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Small-Scale Changes in Wealth and Attitudes Towards Risk

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Small-scale changes in wealth and attitudes toward risk

by Sergio Sousa
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Abstract

This paper reports on an experiment designed to examine the effects of small-scale changes in wealth on risk attitudes. We find that the money given prior to risky choices does not induce a change of subjects' risk preferences. This result supports a key assumption in a recent literature over calibration critique of decision theories. Furthermore, as the money given to subjects in our experiment is administered in between risky tasks and framed as a reward rather than a windfall gain, our result suggests that experimental findings reporting that a prior monetary gain induces individuals to take more risks (*house-money effect*) may be more sensitive to prior experience with the risk-elicitation task or framing of the money than previously thought.

Keywords: risk aversion, wealth effects, risk-elicitation, house-money effect, narrow framing.

JEL Classification: C91, D01, D81

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1 Introduction

This paper reports on an experiment designed to examine the effects of small-scale changes in wealth on risk attitudes.

Assumptions made about how changes in wealth affect attitudes toward risk underpin empirical and theoretical results in a broad range of topics in economics. Ogaki & Zhang (2001), for instance, point out how strikingly different empirical tests of the risk sharing hypothesis involving household consumption models can be when estimation methods are based on preferences that allow relative risk aversion to vary with the level of wealth. Models dealing with phenomena as diverse as life-cycle savings (Weil, 1993), portfolio choice (Hadar & Seo, 1990), and asset pricing (Gollier, 2001), make predictions that are very sensitive to the way risk attitudes are affected by changes in wealth. How risk aversion varies with wealth has also implications for Samuelson's fallacy of large numbers (Samuelson, 1967) and Rabin's calibration theorem (Rabin, 2000), paradoxes that have been the object of considerable attention[1]. Samuelson's paradox refers to a pattern of choice that rejects positive mean gambles, such as an even chance to win \$200 or lose \$100, but accepts one hundred of such gambles in a row. Samuelson regarded that choice behaviour as inconsistent with Expected Utility Theory (EUT). Assuming that the single bet is unacceptable at all wealth levels, he proved a theorem stating that the initial rejection should imply a rejection of any sequence of such bets. But rejection of a gamble at all wealth levels is an assumption that, as showed by Ross (1999), holds only for a limited class of utility functions, namely, those displaying constant absolute risk aversion. Such utility functions describe individuals whose attitudes towards risk are the same across wealth positions. A similar claim has been made by Cox & Sadiraj (2006) and Palacios-Huerta & Serrano (2006) regarding the validity of Rabin's demonstrations that risk aversion over modest stakes within EUT implies absurd risk aversion over large stakes gambles. They point out that Rabin's striking results rely on the assumption that a given risk is *consecutively* rejected across a wide range of wealth levels, which in a sense amounts to saying that risk aversion does not vary with wealth.

Despite the analytical importance of the characterization of absolute and relative risk aversion, there is mixed empirical evidence as to the effects of changes in wealth on attitudes toward risk. Ogaki & Zhang (2001), Guiso *et al.* (1996) and Rosenzweig & Binswanger (1993), for instance, find evidence in support of the decreasing relative risk aversion hypothesis, while

[1] Regarding Samuelson's paradox: see, on the empirical front, Redelmeier & Tversky (1992); Haubrich (1998); Benartzi & Thaler (1999); Gneezy *et al.* (2003); Klos *et al.* (2005); Chen & Corter (2006); and, on the theoretical front, see Nielsen (1985); Ross (1999); Peköz (2002); Hammarlid (2005). Regarding Rabin's theorem, see Rubinstein (2001); Watt (2002); Wakker (2005); Bombardini & Trebbi (2005); Cox & Sadiraj (2006); Palacios-Huerta & Serrano (2006).

Szpiro (1986), using data on insurance, finds empirical support for constant relative risk aversion. Barsky (1997) and Donkers *et al.* (2001), instead, find evidence that risk aversion increases with wealth, while Binswanger (1980) finds that changes in wealth have no significant effect on risk aversion. Even though various methodology-related arguments may be given to explain that discrepancy, it is debatable whether these econometric studies have fully provided evidence on the way attitudes to risk are affected by changes in wealth. Most of the existing results are based on data involving choice behaviour among individuals of different wealth levels[2]. But inferring how risk aversion varies with wealth from cross-sectional observations may not be accurate when preferences are heterogeneous.

At first sight, a data set containing measures of risk attitudes at various wealth positions of an individual (i.e. a long panel) could fully overcome that concern. However, wealth is likely not exogenous to attitudes to risk: unobservable risk-driven choices can underly the changing of wealth positions. Thus, econometric estimates would still have to address the problem of endogeneity that could confound estimation. An alternative approach would be a laboratory experiment, where wealth can be exogenously manipulated. Though this method cannot produce, under incentivised conditions, an extensive map of individuals' wealth states onto their risk attitudes, it can produce evidence that complements econometric studies by providing careful controls of risks taken and changes of wealth experienced. While several experimental investigations (e.g., Harrison, 1986; Holt & Laury, 2002; Bosch-Domenech & Silvestre, 1999; Bosch-Domènech & Silvestre, 2003) have brought evidence about attitudes toward scaled-up risks given subjects' initial wealth level, contributions that test for effects of changes in wealth on attitudes toward a given risk are scarce[3].

This paper fills in the gap in the literature by eliciting experimentally the sensitivity of risk attitudes to small-scale changes in wealth. We avoid the common problems with available data by using a design that administers a carefully controlled "exogenous" small-scale change in wealth. We elicit attitudes to risk through a multiple price list method at two different times, say t_0 and t_1. A sub-group of subjects (treatment group) is awarded money between t_0 and t_1. Another sub-group (control group) is not awarded any money, and their choices are used to detect changing patterns of risk

[2] An exception is Eisenhauer (1997), who uses a long sample of aggregate time series data from the U.S. and finds evidence that absolute risk aversion increases with wealth – which is in contrast with the above mentioned studies based on cross-sectional analysis.

[3] One of the few attempts to investigate this in the lab is done by Levy (1994). He uses a portfolio allocation-type of decision problem repeated over 10 periods, allowing subjects to accumulate their earnings at each period. But by allowing subjects to accumulate earnings across trading periods, his design re-introduces the problem with field data: endogeneity of risk-taking behaviour. Thus, his observed results may not be accounted for by a utility function exhibiting decreasing absolute risk aversion (DARA), but by a "house money" effect (Thaler & Johnson, 1990).

attitudes elicited at t_1 relative to t_0 that cannot be attributed to changes in wealth, induced by the experimenter.

We find that the money given to subjects does not affect subjects' attitudes to risk. Our empirical results contrast with previous studies reporting a "house-money" effect: a change of risk preferences that is induced by money given prior to risky choices (see, e.g., Thaler & Johnson, 1990; Ackert *et al.* , 2006; Harrison, 2007). We argue that the inability of the money given to subjects in inducing changes in their attitudes to risk reflects subjects' tendency to not merge prior gains with he potential consequences of risky choices ("narrow bracketing"). More importantly, as the money given to subjects in our experiment is administered in between risky tasks, these experimental results suggest that the effects a monetary gain may have on individuals' risk preferences may be more sensitive to prior experience with the risk-elicitation task than previously thought.

The rest of this paper is organised as follows. In the next section we describe the experimental design. Section 3 presents theoretical predictions and section 4 presents and discuss the results. Section 5 concludes.

2 Experiment

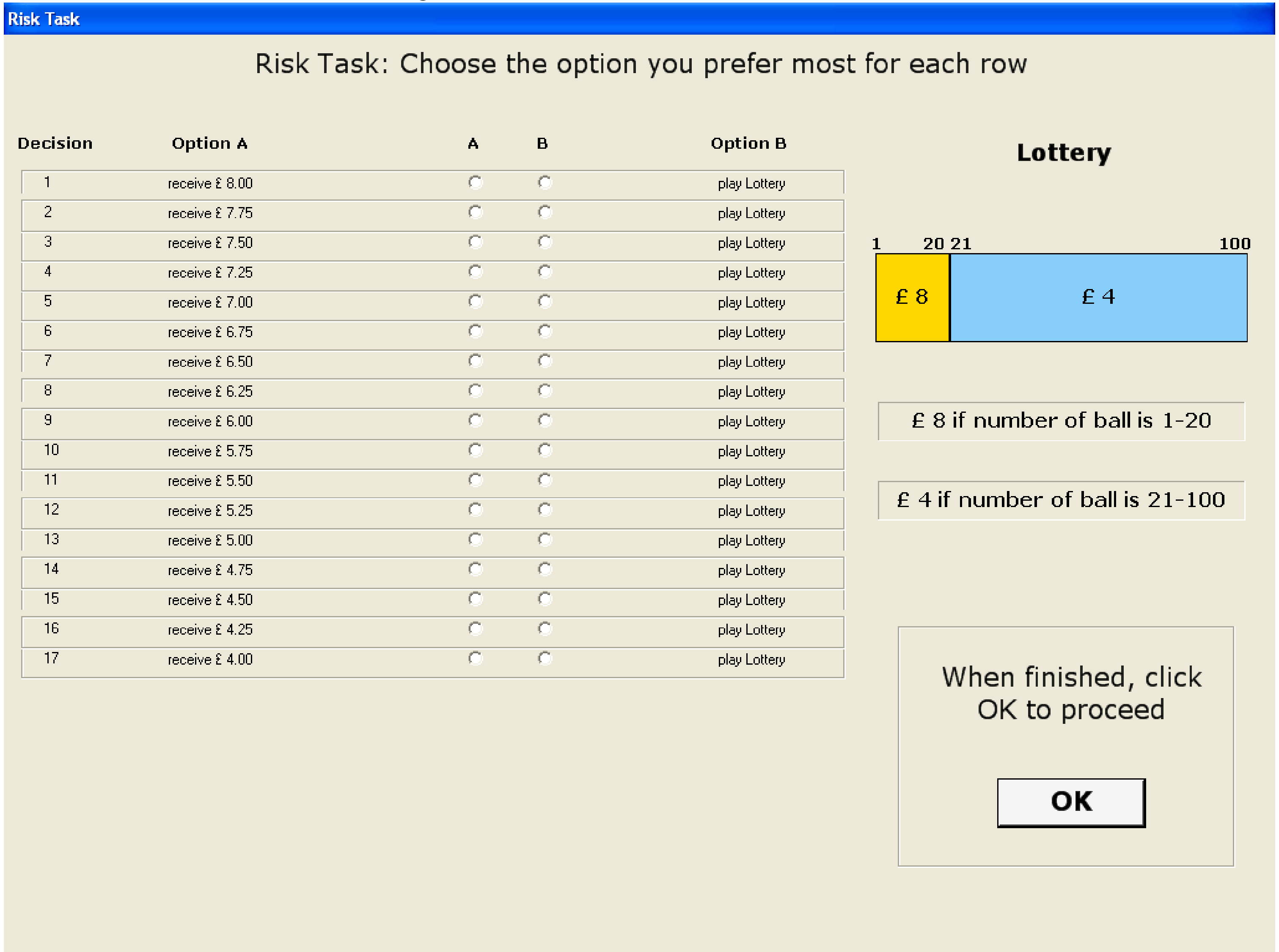
Since the elicitation of individual's attitudes toward risk is the building block of our experiment, we start presenting the method used to this end. Then, we describe the sequence of task stages the experiment consists of, followed by a description of the experimental treatments.

2.1 The risk-elicitation procedure

The question of how to elicit people's attitudes to risk is addressed via a Multiple Price List (MPL) procedure. We implement a sequence of risk-elicitation tasks in each of which a subject faces a number of pairwise choice problems. Figure 1 below presents a screenshot of the set of pairwise problems presented to subjects in a given risk task. In this example, the task consists of eliciting the cash equivalent of the lottery $L(8.00, 1/5; 4.00, 4/5)$, where the fractions indicate the probabilities of winning, and the integer numbers indicate the winnings in British Pounds:

Each decision row on the screen constitutes a choice problem, which is to choose between option A, a sure sum, or option B, the lottery. Subjects are asked to indicate their preference for each choice problem. As one proceeds down the table the sure amount of money decreases and becomes less and less attractive when compared to the expected value of the lottery (in this case £4.80). Because the difference between the sure sum and the expected value of the risky option decreases and turns negative from some point on, even a very risk-averse individual is expected to switch over to the lottery at some row when going down the table.

Figure 1: Illustration of a risk elicitation task



Provided a subject starts by choosing A and switches once, task responses can be reduced to a closed switching interval within which the certainty-equivalent of the lottery option falls into. For instance, if a subject crosses over to the risky option when the sure option offers £6.00, choosing the lottery thereafter, then we know that the sum of money that is regarded as good as the lottery lies between £6.00 and the sum offered in the next row, which is £5.75. We shall use the switching interval midpoint as our operational concept of the observed certainty-equivalent[4].

It is relatively common in this type of task to have some subjects switching back and forth between options as they proceed down the menu of choices. Our software, though, did not permit a subject to have multiple switch points. When one chooses option A, say £5.50, over option B, the lottery, the computer assumes that option A is also preferred over the lottery whenever it is offering a sum larger than £5.50, filling-in the buttons accordingly. Likewise, when the lottery option is chosen over a given amount of money, say £4.00 the computer also assumes that the lottery is preferred to the sure amount when it is less than £4.00. Before proceeding to a new risk task, subjects could change their choices and adjust their switching point as many times as they wished.

In our view, this feature has several advantages. First, it may help to alleviate boredom; subjects who understood it realise that they do not necessarily need to pick an option at every decision row. Second, it gives complete flexibility while embodying a feature that those who understand and take the task seriously would want to obey. Third, it allows subjects to economise on "clicking effort", simplifying the decision problem and thus helping them to focus attention on the provision of a switch point that is as accurate as possible. Fourth, and last, it allows a more refined elicitation of certainty-equivalent from the *entire* sample[5] by eliminating the appearance of non-useable responses, since they violate monotonicity.

2.2 The experimental tasks

The experiment consists of three stages: (1) first risk-elicitation stage, (2) cognitive stage, and (3) second risk-elicitation stage, respectively.

Stage I: Risk tasks

A subject faces a sequence of six risk tasks. For convenience, Table 1

[4]This interval is quite narrow (0.25), which makes the midpoint of the switching interval a more refined estimate of subjects' money-equivalent point of lottery options in each risk task. We keep this variation between sure amounts of money from decision row to decision row constant across all risk tasks.

[5]Provided, of course, a subject's choices do not violate first-order stochastic dominance, which can happen if she prefers a given option over the other in all decision rows in a given risk task.

below presents the set of lotteries used in each of these risk tasks in the order they are presented[6]. They are all binary lotteries and only involve strictly positive outcomes.

Table 1: Lottery option per risk-elicitation task

Lottery	Payoff 1	Pr(Payoff 1)	Payoff 2	Pr(Payoff 2)	EV	Rows
L1	8	0.2	4	0.8	4.8	17
L2	9	0.2	3	0.8	4.2	25
L3	6	0.4	3	0.6	4.2	13
L4	9	0.3	4	0.7	5.5	21
L5	16	0.2	10	0.8	11.2	25
L6	6	0.4	3	0.6	4.2	13

Cognitive stage

After completing a sequence of six risk tasks, subjects are then asked to complete a timed cognitive test. They have twelve minutes. They are told that their answers to these questions have no effect on their earnings in the experiment.

The cognitive test has two major purposes. First, to allow the small-scale wealth increment to be framed as a reward for completing the test. The idea is then to use this test as an "endogenous" treatment administration route: depending on the treatment condition the subjects were randomly assigned to, they learned that a money reward for submitting a complete set of answers to the test is guaranteed at the end of the experiment. This way, we want to induce them to think that the reward was "earned" rather than received as a "gift" from experimenters. Second, to crowd out subjects' working memory: as the same lotteries will be faced in a later stage task of the experiment, by going through a cognitive test-type of task, subjects' working memory is likely to be loaded with new information; this makes less likely that they will spot the equivalence between first and second round of risk tasks, which might cause them to guess that the experiment tests for consistency, and respond accordingly (see Bertrand & Mullainathan, 2001).

Stage II: Risk tasks

In this stage subjects are asked to complete more risk tasks. They actually face the same sequence of six risk tasks they faced before, though

[6]For roughly half of the subjects. For the other half the order of L2 ad L5 was reversed. The purpose is to perform a small-scale test of order effects. We do not reject the hypothesis that there is no order effect on elicited risk attitudes.

subjects are not told this. We shall refer to this stage as the second risk-elicitation stage.

2.3 The experimental treatments

Our experiment has two treatment conditions[7] in which we manipulate the money reward, say Δw, that subjects are given for completing the *Cognitive stage*. Δw takes one of two values: £0 or £7.00, which will be denoted by *zero* and *nonzero* increment treatment conditions. The experimentally induced increment is modest, but it is larger than the expected value of almost all lotteries used in the risk tasks.

Subjects assigned to the treatment condition in which $\Delta w = 0$ are used as a control group. We use their responses across stages to control for differences in risk attitudes elicited at Stages I and II that are genuinely induced by $\Delta w = 7$ from those differences induced by inherently imprecise preferences (Butler & Loomes, 2007), stochastic choices (Loomes & Sugden, 1995; Loomes, 2005), or even changes in individual circumstances.

2.4 Administration

A total of 138 subjects were recruited on a first-come first-served basis to take part in the experiment, divided in sessions involving 12-16 people at a time[8].

We pre-randomised the increment treatment condition to be assigned to each experimental session, so to all subjects in the session. Subjects in a given session were randomly seated at individual computer terminals in our laboratory. An individual ID number was entered for each subject, and this was used to record their decisions throughout their experiment. They were told at the beginning of the session that although there were many people in the room, their earnings would not depend on what others did.

We told them that in the experiment they would be asked to complete risk tasks and multiple-choice tasks, without mentioning how many of them there were. They were informed prior to responding to the choice problems that one of them would be randomly selected and their winnings determined by the option they chose. We used a random device for the resolution of risk in the event the option chosen in the selected problem was a lottery rather than a sure thing.

Instructions for each task stage were handed out one at a time[9]. Subjects

[7]Our experiment also had a separate "delay" treatment in which we manipulated whether the second risk stage is performed straight after the cognitive stage or at a second session taking place one week later. This treatment had no direct or interacted effect (our regression analysis adds controls for it). Therefore, for the sake of brevity, we omit its description.

[8]Participants were recruited using ORSEE (Greiner, 2004).

[9]Instructions are available as supplementary material (Online Resource).

were asked to read them through with the experimenter, who read them aloud. They experienced a risk task trial round before the "real" ones; the main purpose of this was to demonstrate the feature of the software that "enforces" a single switch point. Throughout the session, there was an experimenter in the room to answer any questions and to ensure that subjects knew how to run the computer program used to present the risk tasks and the cognitive test.

Payment was made at the end of the experiment. The average earnings for subjects in the "non-zero" increment condition were £14.61, with payoffs ranging from £10 to £23. Among those in the "zero" increment conditions the average earnings were £6.70, with payoffs ranging from £3 to £16.

3 Theoretical predictions

This section presents theoretical predictions for the effects of the money increment between risk-elicitation tasks on subjects' elicited measures of risk aversion.

We confine our attention to Expected Utility Theory. While Cumulative Prospect Theory (CPT) is one of the major alternative theoretical accounts of choice under risk, the features that make CPT more general than EUT and potentially better able to explain our data have a limited role in our experiment: *probability weighting* would not make much difference to our analysis of effects of the treatments; and because all lotteries are in the domain of gains, *loss aversion* can only play a role if subjects' reference point is located near or above the high prizes of our lotteries.

Thus, let us start by assuming that an individual has a utility function $u(.)$ whose domain is $(\underline{w}, \overline{w})$, a nonempty interval of wealth levels. Assume that $u(.)$ is strictly increasing, time-invariant, and twice differentiable. This implies that $u(.)$ is a continuous function such that lottery L has a certainty-equivalent. The certainty-equivalent of L, $C(L, w)$, is defined as the amount of money m such that $m \sim L$ at wealth position w, where $\sim$ is a relation of indifference. The amount by which the expected value of L exceeds its certainty-equivalent, $E(L) - C(L, w)$, will be referred to as risk premium. The risk premium depends on w and on L, and henceforth shall be denoted by $\psi(L, w)$. So, if the lottery L has expected value $E(L)$, $\psi(w, L)$ is the maximum reduction in $E(L)$ that an individual with wealth w would accept to make herself indifferent between the lottery L and such amount with certainty, that is

$$u[w + E(L) - \psi(w, L)] = u[w + C(L, w)] = pu(w + x) + (1 - p)u(w + y).$$

Consider now that the decision maker attaches the risk premium $\psi(w_0, L)$ to L when her wealth level is w_0. Let us assume for simplicity that when her wealth level is is soon to be w_1, she attaches $\psi(w_1, L)$ to L as if her wealth level were w_1 – which is very much in the spirit of the asset integration axiom of EUT. Let us also assume that $w_1 = w_0 + \Delta w > w_0$.

Proposition 1 *Let $\psi(w_0, L)$ and $\psi(w_1, L)$ be the risk premium a decision maker who obeys EUT assigns to a given lottery L before and after an increment of $\Delta w > 0$ has been given to him. Then $\psi(w_1, L) \gtreqless \psi(w_0, L)$ if, and only if, the decision maker displays increasing, constant, or decreasing absolute risk aversion, respectively.*

Proof *See Appendix A*

Proposition 1 states an intuitive result: under EUT, the changes in an individual's risk attitudes that the money increment Δw will induce, if any at all, are contingent upon the form of risk aversion exhibited by one's utility function.

4 Experimental Results

Our initial analysis of data focuses on the overall distribution of subjects' risk attitudes. We then examine whether and how the small-scale change in wealth induced by the experimenter affected risk-taking behaviour.

4.1 Elicited Risk Attitues

A subject's attitude to risk in a given risk task featuring lottery L is measured here by the risk premium $R(L)$, which is the difference between the expected value of the lottery L ($E(L)$) and the certainty-equivalent the subject assigns to L ($C(L)$); i.e., $R(L) = E(L) - C(L)$[10]. By taking into account the expected value of each lottery, this measure is to some extent "normalised" across lotteries with different stakes, making individual's elicited risk preferences readily comparable across risk tasks.

The majority of our subjects were systematically not risk averse throughout the risk tasks. Table 2 shows fractions of subjects in each distributional "class" of risk preference over the entire set of risk tasks. A subject is placed at class $[n, m]$ if she were risk averse in n risk tasks and risk neutral/loving in m, where $n + m = 12$. Very few displayed risk aversion in more than half of all twelve risk tasks. Table 2 shows, for instance, that 77.36% of all individuals in our experiment made either risk-neutral or risk-loving choices in at least 3/4 of all risk tasks. Less than 5% were systematically risk averse in more than half of the risk tasks.
We claim that the data presented in Table 2 support the following

[10]From now on, $C(L)$ is taken to be the midpoint of the switching interval.

Table 2: Distributional classes of risk preferences in all risk tasks

Distributional class of risk preferneces	Frequency	%	Accumulated
[0,12]	47	44.34	44.34
[1,11]	14	13.21	57.55
[2,10]	13	12.26	69.81
[3,9]	8	7.55	77.36
[4,8]	11	10.38	87.74
[5,7]	6	5.66	93.40
[6,6]	2	1.89	95.28
[7,5]	1	0.94	96.23
[8,4]	-	-	-
[9,3]	2	1.89	98.11
[10,2]	1	0.94	99.06
[11,1]	-	-	-
[12,0]	1	0.94	100.00

Note: An individual is placed in class [n, m] if, considering all twelve risk tasks, she displayed risk aversion in n risk tasks out of twelve; and either risk neutrality or risk lovingness in m risk tasks.

Finding 1: *The great majority of subjects' choices exhibit non-risk-averse behaviour in all risk tasks.*

4.2 Wealth effects

We now test the hypothesis that changes in risk premia of subjects in *nonzero increment* condition subjects are not significantly different from changes in risk premia of subjects in a *zero increment* condition. We start with some non-parametric tests. Table 3 reports the results of Mann-Whitney and Wilcoxon Signed-rank tests. Tests are performed for each risk task[11], as it is of interest to see whether potential wealth effects on attitudes to risk are robust to risk tasks involving different lottery prizes and probabilities. The results do not show any systematic differences between those who knew £7 was guaranteed at the end of the experiment and those who were not expecting such extra gain (between-subject analysis). Test results show that we cannot reject the hypothesis that there are no systematic differences between measures of risk elicited before and after the increment (within-subject analysis).

Are these results robust to some individual controls? In order to ex-

[11] $L3$ and $L6$ are pooled as they are identical.

Table 3: Effects of monetary gain on attitudes to risk, within- and between-subjects

Risk Tasks	*Within-subject*	*Between-subject*
	Wilcoxon signed-rank test	Mann-Whitney two-sample test
L1 *(£8,0.2;£4)*	z = 1.34 *p = 0.18*	z = 0.585 *p = 0.55*
L2 *(£9,0.2;£3)*	z = 0.94 *p = 0.35*	z = 1.542 *p = 0.123*
L3/L6 *(£6,0.4;£3)*	z = -0.97 *p = 0.33*	z = -1.93 *p = 0.23*
L4 *(£9,0.3;£4)*	z = -1.00 *p = 0.32*	z = -0.827 *p = 0.41*
L5 *(£16,0.2;£10)*	z = -0.72 *p = 0.47*	z = 0.045 *p = 0.96*

Note: Wilcoxon signed rank sum test: the null hypothesis is that before- and after-treatment measures of risk aversion (risk premia) from subjects assigned to the non-zero increment condition are not significantly different. Mann-Whitney two-sample test: the null hypothesis is that changes in attitudes to risk (variation in risk premia in a given risk task) across stages among *treated* (Δw=7) and *untreated* (Δw=0) subjects are not different. Tests are performed on aggregated sample.

amine that, we regress individuals' risk premia on individual and structural parameters of the experiment. With the panel data structure of our dataset, we can now look at the same issue not only exploiting the heterogeneity within a given subject's sequence of risk aversion measures, but also controlling for fundamental characteristics of the experiment and some observed demographics[12]. To this end, we will implement the following panel data regression specification:

[12]The sample used in our regression analysis, when the model used to estimate risk behaviour includes controls for treatment conditions and income class, is slightly different (102 subjects) since we excluded some subjects with missing income data.

$$
\begin{aligned}
y_{it} &= b_1 INCREMENT_i + b_2 DELAY_i + b_3 EXPECTVAL_{it} \\
&\quad + b_4 ROWS_{it} + b_5 L1L5ORDER_i + +b_6 LOWINCOME_i \\
&\quad + b_7 FEMALE_i + b_8 AGE_i + b_9 POSTGRAD_i + b_{10} OVSCORE_i + u_{it}
\end{aligned} \tag{1}
$$

where y_{it}, the risk premium derived from subjects' choices in each risk task, is the dependent variable; the set of regressors mostly include dummies for characteristics of the experiment as well as for subject-specific characteristics:

1. $INCREMENT$ is a dummy variable for whether i received the money increment;

2. $DELAY$ is a dummy variable for whether i is assigned to the delay treatment;

3. $EXPECTVAL$ is the expected value of the lottery option in the risk task faced in period t;

4. $ROWS$ is the number of decision rows in the risk task i faces in period t;

5. $L2L5ORDER_i$ is a dummy for the order in which the risks involving lotteries $L2$ and $L5$ were faced[13];

6. $LOWINCOME$ is a dummy equal to one if i said that her average monthly income is less than £$1,000$; We use this information to control for wealth effects due to income differences outside the lab.

7. $OVSCORE$ is the the overall score in the cognitive test;

8. $FEMALE$, and $POSTGRAD$ are two dummies: they are equal to one if i is female (postgraduate student), respectively.

9. AGE is the i's elf-reported age. u_{it} is a composite error term including a random intercept that captures subject-specific effect and a overall disturbance term assumed to be i.i.d over i and t.

We use a generalized least square random effects estimator to fit (1). In Table 4, we report the estimation results for this specification. The fact

[13] We randomised across subjects the order those two lotteries were faced. We did this to test for order effects in relation to the L2/L5 comparisons. Note that, while in a very moderate scale, this randomisation can also be seen as a partial test of order effects in our sequence of risk-elicitation tasks, as a full test for all possible sequence with which the tasks could be faced would be cost prohibitive.

that the coefficient in front of $INCREMENT$ is not statistically significant suggests that risk attitudes, as measured by the lottery risk premium, are not influenced by the scale of the prior gain received. The estimates also reveal that the effect of scaling up the stakes of the lottery option in the risk tasks – reflected in its expected value – is to decrease elicited measure of risk aversion.

Estimates showed that an increase in the number of rows in a risk task tended, on average, to reduce subjects' risk premia. The coefficient in front of $ROWS$ is negative and statistically significant. Recall that risk tasks with more decisions rows have larger stakes, so the coefficient of the number of rows variable captures the effect of stake size on risk attitudes. This is consistent with the sign of the coefficient of the expected value variable, which also reflects the size of the lottery stakes. The remainder of the variables, including most demographic controls, are not statistically significant. Thus, our regression analysis confirms the non-parametric tests. Altogether, they support the following finding.

Finding 2: *Elicited risk attitudes do not seem to be affected by prior money given to subjects.*

This result contrast with previous studies reporting a "house-money" effect: a change of risk preferences that is induced by money given prior to risky choices. This is consistent, however, with the idea that individuals adopt a narrow frame, simply not merging prior gains with the potential consequences of taking a given risk (Barberis *et al.* , 2006). Thus, the money given to subjects does not induce changes in their attitudes to risk. It is also worth noting that, as the money given to subjects in our experiment is administered in between risky tasks, these experimental results suggest that the effects a monetary gain may have on individuals' risk preferences may be more sensitive to prior experience with the risk-elicitation task than previously thought.

5 Conclusions

This paper reports on an experiment designed to examine the effects of small-scale changes in wealth on risk attitudes.

We have observed that risk attitudes do not seem to be systematically affected by the small-scale change in wealth. The experimentally induced increment is modest, but it was larger than the expected value of almost all lotteries used in our experiment. Theoretically, and from a EUT standpoint, this result suggests that overall subjects display risk attitudes consistent with constant absolute risk aversion. This support a key assumption in a recent literature over calibration critique of decision theories (Rabin, 2000; Cox & Sadiraj, 2006; Wakker, 2005; Safra & Segal, 2008), namely, that

Table 4: GLS estimates of a random-effects model, with the risk premium implied by the subject's choices as the dependent variable

Variable	Description	Estimate (Mean Effects)	Standard Error	p-value	Lower 95% Confidence Interval	Upper 95% Confidence Interval
Constant		0.45	0.81	0.58	-1.13	2.03
	Treatments					
delay	Delay condition	0.07	0.11	0.56	-0.16	0.29
increment	Increment condition	-0.12	-1.00	0.31	-0.35	0.11
evalue	Expected value	-0.05	0.01	0.00	-0.07	-0.03
rows	Number of rows (binary choices) in risk task	-0.04*	0.01	0.00	-0.05	-0.03
L1-L5 order	Second and fifth tasks order	-0.05	0.12	0.65	-0.28	0.18
	Individual characteristics					
female	Female	-0.06	0.12	0.61	-0.29	0.17
age	Age	0.01	0.04	0.78	-0.06	0.08
postgrad	Taking some postgraduate education	-0.08	0.13	0.53	-0.35	0.18
IncLow	Lower level income	0.01	0.12	0.95	-0.23	0.24
ovscore	Overal score in cognitive test	-0.01	0.03	0.70	-0.08	0.06
σ_u	Standard deviation of individual effect	0.51				
σ_e	Standard deviation of residual	0.74				

Note: 1.188 observations based on 102 subjects. Delay condition takes value 1 if subject is assigned to the treatment conditions in which second stage takes place one week later; takes 0 otherwise. Increment condition takes value 1 if subject is assigned to the treatment conditions in which subjects learn, at the end of the first stage, that £7 is already guaranteed at the end of the experiment; takes 0 otherwise. L2-L5 order takes value 1 if L2 was faced before L5 and 0 otherwise. * 1% significance.

attitudes toward a risk do not change over a given range of wealth levels. This result is also consistent with a narrow bracketing of problems, whereby individuals tend to evaluate new gambles they are offered in isolation from other wealth-relevant events.

As a by-product, this result also raises the question of whether the "house-money" effect – an increase in one's willingness to take risks induced by the receipt of a prior gain – is a genuine behavioural phenomena or an experimental artefact. The sum of money given to subjects was administered in between the risk elicitation stages and in a way to induce them to think the money given was a genuine earning rather than a windfall gain granted by the experimenter (as a reward for completing a cognitive test). While our experiment does not intend to disentangle this question and the increment administration may not have legitimised the money given with effort, we think the results suggest that the effects a monetary gain may have on individuals' risk preferences may be more sensitive to prior experience with the risk-elicitation task, or the framing of the money, than previously thought.

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Appendix

Proof of **Proposition 1**

We provide a demonstration for the case where $\psi(w_1, L) > \psi(w_0, L)$. Proofs for the other cases use similar arguments and are therefore omitted.

We first prove that $\frac{\partial R_A(w)}{\partial w} > 0 \Rightarrow \psi(w_1, L) > \psi(w_0, L)$, where $R_A(w) \equiv -u''(w)/u'(w)$ are Arrow-Pratt local measures of absolute risk aversion.

Assume that $\frac{\partial R_A(w)}{\partial w} > 0$ for all $w \in [\underline{w}, \overline{w}]$, where $0 \leq \underline{w} < \overline{w}$. Assume that $u(.)$ is monotone and strictly concave over $[\underline{w}, \overline{w}]$. Consider that u_0 and u_1 are the utility function evaluated at w_0 and w_1, respectively, where w_0, $w_1 \in [\underline{w}, \overline{w}]$. Since $\frac{\partial R_A(w)}{\partial w} > 0$ and $w_1 > w_0$, we can infer that the decision maker is more risk averse at w_1 than at w_0, that is, $-u_0''/u_0' < -u_1''/u_1'$. In this case, and at least over a closed ball with center w_1 and radius $r \geq w_1 - w_0$, we can see u_1 as a concave transformation of u_0, that is, $u_1 = \phi(u_0)$ where ϕ is a monotone and strictly concave function. Observe now that

$u_1(w + E(L) - \psi(w_1, L)) = E[u_1(w + L)]$ (by risk premium definition)

$E[u_1(w + L)] = E[\phi(u_0(w + L))]$

$E[\phi(u_0(w + L))] < \phi(E[u_0(w + L)])$ (by Jensen's inequality)

$\phi(E[u_0(w + L)]) = \phi(u_0(w + E(L) - \psi(w_0, L))$

$\phi(u_0(w + E(L) - \psi(w_0, L)) = u_1(w + E(L) - \psi(w_0, L),$

This implies, by monotonicity of u_1, that $\psi(w_1, L) > \psi(w_0, L)$. This completes the first part of the proof.

We now have to prove that $\psi(w_1, L) > \psi(w_0, L) \Rightarrow \frac{\partial R_A(w)}{\partial w} > 0$. We do so using a simple argument. Let A be the statement that $\frac{\partial R_A(w)}{\partial w} > 0$ and B that $\psi(w_1, L) > \psi(w_0, L)$. Assume that $(\sim A)$ holds. If that is the case, then we know that it cannot be true that $-u_0''/u_0' < -u_1''/u_1'$. From the first part of the proof, we know then that $(\sim A)$ implies that $\psi(w_1, L)$ cannot be greater than $\psi(w_0, L)$. Thus, as $u(.)$ is strictly concave, it must be that $(\sim A) \Rightarrow (\sim B)$. Hence, $B \Rightarrow A$. This completes the proof.