"Health and Environmental Benefits of Reduced Pesticide Use in Uganda: An Experimental Economics Analysis"

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(Abstract)

Two experimental procedures are employed to value both health and environmental benefits from reducing pesticides in Uganda. The first experiment, an incentive compatible auction involves subjects with incomplete information placing bids to avoid consuming potentially contaminated groundnuts/water in a framed field experimental procedure. Three experimental treatments (information, proxy good, and group treatments) are used. Subjects are endowed with a monetary amount (starting capital) equivalent to half the country's per capita daily income (in small denominations). Two hundred and fifty seven respondents are involved in a total of 35 experimental sessions in Kampala and Iganga districts. Tobit model results indicate that subjects place significant positive values to avoid ill health outcomes, although these values vary by region, by treatment and by socio-economic characteristics. Gender differences are important in explaining bidder behavior, with male respondents in both study areas bidding higher to avoid ill health outcomes than females. Consistent with a priori expectation, rural population's average willingness to pay (WTP) to avoid ill health outcomes is lower (by 11.4 percent) than the urban population's WTP possibly reflecting the poverty level in the rural areas and how it translates into reduced regard for health and environmental improvements. Tests of hypotheses suggest (i) providing brief information to subjects just prior to the valuation exercise does not influence bidder behavior, (ii) subjects are indifferent to the source of contamination: WTP to avoid health outcomes from potentially contaminated water and groundnuts are not significantly different, and (iii) the classical tendency to free-ride in public goods provision is observed, and this phenomenon is more pronounced in the urban than the rural area.

The second experimental procedure involved 132 urban respondents making repeated choices from a set of scenarios described by attributes of water quality, an environmental good. Water quality is represented by profiles of water safety levels at varying costs. Analysis using the conditional (fixed effects) logit showed that urban subjects highly discount unsafe drinking water, and were willing to pay less for safe agricultural water, a result not unexpected considering that the urban population is not directly involved in agricultural activities and thus does not value agricultural water quality as much as drinking water quality. Results also show that subjects' utility increases with the cost of a water sample (inconsistent with a downward sloping demand curve), suggesting perhaps that they perceived higher costs to be associated with higher water quality. Some theoretically inconsistent results were obtained with choice experiments.

Dedication

To my family

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1.0 Introduction

Chemical use is prevalent in many agricultural systems. Also prevalent is documented evidence of health and environmental risks associated with chemical exposure. A wide array of chemicals exists including fertilizers, insecticides, fungicides, herbicides and more. All are potentially harmful if incorrectly used, and can be linked to adverse human health conditions including cancer, reproductive disorders, birth defects and more. The risk posed by extensive pesticide use in particular has generated concern and as a response, organic farming and Integrated Pest Management (IPM) are now being promoted and incorporated into national agricultural policies in many countries. In addition, a heightened awareness of the need for a cleaner environment has triggered the establishment of many programs geared to ensuring sustainable agricultural practices. How sound these practices are is dependent on the nature and magnitude of economic, health and environmental benefits accruing from them.

Studies on economic impacts of reduced chemical use are relatively common, but not many exist on health and environmental impacts, and as such, the value of health and environmental benefits due to reduced pesticide use is still largely unknown.[1] These impacts are unknown for a reason – their evaluation is very difficult because of the complex and wide range of health and environmental variables involved, coupled with the non-market nature of these parameters. The lack of markets for health and environmental services means that unlike man-made products, they are not explicitly priced, so that their monetary values cannot be readily observed.[2] Nonetheless, difficulties in quantifying benefits that can be derived from reduced pesticide use should not be used as an argument for abandoning attempts to do so. It is imperative to derive credible estimates of people's values in contexts where there are either no apparent markets or very imperfect or incomplete markets. Monetary expressions of value provide a generally acceptable method of comparison among programs, and act as a basis for policy formulation. This study examines the magnitude of health and environmental benefits accruing from pesticide-use reduction using the most current non-market valuation techniques in a developing country setting, Uganda.

[1] Obviously there exists a trade-off between potential economic advantages of responsible pesticide use and the potential disadvantages of pesticide poisoning.

[2] Pesticide-intensive agriculture may extract a high price from society – most of which is not valued using standard economic surplus models.

1.1 Problem Statement

Many countries' agricultural production, disease vector control in public health and animal husbandry are dependent on pesticide usage. In agricultural production, pesticides often account for a significant share of total variable costs and a sizeable portion of farmers' budgetary expenses. In Tanzania for example, pesticides represent about 90% of the cost of purchased inputs for coffee (Ngowi, 2002) which covers over 250,000 ha of the country's cultivable land. In Uganda, widespread pesticide use is due to an equally widespread occurrence of insects and diseases on many crops and livestock, facilitated by a warm humid climate throughout the year. In order to control these pests, many farmers spray their crops with pesticides as they are regarded as a fast-acting alternative to cultural pest control methods.

Unfortunately many of the chemicals do not meet internationally accepted toxicity standards. The Food and Agricultural Organization (FAO) estimates that over $300 million is spent annually by developing countries on pesticides that are highly toxic to humans and damaging to the environment. During the period 1993-94, FAO estimated about 100,000 tonnes of pesticides were applied in developing countries, with 20,000 tonnes in Africa. This figure has been revised to an estimate of over 120,000 tonnes of obsolete and potentially dangerous pesticides used in Africa alone (FAO, 2004).

In the developed world, laws and regulations regarding pesticide use are relatively more stringent, requiring adherence to strict guidelines of proper pesticide handling in order to reduce risks associated with them. Elsewhere these laws are either non-existent or ignored and environmental pollution due to pesticides is likely to continue unabated. Even registered and approved chemicals may be subject to abuse and misuse such as adulteration, dilution or using field pesticides during post-harvest storage. Unregistered pesticides pose an even greater risk because they are potentially more dangerous, but also since they are illegal, mixing instructions and labels are often deliberately removed. Moreover, because the market price is often deemed too high, farmers opt for cheaper low quality chemicals, or prefer purchasing from vendors instead of licensed shops. All these factors increase the potential risk from pesticides posing a threat to human health and other living organisms.

Yet, in the developing world it is not likely that these problems will end soon. In fact world wide calls for the phasing out of obsolete and environmentally less friendly pesticides should be less effective in the developing countries for a number of reasons. First, illegal trade still abounds. Illegal trade offers its participants greater gains than costs and is hence profitable. Furthermore, there doesn't seem to be any other alternative to control the rampant pest situation and the subsequent losses that farmers experience. When such desperation sets in, affected farmers may turn to excessive pesticide use. In addition, developed countries (who are the largest pesticide manufacturers) have not kept their agreements and continue to dump obsolete chemicals in the developing world, sometimes even as donations past their expiration date (FAO, 2004).[3]

Clearly then, the best alternatives to this precarious pesticide situation may be two options: avoiding pesticides altogether and using strictly non-chemical pest control methods, or judicious use of pesticides. In Uganda, the IPM and Peanut CRSPs (Collaborative Research Support Programs) are choosing the second option, by introducing pest management packages that reduce reliance on pesticides to control major pests on specific crops in collaboration with the International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) and other international research organizations. Groundnuts are among the crops focused on in Uganda by these projects. Groundnuts are the second most widely grown legume in the country. The Ministry of Agriculture estimates that 283,000 ha were planted and 219,000 mt harvested in 2005/06 (Uganda, 2007). However, because the crop is affected by a host of pests and diseases, some of which could cause close to 100% yield loss, frequent spraying occurs. Notable among the diseases are groundnut rosette a virus transmitted by aphids, and cercospora leafspot a fungal disease. Pest management packages developed for reducing the need for pesticides on groundnuts in Uganda consist of three integrated cultural practices: altering planting time and planting density and developing host resistance. Economic benefits of these packages such as increasing yields and farmer profits have been documented (CRSP Annual report, 2002). However benefits to the environment to which these research programs contribute are not quantified.

Most studies that measure the impact of reduced pesticides in the environment state findings in qualitative terms: reduced risk of human and animal exposure to toxic chemicals; preserved species diversity; reduced runoff and leaching potential, hence less ground and surface water contamination; reduced fish poisoning and preservation of beneficial insects. A departure from this is Kovach's widely known Environmental Impact Quotient (EIQ) study

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[3] In 1986 a 170 cubic meter storage shed of such a donation collapsed in Tanzania leaving high concentrations of pesticide residue in the soil (Kishimba et al., 2004). Moreover to destroy existing stocks of such pesticides requires large sums of money and may necessitate funding from financial institutions (FAO, 2004).

which expressed impact of pesticides on the environment by scoring their effects on a set of environmental categories (Kovach et al., 1992). Using EIQ numbers, Kovach was able to compare impacts on the environment of different pest management options (based on amount of active ingredients in pesticide formulations), with small numbers indicating less impact on the environment. However, weights across categories are arbitrary, and no monetary values were associated with these impacts.

To evaluate the impacts of a pesticide-use reducing program on the environment, the changes in quality of health and the environment can be expressed in monetary terms. Non-market valuation methods are important in translating such impacts into monetary terms. Expressing benefits in monetary terms is a convenient means of expressing the relative values that society places on different uses of resources.

Quantifying health and environmental improvements is complex, probably because no single parameter can perfectly represent the environment thus making it hard to capture the magnitude of benefits in one measure. The Contingent Valuation survey method (CV) has been suggested and applied as one means for valuing health and environmental benefits (Higley and Wintersteen, 1992; Mullen et al, 1997; Cuyno et al., 2001; Brethour and Weersink, 2001). This method has however received criticism, due to several potential biases including vehicle, strategic, hypothetical, starting point, and information biases. Each of these biases will be explained in detail later.

Monetary-based measures proposed such as the Environmental Impact Level (EIL) and Cost Of Illness (COI) were found inadequate (Berger et al., 1987). The EIL is limited by its field-based context and its focus on only environmental but not health parameters. Measures such as the COI are often not a viable option in developing countries where many illnesses go unreported, and where the physical condition could be affected by many factors and cannot be directly traced to pesticide exposure or toxicity.

Recent developments in research for non-marketed goods suggest experiments as the appropriate methods for valuing improvements in the environment and health. The choice experiment valuation method uses people’s stated preferences and is based on the idea that individuals derive utility from the characteristics of goods rather than directly from the goods themselves. Choice experiments have been used to value non-market goods in transportation, air quality, architectural designs and only recently, the environment and health (Lusk, and Schroeder, 2004; Hanley et al., 2001; Ryan et al., 2001; Hanley et al., 1998; Boxal et al., 1996). Choice experimental methods have potential advantages over other stated preference methods in that health and environmental attributes can be varied in an experimental design allowing respondents to make repeated choices between those attributes which is an indication of the value of those attributes.

Another experimental method – experimental auctions where subjects place values on environmental improvements in a researcher-controlled setting and subjects are obligated to pay these values, is commended widely by experimental economists for its non-hypothetical nature producing more realistic responses. Using this method a link is maintained between subject behavior and outcomes (salience) (Freeman, 2003a). Experimental auction methods are also becoming accepted as a technique that can provide measures of economic choice-making that may be more accurate than those provided by surveys. Experiments are now commonplace in industrial organization, game theory, public choice, finance, most microeconomic fields, and some aspects of macroeconomic theory, and their use is rising (see a review by Maupin, 2006).

With experimental methods, economists are no longer content merely to observe. They can easily control important factors and *then* observe behavior. In these experiments, for the reliability of responses as economic data, agents are economically motivated based on induced value theory and then their behavior is observed.

1.2 Justification

Reduced pesticide use may have both health and environmental benefits to Uganda, and quantifying these benefits is important for policy formulation. Moreover because of the importance of groundnuts in the Ugandan economy, and the wide area on which the crop is grown, the benefits of reduction in pesticide use due to pest management programs on groundnuts may be large, as these benefits translate into reduced social costs for a large population over a wide geographical area.

In developed countries, there is a heightened awareness of the benefits of a clean environment. Non-profit organizations that advocate environmental preservation are currently receiving increased recognition. Legislation for national allocation of more funding for activities aimed at enhancing natural resources is increasingly being passed in many developed countries. The developing world should not be left behind. Contrary to popular belief, poor populations may in fact value their environment, and their environmental interest could be increasing too. It is of interest to know how and to what extent they do value their environment.

Currently, studies quantifying health and environmental impacts of various programs are becoming necessary to pinpoint those with harmful impacts (to be avoided) and those with beneficial effects (to be enhanced). Such impact studies are increasingly becoming an important condition for project funding by some key aid organizations.[4] Nonetheless, a credible concern is: Do individuals appreciate such programs? In January 2007, the Ugandan ministry of health endorsed the use of DDT for internal residual spraying (IRS) as a public health vector control mechanism against mosquitoes (The New Vision, 2007, The Daily Monitor, 2007). The use of this and other chemicals in agriculture and vector control poses health and environmental risk due to the chemicals' persistence in the environment and heavy accumulation in animal body tissues (see chapter 2). While the international community and non-governmental organizations have raised concern over these chemicals, not much is heard from the local population about this issue. The question is: Do low income populations care about their environment? What value do individuals attach to an environmental amenity that is free from pesticide contamination? What are people's willingness to pay to avoid environmentally degrading situations?

1.3 The Pesticide Situation in the Study Area

The majority of Uganda's population works either directly or indirectly in the agricultural sector. Countrywide, most agricultural activity takes place on farms averaging 0.9 ha, although average farm size varies by region and has been declining over the years (Uganda, 2007). Figures on pesticide use are not readily available, and where available, they are mostly rough estimates. Reasons for this situation are varied: It is costly to establish active ingredients used, as most chemicals are mixed into concoctions to provide a formulation known only to the applicator; many chemicals are stored for long periods such that any inventory estimation would be inaccurate; and the illegal trade and sale of banned pesticides mentioned earlier means that that portion of the market is not documented. Missing information about pesticide use in most cases translates into under-estimates. For

[4] The United States Agency for International Development (USAID), one of the leading donor agencies made it a necessary condition that, especially for large projects environmental impact assessments be done before they are funded (Ecaat, 2004). The Peanut CRSP and IPM CSRP are such projects funded by the USAID.

instance Uganda is currently reported among those with the lowest pesticide usage rate at only 17kg/ha.

However, even if these low usage rates are correct, handling and storage procedures may be more important factors when attempting to estimate numbers affected by pesticide exposure at household and national levels. Open pesticide containers can be found stored in kitchens, as well as in market places. Used empty containers are sometimes used as measuring devices for food and other items in markets and on farms. In fact used containers were given out to farm workers on large estate farms as a work incentive (although this practice is declining due to recent environmental awareness and education about possible dangers). Forty four pesticides are currently registered for use in Uganda (Kegley, Bill and Orme, 2007). However many more find their way into the country and into farm shops, farms and eventually into the environment.

Groundnut farmers use a variety of these pesticides to control a host of pests ranging from pre-harvest termites to post-harvest storage pests. While many kinds are contact pesticides, depending on the pest, also in use are systemic pesticides, which are potentially more risky because they penetrate living tissue. With 80% of the country's population involved in agriculture, the 3% estimate by Jeyaratnam (1990) of all agricultural workers in developing countries who are affected by pesticide poisoning each year would translate into well over 700,000 cases in Uganda annually.

The current situation is such that farmers continue to use pesticides, although they have an option to stop, or at least reduce spraying and benefit from reduced chemicals in the environment. A goal of this research is to find out what attributes of a pesticide free environment people in a developing country setting most value and how much people are willing to pay to regain a cleaner environment and better health due to reduced pesticide use.

1.4 Objectives

The overall objective is to establish health and environmental impacts of reduced pesticide use in Uganda. The specific objectives are:

i) To establish the value people place on their health and the monetary trade offs they make in avoiding ill health outcomes;

ii) To establish which attributes of the environment people value most, and

iii) To explain variations in willingness to pay across individuals and situations (grouping, information and proxy good).

1.5 Hypotheses

Testable hypotheses in this study include the following:

i) In valuing health and environmental benefits, individual self-interest is at odds with group/social interest, thus free riding will be observed;

ii) More informed economic agents are able to make informed decisions about avoidance of ill health outcomes accruing from pesticide contamination, thus the amount of information provided will affect bidding behavior;

iii) Gender differences will affect bidding behavior;

iv) Different proxy environmental goods will result in different bids;

v) More formally educated individuals have higher willingness to pay to avoid contamination;

vi) Rural and urban populations differ in their valuation of health and the environment, and;

vii) Previous exposure to harmful effects of pesticides makes subjects more likely to bid high values.

1.6 Contributions of Study

The methods employed in this study are relatively new and attractive for non-market valuation, and the literature on their use is increasing exponentially (for example the work by List (2007) and the reviewed works therein). This study makes four additions to the non-market valuation literature in Uganda:

- To date, no study valuing health and environmental benefits had been conducted using experimental auctions in Uganda. The application of this method using the random *n*-th price incentive compatible auction procedure is the first in Uganda.
- A number of studies 'valuing environmental quality' or 'environmental improvements' using choice experiments are available in the non-market valuation literature. Most are concerned with air quality, aesthetic qualities, wildlife, hunting and so forth. None of these environmental valuation studies have used choice experiments to value the environment as it directly relates to human health. In the health economics literature, choice experiments have been applied to obtain willingness to pay to avoid various illnesses. However, none of the studies has applied choice experiments for WTP to avoid ill health specifically due to pesticide contamination.

- Conducting field experimental auctions with adult non-student decision making agents represents a departure from the mostly student-subject based studies in many experiments. This study's use of non-student subjects in experiments outside of the laboratory is an addition to the growing literature on framed field experiments (FFE) (see List, 2007).
- Using "public goods" experiments that are more realistic than abstract. Public goods experiments in the literature assume a link between a willingness to pay for a public good and "donations" or "contributions" to a given public good. In my view this link is often weak (see 2.4.1.2).

Each of these contributions will be explained in more detail in subsequent chapters.

1.7 Summary of Methods

The evaluation procedure in this study employs two field-based experimental protocols to elicit willingness to pay for health and environmental improvements using the same sample of respondents. The first – the incentive compatible experiment involves subjects placing bids of the amount they are prepared to pay to avoid a bad health situation in both the individual and group (public goods game) setting. Subjects reveal their true demand for good health through a random *n*th price auction, and subjects witness the outcome of their valuation. In the second experiment, environmental attributes are varied in a factorial experimental design that allows respondents to make repeated choices from profiles of environmental attributes they prefer at a specified price. The analysis is based on random utility theory and the characteristics theory of value. Maximum likelihood estimation is used to explain behavior.

1.8 Organization of Dissertation

This chapter has provided the general introduction to the study, objectives and hypotheses. The next chapter gives a review of literature relevant to the study, and includes a discussion of relevant theory. Chapter three presents the methods of data collection and analysis. Chapter four presents the results and a discussion of findings. The final chapter summarizes the study, presents conclusions, policy recommendations, and suggests future research directions.

2.0 Background

This chapter reviews literature regarding the subject of pesticide use, types and effects; methods of valuation of goods including health and the environment and ways of eliciting value information. The last sections of this chapter focus on two experimental methods in detail.

2.1 Types and Classification of Pesticides

The US Environmental Protection Agency (EPA) defines a pesticide as a substance or mixture of substances intended for preventing, destroying, repelling or mitigating a pest (U.S. EPA 2007). These substances could be industrial chemicals or biopesticides. Biopesticides are those in which the active ingredient is biologically based such as protozoa or bacteria or a natural product derived from plant sources. Most literature on the harmful effects of pesticides is in regard to industrial chemical pesticides. Some point to the harmlessness to non-target organisms of biopesticides (such as Bt) and botanicals (such as neem), in order to promote biological over industrial chemical control methods (JinJie and JingYuan, 2000). Pesticides include any substances that can control, repel, attract or disrupt a pest. They can be classified in many ways:

- By local trade name or commercial name;
- By form such as granular, dust (powder), liquid, emulsion, aqueous solution, or as dry solid compound;
- Using warning words such as Danger, Poison, Caution;
- By WHO hazardous classification: Class Ia (extremely hazardous), Ib (highly hazardous), Class II (moderately hazardous), and Class III (slightly hazardous); O (Obsolete as a pesticide), and U (Unlikely to present acute hazard in normal use);
- By chemical group and active ingredient; such as organophosphates, carbamate pesticides, organochlorine insecticides and synthetic pyrethroids, with several subgroups within each of these;
- By type of effect, such as toxicological effects (such as acute toxicity, chronic toxicity), ecological effects (such as breakdown in soil and groundwater, in water and in vegetation), which may further be categorized as slight, moderate or high effects; and

- By type of pest they control; The EPA describes 24 types of pesticides grouped according to the pests they control (Table 2.1).

Table 2.1: Classifications of Pesticides by Type of Pest Addressed

Name	Action/ Pest(s) addressed
Algicides	Algae in water bodies such as lakes, canals, swimming pools, water tanks.
Antimicrobials or biocides	Microorganisms (such as bacteria and viruses).
Attractants	Attract pests (for example, to lure an insect or rodent to a trap).
Bactericides	Bacteria
Disinfectants and sanitizers	Kill or inactivate disease-producing microorganisms on inanimate (non-living) objects.
Fungicides	Fungi (including blights, mildews, molds, and rusts).
Fumigants	Produce gas or vapor intended to destroy pests. Especially used in buildings or soil.
Herbicides	Weeds and other plants that grow where they are not wanted.
Insecticides	Insects and other arthropods.
Microbial pesticides	Microorganisms that kill, inhibit, or out compete pests, including insects or other microorganisms.
Molluscicides	Snails and slugs.
Nematicides	Nematodes (microscopic, worm-like organisms that feed on plant roots).
Ovicides	Eggs of insects, reptiles, birds and mites.
Pheromones	Biochemicals used to disrupt the mating behavior of insects.
Repellents	Repel pests, including insects (such as mosquitoes) and birds.
Rodenticides	Mice and other rodents
Defoliants	Cause leaves or other foliage to drop from a plant usually to facilitate harvest and/or prevent pests and diseases
Insect growth regulators (IGR)	Disrupt life processes of insects.
Plant growth regulators (PGR)	Substances (excluding fertilizers or other plant nutrients) that alter the expected growth, flowering, or reproduction rate of plants

Source: U.S. EPA (2007)

2.2 Impacts of Pesticides on the Environment

The problems associated with estimating effects of pesticides in the environment are associated with three important elements: synergism, uncertainty and latency. The effect of a brief exposure of chemicals may not be readily noticeable because of the latency period between exposure and development of systems. The effect of this situation could be more dangerous especially in developing countries where poor diagnosis and delayed treatment could lead to high fatality and lifelong disability rates associated with pesticide poisoning.

The toxicity of some insecticides (such as pyrethrin) can be increased several times by the addition of other compounds which themselves may not be insecticides and as such health effects may be greater than the sum of the effects of the individual chemicals. This means that evaluating the effect of pesticides based on the amount of active ingredient (as in Kovach et al., 1992; Brethour and Weersink, 2001; Mullen et al., 1997) may be inaccurate. Estimating the effects on the basis of active ingredients assumes away this synergistic characteristic of pesticides. Perhaps the most important problem of estimating pesticide effects is related to uncertainty. Individuals are not certain which compounds/chemicals/ pesticides are involved (either because chemical labels are missing or because of general lack of information).

What comprises the "environment" is the totality of all surrounding conditions, including water, air, humans, animals, plant species, birds, insects and aquatic life. The term in itself is broad and covers many elements, both living and non-living although its preservation is mostly geared to conserving the living component, in addition to conserving the quality of water, air, and the general existence of life. In general humans interact both actively and passively with the environment in a cause-and-effect manner. Active interaction occurs when humans alter the environment say through contamination of water sources, depletion of the ozone layer, air emissions from factories and emissions by humans of high levels of particulate matter into the air.

Passive interaction includes inhalation of polluted air, skin contact, and ingestion of food and water that may be contaminated, all of which can impact the health of the population. Pesticide release into the environment can lead to such passive interaction. One of the basic services provided by the environment is the support of human life. Changes in the life-support capacity of the environment such as though pollution of air or water can lead to increases in the incidence of diseases and impairment of daily activities (Freeman, 2003a)

Active ingredients in common pesticides such as organophosphates and Carbamates interfere with biological systems of organisms by affecting the central nervous system. Most organophosphates were developed as insecticides but their effects on humans, were not discovered until 1932 (U.S. EPA, 2007). Their persistence in the environment however is limited (Ngowi, 2002), which can be a desirable property. The class of organochlorine insecticides including compounds such as DDT and Chlordane have mostly been removed from the legal market (U.S. EPA, 2007) because they persists in the environment and have dangerous long-term effects on humans and other organisms.

A number of qualitative studies have found strong links between pesticide exposure and negative health effects of living organisms including humans, animals, birds, wildlife, insects and fish (Ngowi, 2002; IPM DANIDA, 2004; PANNA, 2004). In addition, adverse effects are correlated with high levels of pesticide toxicity. In humans documented evidence shows that pesticides are linked to several medical conditions some of which, although not fatal in adults, are related to deaths in infants and unborn babies (Taha and Gray, 1993; Heeren et al., 2003). Those that are not fatal can lead to impaired human activity and improper functioning. Eco-toxicity occurs when pesticides release their toxic effects on other environmental categories including soil organisms, birds, mammals and aquatic life. Moreover, there is growing concern of development of pest resistance to pesticides (Coleman and Hemingway, 2007), which may cause impacts that are yet to be known. Most pesticides have similar effects on insects, animals and humans. The effect may be acute (experienced after immediate exposure) or chronic (resulting from prolonged exposure to toxic compounds). Because many other conditions can cause these diseases, it becomes hard to identify them as strictly being caused by pesticide exposure and toxicity (IPM DANIDA, 2004). A review of the effects of pesticides on specific environmental components follows.

2.2.1 Ground- and surface water quality

Ground water, water that percolates into the water table, supplying wells and springs is a major source of drinking water. Hence any contamination of ground water through pesticide leaching is likely to end up in drinking water. Surface water on the other hand is water from sources open to the atmosphere, such as rivers, lakes, reservoirs, wetland areas and seasonal water bodies. The main source of contamination for surface water is run-off and settling pesticide drift. Parameters of concern relating to pesticide water contamination include water solubility (a measure of how readily pesticides dissolve in water), adsorption coefficient (K_{OC}), (a measure of how strongly a chemical adheres to soil in preference to remaining dissolved in water), hydrolysis half-life (the amount of time required for half the pesticide to degrade from reaction with water), and soil half-life (the amount of time required for half of the pesticide to degrade in soil) (Kookana and Correll, 2002; PANNA, 2004). Studies in soil physics have placed thresholds for various chemicals concerning these parameters. For instance any pesticide with a water solubility greater than 3mg/l, or anaerobic soil half-life greater than 9 days has potential to contaminate groundwater (PANNA, 2004). Most organochlorine and organophosphate pesticides have high potentials to contaminate ground water based on all four of these parameters (Kookana and Correll, 2002; PANNA, 2004)

Significant levels of pesticide residue in water have been found by many studies including one by Mwevura (2004) testing for pesticide residues in rice fields in Tanzania. He found DDT, Dieldrin, Hexachlorocyclohexane (HCH), fenitrothion and diazinon in large enough amounts to cause concern over bioaccumulation through the food chain and possible chronic effects. Another study around the Lake Victoria shore found high levels of organochlorine residues including endosulfan, dieldrin and other chemicals (Getenga et al., 2004). Lake Victoria provides much of the drinking water for populations in East African countries.

2.2.2 Birds and Beneficial insects

Two major ways in which birds acquire pesticides, all of which are passive, include (i) when they consume insects and seeds that have pesticide residue, and (ii) when they drink from contaminated water sources. As a result, birds can die in mass numbers, migrate from one location to another, or experience disruption in their mating behavior. All these may result in reduced bird populations. Beneficial organisms such as earthworms (important for soil aeration), bees (pollination) and beetles (biological pest control agents) get exposed to pesticides mainly through feeding on affected leaves and plant parts or through ingestion of water with pesticide run-off, and pesticide leached through the soil. The effect of pesticides on beneficial insects is similar to that on birds and may include accumulation, change in behavior, intoxication, reproductive disruption and mortality. A recent study (Fox, 2004) has established marked increases in bird populations following a period of reduced pesticide use in Denmark, suggesting an inverse relationship between pesticide use and bird numbers.

2.2.3 Aquatic life

Fish and other aquatic forms of life are affected directly through the contaminated water in which they live. Harmful effects of pesticide exposure and toxicity in the most serious form, include death of various species which could lead to species extinction, or the effect may be less adverse such as disruption in mating and reproductive behavior, migration, reduction in fish size, or deposition of residue in body tissue. Human consumption of such fish may have effects such as those described in d) below especially with high pesticide residue concentrations. Significant levels of DDT, endosulfan sulfate and HCH residue found in three major fish species in Lake Victoria could lead to deaths and possibly extermination of those species (Kyarimpa et al., 2004). Presence of Propanil residues in soil long after the application season has been linked to fish kills or disappearances of saltwater fish (Mmoshi and Kishimba, 2004).

2.2.4 Humans and other mammals

Routes of human exposure to pesticides include skin contact (dermal exposure); ingestion of contaminated food and water (oral exposure); or through breathing in of chemical droplets through drift (inhalation exposure). Farm animals get exposed through feeding on contaminated plants and possibly when they come in contact with contaminated farm equipment (such as spray pumps). The main source of wild life pesticide contamination is through preying on other affected animals or by feeding on pesticide-contaminated food. Several health conditions are linked to pesticides including immune system disorders (immunodysfunction), the production of changes in DNA sequence that affects the expression of genes (mutagenesis), the capability of harming the nervous system (neurotoxicity), the ability to cause cancer (carcinogenesis), and the ability to produce non-heritable birth defects in babies (teratogenesis). All these conditions impair life's activity and productivity and at worst lead to death (Kookana and Correll, 2002).

Perhaps the biggest concern about pesticide effects is the danger they pose to children, infants and the unborn. Such individuals are particularly at risk because their bodies have not developed enough resistance and also because of their lifestyle (picking up and eating anything they come in contact with). Both factors render them more vulnerable to the adverse effects of pesticides. This concern is significant because the danger of pesticides gets bestowed on both the current and future generations. A large-scale study in India found evidence that children living in regions of intensive pesticide use may be at risk for impaired mental development (PANNA, 2004). A similar study in Mexico also noted dramatic deficits in brain function in rural children with long-term exposure to pesticides in the Yaqui Valley (PANNA, 2004).

The exact number of pesticide poisonings may be hard to estimate. Moreover the nature of many pesticide illnesses in developing countries complicates pesticide-poisoning identification. Many people may not realize that they are suffering from pesticide poisoning, and instead think they have some common illness. In 1990 a joint WHO and UNEP study estimated 3.0 million human pesticide poisonings globally, 99% of which occurred in developing countries (Schlosser, 1999).[5] Yet, even these estimates can be considered a lower bound. A study in Thailand established that only 2.4% of all who suffer pesticide poisoning go to the hospital or report the incident. Jeyaratnam (1990) established that for every reported case, ten remain unreported. For the period 1989/90 a total of 736 pesticide-poisoning cases were reported in Tanzania, while in Sri Lanka and Malaysia 7% of all agricultural workers were poisoned by pesticides (Ngowi, 2002). Over 50,000 cases of cancer are estimated to occur annually due to pesticide exposure in Thailand alone (IPM DANIDA, 2004). Pesticides can accumulate in the body. Several studies indicate varying percentages of people with pesticides in their bodies. The Ministry of Public Health in Thailand established that over 18% of the 2 million tested blood samples contained measurable amounts of pesticides (IPM DANIDA, 2004). In other countries the situation may not be any different. A recent study in the US established 100% of all tested blood samples (see PANNA, 2004) contained high pesticide levels. In some, traces of pesticides were measured in blood more than 10 years after exposure.

2.2.5 Conclusion on Impact of Pesticides

In conclusion, pesticides may have several effects on various organisms depending on the chemical (type, amount, toxicity, synergistic relationships) and on the nature of the organism itself. Pesticides may reduce numbers of organisms through their lethal effects and through migration (such as of birds and fish species). They also change the well being of organisms through sickness, reduced organism size and impaired reproductive capabilities. As such pesticides can have both quality- and quantity-degrading effects on humans and other environmental components. These adverse effects occur possibly because most pesticides are not fully selective to the target organisms. As Hayo and Werf (1996) note, depending on the application method, the percentage of chemical reaching the target organisms can be as low as 0.2% thus leaving a greater percentage reaching and causing danger to non-target organisms in the environment.

[5] A big problem with pesticide poisoning data though is that many poisonings are intentional thereby inflating global fatal pesticide figures.

2.3 Economic Value and Valuation Methods

The economic theory of value is based on the ability of goods and services to increase the well-being or utility of individuals (Freeman, 2003b). Because goods and services are limited both in quantity and quality compared to their unlimited uses, increasing the provision of one good involves an opportunity cost as it requires decreasing provision of the other. It is these trade-offs that people make as they choose less of one good, substituting more of another that reveal the economic value of goods (Freeman, 2003b).

The two well known measures of economic value include compensating variation and equivalent variation. Both measures are based on the concept of substitutability introduced above. The compensating variation for an environmental improvement is the maximum sum of money that the individual would be willing to pay rather than do without the improvement. It is a compensating payment that leaves the consumer as well off as before the economic change. This measure is referred to as willingness to pay (WTP). On the other hand the equivalent variation is the minimum amount of money that an individual would require to forego the improvement. Or equivalently stated, the amount of money that would generate an increase in utility equivalent to that realized from the improvement in the environmental amenity. This measure is referred to as willingness to accept compensation (WTA).

The major difference between these two measures lies in their assumptions. The equivalent variation measure of value assumes that individuals have the right to a new, higher level of environmental quality and must be compensated if the new level of environmental quality is not attained. The compensating variation measure on the other hand assumes that an individual has no right to the improvement and has to pay to obtain it. In addition, the WTP is constrained by individuals' incomes while with WTA, there is no such limitation. For these reasons WTA and WTP values of the same good/service may differ, and the idea is to use one that best approximates the situation that is being valued (Freeman, 2003b).

As mentioned above, the economic value of an environmental resource resides in the contributions that the variety of environmental functions and services make towards the wellbeing of individuals. Evaluation of environmental services is often stated as simply a means of measuring preferences for environmental resources and not a valuation of those resources themselves (Pearce and Seccombe-Hett, 2000). However, to understand how health and environmental values may be obtained, one must first understand the type of goods, and the nature of values such goods have. These two aspects are necessary in explaining the underlying complexity of valuing health and environmental benefits.

2.3.1 Private and Public Goods; Use and non-use Values

Private goods' consumption by an individual removes them from a set of choices available for consumption by other individuals (excludability) and when consumed, the amount of the good available for consumption by others is reduced (rivalry). Valuing private goods using standard economic techniques is relatively easy because markets for such goods exist and valuation involves obtaining willingness to pay estimates for goods and services that are bought and sold between buyers and sellers who agree on the price of the good or service between them. Estimating the value of goods and services is done using observed (revealed) data of actual consumer preferences including prices, quantities and costs. The assumption is that individuals make the choice of price under rational behavior before exchange takes place.

When goods are not freely traded in the market, such as the case with public goods, this standard framework fails. There are no well-established price mechanisms, and measurement of value becomes complex. Public goods are non-rivaled and non-excludable (once produced, many people can consume them at the same time and there is no systematic way of limiting others from consuming them). Extremes of excludability and rivalry where a good is both strictly non-excludable and non-rival pertain to pure public goods.

The total value (or benefits) of a good or service may be composed of use (active), and non-use (existence) values. Use values are those that people place on things because they will use them. Non-use value on the other hand is the utility obtained from an amenity for reasons other than their expected personal use (Mitchell and Carson, 1989). Non-use values include bequest value (a value placed on things for preserving them for future generations or the value that people place on knowing that future generations will have the option to enjoy an amenity), altruistic value (value that someone gets out of knowing that other people, including those alive today and future generations will be able to enjoy an amenity), and option value (the value that people place on having the option to enjoy something in the future, although they may not currently use it).

These different forms of value can be obtained from sources other than the market. And possibly because there is no direct or all-inclusive method for their valuation, a large body of literature exists on methods used for estimating their value. Also because of their nature, controversy exists on the reliability of these non-market values. Many methods are proposed for valuation of a specific non-market good. Others are modifications of previous 'versions' of some method. In the next section, the most relevant non-market valuation methods are reviewed.

2.3.2 Value Elicitation

The fundamental issue in environmental quality valuation is that people are the best judges of their well-being and they form perceptions of environmental and health quality within their limitations. Obtaining people's preferences can take two observational forms: observing what choices people actually make, and observing what choices they say they would make.

Revealed preference methods are ex post in nature, constructed after observing actual behavior and then using the observed behavior to infer values of goods and services. These methods rely on data that come from observations of people acting in real-world settings where people live with the consequences of their choices. This is analogous to determining people's willingness to pay for a marketed good based on the quantity demanded at different prices. As discussed below several examples of revealed preference methods exist, including hedonic pricing, travel cost method, averting-behavior methods, cost of illness and auctions. Where markets exist, revealed preference methods are the preferred source of value information. However revealed preference methods can also be used for non-market valuation as in the case of demand-revealing experimental auctions.

Stated preference methods on the other hand involve a survey approach that allows people to express preferences for or against predefined alternatives by answering questions that reveal information about their preferences of value. Individual preferences are not observed but rather stated. Examples of stated preference methods include contingent valuation, contingent ranking and attribute based methods.

Maintaining that a major problem with revealed methods is their inability to measure non-use values, proponents of the stated preference methods believe that stated preference methods can be used to generate data that is related to market behavior and thereby providing the only viable alternative for measuring non-use values (Adamowicz et al., 1994; Pearce and Seccombe-Hett, 2000; Adamowicz and Boxall, 2001). Although Adamowicz et al., (1997), Boxall et al., (1996), Adamowicz et al., 1994, and Coursey et al., (1986) show that there is no significant difference between the parameters of revealed preference and stated preference models after accounting for variance effects, they also state that the method most applicable to non-marketed goods including health and the environment is the stated preference approach.

2.3.3 Revealed Preference Methods

This sub-section reviews four major revealed preference methods including hedonic pricing, damage cost avoided methods, cost of illness and the travel cost methods. Their application in other environmental evaluation contexts is considered, as is their limitation or inappropriateness to the current problem. The fifth major revealed preference method, experimental auctions, is reviewed under the section on experimental methods.

2.3.3.1 Hedonic Pricing

The hedonic pricing method is used to infer values of characteristics of a resource based on observable market transactions. The method is used to identify price factors according to the assumption that price is determined by both intrinsic and external characteristics of the good, hence providing an estimate of value for each component. The most common example of the hedonic pricing method may be in the housing market. The price of a property is determined by the characteristics of the house such as its size, location, appearance, and condition, as well as the characteristics of the surrounding neighborhood such as accessibility to social amenities, safety, privacy, level of air and water pollution/ quality, value of other homes, taxes, and interest rates (Reiff and Barbosa, 2005).

The method has been used to estimate economic benefits or costs associated with environmental quality including air pollution (Smith and Huang, 1995), water pollution (Poor et al., 2001), or noise (Nelson, 2004) valuing environmental amenities such as aesthetic views (Morancho, 2003) or proximity to recreational sites and airports (Nelson, 2004). In Palmquist and Israngkura (1999), a 2-stage estimation procedure was employed to value air quality (measured as levels of total suspended particulate, nitrogen dioxide, ozone, and sulfur dioxide) where the first stage hedonic model included ‘property’ characteristics and the second stage estimation included information on the buyers in multiple markets. The method applied in this study can be viewed as a modification of the Palmquist-Israngkura method.

2.3.3.2 Damage Cost Avoided and Replacement Cost Methods

The damage cost avoided and replacement cost methods estimate values of environmental services based on either the costs of avoiding damages due to lost services, or the cost of replacing environmental assets. These methods are also known as the averting-behavior methods, because willingness to pay values for a non-market good are estimated using individual's purchase of market goods that avert the negative effects of the damage. These methods assume that the costs of avoiding damages or replacing natural assets provide useful estimates of the value of these assets or services based on the assumption that, if people incur costs to avoid damages caused by lost environmental services, or to replace the services of the environment, then those services must be worth at least what people pay to replace them. These measures are generally most appropriately applied in cases where damage avoidance or replacement expenditures are easier to estimate than people's willingness to pay for certain environmental services.

The damage-cost-avoided method uses the cost of actions taken to avoid damages as a measure of the benefits provided by the environment. The replacement cost method uses the cost of replacing an environmental service as an estimate of the value of that environment service. The advantage of these two methods is that they are less hypothetical than valuation methods that estimate willingness to pay and it is easier to measure the costs of producing benefits than the benefits themselves, when goods and services are not marketed. The limitation with these methods is that they assume that expenditures to repair damages or to replace environment services are valid measures of the benefits provided. This method is inapplicable in cases where costs to avoid damage due to potential pesticide contamination are unavailable.

2.3.3.3 Cost of Illness Method

The cost-of-illness (COI) method measures direct (monetary medical expenditures) and indirect costs (forgone earnings) associated with a particular illness. This approach may be a good approximation of willingness to pay for improved environmental conditions to the extent that it is based on real money spent for treatment or actual market value of work time (Hall and Brajer, 2002). However it may abstract from important components of the cost of illness. Berger et al. (1987) observes that the COI approach has several shortcomings including the fact that there are zero indirect costs for individuals who are unemployed or retired (those whose foregone earnings are zero), and the method does not include losses associated with the value of leisure time. Thus the method does not capture all of the benefits of better health. Because of these shortcomings, COI calculations are often seen as lower bound estimates of the true willingness to pay (Hall and Brajer, 2002). For instance in many countries where hospital registries are not well equipped to handle categorization of patients by cause of illness (such as pesticide poisoning) and where many cases remain undiagnosed/ unreported, COI estimations would be grossly under-valued. Thus this technique is not appropriate to Uganda.

2.3.3.4 Travel Cost Method

The travel cost method infers value of a recreational experience from the costs that individuals incur to travel to a recreational site. The method requires user participation to assign values to environmental features such as sites, or recreational services using information on user's actual behavior (assuming that individual's travel costs reflect the environmental value). Thus the value of an environmental amenity is reflected in how much people are willing to pay to get there (although critics of this method mention that this may be a function of people's income, personal interest in the type of site, or level of recreational experience and not the value of the environmental amenity per se). Because of these limitations, many suggest that this method be used in conjunction with other valuation techniques to estimate the value of recreational sites (see Randall, 1994). This method is not appropriate in the current study because the condition being valued is not a site.

2.3.4 Stated Preference Methods

In this sub-section two stated preference methods are reviewed. The first, benefit transfer, is a method of obtaining values through secondary data. The more widely employed stated preference method used for non market valuation, contingent valuation is also reviewed here. Contingent valuation may receive the most criticisms regarding its reliability. The other stated preference method, choice experiments is reviewed under the experimental methods section.

2.3.4.1 The Benefit/value Transfer Method

This method involves taking economic values obtained from a primary study in one context and then applying them to another context. It uses existing valuation data from one study – the "study site", and adjusts it to fit another study – the "policy site" (Freeman, 2003 a). The two main approaches that have been used in a benefit transfer are the transfer of the adjusted mean values and the transfer of benefit functions. To create precision in transferring benefit estimates, adjustments in WTP are made for differences in characteristics of a present study.

Benefit transfer methods are useful when the cost of conducting an original valuation is prohibitive and/or there is too little time available to conduct an original valuation study, yet some measure of benefits for policy purposes is needed. The method is reliable when the "study site" and the "policy site" have similarities in terms of environmental features being studied and the populations making use of these environmental features (such as incomes and other socio-economic characteristics). This method's reliability is also highly dependent on the quality of the study to be transferred. Ready et al., (2004) in their study in Europe using values obtained from contingent valuation found that transfer of benefits across countries resulted in substantial transfer error. For this study however this technique is not applicable because to the best of my knowledge no prior study is documented quantifying benefits from a pesticide-free environment in the country from which benefits could be transferred.

2.3.4.2 Contingent Valuation

The contingent valuation technique is used to elicit individual's willingness to pay for non-market goods using a direct survey approach. A hypothetical market is created and respondents' preferences for goods are obtained by asking them to directly state their willingness to pay. It is based on the idea that people are unable to reveal (but are able to state) their maximum willingness to pay (or minimum willingness to accept) for a hypothetical change in the level of provision of a good. Contingent valuation can be used to estimate total economic value (both use and non-use values) of non-market goods and services (Hanemann 1994), to assign monetary values to environmental improvements (Mitchell and Carson, 1989), benefits of hunting (Boxall et al, 1996) and air pollution (Desaigues et al., 2003). More recent efforts have valued risk due to nuclear pollution and toxic waste locations (Hanley et. al, 2001), cultural goods (Throsby, 2003; Santagata and Signorello, 2000) and more.

The debate regarding the appropriateness, accuracy, and reliability of contingent valuation is perhaps one of the longest in the non-market valuation literature, and much has been said in support and criticism of contingent valuation. Because CV is based on the notion of constructing a hypothetical market, clearly defining the good is essential, the type, design and form of questions matters for a reliable and realistic valuation (Coursey et al., 1986). A properly designed and pre-tested survey instrument is important.

This method's acceptance is limited by a number of factors. The principal concern is that answers to hypothetical questions regarding value may not conform to actual behavior and thus may be biased (Coursey et al., 1986). Hypothetical bias occurs because individuals are asked to place a value on hypothetical goods which they may be unfamiliar with. The hypothetical nature of contingent markets has been an issue of much concern, although in its support, Hanemann (1994) argues answering surveys may be hypothetical, but not more so than buying unfamiliar or infrequently purchased commodities.

When respondents are required to value attributes that are not familiar information bias occurs. The nature of the good or service being valued in a contingent valuation study is mostly hypothetical, and most people are unfamiliar with placing values on such goods and services. As mentioned above, this is a problem with CV surveys because the method normally assumes that people will correctly place values on hypothetical goods. The result, especially with mail surveys is a high number of zero responses (Mitchell and Carson, 1989). To deal with this problem researchers have found that providing information about the good may be important (Lusk et al., 2004). However, the amount and type of information presented to respondents may also affect valuation responses.

The other problem with asking respondents to make value statements on non-market goods is that there is no way of telling if the values obtained in a contingent market are real. Since the good or service to be 'purchased' is hypothetical, a respondent may reason that whatever value they state will not affect the status quo – after all the constructed market is hypothetical and the good 'undeliverable'. As such there often is a difference in willingness to pay values between real and hypothetical questions. Ways to circumvent this problem have been devised including Cummings and Taylor's (1999) "cheap talk" aiming to get subjects to think more truthfully about their decisions in a less abstract way.

Another problem with CV is the profound difference between willingness to pay and willingness to accept values. Theoretically individual's WTP to acquire a good and their WTA as compensation to give up the same good can differ because of reasons given in 2.3 above. However in contingent valuation this difference is profound. Coursey et al., (1986) in two hypothetical field and one lab study find that the WTA values far outweigh WTP even when income effects are accounted for. Mitchell and Carson (1989) note that WTA does not elicit valid data under many circumstances. Other problems associated with CV include: the embedding effect, starting point bias, payment vehicle bias, strategic bias and information bias (List, 2007).

The “embedding effect” problem arises when willingness to pay varies with changes in the scale or scope of the item being valued. In addition WTP for a composite change in a group of public goods may be less than the sum of the willingness-to-pay for the individual changes separately. Desvousges et al (1993) compared WTP values to avoid a wide range of waterfowl deaths and found that household’s willingness to pay for preventing 2,000, 20,000 and 200,000 deaths were about the same.

In a closed-ended contingent valuation study, subjects are presented with questions that involve suggesting a starting bid and then adjusting the bid upwards or downwards based upon whether the respondent agrees or refuses to pay the amount. The suggested initial bid can influence respondent’s answers because they tend to base their answers on that amount. Thus, rather than making their own independent valuations, respondents’ answers get biased by the researcher-suggested values. This is referred to as starting point bias.

Respondents may give different willingness to pay amounts, depending on the specific payment vehicle chosen. If respondents are asked for their willingness to pay based on say an increase in taxes or user fees or donations, their values may differ even for the same good/service. This bias is referred to as payment vehicle bias. This bias occurs because some payment vehicles, such as taxes, may lead to protest responses from people who do not want increased taxes.

Two types of strategic bias exist: the first occurs when an individual provides responses in order to influence a certain outcome (such as when supporting a specific cause). The other type occurs when subjects act based on their perceptions of other individuals’ actions in the same bidding situation. Strategic bias is common with repeated exposure to WTP questions.

The above biases create a discrepancy between the subject-stated values and their true valuation. While many other methods may still exhibit these biases, their magnitude in contingent valuation studies is more profound and has led to research into other “reduced-bias” methods. In the following methods some of the above mentioned biases can be controlled for through appropriate protocols and treatments.

2.4 Experimental Methods

An experiment is any setting in which data can be collected in a controlled environment. Two economic experimental methods are reviewed: experimental auctions and choice experiments. The major forms of experimental methods, their limitations and mitigations are discussed below.

2.4.1 Experimental Auctions

Experimental economics literature describes an economic environment as consisting of individual economic agents together with an institution through which agents interact. Agents are defined by their economically relevant characteristics such as preferences, technology, resource endowments and information; while an economic institution specifies actions available to agents and the outcomes that result from each possible combination of agents' actions (Friedman and Cassar, 2004; Friedman and Sunder, 1994).

A key methodological innovation for experimental economics is based on the use of a reward mechanism that allows the experimenter to induce pre-specified behavior in experimental subjects. This is referred to as the induced-value theory. According to Guala, (2005) and Smith (1976), providing a mechanism that encourages people to act under an incentive structure is the distinguishing characteristic of experimental economics from other disciplines and from other economic methods of observation such as surveys and contingent valuations. Five conditions for induced value theory are monotonicity, salience, dominance, parallelism and privacy.

Monotonicity means that subjects must prefer more reward to less, and not become satiated (the reward is often in terms of money, grades, and so forth). Salience means that the reward received must depend on the subjects' choices or actions and that the subject is aware of this link between his actions and the payoffs. Dominance means that changes in subjects' utility from the experiment come from the reward and other influences are negligible (extraneous information is irrelevant). Parallelism is concerned with the ability to generalize from the lab to the field. That is, "things carry over". Privacy is concerned with keeping each subject's rewards known only to them so as to prevent subject malevolent or benevolent behavior that compromise dominance. Promoting the level of each of these conditions in any experimental setting is desirable. Payment of subjects in cash promotes monotonicity and salience, while Friedman and Sunder (1994) suggest having student subjects to obtain both salience and dominance at moderate cost.

The use of experimental methods is not entirely new in economic valuation. Without explicitly mentioning economic experimental auctions, Coursey et al., (1986) recognized that the bidding game produced survey responses that were more realistic than other methods. Experimental economists suggest laboratory experiments to solve other problems associated with contingent valuation. This study employs the framed field experimental (FFE) auction method.

The literature on field experiments is well developed in List's (2007) recent working paper. Economic experiments are described on a 5-point spectrum with laboratory controlled data and naturally occurring data on the two extremes (see figure 2.1 below). Field experiments (defined as any study carried out on an economic agent/material in his/its natural environment) act as the bridge between the two extremes, and provide realism and randomization, both useful elements in experimentation. Field experiments have arisen mostly due to the need to circumvent some of the criticisms of laboratory experiments, mainly that of generalizability. Levitt and List (2007) describe how insights gained from the lab may not always translate well to the world outside the laboratory, hence invalidating forecasts or prediction from the laboratory model. They note that replicating the diversity of real world choices (in terms of the commodity, the stakes, environment, and task) is one of the practical difficulties of laboratory techniques and suggest field experiments as the preferred method.

Controlled Data				Naturally-Occurring Data
Lab Experiment (student subjects)	Artefactual FE (non-student subject pool)	Framed FE (economic agent observed in natural environment, non-student subjects)	Natural FE (subjects do not know they are involved in an experiment)	Natural Experiment Propensity score matching Instrumental variables Structural modeling

Figure 2.1: Spectrum of Experimental Methods. (Source: List, 2007)

List's review of the contribution of field experiments in economics contains recent studies on the economics of charity donations and gift giving, cooperation within members of a fishing community, revenue equivalence in internet-based auctions, price-setting in decentralized trading auctions, public provision of a public good in presence of warm glow, determinants of default rates on loans in micro-financing, labor market field studies on race, gender, and age discrimination, and more.

2.4.1.1 Major Forms of Experimental Auctions

Several experimental auction types are available, each differing in the prediction of economic theory and bidder behavior. They include the first price sealed-bid auction, the second-price sealed bid auction, the Dutch auction and the English auction. In the Dutch oral auction (also known as the descending bid scheme), the offer price starts at an amount believed to be higher than any bidder is willing to pay and is lowered until one of the bidders accepts the last price. Several studies show that this type of auction tends to yield prices that are equal to or lower than the true value.

The English auction (the ascending bid scheme) is a progressive auction in which bids are freely made and announced until no purchaser wishes to make any higher bid. In this auction mechanism, a bidder is able to observe some bidding behavior of his rivals and may make his bidding decisions based on the bids of others.

The first price and second-price sealed bid auctions are similar (they are both sealed-bid, distinct from the oral auctions) except that in the former, the auctioned good is awarded to the highest bidder at the price equal to his bid, while in the latter, the good is awarded to the highest bidder but at the price of the second-highest bidder. In the second-price auction (also known as the Vickery auction) bidders have an incentive to truthfully reveal their true values; under bidding risks foregoing a profitable purchase while over bidding risks making an unprofitable purchase.

In a variant of the Vickery auction procedure - the random *n*th price auction mechanism (credited to Shogren et al., 2001) the *n*th highest bidder (chosen probabilistically) wins the auction but pays the price of the n-1 highest bidder. This procedure has been found to be incentive compatible - it induces subjects to truthfully reveal their preferences. The Vickery auction procedure has been used in environmental valuation where respondents bid on a specific proposal for implementation. If the proposal is supported jointly by the group, the environmental good is supplied. Subjects should understand the connection between their actions and the possible outcomes (salience). In Boyce et al., (1992), subjects bid on preservation of a tree species. Underbidding resulted in physical destruction of the tree. Subjects witnessed this destruction - the outcome of their valuation. Some studies in the field of psychology often subjected respondents to tasting a bitter non-dangerous substance called SOA if subjects underbid (see for example Coursey et al., 1987).

Experimental methods may have some advantages over other methods. For example many of the biases and shortcomings of the CV (see section 2.3.4.2) can be controlled for when experimental auctions are used. In fact, the attractiveness of experimental methods is in being able to control confounding or nuisance factors. For example information biases may be controlled for by providing brief unbiased information to subjects while strategic bias (mentioned below) may be controlled for by not revealing end-round outcomes or limiting experiments to single-shot observations.

2.4.1.2 Some Limitations of Experimental Auctions, and their Mitigation

Like with other methods, a number of issues are of concern when using experimental auctions. Among these are framing issues, choice of subject pool, experimental design, and choice of payment vehicle. However, as mentioned earlier, these issues may be addressed empirically.

Strategic bidding where subjects bid based on their individual perception of other members' bidding behavior has long been of concern to experimental economists. In his recent study Maupin (2006) found that Filipino farmers cared about their peers' bidding behavior and submitted higher bids in subsequent bidding rounds. An earlier study by Cooper et al., (1996) showed that Chinese participants cooperated more than their counterparts in the US (in a game in which players were randomly reassigned new partners at the beginning of each round). Strategic bidding is most likely to occur in multiple-round experimental sessions when subjects repeatedly 'meet' each other during the experiment. Andreoni and Croson (2002), and Mestelman (2004) in their 'strangers and partners' treatments in public goods experiments, find that partners' contributions do not fall because members are afraid that if they decrease their contributions then other members will too. That is, if some members free-ride, then all will (either as a punishment mechanism or otherwise). To avoid strategic behavior, single-shot treatments have been encouraged (Mestelman, 2004), or multiple-round experiments where subjects are informed that the chances of 'meeting' again in the experiment are slim. In addition, not revealing the end-round results when the experiment is on-going or maintaining privacy of subjects' actions might control for this behavior (Friedman and Sunder, 1994).

A number of studies valuing environmental benefits or valuing people's demand for a public good introduce a 'pseudo-good' such as a research organization to which subjects are required to 'donate' as a means of revealing demand for the public good or as support for research on an environmental improvement project. However, peoples' donations in such experiments do not really reflect their valuation. An individual may reason that they can appreciate the output of an organization without having to 'fund' it. In such instances, their 'donations' will be limited. In addition, organizations have their pre-set budgets, rules and regulations. Individuals might feel that their 'donation' will not make a difference. Such studies therefore may yield undervalued estimates of the intended good/service. In my view weak complementarity is rarely achieved through such pseudo-goods.

Studies involving college student subjects are common in the literature and they continue to dominate economic experiments (for example Krause and Harbaugh, 1999). Some authors argue that a realistic approach in subject selection may be to involve adult subjects since adults are the majority of people who make production and consumption decisions, and who make decisions of welfare/utility maximization (List, 2007). Advocates for student subjects on the other hand state that student subjects have low opportunity costs and steep learning curves providing dominance and salience at low cost (Friedman and Cassar, 2004), while others have concerns of whether students are representative of the total population, arguing that value estimates from student and adult groups vary (Maupin, 2006).

2.4.2 Choice Experiments

Choice experimentation is a broad technique in which individuals' preference for a commodity or resource is obtained using multi-attribute criteria. Respondents are presented with various alternative descriptions of a good differentiated by their attributes and attribute levels, combined into choice sets and in each choice set, individuals indicate a preference for the attributes of one alternative over the others. Repeated choices from various sets of alternatives reveal the trade-offs that an individual makes, and the utility individuals derive from the attributes can be estimated.

Four types of choice experiments include contingent ranking, choice rating, paired comparisons and discrete choice, of which the discrete choice methods are receiving the most attention. With contingent ranking individuals compare and rank bundles of attributes in order of preference from the most preferred to the least preferred. The most preferred profile is chosen from the entire set, then the second ranked profile is chosen from the remaining choice set and so forth. Thus the number of alternatives in the choice set decreases as ranking depth increases. Interpretation of ranking data is based on a random utility theory explained in the next chapter. The task is cognitively more demanding than answering willingness to pay questions and respondents may get fatigued especially when the number of alternatives is large.

In discrete choice experiments respondents are simultaneously shown two or more different alternatives and their characteristics, and asked to identify the most preferred alternative in the choice without necessarily ranking or rating them. Choice of one alternative in a set of alternatives simply indicates preference of the chosen alternative but does not say anything about the un-chosen alternatives (detailed later).

Rating is a choice procedure variation of the discrete choice format, where respondents compare alternate situations and score them in terms of strength of preference. Rating data is analyzed using ordered probit or ordered logit models.

Within each choice set attributes are varied in order to estimate their relative importance in predicting choice behavior. Several design alternatives are presented first to decrease the number of choice sets presented to respondents, but also to reduce any collinearity effects (Louviere et al., 2000), and also to provide balance in the choice sets presented. Willingness to pay for changes in the levels of the good can be obtained as long as one of the included attributes of the good in the choice set is a price/cost variable. The method leads respondents to explicitly make trade-offs between the various attributes of a situation without directly asking people to state their monetary values thereby circumventing problems associated with contingent valuation where responses focus on direct monetary value expressions. Of the four types of choice experiments, discrete choice experiments are currently receiving most attention and the following discussion on choice experiments/choice modeling pertains to them.

Choice experiments generate data based on individual’s statement of their unobservable behavioral preferences. The conceptual structure underlying choice experimentation is derived from Lancaster’s (1966) theory of consumer demand: that any good or service can be described by its component characteristics, and consumer demand is driven by commodity attributes. Alternatives are combined into sets called choice sets which must be mutually exclusive, exhaustive and finite (Train, 2003). Mutual exclusivity requires alternatives to be appropriately defined so that an agent’s choice of one alternative excludes choice of another alternative within that choice set. Exhaustiveness can be achieved by including all possible options within a set. The condition that alternatives must be a finite number that can be explicitly listed, is one of the distinguishing features of choice models from regression models. It means that the dependent variable is limited or categorical as opposed to being continuous. As such choice models are often called categorical dependent variable models (CDVMs).

Discrete choice models are of two kinds depending on whether utility is considered constant or random. Constant utility models assume utility is a fixed function defined over choice objects, that is, utility is deterministic but the decision process is probabilistic. Decision makers can choose an alternative whose utility is not a maximum, but rather they behave according to some probability distribution defined over the choice objects in which utilities are parameters (Batsell and Louviere, 1991). These models are not as popular as random utility models (RUMs) which are derived from assumptions of utility maximizing behavior by decision makers. In RUMs, the decision rules are deterministic and uncertainty is captured by random variables representing utility (Batsell and Louiviere, 1991). The decision maker's choice of a discrete outcome is based on the expected utility obtainable from this choice, itself being composed of systematic and random components. The economic theory of value on which these models are based assumes that individuals, given an opportunity, act rationally in their self-interest. This theoretical framework is developed in chapter 3.

2.4.2.1 Some Limitations of the Choice Experiments, and their Mitigation

Although the trade-off process used in choice experiments is deemed one of the method's biggest advantages, it can have problems associated with respondent's familiarity with valuing those trade-offs. Respondents may find some tradeoffs difficult to evaluate, because they are unfamiliar. In such cases respondents may select a decision strategy based on the existence of a trade-off contrast. For instance, whenever an inferior option is included in a choice set the probability that the superior alternative is selected increases because of its dominance effect regardless of knowledge about the option, and because such a choice situation is cognitively easier to process (Alpizar et al., 2001).

If the choices are too complicated, the amount of effort demanded when choosing alternatives in a set of choices is so high that it exceeds the ability of respondents to select the preferred option. When this problem, referred to as task complexity occurs, individuals fail to properly answer choice questions and may resort to simplified decision rules which can bias the results of the analysis (Mogas et al., 2005; Alpizar et al., 2001; and Hernstein, 1992). Task complexity is determined by the number of choice sets; the number of alternatives in each choice set and the number of attributes describing the alternatives (Alpizar et al., 2001, DeShazo and Fermo, 2002). The problem of task complexity can be overcome at the survey design stage by choosing an optimal number of alternatives, and carefully selecting necessary attributes. In their study in Guatemala and Costa Rica in 2001, DeShazo and Fermo, (2002) establish that failing to control for complexity can lead to over- or under-estimation of welfare effects by as much as 33%. In general if choices are complicated, task complexity increases because of cognitive effort involved in comprehending information.

When presented with a large number of tradeoff questions, respondents may lose interest or become frustrated. This is overcome by reducing choice sets using a statistically tested method of orthogonal sets (Louviere et al, 2001). However others argue that by only providing a limited number of options, it may force respondents to make choices that they would not voluntarily make, or that it reduces the chances that the respondent finds an alternative that matches his or her preferences more exactly. In fact DeShazo and Fermo (2002) argue, and confirm that increasing the number of questions or alternatives affects consistency in a quadratic manner – where questions are few, addition of one more question increases the performance of the model while if the number of alternatives is already large adding another question increases confounding information.

A major problem with choice experiments has been their hypothetical nature, where no mechanism ensures that subjects truthfully reveal their preferences. As such there are concerns about the difference between what people say they would do and what they actually do in these situations.

2.5 Field Experiments in Developing Countries

Conducting field experiments in developing countries is currently receiving great attention for a number of reasons. The cost of field experiments in the developing world may be lower than in industrialized countries - the developed world could be said to have a comparative advantage in laboratory experiments. In addition, and perhaps the most important reason is the quest to learn more about the developing world than is currently known. It is clear that many economic experimental studies have in the past been conducted in industrialized countries (mostly with university student subjects), and little was known about the behavior of economic subjects in developing countries in an experimental setting. The current growing body of literature in field experiments may fill this knowledge gap.

Among the earlier works on field experiments in the developing countries is in Henrich et al (2001) in which fifteen diverse populations were involved in a mixture of dictator games, public goods games and ultimatum games designed to test behavior of economic subjects. Since then many more studies have taken the laboratory to the field and substituted the student experimental subjects with adult subjects. Recent work using economic experiments in Uganda involves examining decision making under risk (Mosley and Verschoor, 2005) and in public goods settings (Iversen et al., 2006). The objective in most of these experimental games has been to provide insights into social preference such as tests of presence of altruism, fairness, cooperation, reciprocity or trust within (and without) members of a given group.

2.6 Conclusion: Valuing Health and Environmental Benefits using Experiments

Even with recent advances in non-market valuation methods, valuing passive use remains a challenge because these values are associated with goods and services that do not have direct linkage with expenditure, purchase or other observable market behavior. However experimental economics provides a promising alternative to other methods which have numerous drawbacks that experimentation can address. To value the environment one considers its possession of public good characteristics. However, finding a good that truly represents all the components of this environment is a major limiting factor.

The use of choice experiments and experimental auctions for non-market valuation including the environment is relatively new. Before 1994 choice experiments were only used in the fields of transportation, architecture and landscape design, engineering and marketing. However many recent studies are using choice experimentation in resource and environmental economics and also in health economics. With experimental auctions, although proposed much earlier on, their use in developing countries has been limited. Laboratory experimental auctions have been suggested as the solutions to the many problems of contingent valuation surveys, and as a way of improving results obtainable from choice methods. Because both these methods generate their own data such that economists can rely on them without depending on pre-existing market data, a clear, concise and well-written instruction set for data generation is crucial (Friedman and Sunder, 1994).

The review of literature on non-market valuation has highlighted both advantages and limitations of various methods. The current study uses two experimental methods: field experiments and choice experiments to value health and environmental benefits. Although as mentioned above, experimental methods were proposed earlier on, it is only recently that their use has become widespread. Moreover the literature on their application in developing countries is still limited. Previous studies such as Higley and Wintersteen (1992), Mullen et al., (1997), Cuyno et al., (2001), Maupin (2006) and Brethour and Weersink (2001) have applied contingent valuation or experimental auctions to value pesticide risk reduction.

One of the key advantages experimental methods have is that they allow us to generate a type of revealed preference information in situations where data are not available. However it is important to note that they have their own limitations and may thus not be absolutely better than other approaches under all conditions.

3.0 Methods

From the previous chapter, the advantages of both choice experimentation and experimental auctions motivate the current study to use these methods to capture the advantages in these applied non-market valuation methods and to provide a basis for comparison of the two methods. This chapter is subdivided into three parts. Part one explains sample selection. Part two explains experimental auction procedures. Part three describes choice experimental procedures. In choice experiments, the economic model is the basis of the analysis as it affects the design and analysis of the data. As such a sub-section on the choice experiment economic model is detailed before explaining the experimental design.

3.1 The Sample and Testing Conditions

In this subsection the pre-testing procedures are explained. In addition, the method used for selecting the sample for both experimental locations is detailed, as is a comparison of sampling procedures between this, and other studies in the developed countries.

3.1.1 Pre-testing

One major difference between economic experiments in developing and developed countries is the uncertainty of the experimental environment. While in the developed world, subjects are often brought into an experimental laboratory (usually designed for that purpose equipped with computer terminals and lab assistants) the 'laboratory' in the developing countries may take on several forms. A recent study by Maupin (2006) in Philippines and USA found such major differences in testing environments that results from two different testing environments were incomparable. Henrich et al., (2001) also found 'environmental' differences in small-scale societies that affected valuation results. Because of these variability issues, focus groups and pre-testing are necessary. See A**ppendix 1** for the step-by-step pre-test procedures and results that influenced the final design and procedures for the actual experimental sessions.

3.1.2 Sample Selection and Study Area

Sample selection for game experiments and experimental auctions

The goal in designing an effective non-market valuation study is to formulate it around a specific community that is knowledgeable about the good that one seeks to value. This implies targeting respondents in a location where the specific problem is centered.

Respondents for this study were identified using a two-tiered system, selecting individuals from two classes of people to provide a good basis for comparison of values of the environment between different groups. The first group (the farmer group) comprised semi-randomly selected groundnut producers from Iganga district, the leading producer of groundnuts in Uganda.

Study participants were drawn from each of the three counties that comprise Iganga District. Waibuga sub-county (in Luuka County), Buyanga (in Bugweri) and Nawandala (in Kigulu) were chosen because they are the largest groundnut producing sub-counties, and are also NAADS pioneer sub-counties (NAADS, the National Agricultural Advisory Services is a government program aimed at increasing agricultural production through provision of effective agricultural extension services). Within each of the selected sub-counties, the two largest parishes (in terms of population size) were identified. Two villages from each selected parish were randomly identified and village heads were contacted to provide lists of all groundnut farmers in the village. A random selection of approximately 10 farmers from each village list was then conducted. This randomization was important to eliminate any potential favoritism which would bias the sample. A total of 121 participants formed the farmer group sub-sample. Figure 3.1 shows the sample selection procedure in Iganga.

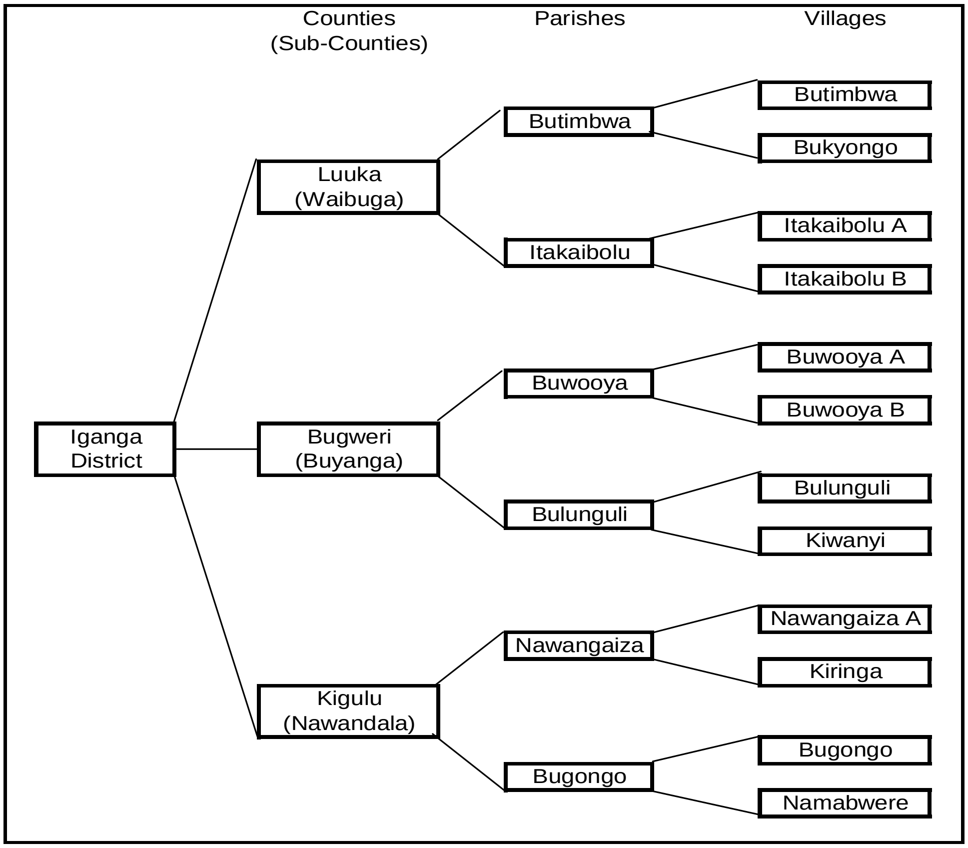

Figure 3.1: Balanced Sampling Design for Iganga District. (Source: Bonabana-Wabbi, 2007)

The second group (the non-farmer group), the more educated, urban population was sampled from Kampala district. Respondent selection was semi-purposeful - targeting the urban respondents in Kampala, by posting 35 "invitation-to-participate" notices in several strategic areas around Kampala (**Appendix 2**).[6]

Both Kampala and Iganga districts were important for this study because the former contains the biggest urban population in the country and is one of the leading consumers of groundnuts, while the latter is the leading producer of groundnuts in the country. Additionally, farmers are generally thought to have fewer years of education compared to the

[6] This being a passive sampling method resulted in an extremely low response rate. (Only 10.7% of the urban sub-sample was obtained using this method). This low voluntarism is not uncommon especially with a non-student subject pool. Huck (2007) notes that professionals are usually harder to motivate, which might explain why their response rate to posted messages is low. The rest of the respondents were obtained by actively recruiting participants at strategic locations, creating a semi self-selected sample of subjects who could at least read. For three markets (Nakawa, Bugolobi, Wandegeya), market leaders were asked to mobilize 2 groups each of market vendors on a date convenient to them. For two schools (Makerere College, Little Swans), the head teachers were contacted and asked to mobilize 3 groups each of teachers during their lunch break. Five groups gathering for a social occasion, and four groups of people working on special projects were included in the sample. Sampling excluded individuals who were full-time students.

more-than-high-school urban dwellers. Unlike the farmer group which may have profitability interests related to pesticide use, the non-farmer group may have environmental concerns over pesticide use. Thus this sample selection method allows capturing the views of a wide spectrum of respondents providing a basis for comparison between different classes of people: farmers vs. non-farmers; rural vs. urban dwellers; and primary producers vs. consumers.

Sample selection for choice experiments

In-person interviews, generally considered to be the best approach for choice experiments (compared to mail or telephone approaches), have advantages in that there is researcher-respondent interaction, a necessary aspect when survey questions are deemed complex and where clarification on some aspects is needed (DeShazo and Fermo, 2002). Especially in cases of low levels of formal education in-person interview sessions become much more relevant. During the pre-testing in Iganga, each of the 16 pre-test subjects were given 5 choice sets. Three of them had implausible combinations. The implausible choices were selected 40% of the time indicating that subjects did not understand them (see **Appendix 1**). As such all the choice experiments for this study were run only with the Kampala subjects where a subset of experimental auction subjects also evaluated choice questions. Thus, this Kampala choice experiment sub-sample was self-selected unlike the experimental auction sub-sample in Iganga.

3.1.3 Sampling Differences

A comparison between the sampling methods used in this study and in experimental studies in many developed countries reveals several differences: In developed countries, respondent selection for surveys is often done using the random digit dialing procedure or through local addresses from the department of motor vehicles. This selection is facilitated by the centrality of local address and telephone data. In the current study, telephone or local address registers do not provide a representative sample of the population. Telephone coverage in Uganda is limited. By March 2006, only about 3% of the population was accessible by telephone. Moreover, this percentage is concentrated in the major towns. In rural areas, although increasing due to liberalization and establishment of new service providers, telephone coverage is still limited. Besides lack of accessibility, the cost of telephone calls is high. In-country one-minute call charges range from Shs. 480 to Shs. 600

(US $0.33- US $0.40) depending on the service provider. The current mail system is still recovering from past infrastructural damage. Thus, although national and private mail coverage is wide, the service is largely unreliable and mail delivery time could take up to 3 months. As such a mail survey becomes intractable and a telephone survey can only be applied in bigger cities, and at a high cost. Thus, these issues: lack of well established mail and telephone systems countrywide and cost implications make in-person choice experiment interviews more appropriate.

3.2 Experimental Procedures I: Game Experiments and Experimental Auctions

As was alluded to earlier, the basic idea behind economic experiments is that we can put people in circumstances that have been prepared by the experimenters and then observe their behavior. This method avoids asking people to state how they would actually behave in certain circumstances, because of the concern that people may not truthfully reveal their preferences. Experimental auctions allow researchers to observe what people do instead of asking them what they would do, and in so doing, experimental methods may increase external validity.

In designing experiments, previous research finds that a single-shot game and a multiple shot game may result in different outcomes. This is perhaps because in a one-shot experiment, subjects do not fully appreciate the game. However after several rounds of "learning", subjects might understand the task and are able to form adequate preferences and beliefs about the situation. In this study, a learning phase was allowed before actual bidding started to help subjects to understand the experiment. Multiple-round games have been found to introduce strategic behavior which biases results.

As was mentioned earlier, non-use non-market values are harder to estimate than market values and although the available methods are not fool-proof, the idea is to use a method that approximates reality while providing an opportunity to learn from it. Experimental economics methods come closer to this goal than other methods of valuation. They (as this study shows) can be employed to provide revealed preference information on people's demand for a pesticide-free environmental public good where market data is unavailable.

3.2.1 Experimental Design: The 'Goods' Experiment

A total of two hundred and seventy seven subjects were involved in the field experimental auctions in both Iganga and Kampala districts. Thirty five experimental sessions were run in all. Each session involved approximately 8 subjects and no subject was allowed to participate in more than one session.[7] Each subject was endowed with Shs. 500 in one hundred shilling coins.[8] The use of an endowment serves as what some refer to as 'starting capital' and is based on the idea that control of subject behavior can be achieved by using a reward structure to induce pre-specified monetary value actions, (Friedman and Cassar, 2004, Smith, 1976), referred to as induced value theory (IVT) in experimental economics literature. Endowing subjects with cash allows actual statements of value with real economic commitments since cash based estimates are unbiased signals of preference (Smith, 1976). One treatment was administered to each sub-group as shown in the simple 2x2 design (Table 3.1) below.[9] **Appendix 3** shows the detailed design incorporating all sessions. A small number of treatment variables provides an opportunity to learn more about these treatment variables than would be the case with a large design.

Table 3.1: Experimental Treatments

		Information structure	
		Information	No Information
Trial Type	Individual Decision	Self-Info	Self-No Info
	Group Decision	Group-Info	Group-No Info

[7] If subjects are not informed of the number of periods to be played the resulting game yields the same equilibria as the infinite game, since no period is known to be the last. Also this procedure eliminates reputation building which introduces strategic bias. Many ways of controlling for strategic bias are imposed here, for example no communication between subjects was allowed.

[8] Given a fixed budget allocation, there is a trade-off between the level of endowment and the number of experimental subjects. The initial plan to endow subjects an equivalent of the 2007 per capita daily income (PCDI) was constrained by budgetary limitations. The Shs 500 endowment represents half the national PCDI. In other studies, tokens or coins have been used to mimic actual expenditure for goods. The tokens are redeemed at the end of the experiment and exchanged for money given to the holders of those tokens. However this payment vehicle introduces some hypothetical bias in that the stakes are not real during the actual experiment, although those in favor of the token approach suggest that this should not really be a problem as long as subjects believe payment will be made.

[9] Each 2x2 cell/subgroup consists of respondents for both the WATER and the GROUNDNUT experiment (See also section 3.2.2 below).

3.2.2 Individual Decision Making

Respondents are each presented with two units of a good differentiated only in terms of labeling. The goods included WATER and GROUNDNUTS, both easily accessible familiar goods in both study areas. These goods are particularly appealing because although most individuals are unfamiliar valuing the environment in its entirety, they can at least identify with components of the environment, which makes this valuation more realistic. Also, these ‘goods’ are used in their most ‘usual’ state to avoid subjects mistakenly valuing the modifications/processing that goes into their production. I chose the presentation of two units instead of having just one unit (of either a “good” or a “bad”) to avoid problems associated with abstraction - making the situation more realistic. Other (visual) characteristics such as color, size, and container type were the same. Using plain white labels pasted on the good and by word of mouth, subjects are informed that the unit of a good labeled A is from a trusted source and is free from pesticides, while that labeled B was obtained from a source of unknown quality and may contain pesticides or pesticide residues. This set up is designed to mimic the real world scenario where chemicals, even though not visible to the naked eye, can be present in the environment hence affecting health and environmental quality. Survey subjects are then asked to bid individually to avoid being subjected to consuming a good labeled B.[10]

3.2.3 Group Decision Making

Provision of a pesticide free environment can be viewed as provision of a public good. A public good as defined earlier is both non-rival and non-excludable in nature. Both these characteristics however present a fundamental problem with public goods provision. Although it is reasonable to assume that everyone would prefer a pesticide free environment, it is in the interest of each individual to free ride and not contribute towards its provision. Procedures for studying voluntary behavior relating to public good provision or free riding behavior vary, but generally the situation is best modeled using the n-person Prisoners’ Dilemma games.[11] In these games the individual rational action seems to lead to a social

[10] Use of neutral terms such as ‘good A’ and ‘good B’ avoids loaded words promoting dominance (Friedman and Sunder, 1994).

[11] In the 2-person version of these games, the two (non communicating) parties (prisoners) each have two options: C (for confess) and D (for defect, remain silent). If one party confesses while the other defects, the confessor is released while the defector is given maximum punishment. If both remain silent, they receive the minimum punishment, while if they both confess then they both receive an intermediate punishment.

dilemma or "Pareto inferior" outcome. The dominant strategy for each individual, acting rationally (and selfish), will be to free ride (Guala, 2005).[12] But then if all respondents play their dominant strategy the outcome will be inferior to the social optimum, that is, less than what could have been achieved by all making full contributions.

Individuals want to maximize utility (V_i). Assuming individuals derive this utility from monetary payoffs and from provision of a public good. Thus:

$$V_i = f(\pi_i, G) \tag{1}$$

where π_i is the monetary payoff private good of player *i*, and *G* is the pesticide-free environment public good.[13, 14] The monetary payoff function π_i is a linear function unlike that considered in other studies (see Mestelman, 2004) and may be specified in the following form:

$$\pi_i = X - g_i \tag{2}$$

Therefore (1) becomes:

$$V_i = f\left(X - g_i, G\right) \tag{3}$$

where X is the initial endowment and g_i , g_j are contributions to the public good by subject *i*, and other respondents in the group, *j*. The public good is either provided or not. Thus:

$$G = \begin{cases} 1 & if \ \sum_{j=1} g_j > Y \\ 0 & \text{Otherwise} \end{cases} \tag{4}$$

However, once provided, all individuals derive utility from it. Free-riding in this case is equivalent to having $g_i = 0$ but expecting $\sum_{j=1} g_j >$ Y, such that G=1 and V_i is maximized.[15]

However the socially optimum outcome is for all individuals to fully contribute, in which case *G*=1. When all subjects contribute their total endowment (g_i=g_j=*X*), G=1 and no subjects are subjected to the environmentally un-friendly condition of drinking/consuming water/groundnuts of unknown quality (potentially pesticide-contaminated). However, if all

[12] Assuming individuals are substantially uninterested in the choices of other people, that is, issues of altruism, fairness and equity are not considered.
[13] Kotchen (2006) decomposes this G into a pure public good and an impure public good that jointly provides both public and private consumption characteristics.
[14] Note that the G is not indexed because it is not individual specific: consumption is in a collective provision situation - once provided, all individuals benefit, or if not provided, no one consumes.
[15] Strict inequality because for efficient provision of a public good, the sum of WTP must exceed the cost of providing it.

free-ride then $g_i = g_j = 0$, G=0, and all subjects consume goods labeled B regardless of the amount of π_i. Each individual, in making g_j contributions, makes a tradeoff between π_i and G. The value Y is not known a priori. It is determined during the experimental sessions by factoring the session binding average[16].

3.2.4 Field Procedures

Bidding followed the *n*th price auction procedure explained to the subjects before the experiment started. Bids were ranked and the n^{th} highest price recorded as the binding price. The *n*-1 highest bidders each paid the n^{th} highest price and did not consume good labeled B, while the other respondents were subjected to consuming good B **(Appendix 4)**.[17] The experiments assume that subjects are motivated to truthfully place a value on the environmental situation as it relates to personal health and these assumptions are validated by getting subjects to actually "consume" their purchase or as Freeman (2003a) puts it: "to live with the consequences of their choices" (pp. 175).

As mentioned earlier, with incomplete information, subjects have been known to make their own assumptions about the study hence affecting their responses to questions. In the information treatments introduced in Table 3.1 above, subjects were given a brief handout with information regarding the potential effects of pesticides in the environment. In the case of the farmer group, this information was first translated into the local language and read out to the subjects. Care was taken to make this information brief, bias-free and as simple as possible hence reducing confusion (**see Appendix 5a – 5d** for the statements). Subjects were not informed before hand which treatment (proxy good, group or information) they would be participating in. A post-experiment evaluation questionnaire asked subjects to justify their responses to the experiment **(Appendix 6).**

In the group decision treatments, each individual in a group was given an initial endowment which they could deposit in a personal account or invest in a communal/group account for the purpose of contributing towards the public good. Individuals were informed that the total sum invested in the group account would benefit all group members equally regardless of how individual members bid (since public goods are non-excludable). The rest

[16] Group bids were ranked in descending order, and then a die rolled to determine the binding rank. The bid amount in the position of the binding rank became the session binding average which was multiplied by the number of group members to obtain Y.
[17] See section 2.4.1.1 above.

of the bidding followed the same procedure as the individual decision treatments described above **(Appendix 7).** A training period with no "real" payoffs was allowed before actual bidding started to familiarize subjects with the auction procedure and gain experience with the game.[18] Again, a post-experiment evaluation questionnaire asked subjects to justify their responses to the experiment and to report their socio-economic information **(Appendix 6).**

3.2.4.1 Procedures in Kampala

In all Kampala sessions, the experimental 'labs' were set up in the same way but the procedures differed according to which treatment was being administered.

- Furniture (stools, desks, chairs etc) was arranged in such a way that subjects would sit as far apart from each other as the room would allow.
- Two units of the good labeled "A" and "B" were placed on this furniture, together with an instructions booklet, a pen and a khaki envelope labeled 'E' containing the endowment.
- The 8 (sometimes 9) participants were then let into the room and got seated at designated positions.
- The researcher briefly introduced herself and informed participants that each one would be given a lunch allowance at the end of the session.
- The researcher then read the instruction sheet out loud after which participants were asked to take about a minute to re-read the instructions and raise their hand if they needed clarification.
- A consent form was passed round for participants to write their names and sign (**Appendix 8**). Participants were told that those who wished to withdraw could do so at this point by not signing.[19]
- Participants were then issued the bidding sheets, asked to open their envelopes, establish the amounts they were willing to pay and write the amount in the appropriate spaces on the bidding sheet.
- For the 'group' treatment, participants were required to answer two questions in the instruction sheet (page 3 of **Appendix 7**) as an indication that they had understood

[18] Non-binding practice (learning phase) trials have been suggested as ways of improving efficiency by allowing learning and also to reduce under-evaluation.
[19] Two subjects (both in the Kampala subsample) out of a total of 278 opted out of the study at this point. When asked for reasons they mentioned that they had failed to understand the instructions.

the instructions. After they answered them, they were ready to proceed with the bidding (which in this case simply involved making the decision of the amounts they wanted to put into envelopes “G” for Group and “P” for Private).

- Each participant was then issued a random code and told to write it on all documents (including the “G” envelope in the case of ‘group’ treatments).
- After bidding had taken place, the bidding sheets were collected and bid amounts arranged in descending order.
- The researcher then rolled a die to determine the binding amount. This amount was communicated to the session subjects.
- The decision to “Consume B, Do not pay” or “Do not consume B, Pay” was made based on whether a subject’s value fell below or above the binding amount. This decision was written on the bidding sheet and returned to participants.
- The researcher then collected:
    a. Unit A from subjects whose binding decision was “Consume B, Do not pay”.
    b. Unit A, Unit B, and the payment from subjects whose binding decision was “Do not consume B, Pay”.
- Those subjects remaining with Unit B were asked to consume it.[20,21]
- After collecting empty containers from those who consumed Unit B, all participants were issued a post-evaluation form.
- For sessions that also involved choice experiments, discussed below, 8 choice sets were presented to the participants and told to write their code on both the post-evaluation form and the choice cards.

[20] The ‘no-communication-amongst-members’ instruction was emphasized by telling participants to read the top of their instruction booklet again. During the pre-test, when the no-communication rule hadn’t been enforced, two members instigated others not to consume Unit B. A strategy had then been devised to obtain the group members’ values without repeating the experiment. They were told as a group that if they did not consume Unit B, it would be an indication that they valued their health more than the endowment and thereafter were asked to relinquish the endowment. Again, group dynamics set in and members were hesitant to surrender the money. This clearly is an indication that people’s behavior can be conditioned by outside influences when confidentiality is relaxed as was the case here when two members (male youths) instigated the rest of the group to behave differently. It was then decided that in the actual sessions, the no-communication rule would be enforced.

[21] Generally I noticed a lot more hesitancy consuming Unit B of the Water treatments compared to Unit B of the Groundnut treatments.

- The session ended after all documents were handed in to the researcher and participants given their lunch allowance. On average each individual decision making experimental session lasted approximately one and a half hours.

3.2.4.2 Procedures in Iganga

The Iganga sessions were similar to the Kampala sessions. They only differed in the following aspects:

- The instruction booklet was not given to participants, but rather instructions were read out loud (sometimes up to 4 times) to participants.
- The written example drawn on a chart was held up for all to see.
- Four out of the 14 Iganga sessions were carried out in the open (under a large tree). The materials were set up (on the ground, chair, mat etc) while the participants were engaged in an 'agricultural production' discussion not related to the topic of research.
- Pen and paper were provided only at bidding time after noticing that some people get intimidated on seeing them (pen and paper). For some who could not even write their names, the researcher had to assist at all times.[22]
- A limited number of post evaluation forms could be used with this group due to the low literacy levels with this group. Instead participants were asked questions in a group discussion as the researcher recorded their responses.
- The literacy level of the farmer sub-sample also didn't allow me to run the choice experiments with them.
- Individual decision making experimental auctions with the farmer group took longer than the urban sessions.[23]

3.2.5 Theoretical Framework

When the dependent variable is either discrete (binary, count, ordinal, nominal) or has a limited outcome space (censored, truncated, sample selected) the use of linear regression (OLS) poses several problems such as yielding biased regression coefficients,

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[22] Some subjects who got overly intimidated by pen and paper thought that they were in an exam setting, failed to write anything for fear of being declared as having "not passed the examination". Some people even asked the researcher to decide for them what to write down!

[23] Most time was spent 'gathering' the participants than actually running the sessions. Especially when it rained, turn out the next day was low or delayed as farmers couldn't pass on the opportunity to do sowing prior to any other commitment. Unfortunately those who arrived early had to wait since the sessions could not start until all members of a group were present.

non-normally distributed error terms and heteroskedasticity. Biased regression coefficients undermine one's ability to trust predicted values, while the other two undermine one's ability to produce unbiased standard errors or to conduct tests of statistical significance.

Limited outcomes are continuous variables characterized by the fact that their observed values do not cover the entire range, that is, they are cut-off at some particular value. An observation is truncated if it is incomplete due to a selection process in the design of the study. That is, a sample is truncated if data is available on a subset of the whole population and observations are not sampled at the lower range of values, upper range of values, or both - only some observations get into the sample. On the other hand a censored variable is one whose value is incomplete due to random factors for each subject. Data is recoded for a subset of the population and we observe the explanatory variables for the whole sample (all individuals are observed but only some observations can be made on the dependent variables). Breen's (1996) distinction between the two limited outcomes is revealing (Table 3.2). According to him, a dependent variable may be truncated when the sample is censored. What determines when to use truncated regression or the censored regression is the nature of the limitations on the data.

Table 3.2: Distinction between Truncated and Censored data

Sample	Dependent Variable	Independent Variable
Censored	Dependent variable is known exactly if some criterion defined in terms of the value of y is met. Y is a truncated random variable	Explanatory variable values are observed for all of the sample, regardless of whether y is known exactly
Truncated	Dependent variable is observed only if some criterion defined in terms of the value of y is met	Explanatory variables are observed only if y is observed

Source: Breen (1996)

Censored data is modeled by specifying a Tobit model. In the general case of censoring from below, suppose that there is a latent (i.e. unobservable) variable y_i^* that linearly depends on x_i via a parameter vector β.

$$y_i^* = x_i\beta + u_i\,. \tag{5}$$

The observable variable y_i is defined to be equal to the latent variable whenever the latent variable is above zero and zero otherwise. That is,

$$y_i = \begin{cases} y_i^* & \text{if } y_i^* > 0 \\ 0 & \text{if } y_i^* \le 0 \end{cases} \tag{6}$$

From the above formulation and following Greene (2003), the probability of an observation being censored for a given value of x_i is:

$$P(y=0) = P(y_i^* \le 0) = P(x_i\beta + u \le 0), \Rightarrow P(u \le -x_i\beta) \tag{7}$$

Equation (7) means that the probability that y=0 is the probability that the random term is less than the negative of the systematic component. Now invoking normality and homoskedastic assumptions, $u \sim N(0,\sigma^2)$ then $\frac{u}{\sigma} \sim N(0,1)$ and hence:

$$P\left(\frac{u}{\sigma} \le \frac{-x_i\beta}{\sigma}\right) = \Phi\left(\frac{-x_i\beta}{\sigma}\right) = 1 - \Phi\left(\frac{x_i\beta}{\sigma}\right) \tag{8}$$

where Φ is the cumulative distribution function. Therefore:

$$P(y>0) = 1 - \Phi\left(\frac{-x_i\beta}{\sigma}\right) = \Phi\left(\frac{x_i\beta}{\sigma}\right) \tag{9}$$

The expected value of the observed dependent variable is:

$$E(y)=P(y>0)\bullet E(y|y>0)= \Phi\left(\frac{x_i\beta}{\sigma}\right)\bullet E(y|y>0) \tag{10}$$

The truncated probability distribution is created by dividing the pdf of the original distribution by the region to the left of the truncation point (when data is censored from below).

$$E(y \mid y>0) = x_i\beta + \sigma\lambda\left(\frac{x_i\beta}{\sigma}\right) \tag{11}$$

where $\lambda(\cdot) = \frac{\phi(\cdot)}{\Phi(\cdot)}$ is the inverse mills ratio. $\phi(\bullet)$ is the density function and $\Phi(\cdot)$ is the cumulative distribution function. Therefore:

$$\text{E(y)}= \Phi\left(\frac{x_i\beta}{\sigma}\right)\left[x_i\beta + \sigma\lambda\left(\frac{x_i\beta}{\sigma}\right)\right] \tag{12}$$

$$= \Phi\left(\frac{x_i\beta}{\sigma}\right)x_i\beta + \sigma\phi\left(\frac{x_i\beta}{\sigma}\right)$$

The two extreme cases of censoring are when all cases are censored, in which case $\Phi(\cdot) = 0$ and E(y) = 0, and when no cases are censored, $\Phi(\cdot) = 1$, $\phi(\cdot) = 0$ and $\lambda = 0$, leading to $E(y) = x_i\beta$.

Using equation (5) since u is normally distributed, then y_i^* is also normally distributed. For uncensored observations where y has a continuous distribution over positive values (y>0), the density function is given by:

$$\frac{1}{\sqrt{2\pi\sigma^2}} e^{\left(-\frac{1}{2\sigma^2}\left(y_i - x_i'\beta\right)^2\right)} \text{ for all y>0} \tag{13}$$

The likelihood function that applies to the uncensored observations is:

$$L(\beta,\sigma^2) = \prod_{y_i|y_i>0} \frac{1}{\sqrt{2\pi\sigma^2}} e^{\left(-\frac{1}{2\sigma^2}(y_i - x_i\beta)^2\right)} \tag{14}$$

$$= \prod_{y_i|y_i>0} \frac{1}{\sigma}\phi\left(\frac{y_i - x_i\beta}{\sigma}\right) \text{ where } \phi(\text{z}) = \frac{1}{\sqrt{2\pi}}\text{e}^{-\frac{1}{2}z^2}$$

and the log-likelihood is:

$$\text{Log}\, L(\beta,\sigma^2) = \sum_{y_i|y_i>0} \log\frac{1}{\sigma}\phi\left(\frac{y_i - x_i\beta}{\sigma}\right) \tag{15}$$

For censored observations such that $y \le 0$, the probability of an observation being censored is:

$$P(y = 0) = P(y^* \le 0) = \Phi\left(\frac{-x_i\beta}{\sigma}\right) \tag{16}$$

as specified above, and the likelihood function that applies to the censored observations is:

$$L(\beta,\sigma^2) = \prod_{y_i|y_i=0} \Phi\left(\frac{-x_i\beta}{\sigma}\right) \tag{17}$$

The corresponding log-likelihood is:

$$\text{Log}\, L(\beta,\sigma^2) = \sum_{y_i|y_i=0} \log\Phi\left(\frac{-x_i\beta}{\sigma}\right) \tag{18}$$

From (6) censored data can be viewed as being mixed continuous-discrete, and combining the two likelihood functions (14) and (17) yields:

$$L(\beta,\sigma^2) = \prod_{y_i>0} \frac{1}{\sqrt{2\pi\sigma^2}} e^{\left(-\frac{1}{2\sigma^2}(y_i - x_i\beta)^2\right)} \prod_{y_i=0} \Phi\left(\frac{-x_i\beta}{\sigma}\right) \qquad (19)$$

$$= \prod_{y_i>0} \frac{1}{\sigma}\phi\left(\frac{y_i - x_i\beta}{\sigma}\right) \prod_{y_i=0} \Phi\left(\frac{-x_i\beta}{\sigma}\right)$$

And the corresponding log-likelihood is:

$$\text{Log}\, L(\beta,\sigma^2) = \sum_{y_i>0} \log \frac{1}{\sigma}\phi\left(\frac{y_i - x_i\beta}{\sigma}\right) + \sum_{y_i=0} \log \Phi\left(\frac{-x_i\beta}{\sigma}\right) \qquad (20)$$

Maximizing the likelihood (or log likelihood) function yields the maximum likelihood estimates of the parameter vector β.

3.2.6 Empirical Specification

This study models WTP to avoid a bad environmental outcome that is due to pesticide use as a function of socio-economic variables (age, education, experience with pesticide poisoning, weekly safe-water expenditure, gender, occupation, past hospitalization) and treatment type (proxy good, information, group), within the sample and in the population beyond the sample (for generalization). The amount of the endowment in this study imposed a restriction on the range of the dependent variable: minimum of 0, maximum of 500. While the population's willingness to pay (WTP) might have extended well above the maximum allowable, this sample's WTP does not, hence calling for the application of a censored regression formulation explained in section 3.2.5. Were the interest to be only in the relation between the independent variables and the dependent variable for the sub-population whose WTP is within the [0,500] range, then ordinary least squares (OLS) might have been appropriate. But my interest is in this relationship for all people for all values, and for this purpose, OLS gives misleading results. Results of the censored regression of WTP on socio-economic and treatment variables (equation 21) and tests of the ability of the variables to predict WTP are reported in Chapter 4.

$$y_i^* = \beta_0 + \beta_m \sum_{m=1}^{3} Treatment_{i,m} + \beta_k \sum_{k=1}^{7} Socio-economic_{i,k} + u_i \qquad (21)$$

m=1…3 for PGood (proxy good), Info, Group treatments respectively

k=1…7 for Gender, Educ. (education), Age, PExp (pesticide poisoning exposure), WaterExp (water expenditure), Salaried, Hosp (past hospitalization) respectively.

where:

y_i^* is WTP

$\beta_0, \beta_m, \beta_k$ are coefficients to be estimated

3.2.7 Summary of Theory on Experimental Auctions

The creation of incentive compatible auction procedures for these experiments was explained. The design involved determining the testing environment, the reward medium, size of experimental trial, information structure, number of runs, form of proxy goods and the factors to control for. The maintained underlying assumptions were monotonicity, salience, privacy and dominance. Parallelism was achieved by default. The limitation placed on the dependent variable, WTP, implied that maximum likelihood estimation was appropriate.

3.3 Experimental Procedures II: Choice Experiments

This sub-section develops the random utility framework (introduced in section 2.4.2 above) under which choice experimental data is analyzed. Also discussed here is the explanation of the experimental design, the choice data, and specification of the choice empirical models.

3.3.1 The Random Utility Model

Random utility models are used to implement an attribute-based stated choice experiment, based on random utility maximization. Rational agents respond to incentives, choosing alternatives that they perceive to yield them the highest utility. Two factors combine to influence choice of an alternative: the characteristics of alternatives (attributes) and the agent's socio-economic environment. Following Train (2003), an agent *q*'s selection of one alternative in choice occasion *t* over another implies that the utility (U_{it}) from that alternative is greater than the utility from another (U_{jt}) for that individual:

$$U_{itq} > U_{jtq} \qquad \forall j \neq i \in C_{tq} \qquad t = 1\ldots\ldots T \tag{22}$$

where C_{tq} is choice set for individual *q* in choice occasion *t*. That is, choosing alternative *i* over alternative *j* if:

$$(V_{itq} + \varepsilon_{itq}) > (V_{jtq} + \varepsilon_{jtq}) \qquad \forall j \neq i \in C_{tq} \tag{23}$$

given that utility (latent and unobservable) is random and can be decomposed into systematic (V_i) and random components (ε_i).[24] The systematic component is a function of observable attributes of both the agent and the choice. Thus, the agent obtains utility U_i while the researcher captures part of this utility in the function:

$$V_i = V(X_i) \tag{24}$$

where X_i is a vector containing all attributes, both of the agent and the alternative. The random component of individual utility captures variations in observed utility and actual utility, including variations due to unobservable alternative attributes, unobserved individual characteristics, and measurement error. The probability that an agent chooses alternative *i* among competing alternatives *j* is therefore given by:

$$P_i = Prob[(V_i + \varepsilon_i) > (V_j + \varepsilon_j)\ \forall j \neq i] \tag{25}$$

$$= Prob[(\varepsilon_j - \varepsilon_i) < (V_i - V_j)\ \forall j \neq i]$$

Equation (25) implies that the probability that a randomly drawn agent *q* chooses alternative *i* over alternative *j* equals the probability that the difference between the random utility levels of the two alternatives is less than the difference between the systematic utility levels of those alternatives in the choice set.

The distribution of the ε_i terms depends on the researcher's specification of the systematic utility. And so what determines the appropriate model to use to analyze equation (25) to estimate parameters that approximate choice behavior are the assumptions made about the distribution of the random utility component. There are two ways of thinking about this distribution: First is from the actual distribution in the population, and the other is from the subjective beliefs about the population. The following discrete choice models are derived depending on which distribution pertains.

3.3.1.1 The Logit

The logit model is derived when the ε_i are independent and identically distributed (iid) extreme value for all *i*.[25] This assumption means that variances associated with the component of a random utility expression describing each alternative in the choice set are

[24] The t and q subscripts are not employed except where confusion might arise.
[25] An extreme value distribution's pdf is skewed to the left, and its location parameter μ is equal to the mode but different from its mean.

identical and that these unobserved effects are not correlated. When the random terms are iid extreme value, their distribution in a population is given by:

$$f(\varepsilon_{iq}) = e^{-\varepsilon_{iq}} e^{-e^{-\varepsilon_{iq}}} \tag{26}$$

and the cumulative distribution is:

$$F(\varepsilon_{iq}) = e^{-e^{-\varepsilon_{iq}}} \tag{27}$$

The cumulative density function of the difference in random terms is given as:

$$F(\varepsilon_{jq} - \varepsilon_{iq}) = \frac{e^{(\varepsilon_{jq} - \varepsilon_{iq})}}{1 + e^{(\varepsilon_{jq} - \varepsilon_{iq})}} \tag{28}$$

which is a logistic distribution. The most important assumption of the logit framework is that the random terms are independent of each other, the so-called Independence from Irrelevant Alternatives (IIA) property (sometimes known as the Red Bus/Blue Bus paradox).[26] IIA implies that the ratio of the probabilities of any two alternatives does not change when the systematic utilities of other alternatives are introduced in the choice set - that the probability of an option being chosen is unaffected by the inclusion or omission of other alternatives. That is:

$$P_{iq}^{1} / P_{jq}^{1} = P_{iq}^{0} / P_{jq}^{0} \tag{29}$$

The superscripts denote before (0) and after (1) a given alternative being omitted from, or included into the choice set. When this property holds, equation (25), which is:

$$P_{itq} = \ Prob[\, \varepsilon_{jtq} < (\varepsilon_{itq} + V_{itq} - V_{jtq}) \ \forall j \neq i] \qquad t = 1, \ldots\ldots T \tag{30}$$

becomes equation (31) below:

$$P_{iq} = \frac{e^{V_{iq}}}{\sum_{j} e^{V_{jq}}} \tag{31}$$

which is a closed-form expression of the logit choice probability.[27] This choice probability has the desirable properties of probabilities, that is, its values fall within the 0-1 range and choice probabilities for all alternatives sum to one.

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[26] The Red Bus/Blue Bus paradox is explained thus: Suppose an individual has the same probability of using his car or taking the bus to a given destination, that is, P(Car)=P(Bus)=1/2. Suppose now that two buses (identical in all aspects, except color, one red, the other blue) are available and that the individual is indifferent between the two bus colors. That is P(Blue Bus) = P(Red Bus)=¼. But then the only way to maintain the original odds of using the car vs taking the blue bus would be if P(Car)=P(Blue bus)=P(Red bus) = 1/3 which contradicts the earlier odds. The introduction of another alternative changes the odds of the available alternatives.

[27] Derivation of equation (31) from equation (30) is given in **Appendix 9.**

V_i in equation (24) is the observed portion of utility, usually expressed as a linear function of the observed factors in a choice set: $V_i = \sum_{k=1}^{K} \beta_k X_{ik}$, where k denotes the attributes/characteristics in alternative *i*. In this specification the coefficients represent the marginal utilities. Within the logit framework, when the researcher aims to model choice based on option characteristics the logit is specified as a conditional logit which assumes that preferences are homogenous across individuals and what matters are the differences in attributes of the options/ alternatives (individual specific variables do not vary across alternatives and thus drop out of the utility difference in the conditional logit). The conditional logit thus estimates how alternative-specific variables affect the likelihood of observing a given outcome (P_i). (In the multinomial logit the independent variables contain individual-specific characteristics). The conditional logit is also known as the fixed-effects logistic model. The homogeneity assumption of the conditional logit means that the weights for the attributes do not vary over the population (since the unit of analysis in conditional logit models is the alternative-specific characteristics). In real life however, this assumption is likely not to hold and various methods can be used to incorporate heterogeneity.

Methods used to estimate the correct utility parameters when heterogeneity is assumed include stratifying the sample into different segments and then estimating separate parameters for each segment (Holmes and Adamowicz, 2003); or utilizing the random parameter/mixed logit framework where parameters are allowed to vary randomly over individuals (Hynes and Hanley, 2006); or using a latent class model (Hynes and Hanley, 2006) or further still, interacting individual specific variables with alternative specific variables (Mtimet and Albisu, 2006).

The IIA condition mentioned above is usually tested using the Hausman-McFadden test. In the case where it is violated, where there are similarities between the unobservable attributes over different alternatives, within the same choice set more complex methods that relax the iid assumption, considered next, are necessary. Otherwise, the logit model may over or under predict elasticities and marginal rates of substitution between attributes when the iid assumption is violated. [28]

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[28] In this study the choice set *Cq* at each choice occasion is constant and includes two alternatives, thus precluding the necessity to test the iid assumption. In effect the multinomial logit model in this case reduces to the multiple binary choice model.

3.3.1.2 Generalized Extreme Value – The nested logit

The generalized extreme value (GEV) is one of the non-iid type of models in which the unobserved portions of utility for all alternatives are jointly distributed as a generalized extreme-value. The GEV class of models allows varying levels of correlation in unobserved factors over alternatives. Alternatives are partitioned into mutually exclusive subsets, called nests, such that unobserved factors having the same correlation fall within the same group (Train, 2003). The simple GEV models maintain IIA for choices within the same nest, but may relax it for choices across the nests. The more complex GEV models allow for any pattern of correlation, fully relaxing the IIA assumption, and hence overcoming the limitations of the logit.

Following Train's (2003) exposition, let the set of alternatives *j* be partitioned into K subsets (nests) denoted by B_k for k=1…K. The utility of decision maker *q* from choosing alternative *j* in nest B_k is the same as in the standard logit. $u_i = V_i + \varepsilon_i + V_{B_k} + \varepsilon_{B_k}$ with two additional terms representing the component of utility which is common to all alternatives in nest k. However now the random terms have a cumulative distribution given by:

$$F(\varepsilon_{iq}) = e^{\left(-\sum_{k=1}^{K}\left(\sum_{j=B_k} e^{-\varepsilon_{jq}/\lambda_k}\right)^{\lambda_k}\right)} \qquad (32)$$

where the parameter λ_k is a measure of independence within a nest (indicating the degree of substitutability between unobserved utility among choices in different nests). The probability of an individual choosing alternative *i* among alternatives is represented as a closed-form expression:

$$P_{iq} = \frac{e^{V_{iq}/\lambda_k}\left(\sum_{j\in B_k} e^{V_{jq}/\lambda_k}\right)^{\lambda_k - 1}}{\sum_{l=1}^{K}\left(\sum_{j\in B_l} e^{V_{jq}/\lambda_l}\right)^{\lambda_l - 1}} \qquad (33)$$

where *l* is an alternative in nest, B_l , $l \neq k$. As can be ascertained from equation (33) above, when λ_k is 1for all nests, there is perfect independence (no correlation in the unobserved component of utility) and the nested model probabilities collapse to the standard logit (equation 31).

3.3.1.3 The Probit

When the ε terms are distributed multivariate normal i.e. $\varepsilon_q \sim N(0,\Omega)$ the probit model is derived. The model is not restricted to the iid assumption: both heteroskedasticity and correlations can be accommodated within the probit. The density function of ε_q is:

$$f(\varepsilon) = \frac{1}{(2\pi)^{J/2}\left|\Omega\right|^{1/2}} e^{-\frac{1}{2}\varepsilon_q \Omega^{-1}\varepsilon_q} \quad (34)$$

And the probability of choosing alternative *i* over *j* from equation (30) can be obtained as an integral over all values of the ε_q . Since the choice probability in the probit below is not a closed-form, estimation is by simulation.

$$P_{iq} = \int I(V_{iq} + \varepsilon_{iq} > V_{jq} + \varepsilon_{jq} \ \ \forall \ j \neq i)f(\varepsilon_q)d\varepsilon_q \quad (35)$$

where *I(.)* is an indicator of whether the statement in the brackets is accepted or rejected. Although the probit allows flexibility in variance and correlation, its limitation is the reliance on normality. When the unobserved component of utility is not normally distributed, the probit gives incorrect predictions.

3.3.1.4 The Mixed Logit

When the researcher perceives some component of random utility to be distributed iid and some other distribution, then a mixed logit may be specified. That is, the unobserved factors can be decomposed into a part that is iid extreme value (no correlations, no heteroskedasticity) and another part where correlations, heteroskedasticity and non-normality are allowed. This model is also referred to as the random parameter logit, or mixed multinomial logit, and is more general than the rest because it allows for almost any type of data and error distributions. Following Train (2003), and using the general representation of utility,

$$\begin{aligned} U_{iq} &= V_{iq} + \varepsilon_{iq} \\ &= \beta_q X_{iq} + \varepsilon_{iq} \end{aligned} \quad (36)$$

where X_{iq} are observable characteristics and β_q is not fixed, but allowed to vary across individuals. That is, the β_q parameters are random, having a distribution (with a mean and standard deviation). Then the probability of individual choosing alternative *i* can be obtained by estimating P_{iq} over all possible values of β .

$$M_{iq} = \int P_{iq}(\beta)\, f(\beta)\, d\beta \tag{37}$$

$$= \int \left(\frac{e^{\beta X_{iq}}}{\sum_{j} e^{\beta X_{jq}}} \right) f(\beta)\, d\beta \tag{38}$$

Thus in a way, a mixed logit is a weighted average of the logit formula evaluated at different values of the parameter β, with the weighs given by the density *f(*β*)*.The distribution on *f(*β*)* can be imposed from subjective beliefs of the researcher about the population. If *f(*β*)* is constant (the parameter is fixed, with zero standard deviation such that all behavioral information is captured by the mean) then such a model reduces to the standard logit model considered above. Other distributions (such as normal, continuous, log-normal and so forth) allow estimation of choice probabilities using simulation.

3.3.1.5 Summary Theory of Discrete Choice Models

Discrete choice models assume that decision makers choose alternatives with the highest utility. The logit framework, the simpler discrete choice model, was derived above as it forms the basis of other formulations. Its ease in computation and interpretation are among its popular characteristics. However, the assumption of iid limits its wide application. Models developed to circumvent the assumption of independence of irrelevant alternatives that is violated in many cases include the generalized extreme value type models, the probit and mixed logit models. The decision of which model is appropriate depends on the underlying assumptions about the distribution of the unobservable component of utility. In either situation, the systematic component of utility is modeled as a function of the attributes of the alternative, the agent's attributes, or both.

3.3.2 Welfare Estimation Using Choice Probabilities

The main aim of estimating parameters in a choice model is to approximate choice behavior. That is: determining the relative importance of attributes as chosen by decision making agents, the trade-offs individuals make between these attributes; and the outcome or benefits from these choice decisions. These trade-offs and benefits can be obtained from the choice probabilities estimated from a logit model and converted into monetary values using consumer surplus theory.

Train (2003) explains how the choice probabilities obtained from equation (31) can be extended into welfare measurements. Consumer surplus is a monetary measure of the change in utility that a person obtains in each choice situation, that is, the welfare gains from a change in behavior or from a change in attributes of alternatives that individuals prefer. It is calculated from:

$$\mathrm{CS}_q = (1/\mu_q)\ \max_j (U_{jq} \quad \forall\ j\ ) \tag{39}$$

where μ_q is the marginal utility of income also referred to as a scale parameter. μ_q measures the change in utility that arises from a unit change in the cost or price variable, and is inversely proportional to the variance of the unobserved utility, that is, $\sigma^2 = \frac{\pi^2}{6\mu^2}$. The scale parameter is not identifiable and the usual practice is to arbitrarily set it to a convenient value such as one (Holmes and Adamowicz, 2003; Karlstrom and Morey, 2003). When this is done the variances of the random components of the utilities are assumed equal. If each ε_i is distributed iid Extreme Value and utility is linear in income then expected consumer surplus can be obtained as:

$$\begin{aligned} \mathrm{E}(\mathrm{CS}_q) &= \frac{1}{\mu_q} E[\max{}_j (V_{jq} + \varepsilon_{jq} \quad \forall\ j) \\ &= \frac{1}{\mu_q} \ln\left(\sum_{j=1}^{J} e^{V_{jq}} \right) + C \end{aligned} \tag{40}$$

where V_{jq} is systematic utility, C is a constant (referred to as the Euler constant = 5.7722), and the change in consumer surplus or compensating variation (CV) that results from a change in the alternatives (before and after a change is made) is calculated as:

$$\mathrm{CV}= \Delta\ \mathrm{E}(\mathrm{CS}_q) = \frac{1}{\mu_q}\left[\ln\left(\sum_{j=1}^{J^1} e^{V^1{}_{jq}} \right) - \ln\left(\sum_{j=1}^{J^0} e^{V^0{}_{jq}} \right) \right] \tag{41}$$

The compensating variation welfare measure in (41) is for a single choice occasion, *t*, but can also be used in the case of multiple alternatives. Then

$$CV = \frac{1}{\mu}(V^1 - V^0) \tag{42}$$

The superscripts on the representation of systematic utility denote before (0) and after (1) a change has been effected. The CS estimate gives the amount that individuals are willing to pay to move from one situation to another. As used in this study, the compensating variation estimates willingness to regain a cleaner environment and better health due to reduced pesticide use. When applied to the relevant population, these estimates translate into welfare effects or benefits from any program that reduces pesticides in the environment.

The relative importance of attributes in equation (24) is obtained from the magnitude of the coefficients and their significance. Likewise an attribute's significance in a chosen alternative is an indication of its importance in a choice decision. Using this information an implied ranking of characteristics is possible, based on calculation of implicit values. Implicit values for a characteristic are obtained by dividing the preference parameter by the negative of the coefficient on the cost attribute, $\frac{\beta_k}{-(\beta_p)}$. Moreover the tradeoff individuals make by choosing an attribute in one alternative over another can also be captured from the choice probability as the ratio between the two coefficients. This trade-off is the marginal rate of substitution between any two attributes and is given by:

$$MRS_{km} = \frac{\partial V_i}{\partial X_k} \Big/ \frac{\partial V_i}{\partial X_m} = \beta_k / \beta_m \tag{43}$$

The extent to which a choice probability changes in response to a change in some observed factor can be obtained using elasticity. The elasticity of the choice probability for alternative *i* with respect to another observed variable is the percentage change in the choice probability of *i* that is associated with a one-percentage change in probability of another variable.

$$\begin{aligned} E_{i,x_{iq}} &= \frac{\partial P_{iq}}{\partial x_{iq}} \frac{x_{iq}}{P_{iq}} \\ &= \frac{\partial V_{iq}}{\partial x_{iq}} x_{iq}(1-P_{iq}) \quad = \beta_x x_{iq}(1-P_{iq}) \end{aligned} \tag{44}$$

when systematic utility is linear in the observable variables with β_x being the coefficient.

3.3.3 Experimental Design: Choice Experiments

According to Batsell and Louviere (1991) analysis of choice experiments requires four steps: Designing the formal experiments; estimating the model parameters from data collected in choice experiments; testing model assumptions; and using the estimated models to forecast choice behavior in real markets. To permit the best possible inferences to be made from choice experimental data, planning which observations to take and how to take them is important. One of the biggest challenges in implementing a choice experiment is the determination of combinations of attributes and their levels. There doesn't seem to be consensus on how choice sets should be generated. Some designs use all possible paired comparisons of profiles, while others use random pairing of profiles. A common goal, however, is to maximize two statistical design properties: orthogonality and balance. Orthogonality implies that across the design, the attributes and attribute levels are uncorrelated with one another (and hence ensures precision of estimates), and balance requires that in the final design all levels of the attributes occur with equal frequency. These statistical design measures must no doubt be considered in combination with non-statistical measures such as task complexity and realism of choices.

The most common statistical design that also has attractive statistical properties is the factorial design, in which each level of each attribute is combined with every level of all other attributes (Louviere et al., 2000). The outcome of a complete factorial (also known as the universe set) guarantees that all attribute effects are independent and achieves the highest quality measured in terms of efficiency. However because full factorial combinations grow exponentially in size and complexity as the number of attributes and attribute levels is increased, the fractional factorial design (FFD) may be more applicable (Louviere et al., 2000).

A fractional factorial design is one in which only a fraction of the treatment combinations is required. In addition to restricting the set of choices to manageable proportions, the FFD is based on the statistical concept that higher-order interactions although significant, may not all be of interest and present interpretation problems. Model variance may be explained by main effects only, and if statistical efficiency is not the only factor under consideration, a less orthogonal design may be adequate (Alpizar et al., 2001).

In general the design should be large enough to allow estimation of the model. Also it should be small enough to be practical. So the idea is to balance the two issues of estimability (which prefers larger designs) and practicality (which prefers smaller designs) bearing in mind that the benefits from building a large number of choice sets may be small compared to the benefit from a small practical number of choice sets.

The process of generating choice sets to present to this study's participants entailed determining the following:

- The type of attributes to include

- Number of attributes
- Level of attributes
- Measurement scale of the attributes, and
- The statistical design to use (combination of attributes and their levels).

As was mentioned earlier, results from the pretests excluded other environmental categories in Table 3.3. [29] As such only the Ground/Surface water choice experiments were conducted. In this environmental category, two attributes drinking water, water used for agriculture (DRINK, AGRIC) at three levels (VerySafe/Always safe (100%), NotVerySafe/Sometimes safe (50%), NeverSafe(0%)) and the third (COST) attribute at 5 levels (Shs. 0, 100, 200, 300, 500) were varied creating a 3^2x5 mixed-level design. Unlike in many choice modeling studies where the qualitative attributes are strictly binary, I chose to vary them over three ordinal levels in order to obtain more information from respondent choices. [30] For these attributes and attribute levels, and for a choice set with two options the full factorial design containing every possible combination yielded a large set of (3^2x5)x(3^2x5) unique (including implausible and dominated) choice sets.

Although such a design achieves perfect orthogonality and balance it is impossible to administer. The PROC PLAN procedure in SAS generated 45 feasible profiles for the fractional factorial out of which the implausible ones were eliminated. For example, a choice set that involves water that is considered safe for human consumption, but unsafe for agricultural crops and animals is implausible and such profiles were removed. Using the remaining profiles all possible pairs of combinations were constructed. An assumption was made that no interactions among attributes were present so a main effects only model was adopted. After elimination of pairs in which one option was clearly dominated, a total of 180 feasible options remained. This elimination method is akin to that explained in Louviere et al., (2000).

[29] This substantial reduction in the experimental design (categories, attributes) is not uncommon. Hanley et al (1998) set out to conduct a valuation of various categories of a forest landscape but could effectively value only a sub-component of it. The original plan for this study may have been a somewhat ambitious effort and also probably unrealistic, at least at this level, given limitations on time and other resources.

[30] There may be benefits to increasing the scale, that is, increasing the levels of these attributes. However, as discussed earlier, it comes with increased task complexity.

Table 3.3: Attributes and Attribute Levels for Health and Environmental Categories

Attribute	Description	Levels
Ground/Surface Water Quality		
Drinking	Perception of water as safe for drinking	Safe, Only after treatment, Never
Agricultural	Degree to which water is safe for crops and farm animals	Very (100%), Sometimes (50%), None
Cost	The total cost for an individual for each alternative	Shs 0, 100, 200, 300, 500
Birds/Beneficial Insects		
Numbers	Expressed as level of birds/insects seen/heard on a regular day	Few, Moderate, Many
Cost	The total cost for an individual for each alternative	Shs 0, 100, 200, 300, 500
Aquatic Life		
Stock	Expressed as level of catch per fishing trip	Low, Medium, High
Size	Expressed as size of catch	Small, Medium, Large
Cost	The total cost for an individual for each alternative	Shs 0, 100, 200, 300, 500

To further reduce the number of choice sets to a manageable level, the second level of elimination required weighting the variables and running a dominance analysis. This procedure involved attaching arbitrary weights to the 3-level attributes - DRINK and AGRIC, and the resulting set inspected for dominance. Suppose that safety of water for drinking is regarded *n* times higher than safety of water for agricultural purposes. Then a choice set that involved comparison of two options in which the level of the DRINK attribute and AGRIC attribute were almost the same would most probably be biased towards respondents choosing the DRINK attribute if the levels of the COST attribute are not sufficiently varied. This second-level dominance assessment was necessary to ensure that respondents are not presented with poorly matched attribute options that result in illogical comparisons. Finally the remaining options were assessed to ensure the final design is balanced (each level of each attribute occurs with equal frequency). Both procedures increased the quality of the design.

One pair was then randomly selected from this feasible set. The process was repeated until the desired number of sixteen 2-option choice sets was obtained. The resulting sets were structured in a table format so as to make assimilation of the information as easy as possible and then printed (see **Appendix 10** for all choice sets). The sixteen choice sets were later checked for design efficiency using PROC OPTEX. The highest efficiency attainable for this number of choice sets using the modified Fedorov procedure was 99.3%. The final design's D-efficiency was 87.2% (see **Appendix 11** for the SAS code that tested the optimality of the final design).[31]

3.3.4 Choice Experimental Data

Data collected was in two sections: answers to choice questions and questions on socioeconomic characteristics. Socio-economic data on respondents such as their age, level of education, gender, locality, and main source of livelihood was collected (during the post-experimental auction evaluation. All subjects signed the consent form). This data is important to highlight any underlying differences in willingness to pay functions for different individuals and for estimating the multiple logit models. Cost was added as another attribute denoting respondents' willingness to pay to avoid a bad environmental situation. Prior to the study, it had been proposed that to make this study's payment vehicle more realistic, urban respondents would be asked to express their payment in terms of an increase in the price of petrol while rural respondents would be asked to express their payment in terms of an increase in the price of paraffin.[32] The idea of selecting an appropriate payment vehicle is to find one that is sensitive enough to make subjects think about their bidding behavior as truthfully as possible. In any case, since the actual 'change' proposed in this experiment could not be effected, such a method is not incentive compatible.

Each respondent was required to evaluate 8 (out of 16) different choice sets by simply identifying their preferred option (viewing only one choice set at a time) until all choice sets were completed.[33] These choice sets were given in a random order so that participants' responses are not affected by ordering effects. Respondents who did not complete at least

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[31] The modified Fedorov is a procedure used to generate statistically efficient linear choice designs through an iterative algorithm that maximizes the determinant of the $X'X$ matrix where the X's are factors (attribute levels) in a choice design (Zwerina, Huber and Kuhfeld, 2007).

[32] The discussion, first with the agricultural extension coordinator, and then with the pre-test group indicated that simple statements of monetary value would be sufficient. An appropriate payment vehicle in this regard was hard to select because no single vehicle can represent the study area population unlike, say, in the USA where income taxes can be used. One alternative was to ask the urban subjects their willingness to accept a rise in public transportation (taxi) fares. Concerns with this payment vehicle were that it would necessarily exclude those subjects who did not require public transportation (that is, those with personal vehicles) or be biased due to widespread sentiments against the current transport system administration.

[33] As discussed above, only the urban sub-sample evaluated choice sets. Also, two 8-member groups in Kampala sub-sample involved local market traders whose level of education was not deemed sufficient to complete the choice task. This reduced the total obtainable choice responses.

50% of the choice task had their partial responses dropped from the analysis.[34] The final total number of responses was 1,056.

Data were entered into a spreadsheet file. Choice data were divided into a number of blocks, with each block representing an individual choice occasion, and each row within a block corresponding to an alternative within that choice set. If *l* is the number of choice occasions, *m* the number of options and *n* the number of cases, then *lmn* rows of data were necessary. For the 2 alternatives (each with 3 attributes), and 8 choice occasions for each respondents, the following procedure was used to create the data rows: For each respondent, two rows were created representing option A and option B. Then eight such rows were created representing eight choice tasks that each respondent faced. The same was done for all 132 respondents. The final *lmn* data set consisted of a balanced panel of 2112 rows of observations. The data grid combined data from the choice questions with the socio-economic and other information obtained from the post-experiment evaluation. (Data for all the analyses is included in **Appendix 12**).

3.3.5 Empirical Specification

As mentioned in 3.3.1 above, and in the following specifications, participant choice of an alternative in a choice set is assumed to be influenced by various factors and it is important to test which factors are influential. Two model specifications are tested: the binary logit and the conditional logit. The significance of the estimated models is the justification of the appropriate model to adopt.

3.3.5.1 Multiple logit specification

$$V_i = V_i(x_{itq}) \qquad i=A, B\ , \qquad t=1\ldots8 \tag{45}$$

$$V_i = \sum_{k=1}^{k} \beta_k x_{iq}$$

where X_{iq} are the attributes for individual q choosing an alternative for each *t*.

$$V_i = \beta_1 Group_{iq} + \beta_2 Educ_{iq} + \beta_3 Gender_{iq} + \beta_4 Age_{iq} + \beta_5 Salary_{iq} + \beta_6 PExp_{iq} + \beta_7 illever_{iq} + \varepsilon_{iq} \tag{46}$$

[34] It is not uncommon for useable responses from choice experiments to significantly fall below the total number of responses. In Hanley et al (1998), the effective sample was reduced from 284 to 181.

This model assumes that individual's choice of alternative *i* over *j* is based in the characteristics of the individual and not on the attributes of the alternative, that is, the model assumes heterogeneity of individuals.

3.3.5.2 Conditional logit (fixed-effects model) specification

The empirical specifications of the utility levels underlying the conditional logit make references at the attributes of each choice and are formulated as:

$$V_i = \sum_{k=1}^{k} \alpha_k x_{ik}$$

$$V_i = \alpha_1 Drink_{ik} + \alpha_2 Agric_{ik} + \alpha_3 Cost_{ik} + \nu_i \qquad (47)$$

This specification implies that the model to use for estimation is one that assumes that individuals have the same preferences. When k=price, then α_k is the change in utility associated with a marginal increase in income, and $-\alpha_k$ registers the marginal utility of money.

3.3.5.3 Estimation of coefficients and model performance

The coefficients in equations (46) and (47) are estimated using the method of maximum likelihood. The technique involves determining the value of α_k and β_k that maximizes the probability that the sampled respondents would choose the alternative that they actually chose. The method of maximum likelihood assumes that the sample in the model mimics the population.

When the preference parameters are multiplied by the level of the corresponding attributes, the utility associated with that given level of attribute, called part-worth utility is attained. When the utility function is additively separable, then the total preference valuation of an alternative is the sum of the individual part-worth utilities of attributes in the alternative. For example in (41) V_i, the utility from alternative *i* can be aggregated over all individuals to obtain alternative *i*'s overall utility.

The most often used goodness of fit measure with maximum likelihood is the log likelihood ratio index (LLI), reported as the pseudo R^2 also known as the McFadden's R^2. This ratio is a measure of how well the model performs and is defined as $1-\frac{LL(\hat{\beta})}{LL(0)}$, where $LL(\hat{\beta})$ is the value of the log likelihood function at the estimated parameters and LL(0) is the value of the function without any variables. The values of this index range from 0 to 1 with zero indicating that the estimated model is no better than no model and is 1 when choice behavior is perfectly predicted. Although both the pseudo R^2 and R^2 of OLS have the same range and the value rises when the model is expanded, the ratio index cannot be interpreted analogous to R^2. A pseudo R^2 value of 1 is highly unlikely in maximum likelihood estimation because, as noted in 3.3.1, the researcher does not have enough information to predict the decision maker's choice. The likelihood ratio Chi-Square tests that at least one of the variables in the model is not equal to zero. The statistic is calculated by $-2(LL(0)-LL(\hat{\beta}))$. The t-values associated with the parameters are interpreted in the same way as in OLS and indicate the statistical significance of the estimated parameters.

4.0 Results and Discussion

4.1 Introduction

This chapter presents results from thirty five experimental auction sessions in Iganga and Kampala districts, plus results from one hundred and thirty two choice experimental subjects in Kampala. Section 4.2 is a descriptive summary providing information about the experimental subjects and variables. The third section focuses on results from the experimental auction models, while section 4.4 presents and discusses empirical results from the discrete choice experiments. Kampala and Iganga sub-samples are treated as separate experiments as the variables for the two samples are not similar. Thus, results in the following sections are presented for each sub-sample separately.

4.2 Descriptive Analysis

General descriptive statistics such as means, percentages and standard deviations are presented for the variables included in the model, as are frequency distributions, plots of data, and comparison of some characteristics.

4.2.1 Urban Sub-sample

The Kampala sample consisted of relatively highly educated respondents having spent an average of sixteen years in school. A typical respondent was aged 35 years and spent about $1 weekly on safe drinking water. The sample in general has had little exposure to pesticide poisoning or hospitalizations from related illness and consisted of slightly more males than females. Table 4.1 presents summary statistics of the one hundred and fifty six respondents.

Table 4.1: Summary of Kampala Sub-Sample Characteristics

Characteristics	Definition (Coding)	Percentage		
Gender	Male (1)	56.12		
	Female (0)	43.88		
Proxy good	Groundnut (1)	42.95		
	Water (0)	57.05		
Info	Information given (1)	43.59		
	No information given (0)	56.41		
PExp	Previous exposure to poisoning (1)	11.54		
	No pesticide exposure (0)	88.46		
Group	Group decision making (1)	51.28		
	Individual decision making (0)	48.72		
Hosp	Hospitalized in past year (1)	19.23		
	Not hospitalized in past year (0)	80.77		
Illever	Been ill in past year (1)	37.82		
	Not ill in past 12 months (0)	62.18		
Occupation	Salaried (1)	40.14		
	Non-Salaried (0)	59.86		
		Mean (Std. Dev.)	Min	Max
Education	Years completed in school	16.30 (2.414)	10	30
WaterExp	Money (Shs.) spent weekly on safe-to-drink water	1,890.7 (2,297.887)	0	14,000
Age in years		35.54 (8.259)	20	59
Amount	WTP bid amount (Shs.)	343.968 (142.87)	0	500

The distribution of WTP values is highly skewed to the left (see Fig 4.1), with the mean willingness to pay to avoid an unfavorable environmental situation (Shs. 343.97) higher than half the subjects' endowment. Both the mode (Shs. 500) and median (Shs. 500) WTP values are higher than the average WTP.

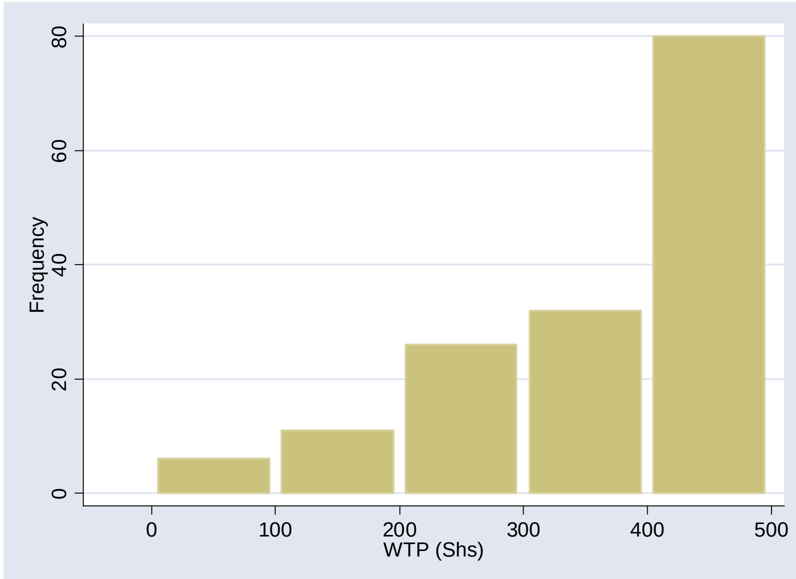

Figure 4.1: Frequency distribution of WTP, Kampala. (Source: Bonabana-Wabbi, 2007)

The distribution of bidding behavior between and within groups is assessed using the box-and whisker diagram (Figure 4.2). The box-and-whisker plots graphically show the smallest observation, the lower quartile, the median, upper quartile, and the largest observation. The median WTP values for people involved in information treatments are equal (at Shs. 400). The medians for other groups are different as shown by the horizontal line within each box. Except for the WTP values for the occupation variable, the lowest and highest values as represented by the whiskers in each plot span the entire range from 0, to the total endowment, 500. In the occupation box plot, the lowest WTP amount for the salaried subjects was Shs.100. Also, the median WTP amount was highest with people who have had experience with pesticide poisoning. The difference in medians between any groupings is largest with the 'Group' treatment. While the differences in medians between treatments for Gender, Occupation, Proxy Good are about Shs.100, differences in medians between the pesticide poisoning experience, and group are in excess of Shs. 100 (20% of disposable income).

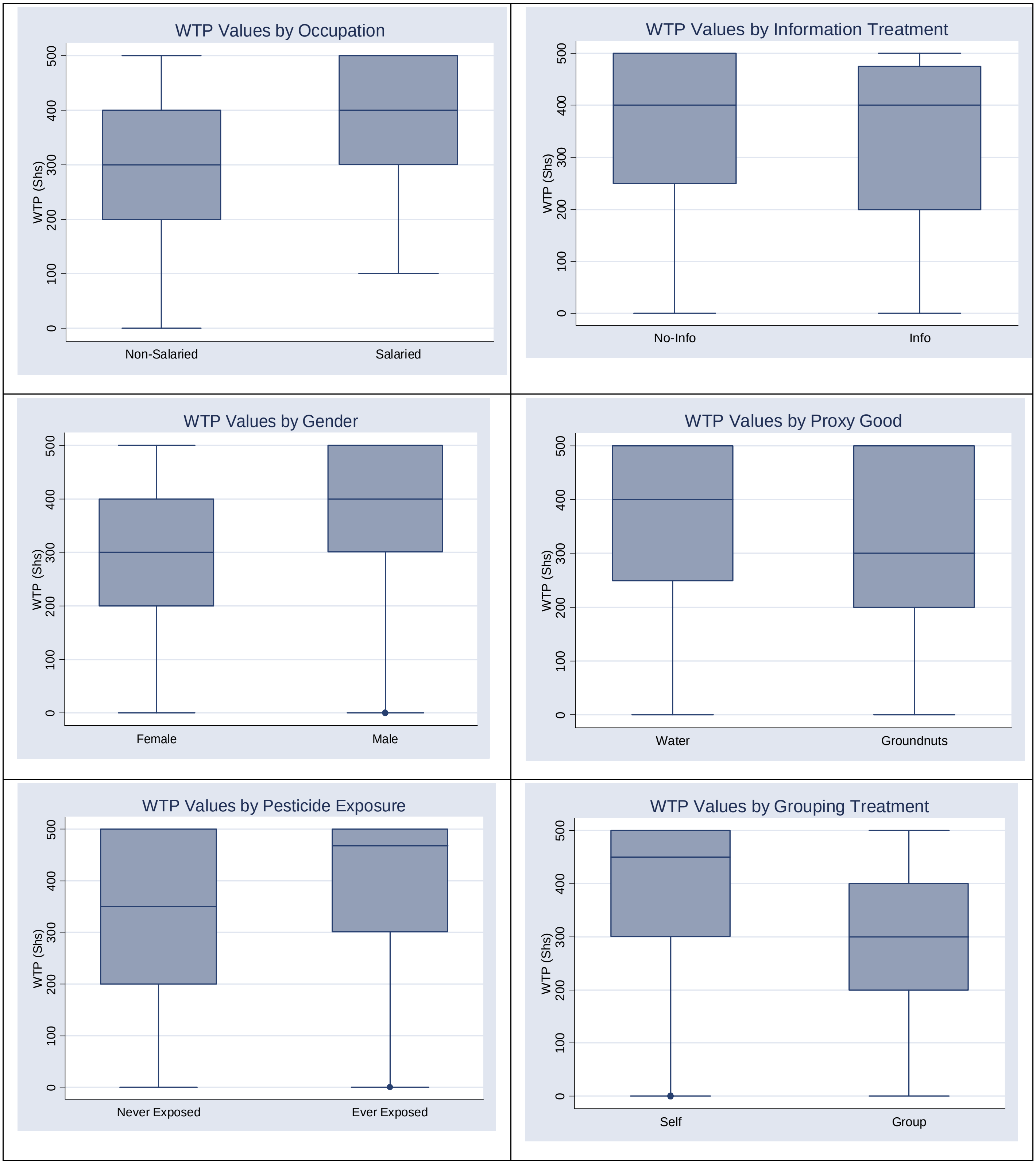

Figure 4.2: Box-and whisker diagrams of WTP values for categorical variables, Kampala. (Source: Bonabana-Wabbi, 2007)

To test whether the groups formed by the categories of the independent variables (Gender, PExp, PGood, Info, Group, Hospitalized, Salaried) are similar, analysis of variance (ANOVA) is used. The independent variables have an effect on the dependent variable, WTP, if the groups within each variable are different.

Analyses of variance for this data set (Table 4.2) show that three variables (Group, Hosp, Salary) are statistically significant at the alpha level of 0.05. Two variables (Gender and PExp) are marginally significant while PGood and Info are not significant. Willingness to pay estimates of subjects involved in the group treatments are different from values in the individual treatments. Salaried subjects have different WTP values compared with their non-salaried counterparts. Also differences exist between males and females' WTP values. The other treatments: Information and Proxy Good did not seem to significantly affect bidding behavior. In general, females, non-salaried subjects and those who participated in group treatments had lower bid values than males, salaried subjects and subjects in the 'individual' treatments.

Table 4.2: Univariate ANOVAs for categorical variables, Kampala

	Source	SS	Df	MS	F	Prob>F
Group	Between groups	227477.422	1	227477.422	11.94	0.0007
	Within groups	2916007.42	153	19058.872		
	Total	3143484.84	154	20412.2392		
Gender	Between groups	64213.2354	1	64213.2354	3.29	0.0717
	Within groups	2650910.13	136	19491.9863		
	Total	2715123.37	137	19818.4188		
PExp	Between groups	55962.4441	1	55962.4441	2.77	0.0979
	Within groups	3087522.39	153	20179.8849		
	Total	3143484.84	154	20412.2392		
PGood	Between groups	14623.9288	1	14623.9288	0.72	0.3991
	Within groups	3128860.91	153	20450.0713		
	Total	3143484.84	154	20412.2392		
Info	Between groups	6285.68388	1	6285.68388	0.31	0.5806
	Within groups	3137199.15	153	20504.5696		
	Total	3143484.84	154	20412.2392		
Hosp	Between groups	103711.872	1	103711.872	5.22	0.0237
	Within groups	3039772.97	153	19867.7972		
	Total	3143484.84	154	20412.2392		
Salary	Between groups	77460.3926	1	77460.3926	3.99	0.0480
	Within groups	2407892.98	124	19418.4918		
	Total	2485353.37	125	19882.827		

Note: See Table 4.1 for a definition of the variables

4.2.2 Iganga Sub-sample

Limited information on socio-economic characteristics of the rural sub-sample was obtained. Information such as weekly expenditure on safe water, occupation (other than farming), or exposure to pesticide poisoning, no doubt important variables, were obtained only from 26 participants who responded to the post-experiment evaluation. As such, analysis of willingness to pay behavior of respondents for the rural sub-sample is somewhat limited.

The rural sub-sample consisted of 121 respondents. On average Iganga respondents were willing to pay 57% of their endowment to avoid an unfavorable environmental situation. The sample also had slightly more men than women. The sample's mean age was 40 years with at least 6 of those years having been spent in school. About equal numbers were involved in the three treatment types (PGood, Information, Group). These statistics are summarized in Table 4.3 below.

Table 4.3: Summary of Iganga Sub-sample

Characteristic	Definition (Coding)	Percentage		
Gender	Male (1)	57.85		
	Female (0)	42.15		
Proxy good	Groundnut (1)	50.41		
	Water (0)	49.59		
Info	Information given (1)	51.24		
	No information given (0)	48.76		
Group	Group decision making (1)	47.11		
	Individual decision making (0)	52.89		
		Mean (Std.Dev)	Min	Max
Education	Years spent in school	6.628 (3.05)	1	16
Age	Years	40.02 (10.37)	20	70
Amount	WTP bid amount (Shs)	286.86 (135.87)	0	500

The distribution of the WTP values is shown in figure 4.3 below. The WTP values for Iganga were more normally distributed than those for the urban sample. The median category of WTP is also the mean category (Shs. 201-300).

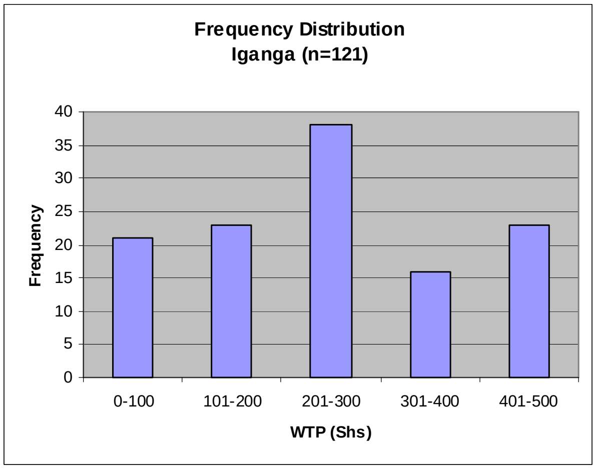

Figure 4.3: Frequency distribution of WTP, Iganga. (Source: Bonabana-Wabbi, 2007)

Box-and-whisker diagrams for the Iganga data are presented in figure 4.4. In all diagrams the median WTP is equal to Shs. 300, a further indication of the normality of this distribution. The box plot for proxy good (PGood) shows that subjects involved in water treatments had their lowest bid at Shs. 100. Other categories of the sample that had their lowest WTP bids different from zero (excluding the outliers – represented by dots in the diagrams) are females, and those who participated in group treatments. These same categories also had more values confined within a smaller inter-quartile range than any other categories. Of the four box plots, the smallest difference in dispersion parameters is seen in the information treatment variable. The lowest WTP, median, lower and upper quartile values are similar between subjects who were given information and those without.

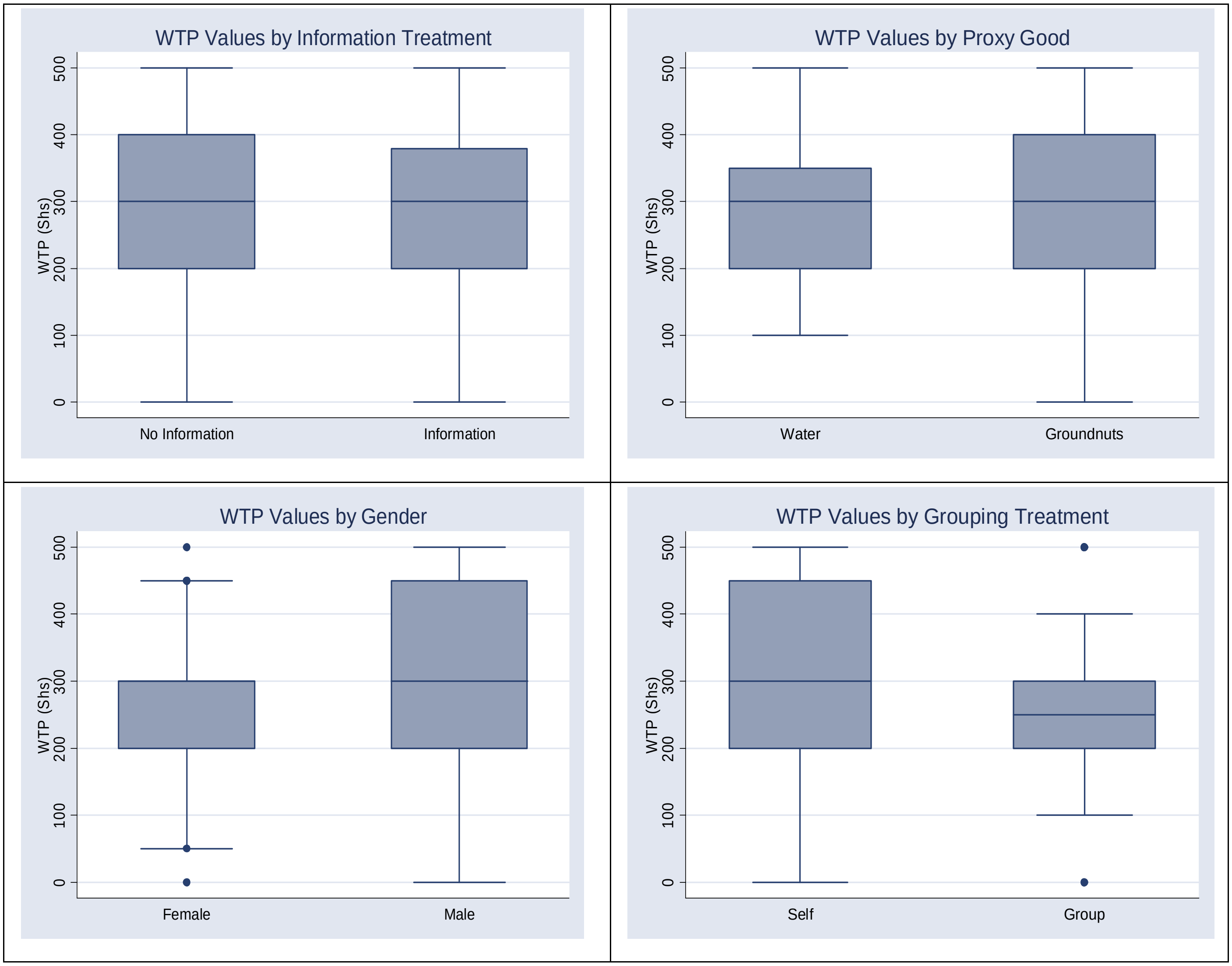

Figure 4.4: Box-and-whisker diagrams for WTP values for categorical variables, Iganga. (Source: Bonabana-Wabbi, 2007)

One-way analyses of variance indicate that the Group variable is significant at the 5% level. The gender variable is significant only at the 10% level. The other treatment variables (PGood, Info) are not significant. That is, there are no significant differences between WTP values for respondents involved in either category of the 'proxy good' or info treatments. Table 4.4 shows results of ANOVAs.

Table 4.4: Univariate ANOVAs for categorical variables, Iganga

	Source	SS	Df	MS	F	Prob>F
Group	Between groups	95969.8791	1	95969.8791	5.39	0.0220
	Within groups	2119436.73	119	17810.3927		
	Total	2215406.61	120	18461.7218		
Gender	Between groups	51263.7824	1	51263.7824	2.82	0.0958
	Within groups	2164142.83	119	18186.0742		
	Total	2215406.61	120	18461.7218		
PGood	Between groups	1479.83561	1	1479.83561	0.08	0.7784
	Within groups	2213926.78	119	18604.4267		
	Total	2215406.61	120	18461.7218		
Info	Between groups	10199.5312	1	10199.5312	0.55	0.4596
	Within groups	2205207.08	119	18531.1519		
	Total	2215406.61	120	18461.7218		

Note: See Table 4.3 for a definition of the variables

General results for the rural sub-sample show that males were willing to pay higher values than females. Like in the urban sub-sample willingness to pay values for respondents involved in 'group' treatments were lower than those for respondents in individual treatments.

4.2.3 Conclusions on Urban, Rural Subject Pool and Variables

Variables in the two samples were different and so a comparison is made in Table 4.5 based on the common variables for the rural and urban sub-samples. About the same proportion of males (slightly more males than females) took part in the experiments in both Kampala and Iganga. The urban sub-sample was more formally educated than the rural sub-sample and appeared to be more concerned about their health than the rural population. That is, farmer's willingness to pay to avoid bad health outcomes was lower than that of non-farmers.

Table 4.5: Summary Comparison of Kampala and Iganga sample Characteristics

	Kampala	Iganga
Education (Mean years)	16.3	6.6
Age (years)	35.54	40
Gender: Males (% of sub-sample)	57	56
WTP (% of endowment)	68.8	57.4

4.3 Results of Experimental Auctions

This section presents results of maximum likelihood estimations obtained using the Tobit model for both data sets. The estimation equation (21) is repeated below:

$$y_i^* = \beta_0 + \beta_m \sum_{m=1}^{3} Treatment_{i,m} + \beta_k \sum_{k=1}^{7} Socio-economic_{i,k} + u_i \qquad (48)$$

4.3.1 Kampala Sub-sample

The general-to-specific approach is adopted. Model 1 is the full model including all hypothesized socio-economic and treatment variables. The model is significant at the 1% level with a log likelihood of -600.359. The LR test for the null hypothesis that all coefficients except the intercept are jointly zero, is significant at the1% critical value level. Using stepwise elimination of variables, an attempt was made to have a model with only socio-economic variables. Model 2 retained three socio-economic variables and included one treatment variable and is significant at the 10% level. Model (3) aimed to include all treatment variables. However these all being binary limits the number of such variables that can be included in the model. After dropping insignificant variables the final model retained the Group treatment and two socio-economic variables and is significant at the 1% level.

Table 4.6: Maximum likelihood estimates of WTP – Kampala

	OLS		Model 1		Model 2		Model 3	
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Age	-1.4815	1.522	-1.827	1.977	-.3870	1.966	-	-
Gender	29.447	25.671	42.899	33.677	-	-	47.460[d]	31.968
Educ	.885	5.412	1.145	7.060	.6476	6.772	-	-
PGood	-49.682[c]	27.868	-69.821[c]	36.757	-	-	-	-
Info	-14.657	25.734	-25.494	33.599	-	-	-	-
Group	-42.831[d]	26.674	-66.551[c]	35.346	-91.3303[a]	33.528	-100.738[a]	32.762
PExp	61.343[d]	38.232	93.393[c]	53.005	-	-	-	-
WaterExp	.0091[c]	.00544	.0147[c]	.00743	.01036[d]	.0074	.0108[d]	.00737
Hosp	-37.833	29.174	-52.012[d]	37.643	-	-	-	-
Salaried	78.326[a]	27.788	123.115[a]	37.267	-	-	-	-
Constant	360.574	104.99	387.706	137.276	399.1323	126.476	376.4491	34.575
R^2	.1770		-	-	-	-	-	-
Prob >F	.0133		-	-	-	-	-	-
Log L/hood	-		-600.3599		-656.8267		-671.054	
LR χ^2 (df)	-		27.69 (10)		9.24 (4)		13.38 (3)	
Prob > chi2	-		0.0020		0.0553		0.0039	
Pseudo-R^2	-		.0225		.0070		0.0099	
N	156		156		156		156	

[a] Significant at 1%, [c] Significant at 10%, [d] Significant at 20%

All three models show that the grouping treatment variable (**Group**) was significant in predicting WTP values. In addition, (at a lower significance level) previous exposure to pesticide poisoning and weekly water expenditure had an effect on WTP. The other variables and treatments did not show significant relationships with WTP values. All socio-economic variables (except gender) were unable to predict bidding behavior.

The negative sign on the **Group** variable implies that subjects who participated in the group treatment were more likely to have lower WTP values than those who participated in 'self' treatments. The (weak) positive sign on **WaterExp** and **PExp** variables suggests that concern about exposure to pesticide poisoning influenced people to bid higher to avoid bad environmental outcomes. The inclusion of both these variables was to act as a proxy to measure how risk averse people are (either in general or due to past exposure). Past experience with pesticide poisoning appears to make people more averse to taking risks with their lives. In addition, people who spend on (purchase) safe drinking water are also likely to pay higher values to avoid pesticides in their environment.

In Model 1 the expected WTP increases by Shs. 0.0146 when water expenditure increases by Shs. 1, holding all other variables in the model constant. That is, the more a person is willing to spend on safe drinking water, the more this person is willing to pay to avoid a bad environmental situation. In addition, a salaried person's expected WTP was Shs. 123.11 (24.6% of subject endowment) higher than a non-salaried person, all else being equal.

A general comment on the three models is necessary here. Important to note is the consistency in signs on coefficients of variables across the models. The coefficient on **Age** and **Group** variables is consistently negative and that on the **Educ**, **WaterExp** and **Gender** variables is consistently positive. OLS estimates for the full model also presented in Table 4.6 have the same directional effect on WTP as maximum likelihood estimates. However, as expected, they have a downward bias relative to the MLEs.

4.3.2 Iganga Sub-sample

Table 4.7 below shows maximum likelihood estimations of three Tobit models for the Iganga sub-sample. Model 1 (the full model) includes all the treatment variables and the available socio-economic variables for this sub-sample.

Table 4.7: Maximum likelihood estimates of WTP - Iganga

	OLS		Model 1		Model 2		Model 3	
	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error	Coeff.	Std. Error
Age	.0995	1.181	.3219	1.377	.278	1.406	-	-
Gender	48.283[c]	25.557	67.099[b]	29.485	74.245[b]	30.026	66.0094[b]	29.376
Educ	-8.341[b]	4.1803	-10.406[b]	4.847	-9.866[b]	4.941	-10.041[b]	4.762
PGood	6.6099	24.937	10.488	28.792	-	-	-	-
Info	-25.309	24.528	-27.7462	28.359	-	-	-26.761	27.958
Group	-55.417[b]	24.900	-61.769[b]	28.711	-	-	-63.337[b]	28.234
Constant	345.969	58.016	351.293	67.186	307.479	63.039	367.833	41.192
R^2	0.0988		-	-	-	-	-	-
Prob > F	0.0606		-	-	-	-	-	-
Log Likelihood	-		-672.476		-675.242		-672.569	
LR χ^2 (df)	-		13.40(6)		7.86(3)		13.21(4)	
Prob > chi2	-		.0372		0.0489		0.0103	
Pseudo-R^2	-		.0099		0.0058		0.0097	
N	121		121		121		121	

[b] significant at 5%, [c] Significant at 10%

Again, Model 1 is the full model including all hypothesized variables for this sub-sample. In this model three variables are significant at the 5% level, and the overall model is also significant at the same level based on the log likelihood. Model 2 included only socio-economic attributes of respondents, had a log likelihood ratio of 7.86 with 3 degrees of freedom (3 independent variables) and was significant at the 5% level. Model 3 had two socio-economic and treatment variables each and was more significant than either model 1 or 2. Its log likelihood ratio was 13.21 with 4 degrees of freedom.

Gender, Education and the Group treatment variables had a significant effect on bidding behavior of Iganga respondents. Males had higher WTP values than females. The respondents with more formal education were willing to pay less to avoid bad environmental outcomes. Subjects who were involved in Group treatments paid significantly less than those involved in 'self' treatments. Information, good proxy and age were not helpful in predicting WTP. Even after elimination of the Information treatment variable that was found not to effect WTP, the gender, education and group variables continued to exert strong effect on WTP (Model 3).

Like in the urban sub-sample, the signs of the coefficients were consistent across models. The Group, Info and Educ. variables retained the same negative sign, while Gender and Age variables exerted positive effects on WTP across all models. Also, like in the Kampala sub-sample the OLS estimates have the same directional effects on WTP as the ML estimates but are biased downwards, and the variability in WTP explained by the variables (R^2) in the OLS model is low.

4.3.3 Comparison of Kampala and Iganga models

Different variables were available for the Kampala and Iganga data and hence the models are different. From tables 4.6-4.7 several issues are apparent. There is a strong consistency in signs of the significant variables. The **Group** variable is significant for both data sets and carries a strong negative sign in both. The **Gender** variable carries a positive sign (and large magnitude, although not significant in the urban data set). The coefficient on the **Info** variable is also consistently negative. The direction on the **Age** variable is mixed, but is not significant in either sample. In the urban population **Educ.** did not seem to matter, yet in the rural population more education was associated with significantly lower WTP. The full models (Model 1 in both tables) have higher log likelihood values (pseudo R^2), as expected, since more independent variables in a model increase explanatory power. The following section uses these similarities and differences to formally test hypotheses specified in Chapter One.

4.3.4 Tests of Hypotheses and Discussion of Results

One of the advantages of experimental methods is that direct control is embedded in the design. The researcher has the flexibility to design an experiment meant to answer a specific research question and by properly controlling for confounding factors, the researcher retains the variables of interest. In this study, three treatments (**Group**, **Info**, **PGood**) were included in the design to examine their effect on bidding behavior.

In this sub-section the more parsimonious and/or significant models from the preceding sub-section are used to conduct simple hypothesis tests and to discuss these treatment effects. In addition, the effects of two socio-economic variables (**Gender**, **Education**) are examined in detail. The t_{cal} value included in Tables 4.8-4.12 is the calculated student's t statistic obtained as the ratio of the coefficient to its standard error and is compared with the tabulated t-statistic t_{tab} to determine the critical region for rejection of the null hypothesis. All hypothesis tests are conducted at the 5% critical level of significance.

4.2.4.1 Does free-riding in health and environmental provision exist? Are there differences in free-riding behavior between urban and rural populations?

$$H_0: \ \beta_3 = 0 \ \text{vs} \ H_1: \ \beta_3 < 0 \qquad (49)$$

where β_3 is the coefficient on the Group variable. Equation (21) and Tables 4.6-4.7 give the basis for testing whether individuals free-ride or not. Moreover, establishing whether rural groups behave differently from urban groups necessitates testing equation 49 for both data sets. Equation 49 is a one-sided test and rejection of the null would not only imply that the 'Group' variable was significant, but that groups indeed had less WTP on average than individual-decision making subjects.

Table 4.8: Test of Hypothesis One with Model 3

	Kampala	**Iganga**
β_3	-100.7382	-63.33695
Std. Error	32.76161	28.23376
t_{cal}	3.07488	2.24330
Is $t_{cal} > t_{tab}$	yes	Yes
Decision	Reject H_0	Reject H_0

In both data sets, subjects appeared to bid highest when involved in individual decision-making treatments. When subjects knew that the decision would have an effect on themselves individually and also other members in the group, they tended to bid less. This is an indication of free-riding behavior in a pesticide-free environment provision. Thus, even when individuals would presumably have preferred a pesticide-free environment (proxied by the goods presented to them), they (individuals) would rather have another person 'paying' for it.

4.2.4.2 Does providing information matter? How does content information influence bidding behavior?

$$H_0: \ \beta_2 = 0 \ \text{vs} \ H_1: \ \beta_2 \neq 0 \qquad (50)$$

β_2 is the coefficient on the information treatment variable. As mentioned in chapter 3, information is hypothesized to influence bidding behavior. In this case, a brief statement of how pesticides in the air, food and water could affect humans and the environment was provided to individuals in the 'info' treatment. Testing hypothesis in equation (50) using results of Tables 4.6-4.7 show that β_2 was not statistically significant in either of the three models in Iganga or Kampala data sets. Because of the direction on this coefficient, had the variable been significant, the sign would have implied that providing information negatively influences WTP bids.

Table 4.9: Test of Hypothesis Two with Model 1

	Kampala	**Iganga**
β_2	-25.49357	-27.74552
Std. Error	33.59933	28.35918
t_{cal}	0.758752	0.97836
Is $t_{cal} > t_{tab}$	No	No
Decision	Do not reject H_0	Do not reject H_0

Results of hypothesis testing for information provision do not support rejecting the null hypothesis. In these samples, information does not seem to influence bidding behavior.

4.2.4.3 How do gender differences influence bidding for health improvements. Are females more sensitive to a better environment as it relates to their health than males?

$$H_0: \ \beta_4 = 0 \ \text{vs} \ H_0: \ \beta_4 < 0 \qquad (51)$$

Table 4.10: Test of Hypothesis Three with Model 3

	Kampala	**Iganga**
β_4	47.46042	66.00936
Std. Error	31.96843	29.37632
t_{cal}	1.4846	2.2470
Is $t_{cal} > t_{tab}$	No	Yes
Decision	Do not reject H_0	Reject H_0

As mentioned above, the coefficient on the gender variable (β_4) is consistently positive in both samples. Results of the one-sided hypothesis tests are conclusive for the Iganga sample. In this sub-sample willingness to pay values were significantly higher for males than females. On average male subjects bid Shs. 69 higher than females in Iganga. The Kampala gender test is marginally inconclusive.

4.2.4.4 What proxy environmental goods solicit the highest bids? Does it matter what good is used as the environmental proxy? Are people more sensitive to consuming safe water or safe food?

$$H_0: \ \beta_1 = 0 \ \text{vs} \ H_1: \ \beta_1 \neq 0 \tag{51}$$

where β_1 is the coefficient on the proxy good variable.

Table 4.11: Test of Hypothesis Four with Model 1

	Kampala	**Iganga**
β_1	-69.82128	10.48768
Std. Error	36.7574	28.79176
t_{cal}	1.8995	0.3643
Is $t_{cal} > t_{tab}$	no	No
Decision	Do not reject H_0	Do not reject H_0

Hypothesis (51) is a two-sided test. In Kampala, subjects were more likely to bid higher values when the treatment was water. Subjects bid Shs. 69.82 more to avoid consuming potentially contaminated water than consume potentially contaminated groundnuts. However the variances were high making the variable only marginally significant. At the 5% level, the null hypothesis is accepted for both data sets. The good does not matter. As expected, as long as an item is to be consumed, what matters is not the state it is in - whether liquid (water) or solid (groundnut) but rather its overall safety is what matters.[35]

[35] The more liberal one-sided test at the 5% level of significance (equivalent to the 2-sided test at the 10% level) shows that urban subjects significantly revealed a higher preference for safe drinking water compared to safe groundnuts.

4.2.4.5 Does formal education matter? How differently do educated individuals behave when bidding to preserve their environment? Do educated individuals bid higher than their less formally educated counterparts?

$$H_0: \ \beta_5 = 0 \ \text{vs} \ H_1: \ \beta_5 > 0 \tag{52}$$

Accepting the null hypothesis in (52) would imply that education (coefficient β_5) does not matter – that educated and less educated individuals behave the same with regard to bidding for the environment.

Table 4.12: Test of Hypothesis Five with Model 1

	Kampala	**Iganga**
β_5	1.144657	-10.04139
Std. Error	7.060154	4.762479
t_{cal}	0.1621	2.1084
Is $t_{cal} > t_{tab}$	no	Yes
Decision	Do not reject H_0	Reject H_0

For the Kampala data, the null hypothesis can not be rejected. Kampala's educated and un-educated behave the same with regards to bidding behavior. However the rural sub-sample is different. Education seemed to move against WTP. The more formal education one had, the less they were willing to bid to avoid bad environmental situations.

4.2.4.6 Do urban and rural populations differ in their valuation of health and the environment?

$$H_0: MWTP_{Iganga} = MWTP_{Kampala} \ \text{vs} \ H_1: MWTP_{Iganga} \neq MWTP_{Kampala} \tag{53}$$

where MWTP is mean willingness to pay. Hypothesis six is a comparison of means from two independent samples with different sample sizes. Table 4.13 below presents the necessary statistics required to test the hypothesis in equation (53).

Table 4.13: Mean and Standard Deviation of Willingness to Pay

	WTP	
	Iganga	Kampala
Number of Subjects	121	156
Mean	286.86	343.968
Standard Deviation	135.87	142.87

The t-statistic is obtained as the ratio of the difference in sample means to the standard error of the difference in sample means:

$$t = \frac{Difference\ in\ sample\ mean}{SE\ of\ \ difference\ in\ sample\ mean} \tag{54}$$

The t-value of difference is 57.108/16.81 = 3.397 with a corresponding P value of 0.0008, suggesting that it is extremely unlikely that a difference in willingness to pay of this magnitude would be observed just by chance. Urban subjects had higher mean willingness to pay to avoid ill health outcomes than rural subjects.

4.2.4.7 Does previous exposure to the harmful effects of pesticides influence the way subjects bid to avoid ill outcomes?

The variable PExp was included in the model as a proxy to measure people's risk aversion based on past experience with pesticide poisoning. For the urban sub-sample where this information was obtained, the variable was positively correlated with WTP. Higher WTP values were obtained from subjects who had had a previous exposure to pesticide poisoning possibly as a means for averting further exposure and its effects.

4.3.5 Conclusions of Hypothesis Testing and of Experimental Auction Results

The preceding tests were conducted for both rural and urban sub-samples in order to establish differences, if any, in bidding behavior across locations. General conclusions are made below grouped according to whether results are: strong in both sub-samples, mixed or unexpected.

a) Strong in both sub-samples (rural and urban populations do not differ)

In the 'group' experimental treatment, each individual acted individually, privately and voluntarily, which allowed me to test their contribution towards a pesticide-free product. Results from the hypothesis test show that free-riding in health and environmental provision does exist. Free-ridding was more pronounced in the urban population where there is a Shs. 100 differential between individual decision-making subjects and group decision-making subjects. In the rural population, this differential is Shs.63. The later constitutes 12.6%, and the former, 20% of subjects' endowment.

Also for both samples, it did not matter what good was presented as a health and environmental proxy. Both are apparently considered the same with regard to their safety. Results do not show significant differences in bid values between groundnut and water treatments.

b) Mixed: Rural and urban populations differ.

It is apparent that populations utilize education differently with regard to bidding for health. Formal education had diverging effects on urban and rural sub-samples. While formal education does not seem to influence bidding behavior in the urban population, it had a strong negative effect on bid values in the rural population, all things equal. This is a rather surprising finding as the a priori expectation was to see a positive influence of education on health and environmental improvements.

Gender differences do not seem to matter in urban populations but do in rural populations. Rural males significantly bid higher for health improvements than their female counterparts. This pattern does not exist in the urban population (at the 5% level). In a public goods game in rural Uganda using married couples, a similar trend was observed by Iversen et al., 2006 where women were less likely to contribute to a common (household) account in the presence of their spouses. These findings run counter to the generally believed notion that males tend to be more individually oriented and unlikely to value environmental improvements (public goods) as much as females (Eckel and Grossman, 1996).

c) Unexpected results in both samples

Information did not seem to significantly influence bidding behavior in either rural or urban populations. People with information about pesticides in the environment and their possible effect on health, did not bid significantly differently from those without. Moreover, the relatively large coefficients (though not significant) are of concern. In both sub-samples the effect of information on WTP moves against theoretical prediction. Furnishing individuals with bias-free information about impact of pesticides should increase awareness and guide people in formulating perceptions about chemical effect on humans and the environment and thereby raise WTP values. The results here indicate otherwise.

The above tests form the basis for formulation of policy implications for the two sub-samples. Explaining differences and similarities in the two samples is necessary. However, considering that the two samples were different in terms of sampling procedure, policy recommendations for the more representative (Iganga) sub-sample may be more reliable. As always there are tradeoffs to be made when a more stringent level of statistical significance is used. Especially for policy formulation and recommendation, a less restrictive level may be more important.

The results of experimental auctions presented in section 4.3 are interpreted in the context of human subjects' willingness to avoid ill health outcomes in varying situations/treatments. What is not considered in the foregoing discussion is subjects' behavior being innately affected by factors other than their willingness to avoid contamination. If subjects interpreted the games differently, or had other reasons for their bidding behavior (such as mere fascination with the game/exercise, or did not understand their task), one would only hope that those reasons were captured in the random part of the model specification. Otherwise, these results could be interpreted differently.

4.4 Results of Choice Experimentation

In this section, results of both conditional logit and multiple logit models are presented. The goal is to determine the significance of the estimated models using goodness-of-fit characteristics of the models, and, using maximum likelihood estimates, determine the direction and magnitude of the utility parameters in the systematic component of utility.

4.4.1 Coefficient Estimates of the Conditional Logit Model

Empirical estimation results of equation (47) (repeated here as equation 55) are presented in Table 4.14.

$$V_i = \alpha_1 Drink_i + \alpha_2 Agric_i + \alpha_3 Cost_i + \nu_i \qquad (55)$$

Model 1 contains all option-specific estimation variables with the qualitative variables measured on a 3-level scale as explained in section 3.3.3. Two categories of each qualitative variable were created, the omitted category becoming the comparison category. Thus the empirical model becomes:

$$V_i = \alpha_{DN} DN_i + \alpha_{DS} DS_i + \alpha_{AN} AN_i + \alpha_{AS} AS_i + \alpha_{COST} COST_i \qquad (56)$$

See Table 4.14 for a definition of the variables. The comparison/omitted/referent category in equation (56) is the 'Always safe /very safe' ***(DA, AA)*** high quality water category. In models 2 and 3, levels of the qualitative variables are combined creating 'either-or' choice situations. For example in model 2, pesticide-contaminated water that was considered 'never safe' for either drinking or agricultural use is compared against that which was 'not very safe or very safe'. The omitted category is the 'not very safe or very safe water' category. In model 3, water that was 'very safe' is compared against that which was 'not very safe or never safe'. In all specifications the quantitative variable is 'cost'.

Table 4.14: Parameter estimates for the water safety choice experiment: Conditional Logit

Variables	**Model 1** Coef. (Std. Err.)	**Model 2** Coef. (Std. Err.)	**Model 3** Coef. (Std. Err.)
Drinking			
Never safe ***(DN)***	-.0246	-.4633[a]	-
	(.2516)	(.1317)	
Sometimes safe/Not very safe ***(DS)***	.0511	-	-
	(.1849)		
Always safe/Very safe ***(DA)***	-	-	.1702
			(.1675)
Agricultural			
Never safe ***(AN)***	.9784[a]	.3302[b]	-
	(.2833)	(.1468)	
Sometimes safe/Not very safe ***(AS)***	.5292[a]	-	-
	(.1786)		
Always safe/Very safe ***(AA)***	-	-	-.4451[a]
			(.1359)
Cost ***(COST)***	.0017[a]	.0005[b]	.0011[a]
	(.0005)	(.0002)	(.0003)
Log likelihood	-707.390	-712.479	-711.504
LR χ^2 (df)	35.28(5)	25.11(3)	27.06(3)
Prob>chi2	0.0001	.0000	.0000
Pseudo-R^2	.0243	.0173	.0187
# of observations	2092	2092	2092
# of iterations	4	3	3

[a] significant at 1%, [b] significant at 5%

All three models were significant at the 1% level of significance, and the individual variables in each model are significant at either the 1% or 5% level (except in model 1 and 3 where the drinking water qualitative variables are insignificant). In addition, the directional effect of the variables on choice is consistent across all 3 models.

Some of these results seem to contradict theory. The positive (and highly significant) coefficient on the cost variable tells us that higher costs increase the likelihood of a safe water sample being preferred. This result implies an upward sloping demand curve which is counterintuitive (however the magnitude of this coefficient is low). In models 1 and 2, the positive coefficients on water samples that are never safe for agricultural use is troubling – people seem to prefer unsafe agricultural water to safe samples. The negative sign on the unsafe drinking water sample is intuitive – indicating people's preference for water that is either sometimes safe or always safe compared to that which is never safe for drinking. Model 3 seems the least useful in explaining choice behavior.

4.4.2 Welfare Measure of Reduced Pesticides from the Conditional Logit Model

Estimation results from the preceding section are perhaps most useful if used to estimate the welfare effects to guide policy formulation. The results can be used to estimate changes in the quality of environmental conditions or welfare improvements that might accrue when pesticides are removed from the environment.

Utility parameters in Table 4.14 are reproduced in Table 4.15. The qualitative attributes are compared with the cost attribute to obtain implicit prices of the variables and provide a basis for ranking the variables in order of importance from the individuals' perspective (see 3.3.2). Welfare measures using the conditional logit results are calculated and presented in Table 4.15.

Model 1 results show that people value agricultural water (low quality and medium quality), followed by not-very-safe (medium quality) drinking water. Unsafe (low safety level) drinking water is the least preferred. While logic dictates that water that is unsafe for consumption be valued least, it is surprising that people would place much value on water that is sometimes safe for agricultural uses compared to that required for drinking. Results of model 2 are more intuitive. It can be seen that the coefficient with the highest magnitude is low quality (never-safe) drinking water. The (un)safety of drinking water decreases people's utility more than either increased costs or increased water safety for agricultural use. This essentially means that people are more likely to discount unsafe drinking water than safe agricultural water. In model 2 the positive MWTP value indicates that subjects were willing to pay over Shs. 900 to move (upgrade) from low quality drinking water to medium-to-high quality drinking water. Likewise the negative sign of 660.4 indicates the people were willing to pay about Shs. 660 to switch from medium or high quality to low quality (unsafe) agricultural water.

The general conclusion that can be made across all models (in Tables 4.13 and 4.14) is that people preferred unsafe agricultural water, but also preferred safe drinking water and at higher costs.

Table 4.15: Marginal Willingness to Pay and Implied Rankings

Water Use/ Category	Safety/Quality Level	Coeff.	Value	Implied ranking	Implicit prices/ MWTP (Shs)†	
Drinking	Sometimes/Medium	α_{DS}	.0511	3	-30.06	Model 1
Agricultural	Sometimes/Medium	α_{AS}	.5292	2	-311.29	
Drinking	Never/Low	α_{DN}	-.0246	4	14.47	
Agricultural	Never/Low	α_{AN}	.9784	1	-575.53	
Cost		α_{COST}	.0017			
Drinking	Never/Low	α_{DN}	-.4633	1	926.6	Model 2
Agricultural	Never/Low	α_{AN}	.3302	2	-660.4	
Cost		α_{COST}	.0005			

† calculated as $\alpha_k / (-\alpha_{\cos t})$, MWTP = Marginal willingness to pay

4.4.3 Multiple Binary Logit Models

This sub-section reports results from estimations of multiple binary logits. Each choice occasion is treated as a time period. Understanding the differences in water attributes (safety levels/water quality levels and cost) is key to understanding the trade-offs people made between the two water samples at each choice occasion. With unlabeled experiments, such as this, analyzing the decisions of respondents in these time periods requires considering each choice decision as a subset of a multiple binary choice decision making setting. Model (22) is repeated here for ease of reference. The operational model is equation (58)

$$U_{itq} > U_{jtq} \qquad t = 1\ldots\ldots 8, \qquad C_{tq} = i, j = (A, B) \tag{57}$$

$$V_i = \beta_1 Group_{iq} + \beta_2 Educ_{iq} + \beta_3 Gender_{iq} + \beta_4 Age_{iq} + \beta_5 Salary_{iq} + \beta_6 PExp_{iq} + \beta_7 illever_{iq} \tag{58}$$

4.4.3.1 Description of Choice Occasions (t = 1, …, 8)

Each respondent made a choice decision at each choice occasion. In choice occasion 1, water samples A and B were differentiated only in terms of the agricultural water quality and cost. Subject's choice of option A over option B was an indication of their preference for free, low quality (unsafe) agricultural water over not-very safe (medium quality) water at a cost of Shs. 300.

In choice occasion 2 (and 3), choice of option A is a preference for unsafe agricultural water at a cost of Shs.100 (and medium safety/medium quality, free agricultural water) over safe agricultural water at a cost of Shs. 500. Choice of option A in choice occasion 4 (and 7) is a preference for unsafe water samples at Shs. 200 over unsafe drinking water at Shs. 300 (and not very safe drinking water at Shs. 500). In choice occasion 5 (and 6) choice of option A is a preference between safe water samples at Shs. 300 (Shs. 500) over not very safe drinking water (never safe drinking water). In choice occasion 8 choice of option A implied preference for safe agricultural water (at a medium cost of Shs. 200) compared to free medium quality (medium safety) agricultural water.

4.4.3.2 Coefficient Estimates

The same approach is employed to obtain the multiple logit estimates as was employed in the conditional logit model. Stepwise elimination of insignificant variables is performed. That is, model 1 in Tables 4.16 and 4.17 below contains all individual-specific variables. After elimination of highly insignificant variables from Model 1, Model 2 obtains.

Table 4.16: Parameter estimates for water safety choice experiment: Multiple Binary Logit (Choice occasion 1-4)*

	Choice Occasion 1		Choice Occasion 2		Choice Occasion 3		Choice Occasion 4	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Variables	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)
Group	-.6612[d]	-.6992[c]	.0725		-.1189		-.2699	
	(.4153)	.3864	(.4142)		(.4105)		(.4114)	
Educ	-.0823	-.1316[d]	.0242		-.0340		-.221[b]	-.2336[b]
	(.0859)	.0829	(.0856)		(.0855)		(.1003)	(.0951)
Gender	.4008	.4193	-.9324[b]	-.8849[b]	.3531	.3396	-.5725[d]	-.2619
	(.4111)	.3889	(.4244)	(.4107)	(.4139)	.(3962)	(.4189)	(.3707)
Age	-.0077		.0147		-.0048		-.0111	
	(.0248)		(.0249)		(.0245)		(.0252)	
Salary	-.3254		-.7644[c]	-.656[d]	-.2745	-.3726	-.495	
	(.4141)		(.4273)	(.4069)	(.4185)	(.4041)	(.4189)	
PExp	-1.958[a]	-1.796[a]	-.7348	-.813[d]	-.1565		.1506	
	(.72498)	.6517	(.6178)	(.6020)	(.5978)		(.6092)	
Illever	-.0068		-.4595		-.0208		.2277	
	(.4058)		(.4113)		(.4068)		(.4090)	
Constant	2.102[d]	2.547[c]	.3234	1.051[a]	.2657	-.5348[d]	4.753[b]	4.1019[a]
	(1.628)	1.448	(1.575)	(.3760)	(1.607)	(.3561)	(1.902)	(1.58)

[a] significant at 1%, [b] significant at 5%, [c] significant at 10%, [d] significant at 20%

*Note: See Table 4.1 for a definition of variables

Table 4.17: Parameter estimates for water safety choice experiment: Multiple Binary Logit (Choice occasion 5-8)*

	Choice Occasion 5		Choice Occasion 6		Choice Occasion 7		Choice Occasion 8	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Variables	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)	Coeff. (Std. Err.)
Group	-.4495		-.117		.2367		-.2824	
	(.4147)		(.445)		(.4123)		(.4127)	
Educ	-.1693^{b}	-.1288^{d}	.1058		.0075		-.0756	-.0764
	(.0880)	(.0798)	(.1004)		(.0859)		(.0863)	.0810
Gender	.4027		-.1812		.3432		-.6085^{d}	-.4585
	(.4159)		(.4585)		(.4103)		(.4219)	.3791
Age	.0231		.0269		-.0346^{d}		-.0039	
	(.0248)		(.0272)		(.0246)		(.0248)	
Salary	-.1369		.3646	.5757^{d}	-.1523		.0654	
	(.4184)		(.4641)	(.4407)	(.417)		(.4238)	
PExp	-.4587	-.2677	-.3084		-.6252	-.5337	.0392	
	(.6042)	(.5466)	(.6226)		(.6033)	.534	(.6074)	
Illever	.839^{b}	.7261^{c}	.6010^{d}	.6088^{d}	-.3881	-.5969^{d}	-.8427^{b}	-.9159^{b}
	(.4135)	(.3769)	(.4493)	(.4235)	(.4062)	.3635	(.4136)	.3865
Constant	1.932	2.034^{d}	-1.980	.4004^{d}	1.092	.4498^{c}	1.868	1.563
	(1.605)	(1.323)	(1.925)	(.2916)	(1.6008)	.2472	(1.657)	1.3749

^{b} significant at 5%, ^{c} significant at 10%, ^{d} significant at 20%,

*Note: See Table 4.1 for a definition of variables

Table 4.18: Comparison of Fit for Model 2 (Parsimonious) Binary Logit Models

Choice Occasion	No. of Observations	No. of Parameters	Log likelihood at Convergence (LL $\hat{\beta}$)	No. of Iterations	Likelihood Ratio Index	LR χ^2 (df)	Prob > chi^2
1	127	5	-81.586	4	0.0732	12.88(4)	0.0119
2	119	4	-77.271	4	0.0574	9.41(3)	0.0243
3	118	3	-77.421	4	0.0130	2.03 (2)	0.4433
4	126	3	-83.338	4	0.0414	7.20(2)	0.0273
5	126	4	-83.6941	4	0.0354	6.14(3)	0.105
6	118	3	-69.518	4	0.0310	4.44(2)	0.109
7	128	3	-86.549	3	0.0217	3.85(2)	0.146
8	125	4	-81.229	4	0.0465	7.93(3)	0.0475

It is important to note the general consistency in signs of the coefficients in the full and restricted models (models 1 and 2) of each choice occasion. In addition the significant variables in the full models retain their effect in the restricted models. A general comment can be made that the significance of individual variables in the choice data is low. Models for choice occasion 1, 2, 4 and 8 have individual significant variables at the 5% level. Some goodness-of-fit measures for the parsimonious models (model 2) are presented in Table 4.18. The variables in models for choice occasion 3, 5-7 models are jointly not significant at the 5%level. The pseudo R^2 statistic of the significant models ranges from 0.0414 to 0.0732.

The dependent variable in all these models is the respondent's binary choice of a water sample of given attributes (explained in the preceding section). The following discussion pertains to the more parsimonious model in each choice occasion. In choice occasion 1 the variable **PExp** had an effect on preference of a water sample. This result is consistent with risk averse behavior. Respondents who had been previously exposed to pesticide poisoning were more likely to prefer a safer water sample (at a cost) in option B than a free but unsafe water sample in option A. The other individual-specific variables were not important predictors of choice of a water sample.

In choice occasion 2 the gender variable was significant and negative indicating that males were more likely to prefer a more expensive water sample with high agricultural quality than an unsafe sample at Shs.100. This result is consistent with the experimental auction results which depicted males as having higher willingness to pay for better health than females in general. In choice occasion 4 only the education (**Educ**) variable was significant at the 5% level. The more education one had, the less likely they preferred option A (with medium safety levels for both agricultural and drinking water, at a cost of Shs 200) over option B (safe agricultural water at a cost of Shs. 500). The negative coefficient on the '**illever**' variable in choice occasion 8 suggests that respondents who had had an episode of illness in the past year were more inclined to prefer option B (safe agricultural water at a cost of 200) than option A (free medium quality agricultural water). This result runs counter to expectations. Experience with past illnesses is expected to induce risk averse behavior such that subjects would want to avoid further exposure by preferring the safer water sample, even for agricultural use.

4.4.4 Conclusions from Choice Experimental Models

It is apparent that the conditional and logit models were not properly suited to this data, perhaps due to specification bias. Goodness of fit measures indicated that not much variation in the observed behavior was predicted by the attributes in the models. All models converged after a few iterations – an indication of little improvement between the model with, and one without the predictors.[36] Although interpretation of the pseudo R^2 statistic in maximum likelihood is different from that in OLS, the goodness-of fit measure for these three models seems rather low. Logit models with extremely good fit have pseudo R^2 in the 0.2-0.4 range. The range in these models was 0.017 – 0.024.

The table below is a summary of conditional logit results (Models 1-3 in Table 4.14) and provides a check on the validity for these results. The second column shows the a priori expected results, while column three gives model results. The last column is the validity check.

Table 4.19: Validity Check for Choice Data Results

Water Use /Category	Expected	Observed	Validity check	
Drinking				
Never safe/Sometimes safe	$\lvert\alpha_{DN}\rvert > \lvert\alpha_{DS}\rvert$	$\lvert\alpha_{DN}\rvert < \lvert\alpha_{DS}\rvert$	No	(1)
Never safe	$\alpha_{DN} < 0$	$\alpha_{DN} < 0$	Yes	(2)
Sometimes safe/Not very safe	$\alpha_{DS} < 0$	$\alpha_{DS} > 0$	No	(3)
Always safe/Very safe	$\alpha_{DA} > 0$	$\alpha_{DA} > 0$	Yes	(4)
Agricultural				
Never safe	$\alpha_{AN} < 0$	$\alpha_{AN} > 0$	No	(5)
Sometimes safe	$\alpha_{AS} < 0$	$\alpha_{AA} > 0$	No	(6)
Cost	$\alpha_{COST} < 0$	$\alpha_{COST} > 0$	No	(7)

36 Maximum likelihood, an iterative process, starts out with an approximated starting value of what the logit coefficients should be and determines the direction and size of the change in logit coefficients which will increase the log likelihood, and the process continues until there are very small improvements in the log likelihood.

Internal validity for these results is low. Many of the results reported in Table 4.19 seem to contradict theory. Six out of eight validity checks (as given by magnitude and sign) of coefficients are theoretically incorrect. While it is expected that high quality water (water at high safety levels) be preferred, that is, that the more ‘safe’ water is said to be, the more likely people will prefer it, some of the results in the above table show the opposite. A comparison of water samples showed that subjects were more likely to prefer the ‘unsafe’ samples for agriculture.

The only plausible explanation for the inconsistency in the results for ‘agricultural water qualities’ is the urban nature of the sample. If one is willing to make the statement that urban subjects have less preference for agricultural water and are hence more likely to discount it compared to drinking water, then results of validity check (5 and 6) in Table 4.19 can be intuitive. A comparison between results from choice experiments and results from experimental auctions (equation 51) shows that urban subjects revealed higher preference for safe drinking water (at the 10% level) compared to Iganga’s preference for safe groundnuts. (See also footnote 21).

The other inconsistencies in the Table 4.19 (results 1, 3 and 7) are of concern. Result 7 suggests an upward sloped demand curve, while result 3 suggests that people would prefer medium-quality drinking water to high-quality drinking water. In Result 1, the absolute difference in the coefficients suggests that subjects discounted medium quality drinking water more than low quality drinking water. However, since this is what the choice data is telling us, we can probably try to understand the mechanism that resulted in these results. Based on this background, several explanations are possible and can be categorized as follows:

a) Possible problematic issues prior to the design stage (Theory)

The most likely possible reason, that subjects considered the hypothetical nature of the choice experiment, should have been averted by the sequence of the experiments. As was mentioned previously, experimental auctions preceded each choice experimental session for all groups. The auction sessions were incentive compatible and with this reasoning (even when unbeknownst to the researcher), could have carried over to the choice experiments as subjects performed the subsequent choice tasks. With that reasoning, situational involvement would be maintained. If subjects did not carry the auction session effects over to the choice experiments, then the level of involvement would be low and subjects could intentionally act irrational in their choices considering the incentive incompatibility of the choice experiments.

A possible source is simply, irrationality. Could it be that the irrationality that was observed during pre-testing (where subjects chose obviously dominated alternatives) also transferred to the urban subjects in another form during the actual experiment (where subjects chose obviously irrational alternatives)? If individuals do not act in their self-interest, then values from all choice experiments or indeed all models that employ the random utility framework become flawed.

However these two potential sources of inconsistencies (of subject irrationality and hypothetical survey nature) can not be restricted only to non-market valuation. In market valuation, subjects are also assumed to act in their best interest when choosing one market good over the other. To behave otherwise invalidates utility measure based on these assumptions.

The functional form of the systematic component of utility in equation (24) is generally assumed to be linear in parameters, additive in attributes. This assumption was not tested against the data. Perhaps a non-linear relationship pertains with the attributes in this data and the observable choice, necessitating a more complex non-linear functional form.

b) Possible problematic issues at the design stage

At the design stage, the range of cost values that were suggested as the payment vehicle is the most probable explanation for the inconsistencies. It is probable that the [0,500] range was restrictive in which case subjects' true valuation at each choice occasion was not properly represented. This was indicated by the skeweness of the WTP values for Kampala, but not Iganga. When the value of the payment vehicle or incentives in an experiment is considered trivial, it manifests as a lot of noise (Friedman and Cassar, 2004). It is important to note that the range of cost values was arrived at after discussion with the pre-test group which comprised only Iganga subjects. However, the rural subgroup did not evaluate the choice set (except during the pretest). While it was assumed that Iganga values would translate well to the Kampala subjects, these results suggest they did not. A separate pilot study is required for each separate location (Louviere et al., 2000). It seems likely that Shs. 500 means a lot more to someone in Iganga than in Kampala.

In addition, the selection of a limited number of attributes and attribute levels may have over simplified the experiment to the extent that it reduced the power of the experiment to predict choice behavior. In my view the dividing line between a simple and a complex experimental design is thin (see also footnote 30).

When individuals are presented with alternatives that are not satisfactory to them, they have an option to express it – by choosing none. The eight two-option choice sets that were presented to subjects might have been inadequate because the 'none' option was unavailable (non exhaustive choice sets). This omission could have triggered 'protest' reasoning in subjects such that they tended to behave irrationally given that the 'freedom' to choose the fall back option was unavailable. By omitting the 'none-of-these' option, subjects might have been put in a position of "forced choice" - choosing an option in a situation where they would not have chosen one (Holmes and Adamowicz, 2003).

Another possibility is that there was not sufficient difference between alternatives presented to subjects (even when the statistical design quality was relatively high at 83.1%) such that, at the individual level, substitutability of alternatives was stochastic. When respondents are faced with alternatives that seem similar to them, they may become indecisive and choose one over the other not based on any systematic evaluation of the attributes of the alternatives, but based purely on chance. This choice behavior negates the use of the multinomial logit framework which is based on the assumption of deterministic choice behavior from the individual's perspective, and only stochastic from the researcher's perspective.

The assumption on which the logit framework is based – that the unobserved component of utility (the errors) are distributed iid extreme value was not tested. Perhaps the distribution was not iid, in which case models such as the Heteroskedastic extreme value, random parameters) were appropriate. If the error distribution in the data is different from the error distribution underlying the model used to analyze the data, this constitutes specification bias. In future work with this data, formal procedures are required to test the assumptions.

c) Problematic issues at the implementation stage

Perhaps the results could be blamed on complexity or that subjects were unfamiliar with the valuation exercise that they were being asked to perform. The post experiment discussion with subjects revealed that none had ever participated in a valuation exercise akin to this. The unfamiliarity with the procedure could have had an influence on the observed results. Or perhaps respondents were fatigued, considering that all choice experiments were conducted after the experimental auctions. Fatigue has been known to affect the cognitive ability of individuals in surveys resulting in inconsistent observed behavior (DeShazo and Fermo, 2002).

Also, there might be tradeoffs to be made when selecting the subject pool. Perhaps the subject pool may have had an influence. Respondents for the study comprised adult professionals (teachers, traders, rotarians, business people, researchers, bankers, retail shoppers etc), and as Huck (2007 pp. 23) notes, professionals may “confuse abstract situations with reality” hence providing inconsistent results. Some authors note that student subjects often readily comply with the experimenter (termed scientific-do-gooders) and may direct results towards the experimenter’s expected outcome. This perhaps explains why most lab experimental auctions have, as their subject pool, student respondents.

5.0 Recommendations and Conclusions

5.1 Introduction

This chapter summarizes the study and gives policy implications and recommendations for future research. Section 5.2 provides the summary of objectives of the research and the motivation for this study. Sections 5.3-5.4 summarize methods and results while sections 5.5-5.6 presents limitations/ challenges and implications for further research, and concludes the dissertation.

5.2 Summary of Research Objectives

Chemical use in agriculture is still prevalent in many agricultural systems despite evidence of both health and environmental risk associated with pesticide exposure. The risk posed by pesticides has generated much concern leading to organic farming and integrated pest management practices increasingly being promoted.

Valuing the contribution that reduced pesticide use makes to health and environment improvement is a complex task and explains why this subject has limited research. The complexity of the situation comes about because both health and the environment are not resources or services that can be traded on the market and as such valuing their improvements requires non-market valuation procedures. In addition the large number of potential factors to consider in a health and environmental study is daunting and most studies are reduced to considering only sub-components of the subject.

Most previous methods that value improvements in the environment due to reduced pesticide use have stated the benefits in qualitative terms. Expressing benefits in monetary terms is a convenient means of expressing the relative values that society places on environmental improvement. Monetary expressions of value provide a generally acceptable method of comparison of programs and acts as a basis for policy formulation. Quantitative measures such as contingent valuation, environmental impact quotient (EIQ) and cost of illness (COI) have been used in the past. However most of these measures have been found inadequate especially for developing countries. For example the cost of illness measure is invalidated in places where many illnesses go unreported. The EIQ measure assumes that formulations of pesticides/chemicals are known and fixed. In developing countries where adulteration and crude chemical concoctions (especially at the time of application) are common, this measure is not viable.

The most promising methods used in valuing benefits or improvements in health and environment involve experimental procedures. Experimental methods in economics allow researchers to observe economic behavior of agents placed in a controlled setting for the purpose of testing a hypothesis, or validating a theory. Proper design of the experimental 'environment' is crucial. The focus of this research was to employ these experimental methods to establish benefits that accrue from pesticide-use reduction. In particular, the methods were used to establish tradeoffs and compromises people make when:

i) They choose one environmental attribute over another;

ii) They choose to avoid ill-health outcomes due to pesticides

In developed countries there is a heightened awareness of the potential benefits from a clean environment. The situation in the developing world may be no different and it was of interest to know how and to what extent the developing world valued their environment.

Chapter 2 provided a review of literature regarding the subject of pesticide use; types and effects; and methods of valuation of goods including health and the environment. Finally methods of eliciting value information were discussed.

5.3 Summary of Methods

This study used primary data collected through two experimental methods. The first – experimental auction, an incentive compatible procedure was designed to obtain peoples' preferences for good health expressed as willingness to pay by revealed preference methods. Agents bid to avoid bad health and environmental outcomes in a situation/environment that mimics real-world scenarios where subjects 'live' with the outcome of their actions. The link between the decision makers' behavior and the outcome was maintained by having subjects witness the outcome (consequences) of their valuation. The other method – choice experimentation/choice modeling – involved designing choice sets of environmental attributes and having agents make repeated tradeoffs on these attributes. The method is based on two assumptions: (i) that the value of an environmental good is composed of characteristics of the good; (ii) that subjects are good judges of how well off they are in any given situation, and (iii) that they can make rational choices, choosing alternatives they prefer over alternatives they forego. Generally it is assumed that human self interest and rational choice direct people's actions based on how much satisfaction their choices bring compared to alternative choices.

Respondents for these experiments, unlike in most experimental auction studies, comprised adult non-student subjects: (i) drawn randomly from a high groundnut-producing district, and (ii) from a self-selected sample in a region of high groundnut consumption. The first sub-sample, the farmer group, was hypothesized to have interests in the use of pesticides (to control pests and boost production), while the urban sub-sample was hypothesized to have health concerns with pesticides in the environment. The two sub-samples were expected to value health and the environment differently.

In both experimental procedures, the method of ordinary least squares was deemed inappropriate and analysis required adoption of maximum likelihood estimation. In the experimental auctions the range of the dependent variable WTP had a mixture of discrete and continuous properties. In the choice experiments the dependent variable (choice of an alternative in a choice set) was limited to the {0,1} binary range. In both cases the dependent variable was not continuous or unbounded and OLS would yield biased estimates.

5.4 Summary of Results

Attainment of wants is constrained by many factors, including budgetary constraints. For the poor, this budgetary constraint is even more pronounced, limiting the choices people can make to satisfy these wants. What happens in a case when people are endowed, and then given the opportunity to make the same choices? How do people utilize the 'extra' income when making decisions to avoid health deterioration due to potential pesticide contamination? Results in this study showed a wide range of outcomes: They varied by location and by treatment.

Results were compared across the rural-urban divide. Rural and urban populations differed in their bidding behavior towards improvements in health and the environment, hence validating the need to treat the two populations as different entities, at least in some aspects. However, strongly across both locations was their tendency to 'prefer' external intervention with provision of health and environmental good. That is, in these experimental auctions, each individual acted individually, privately and voluntarily, allowing me to test their health and environmental good contributions behavior. Results indicated that the free riding phenomenon in environmental good provision was present, thus participation in a good cause is limited. This implies that any environmental-improvement programs may require more than relying on collective effort from individuals. Left on their own, self interested agents do not become involved in ventures that they perceive can be provided for by others in the community. This phenomenon was truer in the urban than rural setting.

Each subject in these experimental auctions had one go at the experiment and made decisions individually and anonymously (that is, inexperienced subjects with incomplete information). This anonymous setting absolves any perceptions of scrutiny of the agent by the experimenter, thereby approximating the natural environment in which subjects normally make their behavioral decisions. Such a procedure's results can be generalized from the sample to the general population, hence achieving parallelism.

Section 4.3 presented other determinants of willingness to pay for improved health (avoidance of pesticide contaminated food and water). Key variables that influenced bids were information, education, gender differences, and occupation. Both information and education attainment depressed willingness to pay values while males and salaried respondents in general had higher bids.

Results of choice experiments were presented in section 4.4. Urban subjects value high quality/safe drinking water. The higher the cost of a water sample the more likely that people preferred it to the less costly alternative. Perhaps subjects thought that more expensive water samples are safer. Some inconsistencies were noted from the choice behavioral data. Urban subjects seemed to obtain high disutility from high quality (very safe) agricultural water. In addition, these subjects discounted medium quality drinking water much more than they discounted low quality (unsafe) drinking water. These results were un-anticipated and represented either illogical choice behavior of respondents or flaws in the empirical model.

The use of choice experiments in valuing environmental improvements seems to be limited because of its complexity in terms of selecting appropriate environmental attributes for valuation. Other stated preference methods are known to be equally limited by a number of problems, the most challenging of which is the validity or reliability of results from these methods. When respondents are asked hypothetical questions, some bias becomes inevitable and makes it hard to draw inferences from the data. The un-anticipated results from the choice experiments question the minimum number of observations (and other factors mentioned in section 4.4.4) that are required for meaningful inferences to be drawn from choice behavior.

5.5 Limitations and Implications for Further Research

One of the factors limiting the use of experimental methods in valuing both health and environmental benefits is the assumption that individuals have an understanding of the contributions that these environmental amenities/categories make to their well-being. Individuals are assumed to be the best judges of their own well-being and know how each component in the system they are valuing affects their well-being. When this assumption fails then valuations using these methods become flawed. People are also presumed to have well-defined preferences over non-market goods and services. In the case of market goods and services, these preferences are well-defined and are expressed in terms of demand for the goods and services. If the same does not hold true for non-market goods, then valuation of non-market goods using the assumption about well-defined preferences becomes invalid. In this study it is assumed that subjects' bid values are a direct translation of their willingness to avoid contamination. If this assumption is violated (that is, that something else was going on in the subjects minds), then the results would have to be interpreted in light of the assumption violation. A test of transitivity of subject's preferences is necessary. If subjects' preferences are intransitive with market goods, then they may be expected to be intransitive with non-market goods too.

As was mentioned before, the concept of compensating surplus on which these valuation methods is based is constrained by individuals' incomes. Consequently individuals' choices amongst alternatives are constrained. If the distribution of income is skewed then welfare estimates based on WTP would be biased. One way to circumvent this problem in valuation studies is to endow subjects with a 'fixed income' which is then interpreted as disposable income and WTP valuations of individuals is interpreted against the background of this endowment. However, the amount of 'fixed income' subjects receive is crucial in interpreting their behavior. Unrealistically low or high disposable income may result in violation of the non-satiety condition assumed in these experiments. Future research with these methods may require higher endowments for the higher income population than the rural areas.

One of the major challenges faced in the analysis of choice experimental data was due to the unlabeled nature of the choice sets. Choice experiments in which the alternatives do not have a specific name or label are referred to as unlabeled or generic choice experiments (Louviere et al., 2000; Hensher et al., 2005). In my study each respondent was presented with eight choice tasks in which respondents had to choose either option A or B. Option A and B were alternative water samples, each with a price attribute and varying levels of water safety. However the attributes in either option were not specific to being in option A or B. These alternatives are referred to as generic because the alternatives have no specific name or label, unlike alternative-specific options where labels can be attached to the alternatives.

In labeled choice experiments, the label is the object of choice, interpretation of choice behavior is straightforward (because the label itself conveys information about the alternative). Agents either choose one label or the other (for example, choosing to take a train, or walk to work as a mode of transportation). As an example, suppose a study is designed to value people's preferences for a shopping trip and subjects had to choose between attributes in say Walmart and attributes in K-mart (or choice amongst say modes of transport "walk, or train" or brands of a product). Alternatives are created pertaining to each of the shopping trips and subjects would be required to indicate their preference between "Walmart" and "Kmart" at each choice occasion based on the attributes within each option, as opposed to a choice merely between A or B.

The preference parameters in generic choice experiments on the other hand are interpreted in terms of their effect on the probability of respondents' choice between option "A" and option "B" or choice between "forest pairs" (see Hanley et al., 1998), or between "bottle types" (Mtimet and Albisu, 2006), rather than their effect on the probability of choosing say, "Walmart". It is perhaps because of this lack of ease in interpretation that most studies in the literature on discrete choice experiments take the easier route and use the non-generic experiments (the alternative-specific choice experiments), and in so-doing, bypass the challenges faced in analysis and interpretation of generic utility parameters.

Discrete choice methods were originally designed for market research, and these methods are well developed in the marketing literature. The use of these methods in environmental valuation requires some adjustment (in terms of definition of attributes and classification of choice alternatives) to make them appropriate for a given environmental situation, which may not be the easy route. Perhaps the choice options could be labeled 'Bottled water" vs "tap water" or "organic groundnuts" vs "conventionally produced groundnuts". The practice of labeling in environmental valuation no doubt eases the analysis but may result in distorted responses when subjects attach more weight to the label than the attributes it represents. Whether to use labeled or unlabeled choice alternatives is a challenge facing choice modelers in environmental economics. The practice in experimental economics with auctions has been to state options in neutral unloaded words (Friedman and Sunder, 1994).

The other challenge with my choice experimental procedures is that no base alternative (status quo) was provided for in the choice occasions against which to compare other alternatives. Most choice experimental studies include a status quo alternative (current practice) against which the general results are compared, which is particularly useful for policy formulation. In future work, inclusion of a status quo alternative may be necessary.

With regards to experimental auction results, the skewness of the WTP data for the Kampala sub-sample is troubling, and invokes skepticism when interpreting results from the Tobit model. Non-normality in the data limits the usefulness of the t-statistic measure of significance. Modeling skewed data using models that assume normality is one way of obtaining biased estimates. The Tobit model assumes normally distributed errors. Data from the urban sub-sample fails the skewness-kurtosis test. The rural data set on the other hand, has its distributional features closely related with the pre-test data with a normal distribution. However, the normality assumption is often relaxed in sufficiently large samples. As a rule of thumb, at least 30 observations are sufficient. An alternative might be to employ non-parametric estimation techniques.

5.6 Policy Recommendations and Conclusions

Among the strong results from this study was the manifestation of free riding in public goods provision, differences in rural and urban sample bidding behavior, and differences in bidding behavior due to income differences. The first of these suggests government intervention may be necessary if one is to see improvement in both health and environmental provision. If the problems associated with pesticides in the environment and their probable effects on humans are to be addressed, reliance on the individuals directly concerned will not be sufficient and a concerted effort of a ‘third party’ may be necessary – a typical public goods problem.

Differences in willingness to pay values between urban and rural populations suggest that programs should be tailored to suit different locations. Rural populations are less willing to pay to avoid ill-health outcomes and also, since their line of work exposes them more to pesticides than the urban population, are more prone to potential contamination. Perhaps the rural population feels that it is their obligation to be provided with a clean environment, or they generally do not care as much about their health and environment, or they perceive the concentration levels in the pesticides to be too low as to cause them any damage, or indeed as one rural respondent mentioned: “I have been subjected to these chemicals for a long time. But look at me, I am still healthy”. Such reasoning, if widespread in the rural population, is an indication that rural people may be discounting the future heavily, that to them, what matters is their immediate survival. In any case the rural-urban differences in WTP suggest that any health and environment improvement programs may require different interventions for the different regions.

Perhaps the most important explanation for differences in bidding behavior for health and environmental improvement can best be attributed to differences in income, rather than differences in non-economic issues (recall that information treatments and education had no significant positive effect on willingness to pay). The high willingness to pay to avoid contamination that higher income (salaried and urban) subjects had is an indication that increased incomes induce health and environmental awareness. As economic development increases and people’s incomes increase, the demand for environmental quality by Ugandans can be expected to rise. People may devote some of their increased income to improving their health and their environment, and protection of natural resources.

Inference made from results from both choice experiments and experimental auctions varied. Knowledge of this variation is important when attempting to compare or combine results from the two methods. One practice that is currently on the increase is to combine revealed preference and stated preference results to predict behavior. When results from either set of data are either inaccurate or doubtful, the combination becomes inappropriate. In the current study, even with the same individuals, and when the ‘goods’ being valued were identical, different methods yielded diverging results, suggesting that they should not be combined.

In the strictest sense that choice experiments valued environmental improvements and experimental auctions valued health improvements, the failure to obtain choice experimental data from rural respondents precludes the testing of whether urban populations care more about their environment than the rural populations. However differences existed between salaried and non-salaried respondents and with the positive correlation this variable has with incomes, results from the experimental auctions can be interpreted as higher income populations care more about a cleaner environment than low income populations.

There may not be as clear a distinction between “health and the environment” as presented in this study. There shouldn’t be. The two components are inter-related. One needs a good environment to support human health, and some life-support activities of humans may be put at risk when the environment is overly degraded. Environmental valuation requires creating a link between changes in the environment and how they affect health, and then estimating how these changes in health are reflected in WTP values.

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Appendices

Appendix 1: Pretest Procedures and Findings

a) Step-by-Step Pretest Procedures

1. Participants for the pre-tests arrived at Waibuga sub-county headquarters and got seated.
2. The coordinator introduced the researcher (Jackline Bonabana-Wabbi) and the research assistant (Basil Mugonola) as a team from Makerere University Faculty of Agriculture. We then addressed all the participants and told them we were there to learn from them through their actions on a simple task.
3. Participants were divided into two groups of eight individuals and all were asked to move out of the room as 'we prepare the experimental lab'.
4. The first 8 farmers were the 'no information water treatment' group
5. For the water treatments, two hundred and fifty millimeters of water were poured into two identical clear plastic cups labeled A and B. Water poured in cup labeled A was bottled water purchased in supermarkets. Water poured in cup B was ordinary tap water. For the groundnut treatments, it was impossible to obtain certified pesticide-free groundnuts for sample A. One eighth kilo of roasted pre-packaged groundnuts purchased from retail shops were presented, having been labeled A and B.

Appendix Table 1: Pretest (Information, Proxy good) Treatments

	No-Information given	**Information given**
Water	Farmers 1-8 (Session One)	Farmers 9-16 (Session Two)
Groundnuts	Farmers 1-8 (Session Three)	Farmers 9-16 (Session Four)

6. The first session participants were told to get seated at a desk which had the two samples, a bidding sheet, a khaki envelope with Shs. 500 endowment and a pen.
7. The experiment instructions were read to the participants and then they were given an opportunity to ask questions for clarification before bidding started. Bidding followed the *n*-th price auction.
8. At the end of the trial, participants who had bid amounts less than the binding amount were then told to consume B.
9. Session one participants were then asked to wait out side while the next session was being run, after which they would be let in again to run session three.
10. After the four sessions were run, all participants were gathered back into the room and presented with the 5 choice sets below. Using the first choice set as an example, the researcher explained what she expected subjects to do for the four remaining sets.

Appendix Table 2: Pretest Choice Sets

	A	B	No. 1
DRINKING	Never safe for drinking	Not Very safe for drinking	
AGRICULTURAL	Very safe for crops and animals	Very safe for crops and animals	
PRICE YOU WOULD PAY			
Tick either A or B			
	A	**B**	**No. 2**
DRINKING	Never safe for drinking	Never safe for drinking	
AGRICULTURAL	Not Very safe for crops and animals	Never safe for crops and animals	
PRICE YOU WOULD PAY	100	200	
Tick either A or B			**8/6**
	A	**B**	**No. 3**
DRINKING	Not Very safe for drinking	Never safe for drinking	
AGRICULTURAL	Not Very safe for crops and animals	Never safe for crops and animals	
PRICE YOU WOULD PAY	100	0	
Tick either A or B			**9/7**
	A	**B**	**No. 4**
DRINKING	Not Very safe for drinking	Very safe for drinking	
AGRICULTURAL	Never safe for crops and animals	Never safe for crops and animals	
PRICE YOU WOULD PAY	500	500	
Tick either A or B			**6/9**
	A	**B**	**No. 5**
DRINKING	Very safe for drinking	Very safe for drinking	
AGRICULTURAL	Very safe for crops and animals	Not Very safe for crops and animals	
PRICE YOU WOULD PAY	0	0	
Tick either A or B			**10/5**

11. After all choice sets were gathered, participants were engaged in a focus group discussion as the researcher recorded their responses. Participants were asked:
    a. What they understood the sessions were asking them to do.
    b. To explain their reasons for bidding the way they did during the sessions.
    c. If they thought the set-up of the experimental procedures were clear: the 100 shilling coins provided in envelopes, the ‘classroom’ setting, the goods provided and the set of instructions.

d. What they thought about the ranges of values and levels of characteristics they had answered, and to consider a situation where other values and ranges were included.
e. To give suggestions as to how differently the sessions could have been done.
f. To give their general views on pesticides in the environment.
g. If they had ever participated in any session similar to what they had done that day.

12. Participants were paid a full day's per diem allowance at the end.

b) Pre-Test Findings and suggestions for actual experiments

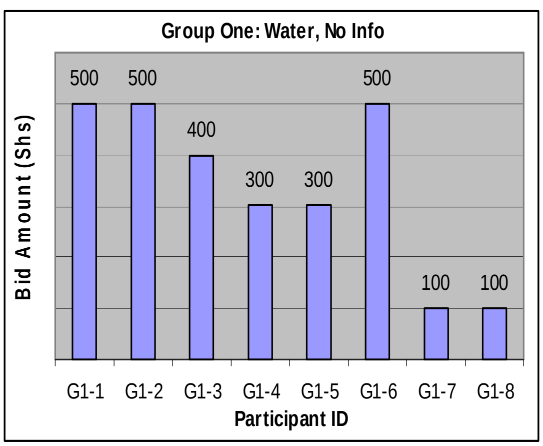
13. Participants believed that the two samples A and B of the same good were different. I had earlier been concerned that participants would not believe that the two samples were different since the contents were the same (amount and color) and were packaged exactly the same.
14. They also believed that sample B was contaminated. There was hesitancy when the experiment ended and those participants who had bid lower than the binding amount had to consume the unit B – an indication that when a unit is contaminated and this information is known, people tend to avoid consuming it. I had previously been concerned that participants wouldn't believe that I could provide a unit of a potentially contaminated good and expect them to consume it, or as one member of the committee said, that if subjects believed the researcher to be benevolent, they would anticipate the 'contaminated' products to not really be contaminated. Had this been the case, I would have observed very low bids across all treatments, and no hesitancy to consume Unit B at the end of the trial.
15. Another concern was that since I had briefly communicated the nature of research to the coordinator/mobilizer, he might offer that information to the participants as he mobilizes them and consequently bias their responses in a way. As such in the actual experiment, I planned on including a question in the post evaluation session as to whether a participant had knowledge of why he/she was invited for the day's session. Farmers revealed that they had been invited to 'attend a field school'. Others said they did not know why they had been invited.
16. The form of the good provided also mattered. People were more willing to consume potentially contaminated groundnuts than water. At the end of the session when those who had bid low values had to consume Unit B, there was a lot more hesitancy when the treatment was 'Water' than when the treatment was 'Groundnuts'.
17. Some people were not confident enough to write down values even though they knew what they wanted to bid. Some were asking too many questions such that the experimenter had to be very tactical not to reveal the 'purpose' of the experiment and bias subjects while trying to answer their queries. In the actual sessions a decision was made to read the entire set of instructions to the whole group more than once. Also the example (in **Appendix 4**) was written on large manila charts and pinned up for all to see).
18. Bid values span the entire range of the spectrum. There didn't seem to be any clustering around the lower end (at 0) or at the high end (at 500). This indicated to me that the endowment was sufficient.

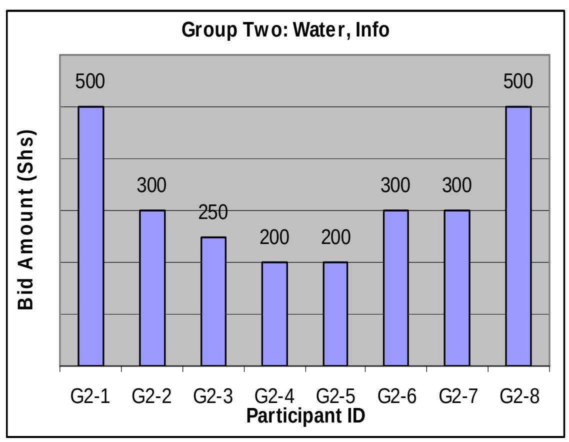
19. Some reasons they gave for bidding the way they bid varied
    a. One person who bid zero mentioned that he has never been affected by chemicals. He said that he believes chemicals used in agricultural production are very diluted as to cause any health problems to his life.
    b. Another person mentioned that he would have to spend much more if he ate the potentially contaminated product and had to go to hospital. He bid his total endowment.
    c. Another mentioned that he was educated enough to know that for good health, one requires to spend on it. He also bid the maximum
    d. One person mentioned that he has always consumed contaminated stuff but that he has never had any health problems. He bid 300 so that he can retain 200 for his lunch but said he could have easily bid zero.

20. In the final design, it was decided to inform session subjects that a 'lunch allowance' was prepared for each and everyone who took part in the session prior to the start of the session. This was done so that the above reasoning doesn't influence bid amounts.
21. Although there were roughly equal numbers of females and males at the pretests, most responses in 18 above were given by males. Further probing was necessary to obtain responses from females. It was then decided that in the final design, each person would be given an opportunity to express their views by giving them their separate post-bidding evaluation form.[37]
22. Of the four sets participants were given to evaluate, set number 2, 4, 5 were implausible combinations. The last column of Appendix Table 2 shows the number of times one option was chosen over the other.

Group one binding amount: Shs. 400

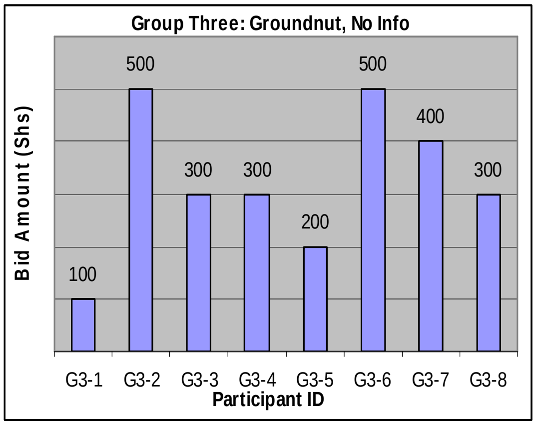
Group two binding amount: Shs. 300



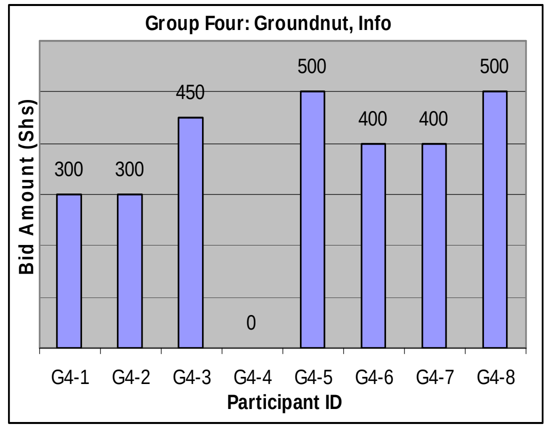


[37] As it turns out, this didn't help much. Post evaluation responses could only be obtained from 26 respondents of the Iganga sample.

Group three binding amount: Shs. 300



Group four binding amount: Shs. 450



Appendix 2: Urban Subjects Recruitment Notice[38,39]

Invitation to Participate

Participants are hereby sought to participate in a research study designed to explore how people make decisions in a complex dynamic setting.

This study will take place in the Faculty of Agriculture computer room at Makerere University. Sessions will last approximately 45-50 minutes each and participants will be allowed to participate in only one session.

Responses will be anonymous and the information gained in this research will be kept confidential.

Limited slots are available, so if you are interested, please call the number below. Your time will be compensated for.

For more information, please contact:

Mrs. Jackline Bonabana-Wabbi
0774-899799

[38] Twenty five of these announcements were posted around Kampala (Garden city mall, Makerere University, Game supermarket, Bukoto, Kabalagala trading centers, at the junction of Kampala Road and Luwum streets and at the National Theatre)

[39] The mention of sessions being carried out in a 'computer room' could explain the low voluntary turn-up. The computer room at Makerere University had only been reserved for the sessions for its convenience (tables, chairs, location, accessibility, secluded atmosphere) and not for the computers in there. This oversight might have influenced the low number of volunteers. On the other hand, a number of other reasons could be blamed for this low turn-out. For example the announcement did not mention the amount of compensation that participants would get.

Appendix 3: Sample Selection and Treatments in Iganga and Kampala

Iganga (121)			Info (62)	No Info(59)
	Water (60)	Group (34)	8	26
		Individual (26)	18	8
	Groundnuts (61)	Group (26)	18	8
		Individual (35)	18	17
Kampala (156)			Info (68)	No Info (88)
	Water (89)	Group (41)	16	25
		Individual (48)	24	24
	Groundnuts (67)	Group (39)	23	16
		Individual (28)	5	23

Appendix 4: Individual Decision-Making Treatment: Groundnuts and Water

Important: While you are here no communication between yourself and any member of this group is allowed

1. On the desk in front of you there is an envelope and its contents which has been given to you which at a later time you will open.
2. On that desk also there are two 'units' of a good.
3. Unit A is from a trusted source and is free from any pesticide contamination.
4. Unit B is from a pesticide-contaminated source and could have pesticide residue.
5. Your task in this session is to decide how much of what you have been given (in the envelope) you wish to pay me so that you do not have to consume Unit B.
6. You will write down this amount in the space provided on the sheet of paper.
7. You will write your identification code (not your name) in the space provided at the top of the paper.
8. One of us will come around and note the amount you have written.
9. At the end I will arrange the amounts from all of you in order from the highest to lowest.
10. I have here a 6-sided die. I will roll it after arranging the amounts you wrote. The number that comes up will be the 'rank' of the bids and the amount it corresponds with will be 'binding' amount.
11. I will then identify those of you who chose to write amounts higher and those who chose to write amounts lower than this binding amount.
12. For those of you who will have chosen an amount higher than the binding amount, I will collect the binding amount and remove both Units A and B. You will not consume Unit B.
13. For those of you who will have chosen an amount equal to, or less than the binding amount, I will collect Unit A and leave Unit B. You will consume Unit B and will not be required to pay what you wrote down.

As an example, suppose that in the session you chose to pay an amount and wrote it down. Also suppose that the amounts of all the other participants were collected and put in a table as below.

Example

ID Code	Amount
S814	2300
S960	1100
S203	0
S395	4500
S427	5100
S051	0
S582	3400
S198	2000
S724	2500
S639	3300
S941	1600
S436	900

Suppose that after arranging all the amounts the results appeared as in the table below. Also, suppose that after rolling the die, the number ‘4’ turns up. Then by our decision rule, 3,300 would be the binding amount.

ID Code	Amount	Rank
S427	5100	1
S395	4500	2
S582	3400	3
S639	**3300**	**4**
S724	2500	5
S814	2300	6
S198	2000	7
S941	1600	8
S960	1100	9
S436	900	10
S203	0	11
S051	0	12

If you had chosen 1,600 you would have chosen an amount lower than 3,300. So you would have to consume Unit B.

If you had chosen 3,400 you would have chosen an amount greater than 3,300 and therefore you would not have to consume Unit B.

Now, let us see if you understood:

- Suppose you had chosen the amount of S436, what would happen?
- Suppose you had chosen the amount of S582, what would happen?

Is everyone clear on how the experiment works? Before the experiment starts, if you have any questions, please raise your hand and I will come to your desk. Remember that while you are here no communication between yourself and any member of this group is allowed (or you will be disqualified).

Now if there are no questions, take a minute to think about the amount you are willing to pay

---

Bidding Sheet: Individual Decision Making
(For Both the information and no-information treatments)

Identification Code: ___________________________

Amount you are endowed with Shs. __________

Amount you are willing to pay Shs. __________

Appendix 5: Brief Information (information treatment only):

a) Water

Chemicals used in agriculture may end up in the air and get inhaled, may end up on food and get ingested by humans, or may come in contact with skin. These chemicals may also be washed into rivers, wells, and other water supply sources. Drinking such water can cause human health problems that may require health care expenses to treat those conditions. Thus costs may be involved when the water you drink is contaminated with pesticides.

b) Groundnuts

Chemicals used in agriculture may end up in the air and get inhaled, may be washed into rivers, wells, and other water supply sources, or may come in contact with skin. These chemicals may also remain on the sprayed food and get ingested by humans. Consuming such food can cause humans health problems that may require health care expenses to treat those conditions. Thus costs may be involved when the food you consume is contaminated with pesticides.

c) Amaadhi (Water translated in Lusoga)

Eidhagala lyetukozesa mu kulima lisobola okufuumukira mu mpewo gyetwiisa, oba lyaajya ku mmere gyetulya, oba lyaajya ku mibiri gyaiffe. Eidhagala lino lisobola okukulungusibwa amaadi me lyaajya munsulo edhivaamu amaadhi getunywa. Okunywa amaadhi gano kisobola okutulwaaza kyaatweetaagisa okujya mu malwaaliro okwedhandhabisa. Nolwensonga eyo, wabaawo okukosebwa bwetunywa amaadi nga galimu eidhagala lino.

d) Amaidho (Groundnuts translated in Lusoga)

Eidhagala lyetukozesa mu kulima lisobola okufuumukira mu mpewo gyetwiisa, oba lisobola okukulungusibwa amaadhi me lyaajya munsulo edhivaamu amaadhi getunywa, oba lyaajya ku mibiri gyaiffe. Lino eidhagala lisobola okusigala ku mmere gyetulya. Okulya emmere eno kisobola okutulwaaza kyaatweetaagisa okujya mu malwaaliro okwedhandhabisa. Nolwensonga eyo, wabaawo okukosebwa bwetulya emmere elimu eidhagala lino.

Appendix 6: Post-Experiment Evaluation:

Identification Code: ____________________________

These questions will now ask you to state the reasons for choosing the values that you chose to write down.

Amount you chose to pay above____

1. **You could have chosen any other amount but you thought and decided the amount above.** Why did you choose that specific amount that you chose (Please explain clearly)____
   ____
   ____
2. Did you feel that you knew what you were doing? Did you feel like you understood the experiment?____
3. Have you been involved in this type of experiment before? _____________________
4. What is the commonest form in which you consume groundnuts? ________________
5. How often do you consume groundnuts in the following forms in a week?

Form	Frequency
Boiled	
Paste	
Roasted	
Sauce	
Raw	

6. How many times a week do you drink bottled water? __________________________
7. On average how much do you spend on drinking water per week? ______
8. When preparing groundnuts for eating what steps do you take to rid them of foreign matter before you consume them?

Pre-Consumption preparation	Tick if applicable
Washing only	
Boiling	
Roasting	
Other (please specify)	

9. What are the sources of your household income (name them)
   ____
10. What portion of your monthly income do you use to purchase food crops? ________
11. What are the main food-crops consumed?_________________________________
12. What are your main sources of drinking water__________________________________
13. How do you normally make water ready for drinking?

Water	
Nothing	
Decanting	
Chemical purifiers	
Boiling	
Other (please specify)	

14. How many times have you been hospitalized in the last one year? Circle the one option that best represents your answer: Never 1-10 times 11-20 times More

15. How many times during the past one year have you fallen ill that you did not work because you were feeling ill? Never 1-10 times 11-20 times More

16. How many times during the past one year has anyone in your household fallen ill? Never 1-10 times 11-20 times More

17. Have you or a relative ever been hospitalized from pesticide poisoning? If so, please explain ____

The next set of questions will ask about your characteristics

1. Age ________________ Years
2. Gender ________________
3. What is the number of years you have completed in school? _____________________
4. What are your general views about pesticides in the environment (write as much as you can)

____

____

Thank You for Your Responses

Appendix 7: Group Decision-Making Treatment: Groundnuts and Water

Important: While you are here no communication between yourself and any member of this group is allowed

1. At the desk in front of you there are two ‘units’ of a good.

2. Unit A is from a trusted source and is free from any pesticide contamination.

3. Unit B is from a pesticide-contaminated source and could have pesticide residue.

4. Your task in this session is to decide how much money you will pay so that you do not have to consume Unit B.

5. On your desk you will find three envelopes marked differently. One is marked with letter ‘P’, another is marked with letter ‘G’

6. Another envelope marked with letter ‘E’ contains the amount you will use to make your payments.

7. Amounts you place in ‘P’ will benefit only you. Amounts you place in ‘G’ will be added with the amounts your group members put in their envelope marked ‘G’.

8. The total amounts in envelopes marked ‘G’ will be marked as the amount the group as a whole is willing to pay to avoid consuming Unit B.

9. I have here a 6-sided die. I will roll it after adding the group total from the ‘G’ envelopes. The number that comes up will be the ‘binding’ amount

10. Take a minute now to make your decisions.

11. You will place what you have chosen as your ‘private’ amount into envelope marked ‘P’ and place the group amount in envelope marked ‘G’.

12. Then you will write your identification code (not your name) at the top of the envelope marked ‘G’.

13. One of us will come around and collect the envelope ‘G’. You will remain with envelope ‘P’ and emptied envelope ‘E’.

14. The amounts in envelope ‘G’ will be arranged in descending order and a binding average will be determined by rolling a die.

15. The value (Y) of avoiding consuming Unit B as a group will be obtained by multiplying the binding average by the number of group members.

16. If your group contributions are below Y, you will **all** have to consume the good Unit B regardless of the amount you will have put in your envelope ‘P’ and regardless of the amount you individually placed in envelope marked ‘G’.

As an example, suppose that in the session you chose to pay an amount and placed that amount in envelope marked ‘G’ putting the rest in your envelope marked ‘P’. Also suppose that the amounts in envelopes marked ‘G’ from all other participants were collected and put in a table as below.

Example

ID Code	Amount
S814	2300
S960	1100
S203	0
S395	4500
S427	5100
S051	0
S582	3400
S198	2000
S724	2500
S639	3300
S941	1600
S436	900

Suppose that after arranging all the amounts from highest to lowest, the results appeared as in the table below. Also, suppose that after rolling the die, the number 4 turns up. Then by our decision rule 3,300 would be the binding average.

ID Code	Amount	Rank
S427	5100	1
S395	4500	2
S582	3400	3
S639	3300	4
S724	2500	5
S814	2300	6
S198	2000	7
S941	1600	8
S960	1100	9
S436	900	10
S203	0	11
S051	0	12

This binding average would then be multiplied by the number of group members to obtain the binding total. ie 3300 x 12 = 39600

If the group contributions are less than this binding total I will only remove Unit A and the whole group would have to consume Unit B.

If the group contributions are greater than this binding total I will come and remove both Unit A and Unit B. The whole group would not have to consume Unit B.

Now, let us see if you understood:

Suppose, using our example above, that the group contributions were 17,500 and the binding total was 39,600 what would happen to the whole group? ____________________________

Suppose, using our example above, that the group contributions were 40,250 and the binding total was 39,600 what would happen to the whole group? ____________________________

Is everyone clear on how the experiment works? Before the experiment starts, if you have any questions, please raise your hand and I will come to your desk.

Remember that while you are here no communication between yourself and any member of this group is allowed (or you will be disqualified).

Now if you have no further questions, you are ready to begin. Take a minute now to make your decisions.

Appendix 8: Consent Form

VIRGINIA POLYTECHNIC INSTITUTE AND STATE UNIVERSITY

Informed Consent for Participants in Research Projects Involving Human Subjects

Title of Project: Health and Environmental Benefits of Reduced Pesticide Use
Investigators: Dr. George W. Norton, Mrs. Jackline Bonabana-Wabbi

I. Purpose of this Research

This study aims to establish the value that people place on their health and environment. The total number of subjects will be two hundred and fifty eight obtained from a self-selected sample of adult males and females in Kampala and Iganga.

II. Procedures

Valuation will be obtained from people's willingness to pay to avoid bad environmental situations.

III. Risks

No risks are involved in this experiment.

IV. Benefits

No promise or guarantee of benefits have been made to encourage you to participate. Benefits to you will come from knowing that you have participated in a study aimed to find out how people value their health and environment.

V. Extent of Anonymity and Confidentiality

You will be assigned a random code which you will use for identification instead of your names. Responses will remain anonymous and study results will in no way be related to participants individually. It is possible that the Institutional Review Board (IRB) may view this study's collected data for auditing purposes. The IRB is responsible for the oversight of the protection of human subjects involved in research.

VI. Compensation

You will receive an amount of money equivalent to the average per capita daily income as compensation for participating in this study.

VII. Subject's Responsibilities

I voluntarily agree to participate in this study.

VIII. Subject's Permission

I have read the Consent Form and conditions of this study. I have had all my questions answered. I hereby acknowledge the above and give my voluntary consent:

Subject signature: ________________ Date: ___________________

Should I have any pertinent questions about this research or its conduct, and research subjects' rights, and whom to contact in the event of a research-related injury to the subject, I may contact:

Mrs. J. Bonabana-Wabbi	540-231-2983/jbonaban@vt.edu
Dr. George W. Norton Investigators	540-231-7731/gnorton@vt.edu Telephone/e-mail
Dr. Dan B. Taylor Faculty Advisor	540-231-5032/taylord@vt.edu Telephone/e-mail
Dr. Kevin J. Boyle Dept Reviewer/Dept Head	540-231-2907/kjboyle@vt.edu Telephone/e-mail
David M. Moore Chair, Virginia Tech Institutional Review Board for the Protection of Human Subjects Office of Research Compliance 1880 Pratt Drive, Suite 2006 (0497) Blacksburg, VA 24061	540-231-4991/moored@vt.edu Telephone/e-mail

Appendix 9: Derivation of Logit Probabilities from the iid Extreme Value Distribution Assumption (Kuminoff, 2007; and Train, 2003).

$$P_{iq}\bigg|\varepsilon_{iq} = \prod_{i\neq j} e^{-e^{-(\varepsilon_{iq}+V_{iq}-V_{jq})}}$$

$$P_{iq} \quad = \int_{s=-\infty}^{\infty}\left(\prod_{i\neq j} e^{-e^{-(s+V_{iq}-V_{jq})}}\right)e^{-s}e^{-e^{-s}}\,ds$$

$$= \int_{s=-\infty}^{\infty}\left(\prod_{j} e^{-e^{-(s+V_{iq}-V_{jq})}}\right)e^{-s}ds$$

$$= \int_{s=-\infty}^{\infty} e\left(-\sum_{j} e^{-(s+V_{iq}-V_{jq})}\right)e^{-s}ds$$

$$= \int_{s=-\infty}^{\infty} e\left(-e^{-s}\sum_{j} e^{-(V_{iq}-V_{jq})}\right)e^{-s}ds$$

Letting $t = e^{-s}$ then $dt = d(e^{-s})$ implying that $dt = -e^{-s}ds$. Then the probability of choosing *i* over *j* is:

$$=\int_{\infty}^{\infty} e\left(-t\sum_{j} e^{-(V_{iq}-V_{jq})}\right)(-dt)$$

$$=\int_{0}^{\infty} e\left(-t\sum_{j} e^{-(V_{iq}-V_{jq})}\right)dt$$

$$=\left.\frac{e^{-t\sum_{j} e^{-(V_{iq}-V_{jq})}}}{-\sum_{j} e^{-(V_{iq}-V_{jq})}}\right|_{0}^{\infty}$$

$$=\frac{1}{\sum_{j} e^{-(V_{iq}-V_{jq})}}$$

$$=\frac{1}{e^{-V_{iq}}\sum_{j} e^{V_{jq}}}$$

$$P_{iq} = \frac{e^{V_{iq}}}{\sum_{j} e^{V_{jq}}}$$

Appendix 10: Choice Questions

In the choice card below, varying levels of water attributes (safety and cost) are listed. The choices are provided in tabular form. We are interested in knowing which set of qualities you would prefer.

As an example, in the choice set (number 8) below

Assuming that the following scenarios were available:

- Choice A involves water you consider never safe for human consumption (drinking) due to pesticide contamination, but is very safe for farm animals, and the price you would pay for it is Shs. 200.
- Choice B involves water that, due to pesticide contamination, you consider never safe for drinking, and is not very safe for agricultural crops and animals, but costs you nothing.
- Which option would you prefer? Tick the option you choose

Choice set # 8

	A	**B**
DRINKING	Never safe for drinking	Never safe for drinking
AGRICULTURAL	Very safe for crops and animals	Not very safe for crops and animals
PRICE YOU WOULD PAY	200	0
PLEASE CHOOSE ONLY ONE		

Choice set # 1

	A	**B**
DRINKING	Never safe for drinking	Never safe for drinking
AGRICULTURAL	Never safe for crops and animals	Not very safe for crops and animals
PRICE YOU WOULD PAY	0	300
PLEASE CHOOSE ONLY ONE		

Choice set # 2

	A	**B**
DRINKING	Never safe for drinking	Never safe for drinking
AGRICULTURAL	Never safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	100	500
PLEASE CHOOSE ONLY ONE		

Choice set # 3

	A	B
DRINKING	Never safe for drinking	Never safe for drinking
AGRICULTURAL	Not very safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	0	500
PLEASE CHOOSE ONLY ONE		

Choice set # 4

	A	B
DRINKING	Not Very safe for drinking	Never safe for drinking
AGRICULTURAL	Not very safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	200	300
PLEASE CHOOSE ONLY ONE		

Choice set # 5

	A	B
DRINKING	Very safe for drinking	Not very safe for drinking
AGRICULTURAL	Very safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	300	100
PLEASE CHOOSE ONLY ONE		

Choice set # 6

	A	B
DRINKING	Very safe for drinking	Never safe for drinking
AGRICULTURAL	Very safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	500	100
PLEASE CHOOSE ONLY ONE		

Choice set # 7

	A	B
DRINKING	Not very safe for drinking	Not very for drinking
AGRICULTURAL	Not very safe for crops and animals	Very safe for crops and animals
PRICE YOU WOULD PAY	200	500
PLEASE CHOOSE ONLY ONE		

Appendix 11: Test of Design Optimality SAS code

```
proc plan ordered;
factors drink=3 agric=3 cost=5/noprint;
output out=can
drink nvals=(0 to 100 by 50)
agric nvals=(0 to 100 by 50)
cost nvals=(1 to 5);
proc print data=can;
data preset;
input drink agric cost;
datalines;
0 0 1
0 0 2
0 50 1
0 100 4
0 100 2
0 100 2
50 50 3
0 100 3
50 50 4
0 100 5
0 50 4
50 50 3
50 100 4
100 100 5
100 100 1
100 100 4
50 100 3
50 100 1
50 100 2
50 50 2
50 50 5
50 50 4
0 50 1
0 100 3
0 100 1
0 50 2
0 50 3
0 100 5
0 0 5
100 100 3
50 100 5
0 50 2;
proc optex data=can;
class drink agric cost;
model drink agric cost;
generate n=32 augment=preset;
output out=trial8;
proc print data=trial8;
run;
proc optex data=can;
class drink agric cost;
model drink agric cost;
generate n=32 method=m_federov;
output out=trial9;
proc print data=trial9;
```

```
run;
```

Appendix 12: Data (See attached files)

The experimental auction data file for Kampala (auctionkla.pdf) contains 18 variables and 156 observations. The qualitative variables (gender, treatment variables, occupation, hospitalization, illnesses and pesticide exposure) are coded on a 0,1 binary scale. (See Table 4.1 for a definition of the variables. Four quantitative variables (age, education, willingness to pay amount and weekly water expenditure) are recorded exactly.

The experimental auction data for Iganga (auctioniganga.pdf) contains 7 variables and 121 observations including four qualitative variables (gender, proxy good, group, info) coded on a 0,1 scale and three quantitative variables (WTP, education and age).

Data for the choice experiments is contained in choicedata.pdf. The file contains 29 variables and 2112 observations. In addition to the qualitative and quantitative variables from the auction data, the dataset contains responses on 8 choice tasks for each of 132 individuals. Two water category qualitative variables each at three different levels are included. These are converted into a 0,1 scale (creating 3 variables each). Definition of the variables is given in Tables 4.1 and 4.14.