Chapter 1

General Introduction

1.1 Introduction

Deforestation is more often the rule than the exception in many of tropical countries. It is also becoming an issue of overarching concern in the present era of climate change. A host of factors, such as the expansion of agricultural land into forest areas and the increasing extraction of forest products for fuel and construction purposes, are major proximate causes of deforestation[1] in developing countries. Fundamental causes that underlie these proximate ones include poverty, agricultural market integration, agricultural subsidies, agricultural technological change, market failure, institutional failure and policy failure (Angelsen, 1999). These countries are dependent[2] on agriculture, where land is the key input to production. Agricultural subsidies, technological change in agriculture and the globalization of agricultural markets increase land rents, giving rise to the conversion of forests into agricultural land.

Although it is tempting to argue that this conversion is warranted, assuming that the benefits outweigh the costs, it should be kept in mind that the benefits are exaggerated by subsidies, while the marginal cost – marginal benefit calculus fails to account for externalities arising from forestry. In other words, forest conversion decisions do not reflect the socially optimal land use allocation between

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[1] Non-economists define deforestation as a reduction in the stock and the quality of forest cover.

[2] As economies undergo structural changes and grow the share of agriculture decreases, while the shares of manufacturing and services increase. Under such circumstances, people are expected to leave rural areas and move to cities and, hence, deforestation is expected to slow down as countries become richer.

agriculture and forestry. A similar argument holds with regards to the extraction, from forests, of timber for both energy and construction purposes. Therefore, social planners would choose a different land use and forest use allocation than would be observed in the economy. It is this disparity in allocation that we regard as deforestation in economics[3].

Institutional failure and market failure provide the primary explanations for these differences. Institutional failure, especially with regard to the management of natural resources, takes the form of ill-defined property rights and the absence of complementing governance institutions for open access resources. For the most part, natural forests have been historically owned and managed by the state (Sterner, 2003), resulting in *de facto* open access for the forests. Within developing countries, due to imperfect incentives, as well as prohibitively high information, monitoring and enforcement costs, open access forestry has become the norm. Another form of institutional failure that is common in developing countries is land tenure insecurity. Uncertain land ownership discourages investment, providing farmers with an incentive to overexploit their current allotment before moving on to a new plot that could be carved out of the forest. Combined, these institutional failures have contributed to forest conversion over many decades.

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[3]Following this line of argument, different levels of deforestation could be optimal for local, national and global social planners, as the externalities differ across these scales.

Market failures, especially with regard to forest products and services, are equally important in explaining deforestation. Apart from woody biomass, i.e., timber and fodder, forests provide multiple environmental services, including siltation control, soil erosion protection, carbon sequestration and biodiversity conservation. Markets do not generally exist for these environmental services, because these services are public goods. In other words these goods are both non-exclusive and non-rivalry[4], and, in this case, they provide positive externalities.

One question still remains, however, and that is whether or not it is possible to substitute for forests as inputs to production in the economy. Several compelling reasons suggest that substitution is limited, at best. Forests simultaneously produce public and private goods as joint products. Timber, fuel wood and non-timber forest products that can be produced privately, and they can be produced jointly with local public goods, such as reduced soil erosion, watershed protection and nutrient cycling, as well as global public goods, including carbon sequestration and biodiversity. Of particular importance, in many developing countries, is the natural forestry capital associated with agricultural and energy production. For example, reduced soil erosion services and increased nutrient

[4] Non-rivalry implies that the social cost of providing the good to an additional individual is zero. Therefore, setting a positive price and excluding those that derive positive marginal benefit from the good doesn't yield Pareto efficiency. Moreover, if the good is a pure public good, concealing one's preferences and not contributing, or contributing less than one is willing to pay towards supplying the good, constitutes the dominant strategy for all participants. The result is free-riding; the market provides less of the public good than is socially desired (Hanley et.al, 1997).

cycling are important inputs for agriculture. Furthermore, energy production in these countries arises primarily from forest products, notably fuel wood. Although the substitution of man-made inputs, such as fertilizer, soil conservation structures and hydroelectric dams, is possible, energy production substitution is rather limited in developing countries, while substitution remains less than perfect. There still exists a need for forest cover; forests can protect dams and fields from flood-related damages, such as erosion and siltation. More tellingly, technological advances have not developed forest substitutes capable of carbon sequestration and biodiversity conservation. In summary, diminution of the forest stock would reduce the supply of these products and services, which, in turn, could limit economic growth. Similarly, deforestation could contribute to reduced welfare both directly, through a reduction in many of the aforementioned services, and indirectly, through a reduction in economic growth.

It thus follows that significant welfare losses obtain under deforestation, leading to calls for the design and implementation of alternative policy instruments that could mitigate or, preferably, reverse the decline. Past policies, typically command and control in nature, have been commonplace in developing countries. Policies of this nature have included state ownership of forests, timber harvest concessions, forest product trade restrictions and public afforestation programs (Arifin et.al, 2008, Sterner, 2003). Recently, however, there has been a growing interest in alternative policy instruments, primarily based on revised incentive structures (Sterner, 2003). Examples include: reductions in agricultural subsidies, the development of transfer payment mechanisms for non-timber services both within and between countries(Bulte et.al 2008), and the development of enforceable property right institutions. However, the choice between these policy instruments depends on the extent to which each could be defended on efficiency, equity and effectiveness grounds (Sterner, 2003).

1.2 Motivation

Environmental policy instruments are primarily developed for the correction of the market failures that result from the pervasiveness of externalities associated with the environment. Forestry policy is no different, and, for the purposes of this analysis, focus on failures related to deforestation, land degradation and pollution. Two externalities form the basis of concern for the analysis, stock externalities and services externalities.

Under open access regimes, forest product harvest by each user poses what is called a stock externality. As noted earlier, forest stocks are a key factor in the production of household energy and agriculture. By harvesting or clearing[5], each household imposes production diseconomies on each other, which is not accounted for in the harvesting/clearing cost calculus[6]. The failure[7] to account for

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[5] Natural forests are cleared for conversion into agricultural land.

[6]This is particularly true for natural forests considered in this study, in which the extraction cost in terms labor required for harvesting fuel wood, logs and other timeber and non-timber products depends on the density (stock) of the forest.

[7] Households fail to account for this effect, due to strategic reasons. That is, free-riding is the dominant strategy in an open access resource extraction and provision game, which results in a Nash Equilibrium corresponding to lower stock levels and reduced productivity for other factors, such as labor.

this external effect leads to excessive harvest and clearing relative to the socially optimal level, leading lower resource levels characterized by deforestation. In order to manage resources under a stock externality, policy instruments must compel forest users to account for the external cost of their actions. Rather than making their decisions based on the private shadow price, households must base their decisions on the social shadow price. Appropriately defining and enforcing property rights is one instrument that can work, and the definition can be based on nearly any ownership structure, although enforcement is most easily handled through either private ownership or community ownership, and each might have different distributional consequences. The advantages of common property rights are the potential for low monitoring and information costs, as well as better distributional consequences (Sterner, 2003, Dasgupta and Heal, 1979).

Another form of forestry externality arises regardless of whether or not the forest is owned by individuals or the community. As noted earlier, forest stocks are multi-functional; in addition to timber and other biomass products, forests provides local and global externalities, such as biodiversity conservation, carbon sequestration, flood protection services and soil erosion protection. These additional services are public goods and, hence, are characterized by market failure. Consequently, private or communal land use decisions do not account for these externalities.

Although property rights allocations are important, it should be noted that property rights entail a wide range of control issues[8]. Oftentimes control is granted in such a way that exclusion and alienation rights remain with the state, while access, withdrawal and management rights are bestowed on others, such as forest user collectives. A rights allocation of this sort has been given several different names, including joint-management, co-management and participatory forest management. The last of these has been used extensively in many developing countries. However, in the event that forests don't generate significant national and global externalities, control rights are granted to user groups. Community plantations on communal grazing land and private plantations both fall under this category. In many developing countries, such community plantations are used to supply energy, rehabilitate degraded communal lands and reduce pressure on natural forests.

In recent decades, the devolution of natural forest management to the local community (Sikor, 2005, Larsson and Ribot, 2004, White and Martin, 2002 and Agrawal and Ostrom, 2001) and the establishment of community forestry on communal lands (Bluffstone and et al., 2008 Cooke-St.Clair et al. 2008, Köhlin and Amacher, 2005,andGebremedhin et al., 2003, Mekonnen, 2000, and Köhlin, 1998) in developing countries as well as emerging countries, has continued apace. In spite of widespread adoption of these policy instruments, however, empirical

[8] Note that property right regimes differ in two major dimensions, the scope of the exercising group (private, common, state and open) and the degree of control granted to the exercising group, e.g. access right, withdrawal right, management right, exclusion right and alienation right (Ostrom, 2002) .

evidence defending them on efficiency and equity grounds are largely missing or anecdotal. Therefore, additional research is necessary, and that research should improve our understanding in, particularly, four major perspectives related to the welfare impacts of these programs.

First, although many of these programs have been implemented, both researchers and policymakers have ignored the preferences of the potential program participants. Empirical literature aimed at evaluating community forestry welfare has made use of contingent valuation (Carlsson et al., 2004, Köhlin, 2001, and Mekonnen, 2000), cost-benefit analysis (Mekuria et al., 2010,Babulo, 2007, and Jagger and Pender, 2003) and treatment effects estimation via selection models (Köhlin and Amacher, 2005). This research has established that community forestry programs have the potential to significantly benefit program participants. Although each of these studies has provided an important contribution to our understanding of the welfare impacts of community forestry in developing countries, they are limited in two significant ways. Specifically, community forestry is not a single-typed program; it is comprised of multiple attributes, such as the size of the stake of the program, the cost of implementation, the composition of the plantation, the quality of communal land to be allocated and, relatedly, the opportunity cost of alternative land use. Understanding the relative household valuations of these attributes is particularly important, as that information can be used to design incentives that are more likely to lead to the successful implementation of such programs. However, the aforementioned literature does not examine those effects, and therefore, cannot provide the requisite information. Instead, that literature has generally only examined a single attribute of community forest (Carlsson et al., 2003).

Second, these studies have not considered preference heterogeneity related to the various attributes, since only a single attribute is considered. The result is a set of analyses that cannot provide information that could be used to target community forestry program interventions to the community in question.

Third, the foregoing CVM studies estimated welfare impacts without controlling for anomalous preference outcomes that can arise in CVM studies, such as incentive incompatibility, framing effect and anchoring effects. Therefore, it is likely that the results in the literature provide biased estimates of the welfare impact of the programs.

Fourth, theoretical results maintain that the decentralization of natural forest management generates rents and avoids the rent dissipation associated with open access exploitation (*de jure* state property regime). A sizeable body of empirical literature has emerged, and this literature has attempted to validate the previous hypothesis by estimating observed program benefits as opposed to perceived welfare benefits, which is implicit in contingent valuation studies. Unfortunately, this literature has produced inconclusive evidence. Specifically, some of these studies reveal that decentralization offers benefits to program participants (Copper, 2008, and Mullan et al., 2009), while others find contrasting welfare impacts across study villages (Jumbe and Angelsen, 2006); still other studies conclude that there are significant welfare losses (Copper, 2007, Basundhara and Ojha, 2000 and Nuepane, 2003). Methodologically, these studies proceed along different lines. Methods include cost-benefit analysis, computable general equilibrium simulations and econometric methods, the last of which take into account panel data methods, propensity score matching and instrumental variable methods. Such variation in methodology is one possible explanation for the inconclusive evidence. Of interest here, however, are the econometric models and the extent to which their identification strategies could be defended. Whereas matching methods are limited by the restrictive conditional independence assumption, panel data and IV methods are able to relax that restriction in different ways. However, they, in turn, suffer from the assumption of constant treatment effects across the population. Assuming away treatment effect heterogeneity is likely to blur identification of the true welfare effect. Therefore, there is a need to employ alternative strategies that help identify the correct program impact, such that the estimate can inform both policy and academic debates surrounding whether common property right forestry management can effectively revive rural development, while helping to protect the environment.

1.3 Objectives

The present dissertation has been spurred by the aforementioned gaps in the literature. It is organized under the unifying theme of analyzing the welfare effects of common property rights forestry management programs. It comprises of three independent analysis chapters. The first two analysis chapters engage with the valuation of perceived welfare gains that could arise from the establishment of community forestry programs in selected Ethiopian villages. The analysis draws on the stated preference approach. Accordingly, the analysis focuses on the estimation of compensating variation. Contingent valuation methods underpin the analysis; however, the analysis also controls for potential anomalous response elicitation germane to such studies. The analysis includes tests of whether the program is welfare-improving and whether the valuation could have been influenced by the presence of starting point (anchoring) and incentive incompatibility biases. The second analysis chapter aims to identify salient community forestry program attributes, in the sense that these are the attributes that peasant farmers prefer to have included in the program. To that effect, choice experiment methods were employed to estimate the welfare associated with the selected community plantation attributes. Additionally, the application of recent advance in discrete choice econometric models enabled the consideration of both preference heterogeneity and the sources of that heterogeneity.

In the last analysis chapter, by employing a quasi-experimental approach, welfare outcomes that can be attributed to one common property natural forest management program currently run in Ethiopia is considered. The chapter is aimed at estimating welfare improvements brought about by the program. Both matching and IV methods are used to identify the causal impact of the program. An application of the IV method, based on a single binary IV, accounts for program impact heterogeneity via the local average treatment effect, as opposed to matching which only estimates the average treatment effect on the treated. Moreover, with IV methods, both parametric and non-parametric specifications were implemented.

1.4 The Data

The analyses were based on data collected from selected villages in rural Ethiopia during 2009. The data was obtained from rural household responses to different sorts of surveys. The first two analysis chapters were based on an experiment designed for stated preference elicitation. The survey involved valuation questions, where a subject was asked to choose between the status quo and an improved community forestry management scenario for contingent valuation. The survey was extended to include decisions between multiple community forestry program scenarios, in the choice experiment. In the choice experiment, a set of alternative choice sets were designed from four attributes: tree species mix, harvest quota, type of communal land to be used for the forest and the cost of the program. Moreover, the same respondents were further interviewed to elicit data on socio-economic status, as well as access to alternative forest resources.

Data for the last chapter was obtained from a survey that was different than the survey described earlier. The survey was fielded to generate information regarding welfare, especially the impact of natural forest management decentralization in southwestern Ethiopia. In this survey data was collected on a range of variables: household characteristics (eg age, education, gender, and family size), consumption and sale of various goods and services, forest product harvest labor and other activities. More importantly, additional information that was expected to explain household participation decisions was collected. This information included household circumstances that prevailed immediately before the inception of the program, including the distance to the program forest and alternative forests, household assets, household characteristics, participation in off-farm employment, ownership of private trees, participation in extension services, and experience with alternative collective actions. Furthermore, data on community level variables, such as population size, ethnic structure, forest status and location were collected.

1.5 Summary of Analysis

Several findings emerged from the research reported in this thesis. The results from the first chapter indicate that community forestry programs offer sizeable welfare benefits. Furthermore, double-bounded CVM studies in developing country contexts also suffer from preference revelation anomalies, arising from framing effects and incentive incompatibility effects and, therefore, researchers should control for these anomalies. In the second chapter, the results suggested that perceived welfare outcomes of community forestry largely hinge on its attributes such as type of forest, quality of land upon which the forest was to be situated and productivity. Moreover, the results pointed to significant differences in attribute preferences across the study population. In the third chapter, after controlling for selection biases and treatment-effect heterogeneity, the result revealed that common property rights applied to natural forest management raises participant welfare by between 19.96% and 33.63%.

1.6 Thesis Outline

The remainder of this thesis provides a more detailed description of each of the preceding discussion points. Chapter 2 considers double-bounded CVM and preference anomalies uncovered when using that methodology. Chapter 3 examines a choice experiment used to reveal attribute preferences in the context of flexible discrete choice models. Chapter 4 considers treatment effects associated with community forestry in Ethiopia. Finally, Chapter 6 concludes the thesis.

Chapter 2

Contingent Valuation of Community Forestry in Ethiopia: Should We Care About Preference Anomalies in Double-Bounded CVM?

Abstract

This study examines the potential for anomalous response behaviour effects within the context of double-bounded contingent valuation methods applied to community forestry programs in rural Ethiopia. Anomalous responses considered include shift effects, framing effects and anchoring effects, and these effects are considered within a double-bounded contingent valuation study. The results confirmed the presence of incentive incompatibility and framing effects. However, anchoring effects are not uncovered. After controlling for these biases, the community forestry program considered is shown to offer a welfare gain ranging from Ethiopian Birr (ETB) 20.14 to 22.80. In addition to these welfare benefits, the results raise questions with respect to the validity of previous welfare estimates associated with double-bounded CVM studies in developing countries, suggesting that future studies should control for incentive incompatibility and framing effects bias.

Keywords: Double-bounded CVM, incentive incompatibility bias, anchoring bias

2. 1. INTRODUCTION

The valuation of goods, not traded on open markets, is complicated, since preferences over prices cannot be revealed by behaviour, and, therefore, welfare effects related to changes in prices are not easily uncovered. A common approach to evaluating the welfare effects of changes in non-market goods is the Contingent Valuation Method (CVM), since it derives its theoretical basis from welfare economics. A popular survey design for CVM response elicitation is single-bounded, or dichotomous choice design (Whitehead, 2002, Hanemann, 1994, Herriges and Shogren, 1996). The popularity of the dichotomous choice design is due to: U.S National Oceanographic and Atmospheric Administration (NOAA) recommendations (Arrow et al., 1993), its incentive compatibility property (Haab and McConnel, 2002) and its "take-it-or-leave-it" format, which mimics the decision-making task individuals face in daily market transactions (Herriges and Shogren, 1996, Haab and McConnel, 2002). In its simplest form, a survey respondent is asked if he is willing to pay a given sum of money in exchange for a specified change in a non-market good, and the respondent either agrees to pay or does not agree to pay.

Despite its popularity, single-bounded CVM provides limited information about an individual's true willingness-to-pay (Whitehead, 2002, Flachaire, 2006, Herriges and Shogren, 1996) and requires large samples to attain a given level of precision (Hanemann et al., 1991). These limitations have led researchers to look for alternative designs that retain incentive compatibility, but are more efficient (Haab and McConnel, 2002). Hanemann et al. (1991) first devised a double-bounded format, an extension of the single-bounded format that includes a follow-up question, and proved its improved efficiency properties over the single-bounded format.

Unlike single-bounded CVM, where the willingness-to-pay (WTP) is known to lie either above a specified amount or below it, double-bounded CVM provides additional information. Given its structure, in which an individual is first asked to respond based on one value, and then asked to respond to a second level – if the respondent initially says no, the second value is below the first, and if the respondent initially says yes, the second level is above the first – the double-bounded CVM avails the researcher with additional WTP intervals. Estimation of the model incorporates the additional information into the likelihood function to improve model precision.

The fundamental assumption of double-bounded model, as developed by Hanemann et al. (1991), is that the respondent's preferences remain the same over the two valuation questions, such that observations are independent across the two responses. The result is twice as many observations per individual, and, therefore, greater estimation precision.[1] Subsequent studies, however, argue that double-bounded CVM suffers from a number of anomalies. Most poignant of these anomalies is that the subject's response to the second question may be influenced by the first value proposed to them in the survey (Alberni et al., 1997, Flachaire,

[1]Hahnemann et al. (1991) compare the information matrixes across the single-bounded and double-bounded models. They show that a well-designed bid vector yields lower variances in the double-bounded CVM relative to the single-bounded CVM, and empirically validate the conclusion. Empirically, they also find lower point estimates of WTP in the double-bounded model.

2006, Herriges and Shogren, 1996). In other words, the responses may not be independent across the questions, and, therefore, the WTP varies across the questions. Cameron and Quiggin (1994) estimate a bivariate probit model, based on the double-bounded CVM, concluding that the independence assumption is violated in their survey. In other words, it is possible to estimate different WTP values for the same individual, leading to inconclusive results; it is unclear which WTP is the correct WTP.

Several hypotheses explaining the violation of the independence assumption have, since, arisen in the literature. Key, amongst these, is the presence of anchoring and shifting in preferences. The anchoring effect ensues when the respondent is uncertain about the amenity value of the proposal, in which case, the initial value may be suggestive of the true value. Therefore, the respondent anchors her priors on the initial value. Anchoring arises under the belief that either the initial value provides information about the true value of the good (Herriges and Shogren, 1996) or it provides information related to the quality of the good under consideration (Whitehead, 2002). Shift effects, on the other hand, arise if a respondent understands the first value as information regarding the true cost of the proposal. Under shifting, an individual willing to pay the opening value, may perceive the second bid as an unfair request to pay an additional sum; hence, she will undercut her true WTP. In the same vein, for an individual, who rejected the first bid, the follow-up value could be interpreted as a lower quality good, leading to WTP reductions (Alberni et al., 1997). Moreover, recent studies have identified additional sources of preference anomalies that result from follow-up questions of double-bounded CVM studies. *Inter alia*, via the application of Kahneman and

Tversky's (1979) prospect theory, DeShazo (2002) has established that respondents might frame the follow-up offer as a gain or loss compared to the initial offer, which results in a downward bias in the WTP for subsamples subjected to ascending bid sequences.

Several studies control for these undesirable effects, and empirically examine their validity. Early literature includes Herriges and Shogren (1996), who tested for anchoring, and Alberni et al. (1997), who examined shifting. More recent examples include Whitehead (2002), who tested for both anchoring and shifting, as well as Flachiare and Hollard (2006), who tested for starting point bias. Moreover, Chien et al. (2002) tested for the presence of starting point bias along with compliance bias. However, a consensus about which bias is salient has not been reached. Herriges and Shogren (1996), for example, find evidence of anchoring. Controlling for anchoring in their analysis led to efficiency losses in their WTP estimate, relative to the single-bounded model. They further note that single-bounded models perform better, in the presence of significant anchoring effects.Whitehead (2002) estimates a random effects probit model, allowing for coefficient variation across the two sets of take-it-or-leave-it questions to control for anchoring, and includes a dummy variable for the second question to control for the shift effect. Controlling for these effects yielded a significant improvement in efficiency in his analysis. However, that gain may not obtain with another data set. Both Flachaire and Hollard (2006) and Chien et.al (2002) report evidence of anchoring, while only the former analysis also yielded significant efficiency gains in their WTP point estimates.

Overall, the literature does not provide consistent and robust evidence of anchoring, incentive incompatibility biases and efficiency gains. Of greater concern is that the literature related to anchoring and shifting has focused on developed countries, primarily the US. Therefore, generalizability of the hypothesis of behavioural anomalies associated with the double-bounded elicitation format has not yet been established in the context of developing country data. In particular, although we observe a steady growth in the CVM literature in developing countries, the focus has been on biases related to the CVM scenario and survey administration, rather than anchoring, shifting and framing effect biases. Specific examples include the valuation of water quality and sanitation improvements (Whittington al., 1988, 1990, 1993, Altaf et al., 1993; Singh et al., 1993), biodiversity and recreation (Sattout et al., 2007; Navrud and Mungatana, 1994 and Moran, 1994), health (Cahn et al., 2006; Cropper et al. 2004 and Whittington et al., 2003) and forestry (Lynam et al. 1994; Shyansundar et al. 1995; Mekonnen, 2000 and Köhlin, 2000). While these studies aim to provide useful policy information related to environmental interventions, they do not consider shift effects, anchoring effects or framing effects, although Köhlin (2001) and Carlsson et al. (2004) control for "yea-saying" or compliance bias. Therefore, the main contribution of this research is to provide empirical evidence related to shifting, anchoring and framing effects biases in a developing country setting, through the analysis of a double-bounded CVM survey related to community forestry programs in Ethiopia.

In this analysis, we applied a host of empirical strategies including interval censored data models, bivariate probit models and various random effects probit models to examine whether preference anomalies (incentive incompatibility and starting point biases) are observed. Moreover, we compared the parameter estimates of the latter models with that of single bounded-CVM model. Our data comes from a contingent valuation study of community plantations in selected rural villages in Ethiopia. The results show that significant incentive incompatibility effects and framing biases arise in our data. However, the hypothesis that peasant households anchor their willingness to pay to starting bids is rejected.

The analysis is laid out in the following way; Section 2.2 discusses theoretical and empirical specifications, section 2.3 describes the design of the contingent valuation experiment and data collection method, Section 2.4 presents the results of the analysis and Section 2.5 concludes the analysis.

2.2. THEORETICAL AND EMPIRICAL SPECIFICATIONS

Consider an individual, denoted by *i*, whose willingness-to-pay for a non-marketed good in log form is w_i, facing two take-it-or-leave-it survey questions related to their willingness-to-pay. As noted earlier, the double-bounded survey follows a two-stage process. In the first stage, an individual is offered an initial bid, to which she can respond either yes or no. In the second stage, depending upon the initial answer, the individual is offered a different bid, to which she can also answer either yes or no i.e. if the initial bid is accepted, then higher bid is offered in the second whereas if the initial bid rejected, then lower bid is offered in the second stage These survey values, referred to as bids, will be denoted, in log form, as b_{it}.

2.2.1 General Structure. As each individual is offered two separate bid opportunities, the simplest empirical strategy considers the combination of answers, ignoring the potential for anchoring and shifting. Defining the potential outcomes as $Y_{it} = \{0,1\} = \{\text{"no"}, \text{yes}\}$ yields $Y_i = \{Y_{i1}, Y_{i2}\}$, the observed outcomes for each individual. Assuming rationality, an individual does not agree to pay more than they are willing, the set of observed responses yields a set of intervals for estimating WTP. Mathematically, Y_i = (yes, yes) $\Leftrightarrow w_i \geq b_{i2}$, Y_i = (yes, no) $\Leftrightarrow b_{i1} \leq w_i < b_{i2}$, Y_i = (no, yes) $\Leftrightarrow b_{i1} > w_i \geq b_{i2}$, and Y_i = (no, no) $\Leftrightarrow w_i < b_{i2}$. As the purpose of CVM surveys is to elicit WTP, w_i is not observed; however, WTP can be constructed following the analysis. Furthermore, the inclusion of potential determinants for WTP is also possible.

The preceding structure, with a few assumptions, follows a bivariate probit model. Define $\mathbb{I}$ as an indicator function equal to one if the expression is true, and zero otherwise, such that $\mathbf{Y}_{it} = \mathbb{I}(\mathbf{w}_{it} > \mathbf{b}_{it})$.

Further, assume that the unobserved WTP can be written as $\mathbf{w}_{it} = \mathbf{X}_{it}\boldsymbol{\beta} + \mathbf{u}_{it}$, where $\mathbf{u}_{it} \sim N\left(0, \left((\sigma_1^2 \quad \rho\sigma_1\sigma_2), (\rho\sigma_1\sigma_2 \quad \sigma_2^2)\right)\right)$, $\mathbf{X}_{it}$ is a vector of explanatory variables described further, below, and β_t is a vector of parameters to be estimated[2]. Accordingly, if $\mathbf{w}_i = \mathbf{w}_{i1} = \mathbf{w}_{i2}$ and $\rho = 1$, observed differences are

[2] Cameron and Quiggin (1994) estimate a bivariate probit model, based on double-bounded CVM with the preceding assumptions, concluding that the independence assumption is violated in their survey.

due to randomness in the underlying distribution of the WTP. This restricted bivariate probit is equivalent to the interval model applied by Hanneman et al. (1991). It is also possible to restrict the model in other ways. For example, assuming that there is no correlation between the underlying error terms results in probit models that could be estimated either for each survey question, separately, or pooled across all survey questions.

2.2.2 Common Preference Anomalies. The literature offers several explanations for the divergence between single-bounded CVM and double-bounded CVM, some of which have been described above. These explanations, as alluded to previously, revolve around the proposition that the response to the second bid is not necessarily independent of the first bid.

2.2.2.1 Anchoring Effects. Intuitively, anchored preferences are an adjustment of prior beliefs regarding WTP, based on the initially proposed bid, and that adjustment yields a posterior WTP in the Bayesian tradition. That is, the initial offer may serve as an anchor, if the respondent assumes that the initial offer conveys information on the true value of the good (DeShazo, 2002). Respondents who are assigned ascending sequences interpret the follow-up bid as a lower weighted average bid, which increases the probability of accepting the follow-up bid. On other hand, respondents who are assigned a descending sequence may construe the follow-up bid as a higher weighted average bid, which decreases the probability of acceptance (Watson and Ryan, 2007, DeShazo, 2002). Therefore, if anchoring occurs, the middle interval is dependent on the relative strengths of effects in the upper and lower intervals.

Following Herriges and Shogren (1996), anchoring allows the individual's stated WTP to change over the survey, and be related to the initial bid.

$$w_{i2} = (1-\gamma)w_{i1} + \gamma b_{i1} \tag{1}$$

In equation (1), the posterior WTP is a weighted average of the prior WTP and the information provided by the initial bid, based on the weighting factor $\gamma \in [0,1]$, which is assumed constant. If the individual held (very) loose priors regarding her own WTP, the posterior WTP would be relatively more dependent upon the initial bid, and, vice versa.

2.2.2.2 Shifting Effects. Under shifting, an individual willing to pay the opening bid, may perceive the second bid as an unfair request to pay an additional sum; hence she will undercut her true WTP. In the same vein, for an individual, who rejected the first bid, the follow-up value could be interpreted as a lower quality good, leading to WTP reductions (Alberini et.al., 1997). Along these lines, shifting is modelled as a change in the WTP that is independent of the initial bid.

$$W_{i2} = W_{i1} + \delta \tag{2}$$

2.2.2.3 Anchoring and Shifting Effects. In the presence of shifting and anchoring, the posterior WTP is modified to account for the weighted average of the prior and the initial bid, as well as adjusted for the shift.

$$W_{i2} = (1-\gamma)W_{i1} + \gamma b_{i1} + \delta \tag{3}$$

Therefore, in the second stage, $Y_{i2} = \mathbb{I}\big((1-\gamma)W_{i1} + \gamma b_{i1} + \delta + u_{i2} > b_{i2}\big)$, and, in the first stage, $Y_{i1} = \mathbb{I}(W_{i1} + u_{i1} > b_{i1})$.

*2.2.3 Additional Preference Anomalies.*In addition to the common anomalies of shifting and anchoring, recent research has offered a more explicit description of effects, most which relate back to shifting and anchoring.

2.2.3.1 Framing Effect. From Kahneman and Tversky's (1979)prospect theory, DeShazo (2002) argues that initial approval by respondents can be interpreted as a reference point. Relative to this reference point, the follow-up question is framed negatively, and, thus, respondents are more likely to reject the second bid. However, respondents rejecting the first bid, such that they are subject to a descending bid sequence, are assumed not to form a reference point, which results in a different behavioural response, compared to respondents subject to ascending bid sequences DeShazo (2002), therefore, concludes that response inconsistencies or preference anomalies are only observable for respondents facing ascending iterative questions[3]. This conclusion further suggests that the double-bounded CVM model should only include descending follow-up question, in practice.

2.2.3.2 Strategic Behaviour Effects. Similar to framing effects are strategic behaviour effects, in the sense that they are both related to anchoring. With strategic behaviour, respondents may understate their WTP, in an effort to maximize their gain. Strategic behaviour arises, because the presence of a follow-up question signals price flexibility. If respondents understand the double-bounded CVM questionnaire, they may attempt to understate their true WTP, in an effort to

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[3]Flachaire and Hollard (2006) and Watson and Ryan (2007) provide some evidence of DeShazo's (2002) framing effects.

game the results (Carson, 1999, DeShazo, 2002). Similarly, the existence of a higher follow-up bid is likely to increase the probability of rejection, thus resulting indownward bias of reported WTP values (Watson and Ryan, 2007). However, the probability of approval of the follow-up bid will be higher if respondentsbelieve that their action is consequential in sense that it can induce the government to partake in provision of the goods

2.2.3.3 Cost Expectations Effects. In addition to anomalies related to anchoring, there are at least two associated with shifting. One such example is the cost expectations effect. Specifically, respondents may understand the first bid to be a fair representation of the actual cost of the good in question, such that the follow-up (higher) bid is seen as an attempt to obtain funding beyond what is necessary (Carson et al. 1999, DeShazo, 2002). Under these circumstances, approval, conditional on initial acceptance, is less likely than it otherwise would be (Watson and Ryan, 2007, Flachaire and Hollard, 2006, Alberini, 1997). On the other hand, the first bid could be understood to be information related to the quality of the good in question. Consequently, the respondent is more likely to reject the follow-up bid than she should be, conditional on rejecting the first bid (Alberini, 1997 and DeShazo, 2002). Cost expectation effects are, thus, similar to shifting effects, except that the shift parameter δ is always negative, suggesting a downward bias in the WTP (Whitehead, 2002, Flachaire and Hollard, 2006, DeShazo, 2002).

2.2.3.4 Yea-Saying Effects. Rather than perceiving the bids as information related to the good in question, respondents may, instead, feel that they should attempt to garner approval from the survey enumerator by agreeing. Yea-saying bias describes the tendency for respondents to accept any proposed bid. Under these circumstances, respondents overstate their true WTP in order to acknowledge the interviewer's proposition (Flachaire and Hollard, 2006, DeShazo, 2002), and it is often associated with ascending bid sequences (DeShazo, 2002, Watson and Ryan, 2007) rather than with descending bid sequences. The resulting upward bias in WTP is associated with a shift parameter δ that is always positive (DeShazo, 2002, Chein et.al, 2005 and Watson and Ryan, 2007). In other words, the yea-saying effect is the exact opposite of the cost expectation effect.

2.2.4 Implementation. The primary empirical strategy follows Whitehead (2002), whereby random effects probit models, exploiting the panel structure of double-bounded CVM data, are implemented. In the model, two observations are available for each individual, $Y_{it} = \mathbb{I}(X_{it}\beta + u_{it} > b_{it})$. The underlying unobserved component can be decomposed into an individual (random) effect α_i and an idiosyncratic effect, η_{it} giving rise to the general error term $u_{it} = \alpha_i + \eta_{it}$, where $\alpha_i \sim N(0, \sigma_\alpha^2)$, $\eta_{it} \sim N\left(0, \sigma_\eta^2\right)$ and $\mathcal{E}[\alpha_i X_{it}] = 0$, such that the variance of the unobserved error is $Var(u_{it}) = \sigma_\alpha^2 + \sigma_\eta^2$. Due to the common error component for each individual, that remains fixed across valuation questions but varies across individuals, the underlying unobserved error components are correlated, $\rho_\alpha = \frac{\sigma_\alpha^2}{\sigma_\alpha^2 + \sigma_\eta^2}$, which is defined as a fraction of the variance attributed to the individual specific effect, α_i .

This structure helps us discriminate between models assuming that the WTP remains constant across valuation questions and those that assume otherwise

(Haab and McConnel, 2002, Alberini et al., 1997). If a fraction of the variance attributed to the individual specific component, ρ_{α}, is zero, then correlation between the WTP error terms is one[4]. The difference between WTPs is thus, due to the random component η_{it}. The error component model (random effects models), therefore, collapses to what is known as interval-censored data model, a simple probit model estimated over pooled survey response data across valuation questions (Hanneman et.al., 1991). However, if a fraction of the variance attributable to individual specific components is non-zero, then error component models (random effect probit or logit model) (Alberini, 1997) or bivariate probit models (Cameron and Quiggin, 1994) could be used for estimation. The problem with the latter though is that two different estimates of WTP arise and we would not way know which one to use for program evaluation.

As alluded to in the preceding subsections, we implemented a range of empirical models in the analysis: unrestricted bivariate probit, restricted bivariate probit, an interval data model, probit models for single-bounded CVM response and random effects probit models. The restricted bivariate probit model imposes cross-equation parameter restrictions, such that the mean WTP underlying each response is identical $(\beta_o = \beta_1)$. However, unlike the interval data model, which assumes identical mean WTP as well as dispersion parameters $(\beta_o = \beta_1, \quad \sigma_0 = \sigma_1)$, the restricted bivariate probit model doesn't impose equality of the WTP dispersion

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[4]Note that the fraction of the variance attributable to randomness in the WTP, η_{it} is could be expressed as $\rho_{\eta} = \frac{\sigma_{\eta}^2}{(\sigma_{\alpha}^2 + \sigma_{\eta}^2)} = 1 - \frac{\sigma_{\alpha}^2}{(\sigma_{\alpha}^2 + \sigma_{\eta}^2)}$. It then follows no individual specific component (equivalently, $\sigma_{\alpha}^2 = 0$) implies that $\rho_{\eta} = 1$.

parameter $(\sigma_1 0 \neq \sigma_1 1)$. Therefore, the unrestricted bivariate probit model nests both the interval data model and the restricted bivariate probit model as special cases. In terms of the random effects probit model, a number of specifications are possible. The most general empirical specification to be considered allows for anchoring and shifting within the random effects specification. In the presence of anchor and shift effects, the WTP is defined as in (4).

$$w_t = \beta_0 + \Gamma z + \beta_t b_t + \delta(t-1) + \gamma(t-1)b_t \quad (4)$$

Given equation (4), for the second survey question, $w_2 = \beta_0 + \Gamma z + \beta_2 b_2 + \delta + \gamma b_2$; however, for the first survey question, $w_1 = \beta_0 + \Gamma z + \beta_1 b_1$. When neither shifting nor anchoring are assumed to be present, equation (4) reduces to $w_t = \beta_0 + \Gamma z + \beta_t b_t$. In the preceding specifications, z represents a vector of individual specific controls and Γ represents the vector of parameters to be estimated for those controls.

2.3. STUDY AREA, DESIGN AND DATA

For this analysis, a valuation exercise for WTP elicitation, related to the establishment of a community forest program, was conducted. The design follows the double-bounded CVM, and the survey was conducted in selected sites in Ethiopia. These sites were chosen, because the Ethiopian Federal Ministry of Agriculture, in collaboration with the World Bank, selected these sites for sustainable land management interventions. In these sites, as in most parts of rural Ethiopia, communities use common property woodlands for grazing and fuel wood collection. The areas selected are, according to the local Departments of

Agriculture, experiencing unprecedented deforestation, as well as increased demand for woody biomass. Households in these areas use cow dung and crop residues, which could be used, respectively, for fertilizer or fodder, as sources of energy and walk long distances to harvest fuel wood from natural woodlands.

Although Ethiopia has a long history of initiating and implementing community forestry programs, the experience has not generally been successful, and that lack of success is at least partly due to an approach that didn't accommodate the preferences of either the local community or the individuals slated for intervention (see chapter 3). Benin et.al. (2002), however, outline a more recent approach, which emphasizes local community involvement in resource conservation and management, which forms part of the incumbent government's rural development polices. This change in government behaviour has led to the establishment of area enclosures and plantations or woodlots, and these have been carried out in a more participatory fashion than before. Local Departments of Agriculture still identify the area to be enclosed or planted; however, the community members determine the operational rules associated with these community resources (Gebremedhin et al., 2003; Fekadu, 2008).

2.3.1 Survey and Bid Response. The CVM surveys included questions related to WTP for a proposed community plantation, as well as information on household socio-economic status. For the survey, 15 households from each of 40 sites, a total of 600 households, were randomly selected. A team of trained enumerators conducted the interviews. However, in order to conduct the CVM study, starting bids were necessary. Starting bids were obtained from a pilot study of 60 randomly selected households, in which an open-ended CVM question format was used. The result of the pilot study was a vector of five starting bids: 10, 20, 32, 50 and 80.

During data collection, the scenario was first described to the respondents. Following the description, value elicitation questions ensued. To make the scenario as realistic as possible, a suitable area of land for the establishment of the proposed community plantations was identified, and its size specified, for each survey site. Following the description, respondents were initially asked if they were willing to participate in the program[5]. For those willing to participate, they were further asked if they were willing to pay the initial – randomly assigned – bid. Regardless of whether the respondents were willing to pay the initial bid, a follow-up question was also asked of the respondent. Follow-up bids were either 50% of the initial bid, if the initial response was rejected, or 150% of the initial bid, if the initial response was accepted. Table 2.1 summarizes the bids and proportion of acceptance for each bid.

In order to capture inconsistencies, a final open-ended question, regarding the maximum willingness to pay, was asked of the participants. In cases where the open-ended value was lower than the approved bid in the follow-up question, respondents were asked to explain their decision. Following Carlsson et al. (2004), we recoded these inconsistent responses into a "no" response for the

[5]About 6.5% of the respondent protested in the sense that they aren't willing to participate. These responses are not included in the analysis

second bid. Köhlin (2001) argues that these inconsistencies are obtained when respondents want to conform to social norms, especially in cultures characterized by courtesy, collective decision-making or paternalistic decision-making.

2. 3.2 Additional Survey Data. As noted above, the survey included questions related to a number of socio-economic variables, including the sex of the respondent, the age and education (both in years) of the household head, the size of the household, the household's non-food expenditure, the household's ownership of livestock (measured in tropical livestock units, where 1TLU=250kg), a measure of forest access, based on a GIS data, distances to the nearest town, land holdings, measures of wealth (whether a household has corrugated metal on their house or not) and experimentally determined household rates of time preference. Descriptive statistics of this data are presented in Table 2.2.

We postulate that the demand for community forestry depends on covariates vindicated by economic theory. These include income and wealth, the price of the good, other prices and other taste shifters. From this list, covariates were sorted into three broad categories: (1) wealth and income – ownership of a house with corrugated roofing, land holdings and non-food expenditure; (2) the price of the good – livestock ownership, rate of time preference and education; (3) other prices – access to alternative forests, household size, and the distance to town.

Whereas proxies for wealth and income are relatively clear, variables used as price proxies merit further explanation. With regard to the price of the good, community forestry involves both temporal and intertemporal trade-offs. Starting with livestock, we expect that livestock has two opposing effects; wealth effect and prices effect. The wealth effect arises from the importance of livestock as major asset holding with the implication that the demand for community forestry increases with increased livestock wealth (more capacity to pay). In contrary, the establishment of community forestry on grazing land implies a potential income loss from livestock production, as grazing land is a major input of production. The foregone income is what we describe as the prices to be paid for community forestry establishment. We thus, expect that higher prices (higher holding livestock implying higher income to be given up) lowers demand for the goods under consideration. The net effect of livestock holding wills thus, either is negative or positive depends on the strength of either effect.

Moreover, given that community forestry establishment and management requires labour, income from alternative employment may have to be sacrificed, the value of which depends on the level of education. Therefore, such opportunity cost should be construed as part of the cost of establishing the community forest, in addition to the direct contribution suggested by the proposed bids. We, therefore, hypothesize that the level of education is expected to reduce the demand for community forestry.

Likewise, community forestry involves an intertemporal trade-off, in the sense that the benefits given up today to establish the programme must be weighed against benefits that accrue at later dates. We capture these intertemporal trade-offs through the household head's rate of time preference, assuming that this rate is inversely related to the demand for community forestry[6]. Moreover, we presume that time preferences are dependent on household wealth[7] measures (education, landholding and ownership of corrugated house etc.) and household head characteristics, such as age and sex.

With regard to proxies for other prices, recall that both access to alternative forests, typically open access natural forests, and the opportunity to buy from markets, as measured by the distance to town, are potential community forestry substitutes[8]. We, therefore, argue that better access to alternative forests and shorter distances to town will lower the prices of forest products obtained from these alternative sources. Subsequently, these measures are expected to be associated with reduced demand for community forestry. Moreover, the size of the household is likely to reduce WTP, partly because larger households have less discretionary income per capita and partly because a larger household increases

[6]The logic follows from the fact that community forestry establishment is an investment venture, the return of which realizes after sometimes, the shortest beingfive years for Eucalyptus species. This implies that an intertemporal rate of substitution (willingness to give up current consumption in favour future consumption) can be taken as the opportunity cost of next best alternative project instead of market interest rate because of imperfection of capital market in our study villages.

[7]Theoretical economic literature postulate that rate of time preferences depends on wealth level. This is best elucidated by the claim that the poor is short-sighted (myopia, impatient), in the sense that they have higher rate of time preference.

[8]Note that better access to alternative forest implies low cost of collecting forest products in terms of labour allocation. From non-separable households models framework, where a household is both producer and consumer of forest products, a situation that widely prevails in our study villages, it follows that, for a given household's demand schedule , say for fuel wood, better access to alternative forest results in downward shift of supply schedule(low marginal cost function). This in turn yields lower household specific equilibrium shadow price of the products considered (fuel wood in this example). It is this theory that informs our hypothesis of the inverse relationship between demand (WTP) for community forestry and access to alternative forests.

the supply of labour available for collecting forest products from open access forests.

2.4. EMPIRICAL RESULTS

In this section, we present the results of our empirical analysis, including tests for preference anomalies. Following these tests, we present the welfare results. Specifically, we present the valuation of the program's perceived welfare benefits based on a host of empirical strategies that include determinants of household WTP.

2.4.1 Testing for Preferences Anomalies. The bid-response data conform to a priori expectations, as informed by economic theory; the share of approvals generally falls as the bid rises (see Table 2.1). Moreover, from an analysis of the raw data, we found that some households chose to give lower WTP values in the open ended follow-up question than would be uncovered from the closed-end questions; 14.7% of respondents were inconsistent in this way. Köhlin (2001) offers several explanations regarding the sources of this inconsistency, which include yea-saying (or compliance bias), strategic behaviour and cultural bargaining experiences that might be triggered by the preference elicitation format. In our case, when asked to explain responses that were inconsistent, 2.5% of the subjects reported that they wanted to please the enumerator, 42.5% thought it was obligatory to report, 52.5% felt they were too poor and could not afford to pay, while 2.5% gave other reasons. According to these responses, 45% of the inconsistencies arose from "yea-saying" or compliance bias.

In what follows we test for incentive incompatibility bias, anchoring effect bias and framing effect bias by employing a range of empirical models. To that effect, we first fit restricted bivariate probit models, the interval data model and unrestricted bivariate probit models (see Table 2.3). The likelihood ratio test supported the hypothesis that the unrestricted bivariate probit model and restricted bivariate probit model fit the data better than the interval data model, $\chi^2 = 61.54, p = 0.00$ and $\chi^2 = 77.56, p = 0.001$, respectively. However, the unrestricted bivariate probit model is not an improvement over the restricted bivariate probit model, $\chi^2 = 16.04, p = 1$. In addition to these tests, we find that the error correlation deviates significantly from unity, $\rho = 0.528$ for the unrestricted bivariate probit and $\rho = 0.553$ for the restricted bivariate probit, supporting the hypothesis that WTP varies across the valuation questions. Equivalently, the results lend support to the claim that preference anomalies are present in the responses and that parameter estimates from standard double-bounded models are not appropriate for inference.

Moreover, via a likelihood ratio test, as was done in DeShazo (2002), we also tested whether parameter consistency holds across the two WTP equations for the ascending bid sequence subsample and descending bid sequence subsample. The result revealed that the null hypothesis of parameter consistency for the descending bid sequence subsample could not be rejected $(\chi^2 = 1.54, p = 0.67)$. In contrast, the null hypothesis of parameter consistency for the ascending bid subsample is rejected $(\chi^2 = 769.75, p = 0.00)$. When combined, these results lead us to reject the null hypothesis of no framing effects within the survey[9].

2.4.2 Controlling for Preferences Anomalies Given the preference anomalies observed in the preceding analysis, we also implemented a series of random effect probit models accounting for WTP variation and compared those results with that of a single-bound probit model and a simple random effects probit model. The random probit model is denoted as the naïve model, as it assumes equal WTP values across bid questions; hence, we don't account for anchoring, incentive effects or both. Comparing the single-bound and simple random effects probit models, we see that the latter yielded lower WTP point estimates, as well as lower standard errors. This finding supports Hanemann et al. (1991), who conclude that double-bounded models yield both lower point estimates and improved efficiency.

In what follows, we return to models that account for differences in WTP. In other words, we control for shift-effects and starting point biases (see Table 4). The shift effect is introduced as a dummy variable A to test whether willingness to pay differs across the valuation questions. This model is referred to as the shift effect model, hereafter. Our results point to both negative and statistically significant shift effects, suggesting that there is a negative shift effect following the first valuation question. The result is in line with Alberin et al. (1997) and Whitehead

[9]Note that these results also point to the presences of the yea-saying effect. However, as we have controlled for yea-saying problems, as we noted earlier, the test points to the presences of framing effects.

(2002). The negative sign implies that there is a downward shift in WTP, sometimes referred to as nay-saying (Chien and Shaw, 2005) as opposed to yea-saying. Equivalently, the result confirms that there is no yea-saying bias, partly because it has been controlled for in the analysis – inconsistent responses to open-ended follow-up questions were recoded.

The shift effect model was then altered to, instead, allow for anchoring. In the anchoring effect model, A_n is introduced to capture potential starting point bias. The results point to negative and significant anchoring effects. Although the absolute value of the coefficient lies in the unit interval, it implies a negative starting point effect, which violates the assumptions of the standard starting point bias model. Consequently, we cannot conclude that anchoring effects are present. Our conclusion is contrary to Chien et al. (2005), Whitehead (2002) and Flachaire and Hollard (2006), all of whom found evidence of anchoring bias in their data.

In case the anchoring effect inappropriately captures the framing effect, we accounted for the simultaneous presence of both shift effects and anchoring effects. This model is referred to as the shift-anchor model, hereafter. As with the shift model, the estimated shift effect is negative, implying a downward shift in WTP. Similarly, as with the anchor effect model, the anchoring coefficient remains negative. Moreover, the likelihood ratio test indicates that this model is not an improvement over the shift-effect model, $\chi^2 = 0.78, p = 0.382$. However, the likelihood ratio test also confirmed that all of these models outperform the simple random effects probit model and anchor model. Generally, the WTP point estimate is lower in all of the bias corrected models (shift-effect, anchoring effect and shift-anchor effect models) compared to the single-bound estimate.

Finally, the preceding random effects probit models were implemented for both the ascending bid sequence subsample and the descending bid sequence subsample, separately. In each of these subsamples, the shift-effect is present in the shift-effect and shift-anchor effects models. As before, we fail to detect evidence of anchoring effects in either of the subsamples for either the anchor-effect or the shift-anchor effects model.

2.4.3 Welfare and Estimation Efficiency. Although the existence of preference anomalies is interesting, on its own, the primary purpose of CVM is the elicitation of preferences. Preference anomalies should be controlled in the analysis, such that appropriate welfare estimates can be obtained. Upon calculation of the welfare effects, we found that the shift-effect model yielded the lowest median willingness to pay, ETB20.14, whereas the anchor-effect model and the shift-anchor model yielded slightly higher WTP estimates, ETB22.80 and ETB30.41, respectively[10]. However, WTP values for either the ascending or descending bid sequence subsamples were generally lower than for the full sample. As elucidated earlier, the likelihood ratio test results for model selection support the choice of the shift-effect and shift-anchor random effects probit models, although the shift-anchor effects model yields an inconsistent, with respect to theory, negative anchor effect. As such, we report the willingness to pay estimate for these models as our measure

[10]During survey time the exchange rate between USD and ETB was 13.8ETB/USD

of the community forestry welfare impact, which ranged between ETB20.14 and ETB22.80. However, our preferred estimate is the lower estimate of ETB20.14, as the higher estimate includes the inconsistent negative anchoring effect.

In addition to examining the welfare effect, efficiency is also relevant, given the fact that the double-bounded model has been shown to be more efficient. The welfare estimate, median WTP, is computed from the model parameters, and, hence its distribution depends on the distribution of the parameters. The Delta method is used to derive the standard errors of the welfare estimates (Greene 1997). On the basis of efficiency, as measured by the relative standard errors, all of the random-effects probit models (naïve, shift, anchoring, and shift and anchoring models) outperform the single-bounded models. Amongst the random-effects probit models, the shift-effect model yielded the lowest standard error estimate.

2.4.4 Welfare Determinants. Further analysis of the bid function allows for the identification of salient determinants of WTP. In the analysis, the parameters, which capture the link between socio-economic covariates and WTP, for the most part, accord with our a priori expectations. However, some do not, which led to additional investigation, discussed below. The results are reported in Table 2.5 and Table 2.6. In Table 2.5, the random-effects probit models with selected covariates are presented. One concern that arises in an analysis of this nature is that the model could suffer from endogeneity, arising from the relationship between the rate of time preference and the error term[11].

Along those lines, Davidson and MacKinnon's (1993) test rejected the hypothesis that the rate of time preference is exogenous $(\chi^2 = 05.31, p = 0.0212)$. Therefore, the models were extended to include IV methods. The rate of time preference was instrumented by household head age, gender, land holdings per capita and wealth variables. Following Davis and Kim (2002), instrument relevance based on Shea's (1997) partial r^2, revealed that the null hypothesis of no instrument relevance is rejected (r^2 =*0.027*). Therefore, IV results are further discussed. Once IV methods were applied, previously inconsistent, with expectations, parameter estimates were found to conform to our *a priori* expectations. For example, the rate of time preference estimate and the estimate for the measure of access to alternative forests were counter-intuitive – they were positive – in the uncorrected random effects model. Following correction, the signs changed, yielding negative results, which were consistent with our expectations.

As expected, the parameter on (logged) bids is negative and significant, supporting the claim that respondents are rational, when faced with increasing cost. In addition, the (logged) income effect is also positive and significant, implying that community forestry is a normal good. Livestock ownership effects were estimated to be negative and significant, suggesting that rural Ethiopian farmers believe there are significant opportunity costs, mostly in the form of reduced grazing land,

[11]We expect that there exist some unobserved household head's charactertics that is correlated both rate of time preference and willingness to pay in a bid function equation.

associated with community forestry. Household size, although not significant, is found to be positive, which was not expected. Possibly, larger households require more biomass, which may offset the effect of increased labour supply. Similarly, other price proxies – access to alternative forests and the distance to town, carry the expected signs, but are not significant. Finally, the rate of time preference is found to be an insignificant, but negative, determinant of WTP, as expected.

2.5. CONCLUSION

Single-bounded CVM has acclaimed desirable properties, such as incentive compatibility and survey implementation benefits; however, proper implementation requires a relatively large sample. The double-bounded contingent valuation method has been employed as an alternative method to improve efficiency, since it requires fewer survey respondents. However, it also suffers, *inter alia*, from biases resulting from a range of preference anomalies, including anchoring effects, incentive incompatibility (shift) biases and framing effect bias. Although several studies have tested for these biases, the majority of these studies have been undertaken in developed countries.

In this study, we applied a double-bounded contingent valuation format and tested for the aforementioned biases, employing a host of empirical strategies. Our data comes from a contingent valuation survey of community plantations in selected rural villages in Ethiopia. The analysis revealed that there are significant incentive incompatibility and framing effects in our data. However, the hypothesis that peasant households anchor their willingness to pay to the starting bid is rejected. Estimation of compensated variation, as a welfare measure, after controlling for the preference anomalies, showed that community forestry programs offer welfare gains of approximately ETB20.14 for this study's peasant households. Furthermore, controlling for shift effects and anchoring effects improved the statistical precision of the welfare estimate, a result that confirms a number of the developed country studies.

Moreover, analysis of bid functions found that household income, program establishment costs and livestock holdings are important determinants of WTP. The first of these suggests that community forestry is a normal good, while the effect of program establishment costs are consistent with the expectation that increased prices reduce demand. The last of these results points to opportunity costs related to foregone grazing land, land that would be required to establish the community forest. This result also implies that the establishment of community forestry, in livestock dependent and land-poor villages will be a welfare reducing proposition.

Overall, the result provides support to the furtherance of community forestry programs, as they offer significant, but economically small, welfare benefits to rural Ethiopian households, at least for the households in this study. Additionally, the failure to account for incentive incompatibility bias and framing effect biases yields a biased welfare estimate within the double-bounded contingent valuation method. Therefore, although such methods improve relative precision, care must be taken in their use in developing countries, as well as developed countries.

Table2.1. Descriptive statistics of Bid Vectors used for Double-Bounded CVM

B^l	*Proportion “yes”*	B	*Proportion “yes”*	B^u	*Proportion “yes”*
5	0.50	10	0.91	15	0.74
10	0.41	20	0.76	30	0.57
16	0.66	32	0.75	48	0.55
25	0.43	50	0.70	75	0.28
40	0.37	80	0.57	120	0.23

Source: Author’s analysis

Table2.2. Descriptive Statistics of survey data

Variable	*Description*	*Mean*	*Std.Dev*	*Minimum*	*Maximum*
density	Per-hectare biomass per-capita	0.25	0.50	0	3
tlu	Animal holdings (TLUs)	8.64	6.53	0	42
sex	=1 if respondent is male	0.89	0.30	0	1
age	Household head age	45.43	12.74	23	90
hhsize	Household size	6.48	2.42	1	15
yrsschool	Household head education	5.50	2.94	0	14
expenditure	Non-food expenditure/year	4184.1	5402.8	122	36500
Wealth	Corrugated house	0.4	0.490	0	1
lndszpc	Land holding per capita in hectare	0.817	0.972	0	4.961
Rtp	Rate of time preference	0.252	0.278	0	2
WTP	open-ended WTP	38.80	24.86	10	80
WTPa	Open-ended WTPa	55.129	40.157	10	240
WTPd	Open-ended WTPd	8.881	5.684	1	20

Source: Author's analysis

Note that WTPa and WTPd, respectively refers to open-ended willingness to pay by ascending bid and descending bid subsamples of doubled bounded CVM question

Table 2.3. Parameter estimates of simple probit model and bivariate model

VARIABLES	Single-bounded CVM model	Constrained biprobit model $(\beta_1 = \beta_2)$ (all observation)	Unconstrained bivariate probit (all observation)		Constrained biprobit model $\beta_1 = \beta_2$ ascending-bid subsample	Unconstrained bivariate probit (ascending-bid subsample)		Constrained biprobit model $\beta_1 = \beta_2$ descending bid subsample	Unconstrained bivariate probit (descending-bid subsample)	
lnbid1	-0.023*** (0. 003)	-0.012*** (0.0541)	-0.529*** (0.0857)		-0.501*** (0.0916)	-0.109 (0.273)		-0.132 (0.150)	-0.245 (0.430)	
logincome	0.00037* (0.00217)	0.013 (0.0374)	0.002* (0.006)	0.009 (0.004)	0.006 (0.0056)	0.021 (0.1609)	0.004 (0.0058)	0.171* (0.0987)	0.035 (0.2560)	0.215* (0.1102)
lnbid2				-0.573*** (0.0814)						
lnbidh							-0.540** (0.0953)			
lnbidl										-0.208 (0.166)
Constant	1.823*** (0. 45)	1.091*** (0.3310)	0.834 (0.5121)	1.635*** (0.0.4440)	4.257*** (0.398)	2.985*** (0.968)	2.577*** (0.372)	-1.692*** (0.598)	-2.881* (1.682)	0.316 (0.510)
Log-likelihood	-298.75	-637.942	-645.952	-645.952	. -630.579	-245.703	-245.703	-114.252	-113.483	-113.483
rho		0.553	0.528	0.528	0.616	0.139	0.132	0.114	-0.047	-0.047
Observations		550	550	550	408	408	408	145	145	145
WTP	80.52	89.296	84.043	69.621	285.004	1023.83	106.260	210.526	152.641	2.349
	(8.65)	(8.2788)	(9.5288)	(6.7376)	(47.2462)	(423.7290)	(12.1472)	(181.5470)	(111.3960)	(21.1578)

Source: Author's analysis, Standard errors in parentheses*** p<0.01, ** p<0.05, * p<0.1

Table2.4. Parameter estimates of random-effect probit models without covariates (robust se in parenthesis)

VARIABLES	Interval-data	Naïve	Shift	Anchoring	Shift-anchoring	Naïve		shift		Anchoring		Shift-anchoring	
						ascending	descending	ascending	descending	ascending	descending	ascending	descending
lnbid	-0.381***	-0.579***	-0.575***	-0.591***	-0.578***	-0.262***	-0.973***	-0.260***	-0.976***	-0.264***	-0.977***	-0.261***	-0.979***
	(0.0544)	(0.0920)	(0.0930)	(0.0928)	(0.0932)	(0.0875)	(0.120)	(0.0875)	(0.120)	(0.0875)	(0.120)	(0.0876)	(0.121)
lnincome	0.013***	0. 002*	0. 002*	0.002*	0.002	0.002	0.004*	0.002	0.003*	0.002	0.003*	0.002	0.003*
	(0.0372)	(0. 0014)	(0. 0014)	(0. 0014)	(0. 0014)	(0.0013)	(0.0017)	(0.0013)	(0.0017)	(0.0013)	(0.0017)	(0.0014)	(0.0017)
A			-0.479***		-0.358*			-0.976***	-3.190***			-0.953***	-3.022***
			(0.1340)		(0.1930)			(0.130)	(0.179)			(0.186)	(0.270)
An				-0.080***	-0.003					-0.00163	-0.00523	-0.000616	-0.00448
				(0.0048)	(0.0036)					(0.00355)	(0.00555)	(0.00347)	(0.00552)
Constant	0.782**	2.701***	2.930***	2.898***	2.942***	1.368***	2.741**	1.851***	4.343***	1.404***	2.853**	1.853***	4.356***
	(0.3315)	(0.374)	(0.345)	(0.344)	(0.346)	(0.463)	(1.202)	(0.320)	(0.449)	(0.454)	(1.143)	(0.321)	(0.449)
Observations		1,086	1,086	1,086	1,086	1,086	1,086	1,086	1,086	1,086	1,086	1,086	1,086
Log-likelihood		-656.91	-654.05	-655.40	-653.66	-689.81	-483.39	-685.47	-477.16	-689.71	-482.93	-685.45	-476.83
WTP		25.71	20.14	30.41	22.82	14.98	15.93	9.21	3.24		17.65	9.42	3.86
		(6.31)	(4.053)	(6.054)	(6.57)	(5.26)	(18.57)	(0.98)	(0.95)	15.42	(19.50)	(1.57)	(1.39)
										(5.17)			

Source: Author's analysis, Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10

Table 2.5. Parameter estimates of random-effect probit model with covariates (robust se in parenthesis)

VARIABLES	*Naïve*	*shift*	*Anchor*	*Shift-anchor*	*Shift-ascending*	*Shift-descending*	*Anchor-ascending*	*Anchor-descending*	*Shift-anchor ascending*	*Shift-anchor descending*
A		-0.545**			-0.992***	-3.926***			-1.066***	-4.163***
		(0.226)		-0.711**	(0.220)	(0.314)			(0.304)	(0.500)
An			-0.00651	(0.0417)			-0.000442	0.00444	0.00201	0.00626
			(0.00771)	0.260**			(0.00601)	(0.00997)	(0.00572)	(0.00987)
lntotexp	0.292**	0.294**	0.292**	(0.315)	0.356***	0.172	0.355***	0.169	0.357***	0.170
	(0.130)	(0.131)	(0.130)	0.00452	(0.127)	(0.158)	(0.126)	(0.157)	(0.127)	(0.158)
lnbid	-0.648***	-0.633***	0.647***	(0.00604)	-0.360**	-0.968***	-0.366**	-0.970***	-0.364**	-0.979***
	(0.166)	(0.166)	(0.167)	0.295**	(0.155)	(0.220)	(0.155)	(0.220)	(0.155)	(0.221)
tlu	-0.0114	-0.0115	-0.0115	(0.131)	-0.0101	-0.0112	-0.0101	-0.0109	-0.00996	-0.0108
	(0.00730)	(0.00731)	(0.00734)	-0.643***	(0.00626)	(0.0119)	(0.00627)	(0.0116)	(0.00624)	(0.0116)
hhsz	0.108**	0.109**	0.109**	(0.167)	0.116**	0.0381	0.116**	0.0375	0.116**	0.0375
	(0.0544)	(0.0545)	(0.0544)	-0.0111	(0.0523)	(0.0662)	(0.0522)	(0.0659)	(0.0524)	(0.0662)
dstwn	0.000154	0.000152	0.000153	(0.0546)	-0.000246	0.000416	-0.000245	0.000432	-0.000239	0.000446
	(0.00145)	(0.00145)	(0.00145)	0.241	(0.00143)	(0.00184)	(0.00143)	(0.00184)	(0.00143)	(0.00184)
fdensity	0.00431	0.00444	0.00435	(0.249)	0.132	0.0618	0.132	0.0597	0.132	0.0589
	(0.288)	(0.289)	(0.288)	0.000164	(0.284)	(0.361)	(0.283)	(0.360)	(0.284)	(0.361)
rtp	0.403	0.406	0.400	(0.00145)	0.162	0.560	0.159	0.563	0.169	0.571
	(0.375)	(0.376)	(0.376)	0.00408	(0.352)	(0.481)	(0.352)	(0.481)	(0.353)	(0.485)
age	-0.0214	-0.0217	-0.0216	(0.289)	-0.0248*	-0.00838	-0.0248*	-0.00804	-0.0245*	-0.00798
	(0.0137)	(0.0137)	(0.0138)	0.424	(0.0133)	(0.0172)	(0.0133)	(0.0172)	(0.0133)	(0.0172)
edu	0.0166	0.0164	0.0164	(0.377)	-0.00879	0.0531	-0.00877	0.0540	-0.00798	0.0549
	(0.0415)	(0.0416)	(0.0416)	-0.0208	(0.0399)	(0.0528)	(0.0399)	(0.0527)	(0.0400)	(0.0530)
	(0.121)	(0.121)	(0.121)	0.0180	(0.118)	(0.151)	(0.118)	(0.151)	(0.118)	(0.151)
Constant	0.890	1.104	0.908	(0.121)	0.0499	3.007*	-0.406	0.969	0.0403	3.030*
	(1.354)	(1.345)	(1.370)	1.081	(1.284)	(1.690)	(1.332)	(2.216)	(1.284)	(1.690)
				(1.346)						
Log-likelihood	-246.893	-244.797	-246.889	-244.515	-258.081	-170.503	-261.439	-175.960	-258.019	-170.306
Observation	928	928	928	928	928	928	928	928	928	928

Source: Author's analysis , Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10

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Table 2.6. Parameter estimates of random-effect IV-probit model with covariates (robust se in parenthesis)

VARIABLES	*Naïve*	*Shift*	*Anchoring Random effect*	*Anchoring Fixed effect*	*Shift-anchoring*	*Shift-ascending*	*Shift-descending*	*Anchoring-ascending*	*Anchoring-descending*	*Shift-anchor-ascending*	*Shift-anchor-descending*
rtp	-1.294	-1.319	-1.373	-1.349	-1.349	-2.034	-0.0126	-2.145	-0.238	-2.099	-0.0231
	(1.098)	(1.106)	(1.151)	(1.156)	(1.156)	(1.406)	(0.632)	(1.488)	(0.808)	(1.484)	(0.654)
A		-0.0963*			-0.0792	-0.192***	-0.690***			-0.135	-0.707***
		(0.0559)			(0.0911)	(0.0711)	(0.0320)			(0.117)	(0.0515)
An			-0.00161	-0.000466	-0.000466			-0.00351**	-0.00981***	-0.00155	0.000458
			(0.00126)	(0.00198)	(0.00198)			(0.00163)	(0.000884)	(0.00254)	(0.00112)
lntotexp	0.0584*	0.0582*	0.0586*	0.0584*	0.0584*	0.0743*	0.0237	0.0750*	0.0257	0.0746*	0.0237
	(0.0315)	(0.0317)	(0.0323)	(0.0321)	(0.0321)	(0.0403)	(0.0181)	(0.0418)	(0.0227)	(0.0412)	(0.0181)
lnbid	-0.109***	-0.0996**	-0.1000**	-0.0985**	-0.0985**	-0.0539	-0.103***	-0.0532	-0.116***	-0.0507	-0.104***
	(0.0394)	(0.0401)	(0.0418)	(0.0412)	(0.0412)	(0.0510)	(0.0229)	(0.0540)	(0.0293)	(0.0530)	(0.0233)
tlu	-0.00277*	-0.00280*	-0.00292*	-0.00285*	-0.00285*	-0.00300	-0.00117	-0.00327	-0.00185	-0.00314	-0.00113
	(0.00156)	(0.00157)	(0.00162)	(0.00163)	(0.00163)	(0.00199)	(0.000896)	(0.00210)	(0.00114)	(0.00210)	(0.000923)
hhsz	0.0130	0.0131	0.0133	0.0131	0.0131	0.0138	0.00313	0.0143	0.00434	0.0140	0.00307
	(0.0125)	(0.0125)	(0.0128)	(0.0127)	(0.0127)	(0.0160)	(0.00717)	(0.0165)	(0.00898)	(0.0163)	(0.00717)
dstwn	0.000318	0.000321	0.000328	0.000326	0.000326	0.000383	7.81e-05	0.000396	0.000106	0.000392	7.76e-05
	(0.000453)	(0.000456)	(0.000467)	(0.000464)	(0.000464)	(0.000580)	(0.000261)	(0.000604)	(0.000328)	(0.000596)	(0.000262)
fdensity	-0.0677	-0.0696	-0.0722	-0.0714	-0.0714	-0.0837	-0.00349	-0.0888	-0.00635	-0.0872	-0.00385
	(0.106)	(0.107)	(0.110)	(0.110)	(0.110)	(0.136)	(0.0612)	(0.142)	(0.0772)	(0.141)	(0.0619)
Constant	0.732**	0.751**	0.744**	0.712**	0.752**	0.594	0.909***	0.582	0.844***	0.595	0.909***
	(0.325)	(0.327)	(0.334)	(0.338)	(0.331)	(0.416)	(0.187)	(0.432)	(0.235)	(0.425)	(0.187)
Observations	928	928	928	928	928	928		928			928
						928		928	928		

Source: Author's analysis, Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10