An endowment effect for risk levels: Evidence from a Ugandan lab☆

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A R T I C L E I N F O

A B S T R A C T

Risky choices often have a natural starting-point. For example, insurance-type decisions concern reducing risk while investment-type decisions concern increasing it. A recent reference-dependent utility model predicts such references influence behaviour. Stochastic references act as endowments, with riskier references leading to riskier choices. We test this prediction in an identical choice set using a between-subject design in rural Uganda. Subjects are subtly assigned to one of three reference lotteries. On average, those with riskier endowments risk half a standard deviation more coins. We also consider the effect of introducing a new stochastic reference. In a within-subject second round, we test whether a social signal acts as a competing reference. In our experiment, information on peers' choices is a stronger pull than the initial treatment effect. On average, subjects converge to the social signal by 0.37 for each unit of difference. Previous research focuses on the absence or presence of risk, allowing either the reference or prospect to be non-degenerate. Our results allow both to contain an element of risk, and show that the endowment effect can operate on the level of risk: risky choice is influenced by the riskiness of the reference.

1. Introduction

Does the riskiness of someone's starting position affect their risky choice? For example, imagine a farmer considering investing in fertiliser for the first time. This would increase their expected income, but also mean greater volatility. Their reference is of a lower but more stable income, and they weigh up taking on more risk. Alternatively, imagine they are considering a new index insurance product. This pays out in the case of poor rains, reducing the volatility and expected level of income. Here, the reference is of a higher and less stable income, and they weigh up decreasing both risk and

income. In both cases, the farmer faces a trade off between expected income and risk. Crucially however, the reference differs: it is safer in investment-type decisions and riskier in insurance-type decisions. Are such risky choices immune from endowment effects, or could the risk level act as a reference?

While the endowment effect was established in risk-free experiments (see Ericson and Fuster, 2014, for a review), theoretical work has since added an element of risk. Koszegi and Rabin (2007, hereafter, KR) predict an endowment effect for risk, with risky endowments lowering risk aversion. This has been tested where either the reference or the prospect (but crucially, not both) involves an element of risk. Sprenger (2015) document a gap between the willingness-to-accept and willingness-to-purchase lotteries (see also Knetsch and Sinden, 1984; Kachelmeier and Shehata, 1992). Outside of the lab, Anagol et al. (2018) found a similar result in a large natural experiment in India, involving 1.5 million people. These results are consistent with an endowment effect for risk when either the reference or prospect involves some risk.

However, most risky choice regards the level of risk and not simply whether to avoid or accept it. Likewise references themselves naturally differ in their level of risk. Having considered the effect of non-degenerate prospects or references, the next logical step is to allow both to be non-degenerate. We show that KR has implications for such choices. KR implies an endowment effect for risk levels, with riskier references leading to riskier choices. Current empirical tests of KR have been more limited than the theory.

We test the prediction that, ceteris paribus, more widely distributed reference lotteries yield less risk aversion in an artefactual field experiment among 292 randomly selected participants from farming villages in eastern Uganda, using a between-subject design. We implement an investment-like treatment, which has a relatively safe starting point, and an insurance-like treatment, which has a relatively risky starting point. Subjects face an identical choice set, choosing how many coins to risk out of 10, where risked-coins are doubled in value with an 80% chance, and worthless with a 20% chance. We find that subjects risk an average of 5 coins in the investment treatment, compared to 6.4 in the insurance treatment (and 6 in the neutral treatment). The difference, around half a standard deviation, is statistically significant and robust to controlling for socio-economic characteristics. Our key finding is that a relatively risky starting point yields less risk aversion than a relatively safe starting point, even in an identical choice set. There is a strong endowment effect for risk levels.

The validity of our test hinges on the assumption that the starting points we provide in the experiment act as reference lotteries for the subjects participating in them, rather than as advice or clues about what experimenters expect of them. We have sought in the design to assign reference lotteries as subtly as we could: by pre-placing some of an endowment in a "safe" basket and some in a "risky" basket.[2] Moreover, we emphasised strongly in the script that there are no right or wrong choices, but that we were simply interested in how people in eastern Uganda take decisions. Finally, it is not likely that the starting points were interpreted as advice, given actual behaviour: some 85% of subjects deviated from the starting point in the lottery they chose: it appears to exert a pull on, but it is not a focal point for behaviour.

In our experiment, despite subjects having complete freedom to choose one out of eleven lotteries, the subtle assignment of an initial lottery thus exerts a large influence on the lottery that is chosen between treatments that are identical in the choice set presented to subjects and differ only in that initial assignment. In the initial framing, we purposively mimic the way insurance and investment decisions naturally present themselves in real life. We next investigated whether *altering* stochastic referents matters for risky choice behaviour in the manner predicted by KR.

Social effects are a plausible transmission mechanism for altering the stochastic referent[3] and so we test for this using a within-subject design. In a second round of the experiment, subjects were informed of the most popular choice in a parallel session. We find social effects that are larger than the effect of the framing found in the main experiment, i.e. of subtly suggesting a reference lottery. The propensity to change the number of coins risked in the two rounds, and the direction of any change, is strongly influenced by the social mode. A bivariate regression estimates convergence of 0.369 per unit of difference.

Our experiment took place in rural Uganda. This is a fitting setting to test for an endowment effect for risk levels, and has an apparent example of its effect outside of the lab. In rural Uganda, people are used to high exposure to risk, low use of fertiliser and (until very recently) an absence of insurance products. KR's theory pertains to situations where there is a clear reference, and in both cases there are clear references of little or no purchase of either investment or insurance. This mimics our experimental set-up, and shows that people do not bring strong references of either high or low risk aversion to all risky choices. Furthermore, villages in rural Uganda are a small-scale societies and so the social effects of the second round are also relevant to the local context.

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[2] An alternative would have been to assign reference lotteries not only randomly (which we did) but also transparently, for instance by tossing a coin for a subject to see, as in the tests of reference-dependent preferences of Ericson and Fuster (2011), Heffetz and List (2014) and Goette et al. (2019). However, this would have precluded pursuing our interest in the effect on behaviour of features of the decision interface—of naturally suggested courses of action. Moreover, we worry that transparently randomly assigning an endowment or a referent does not necessarily alleviate experimenter demand concerns: subjects may still (truthfully) infer that experimenters hope and expect their behaviour to be influenced in particular ways by the random assignment they (subjects) witnessed, and adjust their behaviour accordingly.

[3] There is a body of evidence regarding how social networks influence people's risky choice, with several different conceptualisations (Conley and Udry, 2010; Bandiera and Rasul, 2006; Karlan et al., 2014; Cai et al., 2012). We have in mind the mechanism where another's choice may change one's expectations about 'normal' behaviour in a given setting. This is different from the mechanism tested and confirmed in the experiments of Schwerter (2013), in which the motivation to catch up with a paired person's earnings is found to influence risky choice behaviour.

Our findings also shed some new light on models of expectations-based reference-dependent preferences. We test whether more widely distributed reference lotteries yield more risk taking. This is an important, plausible and previously untested KR prediction.[4] In risky choice decisions, experimental evidence for an endowment effect for risk (Sprenger, 2015; Knetsch and Sinden, 1984; Kachelmeier and Shehata, 1992) provides support for the KR theory so long as the stochastic referent is altered by the experimental manipulation.[5] When the endowment effect for risk is tested for, subjects choose between sure amounts and gambles.[6] By contrast, we do not compare between certain reference points and stochastic reference points but between varying stochastic reference points. This is qualitatively different from tests for the endowment effect for risk. As Kahneman and Tversky (1979) pointed out when motivating their original articulation of prospect theory, certainty exerts a particular pull at the expense of any gamble compared with it that would not have been present had the comparison been between gambles. Sprenger (2015) shows that this pull greatly diminishes when the gamble is presented as the first option and the certain amount as the second, rather than the other way around. However, the pull of certainty may still be an influential confound when the interest is in how risky choice is affected by the distribution of the reference lottery. When that is the interest, it is crucial to ensure that reference lotteries are non-degenerate.

The rest of the paper is structured as follows: in Section 2 we obtain theoretical predictions for our experiment and present the experimental design. Sections 3 and 4 contain the analysis of the first and second round decisions, with Section 5 exploring alternative explanations. Section 6 discusses the results in the context of persistent findings of underinvestment and underinsurance in developing countries, and Section 7 concludes.

2. Experimental design and theoretical predictions

In this study, we investigate the influence of stochastic referents on risky choice. For that purpose, we designed and implemented an experiment in which subjects face binary referent gambles $G = (p; r_1, r_2)$, in which reference outcome r_1 occurs with probability p and reference outcome r_2 with complementary probability $1 - p$. The binary referent gambles G vary across our experimental treatments in terms of the width of their distribution: for a given p, $r_2 - r_1$ is experimentally varied. We investigate whether a riskier reference leads to subjects choosing a riskier binary gamble $F = (p; x_1, x_2)$. In this section, we present the experiment and obtain theoretical predictions.

2.1. Field experiment

To examine the effect, if any, of changing the distribution of a stochastic referent, we used an artefactual field experiment with 292 randomly selected participants from a predominately agricultural community in Eastern Uganda.[7] Before the experiment began we explained that they would be asked to make two decisions, one of which (decided by a coin toss) would be played out for real. In the experiment each subject was endowed with 5000 Ugandan Shillings in the form of ten 500 shilling coins, approximately a local daily wage. Each participant was asked to distribute the coins between two options, represented by a safe basket and a risky basket. Each coin placed in the safe basket meant a secure income, whereas each coin put in the risky basket offered an 80% chance of being doubled in value and a 20% chance of becoming worthless. Chance was determined at the end of the experiment by picking one counter out of a bag with 4 white and 1 green counters. If the subject picked the green counter all the coins in the risky basket would become worthless, whereas if a white counter was picked they would double in value. Table 1 shows the pay-off table of the different options, with expected values and spread.

To present the choice task, all subjects from a session approached a table where an experimenter explained the decision problem using the coins, and two baskets representing the safe and risky options. Subjects then returned to a waiting area (conferring was not allowed) before being recalled to answer control questions and make their decision in private. The same lottery game was presented in one of three ways to separate groups of participants: where inaction leads to a risky choice (insurance treatment); where inaction leads to a safe choice (investment treatment); and where inaction is not possible (neutral treatment). In particular, the three treatments differed in the starting position of the coins, both during the explanation of the game and when the subject approached the table to make their decision. In the insurance treatment 9 coins started in the risky basket and 1 in the safe basket. In the investment treatment the starting positions were reversed. In the neutral treatment 1 coin was placed in the safe and 1 in the risky basket, with 8 coins placed in between. Consequently, the instructions given to the participants and the option set across the three treatments were the same: the only difference was the initial distribution of the coins between the baskets.

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[4] Outside the domain of risky choice, previous studies have looked for evidence for expectations-based reference dependence in naturally occurring data on golf players' performance (Pope and Schweitzer, 2011), domestic violence (Card and Dahl, 2011), New York City cabdrivers' labour supply decisions (Crawford and Meng, 2011), and food choices (Karle et al., 2015); and in tailor-made endowment effect (Ericson and Fuster, 2011; Heffetz and List, 2014) and real-effort experiments (Abeler et al., 2011; Gill and Prowse, 2012), with mixed support.

[5] Sprenger (2015) estimates a KR model under the identifying assumption that the referent is the first presented option in a variety of certainty equivalent and probability equivalent tasks; its within-sample and out-of-sample performance suggests that the referent is indeed the first presented option.

[6] To test validity, Sprenger (2015) also adds choices between gambles in uncertainty equivalent and inverted uncertainty equivalent tasks, but this does not permit a direct test of the proposition of core interest in our study.

[7] Two subjects took part in the experiment but not the survey.

Table 1
Lotteries: Expected Values and Spread.

Coins in safe option	Coins in risky option	x_1 $p = 0.8$	x_2 $1 - p = 0.2$	Expected Value	Spread
10	0	10	10	10	0
9	1	11	9	10.6	2
8	2	12	8	11.2	4
7	3	13	7	11.8	6
6	4	14	6	12.4	8
5	5	15	5	13	10
4	6	16	4	13.6	12
3	7	17	3	14.2	14
2	8	18	2	14.8	16
1	9	19	1	15.4	18
0	10	20	0	16	20

Amongst our subject pool literacy cannot be guaranteed, and so our choice of method was motivated by a desire to ensure understanding. The elicitation procedure we use is closest to Gneezy and Potters (1997, hereafter GP), who frame a choice problem as an investment game where each subject is given an endowment X, of which they may choose to invest $x \in [0, X]$. The part invested has an uncertain yield such that the individual's final payoff will be $X - x + kx$ with probability p and $X - x$ with probability $1 - p$ (see Charness et al., 2013, for a survey on this method).[8] Levels of comprehension were high, with 92% of subjects answering all control questions correctly.[9]

In the second half of the experiment, subjects were then presented with the same decision at a different table laid out in the same way. Two sessions ran in parallel with approximately 9 subjects per session, sharing a common waiting area but using a different table in the first round before switching for the second round. Before making their second round decision, they were told of the most popular decision on the new table in the previous round, i.e. the modal decision of the subjects who had played at that table in the first round. For all 16 pairings and the most popular choice by session, see Table 7.

The experiment took circa two and a half hours, and average earnings were 9000 Ugandan shillings once an unannounced show-up fee of 2000 is included (final pay-offs are equal to around twice a local daily wage). All other standard procedures (including random cluster sampling of the local area (detailed below), random allocation to treatment, optional participation at all stages, no communication and anonymity) were followed, and the experimental script is available as appendix A. The experiment was complemented by a questionnaire organized a few weeks before the experiment, which captured data on individual and household characteristics.

Experimental participants were selected in line with the standard practice of the organisation implementing the fieldwork. Five sub-counties were randomly selected from the districts of Sironko and lower Bulambuli, which together comprise the former Sironko District in eastern Uganda. For each of the selected sub-counties a list of all villages was obtained, and from each sub-county 10 villages were randomly selected. Working together with village chiefs and their assistants, for each of the selected villages, a list of all (18+) adults was obtained, organised by household. Once the village-level sampling frames were produced, the village chief and a broad range of village representatives were invited to witness the selection in all its details. Consecutively numbered pieces of paper were placed inside a bag, as many pieces of paper as there were households in a village. 20 households were then randomly selected for each village. For each household (one at a time) consecutively numbered pieces of paper were again placed in a bag, as many as there were adult household members. One piece of paper was selected randomly and that determined who we invited for the research. If that person was not available, a new household and member were selected following the same procedure as outlined. The sample selected was then randomly allocated to the experiment reported on here and other experiments conducted during the same fieldwork period in the area.

2.2. Theoretical predictions for the experiment

In this subsection, we spell out the theoretical predictions for the experiment, finishing with a non-technical summary. Subjects choose one out of eleven possible binary gambles $F = (0.8; x_1, x_2)$ with $x_1 = 10 + x$ and $x_2 = 10 - x$ by investing $0 \leqslant x \leqslant 10$ coins of 500 shillings. In a between-subject design, we study the influence of reference lotteries $G = (0.8; 11, 9)$ and $G' = (0.8; 19, 1)$ on investment x.[10] In a within-subject design, we also study the influence of the social mode $G'' = (0.8; r_1'', r_2'')$, which in practice is mostly a more widely distributed reference lottery than G and less so than G', i.e. $19 > r_1'' > 11 > 9 > r_2'' > 1$.

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[8] A related method used by, for example, Eckel and Grossman (2002) is framed as a free choice between various binary lotteries which have a constant relation between proximate options. Conceptually the two approaches are identical; one presents the mechanism and the other the possible final options. We presented the choice using the GP method, as it is easier to understand.

[9] Three control questions checked for the understanding of mechanisms, with 6% answering one incorrectly, 2% answering two incorrectly and none answering all incorrectly. Because misunderstanding is shown to be rare, we do not drop any data for reasons of comprehension.

[10] The neutral treatment's reference is $(0.8; 3, 1)$, assuming the coins between baskets are not included in the reference.

It is straightforward to see that in models without stochastic referents, referent gamble G should have no bearing on decision making. So under expected utility, subjects maximise $p.u(x_1) + (1 - p).u(x_2)$, with $u(x)$ denoting utility of an outcome x, representing a transitive and complete preference ordering. Clearly, G does not feature.

In models with non-stochastic referents, referent gambles are likewise not recognised. In Tversky and Kahneman (1992) articulation of prospect theory decision makers maximise in our binary environment $\pi(p).v(x_1|r) + [1 - \pi(p)].v(x_2|r)$, where $\pi(p)$ represents any nonlinear probability weighting function and $v(x|r)$ loss-averse utility relative to deterministic reference point r. Referent gambles again do not feature, so experimental attempts to manipulate the riskiness of a referent should be irrelevant according to these models. An interesting transition towards KR's incorporation of stochastic referents are the disappointment aversion models of Bell (1985), Loomes and Sugden (1986) and Gul (1991). Here the referent is equal to the certainty equivalent value of the lottery that is chosen. In our set up, if we denote the chosen lottery as $F^* = (p; x_1^*, x_2^*)$, then this is evaluated relative to c, its certainty equivalent value, so $F^* = (p; x_1^*, x_2^*|c)$. Although the referent now derives from a lottery, it is non-stochastic in its influence on the evaluation of lotteries in the choice set. Only the chosen lottery, in our case lottery F^*, should influence risky choice according to disappointment aversion models, and so they do not predict an endowment effect for risk levels.

In Koszegi and Rabin (2006) a model of reference-dependent preferences is introduced in which utility consists of two parts: consumption utility and "gain-loss" utility. The former is subject to the assumption conventional in expected utility theory of diminishing marginal utility of wealth and the latter is thought of as derived from the difference between the consumption utility that corresponds with a reference and actual consumption utility. The model is applied in KR to study preferences over monetary risk and we follow the latter's notation. Utility u derived from wealth w is defined as

$$u(w|r) = m(w) + \mu(m(w) - m(r)), \tag{1}$$

where $m(w)$ is reference-independent consumption utility and μ converts deviations from a reference level of consumption utility into gain-loss utility. Similar to prospect theory μ exhibits loss aversion, and is concave in gains and convex in losses. Probability weighting is abstracted from. The reference, given by r, is allowed to be stochastic. When a lottery F is evaluated, it is compared with reference lottery G:

$$U(F|G) = \int \int u(w|r) \, dG(r) \, dF(w) \tag{2}$$

For the binary gambles $F = (p; x_1, x_2)$ and $G = (p; r_1, r_2)$ we are considering in this study, the utility of lottery F can thus be expressed as:

$$\begin{aligned}
U(F|G) = \; & p \cdot p \cdot [m(x_1) + \mu(m(x_1) - m(r_1))] \\
& + p \cdot (1 - p) \cdot [m(x_1) + \mu(m(x_1) - m(r_2))] \\
& + p \cdot (1 - p) \cdot [m(x_2) + \mu(m(x_2) - m(r_1))] \\
& + (1 - p) \cdot (1 - p) \cdot [m(x_2) + \mu(m(x_2) - m(r_2))]
\end{aligned} \tag{3}$$

The evaluation of binary gamble F against reference gamble G thus takes place as follows. There are two possible outcomes and two possible reference outcomes, giving rise to four possible combinations: obtaining x_1 against reference r_1, x_1 against r_2, x_2 against r_1 and x_2 against r_2. The utility of each possible combination of outcome and reference (consumption plus gain-loss utility) is multiplied by its associated joint probability, and the utilities thus weighted are summed to yield the "expected" utility of lottery F.

The reference lottery G is conceptualised as recently held probabilistic beliefs about outcomes: expectations about risk. For situations in which probabilistic beliefs about outcomes are exogenous to the actual choice set, so-called "surprise" decisions, KR develop two propositions.[11] Proposition 1 states that a lottery evaluated relative to a deterministic reference level of wealth can be no less attractive when it is evaluated relative to a reference lottery, which implies an "endowment effect for risk" when the reference lottery is the evaluated lottery itself. Such an endowment effect, in which a lottery is valued more when owned, has been found in the lab (Knetsch and Sinden, 1984; Kachelmeier and Shehata, 1992; Sprenger, 2015), the last mentioned being an explicit test of KR's proposition 1.

Proposition 2 states that a sufficiently widely distributed reference lottery induces approximate risk neutrality. We test its implication that, ceteris paribus, more widely distributed reference lotteries decrease or not increase risk aversion. Formally, KR show that for certain existing $A, \varepsilon > 0$, all constants k, any lottery F with positive expected value and any lottery H, $U(H + F|G) > U(H|G)$ when $Pr_G[r \in (k + A, k - A)] < \varepsilon$. In words, if G is sufficiently widely distributed, then F will be accepted. An immediate corollary is that if lottery G' satisfies $Pr_{G'}[r \in (k + A + v, k - A - v)] < \varepsilon$ for $v > 0$ and the $A, \varepsilon > 0$ delimited in the proposition, then $U(H + F|G') \geq U(H + F|G)$. Intuitively, since the proposition states that for a sufficiently widely distributed reference lottery, F will be accepted, the decision-maker will be no less willing to accept F for an even more widely distributed reference lottery.

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[11] When it is implausible that probabilistic beliefs are exogenous to the choice set, because risks are anticipated, the decision maker's referent gamble should be determined by prior considerations of what she would do if faced with that choice set. For such situations, KR develop a number of personal equilibrium concepts according to which experimental attempts to manipulate reference gambles should be irrelevant. Our test of whether assigned reference gambles influence risky choice, if confirmed, thus favours KR's model of risky choice as a "surprise" decision over both KR's major predecessors and over the personal equilibrium versions of KR.

In our binary environment, KR proposition 2 may be applied as follows. A decision maker will be no less willing to accept lottery $F = (p; x_1, x_2)$ when the reference lottery is $G = (p; r_1, r_2)$, with $r_1 = k + A$ and $r_2 = k - A$, than when it is $G' = (p; r_1', r_2')$, with $r_1' = k + A + v$ and $r_2' = k - A - v$. We test this application of KR proposition 2.

To summarise, the model predicts greater risk aversion when the reference is relatively safe, and greater risk taking when the reference is relatively risky. A non-technical way of explaining these predictions is as follows. Since changing the reference lottery does not alter consumption utility, the only way that it changes the expected utility of a lottery under consideration is through the gain-loss utility terms. Each of the two possible outcomes of a prospect are compared to the two possible outcomes of the reference, giving rise to four such terms. Each gain-loss utility term is weighted with the joint probability of a particular outcome in the chosen lottery and a particular reference in the reference lottery. Whenever the former is lower than the latter, a loss is registered by the decision maker. Because of loss aversion, losses are felt more heavily than gains of identical size. For a relatively risky lottery in the choice set, a more widely spread reference lottery will reduce the proportion of incidences in which a loss is registered; hence the riskier reference pulls subjects towards riskier lotteries.

3. Analysis of 1st round treatment effects

The first round decisions are displayed in Fig. 1, with summary, test, and frequency statistics in Tables 2–4. Summary statistics of some basic characteristics are displayed in Table 5, broken down by treatment. Furthermore, a simple regression analysis of the results is shown in Table 6, where the dependent variable is the number of coins risked.
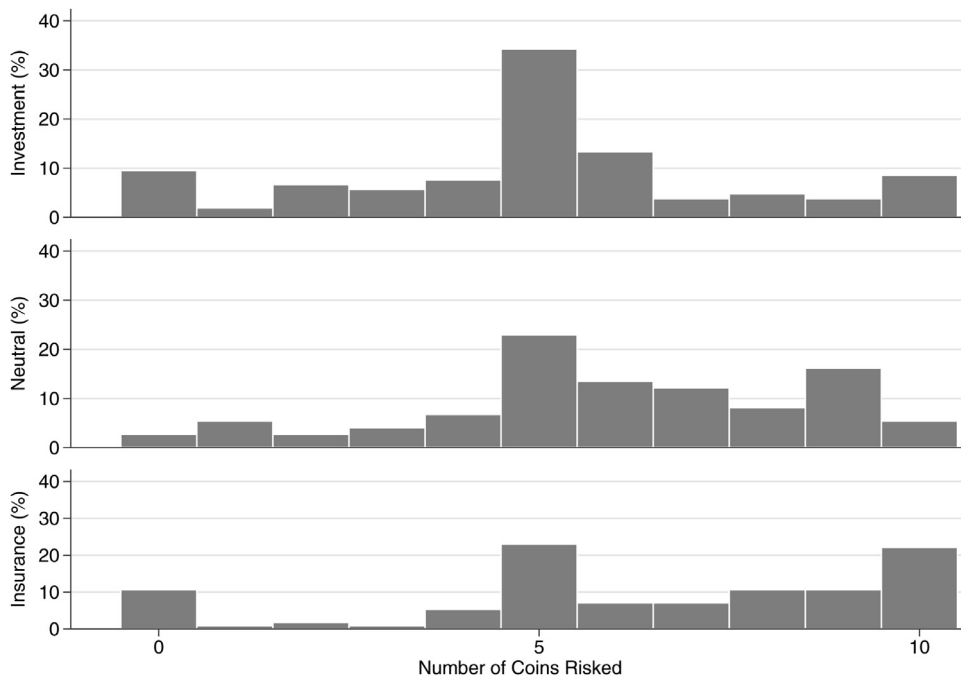
Fig. 1. Number of Coins Risked, by Treatment.

Table 2
Summary Statistics.

Treatment	Mean	S.D.	N
Investment	4.99	2.67	105
Neutral	5.96	2.55	74
Insurance	6.37	3.13	113
Total	5.77	2.88	292

Table 3
Two-Tailed *T*-Test for Difference in Means.

Test	T Stat	P Value
Invest. = Insurance	3.49***	0.001
Invest. = Neutral	2.43**	0.016
Neutral= Insurance	0.94	0.345

Table 4
Number of Coins Risked, by Treatment.

Coins Risked	Treatment			Total
	Inv.	Neut.	Ins.	
0	10	2	12	24
1	2	4	1	7
2	7	2	2	11
3	6	3	1	10
4	8	5	6	19
5	36	17	26	79
6	14	10	8	32
7	4	9	8	21
8	5	6	12	23
9	4	12	12	28
10	9	4	25	38

Table 5
Individual and Household Characteristics, by Treatment.

	Treatment		
	Inv.	Neut.	Ins.
Female	51%	52%	58%
Age	42	41	39
(Stan. Dev.)	14	14	14
Married	13%	18%	19%
Catholic	33.3%	21.6%	39.8%
Anglican	33.3%	41.9%	31.9%
Muslim	9.5%	8.1%	7.1%
7th Day Ad.	1.0%	0%	2.7%
"Born Again"	18.1%	23.0%	15.0%
O. Protestant	3.8%	4.1%	3.5%
Total	105	74	113

Note: For Table 5 we tested for balance on covariates between treatments using a multinomial logit with the treatment as the dependent variable. The nine independent variables above are then included as right hand side variables, and we test for joint significance using a χ^2 test with 16 degrees of freedom. The test statistic (14.13, $p=0.59$) indicates the treatments do not look unbalanced on observable characteristics.

There are three aspects to highlight. First, there is a significant and substantial treatment effect, which is the first evidence (which we are aware of) for an endowment effect for risk levels. Those in the insurance treatment risk an average of 1.38 more coins than those in the investment treatment (Table 2), despite the subtle assignment mechanism. This treatment effect is 0.5 standard deviations (Table 2) and around four times larger than the gender effect (Table 6). A two-sample *t*-test for a difference in means (Table 3) shows that the investment treatment is significantly different from the other two treatments at the 5% level. The same test does not show a significant difference between neutral and insurance treatments.[12] These results are also found in the regression results in Table 6, which allow for clustering at the session-pair and village level using Cameron et al's (2012) method. It may appear remarkable that the investment and neutral treatments are significantly different, as both treatments assign only one coin to be risked. The difference must then stem from subjects treating the coins in the safe basket and those placed between the two baskets differently.

Second, the observed behaviour is not consistent with inertia: only 2 people in the investment treatment and 12 people in the insurance treatment make no change (see Table 4). The evidence suggests that the treatment affects the attractiveness of the entire choice set, and is not consistent with a binary disutility incurred by deviating from the status quo. Third, a simplifying heuristic appears to have played a role in subjects' choices. The most popular option, chosen by 27% of subjects, is to risk exactly half of the coins.

4. Analysis of 2nd round treatment effects

Once subjects had made their first round choice they returned to a central waiting area. They were then told that they were to go to the other table to face the same decision problem, where they would be told of the most popular decision on that table in the previous round. This allows us to measure the effect of a new reference, comparing the pull from the starting position of coins to the pull from announcing the social mode (i.e. the most popular choice in a paired session). All

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[12] The non-parametric Epps-Singleton test does report a significant difference (W=20.260, one-sided $p = 0.0002$) between the neutral and insurance treatments.

Table 6
Regression Analysis of First Round Choices.

	(1)	(2)	(3)
Neutral Treatment	1.008**	1.039***	1.035**
	(2.49)	(2.64)	(2.48)
Insurance Treatment	1.352***	1.356***	1.315***
	(2.89)	(2.95)	(2.77)
Female		0.460	0.335
		(1.42)	(1.04)
Age		0.00373	0.0110
		(0.30)	(0.86)
Married		0.781**	0.794**
		(2.18)	(2.27)
Catholicism			0.771
			(1.25)
Anglicanism			0.280
			(0.42)
Islam			0.666
			(0.82)
7th Day Adventists			0.616
			(0.53)
'Born Again'			1.460*
			(1.77)
Constant	5.019***	6.740***	7.553***
	(21.27)	(8.73)	(7.77)
Observations	290	290	290

Note: The dependent variable is the number of coins risked in the first round. Robust t statistics (in parentheses) are taken from a regression with standard errors clustered at the session-pair and village level, using `cgmreg`'s wild cluster bootstraps. Note that session-pair ($N=16$) and village clusters ($N=45$) are cross-cutting. For details on how sessions are paired, see Table 7. ***, ** and * denote significance at the 1%, 5% and 10% level respectively. All coefficients on religion should be understood in comparison to 'other protestant'.

Table 7
Most Popular First Round Choice, by Session.

Table A				Table B			
Session	Treatment	Social Mode	n	Session	Treatment	Social Mode	n
1	Investment	5	8	2	Insurance	5	10
3	Investment	5	8	4	Insurance	4	8
5	Investment	10	10	6	Insurance	5	9
7	Investment	9	7	8	Insurance	5	8
9	Investment	9	10	10	Insurance	2	11
11	Investment	5	10	12	Insurance	5	11
13	Neutral	10	8	14	Investment	6	10
15	Neutral	5	10	16	Investment	5	10
17	Investment	5	6	18	Investment	5	5
19	Investment	5	10	20	Investment	10	11
21	Neutral	9	9	22	Insurance	5	5
23	Neutral	5	7	24	Insurance	9	10
25	Insurance	10	10	26	Insurance	5	10
27	Insurance	0	10	28	Insurance	10	11
29	Neutral	9	9	30	Neutral	3	10
31	Neutral	5	9	32	Neutral	5	11

Note: Each row shows one set of paired sessions, with tables A and B paired. 'Social Mode' in each case shows the most popular number of coins risked. In round 2 Table A received Table B's first round signal, and Table B received Table A's. Session sample size is given by n.

pairs are shown in Table 7, for example session 9 was told of the most popular choice in session 10's first round, and vice versa.

For us to examine the effect of the social mode, we require variation in the distance between one's own first round choice and the paired sessions' modal choice. For this reason we chose to pair the sessions we expected to be most different more often. In practice this meant using the investment-insurance pair six times, and all other combinations twice.
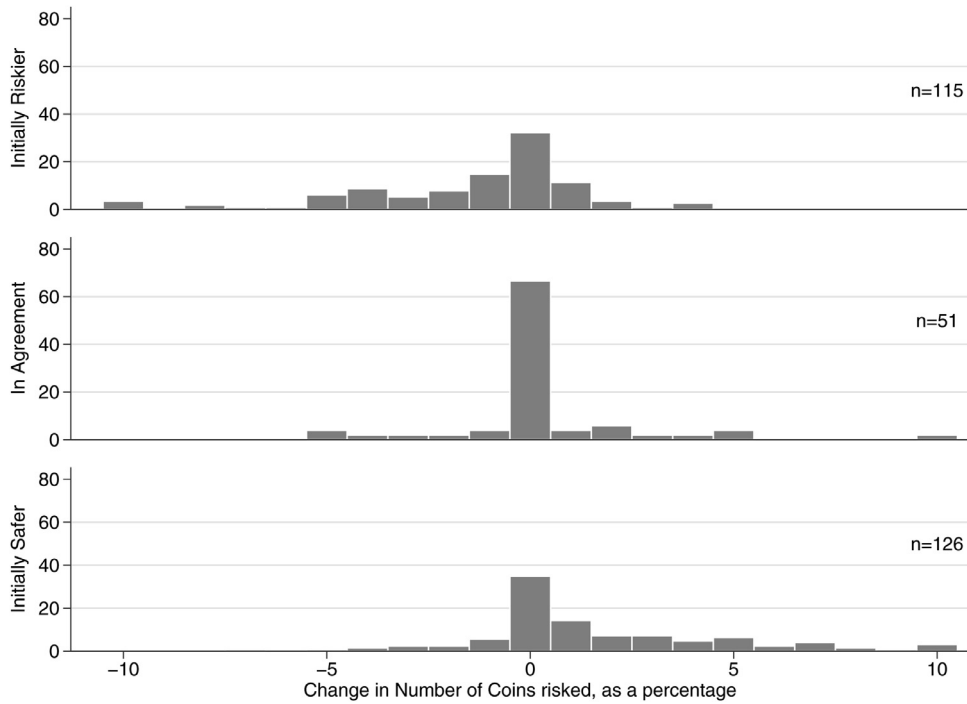
Fig. 2. The Difference Between Coins Risked in the Two Rounds, by Social Signal.
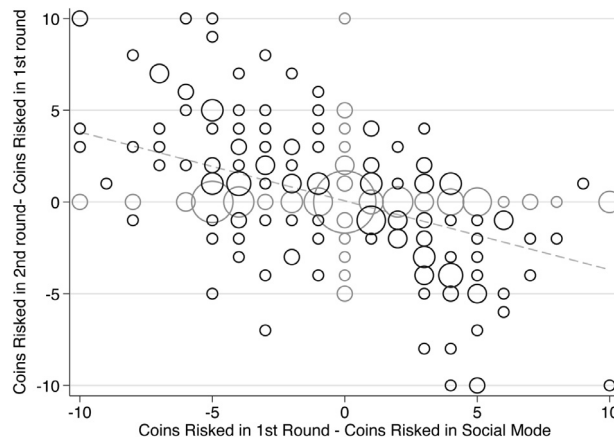

Fig. 3. Adjustment Towards the Social Mode *Note:* Those at zero on either axis are included in a lighter gray, to aid readability. The size of the circle indicates the frequency of a particular data point.

What characterises people's behaviour in the second round? The majority risk a different number of coins in the two rounds; only 115 people (39%) choose to risk the same number of coins in both rounds. Fig. 2 plots these changes in coins risked on the x axis. The top panel is for people who initially risked more coins than the socially most popular option (the magnitude of this difference is not shown here), the middle panel for those that had risked the same number of coins as the most popular option and the bottom panel for people who risked fewer coins than the most popular option. The figure illustrates several points. First, the middle panel relates to the 51 subjects who agreed with the social mode (i.e. they risked the same number of coins as was the most popular option on the other table). Of these, exactly two-thirds made the same choice in both rounds. By contrast, subjects that received a social mode that differed from their first choice were consistent in only around 30% of cases. This is clearly evidence of a social effect, as the propensity to change is influenced by one's peers. Second, Fig. 2 shows that the direction of change is overwhelmingly towards the social signal as those in the top panel tend to risk fewer coins, and those in the bottom panel tend to risk more.

Fig. 2 does not include information on the distance between the social signal and an individual's choice. For this analysis, Fig. 3 plots the difference in the number of risked coins between the two rounds on the *x*-axis and the difference between

Table 8
Regression of the Change in Coins Risked.

	(1)	(2)	(3)
1st Decision - Social Signal	0.369***	0.369***	0.374***
	(7.55)	(7.66)	(8.22)
Gender		0.0276	0.0170
		(0.09)	(0.05)
Age		0.00166	0.00391
		(0.15)	(0.31)
Married		0.0943	0.0690
		(0.22)	(0.17)
Catholicism			1.402
			(1.64)
Anglicanism			1.232**
			(2.09)
Islam			1.112
			(1.48)
Seventh Day Adventists			1.453
			(1.48)
'Born Again'			0.984
			(1.14)
Constant	0.0454	0.0473	1.178
	(0.18)	(0.05)	(1.02)
Observations	289	289	289

Note: The dependent variable is the change in the number of coins risked between the second and first rounds. For other notes, see Table 6.

the number of coins risked in the second round and the most popular choice in the first round on the *y*-axis. The line of best fit corresponds to a coefficient of 0.369 in a bivariate regression (Table 8), meaning that for each unit of difference between a subject's own choice and the social mode, we should expect convergence of 0.369. By contrast, almost none of the controls (gender, age, marital status and religion) are close to significance; there are no general shifts in risk taking in the second round.

How should we interpret the size of the social mode effects? A simple comparison to the effect of the default lottery is informative, where the average difference in the number of coins risked between risky and safe treatments is 1.4. These two treatments differ by 8 coins in their assignment to the risky basket. The social mode sees convergence of 0.369 per unit of difference, which over 8 units of difference translates to around 3 units of convergence. This is *prima facie* evidence that the social mode in this experiment is stronger than the reference implied by the starting position of the coins.

5. Alternative explanations

A number of different related theories could plausibly influence the results. Some of these are difficult to test. For example, we believe our design reduces the possibility that experimenter demands effects play a role in our results. Assignment is subtle, and subjects are not aware that other treatments exist. Subjects could not easily guess the intended wishes of experimenters, making it difficult to alter their behaviour in order to meet these wishes. However it is possible that experimenter demand has some effect, especially in the second round where the social mode is announced. Likewise the second round effects could be interpreted as being evidence in favour of social learning models, where subjects incorporate information on peers' choices. We cannot rule out that effect, as it could plausibly resemble (or operate via) stochastic references in many domains.

We can test other alternative explanations, namely default bias and transaction costs, as they make explicit predictions that differ from our preferred explanation. To do this we use a mixed logit model, referred to as the mixed multinomial logit model by McFadden and Train (2000) and the random-parameters logit model by Cameron and Trivedi (2005). It allows us to estimate the elements of a choice which affect the likelihood of it being chosen, whilst allowing for unobserved heterogeneity. Let the utility of each option *a* in round *r* for individual *i* be given by

$$U_{iar} = \mathbf{x}_{iar}\beta_i + \epsilon_{iar} \tag{4}$$

where $\mathbf{x}$ is a vector of alternative-specific variables, i.e. $\mathbf{x}$ captures relevant characteristics of a given option. β_i are random coefficients that vary over individuals in the population, and ϵ_{iar} is an error term following the type I extreme value distribution. The probability of choosing a given option is given by $P_{iar}(\beta) = \frac{e^{\mathbf{x}_{iar}\beta_i}}{\sum_{a=1}^{a=A} e^{\mathbf{x}_{iar}\beta_i}}$, and the model is estimated using maximum simulated likelihood.

We estimate a simple reduced form equation, capturing these alternative explanations for our results. We capture the riskiness of each option using the expected value ($\beta_1 EV$) and its square ($\beta_2 EV^2$). Concave consumption utility would be

Table 9
Mixed Logit, Both Rounds.

		(1)	(2)	(3)	(4)
Expected Value					
β_1	Linear	1.612**	1.035	1.322	0.877
		(2.03)	(1.35)	(1.60)	(1.08)
β_2	Squared Term	0.0525*	0.0303	0.0397	0.0223
		(1.76)	(1.05)	(1.28)	(0.74)
	SD of Squared Term	0.0194***	0.0197***	0.0176***	0.0175***
		(6.36)	(6.35)	(5.00)	(4.77)
Absolute Deviation from Assigned Choice					
β_3	Linear	0.115	0.167	0.0967	0.347***
		(0.86)	(1.63)	(0.63)	(2.92)
β_4	Squared Term	0.000805	0.0268**	0.0304*	0.0577***
		(0.06)	(2.31)	(1.65)	(3.72)
	SD of Squared Term			0.0351***	0.0400***
				(4.55)	(5.13)
Absolute Deviation from Social Mode					
β_5	Linear	0.248***	0.247***	0.242***	0.240***
		(3.44)	(3.41)	(3.23)	(3.15)
β_6	Squared Term	0.0222**	0.0222**	0.0204**	0.0200**
		(2.36)	(2.34)	(2.07)	(1.96)
	SD of Squared Term			0.00196	0.00310
				(0.09)	(0.12)
Dummy Variables					
β_7	Focal 5/5	1.431***	1.394***	1.394***	1.358***
		(12.04)	(11.78)	(11.66)	(11.43)
β_8	Default	0.933***		0.727**	
		(3.22)		(2.43)	
	Observations	6369	6369	6369	6369
	χ^2	283.6	275.5	270.3	260.4

Note: The dependent variable takes the value 1 if the option is chosen, and 0 otherwise. The coefficients can be interpreted as changes in implied utility, holding other elements constant. T statistics are provided in parentheses.

represented, respectively, by positive and negative coefficients. We allow for heterogeneous risk preferences by allowing β_2 to vary, and estimate its standard deviation. An extremely risk averse subject would be described by a large negative β_2, as they dislike the implied extra risk.

We use an analogous set up for the two references we consider. The pull of the starting position of the coins, the 'assigned choice', is captured by the absolute distance from a given option ($\beta_3|x - r|$, i.e. the difference in the number of coins risked) and its square ($\beta_4|x - r|^2$). We model the neutral treatment as having no assigned choice, as the initial placement of the coins does not match a possible option. These values are then zero, allowing cleaner identification of the assignment. Competing explanations make different claims about these terms. We cannot estimate a full structural model for KR, as we do not have sufficient variation to identify each element of Eq. (3). We estimate only the pull of deviations from the assigned lottery. KR assumes that μ is concave in gains and convex in losses, so each reference and prospect will combine convex and concave elements. KR does not make an unambiguous prediction here, only that it expects nonlinearity.

Competing theories do make clear predictions. In a transaction costs model a subject 'pays' some effort cost for moving a coin. This could conceivably lead to the treatment differences we observe, for example if those in the insurance treatment tend to risk fewer coins in part to avoid the extra costs of moving coins. In practice we find this somewhat implausible as the additional physical effort to move another coin appears negligible. However, this is a useful theory to test, as it can represent the idea that the pull of the assigned lottery is linear. It is possible to conceive of other theories that might make such a prediction. The idea of transaction costs is consistent with a linear pull of the assignment, i.e. a significant and negative effect of the assigned choice ($\beta_3 < 0$), with a small and insignificant coefficient on the squared term ($\beta_4 \approx 0$).

The pull of the social mode in round 2 is captured by analogous absolute distances from the social mode ($\beta_5|x - s|$, i.e. the difference in the number of coins risked) and that term squared ($\beta_6|x - s|^2$). Both rounds are included in the estimation, with no social mode in the first round allowing cleaner identification of the social signal's pull. KR would again allow nonlinearity.

In order to control for the focal option of risking half of the coins, we also use a dummy for that option, $\beta_7 focal$. This is included simply as a control, not as a direct test of a theory.

We can explicitly test default bias, which is also known as inertia. It has received compelling empirical support from hypothetical surveys (Samuelson and Zeckhauser, 1988; Ritov and Baron, 1992; 1995) and natural experiments (Madrian and Shea, 2001; Duflo and Saez, 2003). It is almost exclusively discussed in a binary or ordinal choice set, where proof of its effect takes the form of a difference in the popularity of an option when it is the default option to when it is not, with popular examples including organ donation and enrolment in a pension plan. We are only aware of two cases where continuous

options are examined, both unincentivised atheoretical hypothetical questions reported in Samuelson and Zeckhauser (1988). Choi et al. (2003) is representative of the theoretical understanding of default bias, where it is explicitly conceptualised as a cost of switching from the default, a cost that does not depend on the distance between the default and the alternative. They argue that sometimes defaults should be made deliberately 'bad' in order to encourage people to opt out of them. This illustrates the different implications of default bias versus a stochastic reference point: the latter exerts a pull on the entire choice set, and so a bad default would hold sway rather than being ignored. To test this we introduce a simple dummy variable capturing whether the option is the default ($\beta_8 default$), with the theory of default bias consistent with $\beta_8 > 0$.

In Table 9 we estimate Eq. (4) in four ways. In odd columns we include the *default* dummy, and in columns 3–4 we allow for unobserved heterogeneity in the pull of the two references. The results show that the average subject is risk averse (as $\beta_1 > 0$ and $\beta_2 < 0$), attracted to the focal 5/5 option ($\beta_7 > 0$) and dislikes departing from either reference ($\beta_4 < 0$ and $\beta_5 < 0$). Risking half of the coins is focal, with the implied extra utility around 1.4. We find strong evidence against default bias, with the option of not moving any coins seeing an implied disutility of 0.7–0.9.

The results show the pull of the assigned reference is non-linear, with an increasingly disutility of deviating from a given allocation ($\beta_4 < 0$). The results imply subjects actually gain utility from small deviations (in contrast to the theory of default bias) but that larger deviations incur a high cost. In the even columns, which do not include the default bias variable, we see the squared term is significant and negative. This is inconsistent with the idea that transaction costs drive the result, or indeed any theory which implies a linear pull of a given reference. The results are consistent with the convex loss function of μ dominating the concave gain element as one moves further from the reference. The pull of the social mode is also found to be significantly nonlinear, but with a negative linear term and positive squared term.

6. Discussion

In this section we first briefly discuss our contribution to the experimental literatures on risk preferences. Second, we discuss an example of how our findings relate to a common paradox in developing countries: the simultaneous persistence of underinvestment and underinsurance in farming communities. This illustrates how an endowment effect for risk levels may manifest itself outside of the lab.

6.1. Stochastic reference lotteries

In the first round of the experiment, we show that reference lotteries affect risky choice in a continuous option set. Changing the reference lottery has a significant and sizeable effect, despite the subtle difference in assignment. There is, perhaps surprisingly, even a significant difference between the neutral and investment treatments, despite the reference lottery in each case risking only one coin. This difference must then come from treating coins in the safe basket and those placed on the table between the two baskets differently.

In the experimental literature, our paper is most closely related to Sprenger (2015), who showed that in cases where either the reference *or* the prospect is a stochastic lottery there is an endowment effect for risk. This was established through finding a significant gap in the willingness to pay for a lottery when endowed with a certain amount, and the willingness to sell an endowed lottery. By using a different experimental set-up, we are able to explore a continuous option set where reference lotteries have different degrees of risk. We show that changing the reference lottery affects the entire distribution of choices, finding evidence that the spread of the reference affects one's evaluation of the entire option set.

In the second round of the experiment, the social mode has a large effect on subjects' choices. It affects the propensity to deviate from one's first round decision, and the direction of that movement. On average, subjects move 0.369 units towards the social mode for each unit of difference. These second round estimates should be treated with some caution, as we cannot rule out an experimenter demand effect in the second round. The social signal is explicitly announced to the subject, perhaps implying that they should respond to this new information. First round decisions do not suffer from this concern, as treatments are more subtle.

6.2. Underinvestment and underinsurance

A further innovation in this paper is to play the experimental games in rural Uganda, whereas previous related research was conducted in Western labs. This decision was motivated in part by two common puzzles. The first puzzle is that of underinvestment, found in many developing countries. Duflo et al. (2008) offer a compelling empirical example from Western Kenya: investment in fertilizer has an expected annualised return of 69.5%, but only 37% of the sample report ever having used fertilizer. The second puzzle is that of underinsurance, typically index insurance products for rural farmers that pay out in the case of poor rains (Karlan et al., 2014). It has been estimated that the effect on welfare of index insurance products is equivalent to an increase in consumption of almost 17%, yet take-up remains low (de Nicola, 2012). The most commonly repeated explanation is a lack of trust in or understanding of insurance products (Cai et al., 2012; Giné et al., 2008; Karlan et al., 2014). Index insurance products are relatively new in these contexts.

A common factor has been mentioned in both insurance and investment contexts: risk preferences. Low levels of real-world investment has been linked to observed high risk aversion in the lab (Liu, 2013). Conversely, low levels of insurance have been explained with reference to low levels of risk aversion (Zant, 2008). However, it cannot simultaneously be the

case that risk aversion is low enough to explain low levels of insurance behaviour, and high enough to explain low levels of investment behaviour.

We offer a simple observation: while insurance and investment decisions are conceptually identical (a choice between a higher paying but riskier option and a lower paying but safer alternative) they differ in their framing. In insurance decisions inaction leads to taking a riskier option, whereas non-investment leads to a safer alternative. This is not a superficial framing effect but rather a fundamental and innate aspect of investment and insurance decisions; nature's framing of investment and insurance decisions implies different reference lotteries even for decisions that are conceptually identical. Our test of the KR model confirms the importance of reference lotteries on risky choice, with experimental evidence that is consistent with the model's prediction that a reference lottery with a greater spread invokes more risk neutral behaviour. If our observation regarding nature's framing of investment and insurance decisions is valid, we would then expect both underinvestment and underinsurance where these products are new.

A number of papers have gone beyond offering explanations of underinsurance or underinvestment to offering or testing solutions. On the insurance side, Giné et al. (2008) hint that in India social effects may hold the key to combating low levels of trust and understanding, as a large proportion of people ask their friends before deciding whether or not to purchase the insurance product. Likewise, Cai et al. (2012) provides evidence that an insurance education program in China is subject to substantial positive spillover effects from participants to their friends. Karlan et al. (2014) built on some of these findings in designing a policy intervention to increase demand for index insurance, focusing on increasing trust for insurance products. It included providing free insurance for a randomly selected subgroup of Ghanaian farmers and found a significant increase in the demand for insurance products once the intervention had finished, especially amongst those who knew someone who had received a pay-out from the insurance company. On the investment side, Duflo et al. (2011) argue that while many farmers plan to use fertilizer, procrastination means that more pressing concerns take precedence. They report evidence that 'nudging' farmers by marketing fertilizer with a 15% discount in the period directly after the sale of crops increases fertilizer purchase substantially.

The model and experimental results presented here offer a new framework through which to interpret these successes, as each intervention discussed above can be seen as introducing a new reference. Duflo et al. (2011) argue the increase in fertiliser purchase is related to combating procrastination. However, as discount vouchers were disseminated in public gatherings such as churches and schools, this may have changed people's expectations of others' behaviour and challenged the local norm of low fertiliser purchase. Karlan et al. (2014) argue that the endowment of insurance increases insurance take-up by increasing trust, as insurance take-up is increasing in payouts to the farmer themselves and their social network. However, it is also possible that by endowing insurance a new less-risky (lower spread) reference is created, invoking less-risky behaviour.

Our results do not rule out procrastination or low trust as reasons for underinsurance and underinvestment, rather they offer a complementary explanation. Furthermore, our results provide further evidence that social information is a key channel for policy-makers who wish to combat underinsurance and underinvestment. Our results also suggest these two puzzles are not unrelated.

7. Conclusion

We apply KR's (Koszegi and Rabin, 2007) model of risky choice to a case where both prospect and reference are non-degenerate lotteries, where a stochastic prospect is evaluated against the full distribution of the stochastic reference. Pre-existing empirical evidence has been limited to allowing only one of the prospect and reference to be a lottery (Knetsch and Sinden, 1984; Kachelmeier and Shehata, 1992; Sprenger, 2015), and shows support for KR's prediction of an endowment effect for risk. Our novel setting allows us to present the first test of a corollary of KR: a reference lottery with a higher spread should invoke less risk averse behaviour. In an investment treatment, the reference lottery is relatively safe and in an insurance treatment it is relatively risky. We added a neutral treatment to this between-subject design, before using the second round to examine the effect of communicating the most popular choice in a first-round parallel session in a within-subject design.

Our experimental results find strong evidence that more widely distributed reference lotteries (in the form of the starting position of the coins) invoke less risk averse behaviour. Mean investment is around a quarter higher when the reference is relatively risky compared to when it is relatively safe. This difference is one half of a standard deviation, and around four times larger than the gender effect. This shows that loss aversion can be felt even over subtly assigned lotteries in a continuous choice environment. Moreover, behaviour in the second round shows that the pull of social reference point is even larger.

We discuss these results in the context of underinvestment and underinsurance in developing countries. This illustrates how an endowment effect for risk levels can manifest itself outside of the lab. We argue that while investment and insurance decisions are conceptually identical (both are choices between a riskier but higher paying option and a less risky but lower paying alternative) nature's framing of them means that we should expect underinvestment and underinsurance when new investment and insurance opportunities are introduced. The experiment mimics nature's framing of a risky choice problem, and finds that a subtle suggestion of a different reference lottery has a large influence. While investment and insurance are helpful pedagogical framings, many risky choices have a natural reference. Our results show that the riskiness of this reference can influence the entire choice set in new situations.

Declaration of Competing Interest

We declare we have no conflicting interests

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2020.12.013.

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