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Investment behaviour when agents' influence on the success probability is hard to detect: An experiment*

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Abstract

In the context of investment decisions, "contingency" refers to the influence agents may exert over the probability distribution of returns on investment. Often, contingency is difficult to detect and investment decisions are influenced by recent experience of (non-)contingency. To investigate the behavioural influence of prior (non-)contingency on investment decisions, we conduct an economic experiment in rural Uganda. Subjects are asked to invest any amount they wish of their endowment, with success dependent on whether they are correct in detecting the heavier of two objects. In one task, there is contingency: trying hard to detect the weight difference should influence the success probability. In another version, there is non-contingency: the weight difference is below the differential that humans are able to perceive. To investigate the effect of prior experience of (non-)contingency we experimentally vary the priming of (non-)contingency with a guessing game organised before the investment tasks. Our main finding is that priming contingency raises investment in the contingency condition. We find in addition that stated perceptions of confidence are also affected by priming contingency. In both cases, the effect is mediated by individuals' risk aversion. Individuals who are less risk averse respond more positively to priming contingency. We conclude that alertness to contingency matters for investment decisions, the more so the less risk averse people are.

Keywords: Contingency, learned helplessness, investment, risk preferences, lab-in-the-field experiment, rural Uganda

JEL Classification: C93, D03, D81, O13

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1. Introduction

People often take investment decisions for which their influence on the success probability is difficult to know. Students can influence the probability of obtaining a good job through their choice of degree course and university, but how and by how much is partially obscure to them. Farmers are more likely to obtain a good yield by selecting seed suitable for their soil and obtaining and following the best agronomic advice. This will surely influence the success probability, but its precise influence depends on many factors, which are typically not fully known or understood. And many other examples could be given, since it is rare that certainty is obtainable to the decision-maker on the relationship between applying discernment and effort on the one hand and the probability distribution of returns on investment on the other.

In this study, we are interested in investment behaviour when agents' potential influence on the success probability is not obvious. In particular, we ask whether recent experience of (lack of) influence on the success probability affects investment behaviour in new situations. In order to conceptualise this research interest, we borrow from the experimental learned-helplessness literature in psychology. In this literature, the word "contingency" is used to describe a task where the subject's actions affect the probability of reward (e.g. escape from an unpleasant noise), whereas non-contingency is a situation where there is no link between actions and outcomes. In learned-helplessness experiments, some subjects first encounter a problem that cannot be solved through their own efforts, such as an impossible puzzle. Exposure to the impossible problem lowers persistence and problem solving ability in subsequent trials even when the subsequent problems can actually be solved.[1]

We study investment behaviour in an economic experiment and adapt several design elements from this programme of research. We make contingency salient in some treatments and non-contingency in others in a between-subject design. For this we use a priming task, based on (Cohen et al., 1976), who introduced non-noxious non-contingent tasks into learned helplessness experiments. The task consists of a card guessing game in which, in one treatment, the rule determining the correct card was straightforward, in two other treatments, the rule was complex or confusing, and a fourth treatment had no

[1]Many of the initial experiments involved electric shocks (Thornton and Jacobs, 1971) or other physical irritants such as dissonant noises (Hiroto, 1974). In one treatment pressing a bar could give relief; in the other there was no working bar. Subjects were subsequently placed in a similar situation and exposed to further unpleasant stimuli. Subjects who had learned to be helpless were slower to shut off the stimulus, compared to subjects who had faced the working bar (Diener and Dweck, 1980). Later experiments have usually dispensed with the painful tasks and typically involve puzzles, word problems and similar challenges. These early experiments have inspired a vast research literature on learned helplessness in psychology; see Overmier (1996, 2002) for accounts of the evolution of the discussion. (Abramson et al., 1978) is probably the most influential conceptualisation of learned helplessness.

prime.

The device we use for capturing contingency in investment behaviour is an investment task in which subjects are asked to invest any amount they wish of their endowment, with investment success dependent on whether they were correct in detecting the heavier of two objects. In one version of the task, there is contingency: trying hard to detect the weight difference should influence the success probability. In another version, there is non-contingency: the weight difference subjects are asked to detect is, according to psychophysics (Gescheider, 1997), imperceptible. The tasks are superficially identical, but the probability of investment success in one task depends on careful discernment of the profitable way forward, whereas in the other task the success probability is independent of subjects' attempts to discover the correct course of action. We call the former the "contingency condition", and the latter the "non-contingency condition". The average success probability between the two experimental conditions differs substantially: it is about 50 per cent in the non-contingency condition and about 60 per cent in the contingency condition.

We combine both treatment variations in a 2 by 4 factorial design - where the '2' refers to within-subject variation in the investment tasks, the order of which is randomised, and the '4' refers to between-subject variation in the priming of contingency or non-contingency. With this design, we thus investigate whether the prior experience of (non-)contingency affects subjects' investment decisions where (non-)contingency is present.

Our key finding is that priming contingency raises investment in the contingency condition. When people have recently experienced contingency, in an unrelated task, then they commit a larger share of their endowment to an investment characterised by contingency. Their alertness to contingency appears to be heightened as a result of the priming, even though the priming task (guess the correct card) bears little resemblance to the investment task (investment succeeds when the heavier object is detected). We thus show that, in the context of investment decisions, people are more alert to the presence of contingency when they have recently experienced contingency.

The relevance of detecting contingency in investment situations may be illustrated by reference to the livelihoods decisions our subjects, Ugandan farmers, have to take. In rural areas of developing countries, many factors interfere with or may obscure the link between an investment (e.g. a new fertiliser) and an outcome (e.g. yields or farm profits): the weather, pests and diseases, erratic price fluctuations, inappropriate, ill-understood or fake agricultural inputs, predatory governments and/or rebel movements, among other factors (Fafchamps, 2003; Dercon, 2008). In such a "noisy" decision-making environment, the prevalence of rewards for exploration – for establishing the degree of control one truly has over an outcome – may be relatively low (Teodorescu and Erev, 2014). Contingency is costly to detect and as a result, previous experience of contingency and non-contingency may guide investment decisions when farmers are faced with a new prospect, even in circumstances where past experience is an unreliable guide to the controllability of events in the problem currently faced.[2]

The identified effect of recently experienced contingency on people's alertness to the presence of contingency in a subsequent task bears resemblance to two important strands of literature in economics. First, our finding is relevant for the large literature in economics on subjective probabilities. In that literature, intuitive reasoning, which is often "good enough" for obtaining reasonably precise estimates of probabilities, is shown to lead to serious errors in certain conditions (e.g. Plous, 1989; Charness and Levin, 2005; Fox and Clemen, 2005; Dohmen et al., 2009). The typical source of error in that literature is the use of heuristics.[3] By contrast, the subjective probabilities that we consider stem not from heuristics but from the error-prone attempt to detect the degree of contingency in certain situations.

Second, it is worth considering some similarities between our experiment on contingency and investment behaviour and recent research on the link between locus of control and effort in a variety of economic situations. Locus of control refers to a belief in the general controllability of events (James and Rotter, 1958; Smith et al., 1995). Individuals who believe that, generally, probabilities of success and failure are outside their control are referred to as having an external locus, while those who have a stronger belief in their ability to control probabilities are described as having an internal locus. The surge of interest in locus of control within economics has been particularly notable in the field of labour economics where it is often measured using a standard scale and then used as a variable that can explain variation in educational investment and job search (e.g. Lekfuangfu et al., 2018; Caliendo et al., 2015) or investment in children (Lekfuangfu et al., 2018). The related experimental work by McGee and McGee (2016) explores the relationship between locus of control and search in a word-number puzzle task. As with the non-experimental papers just mentioned, it takes locus of control as a given.

Research on contingency is clearly related to that on the locus of control but the two concepts are distinct. Contingency refers to the influence agents may exert on the

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[2]As Denrell and March (2001) point out, in their discussion of the 'hot stove' effect, past experience of non-contingencies may lead to individuals abandoning efforts in a new situation prior to obtaining accurate information about whether it is also non-contingent.

[3]These heuristics were introduced in Tversky and Kahneman (1974). For example, when adopting a so-called "availability heuristic", people sometimes overestimate the relative frequency of objects or events because they are more easily brought to mind, as in words that start with the letter r compared to words that have r as their third letter (ibid. p.1127).

probabilities of outcomes. Especially in stochastic environments, contingency may be costly to detect and beliefs about contingency may be influenced by past experiences of apparently similar environments. Locus of control may well affect people's inclination to detect contingency but is not synonymous with it. In concrete terms, the key difference of our paper from the existing economics literature is that we experimentally manipulate contingency rather than treating it as a given.[4]

The remainder of the paper is laid out as follows. In Section 2, we present our experimental design. In Section 3, we describe our fieldwork implementation, including sample selection and sample characteristics. Section 4 contains the results, Section 5 discusses them and concludes.

2. Design

To investigate whether investment behaviour is influenced by the prior experience of contingency or non-contingency, we use an experiment in which we manipulate the sense of contingency by using a priming task that varies across four between-subject treatments (see below). Thereafter, all subjects invest twice, once when the success probability is 50% and labelled as outside their control (the non-contingency condition), and once when it is well above 50% if careful discernment is applied (the contingency condition). If we find that priming contingency or non-contingency affects the investment decisions, then we infer that the subjective success probability is affected by alertness to contingency.

2.1. Subjective probabilities in the standard portfolio problem

Each of the investment decisions we consider in our experiment is a version of the standard portfolio problem. This is a classical problem in the economics of uncertainty, with many real-life applications (Gollier, 2001, Ch.4). In the standard portfolio task, an agent decides how much of a sure wealth to invest in a risk-free asset and in a risky asset. In our experiment, subjects' initial endowment is equal to E, $E - \alpha$ of which is invested in the risk-free asset and α in the risky asset. The return of the risk-free asset is zero in our experiments, so that the portion $E - \alpha$ of the endowment not invested in the risky asset is simply retained. The return of the risky asset is a random variable $\tilde{y}$. The value of the portfolio is equal to $E - \alpha + \alpha\tilde{y} = E + \alpha(\tilde{y} - 1)$, which the agent decides on by choosing α, subject to the constraint $0 \leq \alpha \leq E$. The expected value of $(\tilde{y} - 1)$ is set to be greater than zero in our experiment or risk-averse decision makers would not invest.

[4] Preuss and Hennecke (2018), which examines how unemployment affects locus of control is one clear example of a non-experimental paper that does explore how life events can affect the detection of contingency.

In our experiment, the probability distribution of $\tilde{y}$ is a two-point distribution: with probability of success p the investment is multiplied by a factor k and with probability $(1-p)$, the investment is lost. Subjects thus face the prospect $[p,\ E+\alpha\,(k-1)\,;(1-p)\,,\ E-\alpha]$, in which α is freely chosen subject to the constraint mentioned.

However, subjects believe that they face prospect $[p^*,\ E+\alpha\,(k-1)\,;(1-p^*)\,,\ E-\alpha]$, since the success probability is not unambiguously known. Experiments are designed to create conditions that we expect to cause the perceived probability p^* of success to deviate from p. The way we test for discrepancies between p and p^* is, in a between-subject design, to manipulate conditions for p^* in various ways while holding p constant. If investment responds to treatment, then we infer that p^* is sometimes a biased estimate of p.

2.2. Experiment

To manipulate a sense of (non-)contingency, we use a card guessing game. The game is similar to priming tasks used in psychology experiments (e.g. Feather (1961), Cohen et al. (1976)), but adapted for a low literacy environment. Subjects are presented with ten pairs of cards, one pair at a time, and are asked to guess which card is correct, according to a rule that we do not share with them. So subjects are told that there is a rule according to which one card is right and the other wrong, but we do not tell them what that rule is. Rather, after each guess, subjects are told whether they guessed correctly, but no further feedback is given. The pairs of cards shown to subjects is depicted in Figure 1.

Using this task, we implement four treatments (see Table 1) with each participant randomly assigned to only one of the treatments. These treatments vary in the rule that is used to determine which card is right. In treatment T1, the rule is "the card with the bird is correct". This means that if subjects are shown the first card and point to the card on the left, then they will be told that they identified the correct card. After the feedback, the second pair will then be shown (see Figure 1 for all cards). This rule was designed to be straightforward and bring about an elevated sense of contingency.

Table 1: Summary of cards treatments

Treatments	**Rule**	
T1: Straightforward rule	Card with the bird is correct	Contingent
T2: Complex rule	Card is correct if it has a red letter at the top; otherwise, the card with a yellow letter is correct	Non-contingent
T3: Confusing rule	Any card with a letter is correct	Non-contingent
T4: No priming task	No cards used	No priming

Two other rules were designed to bring about an elevated sense of non-contingency, in different ways. In both treatments T2 and T3, the rule was construed such that subjects would be right or wrong for no apparent reason related to their mental exertions. In treatment T2, that is the case because the rule is complex; in treatment T3, that is the case because the rule is trivial, since in effect, there are no wrong answers. In other words, in treatment T2 we prime non-contingency by complexity and in treatment T3 we prime it by triviality. Treatment T4 has no card-guessing game, which we use as control treatment. A comparison between this control treatment and the treatments that prime contingency or non-contingency allows us to identify the effect of the experience of contingency and non-contingency, respectively.

Figure 1: Pairs of cards shown to subjects (one pair at a time)

The card guessing priming task is followed immediately by two investment tasks, which each subject plays. Both of these are based on a weighing task, which involves the identification of the heavier of two small containers, weighing about 35g each. An investment out of 20 counters, each worth 400 Ugandan shillings, is tripled if the correct container is identified to be the heavier one, and lost if not. That is, k=3. The containers are transparent, and filled with locally popular nuts (ground nuts). The key difference between the two games, is that in one condition, the contingency condition, the heavier container is 2g heavier than the lighter one, whereas in the non-contingency condition, the weight difference is .2g. According to evidence from psychophysics, the latter difference cannot be detected, whereas many people, with effort, can detect the former difference in weight for a base weight of about 35g (Gescheider, 1997, Ch.1). We confirm this in our experiment (see the next section). Importantly, we tell subjects these facts - both the difference in weights in the two tasks and the fact that a 0.2g difference is all but non-detectable, whereas a 2g can be sensed by many people. For example, when subjects play the 2g task they are told (the experimental instructions are in the online appendix):

> *'We will show you later that one is heavier, using the machine that can detect small weight differences. The difference is small, but we know that if you try hard enough, many people can tell the difference. We really believe that not only machines can tell the difference in weight but many people can too, if they try hard.'*

We refer here to a 'machine', which in the actual experiment is a 'digital balance'. To this we add the (truthful) information that:

> *'We know this because we have tried this with hundreds of people before and two out of three people correctly decide which container is heavier.'*

On the other hand, with the 0.2g task the subjects are told:

> *'We really believe that it is impossible to tell the difference in weight between these two containers; the difference is too small for a person to feel it with their own hands. We will show you later, using the machine that can detect very small weight differences, that one is heavier but whether or not you get it right is a matter of luck. Why do we say that it is a matter of luck? Because we really believe that machines can tell the difference in weight, but people cannot.'*

To this we add the (truthful) information that:

> *'We believe this because we have tried this with hundreds of people before and they guess the wrong container as often as they guess the right one.'*[5]

In short, if subjects believe the message about the weight differences, then investment levels should be higher in the 2g task. In addition, if the priming influences alertness to

[5]We used the opportunity provided by other experiments to try out these weighing tasks in other villages in Uganda.

contingency, then the investment made in the 2g task should be higher in treatment T1 and lower in treatments T2 and T3 compared to treatment T4.[6] In contrast, in the 0.2g task the priming should not make a significant difference to investment behaviour. We are thus able to examine the influence of a manipulated sense of contingency on investment behaviour in situations in which we have purposively created scope for p^* to deviate from p.

Subjects are randomly assigned to a card guessing treatment, are informed that only one of the subsequent tasks will be randomly chosen for payment, and play both investment tasks in a randomly determined order. Incentives are substantial: the initial endowment of 8,000 shillings represents about two daily wages, so tripled would be about six daily wages.

After respondents have taken the two investment decisions, we also elicit a measure of confidence that the chosen container is correct. For each of the tasks in turn we present the subject with two boxes, one marked "correct" and one marked "incorrect" and with each containing 50 counters. We tell subjects:

> *'In each of these boxes are 50 counters. That is supposed to indicate that there is a 50/50 chance of you identifying the heavier container. If you feel more confident than that, you can move as many counters as you like from the "incorrect" box to the "correct" box. If you feel it is just a matter of chance, then you leave it as 50/50. But the more confident you are, the more counters you should move to the "correct" box.'*

We do this to gain some insight into whether the subjects actually viewed the two tasks as significantly different. At the same time, we made it unincentivized to avoid raising the complexity of the experiment.

2.3. Hypothesised treatment effects

The main idea of the study is that contingency is sometimes difficult to detect and when it is, may be mistaken for non-contingency, especially if contingency is not salient. Our

[6] It is worth noting that the expected channel of influence is through contingency being salient, not greater accuracy in detecting weight differences. Prior research in psychology is equivocal about whether we should expect higher accuracy in treatment 1 compared to treatments 2, 3 and 4. Eisenberger (1992) notes that many early experiments do not produce evidence of 'learned industriousness' (e.g. Maier and Seligman, 1976), but when there is no effective ceiling on performance (e.g. Eisenberger et al., 1976) priming contingency is associated with higher effort and performance. In our case, we cap the evaluation time for the task at 30 seconds and, anyway, it is not obvious that further time would improve subjects' accuracy. Thus we have no strong prior that treatment 1 should produce higher accuracy than treatments 2, 3 and 4. What we expect instead is that, for given accuracy in detecting a weight difference, greater salience of contingency makes it more likely that the detected weight difference translates into higher investment.

design allows us to test whether the prior experience of (non-)contingency affects subjects' evaluation of the probability of investment success, which in turn influences their investment decisions. We next spell out the hypothesised treatment effects.

We begin with the situation in which contingency is salient, i.e. treatment T1. We expect that the prior experience of contingency (which makes contingency salient) would influence investment decisions in an investment environment where contingency is present. We therefore expect that recently experienced contingency (T1) heightens alertness to contingency, and therefore raises investment in the 2g investment game. By contrast, when contingency has been recently experienced (T1) but no contingency is currently present (0.2g game), there is no reason to suppose that non-contingency would be mistaken for contingency. This translates into our first hypothesis.

Hypothesis 1 *Priming contingency in the cards guessing game (T1) increases investment in the investment game where contingency is present, i.e. the 2g condition, and does not affect investment in the investment game where non-contingency is present, i.e. the 0.2g condition.*

To test hypothesis 1 we compare investment behaviour in the contingency condition (the 2g game) between participants allocated to treatment T1 in which we prime contingency and treatment T4 in which no priming is used; and compare investment behaviour in the non-contingency condition (the 0.2g game) between T1 and T4.

We next consider the situation in which non-contingency is salient, i.e. treatments T2 and T3. At the outset it should be noted that we have no expectation about the differential impact of T2 and T3. Based on our reading of the relevant psychology literature (reviewed above), we considered that non-contingency may result both from problems that are too difficult to solve and from problems that are too easy to solve. We decided to implement the two alternative sources of non-contingency as something that is empirically interesting (whether complexity or triviality matters more) without having clear priors on which matters more.

When non-contingency is salient because it has been experienced recently (T2 and T3), it is plausible that contingency is not correctly detected. Investment in the contingency condition (the 2g game) would then be lower than when no prime was used. By contrast, there is no reason to suppose that non-contingency would be incorrectly detected. Investment in the non-contingency condition (the 0.2g game) should therefore not be affected. This translates into our second hypothesis.

Hypothesis 2 *Priming non-contingency in the cards guessing game (T2 and T3) decreases investment in the investment game where contingency is present, i.e. the 2g con-*

dition, and does not affect investment in the investment game where non-contingency is present, i.e. the 0.2g condition.

To test hypothesis 2 we compare investment behaviour in the contingency condition (the 2g game) between participants allocated to treatments T2/T3 in which we prime non-contingency and treatment T4 in which no priming is used; and compare investment behaviour in the non-contingency condition (the 0.2g game) between T2/T3 and T4.

2.4. The sample

The site chosen for the experiment was Sironko and Lower Bulambuli, which together are former Sironko district in eastern Uganda (see Iversen et al. (2010) or Verschoor et al. (2016) for more detail on this district). Approximately 300,000 people live in the selected region of around 1270 square kilometres, 95% of whom are small-scale farmers. Within the site, we purposively selected five sub-counties and within each sub-county we then randomly selected 10 villages. For each selected village we compiled a list of all adult (over 18) village members and from that list randomly selected up to 20 adults (with no more than one person per household).[7] After the experiment we conducted a livelihoods survey with each participant.

Overall, we have 395 subjects, which were randomly allocated to the four card guessing treatments (T1 to T4). Table 2 provides descriptive statistics of relevant socio-economic characteristics of the participants by treatment. We observe that all between-subject treatments - i.e. the four card treatments and the order of the investment tasks - are balanced across nearly all characteristics.

Table 2: *Descriptives*

	T1	T2	T3	T4	p[a]	2g first	0.2g first	p[a]
Primary education	54.08%	55.56%	45.45%	56.57%	0.381	50.23%	56.04%	0.249
Male	42.86%	51.52%	44.44%	48.48%	0.609	49.30%	43.96%	0.289
Age	38.11	39.59	40.31	39.52	0.737	39.26	39.53	0.852
Land (acres)	2.53	2.27	2.17	2.48	0.497	2.29	2.46	0.375
Household size	3.10	3.27	3.01	3.26	0.699	3.01	3.34	0.084*
Risk aversion	2.67	2.73	2.75	2.76	0.886	2.80	2.64	0.066*

Notes: Household size is measured in number of persons between 15 and 70 years old. [a] For continuous variables we report the two-sided p-value of an ANOVA test, while for categorical variables we report the two-sided p-value of a chi-square test. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively.

[7]Some of these were randomly allocated to experiments not reported on here, others to the experiment that this study is about. No subject participated in more than one experiment.

Nearly all subjects answered the control questions correctly: 382 out of 395 (i.e. 96.71%). In the priming task, the mean number of correct answers (identification of the correct card) for subjects in Treatment 1 (2) is 7.98 (5.88). In Treatment T1 (T2) the mean number of right answers in the final five questions was 0.41 higher (0.46 lower) than in the first five and 81 (49) subjects out of 98 (99) performed no worse in the last half of the priming task. Thus performance in Treatment T1 was significantly higher than in Treatment T2 (p=0.000, medians test) and the gain between the first and second half was also significantly greater (p=0.023, median). By the end of the task, 75 subjects in Treatment T1 claimed to know the correct rule, while only 5 in Treatment T2 were confident enough to make the same claim. Performance was also higher in Treatment T1 for those who claimed to know the rule (8.56 versus 6.09; p=0.000, medians test), whereas the hypothesis of no difference in median scores for claiming to know the correct rule is not rejected for Treatment T2. On this basis we claim that the priming worked effectively for Treatments T1 and T2.[8]

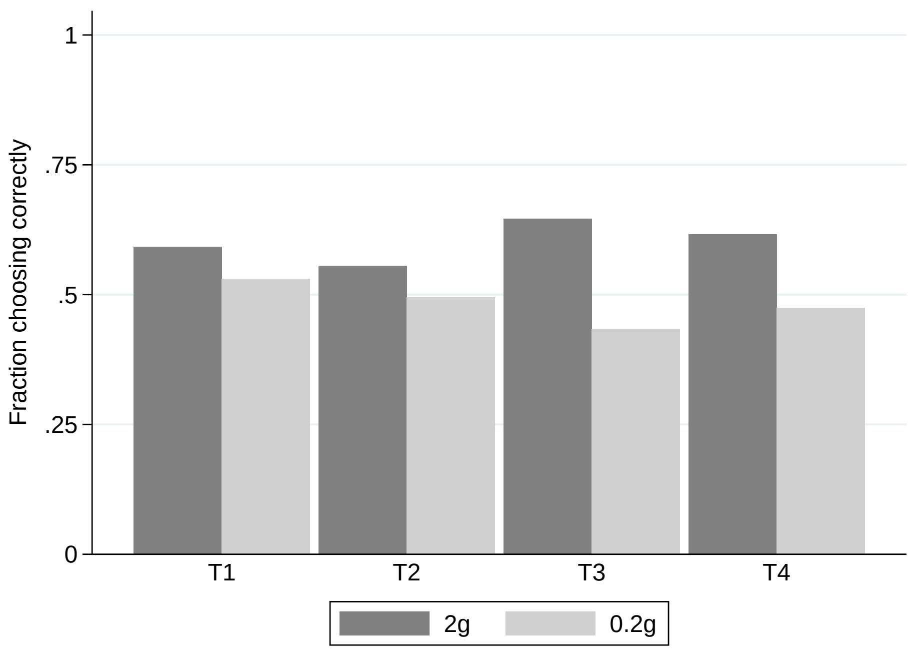


Figure 2: Fraction choosing correctly, by treatments

The percentage that identified the heavier container correctly for the 0.2g difference was 48.5%, which is not significantly different from 50% (p=0.546, two-sided binomial test), whereas at 60.3% the percentage of correct guessers for the 2g difference was significantly different from 0.5 (p=0.000, two-sided binomial test). Figure 2 shows the breakdown by

[8]For Treatment T3 everyone answered all card-guessing questions correctly by construction, so we cannot make the same claims for that treatment.

treatment. In all treatments, the fraction choosing correctly in the 2g case was higher than the corresponding fraction for the 0.2g case. For treatments 3 and 4 the difference is statistically significant at the 5% level (McNemar's test, p=0.005 and p=0.039 respectively). Differences across treatments but within container type are not significantly different at the 5% level. For the 2g container, the percentage of accurate guesses was significantly above 50% for all but Treatment 2 whereas in the 0.2g case, in all treatments the difference from 0.5 was not significant.

3. Analysis

In this Section, we present the main empirical results. To test our research hypotheses, we compare investment levels across the four treatments, separately in the 2g and 0.2 investment tasks. We also analyse whether the experience of contingency or non-contingency has an effect on the confidence respondents have in their investment decision. In a next step, we use regression analysis to test the robustness of the treatment effects on investment levels. Finally, we investigate whether the treatment effects on investment levels and confidence in the investment interact with risk preferences.

3.1. Investment

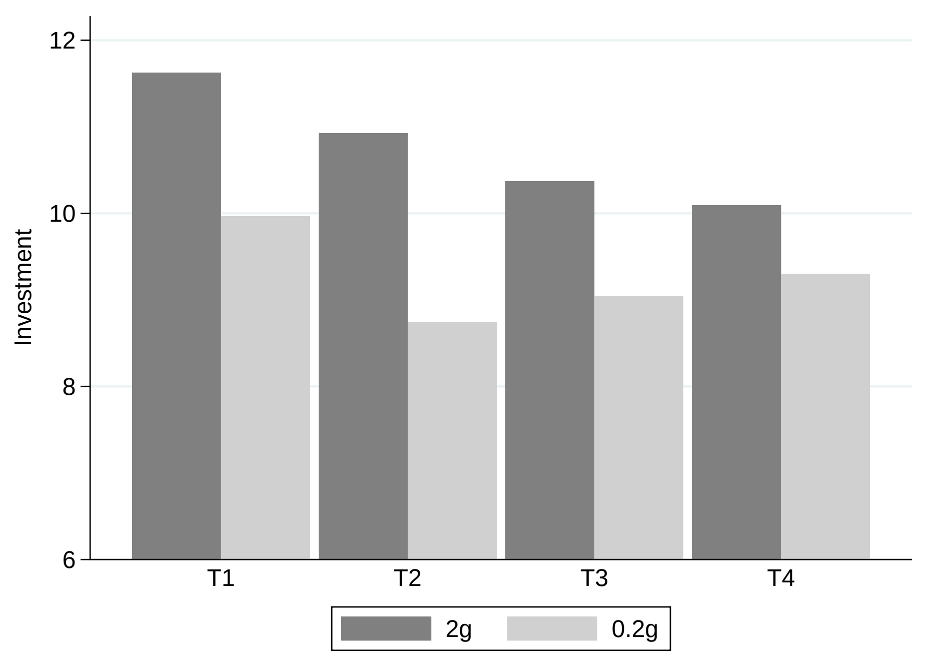


Figure 3: Investment levels, by treatments

In Figure 3 we observe that investment levels, as measured by the number of counters invested, are higher with 2g than with 0.2g, and this is true for all the cards treatments. Using a paired t-test, we find that within-participant differences in investment between the 2g and 0.2g tasks are positive and statistically different from zero, for each of the four treatments (two-sided $p < 0.03$). So when contingency is actually present (the 2g task), subjects on average are discerning: they respond to the increased success probability. Priming non-contingency (in T2 and T3) has not undone that. We summarise this in our first finding.

Finding 1 *People's investment behaviour responds to actual contingency, i.e. the situation in which $p^* > .5$ for the discerning subject; and the effect is not undone by any treatment.*

Table 3: *Investment - Pairwise Treatment Comparisons*

	2g		0.2g	
T1-T2	0.693	(0.812)	1.028	(0.844)
T1-T3	1.330^{*}	(0.753)	0.846	(0.797)
T1-T4	1.623^{**}	(0.783)	0.564	(0.810)
T2-T3	0.636	(0.777)	-0.182	(0.780)
T2-T4	0.929	(0.806)	-0.465	(0.793)
T3-T4	0.293	(0.747)	-0.283	(0.744)

Note: Standard errors of a two-sample t-test between parentheses; ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively.

In Figure 3 we also observe that with the 2g investment task, investment levels increase from T4 (no prime) up to T1 (contingency prime). In contrast, we do not observe such a pattern with the 0.2g investment task. With the 2g investment task, investment levels are significantly higher in T1 compared to T4 (see Table 3). On average, participants in T1 invest 1.6 counters more than participants in T4. However, we do not find any treatment effects with the 0.2g investment task. Priming contingency, in T1, thus leads to an investment response, but only when contingency is present (the 2g task, not the 0.2g task). Subjects appear to become more alert to the presence of contingency through the prior contingency prime. This provides support for hypothesis 1.

We do not observe any significant differences in investment between treatments T2/T3 and T4 in both investment tasks. This provides only partial support for hypothesis 2, according to which priming non-contingency should lower investment in the contingency condition but not in the non-contingency condition. We summarise these observations in a second finding.

Finding 2 *The recent experience of contingency in an unrelated task leads to a greater responsiveness to the presence of contingency in the investment task, while the recent experience of non-contingency does not change this responsiveness.*

3.2. Salience of contingency and confidence

In our experiment, we prime (non-)contingency through the prior experience of (non-)contingency in a card guessing game. One plausible channel of influence through which priming (non-)contingency may lead to an investment response is by affecting confidence.[9] Recall that in the investment tasks, all subjects are informed about the presence of (non-)contingency through quite elaborate instructions, but whether they are confident enough to act on this information may have been affected by the priming task. To investigate whether salience of contingency and non-contingency in the investment tasks (resulting from priming through the cards treatments) translates into, respectively, greater and lesser confidence, we use the decisions made in the last task of the experiment.

Table 4: *Counters Moved - Pairwise Treatment Comparisons*

	2g		0.2g	
T1-T2	0.084	(3.359)	2.392	(3.312)
T1-T3	4.542	(2.979)	3.061	(3.387)
T1-T4	3.772	(3.223)	-0.313	(3.270)
T2-T3	4.458	(2.886)	0.668	(3.427)
T2-T4	3.688	(3.121)	-2.705	(3.320)
T3-T4	-0.769	(2.757)	-3.374	(3.382)

Note: Standard errors of a two-sample t-test between parentheses; ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively.

We find that participants move more counters to the "correct" box with the 2g task than with 0.2g (19.20 and 15.05, respectively; two-sided p-value of a paired t-test = 0.000).[10] At the same time, in tests based on sample means we find no treatment effects on the counters moved (see Table 4). This increase in stated confidence from the 0.2g task to the 2g task suggests that subjects were on average aware of contingency in the 2g task, and no priming has undone that. Moreover, the absence of treatment effects

[9]Other channels of influence are of course possible, for instance the lingering of a recently activated habitual response.

[10]It is worth noting that, in the instructions, we do not explicitly treat one counter as representing 1 percentage point. However, if the participants interpreted the counters in this way, then they were typically overconfident in the sense that the mean number of counters in the "correct" box significantly exceeds the fraction of correct guesses for both the 0.2g and 2g tasks.

on counters moved suggests that priming (affecting the salience of contingency) has not resulted in differences in confidence that the weight difference was correctly detected in the 2g task across treatments.[11] This suggests that, when contingency is present, confidence is on average not a strong channel of influence through which salience of contingency translates into higher investment. We summarise this in our third finding.

Finding 3 *People are generally aware of contingency (i.e. they move more counters in the contingency condition), but in tests across sample means, confidence in this awareness does not respond to priming contingency or non-contingency.*

3.3. Regression analysis

While we do not find any strong imbalances in individual characteristics across the treatments (see Table 2), we run a regression as it allows us to correct statistical inference for possible within-session correlations. It also allows us to increase the efficiency of the estimates by controlling for important correlates of investment behaviour in our experiment, as well as investigate interesting interactions.

Table 5: *Investment - Regression Analysis*

	(1)	(2)	(3)	(4)
	2g	0.2g	2g	0.2g
T1	1.593***	0.534	1.328**	0.391
	(0.513)	(0.494)	(0.590)	(0.476)
T2	0.931	-0.473	0.500	-0.859
	(0.803)	(0.555)	(0.922)	(0.633)
T3	0.414	-0.129	0.096	-0.315
	(0.777)	(0.838)	(0.867)	(0.829)
Constant	11.923***	12.460***	11.959***	16.425***
	(0.975)	(0.826)	(1.940)	(1.809)
R^2	0.129	0.191	0.220	0.330
Observations	395	395	364	364
Controls	No	No	Yes	Yes

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independences within villages; village fixed effects were used, as well as controls for experimenter effects. In models 3 and 4 we also control for gender, age, education, land, household size, task order, shocks and risk preferences of the participant.

Table 5 presents the results. In models 3 and 4 we control for gender, age, education, land, household size, shocks and risk preferences of the participant. We focus on the role

[11] This conclusion is tentative: in Section 3.4 using a Craggit model we find evidence of an effect on the number of counters moved, conditional on moving any. We also find that this average tendency obscures heterogeneity, notably when we consider how risk preferences interact with treatment to affect confidence.

of risk preferences in the next subsection, and will explain there how they were elicited. In all regressions we also control for the order of the 2g and 0.2g tasks, and we use village fixed effects and experimenter fixed effects.

The coefficient on T1 (contingency prime) is statistically significant in both 2g investment regressions. None of the coefficients on T2 (non-contingency prime, complex) and T3 (non-contingency prime, trivial) is significant in any regression, and neither is the coefficient on T1 in the 0.2g regressions. This confirms that priming contingency in a prior unrelated situation leads to higher investment in an opportunity for investment in which contingency is present (hypothesis 1) but provides only partial support for hypothesis 2. We summarise this in our fourth finding.

Finding 4 *Investment in the contingency condition robustly responds to priming contingency, whereas no such response is found in the non-contingency condition.*

3.4. The role of risk preferences

In the classical portfolio problem (see Section 2.1), optimal investment is determined by risk preferences. It would therefore be interesting to test whether risk preferences mediate the treatment effects (alertness to contingency) on investment behaviour. However, we cannot observe risk preferences in the experiment, since we do not directly observe p^*.[12] While we cannot detect risk preferences in the experiment, we can use a measure of risk preferences obtained during the livelihoods survey, in which probabilities are given and unambiguous. In particular, we used a hypothetical investment question, adapted from Dohmen et al. (2005), about subjects' willingness to invest $x \in \{0, 20000, 40000, 60000, 80000, 100000\}$, so up to 100,000 shillings in an asset that yields a return of 100 percent if successful and minus 50 percent if a failure, with equal probability. 100,000 shillings is about a month's worth of daily wages for an agricultural worker. Subjects chose one of six decision cards on which the two outcomes of a possible choice were clearly displayed. 1: refers to 0 invested; 6: refers to 100,000 invested. Thus higher values indicate lower risk aversion.[13] To investigate whether and how risk preferences interact with the treatment effects, we interact this risk preference measure with the treatment indicators in the regressions.

Table 6 reports the results for two regressions where the dependent variable is investment (models 1 and 2) and two regressions where the dependent variable is the counters

[12] Subjects believe they face the prospect $[p^*, 20+2\alpha; 1-p^*, 20-\alpha]$, in which p^* is the subjective success probability, and α the number of counters invested. We infer from the investment response to priming contingency (Findings 2 and 4), an increase in p^*, but we do not directly observe p^*.

[13] For the highest value, individuals may also be risk neutral or risk loving.

moved in the confidence elicitation exercise (models 3 and 4). In models 3 and 4, the dependent variable lies in the range [0,50] as no participant indicated that for their chosen option fewer than 50 counters should remain in the "correct" box.

Table 6: *The role of risk preferences*

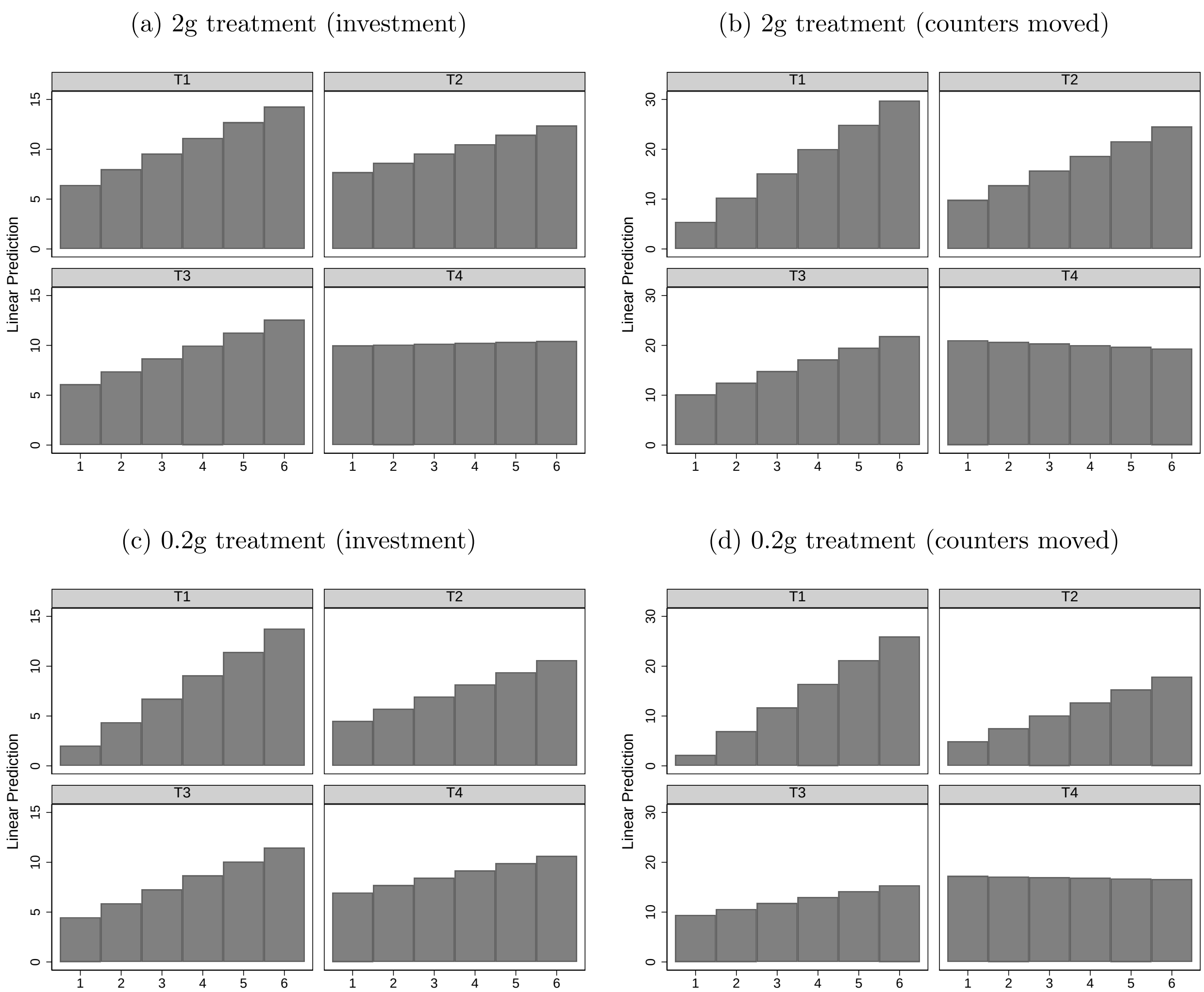
	(1)	(2)	(3)	(4)
	2g	0.2g	2g prob	0.2g prob
Risk pref.	0.088	0.734	-0.331	-0.127
	(0.531)	(0.525)	(1.398)	(1.030)
T1	-5.057*	-6.553**	-20.787**	-19.926**
	(2.917)	(3.005)	(8.984)	(7.739)
T2	-3.144	-2.940	-14.474**	-15.040***
	(3.470)	(3.092)	(6.693)	(4.788)
T3	-5.103	-3.144	-13.552	-9.133
	(3.964)	(3.069)	(9.962)	(7.590)
T1 × Risk pref.	1.484**	1.614**	5.198**	4.877***
	(0.651)	(0.656)	(1.883)	(1.610)
T2 × Risk pref.	0.849	0.485	3.274**	2.721**
	(0.674)	(0.638)	(1.501)	(1.143)
T3 × Risk pref.	1.208	0.662	2.670	1.316
	(0.814)	(0.666)	(2.139)	(1.606)
Constant	9.210***	9.915***	6.786	14.617**
	(2.659)	(2.286)	(8.092)	(6.720)
R^2	0.237	0.347	0.223	0.260
Observations	364	364	364	364

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within villages. Village fixed effects were used, as well as controls for experimenter effects, and gender, age, education, land, household size and locus of control of the participant.

In Table 6 we observe no direct influence of risk preferences on investment and confidence in the investment: the coefficient on the risk preference variable measures the effect in T4 (no priming), since the three other treatments also appear interacted with this variable. In contrast, the interaction between T1 and the risk preference variable is always significant, and that of T2 is significant in the counters moved (confidence) regressions. Whereas it appeared in Section 3.2 that on average there are no effects of priming (non-)contingency on self-reported confidence, we now see that both T1 and T2 interact with the effect of the risk preference variable on the confidence in having detected the heavier container, both in the 2g and in the 0.2g investment tasks.

Because of the interaction terms, the overall impact of treatment is a little hard to judge from the table. In Figure 4 we use the OLS models of Table 6 to summarize the estimated relationship between the risk preference variable on the one hand and the two outcome variables (investment and counters moved) on the other hand, across the four treatments.

Figure 4: *Investment by Treatments and Risk Preferences*



(a) 2g treatment (investment)

(b) 2g treatment (counters moved)

(c) 0.2g treatment (investment)

(d) 0.2g treatment (counters moved)

Notes. Estimates used are from Table 6. Risk preferences: 1 is most risk averse - 6 is least risk averse

From Figure 4, we can see that the positive relationship between the risk preference variable and the outcome variables is clearest for T1 and largely absent for T4. It is important to note that, as Table 6 shows, at the mean level of risk aversion, the effect of T1 on investment and moved counters is positive. In fact, only 23% of subjects have risk preferences lower than 4, so for typical subjects, T1 leads to higher investment. This confirms that the lower is risk aversion (i.e. the higher the risk preference variable), the more investment responds to a greater awareness of contingency. The reason for this appears to be greater confidence that the investment will succeed, which increases (in T1) when risk aversion is lower. For the 2g task, the overall impact of T1 relative to T4 on investment is not-significantly different from zero, except for the low risk averse subjects with risk preferences at level 5 or 6 (who represent 47% of subjects) ($p < 0.05$). For these subjects, the difference between T1 and T2 is also significant ($p < 0.05$). For the 0.2g task, for the less risk averse subjects, investment is higher in T1 compared to T4 and T1 compared to T2, (both $p < 0.01$).

Since many individuals move zero counters in the confidence elicitation task, as a robustness check on the results from the OLS model, we use a Craggit model in Table 7. In this table, the first reported equation for each weight is a probit model where the dependent variable takes the value 1 if any counters are moved and 0 otherwise. In the second equation for each weight, the dependent variable is the number of counters moved, conditional on moving any.

Table 7: *Counters moved with Craggit*

	0.2g		2g	
	Probit	Counters	Probit	Counters
Risk pref.	0.083	-0.061	0.098	-0.646
	(0.109)	(1.633)	(0.110)	(2.090)
T1	-1.035*	-24.742*	-2.554**	-20.018
	(0.581)	(12.869)	(0.995)	(12.269)
T2	-0.956	-20.372**	-0.094	-25.042**
	(0.707)	(10.290)	(0.692)	(10.931)
T3	-0.538	-13.046	-0.513	-18.710
	(0.866)	(10.069)	(1.014)	(14.068)
T1 x Risk pref.	0.326**	5.270**	0.656***	5.106**
	(0.150)	(2.588)	(0.241)	(2.564)
T2 x Risk pref.	0.136	3.711*	-0.005	5.536**
	(0.162)	(2.008)	(0.148)	(2.455)
T3 x Risk pref.	0.022	2.887	0.011	4.569
	(0.195)	(2.106)	(0.217)	(3.026)
Constant	-0.066	22.751**	-0.164	23.833**
	(0.752)	(11.099)	(0.874)	(11.238)
Observations	364		364	

Notes. craggit regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within villages; village fixed effects were used, as well as controls for experimenter effects and task order. We also controlled for gender, age, education, land, household size, shocks and risk preferences of the participant.

The results of Table 7 concur with those obtained using OLS for T1: for high levels of risk aversion (i.e. low levels of the risk preference variable), T1 (which primes contingency) reduces the probability of moving counters relative to other treatments. In the craggit specification, the effect works through both parts of the model. The level of the risk preference variable modifies the treatment effect for T1. Higher levels of this variable (i.e. lower levels of risk aversion) are associated with higher expressed confidence in making the right decision for both the 0.2g and 2g tasks.[14]

Given the results of the OLS and Craggit models, we summarise the results of this section in our fifth and final finding.

Finding 5 *The prior experience of contingency leads to a greater investment response when risk aversion is lower. The lower is risk aversion, the greater is the effect of priming contingency both on investment and on confidence in the investment.*

4. Discussion and Conclusion

We studied the role of contingency in investment decisions, when contingency is difficult to detect. Hypothesis 1, according to which priming contingency should raise investment in the contingency condition but not in the non-contingency condition, is fully confirmed in our experiment. By contrast, hypothesis 2, according to which priming non-contingency should lower investment in the contingency condition but not in the non-contingency condition, is only partly confirmed. The part that is not confirmed is the classical learned helplessness hypothesis (see section 1) applied to the investment domain: that a prior experience of non-contingency negatively affects investment in a situation characterised by contingency. We thus found that alertness to contingency could be heightened but not lowered in our set-up.

We can now spell out the main things we have learnt about investment behaviour. In our experiment, the presence of contingency means that the success probability responds to careful discernment. It follows that alertness to contingency makes it more likely that this discernment will be applied. We show that the alertness to contingency responds to recent experience of contingency: recent experience of contingency raises investment in situations in which the success probability responds to careful discernment. Moreover, lower risk aversion is found to raise investment more when the awareness of contingency has

[14]In addition, higher levels of the risk variable (i.e. lower risk aversion) are associated with more counters moved for T2, conditional on moving any counters, for both the 0.2g and 2g tasks. In further robustness checks, we try an alternative model for the investment equations which allows for a spike of responses at 10. This model yields qualitatively similar results to the OLS equations reported in 6. Since the number of counters moved and the investment level are positively correlated we also re-estimate using an SUR model which again produces similar answers and which is therefore not reported here.

been primed.[15] This suggests that risk preferences are mediated through the awareness of contingency in their influence on investment behaviour. Finally, in our experiment, lower risk aversion combined with a contingency prime is associated with greater stated confidence that the investment will be successful. This suggests that investment behaviour is guided by an interactive mixture of risk preferences, awareness that one can influence the success probability (contingency), and a greater belief that one has done so successfully (confidence).

We discuss three implications of our findings. We have shown that when contingency is present in a noisy decision-making environment, recent experience matters for how alert people are to that. This may be a useful insight for advancing the theoretical literature on optimal expectations. So far this literature has predicted welfare-enhancing biases in information processing when performance depends on self-confidence (Bénabou and Tirole, 2002; Compte and Postlewaite, 2004), or current felicity depends on optimism (Brunnermeier and Parker, 2005). It may be interesting for new theoretical work to be undertaken on the optimal assessment of the presence of contingency. In a decision-making environment marked by a low signal to noise ratio, in which contingency is costly to detect, the costs of establishing the degree of contingency present in each decision-making situation may be prohibitive. Taking recently experienced contingency as one's cue could then be an optimal adaptation, i.e. an optimal expectation despite the errors this is prone too. While this is a claim that has received some attention in mathematical decision sciences (e.g. Huys and Dayan, 2009), investigating this theoretically in economic contexts could be a fruitful line of enquiry.

The second implication of our findings is that it highlights a source of bias that could be useful for the literature on subjective probabilities to consider. The literature inspired by Tversky and Kahneman (1974) focuses on biases resulting from mental short-cuts – the intuitive or "fast" thinking that serves people well in the many situations that require quick judgements but that is also a source of error. So in the representativeness heuristic, when people think about whether A belongs to class B, or A is generated by process B, they are influenced by how similar A is to B, and deviate from probability theory as a result (e.g. Grether, 1980; Charness and Levin, 2005; Dohmen et al., 2009). Likewise, in the anchoring and adjustment heuristic, people's probability assessment is influenced by some initial value suggested by the formulation of the problem (e.g. Plous, 1989; Chapman

[15] Murad et al. (2016) is the only paper we know of that reports on this link between risk attitudes and measures of belief or confidence. Their experiment asks subjects to evaluate which city is bigger in a series of pairwise comparisons. They elicit beliefs about accuracy using both incentivized and unincentivized procedures and find a negative correlation between expressions of confidence and measures of risk aversion. Thus although the experiment lacks the priming treatment that we have and the explicitly framed nature of the tasks (impossible and possible) it shares some features.

and Johnson, 1999; Furnham and Boo, 2011). Whereas in previous literature on subjective probabilities, cognitive heuristics – the need to "think fast" – are the source of bias, our study suggests that recent experiences of contingency and non-contingency may be another source. If the exploration of the features of a decision-making problem relies on such recent experience, then the assessment of probabilities will be biased towards that experience. Specifically, more contingency will be assumed when recently contingency was experienced, and ditto for non-contingency.

A third implication of our findings is relevant for development policy. Underinvestment in agriculture is widely observed in developing countries – the limited uptake of improved varieties of seed is a prominent example. Such underinvestment is held responsible for a major part of the persistence of poverty in the rural areas of developing countries, where most of the world's poor live (World Bank, 2007). Previously identified factors responsible for such underinvestment include risk aversion, credit and insurance market failures, lack of access to information and resources (e.g. labour at peak times), paucity of savings instruments, and the unavailability of good agricultural extension services (see Foster and Rosenzweig (2010) for a review). We add a new factor: the difficulty to detect contingency in a "noisy" decision-making environment. Since factors such as unknown features of the soil, ill-understood agricultural inputs and unidentified pests and diseases co-determine yields, the role of one's own efforts in determining the success of an agricultural investment that requires careful application (e.g. a new fertiliser) is easily obscured and the belief that no contingency is present (even when it is) may therefore take hold. Our experiments show that being alert to the possible presence of contingency – a recent experience of being in control of outcomes through careful discernment – promotes investment in situations characterised by non-obvious contingency. Finding ways of communicating contingency effectively may need to be part and parcel of promoting agricultural investment.

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Appendix

A. Descriptives

Table A.1: *Distribution of Counters Invested (in percentage)*

	2g					0.2g				
	T1	T2	T3	T4	Total	T1	T2	T3	T4	Total
0	4.08	5.05	6.06	7.07	5.57	6.12	13.13	8.08	7.07	8.61
1	1.02	1.01	0.00	1.01	0.76	2.04	3.03	1.01	0.00	1.52
2	4.08	7.07	2.02	0.00	3.29	6.12	1.01	2.02	4.04	3.29
3	2.04	1.01	3.03	1.01	1.77	2.04	6.06	2.02	3.03	3.29
4	2.04	2.02	2.02	5.05	2.78	7.14	5.05	4.04	3.03	4.81
5	2.04	4.04	7.07	12.12	6.33	5.10	6.06	14.14	13.13	9.62
6	3.06	2.02	2.02	2.02	2.28	4.08	1.01	3.03	0.00	2.03
7	0.00	1.01	0.00	1.01	0.51	1.02	2.02	2.02	5.05	2.53
8	3.06	7.07	8.08	9.09	6.84	7.14	4.04	10.10	9.09	7.59
9	3.06	2.02	1.01	2.02	2.03	2.04	1.01	2.02	0.00	1.27
10	22.45	20.20	26.26	23.23	23.04	22.45	26.26	20.20	26.26	23.80
11	2.04	5.05	3.03	1.01	2.78	2.04	1.01	3.03	2.02	2.03
12	6.12	7.07	11.11	7.07	7.85	5.10	9.09	8.08	6.06	7.09
13	8.16	2.02	5.05	3.03	4.56	1.02	1.01	2.02	1.01	1.27
14	5.10	4.04	3.03	4.04	4.05	3.06	5.05	2.02	3.03	3.29
15	11.22	9.09	7.07	4.04	7.85	2.04	4.04	5.05	5.05	4.05
16	1.02	2.02	0.00	2.02	1.27	1.02	1.01	1.01	1.01	1.01
17	3.06	0.00	4.04	2.02	2.28	4.08	0.00	3.03	1.01	2.03
18	1.02	2.02	3.03	4.04	2.53	4.08	2.02	3.03	2.02	2.78
19	0.00	0.00	0.00	0.00	0.00	1.02	0.00	0.00	0.00	0.25
20	15.31	16.16	6.06	9.09	11.65	11.22	8.08	4.04	8.08	7.85
Total	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00

B. Full models

Table B.1: *Treatment Comparisons: Regression Analysis*

	(1)	(2)	(3)	(4)
	2g	0.2g	2g	0.2g
T1	1.593***	0.534	1.328**	0.391
	(0.513)	(0.494)	(0.590)	(0.476)
T2	0.931	-0.473	0.500	-0.859
	(0.803)	(0.555)	(0.922)	(0.633)
T3	0.414	-0.129	0.096	-0.315
	(0.777)	(0.838)	(0.867)	(0.829)
Task order	-0.783	-0.450	-0.241	-0.126
	(0.499)	(0.443)	(0.516)	(0.392)
Shocks			0.018	0.033
			(0.222)	(0.198)
Male			0.884	0.903
			(0.638)	(0.610)
Age			0.011	-0.001
			(0.017)	(0.021)
Highest Level of Eduction			-1.053**	-0.741*
			(0.448)	(0.385)
Locus of control			0.594	0.678*
			(0.350)	(0.379)
Sought advice on fertilizer			0.049	-0.392
			(0.656)	(0.546)
Used fertilizer			-0.689	-1.494**
			(0.904)	(0.619)
Improved seeds			1.098	0.103
			(0.920)	(0.808)
Land			0.319	0.407**
			(0.192)	(0.144)
Household size			0.254	0.018
			(0.147)	(0.128)
Risk pref.			-0.941***	-1.378***
			(0.256)	(0.253)
Constant	11.923***	12.460***	11.959***	16.425***
	(0.975)	(0.826)	(1.940)	(1.809)
R^2	0.129	0.191	0.220	0.330
Observations	395	395	364	364
Controls	No	No	Yes	Yes

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within villages; village fixed effects were used, as well as controls for experimenter effects (not reported).

Table B.2: *Subjective probabilities*

	(1)	(2)	(3)	(4)
	2g	0.2g	2g	0.2g
T1	2.026	1.464	1.604	1.085
	(2.414)	(1.710)	(2.434)	(1.934)
T2	1.507	-1.443	-0.377	-3.304*
	(1.923)	(1.725)	(2.283)	(1.666)
T3	-1.120	-2.471	-2.119	-3.564
	(2.843)	(2.508)	(3.340)	(2.849)
Task order	-0.845	-1.112	0.418	-0.180
	(1.266)	(1.258)	(1.419)	(1.243)
Shocks			0.213	0.492
			(0.632)	(0.571)
Male			2.802	2.875
			(1.663)	(1.893)
Age			0.092	0.003
			(0.071)	(0.068)
Highest Level of Eduction			-3.048**	-3.570**
			(1.398)	(1.563)
Locus of control			1.864	2.545*
			(1.403)	(1.342)
Sought advice on fertilizer			0.104	2.001
			(2.453)	(1.803)
Used fertilizer			2.354	1.434
			(2.599)	(2.590)
Improved seeds			2.420	2.149
			(1.645)	(1.833)
Land			0.460	0.422
			(0.726)	(0.670)
Household size			0.679*	-0.017
			(0.369)	(0.285)
Risk pref.			-2.361***	-2.008***
			(0.559)	(0.429)
Constant	15.793***	19.570***	10.679*	18.162***
	(2.906)	(2.708)	(5.260)	(5.001)
R^2	0.097	0.133	0.201	0.238
Observations	395	395	364	364
Controls	No	No	Yes	Yes

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within villages; village fixed effects were used, as well as controls for experimenter effects (not reported).

Table B.3: *The role of risk preferences*

	(1)	(2)	(3)	(4)
	2g	0.2g	2g prob	0.2g prob
Risk pref.	0.088	0.734	-0.331	-0.127
	(0.531)	(0.525)	(1.398)	(1.030)
T1	-5.057*	-6.553**	-20.787**	-19.926**
	(2.917)	(3.005)	(8.984)	(7.739)
T2	-3.144	-2.940	-14.474**	-15.040***
	(3.470)	(3.092)	(6.693)	(4.788)
T3	-5.103	-3.144	-13.552	-9.133
	(3.964)	(3.069)	(9.962)	(7.590)
T1 × Risk pref.	1.484**	1.614**	5.198**	4.877***
	(0.651)	(0.656)	(1.883)	(1.610)
T2 × Risk pref.	0.849	0.485	3.274**	2.721**
	(0.674)	(0.638)	(1.501)	(1.143)
T3 × Risk pref.	1.208	0.662	2.670	1.316
	(0.814)	(0.666)	(2.139)	(1.606)
Shocks	0.020	0.041	0.228	0.517
	(0.210)	(0.183)	(0.589)	(0.544)
Male	0.913	0.953	3.052	3.181
	(0.658)	(0.612)	(1.775)	(1.846)
Age	0.008	-0.006	0.075	-0.015
	(0.018)	(0.022)	(0.073)	(0.069)
Highest Level of Eduction	-1.150**	-0.831*	-3.301**	-3.763**
	(0.455)	(0.415)	(1.429)	(1.666)
Locus of control	0.701*	0.746*	2.157	2.742**
	(0.398)	(0.400)	(1.292)	(1.301)
Sought advice on fertilizer	-0.042	-0.424	-0.265	1.708
	(0.672)	(0.599)	(2.366)	(1.840)
Task order	-0.191	-0.108	0.549	-0.110
	(0.522)	(0.372)	(1.444)	(1.225)
Used fertilizer	-0.800	-1.551**	1.753	0.866
	(0.950)	(0.620)	(2.701)	(2.494)
Improved seeds	1.131	0.100	2.651	2.363
	(0.890)	(0.788)	(1.816)	(1.886)
Land	0.320	0.414**	0.448	0.410
	(0.194)	(0.151)	(0.754)	(0.711)
Household size	0.264*	0.005	0.732*	0.013
	(0.149)	(0.129)	(0.401)	(0.307)
Constant	9.210***	9.915***	6.786	14.617**
	(2.659)	(2.286)	(8.092)	(6.720)
R^2	0.237	0.347	0.223	0.260
Observations	364	364	364	364

Notes. OLS regression. ***, **, * indicate two-sided significance levels at 1, 5, and 10 %, respectively; robust standard errors (in parentheses) to control for non-independencies within villages. Village fixed effects were used, as well as controls for experimenter effects (not reported).

ONLINE APPENDIX (NOT FOR PUBLICATION): EXPERIMENTAL SCRIPT

Setting up

[Set up six tables.

On the left side of the room are two tables. Each of these has two containers. On one there are two blue containers; on the other two green containers. On each table there is also the digital weighing machine, a stopwatch and 20 counters. WHEN SETTING UP EACH TABLE VERIFY THAT THE DIFFERENCE IN WEIGHT BETWEEN THE TWO CONTAINERS PLACED ON THEM IS EQUAL TO 0.2g.

On the right side of the room are two tables. Each of these has two containers. On one there are two purple containers; on the other two pink containers. On each table there is also the digital weighing machine, a stopwatch and 20 counters. WHEN SETTING UP EACH TABLE VERIFY THAT THE DIFFERENCE IN WEIGHT BETWEEN THE TWO CONTAINERS PLACED ON THEM IS EQUAL TO 2g.

At the top of the room are two tables. These contain stopwatches, disentanglement puzzles and "correct" and "incorrect" boxes with 50 counters in each. These are low, shallow plastic boxes that should be transparent and with the labels "correct" and "incorrect" attached to them.]

Welcome. Thank you for taking the time to come today. My name is …… and I am working with the University of East Anglia. This University is found in the UK. We have invited you here, today, because we want to learn about how people in this area take decisions. You are going to be asked to take decisions that will earn you some money. The money that results from your decisions will be yours to keep; you can take it home with you.

What you need to do will be explained fully in a few minutes. But first, there are three things I would like to explain to you clearly and you should consider them as very important.

> First of all, this is not our money. As I told you before, we work for a university and this money has been given to us by that university for this research.
>
> Participation is voluntary. You may still choose not to participate in the exercise.
>
> We also have to make clear that this is research about <u>your</u> decisions. Therefore I will not allow you to go out to talk with anyone else. This is very important. I'm afraid that if I you decide to talk with someone else, that will be the end of the study and what this means is that you will not be able to earn any money here today. If you have any questions, please raise your hand and ask me. Could I also ask you to switch off your mobile phone if you have one?

Make sure that you listen carefully to me. You will be able to make a good amount of money here today, and it is important that you follow my instructions.

During today's programme, you will be asked to make a few decisions, which will be explained to you clearly. Only one of your decisions will be selected to determine the money you will be paid. At the end of the exercise, we will randomly select one of your decisions to be paid out. Any money you earn will be paid out to you privately and confidentially after all parts of the exercise are complete.

Now, before we explain what you need to do, it is really important to bear one more thing in mind. You will be asked to take decisions about money that are not a matter of getting it right or wrong; they are about what you want to do. However, it is important to think seriously about your decisions because they will affect how much money you can take home.

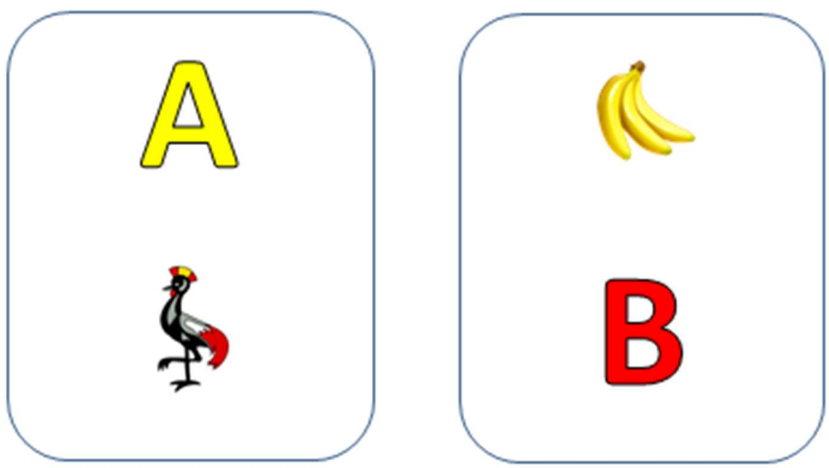
Now, let us begin. You see in this room there are several tables. They correspond to three tasks that you will be asked to make decisions in today. These tasks are different from each other, and you should treat them as such. Only one task will actually be the one which determines the amount of money you leave with today. We will record your decisions on each of these tables and at the end of today's programme, the decision which determines how much money you will earn today will be selected by picking out of this bag, without looking, a piece of paper **[show bag]** that has 1, 2 or 3 written on it **[show pieces of paper]** so that decision 1, 2 and 3 are each exactly as likely to be selected.

[Treatments 1, 2 and 3 only:] Before we start with the tasks in which you can earn money, we will take you through a different task, which is not for money; it is a task that helps us to see how people think.

CARDS TASK [SKIP FOR TREATMENT 4]

[Cards are same and same sequence for all treatments; only the rule changes]

In this first section of today's programme you will see a series of pairs of cards. Here's an example of a typical pair of cards **[hold up example cards]**



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Each card contains a picture and a letter. Altogether, there are three pictures: bird, banana and tree; and there are two possible letters: A and B. The picture will vary across cards. The letter and its colour will also vary across cards. Sometimes the letter will be in red, sometimes in yellow and sometimes in black. Sometimes the letter will be at the top of the picture and sometimes at the bottom of the picture. Have a good look at these two cards.

In each pair, there is at least one card that is correct. There is a rule for whether a card is correct.

For instance, **[hold up same example pair of cards]**, the rule could be that a correct card has a red letter or the rule could be that the card must have an A on it, or a banana, or the picture must be at the top. Suppose the rule was that the card with the red letter was always correct, then this card with the red B would be correct and this card with the yellow A would be incorrect.

Your task is to find out the rule. How? You can see that there are lots of possible rules so at first you will have to guess, but by paying attention you may discover the rule.

Altogether there will be 10 pairs of cards. After you see a pair of cards you will have 30 seconds to choose a correct card. You must choose one card and say so. After you have made your decision we will tell you if your answer is correct or not correct. We won't tell you anything more than this. To help you a little, when we show you the first pair of cards, we will point out a card which is correct. After that you must make your own decision.

At the end of the 10 pairs we will ask you to tell us the rule. If you don't know the rule or you are not sure it's okay to say that you don't know.

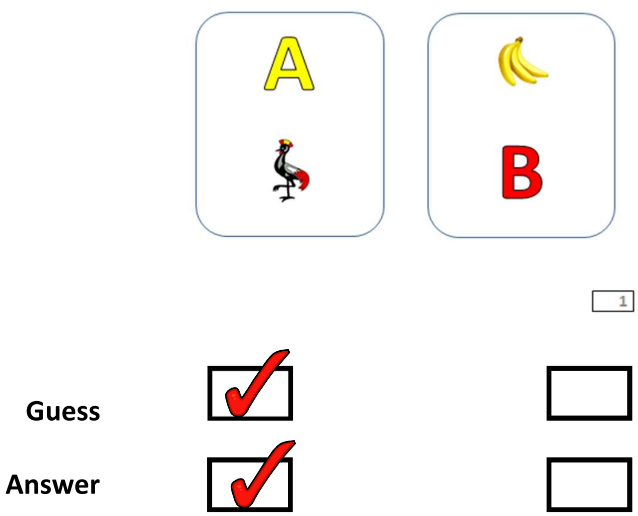
[If asked the question: 'Can both cards be correct?', answer 'yes' and add that 'there is always at least one card that is correct'

Treatment 4: no priming task at all]

Do you have any questions about how the task will work?

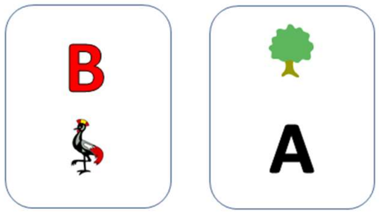
[Answer any questions as clearly and accurately as possible. On each subject's data entry sheet it will say which treatment they should follow. The experimenter needs an answer sheet for the subject, a feedback guide and a pen. The feedback guide is the guide that tells the experimenter what to say after each guess from the subject. Place answer sheet in front of subject. Keep it placed so that all times s/he can see the results, but keep your feedback guide hidden.

[Point to first pair of cards.] Here is the first pair of cards. For this first pair I will tell you a correct card. **[In the left hand box place a clear tick mark in the 'guess' box and then below it another tick in the answer box].** Here **[point to Guess line]** I will put a tick mark for your guess about a correct card and here **[point to below]**, I will mark the answer. If your guess was correct, I'll put another tick, if it was wrong I'll put an X. **[Put the sheet in front of the subject and point to left hand card].** This card is correct.



[Point to the second question.]

Here is the second question. Have a good look at it and then tell me a card that you think is correct. **[Give them 30 seconds and press them for a decision. If they are not clear ask them to be clear about their choice. Record the decision with a tick and then tell them if they are correct or not. If they are correct, then put a tick. If incorrect put an x. Remember to say 'correct' or 'incorrect'].**



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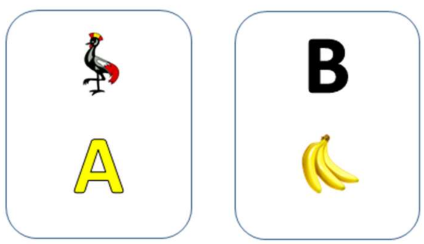
[Point to the third question.]

Here is the third question. Have a good look at it and then tell me a card that you think is correct. **[Give them 30 seconds and press them for a decision. If they are not clear ask them to be clear about their choice. Record the decision with a tick and then tell them if they are correct or not. If they are correct, then put a tick. If incorrect put an x. Remember to say 'correct' or 'incorrect'].**



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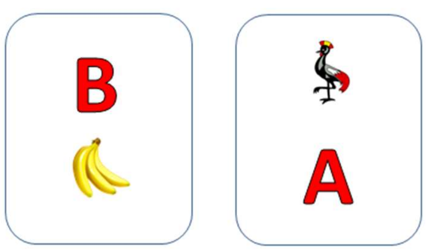
[repeat until you reach question 10]



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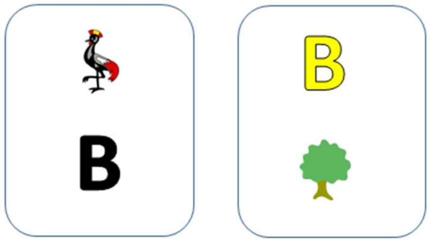

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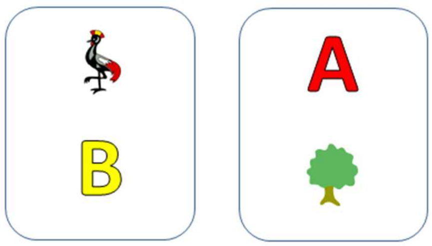

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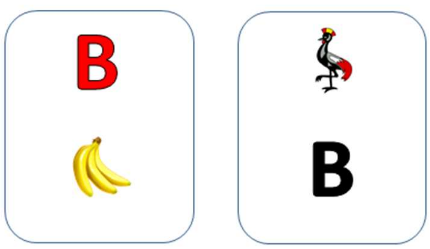

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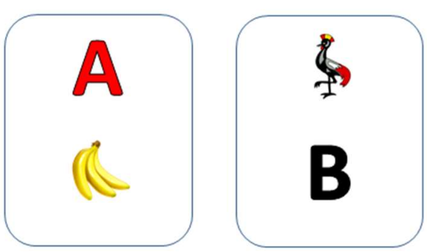

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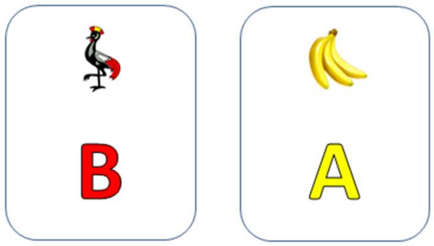

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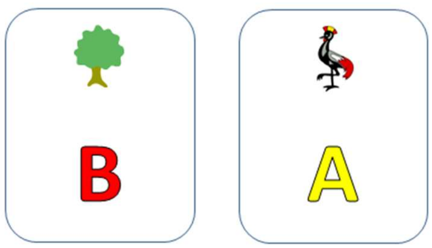

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Thank you for answering all the questions. I'd like you now to tell me what you think the rule is.

[Write down the rule in the box on the answer sheet. Don't prompt immediately, but if they are very hesitant, tell them it's okay to say that they don't know. If they say they don't know, record this].

This concludes the first part of today's programme.

Feedback guide Treatment 1. [where the cell is blank the experimenter should give the feedback 'incorrect']

Treatment 1: card with bird is correct;

Question	Subject guesses left hand	Subject guesses right
1	Correct	
2	Correct	
3	Correct	
4		Correct
5	Correct	
6	Correct	
7		Correct
8		Correct
9	Correct	
10		Correct

Feedback guide Treatment 2. [where the cell is blank the experimenter should give the feedback 'incorrect']

Treatment 2: card is correct if it has a red letter at the top; otherwise the card with a yellow letter is correct

Question	Subject guesses left hand	Subject guesses right
1	Correct	
2	Correct	
3	Correct	
4	Correct	
5		Correct
6		Correct
7	Correct	
8	Correct	
9		Correct
10		Correct

Feedback guide Treatment 3. [where the cell is blank the experimenter should give the feedback 'incorrect']

Treatment 3: a card with any letter is correct (i.e. any guess is correct).

Question	Subject guesses left hand	Subject guesses right
1	Correct	Correct
2	Correct	Correct
3	Correct	Correct
4	Correct	Correct
5	Correct	Correct
6	Correct	Correct
7	Correct	Correct
8	Correct	Correct
9	Correct	Correct
10	Correct	Correct

GENERAL INSTRUCTIONS WEIGHING TASKS

[Take subject to table on the left (blue or green containers), or on the right (purple or pink containers) as indicated by the experiment administrator, before saying:]

Look, there are two containers on this table. We shall give you a chance to weigh these containers with your hands and decide which one is heavier. Then we will do the same thing on a table at the other side of the room **[point to it]**, which also has two containers.

Why are we doing this? You will also see there are 20 counters spread out on this table; and also on that table at the other side of the room **[point to it]**. Each counter is worth 400 Shillings meaning that these 20 counters represent 8,000 shillings.

That money is yours and you can do with it exactly what you like, but we will give you the chance to make two investment decisions. One for this table and one for the other table.

You have the opportunity to invest some of your counters on the container you think is heavier.

At the end of today's programme, if one of these two weighing tasks is selected as the one that determines how much you will leave with, we will weigh the two containers on this accurate machine. If you have chosen the heavier container correctly, then your investment will have succeeded. If the investment succeeds, we triple what you have invested. You then go home with the money you didn't invest plus three times the money you did invest.

If you chose to invest in the wrong container then your investment fails. Now, what happens if the investment fails? Your investment failing means you lose all of it. In this case you go home with the money that you didn't invest.

So, first you will weigh and compare the containers on each table and decide which of the pair is the heavier. Then once you have weighed and compared the containers, you have to decide how much money you wish to invest, using these counters. Remember that each counter is worth 400 shillings. When you have decided the number of counters you wish to invest, you place them next to the container that you think is heavier. You can only invest in one container on each table.

You are free to invest any number of counters you choose: you can invest zero counters; you can invest 20 counters, or any number of counters between zero and 20. If you invest in the heavier container, your investment succeeds and we triple it, if you invest in the lighter container, it fails and you lose your investment.

"Do you have any questions about how the tasks will work?"

[Answer any questions as clearly and accurately as possible.]

We just want to check your understanding of these tasks. If you invest in the heavier container, what happens to your investment? **[pause for answer, correct if necessary]** and if you invest in the lighter container, what happens to your investment? **[pause for answer, correct if necessary. Record whether or not subject answers correctly: 1=both parts correct, 0= one or more parts incorrect]**

Table with BLUE or GREEN containers – .2g difference task

[The table contains two plastic containers with g-nuts in them. The weight difference between the two containers is ~0.2g; there is no detectable difference in volume.]

You see the two containers with g-nuts in them? You have to decide which of the two containers is heavier. Before you come to a decision, you can hold the containers in your hand and compare them as you wish.

Now, before you weigh these containers, I would like you to listen very carefully because I am going to tell you something important about these two containers of g-nuts. We really believe it is impossible to tell the difference in weight between these two containers; the difference is too small for a person to feel it with his or her hands. We will show you later, using the machine that can detect very small weight differences, that one is heavier but whether or not you get it right is a matter of luck. Why do we say it is a matter of luck? Because we really believe that machines can tell the difference in weight but people cannot. We believe this because we have tried this with hundreds of people before, and they guess the wrong container as often as they guess the right one.

Now, please decide which of the containers is heavier. We'd like you to try weighing the containers for precisely 30 seconds: not longer and not shorter. When you are ready, I will set the stopwatch going. After 30 seconds you will hear a loud beep. Please tell me when you hear the beep, which container you think is heavier. **[Hand the subject the two containers; record using a stopwatch the time the subject takes to decide (normally 30 seconds); ask subject to stop trying after precisely 30 seconds; record subject's choice & confidence]**

Now, please decide how much you would like to invest. Remember each counter is worth 400 shillings and that if you decide correctly we will triple your investment. **[Record answer, making sure that the subject has placed counters against the container s/he thinks is heavier, and leave counters and containers in place until resolution.]**

Table with PURPLE or PINK containers – 2g difference task

[The table contains two plastic containers with g-nuts in them. The weight difference between the two containers is 2g; there is no detectable difference in volume between them.]

You see the two containers with g-nuts in them? You have to decide which of the two containers is heavier. Before you come to a decision, you can hold the containers in your hand and compare them as you wish.

Now, before you weigh these containers, I would like you to listen very carefully because I am going to tell you something very important about these two containers of g-nuts. There is a weight difference between these two containers. We will show you later that one is heavier, using the machine that can detect small weight differences. The difference is small, but we know that if you try hard many people can tell the difference. We really believe that not only machines can tell the difference in weight but many people can too, if they try hard. The difference in weight is large enough for many people to be able to correctly decide which container is heavier. We know this because we have tried this with hundreds of people before, and two out of every three people correctly decide which container is heavier.

Now, please decide which of the containers is heavier. We'd like you to try weighing the containers for precisely 30 seconds: not longer and not shorter. When you are ready, I will set the stopwatch going. After 30 seconds you will hear a loud beep. Please tell me when you hear the beep, which container you think is heavier. **[Hand the subject the two containers; record using a stopwatch the**

time the subject takes to decide (normally 30 seconds); ask subject to stop trying after precisely 30 seconds; record subject's choice]

Now, please decide how much you would like to invest. Remember each counter is worth 400 shillings and that if you decide correctly we will triple your investment. **[Record answer, making sure that the subject has placed counters against the container s/he thinks is heavier, and leave counters and containers in place until resolution.]**

TABLE with PUZZLE TASK (ALWAYS AFTER THE TWO WEIGHING TASKS)

In a few minutes we are going to give you the opportunity to try to disentangle these two bits of iron. Let me show you how it is done. **[Demonstrate, against the clock.]** Did you notice that I managed it easily within 10 seconds? I'll next explain the rules of this game and then you can decide whether or not you want to play.

Now, we're going to give you 3,000 shillings. This money is yours. You can decide to do with it whatever you want. If you don't want to play, you can take the money home with you. However, if you do want to play, you'll have to pay 10 shillings for every second that you play.

For instance, if you managed to solve the puzzle in precisely 10 seconds, then you'd have to pay 10 seconds times 10 shillings, or 100 shillings. We'd take those 100 shillings out of your 3,000 shillings, so you'd be left with 2,900 shillings.

Now listen carefully; here comes the most important part of the game. Because you'd been successful in solving the puzzle, we would reward you. This is what we would do: we'd multiply your money by five. So, we'd multiply 2,900 shillings by five and give you five times 2,900 shillings, or 14,500 shillings.

Supposing you'd managed to solve the puzzle in 50 seconds? You'd then have to pay 50 seconds times 10 shillings, or 500 shillings. We'd take those 500 shillings out of your 3,000 shillings. We'd multiply the 2,500 shillings by five, so we'd give you five times 2,500 shillings, or 12,500 shillings.

The maximum number of seconds you can play is 300: 5 minutes. What if you hadn't managed to solve the puzzle at all in 5 minutes? Then you'd have no money left.

However, you can stop at any point: after 1 second, 10 seconds, 100 seconds, 248 seconds – you decide at which point you want to give up. We then see on the clock for how many seconds you have played; we deduct the money for those seconds from your 3,000 shillings and we give you the rest of the money.

As you see, there is a stopwatch right next to you, so it is really easy to see for how many seconds you have played: you should feel completely free to stop at any time you want to.

[Opportunity for asking questions of clarification: the rules of the game can be clarified but don't show them how to solve the disentanglement puzzle.]

We just want to check your understanding of the task. How much money do you pay out of your 3000 shillings for every second that you play? **[pause for answer, correct if necessary]** And if you are successful, what happens to the money that remains? **[pause for answer, correct if necessary. Record whether or not subject answers correctly: 1=both parts correct, 0= one or more parts incorrect]**

Participants decide and play in private with only the experimenter watching: record for how many seconds they played, between 0 (i.e. they didn't decide to play) and 300.

Record whether or not they were successful in disentangling the puzzle.]

[MOVING COUNTERS FROM THE "INCORRECT" TO THE "CORRECT" BOX]

The tasks that affect how much money you will earn today are now complete. However, there is one more thing that we would like to know before we proceed to working out how much money you have earned today. This will in no way affect how much money you will take home, but is simply something we are interested in for our research.

We would now like to ask you how confident you felt when you were weighing the containers. Remember the two containers about which we said that a machine can tell the difference but people cannot, even if they try hard? **[point to the table where the subject was doing the 0.2g task]** Suppose we had given these two containers to you 100 times, and each time you tried to weigh them. After every time you tried, we'd shuffle them behind our backs, like this **[demonstrate with your hands; don't use the actual containers]**, and we'd give them back to you 100 times. Out of those 100 times, how many times do you think you'd have pointed to the heavier container?

Before you answer, have a look at these two boxes. On one of the boxes is written "correct". On the other, "incorrect". In each of these boxes are 50 counters. That is supposed to indicate that there is a 50/50 chance of you identifying the heavier countainer. If you feel more confident than that, you can move as many counters as you like from the "incorrect" box to the "correct" box. If you feel it is just a matter of chance, then you leave it as 50/50. But the more confident you are, the more counters you should move to the "correct box".
[Ask subject to move counters; count the number they move; record the answer; put the counters back in the original box to restore 50/50]

Now what about the other two containers, the ones about which we said that if they try hard many people can tell the difference? ? **[point to the table where the subject was doing the 2g task]** If we'd asked you to weigh those containers 100 times, in the same way as before, how many times do you think you'd have pointed to the heavier container?

As before, if you feel it is just a matter of chance, then you leave it as 50/50. But the more confident you are, the more counters you should move to the "correct box".
[Ask subject to move counters; count the number they move; record the answer; put the counters back in the original box to restore 50/50]

RESOLUTION

[For each individual subject, show bag with numbers 1, 2 and 3 in them. Explain that number 1 stands for the weighing task on the left side of the room, number 2 for the weighing task on the right side of the room, and number 3 for the disentanglement puzzle; draw a numberout of the bag; observe their recorded decision (which container correct/when did they stop), weigh the containers if weighing task is chosen & pay them accordingly; explain carefully how the amount they receive was arrived at and ensure they understand and agree]