ESTIMATING UTILITY FUNCTIONS

Glenn J. Knowles

In reading over the report given to the regional project in last year's meeting by Young, Lin, Pope, Robison, and Selley (1979) I was disturbed by some of the observations made in that report. In particular, when discussing methods for measuring risk preferences, I feel that what the authors called the direct elicitation method was given an unfair hearing vis-a-vis the experimental method. I feel that the experimental method has limited merit and I will point out a number of flaws with this method. On the other hand, I do not wish to state that the direct elicitation method is without errors, especially as it has been conventionally applied. Since I am reluctant to write a defense or an obituary of the methods used to measure risk preferences by estimating utility functions, I will do a little bit of both. The majority of the paper will scrutinize and criticize past and current efforts to measure risk preferences. However, I conclude the paper with what I consider to be some viable and promising alternatives.

One of the major problems with studies done within the agricultural economics profession has been the failure of researchers to examine studies that have been done outside their own discipline. When studies by other economists, statisticians, and psychologists have been mentioned it is usually done only in passing. This has been very unfortunate, since, in my view, much of the work from which valuable insights can be examined has been done outside the agricultural economics profession. With regard to experimental studies with real wagers, two of the better studies were done by Mosteller and Nogee (1951) and Davidson, Suppes, and Siegel (1957). The Mosteller and Nogee study was one of the first to use the Von Neumann-Morgenstern method, while the Davidson, Suppes, and Siegel study was one of the first to use the Ramsey method.

EXPERIMENTAL STUDIES WITH REAL WAGERS

The procedure used by Mosteller and Nogee was to present the subject with a poker dice hand which they would have to beat in order to win. The certainty amount was refusing the bet and the price for accepting the bet was always the same, 5¢. The amount that could be won from each hand was varied and the only choices for the subject were to refuse or

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accept the bet. Since an indifference amount was not directly elicited an operational definition of indifference was used. Mosteller and Nogee describe their procedure for calculating the indifference point (p. 383).

> For each hand a range of offers had been made. The proportion of times the subject elected to play each offer was calculated, and these points were plotted on ordinary arithmetic graph paper with the vertical axis as percent participation and horizontal axis as amount of offer in cents. A freehand curve or a broken-line curve was then fitted to these points. The abscissa value of the point where this curve crossed the 50 percent participation line gave in cents the subjects' indifferences offer for that hand. In other words, for that hand this calculation yielded an interpolated offer which the subject would be equally likely to accept or reject if given the opportunity.

For a series of offers x, they had the following inequality in which some offers the sign went both ways.

$$(1 - P)\ \psi(-5¢) + p\psi(x) \gtrless \psi(0) \qquad (1)$$

The interval of offers for which a bet was sometimes taken and sometimes not, was called the zone of inconsistency.

Mosteller and Nogee admit that this operational definition of indifference contradicts the weak ordering axiom of the expected utility hypothesis, but state that it "supports the experience of psychologists with psychological tests showing that gradation of preference is the rule when persons locate themselves on 'physical' continua" (1967, p. 127). Indeed, this kind of experience by psychologists was the basis for criticisms of the theory. Mosteller and Nogee do not provide a satisfactory answer to the fact that their testing procedure in a strict sense violates the theory they are testing. They state that "the importance of this gradation of preference in utility measurement cannot be assessed until it is known to what purpose the analysis will be put" (1967, p. 128).

Seven different poker dice hands were presented with different offers and over more than one session. With this seven indifference offers, the utility index was calculated and the nine points of the utility function were plotted and a freehand or broken line utility function was drawn. With this utility curve for each subject, predictions were made for more complicated gambles. Their predictions, based on the measured utility functions, are of the form that if expected utility is positive, the gambles will be taken more than 50 percent of the time. The predictions were in general reasonable but not as good as they had hoped.

Among the criticisms and comments Mosteller and Nogee make about their own study is the lack of uniformity of experience that the subjects had. Since the dice were not rolled when a subject refused to play, the number of times a subject saw a particular hand played depended on the participation rate of himself and those in his group. What affect this would have on the results is not entirely clear. However, the implication would seem to be that the zones of inconsistency would be affected (decreasing with increased experience). When the more complicated risk-taking bets were used to check the adequacy of their estimated utility functions, the zones of inconsistency increased. Lack of experience with these situations caused considerable confusion among the subjects. Lack of uniformity of experience may also have had some affect on the assumption of subjective probability and objective probability being equal. Even though the objective probabilities of beating a particular poker dice hand were given, the subject may have revised this according to the number of times he experienced that particular hand and the outcomes.

Other comments that Mosteller and Nogee made are interesting and yield some important insights. Though the use of actual money gambles may have made the experiment more realistic, the triviality of the sums involved raise some doubt as to whether there was enough incentive to provide responses that would reflect the type of decisions that would be made in a practical setting. Another unresolved issue was whether or not there was any utility or disutility of gambling. This would also bias some or most of the responses if present. There also seemed to be some effect of the amount of money in front of the subject. While paying only 5¢ to bet, they could possibly win up to 8 dollars. In effect, the subject was making a sequence of decisions with different levels of wealth.

The next important study already mentioned was conducted by Davidson, Suppes, and Siegel (1957). This study measured both utility and subjective probability and followed the development outlined by Ramsey (1926). The Davidson, et. al. study was inspired by the Mosteller and Nogee experiment and an attempt was made to improve upon their results. There were three major criticisms of the Mosteller and Nogee study. The first criticism was that their experiment did not provide a systematic check of their measured utility curves. The check that they did use involved more complicated bets or gambles than those used to construct the utility curves. This may have introduced some factors into the decision making process that were not taken into account. For example, there were larger zones of inconsistency in the more complicated situations that could not be explained by the axioms of expected utility. In addition there was no check to determine that the measured utility curves were unique up to a linear transformation or that they provided an interval measurement.

The second criticism was already noted by Mosteller and Nogee, and that was that the experiment was designed so that the subjects choose to accept or reject a gamble vis-a-vis a certain prospect i.e. not playing. If there was non-zero utility from just playing, choices would be distorted. The solution to this problem that Davidson, et. al. devised was to present their subjects a choice between two lotteries.

The third criticism of Mosteller and Nogee's experiment was to assume that subjective probability was equal to objective probability. Even though the subjects were given a sheet with the true mathematical odds on it, there is no evidence that the subjects equated subjective and objective probabilities. The subjects could have felt that the die used to generate the events was not fair. One could also argue, as Menger (1934) did, that very low and very high probabilities are undervalued (and therefore, probabilities in the middle are overvalued). Or as Samuelson (1977) stated, small probabilities of pleasant events could have been overvalued. In any case, there was a potential source of bias or distortion. In addition, as was mentioned earlier, the experience of an individual subject with a particular hand may have distorted subjective probability from objective probability.

The strategy used by Davidson, et. al. in their experiment was to test a set of hypotheses, which if true imply a set of axioms from which it can be shown that there exists a utility function unique up to linear transformations and a unique subjective probability function. The first hypothesis was that preference is equivalent to a strict, inequality, $\prec$, which is a weak order. It was not felt to be necessary to test this hypothesis, although the property of transitivity has been questioned in some circumstances.

The second hypothesis states that there exists a chance event whose subjective probability is one-half. This chance event is then used to construct a utility function which is then used to measure subjective probabilities. The procedure used to test if a chance event had subjective probability of one-half rejected the use of a coin and the use of a standard die. Subjects showed a preference for either heads or tails or certain numbers on the die. However, a die with nonsense syllables was finally used.

Having found a chance event with subjective probability of one-half, the particular form of the game allows one to find points that are equally spaced in utility. The procedure they used is shown in Table 1. The outcomes are spaced equally in utility in the following order: f, c, a, b, d, g. In the experiment they used, a = -4¢ and b = 6¢. An approximation analogue was used to find points f, c, d, g. Relating to the first game, an amount c_ℓ was elicited such that

$$b,\ c_\ell \preceq a,\ a \tag{2}$$

and

$$b,\ c_\ell + 1 \succeq a,\ a \tag{3}$$

where (2) states that c_ℓ is elicited such that Option 2 is preferred or indifferent to Option 1 and (3) states that with c_ℓ plus 1 cent Option 1 is preferred or indifferent to Option 2. In game 2 then they found an upper bound for d, called d_h using the lower bound of c, c_ℓ and also found a lower bound for d, called d_ℓ, using the upper bound for c, $c_h = c_\ell + 1$. Therefore we have, using c_ℓ

$$b,\ a \preceq d_h,\ c_\ell \tag{4}$$

and

$$d_h - 1,\ c_\ell \preceq b,\ a \tag{5}$$

Using c_h we have

$$d_\ell,\ c_h \preceq b,\ a \tag{6}$$

Table 1

Let a < b, then there are unique amounts c, d, f, and g such that the subject is indifferent between each action in each game.

		Events	
		E^*	$\tilde{E}^*$
1)	Option 1	b	c'
	Option 2	a	a
2)	1	b	a
	2	d'	c
3)	1	d	f'
	2	b	c
4)	1	b	f
	2	a	c'

		E^*	$\tilde{E}^*$
5)	1	g'	f
	2	b	a
6)	1	g	f
	2	d	c'
(7)	1	g	c
	2	d	a'
(8)	1	g	a
	2	d	b'

The prime (') denotes that amount that was varied in each game.

and

$$b, a \leq d_{\ell} + 1, c_h \qquad (7)$$

For eliciting f_{ℓ} in Game 3 they used c_{ℓ} and d_h and for eliciting f_h they used c_h and d_{ℓ}. Once the approximate determination of f was completed, a check was made on the bounds for c. In Game 4, f_{ℓ} was used to obtain c'_{ℓ} and f_h was used to obtain c'_h. If c'_{ℓ} and c'_h nested c_{ℓ} and c_h, i.e. if $c'_{\ell} \leq c_{\ell} < c_h \leq c'_h$, the check was satisfied. If the check was not satisfied, the initial values of c_{ℓ} and c_h were varied and bounds on d and f were re-elicited. This check in Game 4 was primarily used to check for any utility of gambling from Game 1 where a sure thing option (option 2) was used. Only one-third of the subjects needed to have compensations made and for all but one, one compensation was sufficient. Their results, though tentative, show that the distortion due to utility of gambling was not as strong as originally thought (1967, p. 186). Games 4, 6, 7, and 8 were used to check if the method gave an approximate measurement of utility of the interval scale type.

The original conception of the experiment did involve some checks for transitivity of indifference, but were omitted since they involved sure thing options. The use of a sure thing option in Game 1 was necessary to set up a scheme that allows for the possibility for financial losses for every option chosen. This avoided presenting subjects with windfall situations with sure gains. The original theoretical framework also required perfect measurement of the amounts so that indifference between the options held. However, in the experiment the subject was required to choose one option or the other and there is no direct interpretation for indifference. One could use a statistical definition of indifference as Mosteller and Nogee did. However, as Davidson, et al. report this was not possible:

> The fact is that our subjects responded with nearly 100% probability with respect to all offers presented them, i.e., once they chose a given option over another, they consistently held to this choice, and did not change their minds when the same two options were subsequently presented together. The primary reason for this kind of response is no doubt the relative simplicity of the offers. Mosteller and Nogee, using the much more complicated game of poker dice to generate chance events, did get a distribution of responses. A second reason for this constancy of response we obtained is probably the relatively high ratio of one cent to the amounts of money used to make up the offers. Finally, we remark that we gathered the data relevant to determining a subject's utility curve over a period of about two hours, rather than a period of months as is the case of the Mosteller and Nogee experiments (1957, p. 41).

The Davidson, et. al. study used a system of inequalities to approximate perfect measurement, where the perfect measurement should be between the upper and lower bounds that are elicited. They note that the error in measurement from the perfect measurement accumulates as successive points are determined. In particular, the bounds on d, elicited in Game 2 are less accurate than the bounds on c, elicited in Game 1, since d is elicited with respect to an approximate measurement of c. This problem of accumulating errors will exist whenever there is not perfect measurement and a utility index is set up which depends on previously elicited points. This is discussed in Appendix B.

In summarizing their results, they report that for the nineteen subjects they interviewed all of them satisfied the hypothesis regarding the existence of a chance event with subjective probability of one-half. For fifteen of the nineteen subjects, their behavior was consistent enough to satisfy the hypothesis leading to a utility function that was unique up to linear transformation. For some subjects they redid the interviewing after a few days to several weeks and found little change in the responses. However, they do point out that the checks for transitivity that they did omit should have been run and there were some checks that did fail but were recomputed. While these subjects failed to be consistent in every response, in general they were consistent. Of the four subjects who failed in their responses to have their utility measured, two were particularly averse to gambling. The other two were very tense during the experiment and were aware of their erratic responses. They state that it would "be very interesting to explore possible connections between 'rationality' in decision making of the sort tested here and other personality traits" (1967, p. 194).

For most subjects the utility curves were not consistent with a linear utility function and in fact resembled the type of curve hypothesized by Friedman and Savage. Among the criticisms they had for their own study, one was that the method elicited points that were equally spaced in utility and that a set of alternatives could not be determined in advance. Closely related offers are made from one offer to the next and sometimes subjects realized that there was a predetermined system from one game to the next. They note that:

> A method which, while retaining the merits of the present approach, allowed the utility measurement of alternatives chosen in advance would have clear advantages: it would apply to alternatives other than amounts of money; the same offers could be made to all subjects; the offers could be given in a random sequence; the experimenter would be relieved of the necessity of performing calculations during the experiment; and (equally important) the experimenter would not know, at the time the decisions were made, what decisions a subject should make to verify the theory (1967, p. 204).

The use of actual money wagers has been used in some other studies since the Davidson, et al. study. One of these is by Becker, DeGroot, and Marschak (1964). They used a method similar to the Mosteller and Nogee, the VonNeumann and Morgenstern method, but with some changes.

They elicited certainty-equivalent amounts and did not use a statistical definition of indifference. However, they did have a check on the subjects' consistency in which repeated estimates of the same points on the utility curve were elicited. The consistency check can be described in a four-step procedure, although twenty-four steps were used in the actual experiment.

Step 1: Find x_3 such that

$$\psi(x_3) = \tfrac{1}{2}\,\psi(x_1) + \tfrac{1}{2}\,\psi(x_2) \tag{8}$$

x_3 being the certainty equivalent amount to a lottery or gamble with payoffs x_1 and x_2 which can occur with equal probability y_2.

Step 2: Find x_4 such that

$$\psi(x_4) = \tfrac{1}{2}\,\psi(x_3) + \tfrac{1}{2}\,\psi(x_2) \tag{9}$$

Step 3: Find x_5 such that

$$\psi(x_5) = \tfrac{1}{2}\,\psi(x_1) + \tfrac{1}{2}\,\psi(x_3) \tag{10}$$

Step 4: Find x_6 such that

$$\psi(x_6) = \tfrac{1}{2}\,\psi(x_4) + \tfrac{1}{2}\,\psi(x_5) \tag{11}$$

It can be shown that $\psi(x_3) = \psi(x_6)$, and to meet the requirements of a utility function it should be the case that $x_3 = x_6$. In the Becker, et. al., study they conducted the experiment in three separate sessions with two students. They found that for four consistency checks most of the differences were non-zero. Strictly interpreted this violates the expected utility hypothesis. They did make an important observation though:

> It should also be noted, however, that the differences in prices decrease, on the average, from session to session, indicating that behavior does become, in some sense, more consistent with an expected utility model as the subject becomes more familiar with the task. Thus, despite the fact that the model does not precisely fit the behavior of the subjects, there is some indication that it approximates such behavior and that the model becomes more appropriate as the subject becomes more familiar with the experiment (p. 230).

DeGroot (1970, p. 96) and Anderson, Dillon, and Hardaker, (1977, p. 70-75) have suggested that this method with consistency checks be used to improve the accuracy of the elicitation procedure. If a discrepancy occurs, the steps can be repeated with different numbers until the desired degree of accuracy is achieved. Of course, this can be a very time consuming process and by taxing the patience of the subject it may affect his responses.

One final experimental study I wish to report on is one by Binswanger[1] (1978) conducted in India and discussed in last year's report. Initially there was an attempt to use a hypothetical situation and elicit certainty equivalents in a method similar to that used by Dillon and Scandizzo (1978) in Brazil. Attempts were made to make the questions meaningful in terms of the farmers own experience, although the exact wording of the interviews is not given. However, he found large inconsistencies which he attributed to investigator bias, preferences for other activities, and learning difficulties of the farmers, many of whom were illiterate. It is difficult to say what caused many of the problems Binswanger encountered without knowing more details of the interviews. However, questions that are too elaborate in order to be more meaningful have many potential problems. The problem due to the correlation of the hypothetical situation and the initial prospect is discussed in the next section. The fact that other objectives and attributes of the farmer's utility functions entered into the responses suggests that the questions dealt with more than just financial considerations.

Binswanger cited two main advantages of using real wagers. One was that the investigator could observe real choices rather than hypothetical ones. The other was that the choices were made over a six week period and the subjects had time to reflect upon their choices. These two advantages are important, nevertheless, there are many problems with this study. The subject was given an amount of money and asked to return the next day, when the subject could choose to keep the money, or give it back and choose one of several options whose outcomes were decided by the flip of a coin. The worst outcome of each option occurred for the same event (heads) and was greater than zero but less than the amount to play the game. The experimental procedure used by Binswanger contained many of the problems encountered by Mosteller and Nogee and remedied to some extent by the Davidson, et. al. study. The Davidson, et. al. study found that a flip of a coin did not have subjective probability of one-half for many subjects. The potential problem due to utility of gambling was present in the Binswanger study with one option a sure outcome (not playing). Mosteller and Nogee reported an effect of the amount of money a subject had in front of him upon his decisions. A subject played right after making a decision so that an individual's wealth changed before the next position. Both the Davidson, et. al. study and the Becker, et. al. study had the individual make all the decisions before any betting had begunto avoid having the subject's wealth position change. This problem was not avoided in the Binswanger study. Despite some indications that when the subjects were given money for a full day with the option of keeping it or returning the next day to play the game, I am not convinced that they viewed this as their own, instead of nonpermanent "funny" money.

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[1] Comments are made here not only on the ICRISAT working paper but also on the basis of a seminar delivered at the University of Minnesota in 1978.

Finally, although the amounts of money involved were not trivial for the type of people interviewed, the games were set up so that it was impossible to lose over the whole series of games and the average return exceeded monthly income.

There are two major flaws in using laboratory experiments to estimate utility functions. First, the experiments have often been carried out with amounts that are trivially small. In addition the experiment is usually set up so that the subjects will average out as gainers and not losers. Subjects who realize this may treat their winnings as funny money rather than their own. Secondly, though we observe actual choices, we observe them under artificial and contrived circumstances. The subjects need to go through a learning process that is quite unfamiliar to some subjects. It is difficult to make strong inferences or generalizations about real world risk attitudes from information gathered under laboratory experiments.

FURTHER COMMENTS ON STUDIES WITH HYPOTHETICAL CHOICES

One of the alternatives to using laboratory experiments to estimate utility is presenting hypothetical choices in a realistic or practical setting. I will not elaborate on these studies or mention many of the difficulties with the studies, but refer the reader to last years report by Young, et. al. (1979). However, I would like to make some further comments that investigators should be aware of when using this method. The framing of the questioning when using this method is very critical so as to avoid potential sources of bias in the responses. If the questioning is too abstract, the subject may have difficulty understanding or relating to the problem. On the other hand, attempts to make the questioning more elaborate and realistic to make it easier for the subject to respond may do more harm than good. Extraneous considerations may be introduced into the problem so that responses do not reflect just risk preferences, but other preferences as well. Moreover, the questioning may be framed in such a way that the hypothetical venture is correlated with the subjects initial or current prospect so that responses reflect not only risk preferences but also the joint distribution between the hypothetical venture and the current prospect (see Hildreth (1974) and Hildreth and Tesfatsion (1977)). Appendix A presents an illustration with three farmers presented with the same hypothetical choices and with the same utility function and yet three different responses are elicited due to this correlation.

One should also be aware that there have been instances where responses given by subjects have demonstrated a violation of the axioms of expected utility theory. The most recent study in this context is reported by Kahneman and Tversky (1979) who present a critique of expected utility as a descriptive model and develop an alternative. This result is not surprising since many studies have reported inconsistencies with the axioms of expected utility theory. Furthermore, since most subjects are unfamiliar with either the hypothetical choices or the types of wagers in experimental methods, errors in judgment or calculation by the subject are not surprising given their limited computational ability. Nonetheless, most studies have also shown that these inconsistencies have a tendency to be less frequent as the experience of the subject with the particular situation increases. The adage that practice makes perfect has some relevance here. The lesson here is that risk preferences elicited from one session from a subject may not be very accurate.

Methods of Estimation

The conventional approach to estimating the parameters of a utility function has been to index the data and regress the utility index on the particular functional form. However, as Davidson, Suppes, and Siegel first pointed out, this indexing procedure accumulates or compounds the errors that are made either by the subject or as a result of the elicitation procedure. Appendix B is a formal demonstration of some possible adverse effects due to this compounding of errors. At a minimum the error term in the regression with the utility index as the dependent variable is most likely autocorrelated and heteroscedastic, even if the original response errors are independent and identically distributed with mean zero. This severely limits the confidence one has in the parameter estimates.

Spetzler (1968) also recognized the problems due to the compounding of errors and suggested an alternative indexing procedure that avoided this problem. However, as I have shown in Appendix C his method is biased. In fact, the parameter estimate varies with the index scale. Consequently, my recommendation is to avoid any use of indexing to estimate the parameters of a utility function.

AN ALTERNATIVE

An alternative estimation procedure which does not rely on any indexing is the use of an error in response model.[2] This model explicitly assumes that the subject responds with error. Consider the following Ramsey procedure in Table 2 where X_1, X_2, and X_3 are given to the subject and the subject is asked to respond with X_4 such that the subject is indifferent between action A_1 and A_2. There exists an $\tilde{X}_4$ (not necessarily

Table 2

		States of Nature	
		θ_1	θ_2
Actions	A_{1i}	X_{1i}	X_{2i}
	A_{2i}	X_{3i}	X_{4i}

[2]This model was first investigated by Clifford Hildreth in connection with data we had collected using a Von-Neumann-Morgenstern procedure. Results from this model are in the final stages of completion and should be published soon.

the response of the decision maker) such that for the true utility function ψ, we have

$$\psi(X_{1i}) + \psi(X_{2i}) = \psi(X_{3i}) + \psi(\tilde{X}_{4i}) \quad (12)$$

Therefore, we have

$$\tilde{X}_{4i} = \psi^{-1}\{\psi(X_{1i}) + \psi(X_{2i}) - \psi(X_{3i})\} \quad (13)$$

However, the response by the subject, say X_4^* is equal to $\tilde{X}_4$ plus a random error term, so we have

$$X_{4i}^* = \tilde{X}_{4i} + u_i \quad (14)$$

Combining (13) and (14) we have

$$X_{4i}^* = \psi^{-1}\{\psi(X_{1i}) + \psi(X_{2i}) - \psi(X_{3i})\} + u_i \quad (15)$$

A least squares criterion can be used to estimate the parameter of ψ.

The error in response model has a number of advantages. It avoids the estimation problems that are a characteristic of the utility index models, as shown in Appendices B and C. Without the need to index the data, offers do not have to be repeated and there is much more flexibility in the offers presented in the trials using either the VonNeumann-Morgenstern method or the Ramsey method, an advantage cited by Davidson, Suppes, and Siegel. Finally, some interesting insights into the decision making process by a careful analysis of the error term may be forthcoming.

RECOMMENDATIONS

The recommendations that I have are at odds with last years report by Young, et. al. I do not support the use of the experimental method to elicit utility functions. While actual choices are observed in the method, they are in regard to artificial and contrived circumstances. I doubt seriously that the method can be structured so as to provide nontrivial gains and losses, to overcome the problems of knowing the subjective probabilities of the subjects, to avoid the gambling connotations of the method, and to provide an acceptable interpretation of risk preferences in the real world from an artificial setting.

Nevertheless, the use of direct elicitation methods is not without pitfalls. There are numerous sources of bias and investigators must give careful consideration to the procedure used. Among the numerous factors to consider, I feel strongly about the following four.

1. The method used should avoid sure things or windfall situations. The ventures or hypothetical choices presented to a subject should have both losses and gains as a possibility. This recommendation obviously favors the use of the Ramsey method. Having both losses and gains in a venture adds realism and causes the subject to respond more seriously than if the ventures were windfall gains.

2. The framing of the questions should avoid too much elaboration and realism, unless the investigator is confident that the hypothetical venture is independent of the current prospect and any other extraneous considerations. In many cases the framing of the questions in an abstract manner may be acceptable. For those subjects that have difficulty with the abstract questioning, a relatively simple example that the subject can identify with, yet is independent of the current prospect, may be used.

3. There is a need to ensure that the subjects have enough time and experience to familiarize themselves with the hypothetical choices. Numerous studies have shown that inconsistencies become less frequent with experience. Risk preferences that are elicited from single interviews may not be very reliable.

4. Finally, using an error in response model to estimate the parameters of a utility function should be used. Without the need to index the data, payoffs do not need to be repeated and many econometric problems are avoided.

APPENDIX A

An Illustration of the Impact of an Uncertain Initial Prospect on Estimating Utility Functions Using the Ramsey Method[1]

Suppose there are three farmers from whom we want to elicit information about their risk preferences in order to estimate their utility functions. All three farmers have very similar operations in which they feed beef cattle and grow soybeans for cash and corn for feed. In attempting to elicit information regarding their risk preferences we present each farmer with the same hypothetical situation involving two uncertain actions. There are two mutually exclusive events that affect the outcomes of the actions. The method used in this example is a variation of the Ramsey method. To help motivate the farmers to think about the hypothetical situation, we tell them to think about the events as "favorable" and "unfavorable" economic conditions, a situation similar to that used by Lin, Dean, and Moore (1974, p. 501). Favorable economic conditions will produce good outcomes for the respective actions and unfavorable economic conditions will produce bad outcomes for the respective actions. To illustrate the situation I will use the following example:

Events

	θ_1 (Unfavorable)	θ_2 (Favorable)	
Action A_1	-10	100	(A-1)
Action A_2	-100	y	

The event θ_1 is unfavorable economic conditions and if the farmer had chosen action A_1 and θ_1 resulted, outcome -10 would occur. We would like to elicit an amount y from each farmer such that he is indifferent

[1]This illustration is an adaptation of a USDA-ERS-NEAD seminar given by Clifford Hildreth, October 19, 1977.

between action A_1 and action A_2, i.e. we want

$$E\phi(X + Y_1) = E\phi(X + Y_2) \tag{A-2}$$

Where Y_1 is a random variable depicting action A_1, Y_2 is a random variable depicting action A_2, X is a random variable depicting the initial or current prospect, and ϕ is the utitility of wealth function.

For the purpose of illustration assume that all three farmers have the same constant absolute risk aversion utility function with Pratt-Arrow coefficient of .00001, therefore $\phi(x) = -e^{-.00001x}$. Furthermore, assume that all three farmers have the same initial or current prospect of

$$X = 400\ I_{B_1} + 200\ I_{B_2} \tag{A-3}$$

where $B_1 = B_2^c$ and I_{B_i} is the indicator function of event B_i. Finally, assume that each farmer subjectively perceives the probability of θ_1 and θ_2 as both equal to ½, where $\theta_1 = \theta_2^c$. Similarly for B_1 and B_2. The situation is that all three farmers have the same utility function, the same initial prospect, and are presented with the same hypothetical venture. For each farmer we have

$$\begin{aligned}
E\phi(X + Y_1) = & -e^{-.00001(400 - 10)}P(B_1 \cap \theta_1) \\
& -e^{-.00001(400 + 100)}P(B_1 \cap \theta_2) \\
& -e^{-.00001(200 - 10)}P(B_2 \cap \theta_1) \\
& -e^{-.00001(200 + 100)}P(B_2 \cap \theta_2) \\
E\phi(X + Y_2) = & -e^{-.00001(400 - 100)}(B_1 \cap \theta_1) \\
& -e^{-.00001(400 + y)}P(B_1 \cap \theta_2) \\
& -e^{-.00001(200 - 100)}P(B_2 \cap \theta_1) \\
& -e^{-.00001(200 + y)}P(B_2 \cap \theta_2)
\end{aligned} \tag{A-4}$$

However, we will assume that each farmer has a different subjective joint distribution of their initial prospect and the hypothetical venture. Farmer 1 believes events leading to B_1 and B_2 are independent of θ_1 and θ_2, so that $P_1(B_i \cap \theta_j) = P(B_i)P(\theta_j) = ¼$

Farmer 2 views unfavorable economic conditions as meaning a situation of stagflation, where inflation in food prices is relatively high, especially for beef prices. He, therefore, feels that unfavorable economic conditions in general will yield a very high probability of causing event B_1 to occur since he stands to gain from high beef prices. His subjective joint distribution of B and θ is characterized by the following conditional distributions:

$$P_2(B_1|\theta_1) = .998 \qquad P_2(B_1|\theta_2) = .002 \qquad \text{(A-5)}$$
$$P_2(B_2|\theta_1) = .002 \qquad P_2(B_2|\theta_2) = .998$$

Farmer 3 on the other hand views unfavorable economic conditions as being bad for him personally and with a high probability of causing B_2 to occur. His subjective joint distribution is characterized as:

$$P_3(B_1|\theta_1) = .002 \qquad P_3(B_1|\theta_2) = .998 \qquad \text{(A-6)}$$
$$P_3(B_2|\theta_1) = .998 \qquad P_3(B_2|\theta_2) = .002$$

For Farmers 2 and 3 we use the definition of conditional distributions to obtain

$$P(B_i \cap \theta_j) = P(B_i|\theta_j)P(\theta_j) \qquad \text{(A-7)}$$

Substituting (A-7) into (A-4) and solving for y so that equality (A-2) holds we obtain the following responses for Farmers 1, 2, and 3, respectively.

$$y_1 \simeq 190.18$$
$$y_2 \simeq 100 \qquad \text{(A-8)}$$
$$y_3 \simeq 190.36$$

The response from a risk neutral individual with linear utility would be 190. Farmers 1 and 3 gave risk averse responses (i.e., responses greater than 190) with Farmer 1 giving the response we wanted the method to elicit. However, Farmer 2 gave a risk lover response of 100, much less than Farmer 1's response. This would mislead the investigator into believing that the farmer was a risk lover. If instead the three farmers had the same but different Pratt-Arrow coefficient from before, say .01 where the utility function for all three was $\phi^*(x) = -e^{-.01x}$, the responses would have been

$$y_1^* \simeq 211$$
$$y_2^* \simeq 190.07 \qquad \text{(A-9)}$$
$$y_3^* \simeq 237$$

In both cases in which the three farmers had the same utility function, the elicitation procedure failed to detect this. The investigator would have mistakenly believed that three different utility functions were elicited, when in fact there was a correlation of the hypothetical situation and the initial prospect.

APPENDIX B

The Compounding of Errors in Utility Index Models

One of the major flaws of utility index models is that when errors are made, either by the decision maker or by defects in the method used to elicit responses, they become compounded and may give rise to misleading results. This appendix will show how with the assumption of independent and identically distributed errors with mean zero, the error term in utility index models is compounded, resulting in both heteroscedasticity and autocorrelation. To demonstrate this consider the three trials in Table B-1 with values a and b given and responses, c, d, and f. If ψ is true utility function, unique up to positive linear transformations, then from Trial 1 there exists a value c such that

$$2\psi(a) = \psi(b) + \psi(c) \tag{B-1}$$

However, the decision maker responds with c* so that c and c* differ by a random error term. We can express this as

$$\psi(c) - \psi(c^*) = \delta_c \tag{B-2}$$

where δ_c is a random error, additive in utility, and corresponding to response c. The investigator assigns index numbers to a and b without error and on the basis of this assigns an index number to the response c*, so that

$$I(c^*) = 2\psi(a) - \psi(b) = \psi(c) \tag{B-3}$$

Because of the error in (B-2), the index number in (B-3) differs from the true utility function at c* by an error term so that

$$I(c^*) - \psi(c^*) = \varepsilon_c \tag{B-4}$$

TABLE B-1

	Actions	States of Nature θ_1	θ_2
Trial 1	A_{11}	a	a
	A_{21}	b	c*
Trial 2	A_{12}	b	a
	A_{22}	d*	c*
Trial 3	A_{13}	b	c*
	A_{23}	d*	f*

*indicates responses by the decision maker

However, from (B-3), $I(c^*) = \psi(c)$ and therefore

$$\varepsilon_c = I(c^*) - \psi(c^*) = \psi(c) - \psi(c^*) = \delta_c \quad \text{(B-5)}$$

When the utility index model regression is run, the error for the index number corresponding to the first response is the same as the error due to the response error. However, once the index number depends on previous responses the error compounds. From Trial 2 an index number is assigned to response d* so that

$$I(d^*) = \psi(b) + \psi(a) - I(c^*) \quad \text{(B-6)}$$

From equivalent reasoning to (5.2.4) we have

$$I(d^*) - \psi(d^*) = \varepsilon_d \quad \text{(B-7)}$$

Adding and subtracting $\psi(d)$ and substituting for $I(d^*)$ from (B-6)

$$\varepsilon_d = \psi(a) + \psi(b) - I(c^*) - \psi(d) + \psi(d) - \psi(d^*) \quad \text{(B-8)}$$

From the error in response $\psi(d) - \psi(d^*) = \delta_d$ and d being the true value in Trial 2 that makes the two actions indifferent we have

$$\varepsilon_d = \psi(a) + \psi(b) - I(c^*) - [\psi(a) + \psi(b) - \psi(c^*)] + \delta_d \quad \text{(B-9)}$$

or

$$\varepsilon_d = -[I(c^*) - \psi(c^*)] + \delta_d$$

or

$$\varepsilon_d = -\delta_c + \delta_d \tag{B-10}$$

Therefore the error term to the index number from Trial 2 contains not just the error to the response in Trial 2, but also the error in response to Trial 1. From Trial 3 we have the following steps.

$$I(f^*) - \psi(f^*) = \varepsilon_f \tag{B-11}$$

$$\varepsilon_f = \psi(b) + I(c^*) - I(d^*) - \psi(f) + \psi(f) - \psi(f^*)$$

$$\varepsilon_f = \psi(b) + I(c^*) - I(d^*) - [\psi(b) + \psi(c^*) - \psi(d^*)] + \delta_f$$

$$\varepsilon_f = I(c^*) - \psi(c^*) - [I(d^*) - \psi(d^*)] + \delta_f$$

$$\varepsilon_f = \delta_c - \delta_d + \delta_f \tag{B-12}$$

The utility index model then should have the following equations as observations

$$I(c^*) = \psi(c^*) + \varepsilon_c$$

$$I(d^*) = \psi(d^*) + \varepsilon_d$$

$$I(f^*) = \psi(f^*) + \varepsilon_f$$

where we try to fit the function ψ using the index numbers as the dependent variable. ψ will have an intercept and constant term, but these should be expressed in terms of the other parameters by using the two restrictions that

$$I(a) = \psi(a)$$

and (B-13)

$$I(b) = \psi(b)$$

where no error term is involved. The variance-covariance matrix to this model is

$$E\varepsilon'\varepsilon = E \begin{bmatrix} \varepsilon_c \\ \varepsilon_d \\ \varepsilon_f \end{bmatrix} [\varepsilon_c \quad \varepsilon_d \quad \varepsilon_f]$$

$$= E \begin{bmatrix} \varepsilon_c^2 & \varepsilon_c\varepsilon_d & \varepsilon_c\varepsilon_f \\ \varepsilon_d\varepsilon_c & \varepsilon_d^2 & \varepsilon_d\varepsilon_f \\ \varepsilon_f\varepsilon_c & \varepsilon_f\varepsilon_d & \varepsilon_f^2 \end{bmatrix} \qquad \text{(B-14)}$$

Substituting in equations (B-5), (B-10), and (B-12) and assuming that the errors in response are uncorrelated and have expected value of zero, we have

$$E\varepsilon'\varepsilon = E\delta^2 \begin{bmatrix} \delta_c^2 & \delta_c^2 & \delta_c^2 \\ \delta_c^2 & (\delta_c^2 + \delta_d^2) & (\delta_c^2 + \delta_d^2) \\ \delta_c^2 & (\delta_c^2 + \delta_d^2) & (\delta_c^2 + \delta_d^2 + \delta_f^2) \end{bmatrix} \qquad \text{(B-15)}$$

The error term then for the utility index models is both heteroscedastic and autocorrelated. Even if they were identically distributed so that $\delta_c = \delta_d = \delta_f = \delta$, we would have

$$E\varepsilon'\varepsilon = E\delta^2 \begin{bmatrix} 1 & 1 & 1 \\ 1 & 2 & 2 \\ 1 & 2 & 3 \end{bmatrix} \qquad \text{(B-16)}$$

which is still heteroscedastic and autocorrelated.

The assumption that response errors are independent and identically distributed may not be justified and would therefore further complicate the utility index models. Noncorrelation of the response errors may not be justified, and empirical tests for this should be performed. A response error with a non-zero expected value, due perhaps to utility of gambling or use of decision rules, will be carried throughout the indexing procedure. In any case, untangling these problems in a utility index model will be extremely difficult and our confidence in the parameter estimates from these models is severely weakened.

APPENDIX C

Proof that Spetzler's Parameter Estimate Depends on the Indexing Scale

Spetzler (1968) estimated utility functions for 36 corporate executives to investigate the feasibility of developing a corporate utility function. Individual risk preferences were elicited using a VonNeumann-Morgenstern method in which an indifference probability is elicited. The questioning was in the context of an investment in a project (the certainty amount), with either success or failure of an investment. All amounts were expressed in present value terms. The probability of success was varied until indifference was obtained. For a particular trial we would expect the following to hold:

$$\psi(0) = P_{s_i}\psi(X_{s_i}) + (1 - P_{s_i})\psi(X_{f_i}) \qquad \text{(C-1)}$$

where ψ is the utility of gain function, X_{s_i} is the present value of success, X_{f_i} is the present value of failure ($X_{f_i} < 0$), and P_{s_i} is the decision maker's indifference probability. Spetzler explicitly recognizes the problem of compounding errors in other indexing models (p. 285) but without a formal presentation as is given in Appendix B here. Spetzler states that we would expect some error or deviation from the criterion as in equation (C-1) and proposes minimizing the sum of squared deviations to estimate the parameters of the utility function. From his equation (3) (p. 291) we have[1]

$$\Sigma\{\psi(0) - P_s\psi(X_s) - (1 - P_s)\psi(X_f)\}^2 = \text{minimum} \qquad \text{(C-2)}$$

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[1] Spetzler's equation (3) contained a mistake in which a positive sign appeared in the brackets. I am not sure that this was just a simple typo since the mistake is carried through to his equation (5).

Spetzler chose to fit a logarithmic function to his data as this would agree with the primarily risk averse responses he was eliciting. For this specification we have

$$\psi(X) = a + b\ \mathrm{LOG}(X + \gamma) \qquad \text{(C-3)}$$

Substituting (C-3) into (C-2) we get

$$\Sigma\{a + b\ \mathrm{LOG}(\gamma) - P_s[a + b\ \mathrm{LOG}(X_s + \gamma)] - (1 - P_s)[a + b\ \mathrm{LOG}(X_f + \gamma)]\}^2 =$$

$$b^2\Sigma[\mathrm{LOG}(\gamma) - P_s\mathrm{LOG}(X_s + \gamma) - (1 - P_s)\mathrm{LOG}(X_f + \gamma)]^2 \qquad \text{(C-4)}$$

The minimum for (C-4) is independent of the parameters a and b, which we would expect since utility is unique up to positive linear transformations. The problem though is that the sum of squared residuals converge to zero as γ approaches infinity. To show this we have

$$\text{(C-4)} = \Sigma\{P_s[\mathrm{LOG}(\gamma) - \mathrm{LOG}(X_s + \gamma)] + (1 - P_s)[\mathrm{LOG}(\gamma) - \mathrm{LOG}(X_f + \gamma)]\}^2$$

$$\leq \Sigma\{P_s\ [\mathrm{LOG}(\frac{\gamma}{X_s + \gamma})] + (1 - P_s)[\mathrm{LOG}(\frac{\gamma}{X_f + \gamma})]\}^2$$

$$\text{(C-5)}$$

We note that

$$\lim_{\gamma \to \infty}\Sigma\{P_s\mathrm{LOG}(\frac{\gamma}{X_s + \gamma}) + (1 - P_s)\ \mathrm{LOG}(\frac{\gamma}{X_f + \gamma})\}^2 = 0 \qquad \text{(C-6)}$$

Spetzler did not estimate his parameters by (C-2), but instead he chose an arbitrary scale for the utility function and set $\psi(\$0) = 0$ and $\psi(\$k\ \text{million}) = k$, with $k = 50$. However, this scale is not arbitrary in the sense that the parameter estimate will vary with k. When a scale has been used in other studies all the data is indexed to be consistent with the scale. The index numbers are used as the dependent variable and regressed on the particular functional form. In Spetzler's case, the data is not indexed and this creates the problem. Using the two scaling constraints he solves out a and b as follows:

From the first constraint,

$$\psi(0) = a + b\ \mathrm{LOG}(0 + \gamma) = 0$$

and (C-7)

$$a = -b\ \mathrm{LOG}\ (\gamma)$$

Substituting this into the second constraint we have

$$\psi(k) = -b\ \mathrm{LOG}(\gamma) + b\ \mathrm{LOG}(k + \gamma) = k$$

and (C-8)

$$b = \frac{k}{\mathrm{LOG}(\frac{k + \gamma}{\gamma})}$$

Substituting (C-7) and (C-8) back into the utility function we get

$$\psi(X) = \frac{k\ \mathrm{LOG}(\frac{X + \gamma}{\gamma})}{\mathrm{LOG}(\frac{k + \gamma}{\gamma})} \qquad \text{(C-9)}$$

Substituting (C-9) into the (C-2) we get Spetzler's equation (5) (p. 291):

$$\Sigma\left(\frac{kP_s\ \mathrm{LOG}(\frac{X_s + \gamma}{\gamma}) + k(1 - P_s)\ \mathrm{LOG}(\frac{X_f + \gamma}{\gamma})}{\mathrm{LOG}(\frac{k + \gamma}{\gamma})}\right)^2 = \text{minimum} \qquad \text{(C-10)}$$

We can factor out the k in the numerator so that it does not affect the minimum with respect to γ. However, the k in the denominator does not factor out. Therefore, from the first order condition for a minimum we get an implicit function between k and γ, in the neighborhood of $k = 50$ and the parameter estimate $\hat{\gamma}$. By defining a function S from the first order condition we get

$$S(k, \hat{\gamma}) = \frac{d}{d\gamma} \Sigma\left(\frac{P_s \, LOG\left(\frac{X_s + \hat{\gamma}}{\hat{\gamma}}\right) + (1 - P_s) \, LOG\left(\frac{X_f + \hat{\gamma}}{\hat{\gamma}}\right)}{LOG\left(\frac{k + \hat{\gamma}}{\hat{\gamma}}\right)}\right)^2 = 0 \tag{C-11}$$

Equation (C-11) should be an identity for all k, i.e. the partial derivative of S with respect to k, $S_k(k, \hat{\gamma})$, should be zero. What needs to be shown is that $S_k(k, \hat{\gamma}) \neq 0$.

Let

$$N(\gamma) = P_s \, LOG\left(\frac{X_s + \gamma}{\gamma}\right) + (1 - P_s) \, LOG\left(\frac{X_f + \gamma}{\gamma}\right) \tag{C-12}$$

and

$$D(k, \gamma) = LOG\left(\frac{k + \gamma}{\gamma}\right) \tag{C-13}$$

Then

$$S(k, \hat{\gamma}) = 2 \, \Sigma\left(\frac{N}{D}\right)\left(\frac{N_\gamma D - ND_\gamma}{D^2}\right) = 0 \tag{C-14}$$

or

$$= \Sigma\left(\frac{NN_\gamma D - N^2 D_\gamma}{D^3}\right) = 0 \tag{C-15}$$

Therefore,

$$S_k(k, \hat{\gamma}) = \Sigma \left[\frac{(NN_\gamma d_k - N^2 D_{\gamma k})D^3 - (NN_\gamma D - N^2 D_\gamma) 3D^2 D_k}{D^6}\right]$$

$$= \Sigma \left[\frac{3N^2 D_\gamma D_k - 2NN_\gamma DD_k - N^2 DD_{\gamma k}}{D^4}\right] \tag{C-16}$$

Since X_s and X_f do not enter into the D term or its derivative, this is constant for all terms in the summation. We can therefore take the denominator D^4 outside the summation and investigate when the resulting summation equals zero.

$$\Sigma[3N^2 D_\gamma D_k - 2NN_\gamma DD_k - N^2 DD_{\gamma k}] \tag{C-17}$$

$$= -2D_k \Sigma(NN_\gamma D - N^2 D_\gamma) + \Sigma(N^2 D_\gamma D_k - N^2 DD_{\gamma k})$$

By the first order condition represented by (C-15), the summation on the left is equal to zero and (C-17) becomes just the summation on the right. Now $D_{\gamma k} = -(D_k)^2$ as the readers can check for themselves and (C-17) becomes

$$\Sigma(N^2 D_\gamma D_k + N^2 D(D_k)^2) = \tag{C-18}$$

$$D_k \; \Sigma(N^2 D_\gamma + N^2 DD_k)$$

This equal to zero if and only if

$$D_\gamma \Sigma N^2 = -DD_k \Sigma N^2$$

or (C-19)

$$D_\gamma = -DD_k$$

where $D_\gamma = \dfrac{1}{k + \gamma} - \dfrac{1}{\gamma}$

$$D_k = \frac{1}{k + \gamma} \tag{C-20}$$

$$D = \mathrm{LOG}\left(\frac{k + \gamma}{\gamma}\right)$$

Making the substitutions we get

$$\frac{1}{k+\gamma} - \frac{1}{\gamma} = \mathrm{LOG}\left(\frac{k+\gamma}{\gamma}\right)\frac{1}{k+\gamma}$$

$$1 = \mathrm{LOG}\left(\frac{k+\gamma}{\gamma}\right) + \frac{k+\gamma}{\gamma} \qquad \text{(C-21)}$$

$$\frac{k+\gamma}{\gamma} = 1$$

Therefore, S_k is equal to zero if and only if $k = 0$. But this is precisely the other scaling constraint, i.e. $\psi(\$0) = 0$. However, with just the $\psi(0) = 0$ constraint and estimating b and γ, we would get the earlier result where the sum of squared residuals converging to zero as γ converged to infinity. Estimating LOG then by (C-2) leads to either γ converging to infinity or $\hat{\gamma}$ varying as the scaling constant k varies.

The alternative is to use the error in response analogue. In Spetzler's case the decision maker responds with the probability of success that makes him indifferent between accepting or rejecting the investment. If the decision maker responds with error, the resulting regressing the equation from (C-1) becomes

$$P_{s_i} = \frac{\psi(0) - \psi(X_{f_i})}{\psi(X_{s_i}) - \psi(X_f)} + u_i \qquad \text{(C-22)}$$

In the case where $\psi(y) = \mathrm{LOG}(y + \gamma)$ equation (C-22) becomes

$$P_s = \frac{\mathrm{LOG}(\gamma) - (\mathrm{LOG}(X_f + \gamma)}{\mathrm{LOG}(X_s + \gamma) - \mathrm{LOG}(X_f + \gamma)} + u$$

$$= \frac{\mathrm{LOG}\left(\frac{\gamma}{X_f + \gamma}\right)}{\mathrm{LOG}\left(\frac{X_s + \gamma}{X_f + \gamma}\right)} + u \qquad \text{(C-23)}$$

Estimating by an error in response model should provide much better results.

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