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Hey and Orme go to Gara Godo: Household Risk Preferences

by

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Abstract

This paper explores the risk preferences of peasant household heads in the village of Gara Godo, Southern Ethiopia. Using experimental data, generalisations of expected utility are tested. For a number of respondents, the generalisations perform better than expected utility when assessed by formal statistical tests, but their predictive accuracy is little better than expected utility. As a result, expected utility is a justifiable simplification for modelling purposes. Respondents' degrees of risk aversion are then explored. No convincing evidence for risk living behaviour is found, with most households being risk averse. There is a significant minority, however, for whom risk neutrality cannot be discounted.

Outline

1. Introduction
2. Background
3. Experimental Method
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1. INTRODUCTION

Within semi-subsistence agriculture, peasant households face a multitude of risks; crop yield risk, crop price risk and the risk of personal injury to name but some. Diversification in the crops they grow and the economic activities they undertake can reduce the overall level of risk they have to bear, but it cannot eliminate it. Similarly, poorly developed or absent credit, insurance and forward markets make it difficult for households to pass their risks onto third parties. As a result, peasant households' economic decisions are overshadowed by risk. Their attitude towards this will be a key determinant of the balance of crops they choose to grow, and by implication of their income and consumption patterns.

Modeling a household's economic decisions under risk typically adopts a discounted expected utility approach; the two key parameters overarching these models being the household's intertemporal discount rate and its risk preferences. While expected utility has a number of attractions, notably its tractability, it is unusual to find an exploration of alternative theories of choice under risk. Furthermore, most studies take it as given that households are risk averse. Using experimental fieldwork data from a region of Southern Ethiopia, this paper seeks to identify the appropriate preference function for choice under risk, so testing the appropriateness of expected utility as a modeling strategy. Since the respondents for this study were already part of a consumption and production survey, this paper also seeks to form precise estimates of each household's risk preferences for use in later econometric studies. These estimates will address the question of whether households are risk averse.

The next section reviews work on household risk preferences, while the third explains the experimental method used by the present study. The theories of choice under risk which will be tested are then described, followed by an outline of the data analysis method. The performance of more general theories of choice under risk is then described, followed by an assessment of the performance of expected utility. Conclusions about the method and results are then drawn.

2. BACKGROUND

There are three basic ways in which household risk preferences can be identified; crop production data can be examined, households can be interviewed and experimental methods can be used. Estimates of risk preferences from crop production data typically assume that producers seek to maximize the expected utility of total income: wealth plus the profits from production [Pope (1982)]. Whether these adopt an expected return-variance functional form for utility [Lin, Dean and Moore (1974), Wiens (1976), Wolgin (1975)] or a more general functional form [Antle (1987), Saha, Shumway and Talpaz (1994)], they compare expected utility with risk neutrality/expected profit maximization.

These studies estimate a common production function by pooling individual producers' data, time series data being insufficient to estimate individual production functions. Individual production levels are evaluated using the common production function to form estimates of individual risk preferences, though Wolgin explicitly seeks to estimate only the distribution of risk preferences for his sample. These studies find that production levels are below the profit maximizing level, and the conclusions are that producers are risk averse to varying degrees. Slightly different, Moscardi and de Janvry (1977) adopt safety first considerations, but once subsistence levels have been assured, risk aversion again motivates cropping decisions.

Production based estimates of risk preferences must be viewed with caution for two reasons. Firstly, the limited number of observations for each household make precise estimation of individual risk preferences difficult. Secondly, they rely upon the results of Sandmo (1971) where crop price risk covaries positively with income, hence negatively with the marginal utility of income for the risk averse, so reducing optimal crop output. While this applies to the commercial farmer, the result must be qualified for the peasant household. As shown by Finkelshtain and Chalfant (1991), crop price risk not only affects marginal utility through its direct effect upon income, there is also an indirect consumption price effect due to the peasant household consuming some of the crop produced. This consumption price effect can reduce production, even among the risk neutral households. Not only are production based estimates of individual risk preferences likely to be imprecise, for peasant households they will also be biased, making them unsatisfactory for later econometric work.

The alternative to production based estimates are those based upon interviews and experiments, interviews being used by Dillon and Scandizzo (1978), while Binswanger (1980) used both. Again, both these studies view expected utility as the appropriate model of choice under risk, seeking a single statistic to summarize each household's risk preferences. Dillon and Scandizzo posed household heads hypothetical choices, respondents choosing either a farm with a steady annual income or a farm with a high income in three years out of four but a low income in one year out of four. By a certainty equivalent approach, the income levels were varied until respondents stated indifference. Accordingly, around two thirds appeared risk averse while a third were risk loving.

This method is, however, subject to a number of criticisms. The choices were hypothetical, with little incentive for respondents to think about which they would genuinely prefer. More importantly, the questions seem more a test of attitudes towards unstable incomes rather than risky incomes, so may have been more a test of access to credit than of risk preferences. This view is reinforced by more sharecroppers appearing to be risk loving than owner occupiers. If there is a clientele effect, the more risk averse would tend to become sharecroppers, since sharecropping enables risks to be shared with the landlord [Eswaran and Kotwal (1985), Stiglitz (1974)]. That sharecroppers were more willing to undertake the unstable income streams suggests a better access to credit, possibly through their landlord.

The appropriateness of interviews with no incentives to reveal true preferences was questioned by Binswanger who found little could be concluded from them, preferring instead to rely upon his experimental data. He invited household heads to work through a list of coin tossing gambles, gambles further down the list being riskier but also having a greater expected return. The gamble at which a respondent stopped was then usually played out, though choices involving high payoffs were hypothetical. Respondents were revisited over a number of weeks to give up to four data points for each respondent. The main result is that households were moderately risk averse, the degree of risk aversion increasing as the amounts played for increased.

As with Dillon and Scandizzo, some criticisms of the method can be raised. The hypothetical experiments gave respondents no incentive to reveal their true preferences, though if this was a serious problem it is curious that these were the ones which revealed a greater degree of risk aversion. More worrying is that there were long delays between experiments with payments being made in the interim. These may have imparted wealth effects upon subsequent experiments, and the economic situations of respondents might have changed. Consequently, the later household visits may not have been measuring risk preferences on the same basis as earlier experiments.

Lastly, it is not clear from the text, but if respondents were invited to work their way down the list, errors would tend to cause more to stop prior to reaching their true preference than they would tend to cause others to overshoot. If so, respondent errors would cause the method to over estimate respondents' risk aversion. Respondent errors also mean that Binswanger's limited number of questions for each respondent are not sufficient to form precise estimates of an individual's risk preferences. As with the production based estimates, the method gives a population distribution of preferences within which respondents can be expected to lie.

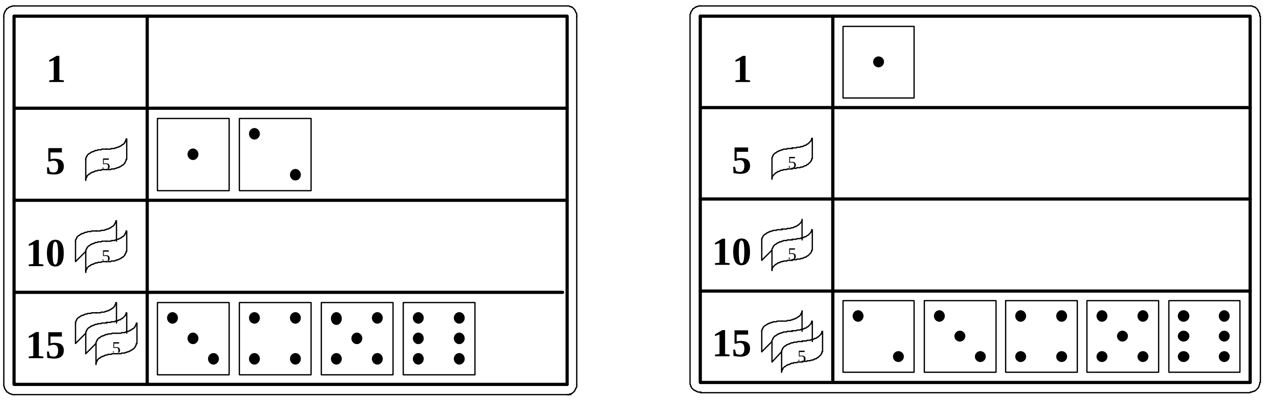
From the above, experiments which try to identify individuals' preference functions for choice under risk should limit their effects to current wealth, incentives being paid to motivate serious consideration of the choices involved. Experiments should occur within the same field visit with payments being made at the end, so that any anticipated wealth effect is common to all choices. Further visits can test the stability of risk preference estimates through time, but this should not be confused with the initial visit from which the estimates are made.

More basic concerns not addressed by these studies are whether expected utility is appropriate and whether respondents actually understand the experimental choices they are presented with. Peasant households are well acquainted with the risks of their everyday lives, but academic experiments are something of a novelty. Bearing in mind these points, experiments were developed which would yield sufficient data about each household for its risk preferences to be estimated independently from others within the sample.

3. EXPERIMENTAL METHOD

The fieldwork for this paper adapted a subset of the experiments performed by Hey and Orme (1994). Each of the sixty four respondents was presented with a series of 120 choices, each involving a choice between a low risk, low return gamble and a high risk, high return gamble. For the gambles a simple physical means of representing probabilities was required so the roll of a dice was used, giving probability gradations of one sixth. Each gamble had a maximum of four possible payoffs which, given budgetary considerations and in the light of a brief pilot study among ten household heads, were set at one, five, ten and fifteen birr (6 birr=US$1). Given the gambles as outlined in the appendix, these payoff levels gave a reasonable distribution of choices between the low risk gambles and the high risk gambles, enabling an efficient estimation of individual risk preferences.

In practice, respondents' winnings averaged between forty and fifty birr, roughly the cost of a sheep or a sixth of the cost of an ox. Since total lifetime assets rarely amounted to more than a couple of oxen and a few sheep for most people in the area, this was felt to be a significant incentive. It also compared favorably with the average annual gross household income of 330 birr. Each choice had its gambles printed on two cards, as shown below. For this example, a roll of a two would see a win of five birr were the left hand gamble chosen, a win of fifteen birr were the right hand gamble chosen.



As an incentive had to be given to motivate serious participation, randomly selecting one lottery from those chosen and playing it out naturally suggested itself. If respondents chose in accordance with expected utility, the random lottery incentive would cause them to view each choice as though it occurred in isolation. However, if respondents chose by some other criterion, the random lottery incentive might have led them to view the entire experiment as a single decision problem [Holt (1986)]. If applicable, this reduction hypothesis would greatly complicate data analysis since choices could not be treated as though they occurred in isolation.

Most experimental evidence does not support the reduction hypothesis in its strictest form, since if applicable the commonly observed common consequence effect should not occur [Starmer and Sugden (1991)]. Though the isolation hypothesis does not strictly apply, looser cross contamination between choices might still occur. Cubitt, Starmer and Sugden (1996) give the example of a choice where the option of a certain payoff may be quite attractive if viewed in isolation. This attractiveness may wane, however, if placed within the context of there being a 1/120 chance of it being selected by the random lottery incentive mechanism. Cubitt, Starmer and Sugden conducted a number of experiments specifically designed to test for such cross contamination effects, but found no systematic evidence for them. As a result, in practice the isolation hypothesis is likely to apply under the random lottery incentive, and data analysis can examine each choice without reference to the other choices within the experiment.

The fieldwork could not strictly adopt the random lottery incentive, however. In the pilot study respondents reported boredom after around thirty choices had been made. Consequently, after thirty choices had been made, the thirty chosen cards (gambles) were put into a hat and one drawn out at random. While the chosen card was not shown to the respondent at the time, this gave the respondent a break and remotivated concentration. Groups of thirty choices were then worked through in a similar manner until all 120 choices had been made, meaning that a total of four gambles were selected as an incentive mechanism for each respondent. The respondent knew that this would occur from the outset, paralleling Hey and Orme. As the four cards chosen were not shown until the end when they were played out, any anticipated wealth effect would be common to all choices.

But if respondents were fully rational this incentive mechanism would invalidate the isolation hypothesis, even for respondents choosing in accordance with expected utility. To see this, consider the extreme incentive mechanism of playing out all 120 gambles that were chosen after all choices had been made. A respondent would be virtually guaranteed of receiving the expected monetary value of his chosen gambles, good luck balancing bad over the 120 rolls of the dice. As a result, he would always choose the high risk, high return gambles regardless of his degree of risk aversion. While much diluted, this probability effect still applies when four gambles are randomly selected instead of one. However, questioning during the pilot study as to how respondents chose between the two gambles gave no indication that they recognized this probability effect. Indeed, trying to explain it to respondents after the experiments was extremely difficult. Consequently, it was felt that the isolation hypothesis was appropriate and that data analysis would be able to examine each choice in isolation.

For each respondent the method for the fieldwork was then as follows. After explaining the format of the gambles, a familiarization round posed thirty pairwise choices. It was explained that if the respondent was indifferent between the gambles in any particular choice, one would be chosen at random. The cards chosen in this round were then put into a hat and one drawn at random, which was then played out and money paid. It was hoped that this familiarization round would underline that money really was at stake, causing the respondent to view subsequent choices seriously. This familiarization round was purely to acquaint the respondent with the method, the data from it being discarded. The operation of the main body of fieldwork as outlined above was then explained and enacted, with the four randomly selected gambles being played out after all 120 choice had been made. Data upon household livestock, respondent age, sex and schooling was then collected.

4. PREFERENCE FUNCTIONS

With the payoff vector $\mathbf{x}$=(1,5,10,15), the two gambles of each pairwise choice can be represented by the probability vectors $\mathbf{p}=(p_1,p_5,p_{10},p_{15})$ and $\mathbf{q}=(q_1,q_5,q_{10},q_{15})$. For instance, the example of the previous section would see $\mathbf{p}$=(0,2/6,0,4/6) and $\mathbf{q}$=(1/6,0,0,4/6). Provided that the isolation hypothesis applies, in assessing each choice a respondent will have had some preference function V($\mathbf{p}$,$\mathbf{q}$) which compared the relative desirability of choosing $\mathbf{p}$ instead of $\mathbf{q}$; if V($\mathbf{p}$,$\mathbf{q}$) was positive $\mathbf{p}$ would have been preferred; if negative $\mathbf{q}$ would have been preferred. For the baseline case of expected utility (EU), gamble $\mathbf{p}$ has the expected utility;

$$\mathrm{EU}(\mathbf{p}) = \mathrm{p}_1\mathrm{U}(1) + \mathrm{p}_5\mathrm{U}(5) + \mathrm{p}_{10}\mathrm{U}(10) + \mathrm{p}_{15}\mathrm{U}(15) \quad \textbf{(1)}$$

with the preference function V(**p**,**q**) just being the difference in **p** and **q**'s expected utilities. Since Von-Neumann Morgenstern utility functions are invariant to a positive linear transformation, U(1) can be normalized to zero, U(15) to unity, which gives the preference function for expected utility as;

$$\mathrm{V}(\mathbf{p},\mathbf{q}) = (\mathrm{p}_5 - \mathrm{q}_5)\mathrm{U}(5) + (\mathrm{p}_{10} - \mathrm{q}_{10})\mathrm{U}(10) + (\mathrm{p}_{15} - \mathrm{q}_{15}) \quad \textbf{(2)}$$

where the parameters to be estimated for each respondent are U(5) and U(10).

A possible restriction to expected utility is that preferences display constant relative risk aversion (CRRA) which implies a smooth, monotonic utility function. Under CRRA $\mathrm{U}(x) = x^{\gamma}$, with γ being the parameter of interest, so imposing one restriction upon EU. Another possibility is the linear utility function of risk neutrality, which implies the additional restriction that $\gamma = 1$. If either CRRA or risk neutrality fits the data well this would be especially welcome for later econometric work, as they provide a simple statistic summarizing each households' attitude towards risk.

The descriptive appropriateness of expected utility has been questioned due to the observed frequency of choice in accordance with the various "paradoxes"; the Ellsberg paradox, the common ratio effect and the common consequence effect [Hey (1991)]. As a result, a large number of generalizations of expected utility have been proposed, these encompassing more behavior at the expense of prescriptive power. But many of these generalizations involve quite complex optimization processes.

In the context of the experiments, the gambles presented were entirely novel to respondents. Respondents also had little opportunity to learn from their mistakes, as the incentive gambles were played out after all choices had been made. Consequently, it is difficult to apply "as if" arguments, with respondents learning from their mistakes and developing rules of thumb which approximate to a complex optimization process. Rather, for theories of choice to be credible it must have been possible for respondents to have explicitly conceptualized the experiments along similar lines to the theory concerned.

Respondents would have had a strength of desire for the payoff levels, and could have modified these by the probabilities of winning them, thinking along the lines of expected utility. Likewise, a respondent might have behaved much as under expected utility, but have tended to focus unduly upon the probability of winning a particular outcome. As a result, weighted utility [Chew (1983)] needs to be examined.

Alternatively, ex post the respondent might have compared his actual win with what would have been won under the rejected alternative. If the former was higher, he would have felt elation, boosting the psychic value of his win. If lower, he would have experienced regret, dampening the value of his win. The layout of the gambles lends itself to the anticipation of elation and regret in the ex ante decision, so regret theory [Loomes and Sugden (1982)] needs to be examined.

Another generalization of expected utility which is credible in the context of the experiments is prospect theory [Kahneman and Tversky (1979)]. Under this a respondent behaves much as in expected utility, but tends to focus unduly upon improbable outcomes and not enough upon more likely outcomes. Unfortunately, in spite of its attractiveness in the context of the experiments, prospect theory is difficult to analyze. It requires that the respondent’s probability weighting function first be identified. This is then applied to the gambles, which are evaluated according to expected utility, only with the modified probabilities. Ad hoc assumptions would have to be made about each respondent’s probability weighting function prior to data analysis, in which little confidence could be placed. This identification problem makes prospect theory difficult to test with the experimental data generated.

Weighted utility (WU) assumes that when forming his expected utility, the respondent modifies the probability vector with the weights $\mathbf{w}=(w_1,w_5,w_{10},w_{15})$. This modified expected utility is then divided by the weighted sum of the probabilities to give the expected weighted utility;

$$\mathrm{EWU}(\mathbf{p}) = \frac{w_1 p_1 U(1) + w_5 p_5 U(5) + w_{10} p_{10} U(10) + w_{15} p_{15} U(15)}{w_1 p_1 + w_5 p_5 + w_{10} p_{10} + w_{15} p_{15}} \qquad \textbf{(3)}$$

The anticipated benefit of choosing **p** over **q** parallels expected utility, being the difference in their expected weighted utilities. As with expected utility, the normalizations U(1)=0 and U(15)=1 can be performed. With the additional normalizations w_1=w_{15}=1 [Chew, Karni and Safra (1986)], this leaves U(5), U(10), w_5 and w_{10} to be estimated. Weighted utility reduces to expected utility if the restrictions w_5=w_{10}=1 are valid.

Regret with independence (RI) evaluates both gambles simultaneously. The base utility of the outcome of the chosen gamble is modified by the regret or elation felt when it is compared it with the likely outcome of the rejected gamble. For instance, were **p** chosen with an outcome of ten birr resulting while under **q** an outcome of fifteen birr might have been anticipated, the modified utility would be U(10)+R(U(10)-U(15)), where R is the regret function. The corollary were **q** chosen is U(15)+R(U(15)-U(10)). Consequently, the anticipated net benefit of choosing **p** over **q** is;

$$\begin{aligned} \psi_{10,15} &\equiv \left(U(10) + R\left(U(10) - U(15)\right)\right) - \left(U(15) + R\left(U(15) - U(10)\right)\right) \\ &\equiv \xi_{10,15} + R(\xi_{10,15}) - R(-\xi_{10,15}) \end{aligned} \qquad \textbf{(4)}$$

where $\xi_{ij} = U(i) - U(j)$. The crucial point to note is that in choosing between **p** and **q** the respondent anticipates regret and elation in his ex ante decision. As (4) implies skew symmetry, in that $\psi_{ij} = -\psi_{ji}$, if the respondent views the gambles as being statistically independent his anticipated likelihood of ψ_{ij} can be defined as $z_{ij} = p_i q_j - p_j q_i$.

Noting that $\psi_{ii} = 0$, and normalizing $\psi_{15,1}$ to unity yields the preference function;

$$V(\mathbf{p}, \mathbf{q}) = z_{15,1} + z_{15,5}\psi_{15,5} + z_{15,10}\psi_{15,10} + z_{10,1}\psi_{10,1} + z_{10,5}\psi_{10,5} + z_{5,1}\psi_{5,1} \qquad \textbf{(5)}$$

where the parameters to be estimated are the ψ_{ij}s. Note that this reduces to expected utility with the three restrictions $\psi_{15,1} = \psi_{15,10} + \psi_{10,5} + \psi_{5,1}$, $\psi_{15,5} = \psi_{15,10} + \psi_{10,5}$ and $\psi_{10,1} = \psi_{10,5} + \psi_{5,1}$.

With regret aversion, i.e. a convex regret function, regret with independence predicts violations of expected utility in accordance with the common consequence and common ratio effects. But it requires that respondents view the gambles as being statistically independent, which is peculiar in the context of the experiments. For instance, on the cards of the previous section no roll of the dice juxtaposes a win of fifteen birr in one gamble with a win of one birr in the other, yet regret with independence would give $\psi_{15,1}$ a weight of $z_{15,1}$=4/6*1/6. For the peasant household choosing between two crops, the likelihood of either giving a very high yield may be remote. Yet if the chosen crop sees a bumper harvest, the likelihood is that the rejected crop would have as well, both being caused by good weather. Just as crop outputs are linked by the weather, in the experiments both gambles are explicitly linked through the throw of the dice. A six will usually see a high payoff under either gamble.

For this reason, regret with dependence (RD) will be explored, it taking the juxtaposition of payoffs under different rolls of the dice into account. The preference function is similar to that for regret with independence, the main difference being the computation of the z_{ij}s. Bearing in mind that $\psi_{ij} = -\psi_{ji}$, under regret with dependence z_{ij} is the number of rolls of the dice where payoff i in **p** is juxtaposed with payoff j in **q**, minus the number of rolls of the dice where payoff j in **p** is juxtaposed with payoff i in **q**.

For instance, in the cards of the previous section, rolls of a three, four, five and six can be ignored as they have the same payoff under either gamble and $\psi_{ii} = 0$. But if a one is rolled the left hand gamble pays five birr, the right hand only one birr, hence $z_{5,1}$=1. If a two is rolled, five birr is won under the left hand gamble, but fifteen birr is won under the right hand gamble, hence $z_{15,5}$=-1. Noting that in the experiments a win of fifteen birr in one gamble was never juxtaposed with a win of one birr in the other, the value function for regret with dependence takes the form;

$$V(\mathbf{p},\mathbf{q}) = z_{15,5}\psi_{15,5} + z_{15,10}\psi_{15,10} + z_{10,1}\psi_{10,1} + z_{10,5}\psi_{10,5} + z_{5,1}\psi_{5,1} \quad \textbf{(6)}$$

where the parameters to be estimated are the ψ_{ij}s. Again, a normalization can be performed. To ease comparison with regret with independence, for each respondent the free estimate of $\psi_{15,5}$ under regret with dependence can be scaled by the absolute value of its estimate under regret with independence, the absolute value preserving signs. Regret with dependence reduces to expected utility with the two restrictions $\psi_{15,5} = \psi_{15,10} + \psi_{10,5}$ and $\psi_{10,1} = \psi_{10,5} + \psi_{5,1}$.

5. DATA ANALYSIS

The experiments generated 120 data points for each respondent, this data being which of the gambles the respondent picked in each of the pairwise choices; taking the value 1 if the low risk, low return gamble was chosen, 0 if the high risk, high return gamble was chosen, no respondent ever expressing indifference. As the preference functions of the previous section are of strictly deterministic choice, if a respondent's choice was deterministic identifying his value function would not be difficult. But when choosing a respondent will have made errors. These may stem from a respondent making a series of small errors in the calculation of his preference function. In the context of the experiments, errors may also have arisen due to some respondents only having had a poor understanding of the choices involved. Indeed, some respondents reported having difficulty understanding the gambles and making the necessary choices. The poorer a respondent's understanding, the greater the likelihood of him making errors. Errors augment the preference function to give;

$$y^* = V(\mathbf{p},\mathbf{q}) + \varepsilon \quad \textbf{(7)}$$

where ε is a random variable about a zero mean. Choice is now stochastic, with **p** being chosen if y* is positive, **q** if y* is negative. Since the data is dichotomous, the natural means of examining this is logit analysis, with ε being assumed to have a logistic distribution about zero.

The key point is that each respondent's data will be analyzed separately, the analysis attempting to identify the appropriate parameterized value function for each respondent. Just as there can be no assumption that respondents had a common value function, so it cannot be assumed that respondents had the same likelihood of making errors. Some respondents will have understood well and made only small errors in their calculations. Other will have understood less well and have been prone to making larger errors in their calculations, so choosing poorly in accordance with their underlying value function. As a result, the analysis also has to form estimates of the size of each respondent's errors. The β of the respondent's logistic error distribution needs to be included in the parameter list for each model, it implying an error variance of $\sigma^2 = \beta^2\pi^2 / 3$. The smaller is β, the more closely the respondent chose in accordance with his preference function[1].

To assess the results for the different functional forms, three criteria arise. Firstly, are the restrictions accepted by χ^2 ? However, though weighted utility and the two forms of regret are generalizations of expected utility, they are not nested and formal χ^2 tests cannot distinguish between them. Instead, the Akaike Information Criterion [Amemiya (1980)] can be employed:

$$\mathrm{AIC} = -2 \ln \mathrm{L}(\alpha) / \mathrm{T} + 2\mathrm{k} / \mathrm{T} \qquad \textbf{(8)}$$

where $\mathrm{L}(\alpha)$ is the maximized value of the likelihood function for the estimated value function, k is the number of regressors and T the number of observations. The smaller the AIC, the better the model. The second criterion is whether more general functional forms perform noticeably better in terms of their predictive accuracy. If the difference in predictive accuracy is small, given expected utility's greater tractability for modeling purposes it may be a justifiable simplification.

[1] This formulation arises because of the normalization of U(15) and $\psi_{15,1}$ at unity in the previous section. An equivalent procedure would be to normalize errors to a variance of unity and permit free estimation of U(15) and $\psi_{15,1}$. High values for these would then correspond to a respondent who chose closely in accordance with expected utility/regret theory, the standardized error variance making it unlikely that he would choose contrary to that predicted by his preference function. In contrast a respondent with low values for U(15) or $\psi_{15,1}$ would be quite likely to choose contrary to his preference function, as the standardized error would be relatively more important.

Lastly, do the parameters for the estimated functional forms make economic sense? This is intimately related to whether at least one of the applied preference functions is a reasonable approximation of a respondent's true preferences, and whether respondents actually understood the experiments. There would be little point using estimates of risk preferences in later econometric work if true preferences were wildly different from those estimated, or if respondents had little understanding of the experimental choices they were making. Any method attempting to restrict the sample to those whose true preferences have been reasonably accurately estimated will, of necessity, be ad hoc.

Putting aside the possibility of respondents having chosen in accordance with a theory not estimated, such as prospect theory, respondents' understanding and accuracy of calculation will be closely related to the estimates of their errors; the β s. A respondent who had a good understanding and was accurate in his calculations will have small errors and have sensible parameter values. The more deterministic a respondent's choice, the greater the confidence inspired in the estimate of his preference function. As expected utility is the baseline case, the natural candidate for assessing a respondent's determinism is the estimate of his β under this model; β_{EU}. Progressively restricting the sample of respondents by their β_{EU} s might be anticipated to strip out those whose choice was largely stochastic.

But a problem arises with this. A respondent might have a high β_{EU} not because he had a poor understanding, but because he chose in accordance with, say, weighed utility. As a result, expected utility would have a low log likelihood while weighted utility's would be somewhat higher. Consequently, restricting the sample by β_{EU} would remove some for whom weighted utility was genuinely a good fit, the sample average superiority of weighted utility relative to expected utility falling away.

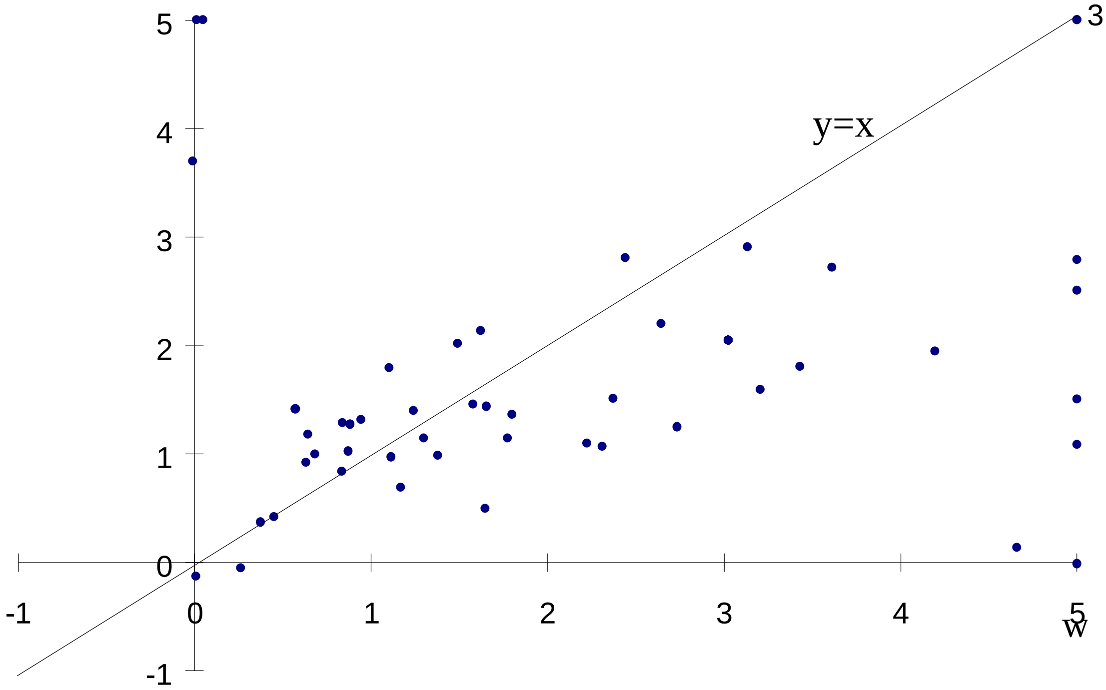
What if the sample restriction has the desired effect, stripping out those who had a poor understanding? For these respondents both weighted utility and expected utility will fit the data poorly. The sample average superiority of weighted utility relative to expected utility should not fall as the sample is restricted, and may even rise.

For this example, the criterion for assessing the sample restriction is the change in weighted utility's superiority relative to expected utility for the remaining sample. In effect, to be able to have confidence in the weighted utility estimates for those with a very low log likelihood under expected utility, it is not sufficient that weighted utility be superior by χ^2. For these respondents, the superiority of weighted utility must be similar to that of those with rather higher log likelihoods. If this is not the case for those excluded, and they also have peculiar parameter values, the parameter estimates under weighted utility would not inspire confidence. It would be evidence of the sample restriction having the desired effect: excluding those with a large stochastic element in their choice.

6. GENERALIZATIONS OF EXPECTED UTILITY

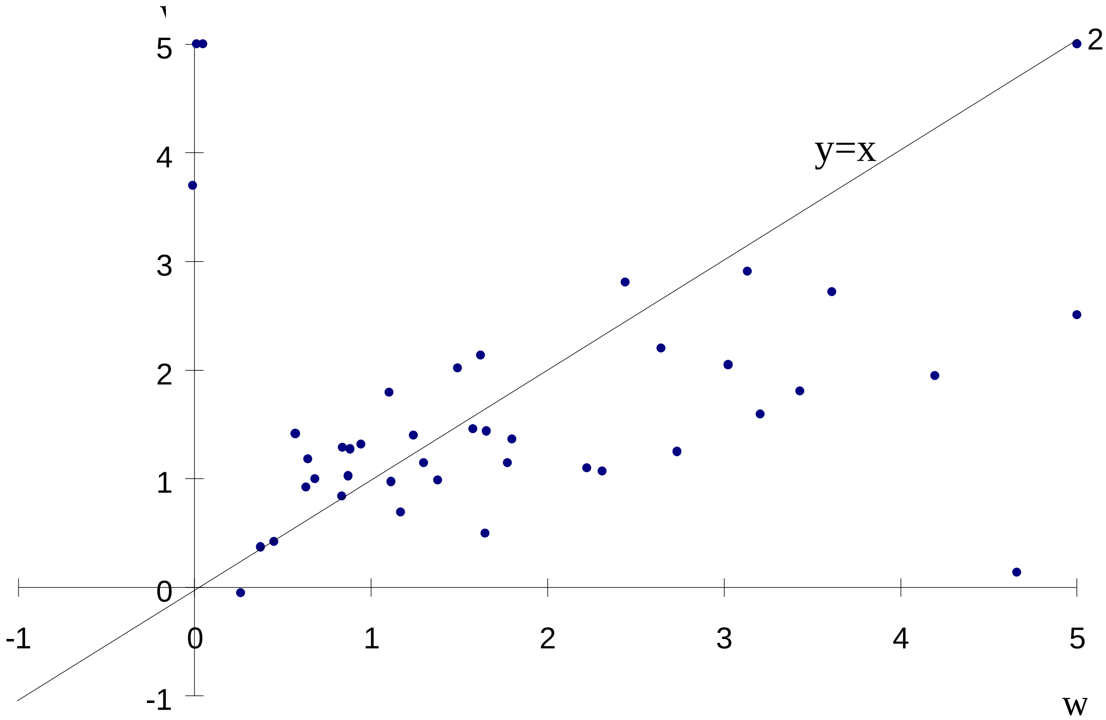
An initial assessment of the generalizations of expected utility by the AIC shows weighted utility to be best for thirty eight respondents, regret with independence to be best for sixteen and regret with dependence to be best for ten. However, as noted in the previous section, performance under the AIC is not sufficient to inspire confidence in the estimated functional forms. The economic meaningfulness of parameters and predictive accuracy also need to be examined.

The upper scatter plot on page 16 shows the estimates of the weight w_5 and w_{10} under weighted utility for each respondent. Each point represents the parameter estimates for one respondent. The additional scatter plots on pages 16 and 17 show how the sample changes as it is restricted by β_{EU}. For the graph of all respondents, there are a number of respondents grouped around the vertical axis who apparently ignored the possibility of winning ten birr. Likewise, those grouped around the horizontal axis seemed to ignore the possibility of winning five birr, while those grouped around the origin apparently paid very little attention to both the possibility of winning five birr and the possibility of winning ten birr. In a similar vein, points in the extreme upper right relate to respondents who primarily concentrated upon the possibilities of winning five and ten birr, ignoring one and fifteen birr. These parameter values are of concern.

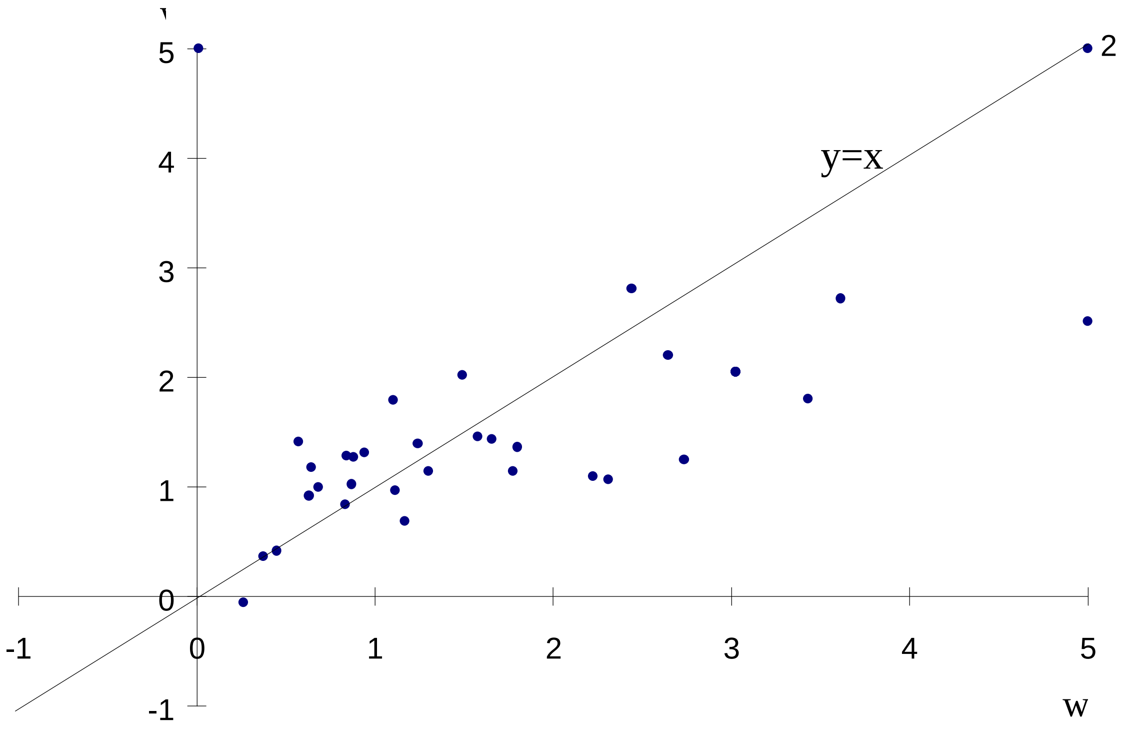


Weighted Utility

$\beta_{EU} < 0.15$

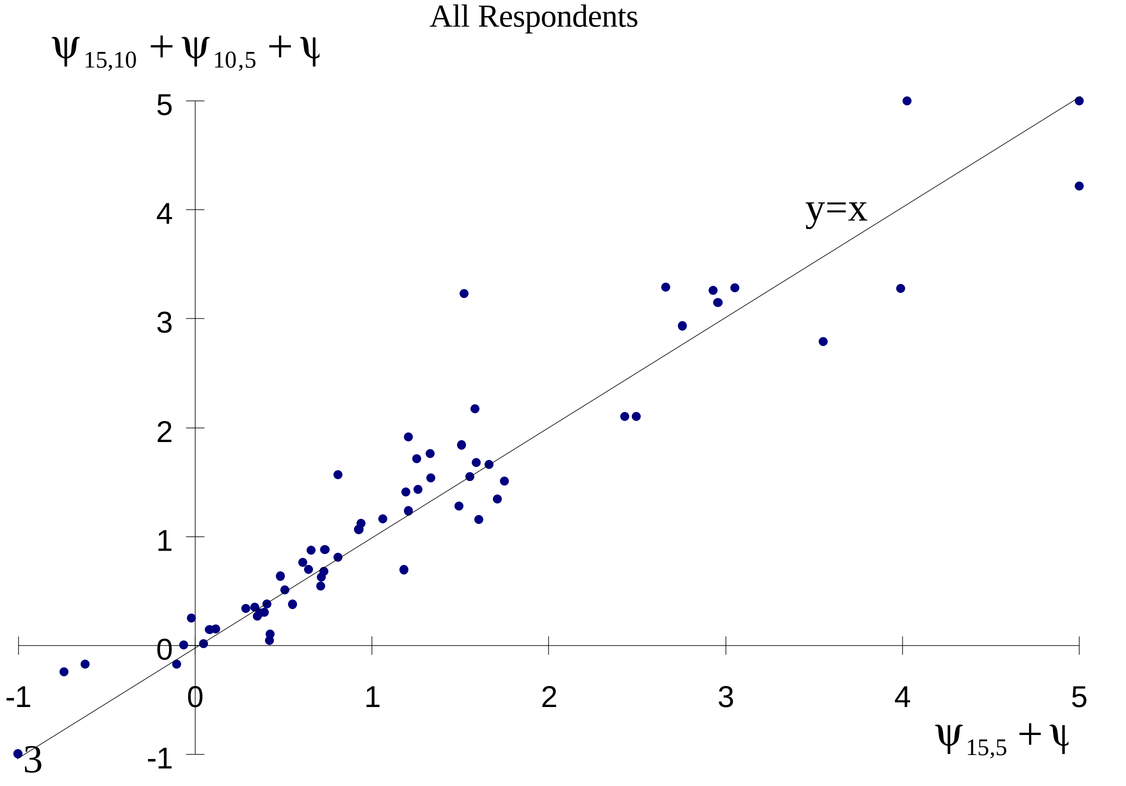


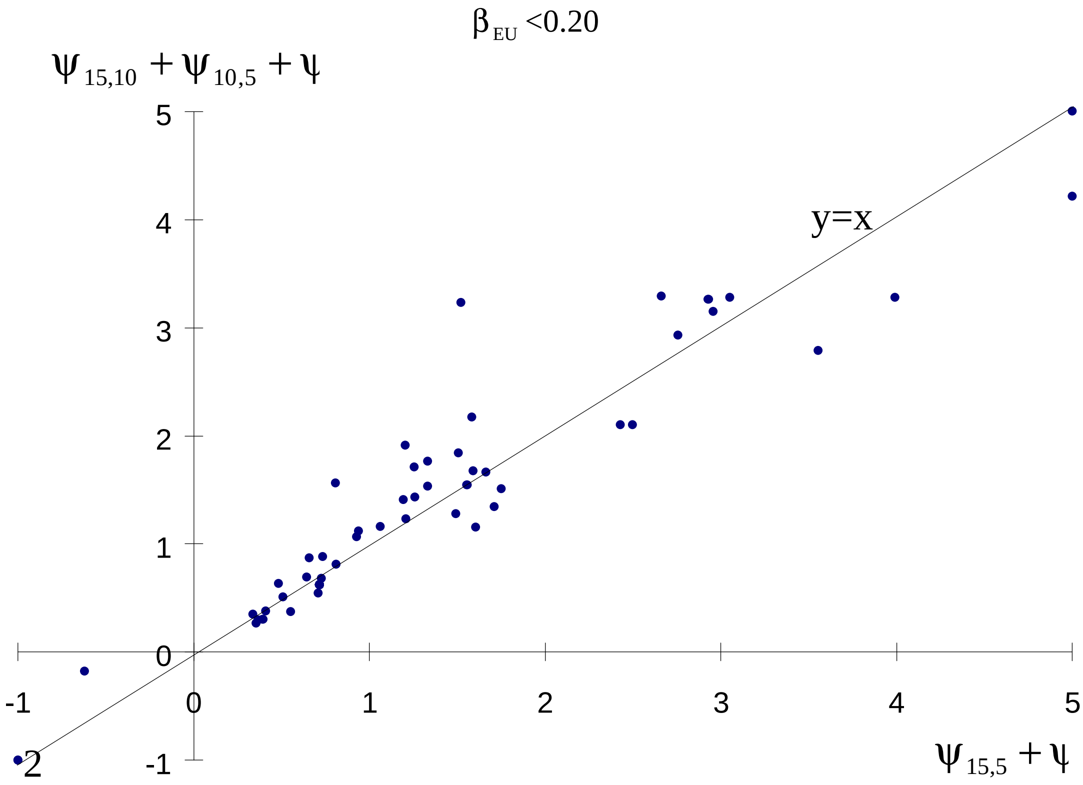
$\beta_{EU} < 0.10$



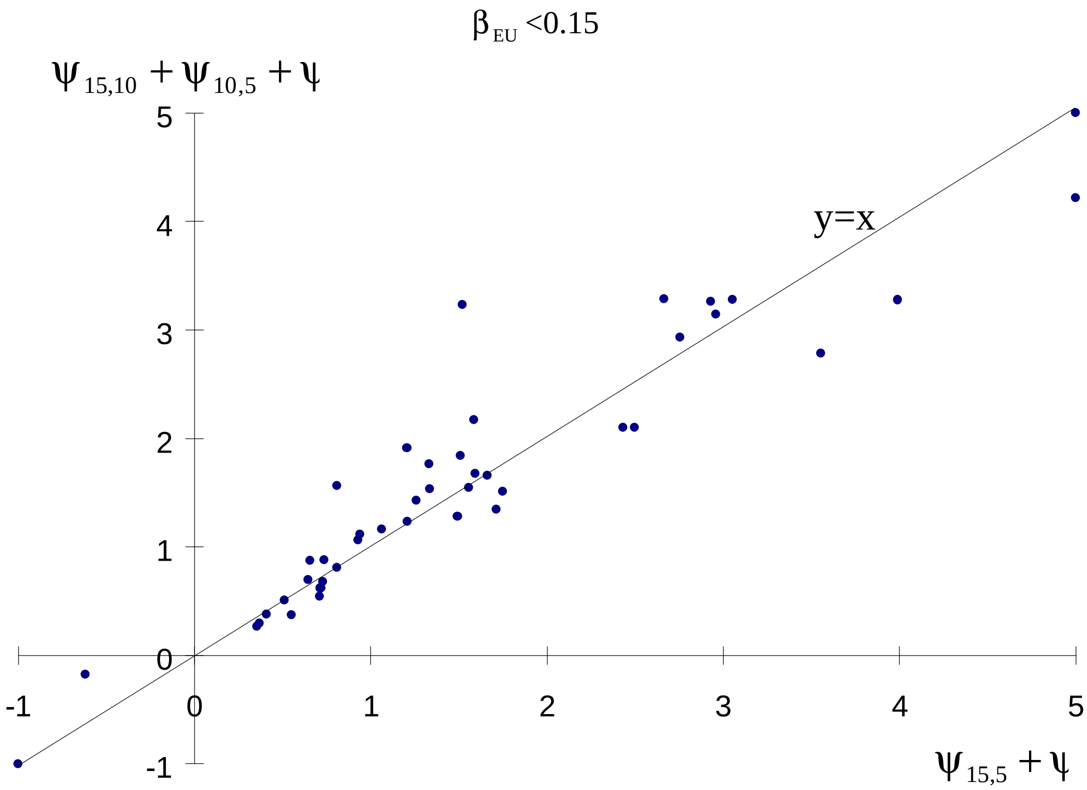
Regret with Independence

All Respondents

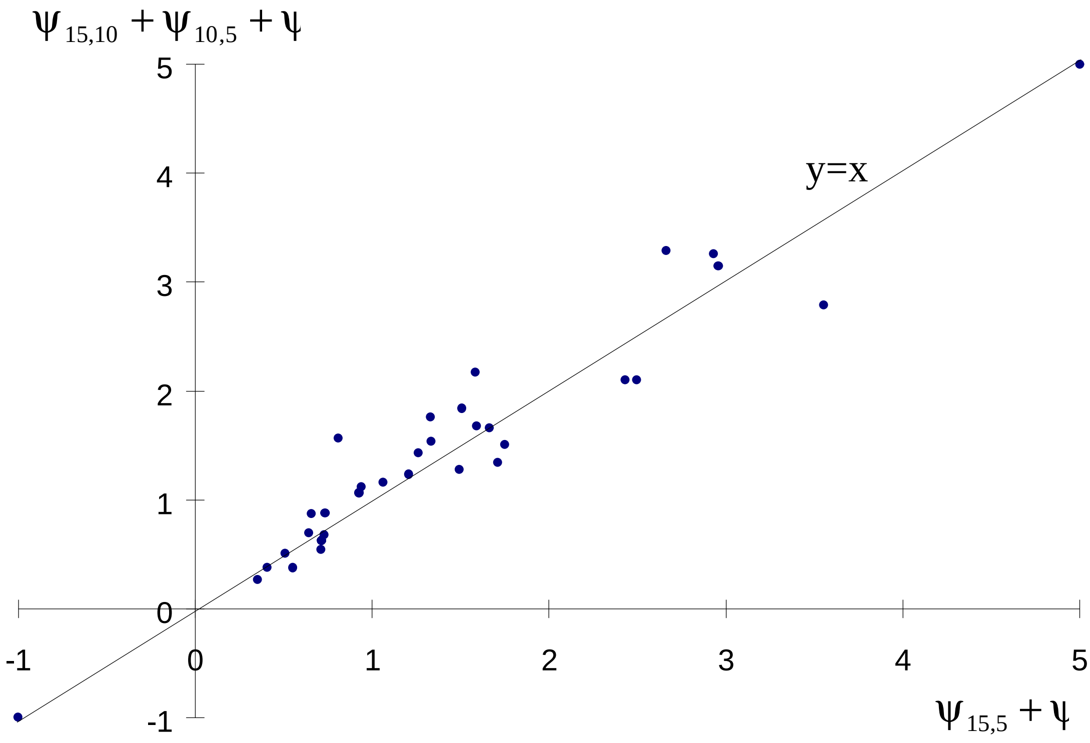




<u>Regret with Independence</u>

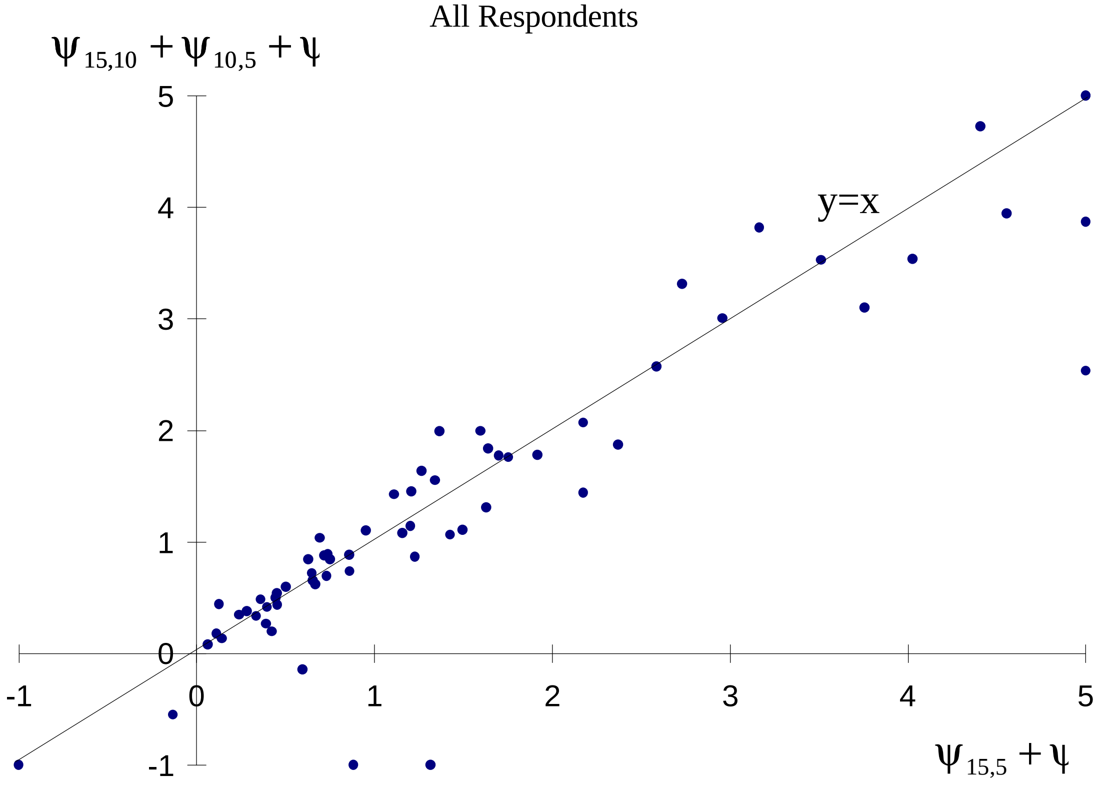


β_{EU} <0.10

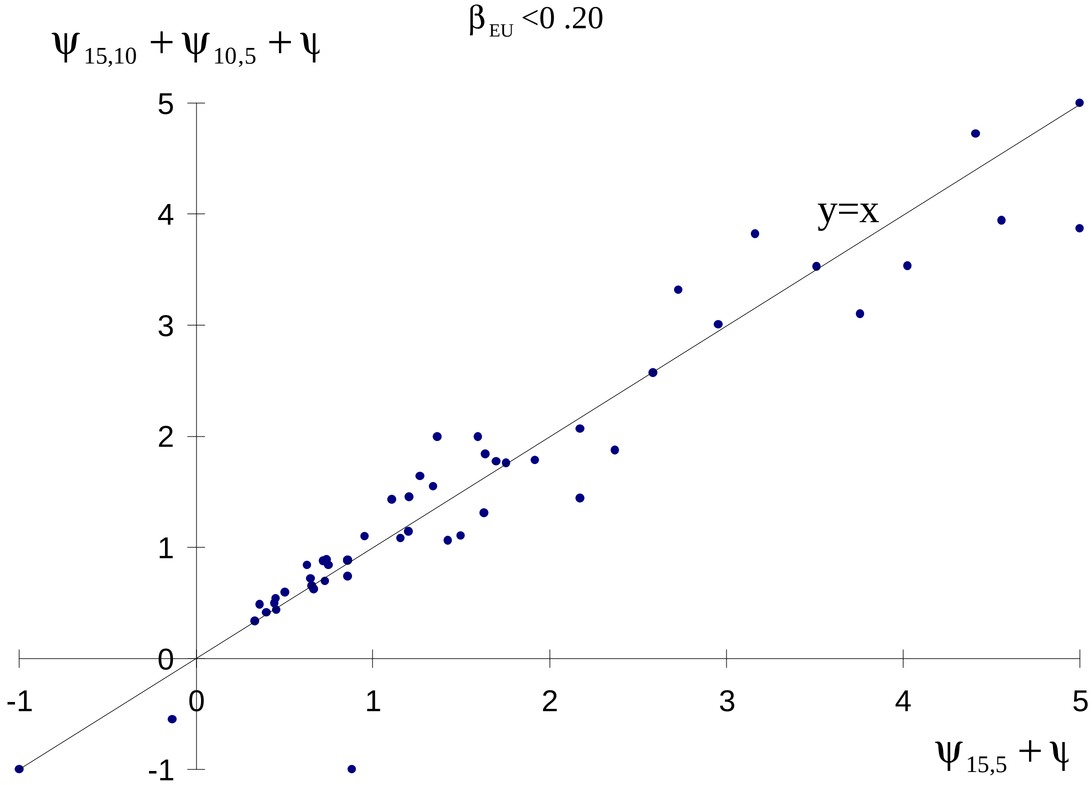


<u>Regret With Dependence</u>

All Respondents



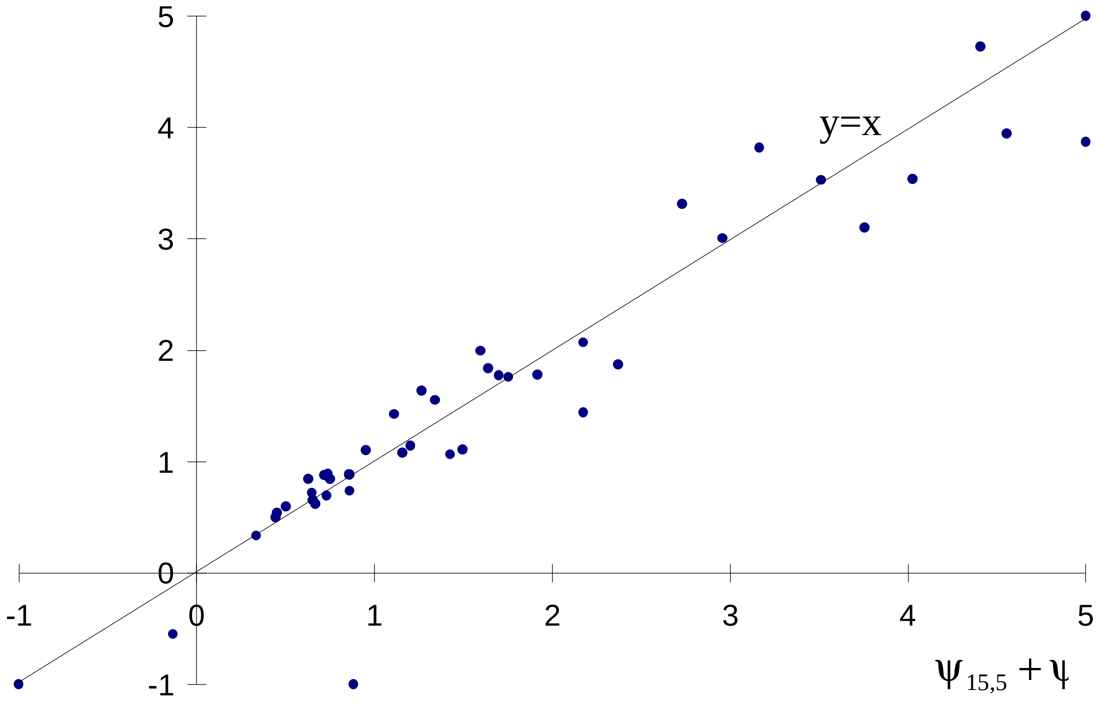
β_{EU} <0 .20



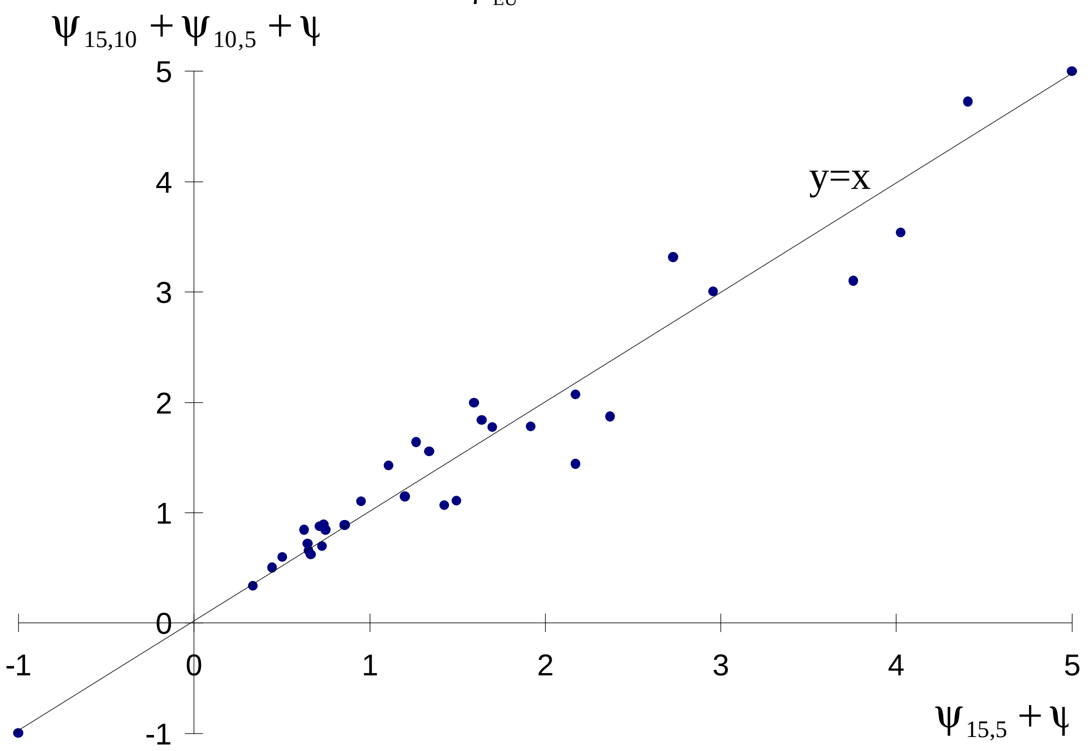
Regret with Dependence

β_{EU} <0.15

$\psi_{15,10} + \psi_{10,5} + \psi$



β_{EU} <0.10



As the sample is restricted by β_{EU}, respondents with these worrying parameter values are gradually excluded. Respondents along the axes are thinned out, and the cluster of points around the origin virtually eliminated. The long tail out to the upper right also thins, while the central core of respondents with reasonable weights is not markedly affected. In terms of the economic meaningfulness of parameter estimates, the sample restriction is having the desired effect. Table 1 shows its effect upon the average superiority of weighted utility relative to expected utility.

Table 1	Weighted Utility		Regret (independence)		Regret (dependence)	
	Average Ll_{WU}-Ll_{EU}	Pred. Acc. vs EU	Average Ll_{RI}-Ll_{EU}	Pred. Acc. vs EU	Average Ll_{RD}-Ll_{EU}	Pred. Acc. vs EU
All Resp.	0.042	+1.7%	0.035	+2.8%	0.017	+1.2%
β_{EU} <0.20	0.037	+2.3%	0.035	+2.3%	0.018	+1.1%
β_{EU} <0.15	0.036	+1.8%	0.036	+2.1%	0.018	+0.9%
β_{EU} <0.10	0.035	+1.7%	0.037	+1.8%	0.015	+0.8%

Restricting the sample sees weighted utility's average superiority falls away. At least some of those excluded due to having a high β_{EU}, and hence a low log likelihood under expected utility, have a noticeably higher log likelihood under weighted utility when compared with their peers. Though the restriction removes those with peculiar parameter values, it also excludes some for whom weighted utility is a reasonable model for choice under risk. However, the average superiority of weighted utility in terms of predictive accuracy is small.

Due to the number of parameters estimated for regret with independence, many graphical representations are possible, the most useful being to plot $\psi_{15,5} + \psi_{5,1}$ against $\psi_{15,10} + \psi_{10,5} + \psi_{5,1}$ for the following reasons. For points which lie above the y=x line, $\psi_{15,5} < \psi_{15,10} + \psi_{10,5}$, which supports regret aversion, though separating this from the possible effects of risk aversion in the base levels of utility is difficult. The closer points are to the y=x line, the less important anticipated regret is in decisions involving payoffs at the top end of the scale. Similarly, recalling that $\psi_{15,1}$ has been normalized at unity, the inclusion of $\psi_{5,1}$ permits a comparison of both $\psi_{15,1}$ with $y_{15,5}+y_{5,1}$, and $y_{15,1}$ with $y_{15,10}+y_{10,5}+y_{5,1}$. Points to the right of 1 in the horizontal imply that $y_{15,1}<y_{15,5}+y_{5,1}$, suggesting that anticipated regret is important in

decisions involving payoffs at the bottom end of the scale, giving some support to regret aversion. Likewise, points above 1 in the vertical imply that $\psi_{15,1} < \psi_{15,10} + \psi_{10,5} + \psi_{5,1}$, again giving some support to regret aversion.

As shown by the scatterplots on pages 18 and 19, most respondents are clustered around the y=x line, suggesting that anticipated regret was not important for gambles involving payoffs at the top end of the scale. The long tail of points out to the right supports regret aversion, anticipated regret having been important for gambles involving payoffs at the bottom end of the scale. Points may be too spread out along the y=x line, however. Points beyond two or three suggest that anticipated regret was more important than the payoffs' base levels of utility. Likewise, negative points are barred by theory, while points around the origin imply no net benefit was felt going from a win of one birr to a win of fifteen.

As the sample is restricted by b_{EU} all but one of the negative points are excluded, as is the cluster of points around the origin. The long tail of respondents thins, though this is at the expense of a slight thinning of the core distribution of respondents with reasonable parameter values. The average superiority of regret with independence relative to expected utility actually rises with the sample restriction, which strongly suggests that it is having the desired effect. Little faith could be placed in the parameter values of those excluded, who did not in any case have markedly higher log likelihoods under regret with independence. The last point to note is that, as for weighted utility, the predictive accuracy of regret with independence is not noticeably better than that of expected utility.

For regret with dependence, the scatterplots of $y_{15,5} + y_{5,1}$ against $y_{15,10} + y_{10,5} + y_{5,1}$ on pages 20 and 21 show a similar pattern to regret with independence. There is a core of points around the y=x line with reasonable parameter values, though there is still a tail of points to the upper right and a few negative values. The sample restriction manages to eliminate most of the negative values and reduces the upper right hand tail, but this is at the expense of a thinning out of the core of the distribution. As shown in Table 1, the sample restriction does not initially exclude those for whom regret with dependence has a noticeably higher log likelihood in comparison with their peers. The last sample restriction seems to be a step too far, however, mostly thinning out the core of respondents with reasonable parameter values. The generalization of regret with dependence is barely superior to expected utility, in terms of predictive accuracy.

7. EXPECTED UTILITY

Turning to expected utility itself, the main points are summarized in the Table 2. The third, fourth and fifth columns show the percentage of respondents for whom expected utility was an acceptable restriction of weighted utility, regret with independence and regret with dependence by χ^2 at the 5% level. The fifth column shows the number of respondents with monotonically increasing utility functions under expected utility, while the last shows the average predictive accuracy of expected utility.

Table 2	No. Resp.	EU > WU	EU > RI	EU > RD	Monotonic	Pred. Acc.
All Resp.	63	46%	65%	79%	56	71%
β_{EU} <0.20	50	50%	66%	80%	48	73%
β_{EU} <0.15	43	53%	67%	79%	41	75%
β_{EU} <0.10	33	54%	64%	82%	32	77%

The first point to note is that the proportions for whom expected utility is an acceptable restriction of regret with independence and regret with dependence remain roughly constant with the sample restriction. Only the proportion for whom expected utility is an acceptable restriction of weighted utility rises slightly as the sample is restricted, though many of the excluded respondents had questionable parameter values under weighted utility. Restricting the sample by β_{EU} does not noticeably bias the sample towards expected utility. The sample restriction also quickly excludes most of those with apparently non-monotonic utility functions, who are of concern. The predictive accuracy of expected utility necessarily rises as the sample is restricted, but it is clear that in proportionate terms there is virtually no predictive payoff from using any of the generalizations of expected utility. As a result, in spite of the formal χ^2 tests, expected utility seems a justifiable simplification for modeling purposes.

To be able to compare respondents' risk preferences requires a single summary statistic, the obvious candidate being the parameter estimates under CRRA. If an appropriate simplification, this would be useful for later econometric work on households' production and consumption data. For comparability with earlier studies, Table 3 describes the distribution of the parameter estimates under CRRA among all respondents in the sample. Figures in parenthesis are the numbers of respondents for whom CRRA is an acceptable restriction of expected utility by χ^2.

Table 3 — No. Resp. with CRRA γ in the ranges:

	0.0-0.2	0.2-0.4	0.4-0.6	0.6-0.8	0.8-1.0	1.0+	Total
All Resp.	11 (10)	19 (11)	11 (6)	7 (5)	8 (5)	7 (6)	63 (43)
β_{EU} <0.20	10 (9)	17 (9)	11 (6)	4 (2)	6 (5)	2 (1)	50 (32)
β_{EU} <0.15	10 (9)	15 (8)	9 (5)	2 (1)	6 (5)	1 (0)	43 (28)
β_{EU} <0.10	6 (5)	11 (6)	9 (5)	1 (0)	6 (5)	0 (0)	33 (21)

Most striking is that as the sample is restricted, the number of apparently risk loving respondents rapidly falls away. There are two possible explanations for this. Firstly, it could be due to the probability effect of the incentive mechanism, as outlined in the third section. The incentive mechanism could have invalidated the isolation hypothesis, causing some respondents to have viewed the experiments as a single decision problem. Logit analysis relying upon the isolation hypothesis would fit the data poorly for these respondents, giving them a high value for β_{EU}. The probability effect would also have caused these respondents to choose riskier gambles, hence a high value for β_{EU} would tend to coincide with a γ estimate which was above the respondent's true preferences. Restricting the sample by β_{EU} would be expected to shift the weight of the distribution towards lower values of γ. This could account for the rapid fall off in apparently risk loving respondents.

But as the sample is restricted, most respondents remain within the 0.2 to 0.6 range In this range the weight actually shifts into the 0.4 to 0.6 range. Furthermore, the number of respondents at or near risk neutrality shows notable persistence. There is no overall shift to the left in the distribution of γ as the sample is restricted, and the falling number of apparently risk loving respondents does not seem to be due to the incentive mechanism having had a probability effect.

The simpler explanation seems preferable, in that the sample restriction cuts out those who had a poor understanding of the experiments. No convincing evidence of risk loving behavior has been found, most respondents being risk averse, some quite strongly so. Yet there remains a significant minority for whom risk neutrality cannot be discounted. Indeed risk neutrality is accepted for six of those for whom CRRA is an acceptable restriction of expected utility.

Turning to determinants of household risk preferences, bivariate correlation coefficients were calculated between γ and the data collected on respondent age and schooling, household livestock and the number of children. Age and schooling showed no significant relationship with γ. Among all respondents that strongest relationship was with household oxen, this being the main determinant of household livestock wealth. But, as shown by Table 4, as the sample was restricted the significance of the correlation with oxen dropped away. Again, figures in parenthesis relate to households for whom CRRA was an acceptable restriction of expected utility.

Table 4	All Respondents		β_{EU}<0.20		β_{EU}<0.15		β_{EU}<0.10	
	Oxen	Child	Oxen	Child	Oxen	Child	Oxen	Child
γ	0.33*	-0.07	0.33*	0.11	-0.08	-0.25*	-0.08	-0.49*
	(0.37*)	(-0.11)	(0.39*)	(-0.15)	(0.05)	(-0.45*)	(0.04)	(-0.52*)
Oxen		0.01		-0.03		0.07		0.09
		(-0.09)		(-0.14)		(-0.05)		(-0.08)

The signs on the correlation coefficients are as would be expected. The wealthier a household was, the more likely it was that choice would tend towards the high risk, high return gambles. Risk aversion appears to fall with wealth. Some skepticism must be applied to this, however, since the sample includes those with considerable noise in their risk preference estimates.

Among those whose risk preferences were estimated most accurately, children caused a respondent to choose less risky gambles. This may have been due to children causing indivisibilities in consumption, requiring money for food, clothing and schooling, the respondent seeking to guarantee these. This is of particular interest since only five respondents were women, only three women and one woman being present in the last two restricted samples. Male household heads' choices were in part influenced by their number of children, giving indirect support to the unitary model of household behavior.

Lastly, the sample restriction needs to be assessed. While not related to any livestock data, β_{EU} had significant bivariate correlation coefficients with respondents' years of schooling (-0.28) and respondents' age (0.49). Though the correlation between schooling and age (-0.44), makes it difficult to separate their effects, the signs of the correlations are as would be anticipated if β_{EU} was related to respondents' understanding of the experiments. The sample restriction achieves reasonable success in excluding peculiar parameter values from the generalizations of expected utility, only slightly biasing the sample away from weighted utility towards expected utility. However, it does not bias the sample towards any particular wealth grouping. The sample restriction also rapidly excludes those with non-monotonic utility functions and the apparently risk loving. There would be no reason to anticipate these effects were a high β_{EU} due to respondents choosing in accordance with a preference function which has not been estimated, such as prospect theory. For these reasons, particularly in the light of the correlations, the sample restriction appears to be having reasonable, if imperfect, success: excluding those who had a poor understanding of the experiments and largely stochastic choice.

8. CONCLUSIONS

In devising experiments to form estimates of household risk preferences a balance has to be struck between complexity, which permits precise estimates, and simplicity, which eases respondent understanding. The experiments of this study were quite complex, and some respondents had difficulty with them. If estimates of all respondents' risk preferences are required for later econometric studies, simpler experiments should be included as an adjunct. These would give some, albeit imprecise, estimates of risk preferences for those who have difficulty with the main experiments.

The complexity of the experiments also demanded that respondents' understanding be examined, the natural candidate for this being the estimates of a respondent's errors under expected utility. While imperfect, this excluded many of the older, less educated respondents with peculiar parameter values. It seems reasonable to assume that these were the respondents who had most difficulty understanding the experiments. Excluding these respondents did not significantly bias the sample towards expected utility or to any particular wealth grouping.

Generalizations of expected utility were superior by formal statistical tests for some respondents, but these respondents often had parameter values of questionable economic meaningfulness. It terms of economic modeling, expected utility is a justifiable simplification on grounds of its predictive accuracy. No convincing evidence of risk loving behavior was found, most respondents being risk averse to varying degrees. However, a significant minority were risk neutral and modeling must take account of this. Risk aversion was declining in wealth, but increased with the number of children in the household.

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Ken Ingersent - agricultural trade, economic development
Tim Lloyd - agricultural markets, econometric modelling
Andrew McKay - poverty, peasant households
Chris Milner - trade and development
Wyn Morgan - futures markets, commodity markets
Christophe Muller – microeconometrics, households, health and nutrition
Tony Rayner - agricultural policy and trade
Geoff Reed - international trade, commodity markets

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Scott McDonald *(Sheffield University)* – CGE modelling
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