1***
Position in HH	Head	61.7	63.7	59.2	51.8	41.7	10.1***
	Other	38.3	36.3	40.8	52.6	46.5	6.1**
Children	No	59.5	53.4	66.8	52.2	41.8	10.4***
	Yes	40.5	46.6	33.2	51.9	45.3	6.6**
Occupation	Employed (contract)	50.5	54.1	42.1	51.5	42.7	8.8***
	Retired	18.2	15.3	28.5	55.5	42.5	13.0***
	Works in own household	12.3	14.0	13.2	49.7	45.3	4.4
	Student	5.4	2.9	4.7	53.9	37.6	16.3
	Freelance or self-employed	4.2	3.1	2.1	51.0	50.4	0.6
	Unemployed	2.0	1.8	0.9	39.1	39.7	−0.6
	Other	7.4	8.9	8.5	54.2	47.0	7.2
Household (HH) income	HH gr. income ≤ €2,250	25.2	23.1	30.6	54.0	42.5	11.5***
	HH gr. income €2,251−€3,130	22.9	26.7	25.5	49.6	41.9	7.7***
	HH gr. income €3,131−€4,350	27.2	27.4	23.8	53.3	46.2	7.1**
	HH gr. income ≥ €4,351	24.7	22.9	20.0	51.1	42.8	8.3**
Holds equity	No	76.6	74.6	-	52.2	43.7	8.5***
	Yes	23.4	25.4	-	51.8	42.6	8.2***
Plays lottery	Never	32.8	29.6	-	51.4	44.5	6.9**
	At least once a year	67.2	70.4	-	52.4	43.0	9.4***
Has savings account	No	12.9	11.6	-	50.4	50.4	0
	Yes	87.1	88.4	-	52.3	42.5	9.8***
Impatient	No	29.6	29.8	-	47.2	43.7	3.5
	Yes	70.4	70.2	-	54.1	42.1	12.1***
	Number of observations	551	551	235	551	551	551

Notes: Numbers indicate column percentages within each main category listed in the first column. For treatment High, the table shows the average relative investment over the three rounds. *,**, *** indicate significance at the 10%, 5%, 1% level of Mann-Whitney U tests for differences of the distributions of the percentages invested across treatments for subjects in the category listed in column 1. The last four variables are not available for non-participants as these questions were only presented to participants. The variable "Impatient" is coded as Yes if the stated time preference choice is above 10 and coded as No otherwise.

Table 2: Descriptive statistics

3 Results

3.1 A first look at the data

We first look at Part 1 of the experiment and compare the average percentage of the endowment invested between the two treatments. Table 3 shows that, on average, subjects invested 43.4% of their endowment in treatment HIGH and 52.1% in treatment LOW—a 8.7 percentage point difference.[12] A Mann-Whitney U test confirms that the distributions of percentages invested differ significantly across treatments ($p < 0.0001$). This finding is in line with the treatment effect found in many studies that have used the Gneezy and Potters (1997) design.[13] Although in our heterogeneous sample the difference between the treatments is somewhat lower than in studies using a more homogeneous group of subjects, our result implies that the main treatment effect carries over to the population at large.

With our heterogenous subject pool, we can analyze whether the treatment effect is present for subsamples of the population. The last three columns of Table 2 show the average percentage invested in the two treatments for subjects with various characteristics listed in the first column of the table, the difference between these two percentages, and the results of Mann-Whitney U tests of treatment differences. Reading Table 2 row-wise, we note that a substantial and significant treatment effect is present in the majority of the subsamples displayed in the table. Considering subgroups that have a substantial number of participants, we find that regarding socio-demographic characteristics only the difference in the group of people aged 45-54 is not statistically significant. In the regressions presented in the next section we control for the effect of background variables.

Before turning to the relationship between the treatment effect and time preference, we first briefly look at the results of Part 2 of the experiment in which we elicited subjects' time preferences. We find a mean choice of 14.31 (standard deviation 6.52) and a median choice of 15.[14] The latter corresponds to an annual interest rate of between 32.5 and 35 percent, which is similar to the range of 27.5 to 30 percent found by Dohmen et al. 2010 for the German population, and the 28 percent

[12] For treatment HIGH, the table shows the average percentages invested in the three individual rounds, which are fairly stable: 42.6%, 42.3%, and 45.4%, respectively.

[13] See footnote 3 for references. The differences observed in these earlier studies range from 11.6 percentage points (student sample in Haigh and List, 2005) to 31.5 percentage points (Fellner and Sutter, 2009). In our pilot lab experiment with students subjects we obtained a difference of 14.1 percentage points. For a non-standard subject pool of professional traders Haigh and List found a difference of 28.7 percentage points.

[14] Means of elicited time-preference choices are virtually the same across the two treatments (treatment LOW: 14.30, standard deviation 6.62; treatment HIGH: 14.32, standard deviation 6.44). To check whether time preference choice was affected by the outcome of the lottery, we regressed it on total earnings and a treatment indicator and found no significant relationship.

Treatment	Mean	SD	#Obs
LOW	52.1	27.5	551
HIGH	43.4	24.5	551
H_0: Treatment has no effect on investment			
$p < 0.0001$ (two-tailed Mann-Whitney U test)			

Note: For treatment HIGH, the table shows the average investment over three rounds.

Table 3: Percentage of per-period endowment invested

reported by Harrison, Lau, and Williams (2002) for their Danish sample.[15]

Now we combine the data from the two parts of the experiment and relate investment behavior to time preferences. Figure 1 shows local linear regression estimates of mean investment in the two treatments (vertical axis) plotted against the time preference (horizontal axis), along with corresponding 95% confidence intervals.[16] As the line indicating investment behavior in treatment LOW is consistently above the line indicating investment behavior in treatment HIGH, this figure replicates the finding that subjects in treatment LOW invest on average more than subjects in treatment HIGH. Interestingly, in treatment HIGH, we find a negative (insignificant) relationship between investment levels and discount rates. This is in line with most other studies (e.g., Anderhub et al., 2001, Burks et al., 2009, Dohmen at al., 2010, Eckel, Johnson, Montmarquette, 2005). In treatment LOW, we find a positive (significant) relationship (in line with Booij and van Praag, 2009). However, the latter effect is confounded with the effect of the treatment manipulation. If we aggregate over the two treatment, we find no significant relationship between investment levels and impatience. It seems that the relationship between risk and time preferences is quite equivocal, which may in part be due to the variation in preference elicitation tasks across studies. Importantly, the difference in investment levels across the two treatments increases with impatience. This means that impatient subjects are on average more affected by our treatment manipulation than patient subjects.

[15] A substantial fraction of subjects switched from Option A to Option B at 20 or chose Option A throughout (i.e., chose 21, see footnote 8). Whereas a choice of 20 means that a subject has a discount rate in the interval from 47.5 to 50 percent, a choice of 21 means that a subject has a discount rate of 50 percent or higher.

[16] For this and also for the results presented below we used only the first period's choice of treatment HIGH. Results are not sensitive to the choice of the bandwidth. For the figure, we used a rule-of-thumb bandwidth.

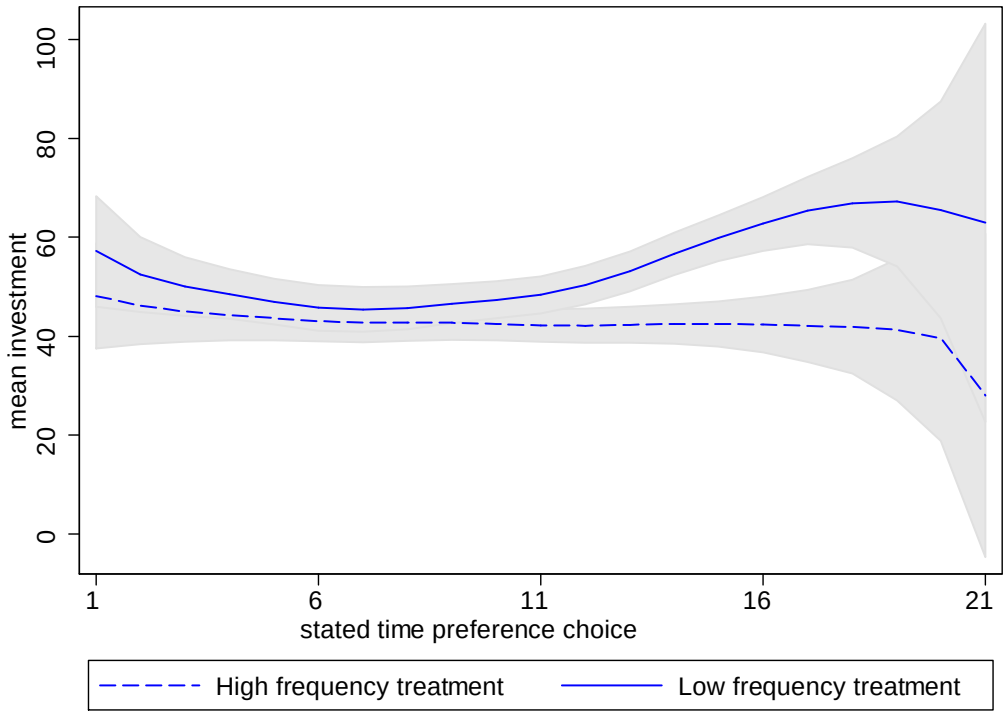

Figure 1: Relationship between investment in each treatment and the time preference

3.2 Regression analysis

The evidence presented in the previous section suggests that there is a relationship between the invested amounts and time preferences. In this section, we examine whether this relationship is robust to the inclusion of various covariates.[17] It is important to do so because these covariates may be related to investment levels and the treatment effect on the one hand, and time preferences on the other hand. For example, less educated individuals are more impatient. So, finding that more impatient individuals react more strongly to the treatment manipulation may simply reflect the fact that less educated individuals react more strongly to the treatment. We rule this out by controlling for education and its interaction with the treatment indicator (as well as other covariates and their interaction with the treatment indicator) and still finding a significant coefficient on the interaction term between the treatment indicator and impatience. This is done by means of regressions of the percentages invested on a treatment dummy "Low", an indicator for a stated time preference choice above 10, "Impatient", and the interaction between the two, controlling for sociodemographic

[17] Regressing the stated time-preference choice on background variables as listed in Table 2, we find that individuals who have a high education, hold equity, or have a savings account are significantly more patient than individuals who have, respectively, low education, no equity, or no savings account.

characteristics. We use data for the only choice made in treatment LOW and the round-1 choice made in treatment HIGH. The coding of "Impatient" is based on Figure 1, which suggests that the treatment effect is higher for stated time preference choices above 10.[18] According to this measure, 70.3 percent of the individuals are impatient (see Table 2). The estimation results are presented in Table 4. All explanatory variables other than "Low" are de-meaned when they are not interacted. Hence, the constant term is always the mean investment in the high-frequency treatment. For the interaction terms the mean was calculated for all observations with Low=1 and then subtracted. This de-meaned value was then interacted with "Low". Hence, the coefficient on "Low" is always the average effect of "Low" for those in treatment "Low" and across all covariates.[19] Our baseline specification, (1), has no covariates, whereas in specifications (2) to (5) we control for groups of covariates.

The estimated coefficients on the treatment variable "Low", the time preference variable "Impatient" and the interaction term between these two variables, "Impatient×Low", are similar across all 5 specifications. The treatment effect is consistently estimated to be about a 9.5 percentage point increase in the percentage of the endowment that was invested, and is always highly significant. This means that the treatment effect (according to which subjects invest more if encouraged to take a broader frame) remains if we control for socioeconomic variables and the stated time preference. The coefficient of the variable "Impatient" is estimated to be negative, but insignificant in all specifications. Hence, subjects' time preferences are not related to their investment behavior in treatment HIGH. However, the coefficient on the interaction term "Impatient×Low" is consistently estimated to be around 8.5, and is significantly different from zero in all specifications, which means that the treatment effect depends on the time preference. Subjects with a higher discount rate are affected more by our treatment manipulation, or, put differently, more impatient subjects invest more when placed in an environment that, arguably, encourages them to take a broader perspective.

[18] Defining "Impatient" by means of a median split and running the same regressions yields very similar results. We also carried out a specification check based on regression specification (1) in Table 4. If, in addition to Low and Time×Low, we include a full set of dummies for stated time-preference choices, and interactions of those with Low, then we cannot reject the null hypothesis that the coefficients on these dummies are jointly zero. Likewise, if we include a second order polynomial in the time preference, interacted with Low, in addition to Low, Impatient and Impatient×Low, we cannot reject the null that the additional 4 coefficients are jointly zero. This suggests that our definition of "Impatient" is appropriate.

[19] This is the treatment effect on the treated. It differs only slightly from the average treatment effect, as treatment assignment was random and the distributions of covariates are almost identical in the treatment and the control group. For the same reason, the intercept varies slightly across specifications.

Dependent variable: percentage of the per-period endowment invested						
	(1)	(2)	(3)		(4)	(5)
Constant	42.551***	42.553***	42.544***	Constant	42.576***	42.636***
	(1.12)	(1.12)	(1.12)		(1.12)	(1.12)
Low	9.531***	9.528***	9.546***	Low	9.565***	9.361***
	(1.58)	(1.58)	(1.60)		(1.58)	(1.58)
Impatient	−1.648	−1.679	−1.723	Impatient	−1.405	−2.218
	(2.44)	(2.45)	(2.47)		(2.47)	(2.46)
Impatient×Low	8.554**	8.947**	8.839**	Impatient×Low	8.045**	9.351***
	(3.46)	(3.47)	(3.50)		(3.53)	(3.49)
Female		−0.004		Education Low	−0.630	
		(2.26)			(2.81)	
Female×Low		−1.550		Education High	−3.517	
		(3.21)			(2.73)	
Age		1.210		[Education Low]×Low	−0.394	
		(7.69)			(3.96)	
Age×Low		−12.340		[Education high]×Low	2.785	
		(10.46)			(3.91)	
Employed			8.471	HH gross income €2251−€3130	−3.854	
			(6.06)		(4.42)	
Freelance			1.930	HH gross income €3131−€4350	−1.162	
			(8.45)		(4.24)	
Unemployed			−3.591	HH gross income ≥ €4351	3.924	
			(6.74)		(4.26)	
Student			0.666	[HH gross income €2251−€3130]×Low	−3.853	
			(3.37)		(6.44)	
Works in own household			0.378	[HH gross income €3131−€4350]×Low	−5.003	
			(3.26)		(6.22)	
Retired			2.096	[HH gross income ≥ €4351]×Low	−9.224	
			(4.16)		(6.30)	
Employed×Low			−9.601	Plays lottery		−2.465
			(8.05)			(2.44)
Freelance×Low			−16.794	[Plays lottery]×Low		3.200
			(11.72)			(3.43)
Unemployed×Low			5.450	Holds equity		2.279
			(8.42)			(2.58)
Student×Low			−3.107	[Holds equity]×Low		−1.778
			(4.90)			(3.71)
Works in own household×Low			2.784	Has savings account		−10.721***
			(4.48)			(3.50)
Retired×Low			1.017	[Has savings account]×Low		13.212***
			(6.21)			(4.88)
R^2	0.039	0.042	0.047		0.049	0.049

Note: ** and *** indicate significance at the 5% and 1% level, respectively. Standard errors are in parentheses. Categories left out: "Other" in occupation, "Middle" in education, "≤ €2,250" in HH gross income.

Table 4: Regression results

Regression (1) formally corroborates the visual insights gained from Figure 1.

In regressions (2) to (5) we include covariates to assess whether there is a relationship between investment behavior and observed differences in gender, age, occupation, education, income, and variables that proxy for risk preferences such as whether the individual plays in a lottery, holds equity, or has a savings account. Except for having a savings account, these variables are not significantly (at the 5 percent level) related to investment behavior and the treatment effect.[20]

Finally, we address the possibility that impatience is related to cognitive ability, and that therefore, the treatment effect may also depend on cognitive ability. To shed light on the role of cognitive ability we matched our data with data on the Frederick (2005) three-item "Cognitive Reflection Test" (CRT) measure that was collected in a different experiment. It is a simple measure of intelligence that is given by the number of correct answers to three questions. Despite its simplicity, Frederick (2005) provides evidence that it has equal or sometimes even better predictive power, e.g. for predicting impatience, than other measures that are substantially more difficult to elicit. In our data, the CRT measure is negatively correlated to our measure of impatience, i.e. more impatient individuals answer less of the CRT questions correctly.

The CRT score is available for 563 out of the 1,102 individuals who participated in our experiment. In order to assess whether this is a selected sample, as before, but now for those individuals for whom this information is available, we regressed the invested amount on an indicator for treatment LOW, our measure "Impatient" for impatience, and the interaction of the two. The results are presented in column (1) of Table 5. They are very similar to our main results that are presented in column (1) of Table 4. We conclude from this that the subsample of individuals for whom the CRT measure is available is not a selected sample. This is confirmed by comparing means of the observable characteristics for this subsample to the ones reported for the main sample in Table 2 (results not reported).

In order to relate the invested amount and the treatment effect to both impatience and cognitive ability by means of a regression, we create a dummy for a CRT measure of at least 2

[20] We also tested for a relationship between the invested amount and the covariates using the following two kinds of tests. For each main category listed in column 1 of Table 2, we tested whether the investment in treatment HIGH is the same across all subcategories listed in column 2 of Table 2 (Test 1), and whether the treatment effect (that is, the effect of LOW) is the same across all subcategories listed in column 2 of Table 2 (Test 2). For example, to conduct these tests for the main category Gender, we first estimated the regression equation $x_p = \alpha_0 + \alpha_1 \times Low + \alpha_2 \times Female + \alpha_3 \times Female \times Low + \varepsilon_i$, where x_p is the percentage of the per-period endowment invested, and "Low" and "Female" are dummy variables coding treatment and gender. Then we tested H_0: $\alpha_2 = 0$ (Test 1) and H_0: $\alpha_3 = 0$ (Test 2). These two hypotheses were not rejected for any of the main categories (at the 5% level), except for having a savings account. This means that both the average relative investment in treatment HIGH and the difference in average relative investments in treatments HIGH and LOW are statistically the same across the subcategories listed in Table 2.

	(1)	(2)
Constant	40.814***	40.836***
	(1.53)	(1.52)
Low	12.027***	11.985***
	(2.21)	(2.21)
Impatient	−1.470	−2.511
	(3.33)	(3.99)
Impatient×Low	11.409**	12.881***
	(4.78)	(4.82)
CRT≥ 2		−4.058
		(4.56)
CRT≥ 2×Low		10.720**
		(4.60)
Impatient×CRT≥ 2		1.147
		(4.87)
N	563	563
R^2	0.062	0.085

Note: ** and *** indicate significance at the 5% and 1% level, respectively. Standard errors are in parentheses.

Table 5: Regression results controlling for cognitive ability

and regress the invested amount on the treatment dummy, the interaction thereof with the variable Impatient, a dummy for a high CRT score, and the interaction thereof with the treatment dummy. Results are presented in column (2) of Table 5. They show that our main result remains to hold: The treatment effect is higher by about 13 for impatient individuals. At the same time, we find that it is higher by about 11 for individuals with a CRT score of at least 2. This means that both impatient individuals and individuals with higher cognitive ability react more strongly to the treatment manipulation.[21]

4 Summary and conclusions

Using a large sample of the Dutch population, we analyze whether individuals' risk attitude can be influenced by a simple treatmen manipulation, and, if so, whether the effect is correlated with individuals' time preferences and demographic and socioeconomic characteristics. The treatment reduces people's decisions flexibility and encourages them to frame a sequence of risky choices

[21] The specification here could still be too restrictive, as it does not allow the dependence of the treatment effect on impatience to depend on the CRT score. When we additionally allow for such a dependence we find that the treatment effect for individuals who are both impatient and have a high CRT score is higher by 25.706, as compared to individuals who neither have a high CRT score nor are impatient. It is by 23.097 higher if they are only impatient, and by 26.781 if they are only smart.

broadly rather than narrowly and, as a consequence, not to fall prey to myopic loss aversion.

We find that the effect of the treatment manipulation that was previously found in student samples is not specific to those samples. We provide evidence that the (average) treatment effect is present in subsamples with diverse socioeconomic characteristics in terms of, for instance, gender, age, occupation, education, and, income. The effect first found in the lab turns out to be a robust behavioral pattern in all strata of the population.

This finding squares well with Rabin and Weiszacker (2009) who show that the tendency to frame narrowly is rather uniform across the population, and does not vary much with observable background characteristics. The fact that a relatively minor change in the decision structure can induce people to bracket more broadly is important, since narrow framing, and related phenomena such as narrow bracketing and myopic mental accounting, have been associated with somewhat distressing phenomena such as the disposition effect (Kumar and Lim, 2008), the stock market participation puzzle (Barberis, Huang, Thaler, 2006), the equity premium puzzle (Benartzi and Thaler, 1995), the willingness to pay large premiums to insure against small risks (Rabin, 2000), and 'adding-up effects' which cause small, seemingly innocent, indulgences to accumulate into a serious (health) hazard (Read, Loewenstein, and Rabin, 1999). Our results suggest that for most of the population variations in presentation and choice structure may have a significant impact on such behavioral patterns.

The result we wish to emphasize is that the effect of our treatment manipulation on risk taking behavior is significantly larger among individuals with high as opposed to those with low discount factors. In other words, the decision frames of impatient people are affected more easily than those of impatient people. This is interesting from a policy perspective, as policy interventions are typically proposed for individuals with "problematic" behaviors such as low savings, overspending on credit cards, obesity, which have all been associated to a high rate of discounting. In this paper, of course, we have only studied one specific treatment effect, namely one that stimulates people to evaluate decisions in combination by reducing the decision frequency. Future work should examine whether the strength of other interventions varies with individuals' time preferences in similar ways. For instance, it would be interesting to see whether default effects or the impact of commitment devices are stronger for impatient than for patient individuals.

Why are framing effects related to time preference? Our results indicate that the effect is not mediated by cognitive abilities. We speculate that 'accessibility' provides the link between the two. Framing effects derive from the fact that many decisions are made intuitively rather

than cognitively (Kahneman 2003). Intuitive thoughts come to mind spontaneously, and they are primarily based on the way in which a problem presents itself to the decision maker. A key aspect is accessibility, that is, the ease with which certain characteristics of a problem come to mind. A decision frame affect the accessibility of different elements, and its impact derives from decision makers' inability to see beyond the most accessible elements. In a similar vein, accessibility is relevant for decisions that involve a trade-off between the present and the future. The immediate consequences of a decision will come to mind more readily and concretely than consequences in the future (Trope and Liberman, 2003). As a result, the former are likely to attain more weight in the evaluation than the latter. Taking these two lines of argument together suggests a link between framing and time preference. Intuitive decision makers tend to be affected more by accessibility. This will render them more sensitive to framing effects as well as induce them to weigh proximate consequences more heavily than distant ones. In sum, 'accessibility' is a factor that may affect both framing effects and impatience. Clearly, however, more research is needed to investigate the validity of this chain of arguments.

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