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Measuring risk preferences in rural Ethiopia[*]

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Abstract

Risk aversion has generally been found to decrease in income. This may lead one to expect that people in poor countries will be more risk averse than inhabitants of rich countries. Recent comparative findings with students suggest the opposite, potentially giving rise to a risk-income paradox. Findings with students, however, may result from selection effects. We test whether a paradox indeed exists by measuring the risk preferences of over 500 household heads across several regions in the highlands of Ethiopia. We do so using certainty equivalents, which are well suited to the task due to their simplicity. We find high degrees of risk tolerance, consistent with the evidence obtained for students using the same tasks. In particular, the level of risk tolerance is higher than the one found for student samples in most Western and even middle income countries. We also find risk tolerance to increase in income proxies within our sample, thus completing the paradox.

Keywords: risk preferences; development; experimental methodology

JEL-classification: C93; D03; D80; O12

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1 Introduction

Uncertainty is a central fact in economic activity, and human life more in general. Rural people in developing countries are especially exposed to the vagaries of chance, since their largely agricultural income strongly depends on highly variable weather patterns and formal insurance against adverse events rarely exists. Nevertheless, our understanding of risk preferences and the role they play in the lives of rural populations in developing countries is still limited.

Poor inhabitants of developing countries have long been considered to be very risk averse (see Haushofer and Fehr, 2014, for a recent review). This conclusion is mostly based on the fundamental economic intuition that risk aversion should decline in wealth or income (Arrow, 1970; Gollier and Pratt, 1996). This intuition has indeed found considerable empirical support within various countries in the West (Donkers, Melenberg, and Van Soest, 2001; Dohmen et al., 2011), although the evidence is less uniform than one might think (see Hopland, Matsen, and Strøm, 2013, for a recent review).[1]

The evidence for developing countries is even less clear. Binswanger (1980) famously found no correlation between risk aversion and wealth, and Tanaka, Camerer, and Nguyen (2010) found a correlation only with average village income but not with personal income, and only for some parameters. Yesuf and Bluffstone (2009) found risk aversion to decrease in the availability of cash in Ethiopia, and Liebenehm and Waibel (2014) found the risk aversion of cattle farmers in Burkina Faso and Mali to decrease in income. Gloede, Menkhoff, and Waibel (2013) found risk tolerance to increase with income in two large rural samples in Thailand and Vietnam. Cardenas and Carpenter (2013), however, found no correlation between risk preferences and economic well-being (an aggregate measure of several wealth indicators) in an experiment in six Latin-American

[1]Even though there is considerable support for this hypothesis, not all studies find clear-cut evidence for the relationship. For instance, and von Gaudecker, van Soest, and Wengström (2011) only found the correlation for gain-loss prospects, and not for pure gain prospects (see also Booij, Praag, and van de Kuilen, 2010). Harrison, Lau, and Rutström (2007) even found an effect to the contrary in the Danish population, while Noussair, Trautmann, and van de Kuilen (2014) found a significant effect of income in a representative sample of the Dutch population only after controlling for household wealth.

countries.

Measurements of risk preferences in developing countries have also often confirmed high degrees of risk aversion on average (Binswanger, 1980; Yesuf and Bluffstone, 2009; Liebenehm and Waibel, 2014). They did, however, generally employ tasks that are seldom—if at all—used in the West, which makes comparisons difficult. The choice lists employed are further asymmetric, limiting the degree of risk seeking they can detect. Especially in the presence of noise, such asymmetric choice lists may result in the systematic overestimation of risk aversion (Andersson et al., 2015). If some subjects decide purely randomly, their choices may be counted towards risk aversion in lists that are skewed towards the detection of the latter. In particular, the Binswanger task is cut off at risk neutrality, so that any random choice would be counted towards risk aversion. Using simulations as well as experimental data, Crosetto and Filippin (2015) showed that Binswanger-style lists overestimate risk aversion, and that noise indeed compounds this overestimation—a problem that may be particularly important in samples with low education levels.

In contrast to the high risk aversion found in these studies with rural populations, recent cultural comparisons of risk preferences using student samples and employing the exact same experimental tasks across a large number of countries have found risk aversion to be considerably *lower* in developing countries than in rich, developed countries (Rieger, Wang, and Hens, 2014; Vieider et al., 2015). Taken together with the prevalent within-country result of risk aversion decreasing in income, the finding of risk aversion increasing in income per capita between countries suggests a *risk-income paradox*. Since these comparative results were obtained with students, however, it remains unclear whether they may be due to a selection effect, whereby in poorer countries children from relatively more affluent families attend universities. In that case, rather than finding a paradox, we might just observe a systematic selection effect.

In this paper, we test whether the between country results obtained with students extend to a large and geographically spread sample of the rural population of Ethiopia. We measure the risk preferences of a large sample of the Ethiopian rural population, covering regions of rural Ethiopia encompassing about 80% of the Ethiopian population and 70% of its landmass. We focus on the rural population, inasmuch as 81% of the population of Ethiopia lives in rural areas (World Bank data for 2013), and rural populations have been described as particularly risk averse in previous research (Haushofer and Fehr, 2014), so that they constitute a stronger test for our hypothesis than urban populations. Notwithstanding some growth over the last decade, Ethiopia remains one of the poorest countries in the world, with a GDP per capita of $1354 in 2013 in purchasing power parity (*PPP*) terms. This makes the sample an ideal testbed for whether the findings with students extend to general population samples.

We measure risk preferences using choice lists between binary lotteries or *prospects* and sure amounts of money. These tasks are commonly used in the West (Tversky and Kahneman, 1992; Bruhin, Fehr-Duda, and Epper, 2010; Abdellaoui et al., 2011), and have the advantage of being comparable to the abovementioned evidence collected with students. This will allow us to assess whether the differences in results described above are due to differences in elicitation tasks or differences in subject pools. They are easy to explain using physical devises, which makes them well suited to a population in which literacy rates are low. They are furthermore amongst the simplest tasks that can be used to measure risk preferences and vary familiar monetary amounts within lists, making them highly suitable for populations with low literacy rates. We further obtain several measurements of risk preferences for each person, allowing us to econometrically separate risk preferences from noise. This may be important, as noise in the measurement of risk preferences may be one of the factors affecting correlations with income in previous investigations.

Obtaining good income measures is often not trivial for the subsistence farmers that make up most of our sample—a fact that may contribute to the inconsistent evidence on the risk-income relationship (correlations with wealth, which is easier to measure, tend to be weaker and less consistent in general). Instead of measuring income directly, we thus recur to income proxies such as land size and altitude, which have been found to correlate strongly with income in agricultural populations. This reduces measurement problems, and has the further advantage that endogeneity may not be a primary concern (although we refrain from making any strong claims on causality, for which our data are not well suited).

We find the rural population of Ethiopia to be highly risk tolerant, thus departing from traditional conclusions about developing countries. Indeed, our rural population sample is significantly more risk seeking than typical student populations in the West. This generalizes the findings obtained with students, showing that they cannot be explained by systematic selection effects. At the same time, we find a strong correlation of risk tolerance with income proxies, indicating that more affluent households exhibit higher risk tolerance. The combination of high levels of risk tolerance in a very poor country with the typical negative correlation between risk aversion and income within our sample thus goes to complete the risk-income paradox.

We conclude the paper by discussing the implications of our findings in terms of the failure to adopt new technologies by poor households in developing countries, which has often been blamed on low risk tolerance. Given the high levels of risk tolerance we find in the aggregate, such an account does no longer seem to hold up (although the explanation may remain valid for the poorest and most vulnerable households within our sample). This indeed appears to reopen the debate on the relative importance of preferences and institutional constraint when it comes to risk taking by farmers in developing countries (Feder, Just, and Zilberman, 1985), an investigation of which forms a promising avenue for future research.

This paper proceeds as follows. Section 2 describes the subject pool and provides details on the experimental tasks and procedures, as well as discussing data quality. Section 3 discusses our modeling assumptions and presents the stochastic structure and econometric methods used to fit functional forms to the data. Section 4 presents the results, using both parametric techniques and a nonparametric stability check of the results. Finally section 5 discusses the results and concludes the paper.

2 Experimental setup

2.1 Subject pool characteristics

A total of 504 household heads were recruited in three regions in the Ethiopian highlands.[2] The study was carried out in the context of an investigation of the effectiveness of improved cookstoves under the REDD+ program (a United Nations program aimed at reducing emissions from deforestation). This focus also determined the stratification technique used to select the sample. Subjects were selected from the three regions involved based on forest cover, with 20% of subjects from Amhara, 50% from Oromia, and 30% from the Southern Nations, Nationalities and Peoples Region (*SNNP*; out of the total population of the three regions, Amhara makes up approximately 29%, Oromia 46%, and SNNP 15%, so that Amhara is slightly undersampled). These regional states represent 80% of the population and over 70% of the land area of Ethiopia.

Thirty-six villages (locally called Got or sub-Kebele) were randomly selected from the three regions from a list of 110 villages previously selected by the Ethiopian Development Research Institute (*EDRI*) to collect forestry data. Out of the 110 sites, we removed 15 sites that were covered during a pilot survey conducted to inform our research. We also removed all sites from Tigrai Regional State, as this state was less interesting in terms of the REDD+ questions asked in the study. We then randomly selected 36 villages from the remaining list. For each of these villages, a list of households was obtained from the local administration. Subsequently, 14 households were randomly picked from each village using systematic random sampling. This resulted in a sample size that could be covered with our research budget, while at the same time ensuring wide geographical coverage. The data were collected by a total of 25 fieldworkers (5 supervisors and 20 enumerators) who were extensively trained on the experiments.

[2]The exclusive use of household heads is unlikely to significantly affect our conclusions. Studying 347 rural Ethiopian couples, Di Falco and Vieider (2016) show that spouses' risk preferences are not significantly different from those of their husbands (although female household heads are much more risk averse than male household heads). This is also consistent with a recent meta-analysis by Filippin and Crosetto (2015), who show that gender differences are task specific and may be weaker than thought.

The supervisors all held a BSc degree and were experienced in field survey work. The enumerators and supervisors were selected so that they were able to speak the local languages. The experimental procedures were refined in extensive pilots before starting the actual experiment, which also gave the enumerators a chance to train on the tasks. Supervisors paid particular attention to making the enumerators follow standardized procedures.[3]

The average age of our subjects is 42.13 years (SD: 13.2), with a range between 20 and 90 years. Since the study was targeted at household heads, 89.9% of respondents are male. At 91% the overwhelming majority of our subjects work mainly in the agricultural sector, with the second largest group consisting of women doing house work (5%), and the third largest of people owning a business (2%). The median household has about 1.5 ha (about 3 acres) of land. About 38% of the respondents are illiterate, with the literate subjects having mostly only primary education (45% of the sample).

2.2 Experimental tasks and explanations

We measure risk preferences using certainty equivalents (*CEs*). CEs are easy to construct and to deploy. Physical representations of the choice problems are straightforward. In contrast to tasks such as the one popularized by Holt and Laury (2002), which have been found to result in high rates of inconsistencies (Lönnqvist et al., 2011; Charness and Viceisza, 2012), only monetary amounts vary within a given choice list, while probabilities stay fixed. This makes it easy to lay out money on a table and represent probabilities physically, which is a great advantage given people's familiarity with money. CEs can also easily be used to estimate one's favorite decision model (although more CEs are typically required for more complex models). Finally, while they allow for the estimation of structural models, they are also straightforward to analyze non-parametrically.[4]

[3]Since several languages needed to be covered, the assignment of enumerators to villages was not randomized, so that we cannot control for enumerator fixed effects in our regressions while also controlling for regional fixed effects. If we drop the latter and add enumerator dummies instead, all the main effects reported below remain stable.

[4]Some scholars have raised doubts on whether CEs are 'realistic' in the sense of modeling real world decision processes, based on the observation that real choices occur between risky

In a typical task or *choice list*, a subject is offered repeated choices between a lottery or *prospect* and different sure amounts of money. The prospect offers a probability p of obtaining a prize, x, or else an outcome y with a complementary probability $1-p$. We will represent such a prospect as $(x, p; y)$. The sure amounts s_j are always included between the prize and the low outcome of the prospect, i.e. $x \geq s_j \geq y$. The extreme outcomes of the prospect, x and y, are explicitly included in the list of sure amounts to serve as a rationality check. If such extremes are not included and subjects always choose either the prospect or the sure amount (i.e., they never switch), it may be difficult to determine whether this is due to true preferences or to a misunderstanding of the task. As long as preferences are consistent, i.e. subjects switch only once (see below), the certainty equivalent can then be taken to be the mean between the first sure amount that is chosen over the prospect, and the last sure amount for which the prospect was preferred over the safe option.

In this experiment, we fix the prize of the prospect at 40 Birr and the lower outcome at 0 throughout. The prize of 40 Birr corresponds to about US \$6 in purchasing power parity (World Bank 2013), for an overall expected payoff from participating equal to \$3 PPP for a risk neutral participant. Considering that most of our subjects live on less than two Dollars a day, the money at stake was significant and well in line with stakes in similar experiments (Yesuf and Bluffstone, 2009; Attanasio et al., 2012). We used a total of 7 choice lists, which offered a prize of 40 Birr with probabilities of $p = \{0.05, 0.10, 0.30, 0.50, 0.70, 0.90, 0.95\}$, and which were administered in random order. Using several choice lists has the advantage that noise can be easily separated from preference parameters in the

alternatives. We are unconvinced of this argument. For one, some choices in the real world do indeed involve tradeoffs between sure amounts and risky options (e.g., the decision whether to pay a sure amount of money for fertilizer to invest in a risky payoff from agriculture that may depend on other variables such as rainfall, or to keep that sure amount). Ultimetely, the question of the relative external validity of different elicitation tasks is one that needs to be addressed empirically, and no conclusive evidence on this point exists to date. One may also worry that having a sure alternative in an elicitation may trigger loss aversion relative to that sure outcome. In a seminal paper, Hershey and Schoemaker (1985) showed that this is indeed the case when a fixed sure outcome is compared to a prospect in which the probability of winning is varied. Varying the safe sure outcome instead, however, they found no such reference dependence.

econometric analysis. Since people may have difficulties grasping the concept of probability, we used only physical representations using colored balls, avoiding verbal references to probabilities. Indeed, the miscomprehension of probabilities may be inherent to risk preferences—further see the modeling section below for a discussion of this issue.

The sure amounts increased from 0 to 40 Birr (included) in steps of 1 Birr. Having a relatively small resolution can assign risk preferences to subjects with a higher degree of precision, thus potentially reducing noise (Crosetto and Filippin, 2015). Probabilities were implemented using 20 ping-pong balls, with balls of different colors associated to the high and low outcomes. The composition of balls was physically shown to subjects for each choice list. We chose to keep outcomes fixed across choice lists while changing probabilities, as we believe that for typical experimental stakes most of the interesting patterns emerge along the probability dimension (see also Fehr-Duda and Epper, 2012, on this point). This will restrict our model to one subjective dimension, so that more complex models which allow for two subjective dimensions, such as rank dependent utility or prospect theory, cannot be estimated based on our data. This methodology can, however, easily be expanded to the latter.[5]

Subjects were initially asked whether they consented to take part in the study. They were explained that the study consisted of various parts, including a questionnaire, and an experimental game. Before beginning the actual experiment, the process was carefully explained to subjects. All explanations and subsequent elicitations took place in individual interviews. Subjects were shown how the urn was composed. They were then shown the prospect, which was explained by laying out banknotes next to the associated colored ping-pong balls used as a chance device. Subjects were asked to choose between this prospect and the sure amount, also physically laid out next to the prospect. The enumerator introduced the example by explaining the entire choice list, i.e. by pointing out

[5] In particular, some choice tasks varying outcomes at a given probability are needed to separate utility curvature from probability transformation in the econometric analysis. To obtain good power for the observations, prospects with a non-zero lower outcome are necessary in addition to varying upper outcomes.

that the prospect would remain unchanged throughout the list, while the sure amount would change from the lowest to the highest amount in the list in steps of 1 Birr. Subjects were then asked for their choice between the prospect and 0 Birr for sure; and then for their choice between the prospect and 40 Birr for sure. Given that for the first everybody ought to prefer the prospect and for the second everybody ought to prefer the sure amount, this serves as a check of understanding, and quite naturally conveys the idea that subjects should only switch once (which was not enforced in case subjects still wanted to switch to and fro in the experiment).

Once a subject had understood this process, the enumerator began eliciting the preferences for different probability levels in random order. This random order had been predetermined, and each enumerator could read the order from the interview sheet (there were 14 different orders in total). Since the outcomes stayed the same throughout the experiment, the enumerator only needed to change and explain the color composition of balls from one task to the other. While enumerators were instructed to ask for a preference for each of the 41 sure amounts, in some instances participants would say that their preferences would stay the same for all higher amounts, or would even directly indicate where they wanted to switch from the prospect to the sure amount. In such cases, the enumerators were instructed to simply encode this switching point directly. The total experiment including the explanations took about 30-40 minutes on average.[6]

At the end of the risk experiment, one of the choice lists was randomly selected for real play—the standard procedure in this kind of experiment (Cubitt, Starmer, and Sugden, 1998). In that choice list, one choice between a given sure amount and the prospect was then extracted for real play, so that overall each decision had the same probability of being played for real. This procedure had been thoroughly explained to subjects while presenting the example at the beginning of the experiment. Subjects were explicitly asked to repeat the randomization procedure to the enumerator before starting with the actual experiment. Subjects

[6]This 40 minute period excludes the time needed for the questionnaire, which was administered in a separate instance.

were also told explicitly that, given this procedure, it was in their best interest to treat every single decision as if it were the one that would be played for real money at the end.

2.3 Data quality

The overall data quality is reasonably good. Only 3 out of 504 subjects, or 0.6% of our sample, switched multiple times from the prospect to the sure amount and back in the choice lists. We will exclude these subjects from the analysis, leaving us with 501 subjects. A further test of rationality are what we call strong violations of first order stochastic dominance, consisting in a preference for 0 Birr for sure over playing the prospect, or of playing the prospect over 40 Birr for sure. No subject preferred the sure 0 Birr to the prospect. On the other hand, 4 subjects, or 0.8% of the sample, indicated a preference for the prospect over 40 Birr for sure in at least one of the choice lists. These subjects will also be excluded from the analysis. Finally, for one subject we do not have responses to the questionnaire, leaving us with a total of 496 subjects.

We next look at (ordinary) violations of stochastic dominance. Such a violation occurs whenever a subject indicates a certainty equivalent for a given prospect that is lower than the certainty equivalent indicated for another prospect offering a lower probability of obtaining the same prize, $CE(p_j) < CE(p_i)$, $p_j > p_i$. About 38% of our subjects violate stochastic dominance at least once. Seen that most violations are relatively small in terms of amounts, this appears to lie within acceptable bounds, considering also the random ordering of the tasks. Vieider et al. (2013) found that about 25% of Vietnamese farmers violated stochastic dominance in a similar setting using a *fixed* ordering of tasks. Looking at total choices, our subjects violate first order stochastic dominance in 5.4% of choices overall. We thus conclude that the data are reasonably consistent, but that controlling for noise in the analysis will be important.

3 Aggregate data and modeling approach

3.1 Non-parametric representation of aggregate data

Table 1: Summary measures of aggregate risk preferences by prospect

prob.	EV	median CE	mean CE	SD	test =EV
0.05	2	7.5	10.88	10.37	$z = 18.21, p < 0.001$
0.10	4	9.5	13.53	10.19	$z = 17.84, p < 0.001$
0.30	12	15.5	18.05	10.05	$z = 11.51, p < 0.001$
0.50	20	22.5	23.01	9.12	$z = 6.26, p < 0.001$
0.70	32	29.5	27.13	8.58	$z = -1.49, p = 0.136$
0.90	36	34.5	32.01	8.51	$z = -7.33, p < 0.001$
0.95	38	37.5	34.38	8.17	$z = -6.54, p < 0.001$
mean	20	22.07	22.71	7.30	$z = 8.19, p < 0.001$

We start by conveying a feel for the data through non-parametric summary statistics for the different prospects, shown in table 1. Taking the mean CE over all the prospects and comparing it to the average expected value (shown in the last row of the table), we find that subjects are on average significantly risk *seeking*. Looking at individual prospects, we see that subjects are risk seeking for small probabilities and risk averse for large ones, as has typically been found in the literature (Preston and Baratta, 1948; Kahneman and Tversky, 1979; Abdellaoui, 2000). However, the risk seeking behavior prevails up to and including a probability of $p = 0.5$, which is much higher than has been found in the West. This serves to exclude explanations purely based on psychological factors that may lead subjects to switch closer to the middle of the list—a point to which we will return once we fit functions to the data.

The findings are, on the other hand, consistent with recent findings across 30 countries with students using the same type of tasks reported by Vieider et al. (2015). Figure 1 puts the level of risk tolerance in our sample into perspective by comparing it to the average in the student data across the 30 countries, based on the data reported by Vieider et al. (2015). The graph plots the average risk premium per country, defined as the expected value minus the certainty equivalent (a measure of absolute risk aversion), against GDP per capita (in logs and corrected for oil rents; see Vieider, Chmura, and Martinsson, 2012, for a discussion). Ethiopia is clearly the poorest country in the data set. It has also one of the lowest average risk premia, or highest levels of risk tolerance, amongst the student data from the 30 countries.

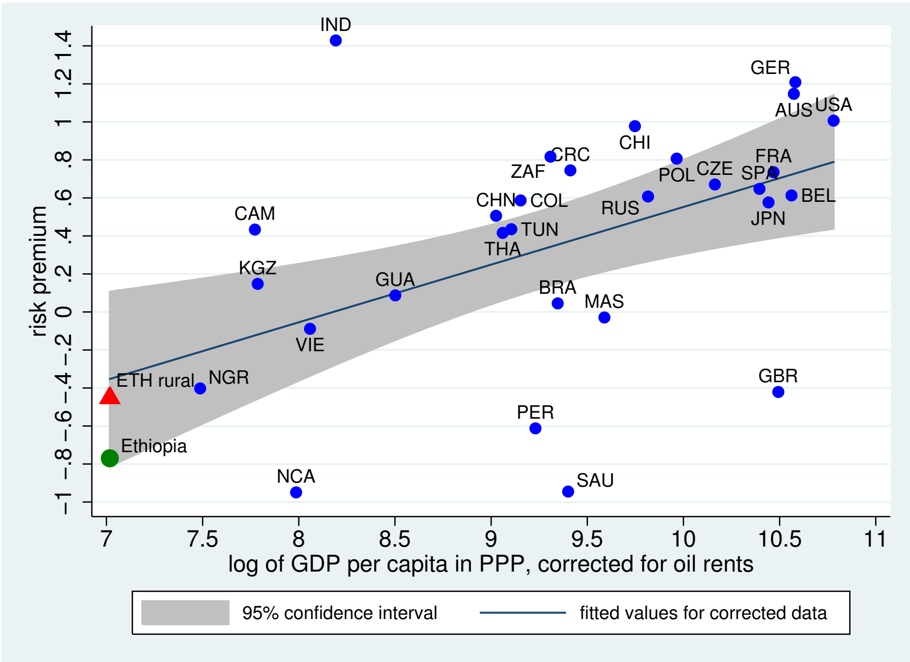


Figure 1: Average risk tolerance relative to student data

Overall, the Ethiopian general population sample (marked by a triangle and labeled 'ETH rural' in the graph) shows somewhat higher risk aversion than the Ethiopian students ($z = -2.22, p = 0.026$; two-sided Mann-Whitney test). This is consistent with findings from a comparison of students and a general population sample in Vietnam reported by Vieider et al. (2013), where a rural population sample was also found to be slightly more risk averse than the student sample. It also corresponds to the results of a comparison of students to a representative population sample in Switzerland with the same type of tasks reported by Fehr-Duda and Epper (2012). At the same time, being significantly risk seeking our sample is clearly more risk tolerant than Western student populations, which are generally significantly risk *averse* (with the exception of the UK, which constitutes an outlier in the correlation). This makes them also more risk seeking than student samples from middle income countries such as China, Thailand, Colombia or Tunisia, which are only sightly risk averse. The data thus support the observation that our general population sample from the poorest country in the student sample is significantly more risk tolerant than student samples in rich countries, thus excluding explanations purely based on selection effects in student data.

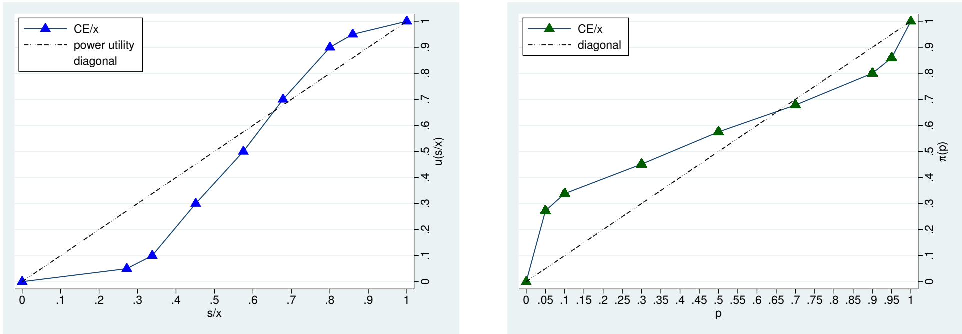
3.2 Modeling of preferences

We can now show how our data fit into different models in a purely non-parametric way. Finding a good descriptive model to fit the data is important inasmuch as this will improve our econometric analysis of the determinants of preferences, reducing potential attenuation bias. We further discuss our modeling assumptions in some detail, as they will determine our choice of functional forms to be used. Since the data patterns we find are relatively complex, they cannot be explained by one simple measure of risk aversion. The use of overly simple measures of risk preferences may indeed be partially to blame for past null findings in correlation analysis, as such measures may confound actual preferences with noise.

We start with an expected utility (EU) model. Since utility functions are unique only up to a positive linear transformation, we can arbitrarily fix the endpoints at $u(y) \equiv 0$ and $u(x) \equiv 1$. Plugging this into the general equivalence $u(CE_i) = p_i u(x) + (1 - p_i)u(y)$, we now simply obtain that $u(CE_i) = p_i$. The non-parametric mean utility function thus obtained is plotted in figure 2(a). This utility function resembles the one proposed by Markowitz (1952). Markowitz recognized that people may be risk seeking for some prospects while being risk averse for others, so that the utility function would have convex as well as concave sections. To accommodate this finding, he proposed to abandon initial wealth integration and to instead measure utility though changes of wealth.[7]

Markowitz based the derivation of this type of utility function on a simple thought experiment. In this experiment, he asked readers about their choices between a prospect offering a prize x with probability $p = 0.1$ or else nothing and the expected value of the prospect. For small x, most people would likely

[7]With initial wealth integration, convex and concave sections of the utility function might co-exist at the same point, since the same pattern has been found for all kinds of wealth levels, thus giving rise to inconsistencies.



(a) Nonparametric EU function (b) Nonparametric Dual function

Figure 2: Non-parametric functions

choose the prospect (e.g., most people would prefer a prospect offering a one in ten chance of \$10 over \$1 for sure). As x got larger, however, people would gradually switch to preferring the sure amount (e.g., most people would prefer a sure \$1,000,000 over a prospect offering a one in ten chance at \$10,000,000).

In our case, however, we kept the amounts fixed, and let probabilities vary instead. In a seminal paper, Preston and Baratta (1948) let both outcomes and probabilities vary systematically across choices. They observed that outcome variation had a negligible effect on the data (although the outcomes did obviously not range up to the amounts in Markowitz's thought experiment). Also, the pattern across different probability levels remained constant, no matter what the outcome level. This pattern gave rise to much experimentation by psychologists in subsequent years, and hit the economic discipline when probability weighting was incorporated into prospect theory jointly with utility transformations and published in Econometrica by Kahneman and Tversky (1979).

This consistent pattern across probabilities, which we also find in our data, suggests a different approach to modeling the choices we observe. One could model risk preferences through a subjective transformation of probabilities into decision weights, rather than a subjective transformation of outcomes into utilities. In other words, we can represent a choice as being linear in outcomes and non-linear in probabilities, such that $CE = \pi(p)x + [1 - \pi(p)]y$, where we will impose that $\pi(0) \equiv 0$ and $\pi(1) \equiv 1$. We can again simply solve this, noting that in our setting $\pi(p_i) = \frac{CE_i}{x}$. This non-parametric *Dual* function is depicted in figure 2(b), and can be seen to mirror the utility function to its left (see Yaari, 1987, for an axiomatization of the Dual function for rank-dependent utility).[8]

Being the dual of each other, the two functions presented above are *prima facie* perfectly equivalent. Nonetheless, we have a strong preference for the dual function. The experimental stimuli varied probabilities across choice lists. The same type of pattern—combining risk seeking for small and risk aversion for large probabilities—has been found for different outcome levels, which directly contradicts EU with a Markowitz-type utility function (similar violations would be observed for the Dual, if we had used significant variations in outcomes instead; see Bouchouicha and Vieider, 2016, for evidence of both violations and a discussion of their relative strength). Also, as we will further discuss below, the coexistence of risk seeking and risk aversion requires two-parameter functions to fit the data. Such functions are much more common, and the parameters have a clearer interpretation, under the dual theory than under EU. An analysis using a one-parameter utility function is nonetheless reported in the appendix.

3.3 Stochastic modeling

We have so far only derived non-parametric functions from the data. While this involves the least tampering with the data, such an approach completely neglects one of the strengths provided by a multiplicity of observations—the possibility to separate noise from genuine preferences. This will lead to attenuation bias in regression analysis, since the noise in the measurements will affect the correlations with our socio-economic variables. In this section, we will thus try to both reduce the number of parameters needed to describe the data (relative to the seven non-parametric data points), and to develop an explicit stochastic structure that allows us to filter out noise from the observations. Alas, this does not come for free. We will need to add some more assumptions, as well as some complexity to

[8]One could also think of this model as a rank dependent utility model with linear utility. Indeed, linearity of utility can often not be rejected in prospect theory models for typical experimental stakes. For instance, Vieider et al. (2013) fail to reject linearity in utility for their Vietnamese farmer sample. Linearity also holds for many (although not all) of the student samples mentioned above—see L'Haridon and Vieider (2015) for details.

the data estimation. Annotated Stata programs for all estimations in the paper are available for download at www.ferdinandvieider.com.

Following Bruhin et al. (2010), we econometrically represent decisions directly using the switching points from the prospect to the sure amount. This takes into account the structure of the experimental setup, in which we elicit certainty equivalents for prospects, $ce_i \sim \boldsymbol{p_i}$, where the subscript i indicates the particular prospect at hand, such that $\boldsymbol{p_i} = (x, p_i)$. This approach takes into account that choices within a given choice list are not independent. It is also much more efficient than a discrete choice approach, drastically reducing estimation time. All the results remain stable if a discrete choice approach is used instead.

We start from the observation that at the switching point the utility of the certainty equivalent is by definition equal to the utility of the prospect. Since outcomes enter the equation linearly, we can simply write:

$$\hat{ce}_i = \pi(p_i)x + [1 - \pi(p_i)]\, y = \pi(p_i)x \tag{1}$$

where $\hat{ce}_i$ is the certainty equivalent predicted by our model. This predicted certainty equivalent will not necessarily be equal to the one observed in the actual data. For instance, decision makers may make mistakes when calculating the utility of a prospect, or our model may be mis-specified relative to the true underlying decision process. We can thus represent the relation between the predicted and observed certainty equivalent as follows:

$$ce_i = \hat{ce}_i + \epsilon_i \tag{2}$$

where $\epsilon_i \sim N(0, \sigma^2)$ is an error term which captures the deviations mentioned above. We can now express the probability density function $\psi(.)$ for a given prospect i as follows

$$\psi(\theta, \sigma_i, \boldsymbol{p_i}) = \phi\left(\frac{\hat{ce}_{\theta i} - ce_i}{\sigma_i}\right) \tag{3}$$

where ϕ is the standard normal density function, and θ indicates the vector of parameters to be estimated. The subscript i to the noise term σ serves to remind us that we allow noise to depend on the characteristics of the single prospect. Since our prospects are, however, invariant except for the probability of winning the prize, this error term simply takes the form $\sigma_i = \sigma x$, which serves to standardize the error term of the model.

The parameters of the model can be estimated by maximum likelihood procedures. To obtain the likelihood function per decision maker, we need to take the product of the density functions above across prospects:

$$L(\theta) = \prod_i \psi(\theta_n, \sigma_{ni}, \boldsymbol{p_i}) \tag{4}$$

where θ is the vector of parameters to be estimated such as to maximize the likelihood function. Taking logs and summing over subjects, we obtain the following log-likelihood function:

$$LL(\theta) = \sum_{n=1}^{N} \ln \left[\psi(\theta_n, \sigma_{ni}, \boldsymbol{p_i})\right] \tag{5}$$

The subscript n to θ indicates that we will allow the estimated parameters to be linear functions of observable characteristics of decision makers in the regression analysis, such that $\hat{\theta} = \hat{\theta}_k + \beta X$, where $\hat{\theta}_k$ is a vector of constants and X represents a matrix of observable characteristics of the decision maker. The subscript n to the noise term σ indicates that the error is also made to depend on the observable characteristics of the decision maker.[9] We estimate this function in Stata 12 using the Broyden-Fletcher-Goldfarb-Shanno optimization algorithm. Errors are always clustered at the subject level.

[9]Yet a different approach would be to estimate a mixture model, allowing for heterogeneity in modelling assumption. Harrison, Humphrey, and Verschoor (2010) do so for several different countries in the developing world. Notice, however, how we could not use these methods to distinguish between EU and dual-EU, as they model the same processes through different parameters. While such methods could be used to distinguish between one- and two-parameter functions, one-parameter functions are a special case of the two-parameter setup in our case. Directly estimating the two-parameter function thus facilitiates the interpretation of regression results, without losing any generality in terms of modeling.

3.4 Aggregate data fitting

We are now ready to fit functional forms to our preference data. In figure 3 we fit a 2-parameter function developed by Prelec (1998) to the data, which takes the form $\pi(p) = e^{-\beta(-ln(p))^{\alpha}}$. The estimated parameters are $\alpha = 0.538$ ($se = 0.013$), $\beta = 0.703$ ($se = 0.009$), and $\sigma = 0.233$ ($se = 0.002$). The result is vastly superior to the fit of Prelec's 1-parameter function characterized by $\beta \equiv 1$, thus making the additional parameter worthwhile ($\chi^2(1) = 646.95, p < 0.001$, likelihood ratio test).[10] An analysis using a 1-parameter EU function is provided in the appendix.

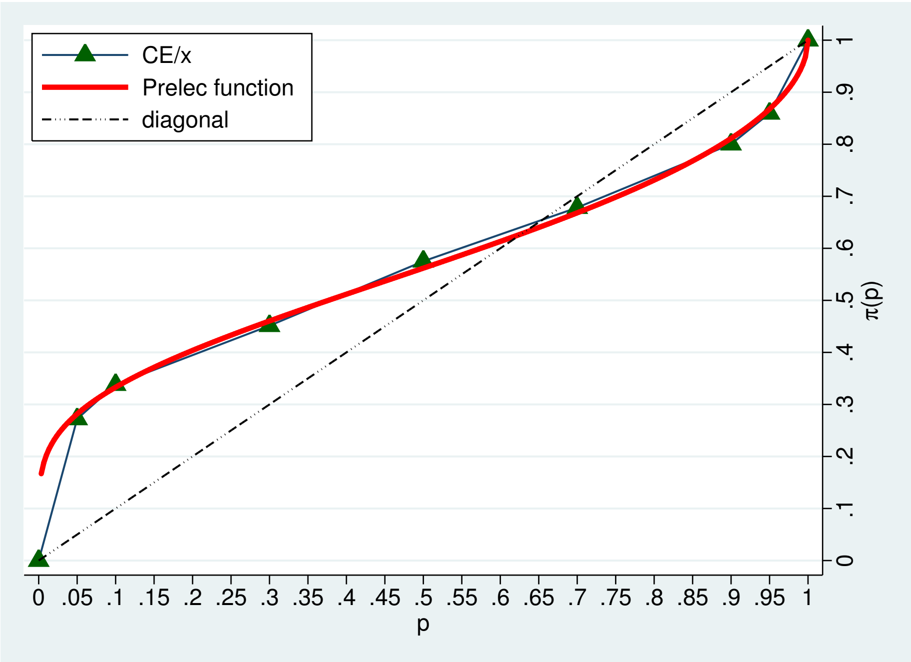


Figure 3: Fitting 2-parameter functions to the data

The parameters of the Prelec function have a precise behavioral interpretation. A parameter combination of $\alpha = 1$ and $\beta = 1$ in combination with linear utility indicates expected value maximization. The parameter β mostly governs the elevation of the function, with values >1 indicating a more depressed function and thus risk aversion under linear utility, and values <1 indicating a more

[10]The parameters of the 1-parameter function are $\alpha = 0.572$ ($se = 0.013$) and $\sigma = 0.256$ ($se = 0.003$). This function has a fixed crossing point of the diagonal at $1/e = 0.37$, and was developed with the explicit aim of fitting aggregate results from the West. In this sense, values of β lower than 1 indicate increased risk seeking, and deviations from what is considered 'typical' based on Western data. Other 1-parameter and 2-parameter functional forms perform similarly.

elevated function, and hence risk seeking. When $\alpha = 1$, β can thus be considered a measure of standard risk aversion. The parameter α governs mostly the slope of the curve, with values <1 indicating *probabilistic insensitivity*, i.e. CEs that change less than proportionately with probabilities. This is a phenomenon whereby people attribute greater weight to a given change in probability if it happens towards the endpoints of the scale close to $p = 0$ or $p = 1$ than if the same probability change occurs in an intermediate region. It is one of the most established findings in the prospect theory literature (Wu and Gonzalez, 1996; Abdellaoui, 2000; Bleichrodt and Pinto, 2000). Since linear probability weighting is considered to be normative (Wakker, 2010), probabilistic insensitivity is often perceived as a rationality failure (Tversky and Wakker, 1995).[11] As such, it may well capture issues of misunderstanding of probabilities, which may be larger in developing countries with low levels of literacy (indeed, they have been found to be associated with grade point average even in student samples; see L'Haridon and Vieider, 2015). We will refer to the two parameters as the risk aversion and the sensitivity parameter respectively.

4 Risk preferences and socio-economic conditions

4.1 Parametric analysis

We are now ready to examine the correlation of our measures with several characteristics of interest using our structural model (a non-parametric stability analysis is provided in the next section). We start by looking at indicators of wealth and income. Especially in developing countries there is a dearth of evidence on the effect of income, probably because good income measures are difficult to come by amongst the poor inhabitants of the rural regions of developing countries, who more often than not are subsistence farmers.

Rather than trying to obtain income measures—which would be unreliable in a sample consisting for the most part of subsistence farmers—we thus look at

[11] The parameter may also capture some systematic noise—as opposed to the truely random noise captured in σ—consisting in answers that are systamatically closer to the midpoint of the choice list.

Table 2: Summary statistics of main regressors

	mean	SD	min	max
land size (hectares)	1.80	1.61	0	10.5
altitude (meters)	2218	337.31	1437	3150
age (years)	42.13	13.14	20	90
literate	0.453	0.498	0	1
middle school	0.169	0.375	0	1
female	0.101	0.301	0	1
unmarried	0.086	0.281	0	1
TLUs**	4.990	3.669	0	26.23
pc1 wealth***	0	1.214	-3.415	2.912

**TLU stands for Tropical Livestock Units

***Wealth is represented as the first principal component of several indicators

some variables likely to be closely associated with income. Table 2 summarizes the income proxies used, along with several other controls in the regression. The land size owned by our households ranges from 0 to 10.5 hectares, with a mean of 1.80. While the use of land size as an income proxy clearly glosses over behavioral issues such as e.g. the use of fertilizer or effort expended on the farm, we consider this a strength of this measure inasmuch as this reduced endogeneity concerns. We do not have direct measures of income in our sample. However, using data collected in a survey run by the International Food Policy Research Institute (IFPRI), which is representative of an area largely overlapping with our study area in the Ethiopian highlands, land size and income show a clear positive correlation ($\rho = 0.318, p < 0.001, N = 892$; Spearman rank correlation). This further confirms the validity of land size as an income proxy.

Our second income proxy is altitude. This measure is taken from GPS measurements, and measured as elevation above sea level. At between about 1450 and 3150 meters, the range of elevations in our data is significant. The productivity of land decreases with altitude for several reasons. The lower temperatures prevalent at high altitudes lead to slower growth of crops. This effect is compounded by stronger winds, which tend to dry out the top soil. Furthermore, land at high altitude is often steeper, which means that water drains quickly and soil is easily eroded, leading to reduced soil quality and hence lower agricultural productivity. Recurring again to the same IFPRI data mentioned above, we indeed find a negative correlation between income and altitude ($\rho = -0.096, p = 0.004, N = 886$).

Nonetheless, altitude is clearly an imperfect proxy, as agricultural practices will also change with altitude. In particular, we find that at higher altitudes land is increasingly used to graze livestock, i.e. there is a significant correlation between altitude and the tropical livestock units ($TLUs$[12]) owned by a household in our data ($\rho = 0.127, p = 0.005, N = 487$). It is thus important to control for this in regression. There is no correlation between land size and altitude.[13] Finally, it is important to notice that altitude varies mostly between villages in our sample. This may make it a less reliable income proxy than land size, as other unobserved factors may also vary between villages.

Table 3 shows our regression results. Regression I looks at proxies for income, while controlling for level of education, business ownership, and some demographics including the sex and age of the respondent, and his marital status. We find land size to be highly correlated with risk preferences, with larger land ownership being associated with higher risk tolerance as indicated by a smaller β parameter, as we hypothesized. We also find higher altitudes to be related to reduced risk tolerance, as well as with increased probabilistic sensitivity. Notice again, however, that altitude varies mostly between villages. Adding village dummies to the regression thus eliminates the effect of altitude, while the effect of land size remains intact. Adding an interaction term between land size and altitude does not yield any additional insights.

Regression II tests the stability of the findings by adding indicators of wealth. In particular, we add tropical livestock units owned. This is important inasmuch as farmers at higher altitudes increasingly switch to livestock. In addition, we add the first principal component of wealth constructed out of a number of wealth

[12]Tropical livestock units were calculated based on the following conversion factor: cattle = 0.7, sheep = 0.1, goats = 0.1, pigs = 0.2, chicken = 0.01 donkey=0.5 horse=0.8 mule= 0.7 camel=1 beehive=0.001. See Jahnke (1982).

[13]Altitude may, in principle, also have effects on the disease environment, with tropical temperatures at lower altitudes likely resulting in a higher prevalence of diseases such as malaria. Notice how this may reverse the effect we predict, as poor health is generally associated with reduced risk tolerance (Akay et al., 2012). We do, however, not find a significant correlation between altitude and self-declared health state ($\rho = -0.014$, $p = 0.752$), likely because none of our subjects live at particularly low altitudes (for instance, malaria is virtually inexistent in the Ethiopian highlands). Since we did not find a direct effect of health on risk preferences either, we will not further mention this variable.

Table 3: Income and wealth

	I			II		
	α	β	σ	α	β	σ
land size	-0.015	-0.057***	0.003	-0.006	-0.054**	0.002
	(0.017)	(0.020)	(0.008)	(0.019)	(0.024)	(0.008)
altitude	0.046***	0.085***	-0.036***	0.040**	0.087***	-0.035***
	(0.015)	(0.024)	(0.005)	(0.017)	(0.028)	(0.006)
literate	0.012	0.060	0.005	-0.001	0.062	0.004
	(0.034)	(0.045)	(0.013)	(0.035)	(0.046)	(0.013)
middle school	0.048	0.116*	-0.001	0.035	0.117*	0.002
	(0.053)	(0.070)	(0.018)	(0.052)	(0.071)	(0.019)
business	-0.038	0.079	-0.038	0.064	0.038	-0.049
	(0.098)	(0.143)	(0.052)	(0.114)	(0.165)	(0.061)
female	-0.110	0.207**	0.029	-0.106	0.204**	0.028
	(0.067)	(0.089)	(0.028)	(0.071)	(0.091)	(0.028)
age	-0.006	0.050**	-0.014***	-0.009	0.047**	-0.015***
	(0.018)	(0.020)	(0.005)	(0.018)	(0.021)	(0.006)
unmarried	0.029	-0.193**	-0.013	0.001	-0.186**	-0.012
	(0.075)	(0.083)	(0.021)	(0.075)	(0.087)	(0.021)
	(0.058)	(0.069)	(0.018)	(0.059)	(0.071)	(0.018)
TLUs				-0.000	-0.005	-0.003
				(0.017)	(0.022)	(0.006)
wealth pc1				-0.017	-0.003	0.006
				(0.015)	(0.015)	(0.006)
Region dummies	✓	✓	✓	✓	✓	✓
constant	0.675***	0.637***	0.181***	0.689***	0.636***	0.178***
	(0.040)	(0.039)	(0.013)	(0.043)	(0.041)	(0.014)
Subjects	493	493	493	486	486	486
LL	$-12,335.17$	$-12,335.17$	$-12,335.17$	$-12,163.75$	$-12,163.75$	$-12,163.75$

Standard errors in parentheses; *p<0.1, **p<0.05, ***p<0.01
Continuous independent variables are entered as z-scores (land size, elevation, age, TLU)

indicators (Filmer and Pritchett, 2001), such as number of houses owned, number of rooms, whether the house has a water closet, materials of roof and wall, and whether the household has a private telephone. Adding these variables does not yield additional insights. Importantly, the effects of altitude (as well as land size) remain significant (this conclusion does not change if we add the single indicators of wealth instead of their principal component).

We also find some effect for the demographic controls. Most notably, we find unmarried subjects, which make up about 9% of the sample, to be less risk averse, and older and female subjects to be more risk averse. These effects correspond to the majority of results in the literature, although not all of them are uncontroversial. For instance, while gender effects have often been found (Croson and Gneezy, 2009), they may be sensitive to the elicitation task and decision context (Filippin and Crosetto, 2015), as well as socialization (Booth and

Nolen, 2012). Notice, however, that females in our sample are female household heads. Female-headed households are also likely to be poorer than male-headed households on average and particularly vulnerable, which may partially explain the strength of the gender effect we find (see also footnote 2).

4.2 Stability analysis

In this section, we replicate the main findings from above using non-parametric data. While non-parametric analysis will likely result in weakened results due to the noise incorporated in the measures, this is nevertheless useful in order to establish the stability of our main findings. We focus on the regression without wealth controls, since the latter was mostly important as a control for our altitude variable, which we consider of secondary importance given the between village rather than between household variation.

Table 4 shows the main regression from the previous section for the seven CEs plus the mean of the seven CEs taken at the individual level, using OLS with robust standard errors. The dependent variable is shown in the header, with the number in parentheses identifying the probability level for which the CE was obtained. The first column shows the regression using the mean response in all the CE tasks. This measure has the advantage that it may to some extent even out any idiosincratic noise occurring between responses. Land size still clearly shows the expected effect, with larger land holdings being correlated with larger certainty equivalents on average, and thus increased risk tolerance. Altitude shows a significant effect in the opposite direction, again as seen previously.

The subsequent regressions use the single CEs as dependent variables, in order of increasing probabilities of winning. The effect of land size can be seen to be significant for all except the largest three probabilities. A similar observation holds for altitude, which is significant for all but the largest two probabilities. The absence of significant effects for high probabilities may well be driven by a limitation of the choice list design employed. Given the high level of risk tolerance found on average, the choice lists indeed provide little discriminatory power once the probability of winning gets large and there is little space to express risk seeking behavior. Importantly, however, the effect of the income proxies shows up clearly for the 50-50 prospect. Offering equal probabilities of winning or not, the latter should have been comprehensible even for subjects who had troubles comprehending more extreme probabilities. It thus establishes the stability of our results.

Table 4: Nonparametric stability analysis

dep. variable:	CE(mean)	CE(0.05)	CE(0.1)	CE(0.3)	CE(0.5)	CE(0.7)	CE(0.9)	CE(0.95)
land size	0.891***	0.922*	1.629***	1.537***	1.085**	0.529	0.353	0.183
	(0.344)	(0.502)	(0.490)	(0.449)	(0.441)	(0.379)	(0.363)	(0.393)
altitude	-1.096***	-2.285***	-2.060***	-1.583***	-0.990**	-0.970**	-0.108	0.323
	(0.356)	(0.510)	(0.480)	(0.477)	(0.442)	(0.403)	(0.408)	(0.367)
literate	-1.093	-1.230	-2.217**	-1.852*	-1.622*	-0.865	-0.314	0.450
	(0.761)	(1.063)	(1.063)	(1.033)	(0.944)	(0.890)	(0.865)	(0.846)
middle school	-1.955*	-3.268**	-3.546**	-2.676*	-1.599	-1.893	-1.187	0.486
	(1.148)	(1.480)	(1.498)	(1.594)	(1.439)	(1.374)	(1.267)	(1.155)
business	-0.922	-0.882	-1.482	-1.455	-1.295	-1.414	1.958	-1.885
	(2.154)	(3.246)	(3.249)	(3.001)	(2.632)	(2.211)	(1.972)	(2.802)
female	-3.302**	-2.507	-2.640	-3.281	-3.810**	-2.913*	-4.804***	-3.162*
	(1.498)	(1.980)	(2.087)	(2.025)	(1.870)	(1.766)	(1.821)	(1.731)
age	-0.868**	-0.765	-1.377***	-0.782	-1.179**	-0.575	-0.735*	-0.664*
	(0.360)	(0.512)	(0.512)	(0.514)	(0.469)	(0.411)	(0.387)	(0.389)
unmarried	2.743*	2.464	2.495	4.081*	2.873	1.741	2.496	3.052**
	(1.472)	(2.344)	(2.332)	(2.158)	(1.755)	(1.700)	(1.746)	(1.279)
Region dummies	✓	✓	✓	✓	✓	✓	✓	✓
constant	24.266***	10.240***	15.436***	19.017***	25.183***	29.497***	33.554***	36.937***
	(0.725)	(1.013)	(1.090)	(1.003)	(0.921)	(0.837)	(0.990)	(0.668)
Subjects	493	493	493	493	493	493	493	493
R^2	0.059	0.089	0.081	0.067	0.049	0.035	0.041	0.058

Robust SEs in parentheses; *p<0.1, **p<0.05, ***p<0.01
Continuous independent variables are entered as z-scores

5 Discussion and conclusion

We have examined the risk preferences of rural Ethiopian households using certainty equivalents. The results expand and generalize recent findings according to which students in poor countries are on average more risk tolerant than students in rich, industrialized, countries. In particular, the finding of high levels of risk tolerance in one of the poorest countries in the world indicates that the differences found in the student comparison are not merely due to systematic selection effects of relatively richer students in poorer countries, but that this result indicates a more general phenomenon.

The negative correlation between risk tolerance and GDP found in the between country data contrasts markedly with the prevalent within-country evidence. Here we find a positive correlation of risk tolerance with income proxies, in agreement with a large (if not always consistent) body of evidence from industrialized countries (Dohmen et al., 2011; Donkers et al., 2001; Hopland et al., 2013). These opposing effects of national income between countries and personal income within countries gives thus rise to a risk-income paradox.

Most evidence on risk preferences in developing countries stems from studies using a single choice list (Binswanger, 1980; Yesuf and Bluffstone, 2009). Responses to such a list may, however, be contaminated by noise. One of the presumed virtues of the Binswanger list is that it does not allow for *any* noise to register in the response, given that subjects are asked to pick their favorite amongst a list of lotteries. This, however, makes it impossible to tease apart econometrically how much noise played into the response, and in general preference data and noise can thus not be separately identified. Andersson et al. (2015) showed how noise may systematically be counted towards risk aversion in some choice list designs, thus resulting in spurious correlations. This criticism particularly applies to the Binswanger design—given that the choice list is capped at risk neutrality, random choices will be systematically counted towards risk aversion.[14]

An additional methodological point is that certainty equivalents, so far rarely used in development economics but a standard tool in decision theory, hold great promise for the application with poor and often illiterate subjects. Comparing different sure amounts of money to a prospect with a constant probability is easy to explain and represent physically, and appears to produce good results. While the variation of probabilities may have created issues of comprehension in our

[14]The Binswanger list is a prime example of asymmetry in choice list design, since it is strongly skewed towards the detection of risk averse choices, but by no means the only one. The choice lists developed by Tanaka et al. (2010) share some of these features, with more choices counted towards (stronger) risk aversion than towards risk seeking. A complete review and assessment of these different tasks and their effect on choices is left for future research. A curious outlier in this respect is Akay et al. (2012), who found high levels of risk aversion eliciting CEs with poor farmers in Ethiopia. The latter finding appears to be driven mostly by subjects who consistently chose the sure amount for all choices. Since the design did not include the lower outcome of the prospect, however, it is hard to tell whether this behavior reflects true preferences, or whether it is driven by misunderstanding of the task.

subject population, such miscomprehension of probabilities is indeed inherent to risk preferences and commonly found also in developed countries (Tversky and Wakker, 1995; Barseghyan et al., 2013). We chose the variation in probabilities explicitly because more interesting effects are thought to emerge along the probability dimension than when varying outcomes (Prelec, 1998; Fehr-Duda and Epper, 2012). Nonetheless, it is still possible to focus on a few tasks offering a 50% probability if one should deem the probability variation undesirable.

The findings of considerable risk tolerance by our subjects raises the question what may be driving the reluctance to adopt new technologies that has often been observed in developing countries, and which has frequently been attributed to risk aversion. In the face of this evidence, such a conclusion does not appear to be tenable—at least not in any simple sense. One possible alternative explanation is that reluctance to switch to new technologies may be driven by downward risk exposure—the extend to which basic consumption needed for survival would suffer in the case of an adverse shock (Dercon and Christiaensen, 2011). Other explanations obviously exist as well, including low trust in the information provided by outsiders, slow information diffusion through social networks, etc. This is an important question raised by our data, the investigation of which will hopefully shed some fresh light on what induces people to take risks in real life decisions beyond their pure risk preferences as measured in economic experiments.

We conclude by pointing out some limitations of our method. We have concentrated on eliciting certainty equivalents for gain prospects only. They provided the cleanest test for our hypotheses, as one need not worry about giving subjects endowments from which losses are deducted as in pure loss or mixed prospects, and about whether subject integrate these endowments into their decisions or not. That said, an extension to mixed gain-loss tasks seems desirable when it comes to predicting behavior, since most real world decisions involve both gains and losses. Our method can indeed be easily extended to such a decision domain (Abdellaoui, Bleichrodt, and L'Haridon, 2008). Obviously, any measure of risk preferences has its peculiarities and may induce some sort of bias, and certainty equivalents are no different in this respect. More evidence on the relative predictive power of different measures of risk preferences is indeed highly desirable, but must be left for future research.

A Results using 1-parameter EU

In the present section, we estimate the same regressions as in the main text using an expected utility framework with a power utility formulation. This will leave our econometric apparatus above intact, except that our predicted certainty equivalent now takes the following form:

$$\hat{ce}_i = u^{-1}\left[p_i u(x) + (1-p_i)u(y)\right] = u^{-1}\left[p_i u(x)\right] \tag{6}$$

where utility takes the form $u(x) = x^\rho$ (alternative functional forms produce similar results). The fit of the resulting function to the nonparametric data is shown in figure 4. As already discussed above, this one-parameter function does not provide a good fit on average, as it cannot account for both risk seeking and risk aversion. Rather, it reflects the average pattern of risk seeking, resulting in a parameter estimate of $\rho = 1.634$ ($se = 0.065$), and thus a globally convex utility function.

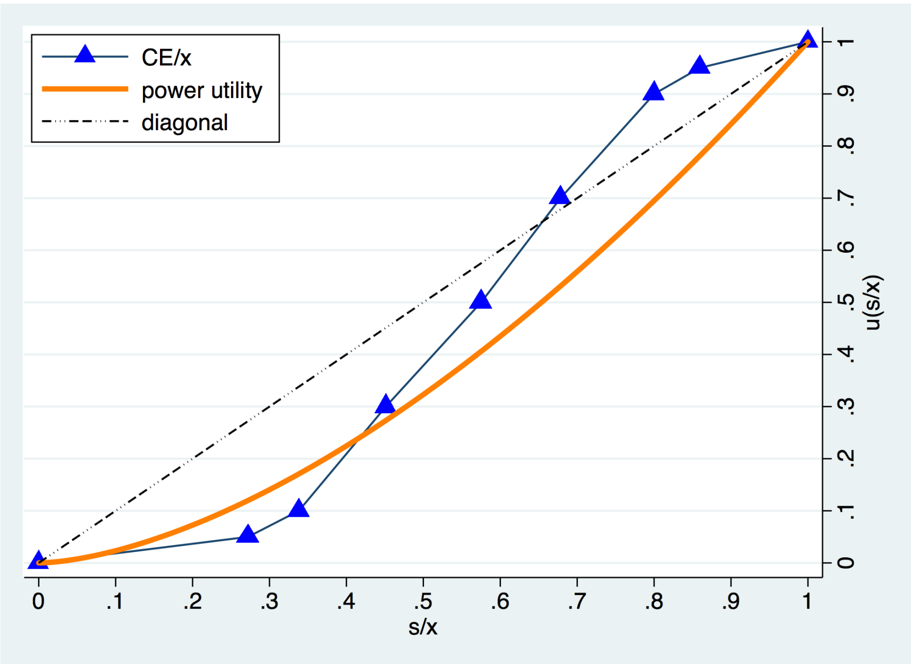


Figure 4: Fitting 2-parameter functions to the data

Table 5 shows the same four regressions as above using the expected utility formulation. Both land size and altitude have the expected significant effects in regression I.

Table 5: Stability analysis EU

	I	
	ρ	σ
land size	0.186***	0.007
	(0.066)	(0.009)
distance road	0.054	0.003
	(0.081)	(0.009)
altitude	-0.280***	-0.036***
	(0.076)	(0.007)
literate	-0.159	0.002
	(0.104)	(0.015)
middle school	-0.250	-0.005
	(0.222)	(0.020)
business	-0.366	-0.030
	(0.309)	(0.054)
female	-0.329*	0.040
	(0.188)	(0.029)
age	-0.179***	-0.012*
	(0.048)	(0.007)
unmarried	0.446**	-0.003
	(0.185)	(0.024)
region fixed effects	✓	✓
constant	1.830***	0.193***
	(0.117)	(0.015)
N_clust	493	
chi2	45.85	

Standard errors in parentheses; *p<0.1, **p<0.05, ***p<0.01
Continuous independent variables are entered as z-scores

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