Journal of Behavioral and Experimental Finance 41 (2024) 100885

Contents lists available at ScienceDirect

Journal of Behavioral and Experimental Finance

journal homepage: www.elsevier.com/locate/jbef

Ambiguity attitudes and demand for weather index insurance with and without a credit bundle: experimental evidence from Kenya

Francesco Cecchi [a], Robert Lensink [b,*], Edwin Slingerland [a]

[a] *Development Economics Group, Wageningen University, Wageningen, the Netherlands*
[b] *Faculty of Economics and Business, University of Groningen, Groningen, the Netherlands*

A R T I C L E I N F O

Keywords:
Loss aversion
Ambiguity aversion
Index Insurance
Willingness to pay
Kenya

A B S T R A C T

We investigate the impact of ambiguity attitudes on the willingness-to-pay (WTP) for index insurance among female smallholders in Kenya. We gauge incentive-compatible measures of ambiguity aversion and insensitivity in the domain of gains and losses, as well as loss aversion. Next, we setup a framed experiment to measure WTP for insurance with basis risk. For a random subsample we introduce an alternative 'rebate' insurance, comparable to an insurance purchased through a loan – repaid in good years and deducted from payout in bad ones – that is expectedly more palatable for the loss averse. We find that ambiguity aversion significantly increases WTP for the standalone insurance, while loss aversion reduces it as expected. The former result is seemingly at odds with previous evidence from the field, but is consistent with a setting in which insurance ambiguity engenders relatively less disutility compared to the vagaries of weather. We show that this apparent divergence is not caused by differences in the method used to estimate ambiguity aversion compared to existing field studies. Rather, we exploit exogenous variation in the familiarity with insurance within our sample to show that it is explained away by the role of experience with the novel technology—a previously underestimated mediator. Ambiguity aversion hinders adoption at early stages but increases when the insurance is better understood. The rebate scenario, instead, all but cancels the effect of loss aversion on WTP, but the increased contractual ambiguity results in significantly lower bids by the ambiguity averse. In the lab, the WTP for rebate-type insurance-credit bundles is not different from that of the actuarially equivalent standalone insurance, implying that evidence from the field on greater uptake for the former may be attributable to liquidity constraints and time discounting effects, rather than to behavioural traits.

* Corresponding author.
E-mail address: b.w.lensink@rug.nl (R. Lensink).

https://doi.org/10.1016/j.jbef.2023.100885
Received 27 November 2022; Received in revised form 8 December 2023; Accepted 12 December 2023
Available online 15 December 2023

1. Introduction

Uninsured risks are a binding constraint for smallholder farmers in developing countries, causing systematic underinvestment and poverty traps (e.g. Karlan et al., 2014; Emerick et al., 2016). Weather index insurance is considered a promising risk-coping strategy that can help relax this constraint and address part of the incompleteness of insurance markets. Recent studies find a positive effect of insurance on technology adoption (Hill and Viceisza, 2012), employment of riskier and higher yielding inputs (de Nicola and Hill, 2013; Bulte et al., 2020) such as fertilizer, seeds and land. Index insurance payouts are based on probabilistic indices such as satellite images or weather stations measuring rainfall or normalized vegetation growth. This overcomes the high transaction costs and information asymmetry problems that limit the viability of traditional indemnity insurance. In theory, this could greatly improve access to insurance for the poor and enable them to smoothen income, stimulate investments, increase revenues and escape potential poverty traps. However, uptake of weather index insurance has been rather low (Carter et al., 2014; Climent et al., 2018).

Low insurance uptake has been typically attributed to financial illiteracy, trust, poor marketing, credit/liquidity constraints (e.g. Belissa et al., 2019). Another often cited reason is basis risk (Elabed et al., 2012; Jensen et al., 2014), also referred to as insurance nonperformance, which can be defined as the imperfect correlation between insurance payments and the actual losses incurred by the insured farmer. Indices that measure rainfall, for example, are not always accurate at the farm level. Therefore, an insured farmer that experiences a drought, might not be paid out if the index estimates enough rain in his area, and vice versa. While basis risk is likely to affect insurance demand under expected utility theory, its implications are exacerbated if farmers systematically deviate from these predictions in ways that have been sketched under prospect theory (Carter et al., 2015). Basis risk should be particularly dreaded in the presence of reference dependence. Across several cultures and contexts (Brown et al., 2023), people have often been found to exhibit loss aversion – i.e. when “losses loom larger than gains” (Kahneman and Tversky, 1979). For the loss averse, negative and positive basis risk do not cancel each other out: the disutility of negative basis risk is overvalued with regards to the utility of positive basis risk, further depressing the insurance valuation. In fact, Lampe and Würtenberger (2020) develop a theoretical model formalizing this intuition, finding evidence of it through a randomized controlled trial in India.

At the same time, ambiguity aversion and ambiguity insensitivity may affect the willingness to pay (WTP) for insurance, too. While the only evidence from the field so far points at a negative relationship between a proxy of ambiguity aversion and insurance uptake (see Bryan, 2019, and Belissa et al., 2020), theoretically the relationship may be less straightforward. First, insurance basis risk is indeed a source of uncertainty with unknown probability, i.e. ambiguity, but the likelihood of negative weather shocks is also ambiguous. It is thus unclear if the ambiguity averse should theoretically overvalue insurance as a way to reduce the ambiguity of insurable weather risks, or if they should undervalue it for its intrinsic payout ambiguity. Second, ambiguity attitudes are understood to follow a fourfold pattern of ambiguity aversion for low probability losses and high probability gains, and ambiguity seeking for high probability losses and low probability gains. Bad weather ambiguity can be considered a low probability ambiguous loss, while index insurance with basis risk is, conditional on bad weather, a high probability ambiguous gain. In both these frames most people are indeed expected to behave in line with ambiguity aversion. This, however, raises concerns about the reliability of results derived from a measure of ambiguity aversion gauged only in the domain of mid-probability gains—as is the case for the two empirical studies mentioned above. In this paper, we circumvent this concern by measuring ambiguity attitudes at three different likelihoods in the domain of gains as well as losses, through which we are able to gauge the S-shaped nature of ambiguity source functions through two indexes (ambiguity aversion and ambiguity insensitivity) that are not dependent on the specific likelihood of the known probability alternative (see Abdellaoui et al., 2011; Dimmock et al., 2016a). Moreover, while Baillon et al. (2020) make clear theoretical predictions about the fact that insensitivity to probabilities may exacerbate underinsurance, no study to date has tested the relationship between ambiguity insensitivity and insurance demand empirically.

Finally, Carter et al. (2015) suggest that some of the behavioural traits mentioned above could be exploited to increase appeal of index insurance products. One example of such design innovations is the ‘rebate’ insurance developed by Serfilippi et al. (2020). In an experiment with farmers in Burkina Faso, they randomly offered two different designs of insurance contracts: a standalone insurance, and one where premium is only due in good weather years—equivalent to an insurance credit bundle where premium is deducted from payouts. Their study shows that the WTP for rebate insurance is on average higher, and especially high for those exhibiting discontinuous preferences around certainty. In our study we aim to replicate their intervention by offering a similar credit-insurance bundle to a random half of our participants, and study an alternative explanation of greater WTP for this otherwise actuarially identical product: that the occurrence of (perceived) losses is reduced by the rebate framing, thus increasing the WTP among the loss averse. In two field experiments, Casaburi and Willis (2018) and Belissa et al. (2019) both find that similar pay at harvest arrangements greatly increase uptake of index insurance, in Kenya and Ethiopia respectively, and attribute this effect to liquidity constraints, time discounting and time inconsistencies. Our lab-in-the-field setting nets out any time preference related effect allowing us to test alternative behavioural explanations.

We gauge an incentive compatible measure of ambiguity attitudes in the gains and loss domains, following Abdellaoui et al. (2011), and of loss aversion following Fehr and Goette (2007). Next, we set up a Becker-DeGroot-Marshack (BDM)-style framed lab-in-the-field experiment to gauge the WTP for index insurance in the presence of ambiguous insurable risks (bad weather probability) as well as basis risk (ambiguous insurance nonperformance probability). Our 276 participants are women belonging to women only farmer-groups from six village areas in Meru County in Eastern Kenya. Meru is a region where most farming activities are performed by women, and where insurance products have been available to smallholders for quite some time, but where uptake rates remain very low. Given the relatively low sample size, to reduce confounding effects we take two measures. First, we restrict our sample to women, to increase consistency or results and avoid having only a few males per treatment. Second, we only invite farmers that participated to a previous randomized intervention on index insurance (see Bulte et al., 2020). This has a twofold advantage. All of our participants have partaken to at least one workshop explaining the workings of index insurance as part of this previous study, ensuring that the concept of insuring against bad weather events is familiar to all our participants. Moreover, as the assignment treatment was random in the above-mentioned study, we can exploit exogenous variation in the experience and familiarity with insurance itself.[1]

Our results confirm a negative relationship between loss aversion and WTP for insurance. Instead, our measure of ambiguity aversion seems to correlate with a greater WTP for insurance, whether it is measured in the loss or gain domain. This result is seemingly at odds with previous literature on the relationship between ambiguity aversion and insurance uptake (Bryan 2019; Belissa et al., 2020), but in line with the theoretical and experimental findings by Lambregts et al. (2021) in the presence of low probability ambiguous insurable losses (such as weather shocks) and low probability ambiguous insurance nonperformance (as is the case of basis risk). We show that this apparent divergence in findings is not caused by differences in the method used to

[1] Insurance was provided for free to the treatment group, conditional on showing proof of purchase of at least one packet of improved seeds.

Table 1
Summary and balance statistics.

Variables	Standalone Insurance	Rebate Framing	Loss Domain	Gain Domain	Pooled Sample
Age	45.43	43.36	44.28	44.49	44.38
	(13.70)	(12.62)	(13,36)	(13.05)	(13.18)
Years of education	5.68	5.81	5.43	6.06	6.03
	(2.09)	(1.61)	(1.75)	(1.92)	(3.55)
Household Size	5.98	6.09	6.09	5.97	5.74
	(3.63)	(3.49)	(3.54)	(3.57)	(1.86)
Catholic = 1	0.36	0.23	0.30	0.28	0.29
	(0.48)	(0.42)	(0.46)	(0.45)	(0.45)
Livestock	3.44	3.17	3.07	3.54	3.31
	(3.07)	(3.78)	(3.26)	(3.62)	(2.44)
Total land	2.18	2.20	2.12	2.27	2.19
	(1.87)	(1.98)	(1.51)	(2.27)	(1.92)
Insured = 1	0.18	0.21	0.18	0.22	0.20
	(0.39)	(0.41)	(0.39)	(0.41)	(0.40)
Observations	136	140	138	138	276

estimate ambiguity aversion. Rather, we exploit the exogenous variation in the familiarity with insurance within our sample to show that it is explained away by the role of experience with the novel technology—a previously poorly understood mediator. Ambiguity aversion hinders adoption at early stages but increases when the insurance product is better understood.

Finally, we show that the credit-insurance bundle does have an effect on the loss averse—who seem to prefer rebate framing over the standard index insurance contract. In contrast, for the ambiguity averse this framing significantly reduces WTP compared to a standalone index insurance product. We conjecture that the ambiguity averse are discouraged by the increased contractual ambiguity surrounding the premium rebate in bad years. These opposing effects result in a null effect in terms of average WTP. This suggests that evidence from the field on greater uptake for insurance products with deferred premium payment may indeed be mostly attributable to liquidity constraints, time discounting and time inconsistency effects.

The remainder of the paper is organized as follows. Section 2 sketches the theoretical benchmarks, predictions, and evidence to date. Section 3 describes the context and experimental design. Section 4 describes the distribution in our sample of the incentive compatible measures of ambiguity aversion, ambiguity insensitivity, and loss aversion. We then present our empirical strategy in Section 4, our results in Section 5, and a mechanism analysis in Section 6. We come to our conclusions in Section 7.

2. Theoretical benchmarks, predictions, and evidence so far

Attitudes towards ambiguity – i.e. uncertainties with unknown probabilities – have long been considered to play an important role in determining the valuation of index insurance products, as well as in the uptake of any technological innovation (Bryan, 2010). In fact, as long as the distribution of returns of a new technology are not fully understood, those who dislike unknown probabilities will discount the value of such innovations. On top of this index insurance products add a unique dimension of ambiguity with respect to payouts—namely, the possibility that even under de facto bad states the index determines otherwise (i.e. negative basis risk). The disutility of basis risk will have to be weighted by prospective insurees, against the beneficial effect that insurance has in reducing downward income risks due to negative weather events. On the one hand, the likelihood that the index insurance will actually payout under bad weather is unknown, constituting a situation of ambiguity. On the other, the likelihood of adverse weather patterns is also unknown to farmers. Ambiguity aversion can thus have a different effect on insurance uptake depending on the relative weight that decision makers will place on these two ambiguous prospects (Lambregts et al., 2021).

This said, a consensus has been forming in recent years that the relationship between ambiguity aversion and index insurance tends to be negative. In a theoretical model with known bad weather probabilities, Clarke (2016) shows that it is rational for risk averse farmers to undervalue an insurance with known nonperformance likelihood.[2] This model can easily be extended to a context of unknown nonperformance: if only the insurance is ambiguous, and weather patterns known, such model would predict lower insurance uptake among the ambiguity averse. Indeed, in a separate model Peter and Ying (2020) show that an ambiguous nonperformance risk always reduces the demand for insurance compared to known nonperformance risk. This theoretical prediction is corroborated empirically by Bryan (2019) as well as Belissa et al. (2020) who find, in Ethiopia and Malawi respectively, that ambiguity averse farmers are significantly less likely to sign up for index insurance.

At the same time, if we consider a hypothetical situation where only the insurable risk is ambiguous (e.g. bad weather) and the insurance is not, we'd expect the opposite result. Indeed, Bouchouicha and Vieider (2017) show theoretically that insurance uptake under unknown probability risks should be higher than that under known probability risk, given ambiguity aversion for small probability losses. Similarly, Bajtelsmit et al. (2015) show that, when the probability of loss is more ambiguous, the demand for insurance increases. This outcome is further formalized theoretically by Snow (2011) in the context of self-insurance and self-protection. He shows that if the insurable risk is ambiguous, the demand for both increases with ambiguity aversion.

Thus, the two corner scenarios in which either only the insurance or only the insurable risk are ambiguous result in clear but opposing predictions on the effects of ambiguity aversion on insurance uptake. The theoretical predictions are less clear once we imagine a scenario where both the insurable loss and the insurance nonperformance are ambiguous. To this end, Lambregts et al. (2021) design a lab experiment with Dutch university students. They vary the ambiguity of the insurance and/or insurable risk in a factorial design. On the one hand, they find lower demand on average for unknown nonperformance risk compared to known one, as in Peter and Ying (2020). On the other, in a context of ambiguous insurable risks that are sufficiently improbable – with probability $< 50\%$, as is the case for insurable negative weather shocks – they find greater demand among the ambiguity averse (as predicted by Snow, 2011).[3] In fact, real-life predictions are particularly hard to make also because people do not tend to exhibit a universal aversion for

[2] Several other studies have shown that introducing a known nonperformance risk decreases the demand for insurance (Herrero et al., 2006; Wakker et al., 1997; Zimmer et al., 2009, 2018).

[3] When including the full range of probabilities for the insurable risk, including high probability losses, the effect of ambiguity aversion on insurance uptake becomes null and very close to zero. This likely implies that for high probability losses the effect of ambiguity aversion is reverted, and negative, although this is not explicitly mentioned in their results.

ambiguity (Kocher et al., 2018). Instead, they are more ambiguity averse for low probability losses and for high probability gains, while often shifting to ambiguity seeking attitudes once the probability of loss (gain) is sufficiently high (low). Among other things, this inverse s-shape source function of ambiguity attitudes explains the coexistence of insurance and gambling (Tversky and Kahneman, 1992).[4]

Both Bryan (2019), and Belissa et al. (2020) – the two empirical studies on which the current evidence on the negative relationship between insurance uptake and ambiguity aversion is based – construct their measures of ambiguity aversion at mid probabilities in the domain of gains. However, this: 1) may have little predictive power in terms of attitudes towards low probability ambiguous losses (such as those induced by basis risk in index insurance contracts and negative weather events); 2) the fourfold pattern of ambiguity aversion assumes a double reflectivity effect: those exhibiting the seeking-to-aversion pattern in the domain of gains, typically exhibit the aversion-to-seeking pattern in the domain of losses. Since in each domain the 'inversion' from aversion to seeking, and vice versa for losses, may happen around any mid-probability point – close, but not necessarily at $p = 0.5$ – it remains to be proven that we can predict ambiguity aversion using attitudes towards ambiguity gauged only at one level of mid-probability gains. Moreover, given the reflectivity property between the domain of gains and losses, those same subjects labelled as ambiguity averse in the studies above, in the domain of gains, are highly likely to exhibit ambiguity seeking attitudes at mid-probability in the domain of losses. Defining them as ambiguity averse or ambiguity seeking therefore becomes a matter of which reference point is taken.

Abdellaoui et al. (2011) proposes to overcome the complexities generated by this fourfold pattern of ambiguity attitudes, by gauging the matching probability – i.e. the known probability at which someone is indifferent between that known probability and an ambiguous prospect – for low, medium and high probability ambiguous prospects. Then, their 'source method', draws a line of best fit between these three matching probabilities, and identifies the slope of such line as a measure of ambiguity insensitivity: the flatter the slope, the more insensible people are to unknown probabilities, with an extreme scenario of assuming all ambiguous prospects hold a probability of $p = 0.5$ of gain (loss). An overall measure of ambiguity aversion can instead be derived by taking the difference between the dual intercept (where the line of best fit meets $p = 1$), and the intercept ($p = 0$). In the domain of gains, the ambiguity averse will tend to have a dual intercept > intercept, while in the domain of losses ambiguity aversion will result in an intercept > dual intercept. Whether field experiments similar to the ones mentioned above would find the same negative relationship by using the comprehensive measures of ambiguity attitudes of the source method has not yet been investigated.

Another potential confounder in the generalizability of the empirical findings thus far lies in the attitude that the ambiguity averse have towards any innovation. As already mentioned, index insurance holds two separate elements of ambiguity: one intrinsic to the novelty of the technology, which will dissipate through experience, through which the beliefs of ambiguity averse and neutral agents will tend to converge (Marinacci, 2002; Epstein and Schneider, 2007); one directly imputable to basis risk. Indeed, in the modes that precede the empirical findings, both Bryan (2019) and Belissa et al. (2020) admit that ambiguity averse farmers may particularly undervalue insurance when the technology itself is new, and thus its effects on risk reduction will be intrinsically ambiguous. Once the ambiguity of insurance performance due to its novelty is eased, its relative disutility may become smaller than the disutility derived from ambiguous weather events, making insurance more and not less attractive to the ambiguity averse. The sample in Bryan (2019) was confronted with a new insurance product, and thus his result may be driven by the distaste of novelty, and not be imputable to basis risk. Belissa et al. (2020) includes both early adopters – adopting when insurance was relatively new and unknown – and late adopters—deciding to adopt in later seasons when the product is better known. When separating these two groups into a subsample analysis, the relationship between ambiguity aversion and insurance uptake is confirmed negative only for early adopters. For late adopters it is actually positive.[5] This evidence is however not conclusive, as it might be driven by negative autocorrelation: in an extreme scenario were all non-ambiguity averse farmers already purchased insurance in the early seasons, late seasons adoption would necessarily be driven solely by the remaining ambiguity averse farmers.

In this study we aim to further investigate the theoretical predictions and empirical evidence mentioned above in a lab-in-the field experiment in which we estimate ambiguity aversion using – for the first time in a non-experimental population – the source method of Abdellaoui et al. (2011) for both gains and losses. Also for the first time, we gauge both ambiguity aversion and loss aversion within the same study on index insurance. In fact, loss aversion can be expected to play an important role for weather index insurance uptake: in the presence of basis risk, the loss averse overvalue the disutility they derive from scenarios where the harvest is bad but the insurance does not payout. If so, even in the presence of equally likely positive and negative basis risk, the latter will not cancel out in the eyes of those seeking to insure, reducing the value assigned to the insurance. Lampe and Würtenberger (2020) develop a theoretical model formalizing this intuition, and through a randomized controlled trial in India show that index insurance demand decreases with loss aversion. If loss aversion and ambiguity aversion are correlated, as can be expected, then the negative relationship found by Bryan et al. (2019) and Belissa et al. (2020) could be partially explained by omitted variable bias.

Finally, Serfilippi et al. (2020) propose to overcome the drag on insurance demand caused by discontinuous preferences around certainty by bundling insurance with credit. In their 'rebate insurance', premium is paid at harvest only in good years, and deducted from payout in bad years. If the loss averse have separate mental accounting for gains and losses, this reduces the disutility derived from premium payment – at least for bad years – and should therefore increase WTP. At the same time, this bundled arrangement may exacerbate the perceived ambiguity of the insurance product, with negative effects on the ambiguity averse (as premium payment becomes ambiguous, too). In two separate field experiments, both Casaburi and Willis (2018) and Belissa et al. (2019) find that pay at harvest arrangements greatly increase uptake of index insurance, in Kenya and Ethiopia respectively, but attribute this effect to time discounting and time inconsistencies. In our experiment we can net out these time effects to focus on the alternative behavioural explanations around loss and ambiguity aversion.

3. Experiment

3.1. Sample

The data in this study was collected in Meru County, Kenya, in May 2017. Meru county, in the Eastern province of Kenya, is characterized by female led-farming, which is why our sample is composed only of female

[4] While the existence of such pattern is well known in literature, to the best of our knowledge it had never been experimentally elicited besides for student populations and non-experimental populations outside the USA. (e.g. Baillon and Bleichrodt, 2015, Dimmock et al., 2016 König-Kersting and Trautmann, 2016 and Trautmann and van de Kuilen, 2015).

[5] Also, they find that the ambiguity averse are less likely to become dis-adopters, indicating that once familiarized, ambiguity aversion invites retaining index insurance, not abandoning it.

respondents. In total 276 female farmers participated in the experiments.[6] In total the experiment took around 2 to 2 and a half hours depending on the group size for that day.[7] Our sample of farmers has an average age of 44 years and 6 years of education. Household size averages 6 people, and about 29% are catholic—the rest identifying in one of the many protestant or evangelic denominations. None of the participants identified as non-religious or non-Christian. Each family owns approximatively 3 to 4 cows on average. This is typical of the area of Meru, where milk production and livestock herding are important side activities for famers, also serving as risk-coping mechanism. On average, our sample farms just above 2 acres of land, ranging from a minimum of 0.25 acres to a maximum of 16. Around 20% have been insured in the previous farming season.[8]

Participants were randomly assigned into the treatments, stratified at the village level.[9] Farmers from 6 different villages and their surroundings participated in the experiment,[10] resulting in 136 participants to the standalone insurance treatment and 140 participants to the rebate insurance. Orthogonally, we elicited ambiguity attitudes in the domain of gains for 138 participants and losses for 138 participants, again randomly assigned. Table 1 provides an overview of the summary statistics listed above, separated by treatment (i.e. either standard insurance or rebate framing), by gain and loss domain for the ambiguity game, as well as pooled. It also serves as a balance test to show that the randomization yielded largely similar groups across the different groups: none of the differences across groups is statistically significant at the conventional levels.

Every subject played 3 games: (i) the ambiguity game; (ii) the loss aversion game, and (iii) the WTP-game.[11] Every game was incentive compatible.[12] The minimum amount that we paid out is 250 KSh (Approximatively 2.5 USD).[13] This is slightly higher than the minimum wage for casual workers in 2015 in the agricultural industry in Kenya, which was set at 228.30 KSh a day (Africapay, 2015). Fig. 1 presents a design matrix of the random assignment into the different treatments. In the next subsections we will describe in detail the experimental design, methods used and procedural details of the 3 games.

3.2. *Ambiguity game*

Based on the 'source method' of Abdellaoui et al. (2011), the ambiguity game captures ambiguity aversion and ambiguity generated likelihood insensitivity (a-insensitivity).[14] The incentivized decisions are not dissimilar to those in the famous Ellsberg experiment (1961). Respondents choose between an unambiguous Box (Box 1) and an ambiguous Box (Box 2).[15] Each Box holds exactly 100 beads. Respondents are asked to choose one of the Boxes to draw a bead from. If this bead is of the winning colour, they win 100 KSh. The contents of Box 1 are known and shown to the participant. The contents of Box 2 are unknown and hence ambiguous. The respondents only know the amount of beads and how many different bead colours are present in the ambiguous Box. Besides stating a preference, respondents could also state to find both Boxes 'equally attractive' which is the same as 'indifference' in the terminology of Dimmock, . et al., (2015, 2016b)[16] We will first explain the procedures of the game and then how ambiguity indices can be constructed

[6] Invitations for participation to the experimental sessions were sent to female farmer groups in the selected villages. A precondition to participation was having already been exposed to information sessions on index insurance through the study carried out by Bulte et al. (2020). This ensured that all our participants had at least some familiarity with index insurance. We excluded from the experiment the few male farmers that showed up to avoid multicollinearity between sex and other characteristics, due to the small sample within each treatment arm.

[7] Nine experienced enumerators explained the experimental protocols in the local language, and made sure the farmers were able to understand the games. Four of them were exclusively trained in the WTP-game, two for each type of insurance design. To avoid mistakes, each trained enumerator only presented one type of insurance design throughout the experiment. The other five enumerators conducted the survey module and the loss and ambiguity attitude games. Farmers were instructed not to talk to each other to reduce information spill-over effects from one group to another, under the threat of a fine when disclosing information to one another. he fine was set at a fraction of the show up fee and thus little less than symbolic. However, enumerators never levied the fine as communication between participants was not an issue.

[8] The relatively high presence of insured participants has to do with the full overlap of our sample with participants to a separate intervention (Bulte et al., 2020). Since the real-life insurance was free and awarded at random, we do not expect this overlap to hinder our analysis. Rather, it means that our sample of respondents had all been trained on the concept behind weather index insurance prior to our work, which helped in their understanding of the framed insurance experiment. This said, all the above-mentioned individual and household characteristics will be included in the analysis as controls to avoid any potential confounding effects. Our results are in all cases robust to inclusion or omission of random assignment to treatment in Bulte et al. (2020) as well as actual insurance uptake.

[9] By employing a stratified randomisation within every village, we increase the likelihood that the subsamples across the two treatments are comparable. If all subjects in one village would play the same game, village-specific unobserved variables might influence the results. For example, some villages have more experience with insurance, are less poor, are more remote than others. By randomly stratifying within every locality, we can control for those unobserved differences between groups. To this end, village area fixed effects (dummies) will be included in the analysis, capturing any potential group dynamics, idiosyncratic experimental variation across the various sessions, as well as any other spatial effects.

[10] See table A1 in Appendix II for a breakdown of the sample across the different villages.

[11] To avoid order effects, whether the WTP-game was played before or after the loss and ambiguity games was again assigned randomly.

[12] Participants received a voucher with the amount won for every game. Once all three games were completed, the vouchers were collected, put in a large jug and one of the vouchers was randomly selected which was paid out in cash to the subject. This was explained prior to the start of the experiment, to avoid income effects in which participants might bias their game play based on previous earnings.

[13] Because luck plays a substantial role in all three games, we decided to not pay out less than 250 KSh. If a farmer was very unlucky in one of the games and also in picking the voucher, they were given the minimum of 250 KSh. This was only explained during the pay-out phase once all games had been completed, thus retaining the incentive compatibility thoughout the games' decision making. This solves ethical concerns that arise from paying one subject more than 4 times more than another subject.

[14] The ambiguity game uses 3 scenarios and subsequent variations on every scenario to estimate the ambiguity attitudes. We offered respondents real monetary rewards based on one of their choices in one of the scenarios. Once again participants were made aware of a show-up fee of 250 KSh, and were offered to play a game where they could win an additional 100 KSh or nothing. Every participant could therefore finish the game with either 250 or 350 KSh.

[15] We used the small coloured beads that are famously used for Kenyan jewellery. Because we could not bring computers with internet access into the field, we used non-transparent lunch boxes with coloured beads instead.

[16] We named this option equally attractive, to reduce any negative connotations of disinterest from the respondents, indifference might imply when translating to Kimeru.

from the results of the game.[17]

In the first scenario, Box 1 contains 50 green and 50 yellow beads. Box 2 contains 100 beads of either green or yellow colour with an unknown composition. The participant wins if a green bead is drawn.[18] There could be between 0 and 100 green beads in Box 2. Following Dimmock et al. (2015), Baillon and Bleichrodt (2015), Baillon et al. (2015), we first identify matching probabilities to help us estimate ambiguity attitudes. A matching probability (*m*) is the objective probability for which an agent is indifferent between the risky option and the ambiguous option. For our game this means that *m* is the indifference between winning under the ambiguous option (Box 2) and winning with probability *m* for the risky option (Box 1). We elicited *m* using a sequence of questions, while changing the colour ratio of beads in Box 1, for which the respondent had to state their choice: Box 1, Box 2, or equally attractive (see Figure A3 in the appendix for a graphical representation).[19]

Changing the ratio of beads was done by method of biSection, as explained in the annex of Dimmock et al. (2016a). After every choice, the difference between the lower bound and the upper round on the matching probability is reduced by half. This would continue until the answer 'equally attractive' was given or until a maximum of three additional rounds. After the final round, the matching probability is the objective probability of Box 1 if the respondent answered equally attractive, otherwise the midpoint of the average of the lower and upper bound of the final round is taken.[20]

Subjects that find both Boxes equally attractive in the first round, where the objective probability of winning in Box 1 is 50%, treat the ambiguous Box (2) as having the same percentage of winning as the known Box (1), i.e. 50% chance of drawing a green bead. Hence the matching probability m is 0.5. If instead the respondent preferred Box 1 over Box 2, then the respondent is averse to ambiguity, with $m < 0.5$. Respondents that choose Box 2 over Box 1 in the first-round display ambiguity seeking behaviour, with $m > 0.5$.

The literature on ambiguity predicts that ambiguity aversion is dependent on the likelihood of the event. Dimmock et al. (2016a) give proof in a large representative household sample that people respond differently to situations if the likelihood of winning is 50–50, very high (90%) or very low (10%). On average people are ambiguity seeking for low likelihoods and ambiguity averse for high likelihoods of winning. We therefore include a scenario with a low likelihood and one with a high likelihood of winning. Other methods to estimate ambiguity attitudes like Baillon and Bleichrodt (2015), employ a similar strategy. Our second scenario has a very low likelihood of winning in the starting scenario (i.e. 10%), whereas the third scenario has a very high likelihood of winning (i.e. 90%). Both the second and the third scenario are played with 100 beads of 10 different colours.

For the second scenario, in Box 1, there are 10 beads of every colour and 100 beads in total. The respondent wins if a green bead is drawn from the Box, thus giving winning odds of 10%. If any colour other than green was drawn, the respondent would not win. After every choice of the respondent, the composition of the beads in Box 1 would be altered using the same method of biSection. For the third scenario, the starting situation is the same as the second scenario, but the winning condition is different. Now, the respondent wins if the bead drawn is *not* green, resulting in a 90% winning probability. Similarly, after every choice, the composition of the beads in Box 1 is rearranged using the method of biSection until the matching probability is found. The second and third scenario provides us with information on whether ambiguity aversion is dependent on likelihood. Together with the matching probability of scenario 1, we can construct indices of ambiguity aversion for moderate, very low and very high likelihoods of winning.

Once we have established the matching probabilities at 10%, 50% and 90%, to properly estimate individual indexes of ambiguity aversion and a-insensitivity we follow the method proposed by Abdellaoui et al. (2011). They use linear regression and estimate a line of best fit over the measured matching probabilities. For the three scenarios explained above, with on the x axis the initial winning probabilities (p) contained in Box 1 (p= 0.1, 0.5, and 0.9) and on the y axis the estimated matching probabilities (m) of the ambiguous Box 2. Assume that the regression line of the source function on the open interval (0,1) is $m \mapsto c + sp$, where c is the intercept and s is the slope. Intuitively, the matching probabilities of scenarios 2 and 3 determine the slope (s), while all three points

[17] In previous studies, ambiguity elicitation experiments have mainly been conducted with university students or in a general survey on the American population. Our respondents are relatively poor female farmers with little education or even illiterate. We therefore tried to keep the experiment as simple and visual as possible. Enumerator followed a fixed script on the tablet reiterating every round the winning/losing condition, the amount of beads of every colour in Box 1, the amount of beads in Box 2 and changes in the ratio of beads in Box 1. The actual content of Box 1 throughout the various rounds was visualised on a white plastic plate. There the enumerators recreated the constellation of the coloured beads in play at the moment so that the respondent could clearly see the colours in play. If it was still not clear to the farmer, the enumerator had a picture of every possible situation on their tablet, that they would show to the farmer. It was not uncommon that an enumerator explained the game multiple times before the respondent understood the game. The lunchBoxes with beads were checked every night and given to another enumerator the next day to minimise enumerators' knowledge of the contents of the ambiguous Boxes. Every 4 days, we removed all the beads from the Boxes and randomly filled the ambiguous Boxes with beads. We used two big jugs to fill with green/yellow beads and beads of ten different colours. After shaking the jugs, we poured the beads into the ambiguous Boxes until the exact amount of 100 beads. In total there were 3 Boxes per enumerator. One Box 1, and two times a Box 2. One ambiguous Box for scenario 1 and one ambiguous Box for scenario 2 and three.

[18] In theory it is possible that a non-neutral response to the first round of scenario 1, can be reconciled with subjective expected utility theory if the subject assigned a very low subjective probability to drawing a winning bead from Box 2. Abdellaoui et al. (2011) and Dimmock et al. (2016) therefore give subjects the opportunity to alter the winning colour in Box 2. They find that less than 2% of the respondents changed the winning colour in Box 2. Dimmock et al. (2015) also test this by allowing respondents to choose the winning colour of the whole game. Fewer than 1% opted for this. All three studies show that people are indifferent about the winning colour and there were no significant differences in the mean matching probabilities of the group that was allowed to switch colour and the group that could not switch. We therefore did not allow respondents to change the winning colour.

[19] If the participants' response was 'equally attractive', the survey continued with the second scenario. If the respondent indicated that Box 1 was preferred, then the enumerator replaced some of the green beads with yellow beads, reducing the known winning probability of Box 1. If the respondent indicated that Box 2 was preferred, then some of the yellow beads of Box 1 were replaced by green beads, increasing the observable winning probability of Box 1. Whenever the subject selects Box 1, this Box is made less attractive. Whenever the subject selects Box 2, Box 1 is made more attractive. The content of Box 2 was never changed, never visible, and remained ambiguous throughout.

[20] After the three scenarios were answered, the survey included 2 check questions to test whether the participants behave consistently. The matching probability of the first scenario was taken as a starting point. For m = 0.5, the respondent was indifferent between Box 1 and Box 2, with an objective winning percentage 50% for Box 1. The check questions take the matching probability of the first scenario and do this − 10 and + 10 winning beads. In our example, the first check question recreates scenario 1 with 40 winning beads for the first question and 60 winning beads for the second question. For the first question, to be logically consistent, the respondent should answer that Box 2 is preferred, for the second question Box 1 should be preferred. These questions will be important for analysing whether the respondents understood the game and behave consistently. After answering the sequence of questions for all three scenarios, the tablet would randomly select one of the three scenarios to be played out for real. This meant that after having selected one of the three scenarios, one of the situations answered by the respondent was randomly selected. The respondent would win if the bead had the winning colour.

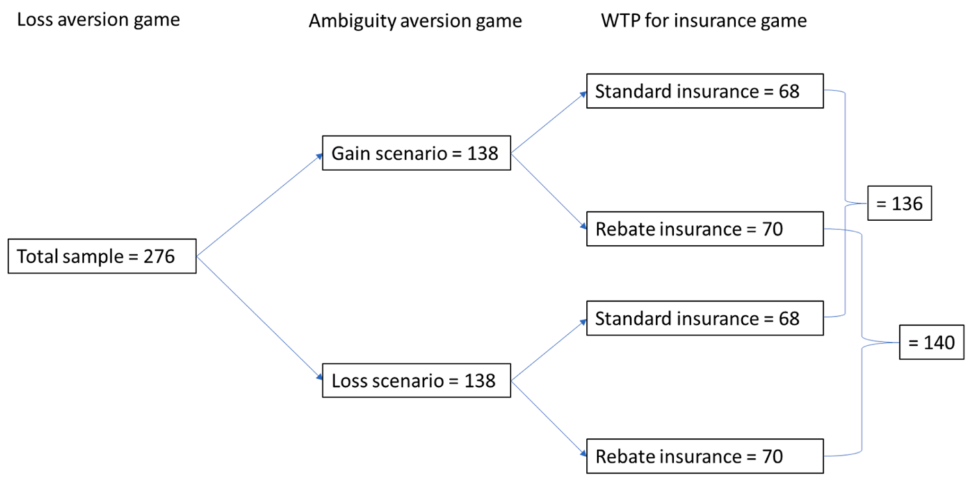

Fig. 1. Design matrix.

are needed to determine the intercept (*c*), at $p = 0$. Let *d* be the distance from 1 of the fitted line at the dual intercept, i.e. at $p = 1$, such that $d = 1 - c - s$.

Abdellaoui et al. (2011) define their index of a-insensitivity as $a = c + d$ ($= 1 - s$). That is, the flatter slope of the best fit line, the less a person is able to discriminate between different probabilities. For example, those who are perfectly a-insensitive will have $s = 0$, thus $a = 1$; while those who do not reveal any insensitivity bias towards probabilities have $s = 1$, thus $a = 0$. Ambiguity aversion is instead defined as $b = d - c$ ($= 1 - s - 2c$). Therefore, as long as the distance of the intercept from zero and that of the dual intercept from one are identical, a subject is not ambiguity averse. Instead, $d > c$ indicates ambiguity aversion. This is a generalization behind the earlier intuition that at $p = 0.5$ a matching probability $m < 0.5$ indicates ambiguity aversion.

For the random subsample that plays the ambiguity game in the loss domain, the game works exactly in the same way, except that they faced the prospect of losing 100 Ksh with given probabilities, instead. In this scenario, contrary to the gain domain, a matching probability greater (smaller) than the rational expected probability indicates ambiguity seeking (aversion). Since in part of the analysis below we will pool the data from the gain and loss domains, we first standardize the loss and gain domain results separately, and then invert the sign of the loss domain before creating a single standardized variable. Doing so, we now have a measure of relative ambiguity aversion, that allows us to interpret coefficients as the effect of a 1 standard deviation increase of ambiguity aversion.

3.3. Loss aversion game

Following Fehr and Goette (2007), we use a simple lottery choice task to measure loss aversion. Subjects are faced with 6 dichotomous choices: they compare a 100% chance of winning 0, with a 50% chance of winning 150 KSh throughout the six choices and a 50% chance of losing 50 KSh up to 175 KSh across the six choices. For all 6 choices, respondents have to indicate whether they 'accept' or 'reject' the lottery (see Figure A1 in Appendix I for an overview of the choice sets). In every subsequent choice the amount lost is augmented with 25 KSh. In case they reject to play participants can keep their 250 KSh show up fee (and win 0).

According to Rabin (2000) , this method measures loss aversion in risky choices: those who reject the lottery even though its expected value is above zero, are overvaluing losses with respects to gains (see more on this below).[21] The method above measures loss aversion rather than risk aversion (Gächter et al., 2010). Due to the small-stakes, in fact, risk aversion parameters derived from these choices would imply absurdly high degrees of risk aversion in high-stake gambles.

Using cumulative prospect theory, we can simply measure loss aversion by taking the following indifference equation:

$$\omega^{+}(0.5)v(G) = \omega^{-}(0.5)\lambda_1 v(L) \tag{1}$$

In this equation G signifies the gain or the fixed amount won in every choice. L represents the loss of the choice. v(x) is the utility of the outcome x which can be either G or L. λ is the coefficient of loss aversion. ω^{+} and ω^{-} are probability weights for gains and losses. We will apply varying assumptions with regard to the probability weighting function. We will first, for the sake of simplicity, estimate λ assuming that subjects have the same probability weighting function for gains and losses, i.e. $\omega^{+}(0.5)/\ \omega^{-}(0.5) = 1$.[22] Our measure of loss aversion is thus reduced to $\lambda_1 = G/L$. In this simple equation G is the fixed gain of 150 and L is the latest choice lottery still accepted by the subject. If a subject accepted all choices, even the last one with a loss prospect of 175 KSh, the estimate of loss aversion would be $\lambda = 150/175 \leq 0.86$. If the subject is not loss averse and thus accepting question 1 till 5, $\lambda = 150/150 = 1$. If all lotteries are rejected $\lambda = 150/50 \geq 3$. To test for the degree of loss aversion in our sample we also make use of more plausible estimates regarding the probability weighting and diminishing sensitivity. Following Gächter et al. (2010), we define our second measure of

[21] The subjects are shown a coin (40 KSh) to signify a coin toss, and if needed also an image on the tablet to make the rules of the game very clear (see Figure A3 in Appendix I, for an example). They are told that at the end of the 6 questions, one of the questions would be randomly selected to be played for real. Their answer, accept or reject, determines whether the coin is flipped or not. If the answer to that specific question was accept, the coin is flipped—if it turns up heads, the subject would win 150 KSh, tails, the subject loses the amount specified by the selected question. Any wins or losses are added or deducted to the initial show up fee.

[22] A second assumption is that diminishing sensitivity, a key tenet of prospect theory, does not play a role. Gächter et al. (2008) argue that for small stakes diminishing sensitivity can be neglected which they base on a study by Fehr-Duda et al. (2006) who predominantly find linear value functions for small stakes. For our study this means that for the range of losses considered, sensitivity should not be greatly different.

Table 2
Expected income with and without insurance.

Insurance price = i	Good Yield	Bad Yield	Negative basis risk
A) *Without insurance*			
Probability	0.8	0.2	-
Savings	5000	5000	-
Net income	20000	0	-
Total earnings	25000	5000	-
Expected earnings without insurance = 21000			
B) *Standard insurance*			
Probability	0.8	0.16	0.04
Savings	5000 – i	5000 – i	5000 – i
Net income	20000	0	0
Insurance pay-out	0	5000	0
Total earnings	25000 – i	10000 – i	5000 – i
Expected earnings with standard insurance = 21800 – i			
C) *Rebate insurance*			
Probability	0.8	0.2	0.04
Savings	5000 – i	5000	5000 – i
Net income	20000	0	0
Insurance pay-out	0	5000 – i	0
Total earnings	25000 – i	10000 – i	5000 – i

Expected earnings with rebate insurance = 21800 – i

loss aversion as follows:

$$\lambda = \omega * \left(\frac{G^{\alpha}}{L^{\beta}}\right) \quad (2)$$

$$\omega \equiv \omega^{+}(0.5)/\omega^{-}(0.5) \quad (3)$$

Where α and β represent diminishing sensitivity for gains and losses respectively and ω represents probability weighting for gains and losses. Gächter et al. (2010) take $\omega = 0.86$, based on a study by Abdellaoui (2000)—the strictest estimate found in literature.[23] Booij and Van de Kuilen (2009), report that most studies find diminishing sensitivity parameters to lie between 0.8 and 1, and themselves find $\alpha = 0.859$ and $\beta = 0.826$,[24] giving us the following equation.[25].

$$\lambda_2 = 0.86 * \left(\frac{G^{0.859}}{L^{0.826}}\right) \quad (4)$$

3.4. WTP for insurance game

In our third game, we seek to elicit the willingness-to-pay for two different index insurance designs. The game is inspired by the WTP-game of Serfilippi et al. (2020), which we extend by introducing basis risk. The game is a framed field experiment, meaning that the context of the experiment is framed in a way that would be familiar to the subjects. 90% of our sample uses at least parts of their land for maize cultivation. We therefore designed this game to simulate a realistic scenario for a maize farmer in rural Kenya. Half of our sample faced a standalone, or index insurance design, the other half faced the insurance credit bundle, from here on consistently referred to as rebate framing, where the farmer only had to pay the premium when the year was good. The two insurance policies were identical and actuarially fair, except that under the rebate-type the premium would get deducted from the insurance pay-out for bad harvests. Both insurance contracts were presented as rainfall index insurances. In Meru county, this is measured by a weather station located in every state-owned primary school. One of the main problems with this technology is basis risk: the possibility that the weather station measures a state of the world different from that observed at the farm level. If the index is not triggered, insurance will not pay out even though the farmer experienced a bad yield. For the sake of simplicity, we eliminated the possibility of upside basis risk—i.e. the possibility of receiving a pay-out while also realizing a good yield.

WTP elicitation was done using an adapted Becker-DeGroot-Marschak mechanism. This method was developed by Becker et al. (1963) and is often used in experimental settings. Subjects indicate the maximum price they are willing to pay for the insurance. The true price is then randomly determined. If the true price was lower or equal to the WTP of the subject, they will purchase the insurance for the true price. If the true price is higher than the WTP of the subject, the subject will not purchase insurance. To facilitate the estimation of their WTP, we framed the BDM as a series of take it or leave it (TIOLI) questions, starting from 1600 KSh. Upon rejection, the price is decreased by 1/4th of the upper boundary until the subject accepts the price given. From this point a method of biSection was used, similar to the one used for matching probabilities, until a precision of 50 KSh in determining the highest acceptable WTP. If a subject accepted the upper boundary of 1600 KSh, a follow-up question ensued where they were asked to state their maximum WTP. Similarly, if the subject was not willing to pay the lower boundary, a follow-up question elicited their maximum WTP. This process resulted in a WTP ranging from a minimum of 100 to a maximum of 4000 KSh. Whether or not the subject purchased insurance had a direct impact on how much money could be won in the game, as we will see in the next Section.

Subjects only played with one of two insurance designs.[26] In the game, the yield depended on weather. Good yield brought a net farming income of 20,000 KSh, while bad yield brought a net farming income of zero. These values were constructed in a way that they were easy to calculate and simple to understand. Savings, yields, net income, and the probability of weather shocks and insurance pay-out were all based on qualitative discussions with local farmers and produce aggregators.[27] Yields and production costs were mentioned in the introduction to provide a realistic farming scenario for the farmers. The ambiguous probability of bad harvest was set experimentally at 20%. The net income after the harvest realisation plus the remainder of the savings equals the subject's total earnings at the end of the game. This final outcome would determine the amount won in the game and is therefore dependent on both the insurance decision and on luck. To calculate

[23] Tversky and Kahneman (1992) estimated $\omega = 0.933$ and Booij and Van de Kuilen (2009) find $\omega = 0.966$. For diminishing sensitivity Gächter et al. take $\alpha = 0.95$ and $\beta = 0.92$, following Booij and van de Kuilen (2007).

[24] Tversky and Kahneman (1992) report instead $\alpha = \beta = 0.88$.

[25] In the analysis below we further standardize λ such that the coefficients can be interpreted as due to one standard deviation change in loss aversion.

[26] In one session between 6 and 14 farmers participated. Two well-trained enumerators carefully explained the procedures and rules of the games complemented by visual aids. All groups received the same amount of extensive information about the rules of the game, regardless of their previous experiences. Farmers were told their starting situation, which was the same for everybody: "You are a farmer with 1 hectare of land which you use solely for maize production and with 5,000 KSh in savings, which can be used to purchase insurance or not."

[27] After a qualitative exploration of historical yields and other variables, we decided to set parameters such that good yield occurred with $p = 0.8$ and was set to 1750 kg of maize, and a bad yield with probability $q = 0.2$ harvesting 500 kg of maize. The revenue in both states of the world, depends on the price of maize. Apparently, due to the effects of local supply and demand, the local price of maize will be higher in a bad year than in a good year. This is because weather related shocks are covariate at the county level. When there is too little rain, everyone's yield will on average be worse, driving up the local price for maize. In the experiment the price of maize in a good year was set to 20 KSh/kg and in a bad year to 30 KSh/kg. At the time of the experiment, the drought that affected Kenya, drove up the price of maize to close to 40 KSh/kg. Gross revenue in the game was therefore either 35,000 or 15,000. All farmers are assumed to face fixed production costs, set to 15000 KSh, leading to our estimation of net income of either 20,000 or zero. We communicated the net income in both states of the world as well as how they were derived.

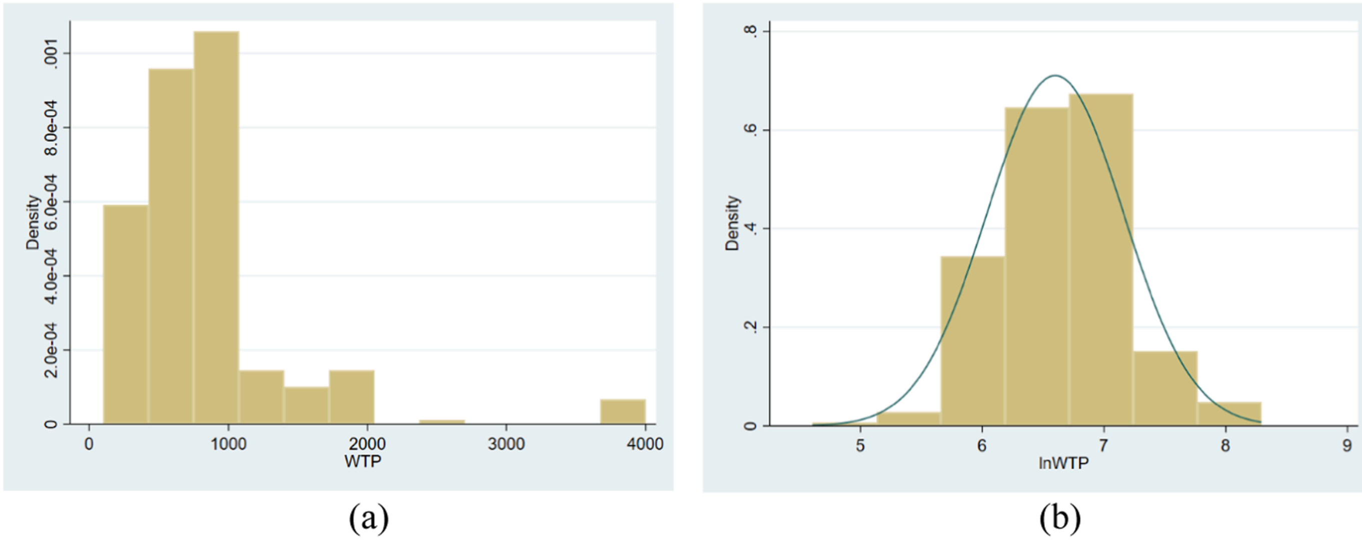

Fig. 2. Distribution of WTP (a), and natural logarithm of WTP (b).

Table 3
Ambiguity aversion in the gains and loss domains.

	Gains domain			Loss domain		
Probability (p)	0.1	0.5	0.9	0.1	0.5	0.9
Matching probability (m)	0.339	0.395	0.621	0.388	0.411	0.773
Ambiguity aversion(gains=p-*m*, losses=m-p)	-0.239	0.105	0.279	0.288	-0.089	-0.127
Ambiguity averse (%)	18.8	65.2	71.0	67.4	22.5	37.7
Ambiguity neutral (%)	11.6	6.5	5.8	7.2	9.4	14.5
Ambiguity seeking (%)	69.6	28.3	23.2	25.4	68.1	47.8
Reference dependencet-test gain(p) = loss(p), (p-value)	-15.38 (0.00)	6.02 (0.00)	12.96(0.00)	-	-	-
Reflection effect t-test gain(p) = -1 $\times$loss(p), (p-value)	1.42(0.16)	0.50 (0.62)	4.87(0.00)	-	-	-
Ambiguity insensitivity index (a)	0.52			0.65		
Ambiguity aversion index (b)	0.097			0.048		

Notes: double underscore: ambiguity attitude exhibited by majority of respondents; single underscore: ambiguity attitude exhibited by a plurality of respondents.

game payout it was sufficient to remove two zeros from the final earnings of the game and add the show-up fee.

Before knowing the outcome of the harvest, farmers had the opportunity to purchase insurance from their savings, which would payout if the yield was bad. In the traditional index insurance contract, the premium was paid independent of the yield being good or bad. In the rebate-contract the premium was only paid if the yield was good. The insurance pay-out was set to 5000 KSh, which would only be paid out if the yield was bad. For the rebate-contract the premium would be deducted from the insurance payout, when the payout took place. Importantly, under both treatments there was a chance that the insurance would not pay out even if the yield had been bad. This introduced an element of ambiguity related to negative basis risk. The likelihood of such ambiguous nonperformance was once again experimentally set at 20%.

Table 2 presents an overview of the options offered to participants, with *i* representing the insurance price. Depending on their treatment, participants could choose between Option A (no insurance) and Option B (buy the standard insurance at price *i*), or between Option A and option C (buy the rebate insurance at price *i*). As can be seen by comparing the expected earnings under the different scenarios in Table 2, the two insurance contracts were actuarially identical, with an actuarially fair price of 800 Ksh. Thus, the only difference between treatments is in the framing of the contract.

During the introduction round, all possible scenarios and mechanisms were explained and most importantly the monetary incentives were clarified. The respondents were shown a plastic jug, which contained green and red ping-pong balls. A green ball represents a good yield, a red ball represents a bad yield. After carefully explaining the outcomes in both states of the world with no insurance, the subjects were introduced to the insurance design they were assigned to.[28] They explained that if you purchased insurance and experienced a bad yield, the insurance would only pay out if a green ball was extracted from the index-jug. If a red ball was extracted, then the insurance would *not* pay out even though you purchased insurance.

Before drawing any balls from the jug(s), the respondents had to indicate their maximum WTP for the insurance. If the price they stated was higher or equal to the 'true price' of the insurance, contained in a

[28] The concept of basis risk was explained by showing the farmers a second plastic jug, which also contained red and green balls. The enumerators did not show the content or ratio of the balls, creating ambiguity.

Table 4
Summary statistics for loss aversion.

Variable	Mean (S.D.)	Median (IQR)	t-test (p-value)
λ_1 (Fehr and Goette, 2007)	2.18(0.84)	2.00(1.50; 3.00)	6.61(0.00)
λ_2 (Gächter et al., 2010)	1.91(0.63)	1.80(1.41; 2.51)	7.23(0.00)

sealed envelope and revealed immediately afterwards, then they would have purchased insurance. At the rate of basis risk set in the experiment (unknown to participants), the actuarially fair price of the insurance was 800 Ksh. We used this price as the price in the envelope: whenever someone indicated their WTP to be equal or higher than 800 KSh, they had to pay the price of 800 in the envelope.[29]

In our experiment we find that the median WTP for insurance in our experiment is 700 KSh, and the average 872. The mean is however driven up by the presence of a few participants bidding significantly above a thousand shillings (see Fig. 2a). That is not uncommon to measurements that are unbound on the upper price side. The distribution of WTP becomes more well behaved when we take its natural logarithm (see Fig. 2b).

4. Ambiguity attitudes and loss aversion in our sample

As mentioned in the previous section, in this study we play two incentive compatible behavioral games to elicit three variables: ambiguity aversion, ambiguity insensitivity, and loss aversion. In this section we describe the distribution of such measures within our sample.

First, we explore the patterns of ambiguity aversion. Table 3 clearly shows a fourfold pattern of preference reversal. In the gains domain the average matching probability (m) is above the expected probability (p) for unlikely gains (ambiguity seeking) and below for likely and very likely gains (ambiguity aversion). In words, people prefer the ambiguous prospect with 10% expected gain up to the point when the known probability alternative reaches a 33.9% chance of winning. The opposite happens in the loss domain: on average people prefer the lottery with an expected ambiguous loss of 10% only when the matching probability of loss in the known alternative reaches 38.8% (ambiguity seeking), and are instead ambiguity averse when the ambiguous loss probability is 50 or 90%. Table 3 also presents the share of participants that exhibit ambiguity averse, neutral and seeking behaviour in the six scenarios. Again, the majority of respondents are ambiguity seeking for likely losses and unlikely gains, and ambiguity averse for likely gains and unlikely losses. Interestingly, these patterns correspond with quite a degree of similarity to the results found by Dimmock et al. (2016b) with regards to a representative sample of the US population—strong evidence of the external validity of these patterns.

The last two rows of Table 3 present a formal test of reference dependence and preference reversal (reflection effect). For reference dependence, in the same way as Dimmock et al. (2015); (2016a), we test the null that each ambiguity aversion parameter estimated for a level of actual expected probability is the same in the domains of gains and losses. The tests reject this null very robustly. For the reflection effect we test if ambiguity aversion in gains corresponds to a mirrored ambiguity seeking in losses, and vice versa. For odds of 10% and 50% we cannot reject the null that attitudes towards gains reflect identical attitudes with opposite sign for losses. Instead, for 90% odds we reject the null: the ambiguity aversion for gains is significantly larger than the respective ambiguity seeking exhibited in the loss domain. This latter effect can also be explained by a greater sensitivity to likelihoods in the domain of losses compared to gains. In fact, our estimated measure of a-insensitivity (based on Abdellaoui et al., 2011) is 0.52 for gains, and 0.65 for losses. Both are significantly different from zero (no insensitivity), but a t-test reveals that a-insensitivity for losses is significantly smaller than that for gains (t = 2.64, p = 0.004)—again, coherent with the predictions of prospect theory. Note that in a large representative sample of the US population, Dimmock et al. (2015) find an estimate of a = 0.32; we find a-insensitivity to be around twice as high. This is in line with the fact that a-insensitivity is a behavioural bias that can supposedly be mitigated with (financial) education. Finally, it is worth noticing that despite the clear fourfold pattern of ambiguity aversion results in ambiguity seeking for medium and high probability losses, the estimated underlying parameter of ambiguity aversion for losses based on Abdellaoui et al. (2011) is not significantly different from that for gains at the usual thresholds: on average participants are ambiguity averse both for gains and for losses.

Next, we present some summary statistics on loss aversion. Table 4 presents the mean and median values of the estimated loss aversion parameters for our sample of female Kenyan farmers. The median of λ_1

Table 5
Willingness to pay for index insurance.

	(1)	(2)	(3)	(4)	(5)
	WTP	lnWTP	lnWTP	lnWTP	lnWTP
Loss aversion (z_λ)	-93.28**	-0.104**	-0.104**	-0.104**	-0.0650
	(46.04)	(0.0459)	(0.0474)	(0.0464)	(0.0528)
Ambiguity aversion (z_b)	76.29**	0.116***	0.122***	0.117***	
	(31.91)	(0.0407)	(0.0390)	(0.0394)	
A-insensitivity (z_a)			-0.0322	-0.0315	
			(0.0406)	(0.0397)	
Ambiguity aversion at an expected negative outcome of:					
▪10%					0.131**
					(0.0642)
▪10% × Gain domain					-0.205**
					(0.0935)
▪50%					0.0874
					(0.0665)
▪50% × Gain domain					-0.00289
					(0.0932)
▪90%					0.0835
					(0.0569)
▪90% × Gain domain					0.0160
					(0.0901)
Constant	834.0***	6.583***	6.364***	6.364***	6.520***
	(41.50)	(0.0447)	(0.297)	(0.297)	(0.298)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	136	136	136	136	136
R^2	0.052	0.076	0.194	0.194	0.246

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns 1-3 we use the standardized version of loss aversion, ambiguity aversion and a-insensitivity. In column 5 we orthogonalize the variables used in columns 1-4. In column 5 we split the estimates of ambiguity aversion at different levels of expected negative outcome and interact them with a dummy representing the gain domain (thus level coefficients refer to the loss domain).

[29] Once all subjects indicated that they understood the rules of the game, they were asked to step out of the group one by one and to state their maximum WTP in a private setting. This reduced the possibility of anchoring to the answers given by the people before them. Enumerators would follow the adapted BDM-method as indicated on their tablet and the answers were recorded digitally and on paper. Once all of the farmers stated their price, the enumerators revealed the true price. Everyone who indicated their maximum WTP to be 800 or higher, had to purchase the insurance at 800. Those facing the traditional contract had to pay this amount with their savings, represented by wooden coins. Those facing the rebate-contract only had to pay if the ball extracted was green. Once it was clear who had bought insurance and who had not, the payout round started. In this round, everyone got to extract a ball from the yield-jug and, if the first ball extracted was red and the participant was insured, also a ball from the index-jug. The winnings of the game were determined by the balls drawn and was written on a voucher consistent with the previous games.

Table 6
Willingness to pay for standard vs. rebate index insurance.

	(1)	(2)	(3)	(4)	(5)
	WTP	WTP	lnWTP	lnWTP	lnWTP
Rebate	56.97	61.40	0.0140	0.00585	-0.0339
	(75.01)	(74.55)	(0.0670)	(0.0691)	(0.0748)
Loss aversion (z_λ)		-93.28**	-0.104**	-0.125***	-0.129**
		(46.03)	(0.0459)	(0.0466)	(0.0497)
Loss aversion (z_λ) × Rebate		116.0	0.0923	0.126*	0.138*
		(72.46)	(0.0662)	(0.0685)	(0.0748)
Ambiguity aversion (z_b)		76.29**	0.116***	0.112***	0.124***
		(31.91)	(0.0407)	(0.0401)	(0.0445)
Ambiguity aversion (z_b) × Rebate		-131.9*	-0.182***	-0.172**	-0.170**
		(79.08)	(0.0655)	(0.0665)	(0.0723)
A-insensitivity (z_a)				-0.00846	-0.0121
				(0.0356)	(0.0421)
Constant	843.4***	834.0***	6.583***	6.385***	6.493***
	(42.55)	(41.50)	(0.0447)	(0.227)	(0.244)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	276	276	276	276	240
R^2	0.002	0.023	0.041	0.090	0.101

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in our sample is 2 and that of λ_2 is 1.80, with an interquartile range of 1.41 to 2.51. This very closely mirrors the estimates of Booij and van de Kuilen (2007), who find a loss aversion of 1.79 for low monetary stakes and 1.74 for high monetary stakes. To verify if this indicates loss aversion, following the standard approach in literature, we test if the median of λ is greater than 1 (i.e. point of indifference between gains and losses). We perform a t-test on a median regression model on our two estimates of loss aversion to verify if $\widetilde{\lambda}_{1,2} - 1 > 0$. Even for the most conservative estimate of the two (λ_2) the t-test is highly significant with t = 7.23: on average our sample shows a significant degree of loss aversion (Table 4, column 3). In what follows, we will use a standardized measure of λ_1 as our preferred loss aversion parameter.

5. Empirical strategy

The effects of ambiguity attitudes and loss aversion on WTP for insurance will be assessed in a regression framework. We will regress WTP on our standardized estimated loss and ambiguity aversion without further controls (naïve), only for the sample that was offered the standard insurance. We then progressively adjust some of the assumptions in this first regression to correct for potential biases. We first take the natural logarithm of the WTP, to address the left skewedness of our WTP data. We further include a-insensitivity, as well as an order dummy O controlling for whether the WTP game was played before or after the ambiguity and loss aversion games, a vector of controls (X') presented in Table 1, as well as village fixed effects (V'), resulting in the following fully specified model:

$$\ln WTP = \beta_1 + \beta_2 \lambda + \beta_3 A\_Aversion + \beta_4 A\_insensitivity + \beta_5 O + \beta_6 X' + \beta_7 V' + \varepsilon \quad (5)$$

Next, to assuage the concern that our results are driven by a correlation between loss and ambiguity attitudes, we create a new set of orthogonal variables (thus uncorrelated), using a modified Gram-Schmidt procedure (Golub and Van Loan, 2013).

Finally, we try to disentangle which of the six individual ambiguity attitude parameters we estimate (at different probability levels, for gains and for losses) helps explain the relationship we find between ambiguity aversion and WTP. To do so, first create standardized measures of the difference between the matching probability and the rational expected probability, and then combine the gain and loss domains such that we can assess the effects of ambiguity aversion at 10% probability of negative outcomes (i.e. 10% chance to lose, 90% chance to win), 50% probability of negative outcome, and 90% probability of negative outcome (90% lose or 10% gain). Our prior is that ambiguity aversion for unlikely negative outcomes (the first of these three variables) will matter more, as drought damage is a low probability ambiguous loss event, and insurance coverage a very likely ambiguous gain event. We always include a dummy for the gain domain to separate the effects of these loss and gain domain measures of ambiguity aversion at different levels of negative outcome probability.

The outcome variable in (5) will also be presented as standardized in Appendix II. This allows an interpretation of effects as standard deviation changes in the WTP for insurance and enables an easier comprehension of the effect sizes. Appendix II will also present a series of robustness tests to probe the robustness of our results to alternative specifications or exclusion of different sub-samples.

We then proceed to assess the effects of the rebate framing on WTP for the pooled sample, without further controls. We continue by including our standardized measures of loss and ambiguity attitudes, interacted with a rebate dummy. Similar to the previous set of regressions, we then take the natural logarithm of WTP, add controls and fixed effects, and replace our measures with orthogonalized variables. As a final robustness check, we run the analysis excluding from the sample participants that exhibited implausibly high levels of a-insensitivity (i.e. those who had a higher matching probability for p = 0.1 than for p = 0.9, and thus an estimated a-insensitivity>1).

6. Results

Table 5 presents an overview of our main results for standalone index insurance, as described in Section 4. In column 1 we use the standardized value of WTP, in columns 2–5 we standardize the natural logarithm of WTP. A one standard deviation increase in loss aversion reduces WTP by around 10% whereas a one unit increase in our standardized measure of ambiguity aversion increases WTP by around 11–12%.[30] Ambiguity

[30] Table A2 in Appendix II presents the same results of Table 6 but with standardized outcome variables, to aid the interpretation of effect sizes. In that table, we can see that a one standard deviation increase in loss aversion reduces WTP by around 18.5 sd. whereas a one unit increase in our standardized measure of ambiguity aversion increases WTP by around 21.7 sd., both constituting medium-low effect sizes.

insensitivity has a negative sign but does not significantly affect WTP.[31] These results are robust to adding socio-economic controls, spatial fixed effects, as well as to an orthogonalization of the variables of interest.[32] While the result on loss aversion confirms our prior of a negative relationship, the strongly significant positive sign of ambiguity aversion goes against the direction anticipated by the results of Bryan (2019) and Belissa et al. (2020), and are more in line with the findings of Lambregts et al. (2021). We take this as evidence that ambiguity aversion may not always reduce index insurance uptake, and that our ambiguity averse subjects likely overvalued the disutility they derived from the ambiguous bad weather. However, we defer a further investigations on the mechanism that might explain this divergence of results to the next section. A-insensitivity, instead, does not seem to significantly reduce WTP, as predicted by Baillon et al. (2020). We further elaborate on the potential reasons behind this result in the discussion section.

In column 5 we further disentangle our measure of ambiguity aversion by taking the difference between the expected probability and the matching probability at different levels of expected negative outcome. We choose to take expected negative outcome levels instead of expected probabilities for gains and losses because we conjecture that both ambiguous rainfall variation and ambiguous insurance payout constitute low probability negative events Namely, weather shocks represent low probability loss in a loss frame, while the absence of insurance payout in bad years represents a low probability of no gain in a gain frame. We include a dummy for elicitation in the gain domain, as well as interaction at each level of negative outcome. Note that we cannot include a-insensitivity in this analysis due to collinearity. We find that the level at which we measure ambiguity aversion does matter. From column 5, we identify three main take home points: 1) the results are driven by ambiguity aversion measured in the context of low probability negative event—i.e. low probability loss prospects and high probability gain prospects. 2) ambiguity aversion measured in the loss domain significantly increases WTP – i.e. people that are ambiguity averse for low probability negative events, such as weather shocks, overvalue insurance –, whereas ambiguity aversion measured in the gain domain significantly decreases it—i.e. people that dislike high probability ambiguous gain events, such as index insurance payout. 3) ambiguity aversion measured at greater magnitudes of expected negative outcomes are less predictive of WTP for both gains and losses. In next discussion section we will further elaborate on possible reasons for the discrepancy between the results of our lab-in-the-field experiment and empirical evidence from the field.[33]

Finally, we set out to investigate the role of the rebate framing. This is as much a confirmatory exercise as it is exploratory. Confirmatory because Serfilippi et al. (2020) find that the overall WTP for rebate-type insurance – where the premium is deducted from payouts in bad years – is significantly greater than for standalone index insurance. Exploratory because this study is the first to investigate how the loss and ambiguity averse might respond to such framing. We offered around half of our sample a standalone index insurance contract opportunity, while another half was offered the possibility to purchase a rebate index insurance. Since the two products are actuarially identical, any difference in WTP should arise from the behavioral perceptions of the product's functioning. Table 6, column 1, shows that we cannot confirm the finding that rebate insurance exhibits a higher average WTP in our setting. While the coefficient remains positive, it is far from statistically significant ($t = 0.76$). Instead, in column 2 we show that the rebate framing does have an effect on the loss averse—all but cancelling the effect of loss aversion on WTP. This result is robust to taking the natural logarithm of WTP, and to adding socio-economic and spatial controls (column 3 and 4). In column 5 we remove from the analysis participants that exhibited levels of a-insensitivity > 1, i.e. who had matching probabilities that were higher for the 10% than for the 90% expected probability of outcome—a robustness check that our results are not driven by particularly noisy signals.

If the loss averse seem to prefer the rebate framing, the same cannot be said of the ambiguity averse. For them, the rebate insurance significantly reduces WTP compared to a standard index insurance product. Under the rebate insurance, a one standard deviation increase in ambiguity aversion decreases WTP by around 7%. We conjecture that the increased contractual ambiguity of rebate insurance – now basis risk affects not only the likelihood of payout, but also that of having to pay the premium – discourages the ambiguity averse. The effect of a-insensitivity on WTP is again statistically insignificant. .

7. Mechanisms explaining the positive relationship

We find that ambiguity aversion robustly increases WTP for insurance, while field experimental evidence from Malawi and Ethiopia seems to point in the opposite direction. In Section 2 we have sketched some of the possible explanations for this apparent divergence. In what follows we will probe these potential mechanisms as follows.

First, we will construct a measure of ambiguity aversion that closely mimics Belissa et al. (2020). We chose this study rather than Bryan (2019) as the latter is based on hypothetical survey questions, while the former is incentive compatible—as in our case. Belissa et al. (2020) measure ambiguity aversion by offering a multiple price list in which participants could choose between a prospect with known probabilities varying from $p = 0$ to $p = 1$, and an ambiguous one with two colours of pens of unknown probability. From this, they derive the Constant Relative Ambiguity Aversion (CRAA) coefficient, following Klibanoff et al. (2005). We perform the same procedure and standardize the coefficient for ease of interpretation.[34] Next, we proceed to check if our preferred measure of ambiguity aversion is at all correlated with the CRAA.

Second, we use the standardized CRAA to predict the effect of ambiguity aversion on WTP for standalone insurance. To test for the possibility that the discrepancy is driven by omitted variable bias, we do so with and without controlling for loss aversion, the latter being the specification used in both Bryan et al. (2019) and Belissa et al. (2020). We run this latter specification using the CRAA for the domain of gains only, and then for both gains and losses—still only measured only through a mid-probability ambiguous prospect.

Finally, we exploit the fact that in our sample about a random subsample of the participants had been exogenously exposed to a more intense familiarity to insurance because of their assignment to treatment in Bulte et al. (2020). This treatment consisted in a free index insurance

[31] Among the socio-economic control variables, not reported in Table 6 but available upon request, the only one that is systematically significant (negative) at the 5% level is the quantity of livestock. The fact that participants with more livestock seem to attach less value to the insurance product in cases of drought confirms the role of livestock in the area of Meru as a traditional risk coping strategy—a form of self-insurance. Having been insured in the previous season enters negatively but is not significant at the conventional levels.

[32] As mentioned in Section 4, all results are also robust to inclusion/exclusion of a dummy variable for real-life insurance uptake, as well as assignment to treatment/control in the study by Bulte et al. (2020) that perfectly overlaps with our sample.

[33] To check the extent to which our results may be driven by miscomprehension, we create a comprehension score based on two control questions and exclude people in the lowest category (35 out of 136 observations). We present the results in Table A3 of Appendix II showing substantial robustness of the results. Similarly, while we control for education, we can exclude the lowest educated quartile in our sample (see Table A4 in Appendix II). Also in this case we find that our coefficients of interest are basically unchanged, with much greater overall explanatory power (R^2). Table A5 instead reveals that the effects are robust to excluding loss aversion from the regressions.

[34] We thank the corresponding author of Belissa et al. (2020) for assistance in the estimation procedure used.

Table 7
Mechanism analysis.

	(1)	(2)	(3)	(4)	(5)	(6)
	Ambiguity Aversion (z_b)	WTP gains domain	WTP gains domain	WTP gains domain	WTP both domains	WTP pooled sample
Ambiguity aversion (z_CRAA)	0.620***	0.139**	0.119*	0.0465	-0.0398	-0.0860
	(0.0673)	(0.0675)	(0.0682)	(0.0786)	(0.0648)	(0.0532)
Loss aversion (z_λ)		-0.161*				
		(0.0820)				
Exogenous experience				-0.281	-0.177	0.0571
				(0.179)	(0.147)	(0.122)
Exogenous experience × z_CRAA				0.183	0.220**	0.177**
				(0.144)	(0.103)	(0.0837)
Constant	0.000	-0.773*	-0.990**	-0.954**	-0.599	-0.0995
	(0.0671)	(0.443)	(0.427)	(0.438)	(0.396)	(0.398)
Controls	yes	yes	yes	yes	yes	yes
N	138	68	68	68	136	276
R^2	0.384	0.245	0.197	0.251	0.0877	0.0233

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

product conditional on the purchase of certified seeds. While only slightly less than half of those treated actually received a free insurance (thus making our test an intent to treat analysis) this constitutes a significantly greater share than those having been exposed to insurance in the control group. We therefore interact the initial treatment assignment in Bulte et al. (2020) to our CRAA based on Belissa et al. (2020) to test the effect of increased familiarity with insurance on insurance uptake with and without ambiguity aversion.

Table 7 presents our findings. Column 1 shows that the CRAA is actually highly positively correlated to and thus predictive of our preferred measure of ambiguity aversion (referred to as z_b in previous tables). The CRAA measured only at mid probability gains explains 38.4% of the variance in z_b. A further test not reported here shows that the CRAA is instead not significantly correlated with ambiguity insensitivity (r(134) = 0.0987, p = 0.250). Also for this reason, the results of column 2 do not come as a surprise: our positive relationship with WTP is confirmed when using the CRAA instead of the source method proposed by Abdellaoui et al. (2011). We therefore rule out that the discrepancy is due to the use of a 'problematic' measure of ambiguity aversion in the previous studies. Similarly, when omitting loss aversion, as in column 3, the coefficient of ambiguity aversion remains relatively stable—albeit now only significant at the 10% level. This again rules out that discrepancies are due to omitting or including loss aversion. Instead, when interacting ambiguity aversion with our exogenous measure of familiarity with insurance, the significant positive effect is fully explained away (column 4). The majority of the effect is transferred to the interaction term, that albeit not significant shows that ambiguity aversion in the presence of previous experience increases WTP by almost 0.2 standard deviations. This result is sustained even when including the CRAA taken at mid probability losses (column 5) and when including also the portion of the sample under the rebate framing (column 6). For the latter two, the interaction term becomes significant at the 5% level. These results indicate that experience has an important role to play in mediating the relationship between ambiguity attitudes and insurance uptake.[35]

[35] The fact that while becoming negative for those without insurance familiarity, our measure of CRAA is never significantly negatively associated with WTP could be attributed to average greater familiarity also for the control group of Bulte et al. (2020), compared to the previous studies, as they all participate into one training. Alternatively, it could be an issue of insufficient power.

8. Discussion and conclusions

This study investigates the role of ambiguity attitudes in shaping the willingness to pay for index insurance in the presence of basis risk. Our paper is the first to investigate the role ambiguity attitudes play in shaping WTP for index insurance not solely based on measures taken against a mid-likelihood of winning. Instead, we estimate ambiguity aversion and a-insensitivity using the source method proposed by Abdellaoui et al. (2011), at different levels of expected probability for both gains and losses. Our findings related to ambiguity aversion go in the opposite direction of empirical findings in the field (Belissa et al., 2020). We find that on average more ambiguity averse subjects are willing to pay more to insure against ambiguous weather shocks in our framed experiment, even if the insurance entails ambiguous nonperformance (negative basis risk). This is more in line with the experimental findings of Lambregts et al. (2021), who find a similar relationship as long as the insurable risk has sufficiently low probability (<50%). As is the case for them, our results suggests that on average ambiguity averse decision makers discount the ambiguity of the insurance less than its effects on the ambiguous low probability insurable loss .

One way to interpret these results is that ambiguity aversion measured for mid-likelihood gains is but a poor proxy of ambiguity aversion. In fact, aversion for ambiguity is notoriously not universal (Kocher et al., 2018), but rather follows a fourfold pattern that makes the very subjects exhibiting greater aversion for certain prospects exhibit highest ambiguity seeking behaviour for other prospects. As we confirm in our sample, on average people exhibit greater ambiguity aversion for 1) high likelihood gains and 2) low likelihood losses; and greater ambiguity seeking for 3) low likelihood gains and 4) high likelihood losses. A subject following such pattern will be labelled as ambiguity seeking if the known probability lottery is set at a low enough chance of winning for her to be ambiguity seeking at that (low) likelihood of winning. But that same subject would be labelled as ambiguity averse if the chance of winning is high enough for her to consider that a 'high likelihood gain'. Let's assume that people like her (following the same pattern and thresholds) are more likely to buy insurance in a given sample compared to a strawman that is perfectly ambiguity neutral. Whether that study will find that insurance uptake is positively or negatively correlated with ambiguity aversion entirely depends on the likelihood of gain in the known probability lottery for which ambiguity aversion has been measured. More so, if we are to believe the reflectivity of ambiguity preferences, these results would have been opposite had ambiguity been measured in the loss domain. Thus, our study makes an important contribution to the debate by introducing the idea that the point at which ambiguity attitudes are measured matters when we want to compare them to demand for insurance, unless some non-frame dependent measure of ambiguity aversion can be used—as is the case for the ‘source method’ used in our paper.

Another way to interpret this results rests in the fact that the degree of perceived ambiguity of a certain prospect may rest in the experience that one has with the occurrence of negative and positive events, and with the performance of the index insurance. This said, as Bryan (2019) notes too, this pattern would not necessarily be intrinsic to index insurance (which by design has ambiguous nonperformance in the form of basis risk) but to any new technology. In this sense, the findings of Bryan (2019) and Belissa et al. (2020) could well be extended to, say, improved seed varieties, fertilizer or pesticides if they are driven by experience with the new technology rather than by the presence of basis risk.

We test these two alternative interpretations by mimicking the measure of ambiguity aversion used in Belissa et al. (2020) only at mid-probability gains. We show that this apparent divergence is not caused by differences in the method used to estimate ambiguity aversion compared to existing field studies. Rather, we exploit exogenous variation in the familiarity with insurance within our sample to show that it is explained away by the role of experience with the novel technology—a previously underestimated mediator. Ambiguity aversion hinders adoption at early stages but increases when the insurance is better understood.

We believe that our results set an important cautionary tale about the fact that current evidence from field experiments that index insurance adoption is hindered by ambiguity aversion may not be conclusive about the relationship between ambiguity aversion and index insurance uptake. While the ambiguity averse are always going to be less likely to be early adoptions of technological innovations, the additional degree of ambiguity intrinsic to index insurance (basis risk) needs not to immediately cause a negative response. Ultimately this will depend on both the experience with weather shocks and insurance nonperformance, as well as the expected likelihood and disutility of either.

Another prediction that we could not confirm relates to the fact that in our experimental setting a-insensitivity does not seem to significantly reduce WTP, as theorized by Baillon et al. (2020). One possible explanation for this result is that we estimated the high and low probability scenarios too close to certainty. In fact, at extremely low and high probabilities insensitivity for probability converts necessarily to over-sensitivity. We cannot rule out that the point of inflection between the region of insensitivity and that of over-sensitivity varies across subjects. If so, some heavily insensitive subjects that however have a smaller insensitivity region may have been estimated by us as less a-insensitive, and vice versa. l’Haridon and Vieider (2019) elicited probability weighting functions in 30 countries all over the world, and the insensitivity region according to their parameter estimates ranges from 8% to 84% for risk preferences. Perhaps a more cautionary measure of a-insensitivity, taking 20% and 80% as high and low probability scenarios, would have yielded more robust results.

Finally, our study further explores the role of an insurance design innovation on willingness to pay: the so called “rebate” framing—mimicking a credit-insurance bundle. In the rebate framing, farmers buy insurance through a zero-interest loan, which they pay back upon realization of the state (e.g. at harvest), unless the payout is triggered, resulting in a payout from which the premium is automatically deducted (thus, rebated). A previous experiment by Serfilippi et al. (2020) had shown that for people with discontinuous preferences for certainty this framing significantly increased WTP compared to an actuarially identical standard insurance. Our prior was that this setting would particularly appeal the loss averse but could increase the distaste by those averse to ambiguity. This was confirmed by our analysis, in which we found no significantly different WTP across the two framings on average. This, however, does not diminish the role of credit-insurance bundles in increasing the uptake of insurance, instead indicating that they might not be a one-size-fits-all solution. In fact, we do find that the rebate framing increases (decreases) the appeal of insurance for the loss averse (ambiguity averse). Moreover, our finding is at odds with evidence from the field on greater uptake for insurance products that postpone premium payment until after the realization of the state. Eliciting WTP for the rebate insurance in the lab allowed us to single out the effect of loss and ambiguity aversion in the absence of a time dimension. Our results are simply indicative that 1) seasonal liquidity constraints (i.e. there is more cash after harvest), time inconsistencies and time preferences play a bigger role in the uptake of credit-insurance bundles, and/or that 2) the relative appeal of a “rebate”-type insurance dependent on the actual prevalence of loss and ambiguity aversion in different contexts of intervention.

Taken together, our findings on loss and ambiguity aversion have both negative and positive implications from a policy perspective. The unambiguous evidence that loss averse farmers dislike insurance products with basis risk, and that the majority of them are indeed loss averse, calls a radical rethinking of insurance products aimed at smallholder farmers—towards solutions that aim to zero-down on basis risk. One direction for this is to continuously update models and refine data collection to avoid discrepancies between the index and farm level outcomes. Another such solution could be Picture Based Insurance (PBI). PBI makes use of the growing presence of smartphones even in rural developing contexts, to verify insurance claims using smartphone pictures of insured plots, to reduce basis risk while at the same time not increasing asymmetric information problems and monitoring costs (e.g. Ceballos et al., 2019). This said, as farmers familiarize with index insurance products and their true returns, while climate change increases the unpredictability of weather patterns, their taste for insurance might increase with time even in the presence of basis risk persists.

CRediT authorship contribution statement

Cecchi Francesco: Conceptualization, Data curation, Formal analysis, Methodology, Supervision, Writing – original draft, Writing – review & editing. **Slingerland Edwin:** Conceptualization, Data curation, Formal analysis, Methodology, Writing – original draft. **lensink Robert:** Conceptualization, Formal analysis, Methodology, Project administration, Supervision, Writing – original draft, Writing – review & editing.

Appendix I

Lottery	Accept	Reject
#1 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 50 KSh. Do you accept or reject to play this game?		
#2 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 75 KSh. Do you accept or reject to play this game?		
#3 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 100 KSh. Do you accept or reject to play this game?		
#4 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 125 KSh. Do you accept or reject to play this game?		
#5 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 150 KSh. Do you accept or reject to play this game?		
#6 If the coin turns up heads, then you win 150 KSh. If the coin turns up tails, then you lose 175 KSh. Do you accept or reject to play this game?		

Fig. A1. The simple lottery choices.

Fig. A2. : Example of lottery choice #1 as shown by the tablet.

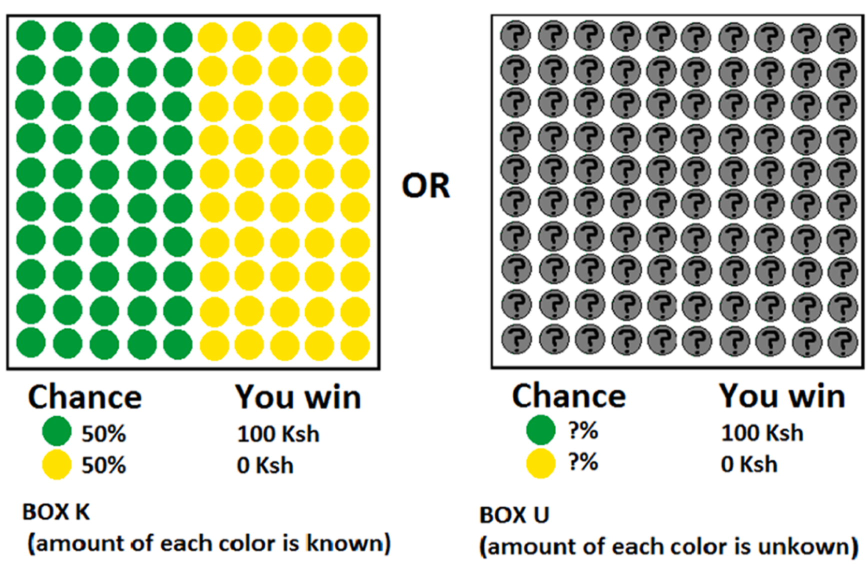

Fig. A3. : Starting scenario 1 of Ambiguity game (Gain).

Appendix II

Table A1
distribution of sample across villages

Village	Observations	Percentage
Akithi	68	24.64
Athwana	29	10.51

(continued on next page)

Table A1 (*continued*)

Village	Observations	Percentage
Kianjai	80	28.99
Mbeu	34	12.32
Nkomo	25	9.06
Thuura	40	14.49
Total	276	100.0

Table A2

Willingness to pay for index insurance (standardized outcome variables)

	(1)	(2)	(3)	(4)	(5)
	WTP	lnWTP	lnWTP	lnWTP	lnWTP
Loss aversion (z_λ)	-0.149**	-0.186**	-0.185**	-0.185**	-0.116
	(0.0736)	(0.0817)	(0.0844)	(0.0827)	(0.0940)
Ambiguity aversion (z_b)	0.122**	0.206***	0.217***	0.208***	
	(0.0510)	(0.0726)	(0.0695)	(0.0702)	
A-insensitivity (z_a)			-0.0573	-0.0561	
			(0.0723)	(0.0708)	
Ambiguity aversion at an expected negative outcome of:					
▪10%					0.234**
					(0.114)
▪10% × Gain domain					-0.366**
					(0.167)
▪50%					0.156
					(0.119)
▪50% × Gain domain					-0.00516
					(0.166)
▪90%					0.149
					(0.101)
▪90% × Gain domain					0.0285
					(0.161)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	136	136	136	136	136
R^2	0.052	0.076	0.194	0.194	0.246

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns 1-3 we use the standardized version of loss aversion, ambiguity aversion and a-insensitivity. In column 5 we orthogonalize the variables used in columns 1-4. In column we split the estimates of ambiguity aversion at different levels of expected negative outcome and interact them with a dummy representing the gain domain (thus level coefficients refer to the loss domain).

Table A3

Willingness to pay for index insurance (excluding lowest comprehension quartile)

	(1)	(2)	(3)	(4)	(5)
	WTP	lnWTP	lnWTP	lnWTP	lnWTP
Loss aversion (z_λ)	-93.31	-0.120**	-0.133**	-0.129**	-0.108*
	(57.76)	(0.0491)	(0.0549)	(0.0528)	(0.0582)
Ambiguity aversion (z_b)	77.14*	0.0976**	0.112**	0.108**	
	(41.84)	(0.0444)	(0.0457)	(0.0457)	
A-insensitivity (z_a)			-0.00509	-0.00330	
			(0.0454)	(0.0436)	
Ambiguity aversion at an expected negative outcome of:					
▪10%					0.126*
					(0.0663)
▪10% × Gain domain					-0.210*
					(0.115)
▪50%					0.0642
					(0.0713)
▪50% × Gain domain					-0.0911
					(0.0996)
▪90%					0.101
					(0.0636)
▪90% × Gain domain					-0.0690
					(0.101)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	101	101	101	101	101
R^2	0.054	0.088	0.206	0.204	0.263

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns 1-3 we use the standardized version of loss aversion, ambiguity aversion and a-insensitivity. In column 5 we orthogonalize the variables used in columns 1-4. In column we split the estimates of ambiguity aversion at different levels of expected negative outcome and interact them with a dummy representing the gain domain (thus level coefficients refer to the loss domain).

Table A4
Willingness to pay for index insurance (excluding lowest education quartile)

	(1)	(2)	(3)	(4)	(5)
	WTP	lnWTP	lnWTP	lnWTP	lnWTP
Loss aversion (z_λ)	-137.4**	-0.139***	-0.173***	-0.160***	-0.130*
	(53.29)	(0.0521)	(0.0565)	(0.0528)	(0.0687)
Ambiguity aversion (z_b)	96.48***	0.150***	0.165***	0.159***	
	(35.71)	(0.0479)	(0.0480)	(0.0497)	
A-insensitivity (z_a)			-0.0124	-0.0102	
			(0.0449)	(0.0436)	
Ambiguity aversion at an expected negative outcome of:					
▪10%					0.160**
					(0.0695)
▪10% × Gain domain					-0.129
					(0.101)
▪50%					0.0533
					(0.0737)
▪50% × Gain domain					-0.0201
					(0.101)
▪90%					0.131*
					(0.0678)
▪90% × Gain domain					-0.00581
					(0.111)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	99	99	99	99	99
R^2	0.099	0.128	0.306	0.294	0.339

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns 1-3 we use the standardized version of loss aversion, ambiguity aversion and a-insensitivity. In column 5 we orthogonalize the variables used in columns 1-4. In column we split the estimates of ambiguity aversion at different levels of expected negative outcome and interact them with a dummy representing the gain domain (thus level coefficients refer to the loss domain).

Table A5
Willingness to pay for index insurance (without loss aversion)

	(1)	(2)	(3)	(4)	(5)
	WTP	lnWTP	lnWTP	lnWTP	lnWTP
Ambiguity aversion (z_b)	65.79*	0.104**	0.115***	0.110***	
	(34.39)	(0.0430)	(0.0399)	(0.0403)	
A-insensitivity (z_a)			-0.0524	-0.0393	
			(0.0406)	(0.0404)	
Ambiguity aversion at an expected negative outcome of:					
▪10%					0.128*
					(0.0647)
▪10% × Gain domain					-0.224**
					(0.0926)
▪50%					0.105
					(0.0639)
▪50% × Gain domain					-0.0263
					(0.0951)
▪90%					0.0792
					(0.0568)
▪90% × Gain domain					0.0372
					(0.0893)
Controls	no	no	yes	yes	yes
Village level f.e.	no	no	yes	yes	yes
N	136	136	136	136	136
R^2	0.0175	0.0380	0.163	0.161	0.236

Robust standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns 1-3 we use the standardized version of loss aversion, ambiguity aversion and a-insensitivity. In column 5 we orthogonalize the variables used in columns 1-4. In column we split the estimates of ambiguity aversion at different levels of expected negative outcome and interact them with a dummy representing the gain domain (thus level coefficients refer to the loss domain).

References

Abdellaoui, M., Baillon, A., Placido, L., Wakker, P., 2011. The Rich Domain of Uncertainty: Source Functions and Their Experimental Implementation. American Economic Review *101* (2), 695–723. https://doi.org/10.1257/aer.101.2.695.

Becker, G.M., DeGroot, M.H., Marschak, J., 1963. Stochastic models of choice behavior. Behavioral Science 8 (1), 41–55.

Baillon, A., Bleichrodt, H., 2015. Testing Ambiguity Models through the Measurement of Probabilities for Gains and Losses. American Economic Journal: Microeconomics 7 (2), 77–100. https://doi.org/10.1257/mic.20130196.

Baillon, Aurélien, Bleichrodt, Han, Emirmahmutoglu, Aysil, Jaspersen, Johannes, Peter, Richard, 2020. When Risk Perception Gets in the Way: Probability Weighting and Underprevention. Operations Research 70 (3), 1371–1392. https://doi.org/10.1287/opre.2019.1910.

Bajtelsmit, Vickie, Coats, Jennifer C., Thistle, Paul, 2015. The effect of ambiguity on risk management choices: An experimental study. Journal of Risk and Uncertainty 50 (3), 249–280.

Belissa, Temesgen, Bulte, Erwin, Cecchi, Francesco, Gangopadhyay, Shubhashis, Lensink, Robert, 2019. Liquidity constraints, informal institutions, and the adoption of weather insurance: A randomized controlled Trial in Ethiopia. Journal of Development Economics 140, 269–278.

Belissa, Temesgen, Keno, Robert, Lensink, Asseldonk, Marcel Van, 2020. "Risk and ambiguity aversion behavior in index-based insurance uptake decisions: Experimental evidence from Ethiopia.". Journal of Economic Behavior & Organization 180, 718–730.

Booij, A.S., van de Kuilen, G., 2009. A Parameter-Free Analysis of the Utility of Money for the General Population under Prospect Theory. Journal of Economic Psychology 30, 651–666. https://doi.org/10.1016/j.joep.2009.05.004.
Booij, A.S. and van de Kuilen, G. (2007), A Parameter–Free Analysis of the Utility of Money for the General Population under Prospect Theory, Working Paper, Amsterdam School of Economics. Available at: http://www1.fee.uva.nl/pp/bin/754fulltext.pdf.
Bouchouicha, Ranoua, Vieider, Ferdinand M., 2017. Accommodating stake effects under prospect theory. Journal of Risk and Uncertainty 55 (1), 1–28.
Bryan, G., 2010. Ambiguity and insurance. Unpublished manuscript. Yale University Department of Economics.
Bryan, G., 2019. Ambiguity aversion decreases the impact of partial insurance: Evidence from African farmers. Journal of the European Economic Association 17, 1428–1469.
Bulte, E., Cecchi, F., Lensink, R., Marr, A., Van Asseldonk, M., 2020. Does bundling crop insurance with certified seeds crowd-in investments? Experimental evidence from Kenya. Journal of Economic Behavior and Organization 180, 744–757.
Casaburi, Lorenzo, Willis, Jack, 2018. Time versus State in Insurance: Experimental Evidence from Contract Farming in Kenya. AMERICAN ECONOMIC REVIEWVOL 108 (12), 3778–3813.
Carter, M., Elabed, G., Serfilippi, E., 2015. Behavioral economic insights on index insurance design. Agricultural Finance Review Agricultural Finance Review Agricultural Finance Review 75 (1), 8–18 https://doi.org/10.1108/.
Ceballosa, Francisco, Kramera, Berber, Robles, Miguel, 2019. The feasibility of picture-based insurance (PBI): Smartphone pictures for affordable crop insurance. Development Engineering 4, 100042.
Clarke, D.J., 2016. A Theory of Rational Demand for Index Insurance. American Economic Journal: Microeconomics 2016 8 (1), 283–306. https://doi.org/10.1257/mic.20140103.
De Nicola, F., 2015. The impact of weather insurance on consumption, investment, and welfare. Quantitative Economics 6 (3), 637–661. https://doi.org/10.3982/QE300.
De Nicola, F., Hill, R.V., 2013. Interplay among Credit, Weather Insurance and Savings for Farmers in Ethiopia. Presentation at the American Economic Association Meeting. San Diego.
Dimmock, S.G., Kouwenberg, R., Wakker, P.P., 2016a. Ambiguity attitudes in a large representative sample. Management Science 62 (5), 1363–1380. https://doi.org/10.1287/mnsc.2015.2198.
Dimmock, S.G., Kouwenberg, R., Mitchell, O.S., Peijnenburg, K., 2016b. Ambiguity aversion and household portfolio choice puzzles: Empirical evidence. Journal of Financial Economics 119 (3), 559–577. https://doi.org/10.1016/j.jfineco.2016.01.003.
Elabed, G., Carter, M.R., 2015. Compound-risk aversion, ambiguity and the willingness to pay for microinsurance. Journal of Economic Behavior and Organization 118, 150–166. https://doi.org/10.1016/j.jebo.2015.03.002.
Emerick, Kyle, De Janvry, Alain, Sadoulet, Elisabeth, Dar, Manzoor H., 2016. Technological innovations, downside risk, and the modernization of agriculture. American Economic Review 106 (6), 1537–1561.
Epstein, L.G., Schneider, M., 2007. Learning under ambiguity. Review of Economic Studies 74 (4), 1275–1303.
Falco, C., Kong, D., Rotondi, V., Spelta, V., 2016. Investment, Insurance and Weather Shocks: Evidence from a Lab Experiment in Cambodia 10 (July), 1–38.
Fehr, E., Goette, L., 2007. Do Workers Work More if Wages Are High? Evidence from a Randomized Field Experiment. The American Economic Review 97 (1), 298–317.
Golub, G.H., Van Loan, C.F., 2013. Matrix Computations, 4th ed. Johns Hopkins University Press, Baltimore.
Hill, R.V., Viceisza, A., 2012. A field experiment on the impact of weather shocks and insurance on risky investment. Experimental Economics *15* (2), 341–371. https://doi.org/10.1007/s10683-011-9303-7.
Jensen, Nathaniel D., Christopher B. Barrett, and Andrew G. Mude. Basis risk and the welfare gains from index insurance: Evidence from northern Kenya. (2014).
Kahneman, D., Tversky, A., 1979. Prospect Theory: An Analysis of Decision under Risk. Econometrica *47* (2), 263–292.
Karlan, Dean, Osei, Robert, Osei-Akoto, Isaac, Udry, Christopher, 2014. Agricultural decisions after relaxing credit and risk constraints. The Quarterly Journal of Economics 129 (2), 597–652.
Klibanoff, P., Marinacci, M., Mukerji, S., 2005. A smooth model of decision making under ambiguity. Econometrica 73, 1849–1892.
Kocher, Martin G., Lahno, Amrei M., Trautmann, Stefan T., 2018. Ambiguity aversion is not universal. European Economic Review 101, 268–283.
König-Kersting, Christian, Trautmann, Stefan T., 2016. Ambiguity attitudes in decisions for others. Economics Letters 146 (32), 126–129.
Lampe, Immanuel, Daniel, W.ürtenberger, 2020. Loss aversion and the demand for index insurance. Journal of Economic Behavior & Organization 180, 678–693.
Lambregts, Timo R., Van Bruggen, Paul, Bleichrodt, Han, 2021. Insurance decisions under nonperformance risk and ambiguity. Journal of Risk and Uncertainty 63, 229–253. https://doi.org/10.1007/s11166-021-09364-7.
L'Haridon, Olivier, Vieider, Ferdinand, 2019. All over the map: worldwide comparison of risk preferences. QUANTITATIVE ECONOMICS 10 (1), 185–215.
Marinacci, M., 2002. Learning from ambiguous urns. Statistical Papers 43 (1), 143–151.
Peter, Richard, Ying, Jie, 2020. Do you trust your insurer? Ambiguity about contract nonperformance and optimal insurance demand. Journal of Economic Behavior & Organization 180, 938–954.
Rabin, M., 2000. Risk aversion and expected-utility theory: A calibration theorem. Econometrica 68 (5), 1281–1293.
Serfilippi, E., Carter, M., Guirkinger, C., 2020. Insurance contracts when individuals "greatly value" certainty: Results from a field experiment in Burkina Faso. Journal of Economic Behavior and Organization 180, 731–743.
Snow, Arthur, 2011. Ambiguity aversion and the propensities for self-insurance and self-protection. Journal of Risk and Uncertainty 42 (1), 27–43.
Trautmann, Stefan T., Vieider, Ferdinand M., Wakker, Peter P., 2011. Preference reversals for ambiguity aversion. Management Science 57 (7), 1320–1333.
Trautmann, Stefan T., van de Kuilen, Gijs, 2015. Ambiguity attitudes. In: The Wiley Blackwell Handbook of Judgement and Decision Making. Blackwell Publishing, pp. 89–116.
Tversky, A., Kahneman, D., 1992. Advances in Prospect-Theory - Cumulative Representation of Uncertainty. Journal of Risk and Uncertainty 5 (4), 297–323 https://doi.org/Doi 10.1007/Bf00122574.