ECONOMETRIC ANALYSIS OF THE WELFARE EFFECTS OF COMMON PROPERTY RIGHT FORESTRY PROGRAMS

by

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Study Leader: Professor Steven F Koch

Co-leader: Professor James N Blignaut

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Dedication

To my late mother Dabo Edema- for her irreplaceable love and caring

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Declaration

I declare that this thesis I hereby submit for the degree of PhD in Economics at University of Pretoria is entirely my own work and has not been submitted anywhere else for the award of a degree or otherwise.

Signed:……………………………………………..

Name : Dambala Gelo Kutela

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Promoter: Professor Steven F Koch

Department: Economics

Degree: PhD

Abstract

This thesis proposes to empirically evaluate fundamental welfare outcomes associated with common property rights forestry. The inferences made were based on data collected from selected villages in rural Ethiopia, where common property forestry programs are being run or are planned. The thesis comprises of three separate analysis chapters. The first of these analysis chapters engaged with the estimation of compensating variation, for community forestry intervention, using double-bounded contingent valuation methods while controlling for biases arising from anomalous preference revelation. The second analysis chapter aimed to identify salient community forestry program attributes that are preferred by potential program participants, estimate welfare effects and test preference heterogeneity for each of the selected attributes. The third analysis chapter aimed to estimate average treatment effects associated with the implementation of natural forest management decentralization, paying particular attention to identification issues.

The results from the first analysis 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, and, therefore, researchers should control for these anomalies. From the second analysis chapter, the welfare gain offered by community forestry was found to hinge largely on the proposed attributes of the program, such as the type of forest, area enclosure and type of land upon which the forest was to be situated. Moreover, the results pointed to significant differences in attribute preferences across the study population. In the third analysis chapter, after controlling for selection bias and treatment-effect heterogeneity associated with program participation, forest management decentralization programs were found to increase the average welfare of participant households between 19.96% and 33.63%. The results support the claim that common property right forestry management can be used to revive rural development and provide incentives for environmental protection, the latter of which has been uncovered in related research.<p>

Lead Promoter: Professor Steven F Koch

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Table of content

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List of tables

List of appendix

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.

[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

[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 $\mathbf{u}_{it} \sim \mathrm{N}\left(0, \left((\sigma_1^2 \quad \rho\sigma_1\sigma_2), (\rho\sigma_1\sigma_2 \quad \sigma_2^2)\right)\right)$, $\mathbf{X}_{it}$ 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

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[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.

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

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.

$$W_{i2} = W_{i1} + \delta \qquad (2)$$

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.

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

Therefore, in the second stage, $Y_{i2} = \mathbb{I}\left((1-\gamma)w_{i1} + \gamma b_{i1} + \delta + u_{i2} > b_{i2}\right)$, 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

[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, $\rho_\alpha = \dfrac{\sigma_\alpha^2}{\sigma_\alpha^2 + \sigma_\eta^2}$, 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

[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 $(\sigma_{\downarrow}0 \neq \sigma_{\downarrow}1)$. 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).

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

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

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[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

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

Chapter 3

Does One Size Fit All? Heterogeneity in the Valuation of Community Forestry Programs

Abstract

Through the implementation of a choice experiment valuation exercise, this study set out to identify the set of community plantation attributes that impact the welfare of potential community forestry program participants. We employed a combination of choice models to evaluate the preferences, welfare impacts and choice elasticities associated with alternative community forestry programs, allowing for different assumptions regarding heterogeneity. In line with economic theory, increased participation costs reduce the demand for community forestry, while increases in expected productivity raise the demand. With respect to preferences for the other alternatives considered: type of forest, area enclosure and type of land upon which the forest was to be situated, the results point to significant differences in preferences across the study population, suggesting that programs should be tailored to the communities in which the program is to be implemented.

Keywords: community forestry, choice experiment, conditional logit, random parameters logit and latent class model

3.1. Introduction

Deforestation presents several pressing problems in developing, as well as emerging countries, including energy shortages, due to reduced fuel wood supplies, and reductions in agricultural production, due to soil erosion (Köhlin, 1998 and Angelsen and Koimowitz, 1999), although soil erosion depends upon a number of other factors, such as soil type, rain and agricultural practices. Each of these pressing problems results in further household level adaptations and has additional detrimental effects on agricultural and non-agricultural household production. First, reduced fuel wood availability increases fuel wood collection efforts, implying less time for other activities. Second, energy shortages result in the substitution of crop residues and animal dung for fuel wood and limits crop production, as these materials could otherwise be used for soil fertility management (Mekonnen, 1999).

A range of policy responses to counteract deforestation and deal with its effects has been observed in different countries (Köhlin and Amacher, 2005). Demand-side interventions include the dissemination of improved cook stoves and the subsidization of commercial fuel sources, while supply-side policy instruments focus primarily upon the expansion and improved production capacity of forests (Cooke-St. Clair et al., 2008). Tree planting in community plantations and woodlots (Mekonnen, 2000; Gebremedhin et al., 2003; Carlsson et al. 2004; Köhlin and Amacher, 2005 and Cooke-St. Clair et al., 2008) and the creation of area enclosures within communities (Shylendra 2002, Tefera et al., 2005, Babulo, 2007 and Mekuria et al., 2010) are important supply-side interventions.

This research focuses on supply-side interventions, paying particular attention to the attributes of community forestry. Community forestry is expected to improve forest cover, with a concomitant increase in the supply of fuel wood, which is expected to yield a number of direct and indirect benefits for the local community. First, by reducing the use of crop residues and animal dung for fuel, these programs are expected to increase agricultural production, partly because forests are an important source of livestock fodder (Shylendra 2002; Jagger and Pender, 2003; Tefera et al., 2005; Babulo, 2007 and Mekuria et al., 2010). Second, community forests, if properly monitored, offer an alternative to communal grazing arrangements, which, due to open access, often result in further degradation[1]. Third, by virtue of being a substitute resource for open-access natural forests, community plantations can reduce pressure on open-access resources (Linde-Rahr, 2003). Fourth, community forests are often located closer to villages, such that their use is expected to unleash labour for other purposes. Fifth, the increased supply of fuel wood implies lower fuel wood prices. Finally, community plantations, if designed with multipurpose tree species and area enclosure, offer environmental protection services, such as soil and water conservation, by way of reducing soil erosion and downstream siltation (Mekonnen, 2000; Shylendra 2002; Tefera et al., 2005; Babulo, 2007 and Mekuria et al., 2010).

Although there are many potential benefits, the successful implementation of community forestry programs is limited by a host of internal and external factors. External forces include decentralization reforms and market development shocks related to the village economy (Sikor, 2005). In many developing and emerging

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[1] We are here referring to a type of grazing land under joint use by villagers, without enforceable access rule and use (harvest) rule.

economies, community forestry programs have often arisen as the outcome of a broader decentralization process associated with promoting public service performance and rural development (Agrawal and Ostrom, 2001; Larson and Ribot, 2004 and Ezzine-de-Blas et al., 2011). However, the decentralization of natural resources management has been plagued by state and local conflicts, to the detriment of such programs (Ezzine-de-Blas et al., 2011). Such conflicts yield incomplete property rights transfers to the local community, and, hence, incomplete decentralization of resource management attenuating local participation incentives (Larson and Ribot, 2004). In the same vein, community forestry is bound to influence market development (Sikor, 2005 and Richard, 1997). The integration of village economies into national and regional markets generates heterogeneous incentive structures among villagers and undermines collective action, which could further weaken community forest management (Ostrom, 1999). Additional research has outlined a set of community and resource factors affecting collective action sustainability (Ostrom, 1999 and Sikor, 2005). One such factor is group member heterogeneity; another relates to local power structures. The efforts of local elites to capture program outcomes, as described in Adhikari et al. (2004), may attenuate collective action, resulting in increased free-riding (Ostrom, 1999).

In line with the preceding concerns, Gebremedhin et al. (2003), for example, argue that community programs in Ethiopia have often failed, because the views of the community have been ignored during the design and implementation phases. Given thatthese programs compete for both land and monetary resources, it is in the interest of policymakers, program implementers and donors to quantify the contributions of these programs towards household welfare, and uncover evidence of the potential for such programs to positively affect rural development and protect the environment.

The quantification of household valuations of community forestry programs has proceeded along a number of lines. Köhlin and Amacher's (2005) selection model relies on revealed preference data to estimate the welfare effects of community forest plantations in terms of the value of decreased fuel wood collection times that such plantations offer. Cost Benefit Analyses (CBA), of which there are many, find that the return to community woodlots (Jagger and Pender, 2003) and area enclosures (Babulo, 2007 and Mekuria et al., 2010) are substantial in Tigray, northern Ethiopia. Mekonnen (2000) and Carlsson et al. (2004) apply Contingent Valuation Methodology (CVM) to estimate willingness to pay (WTP) and examine its determinants with respect to community woodlots that are financed, managed and used by different communities in Ethiopia. Köhlin (2001), also using CVM, estimates the WTP for community forestry in Orissa, India. Similarly, Riera and Mogas (2004), Brey et al. (2007) and Wang et al. (2007) apply CVM to recreational and conservation attributes, while Qin et al. (2009) examine contractual relations, especially private property rights, associated with forestry management. 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, because they only allow for the estimation of single attributes within multi-attribute programs (Carlsson et al., 2003).

Such limitations, however, can be alleviated through the application of choice experiments (CE), which include multiple attributes and are, thus, capable of allowing for the estimation of the value of each of the attributes, as well as the program's welfare effects. Batsell and Louviere (1991) and Louviere (1991) have employed experiments of this nature, which are common in marketing, geography and transportation economics. Recently, these valuation methods have received more attention in environmental economics, including the valuation of wetland management and biodiversity (Boxall and Adamowicz, 2002; Carlsson et al., 2003 and Milon and Scrogin, 2006), as well as forestry (Hanley et al., 1998; Riera and Mogas, 2004, Mogas et al., 2006 and 2009; Brey et al., 2007; Wang et al., 2007 and Qin et al., 2009). However, with the exception of Arifin et al. (2009), who examine community forestry in Indonesia, there is a dearth of literature involving the valuation of community forestry program attributes that are typically relevant to peasant farm household preferences and welfare.

The motivation of the present study owes to this paucity in the literature. This study applies CE to evaluate the welfare effects of community forestry program attributes in Ethiopia, paying particular attention to peasant farmers' preferences. Importantly, community forestry is not a single attribute program; several design options are available and each of these has different benefits for different types of households, such that households are presumed to value each of the attributes differently. Therefore, we estimate WTP for selected attributes of community forest plantations that are financed, managed and used by the communities. In addition to estimating individual WTP for different attributes of a community forest, we also seek to identify sources of heterogeneity that can affect preferences. Moreover, the study provides more information than is available via standard CVM and CBA studies of community plantations. It quantifies peasant trade-offs over plantation attributes, with implications for the design of community forestry interventions. It also evaluates the welfare impact of various types of forests, rather than just one type. The study also contributes to the small, but growing literature on the application of CE to evaluate environmental policy instruments in developing and emerging countries.

In this study, a combination of empirical strategies is employed, including conditional logit (CL), random parameters logit (RPL) and latent class models (LCM). These different strategies account for some of the limitations of standard choice models, such as Independence of Irrelevant Alternatives (IIA) in the traditional conditional logit model and restrictive parameter distribution assumptions associated with the random parameters logit model. Two major findings emerged from this study. First, the community forestry attributes considered offer substantial welfare benefits; particularly, the development of community forestry on village wastelands improves average household welfare. Second, preferences for community forestry plantations are heterogeneous amongst individuals and across groups of peasant farmers, such that community plantation interventions yield varying welfare impacts.

The remainder of this chapter is organized as follows. Section 3.2 discusses the economic and econometric framework. Section 3.3 describes the design of the choice experiment and data collection methods. Section 3.4 presents the results of the empirical analysis and Section 3.5 concludes and discusses policy implications generated from the study.

3.2. Theoretical framework

3.2.1 Economic Model. CEs are based on Lancaster's (1966) characteristics theory of value; a consumer's utility is a composite of the utilities for the underlying characteristics of the goods consumed. Given that environmental services contain a number of underlying characteristics, CE offers a useful representation (Hanley et al., 1998, 2001). The representation that we follow is due to Hannemann (1984) and Alpizar et al. (2001), who specify the economic model that underpins the behavioural aspects of our choice experiment. Therefore, we assume that our peasants maximize utility by choosing the program $i \in \{1, \dots, N\}$ with the greatest attribute benefit, subject to its cost, outlined in (1) and (2).

$$\max_{\delta} U[\delta_1 c_1(A_1), \dots, \delta_N c_N(A_N), z] \quad (1)$$

$$y = \sum_{i=1}^{N} \delta_i p_i + z$$

subject to: $\delta_i \delta_j = 0, \forall i \neq j$ (2)

$$z \geq 0, \delta_i = \{0,1\} \forall i$$

In equations (1) and (2), $U[\cdot]$ is a quasi-concave utility function; $c_i(A_i)$ is the composite of alternative *i*, which is a function of generic and alternative specific attributes, given by the vector A_i; δ_i is a binary indicator equal to one if alternative *i* is chosen; p_i is the cost of the alternative; *y* is income and *z* is the composite bundle of ordinary goods with its price normalized to unity, such that its value is equal to income net of the cost of the chosen alternative. Therefore, $z = y - p_k$, if alternative *k* is chosen. Other important properties that follow from (1) are: $U_j = 0$ if $\delta_j = 0$

(where U_j is the marginal utility of choice *j*), and $V_k(A_k, y, p_k) = U[c_k(A_k), y - p_k]$ is the indirect utility of the chosen alternative (Alpizar et al., 2001). An individual chooses alternative *k*, if the indirect utility associated with choice *k* exceeds the indirect utility of any other alternative.

$$V_k(A_k, y, p_k) \geq V_j\left(A_j, y, p_j\right), \forall k \neq j \tag{3}$$

In what follows, we derive a number of econometric specifications that can be derived from equation (3) with respect to our CE study.

3.2.2 Econometric Specification. In CE field settings, individual choices aren't completely deterministic, as suggested by (3); instead, choices are affected by alternatives that are not included in the experiment and by other unobservable individual characteristics (Hannemann and Kanninen, 1996). McFadden (1974) made use of this intuition in developing the random utility model, which was accomplished through the inclusion of an error term in (3).

$$V_k(A_k, y, p_k; \varepsilon_k) \geq V_j\left(A_j, y, p_j; \varepsilon_j\right), \forall k \neq j \tag{4}$$

For simplicity, the indirect utility function is assumed to follow a standard linear regression framework, whereby the error term is appended to a linear-in-parameters function of the observables. This functional specification leads to a conditional logit model, which is often used to model discrete choice behavior under the random utility framework (Train, 1998 and Greene and Hensher, 2003). However, CL is underpinned by IIA, and, therefore, fails to account for unobserved heterogeneity and potential correlation between available choices. Given this limitation, more flexible approaches are desirable. One such extension is the RPL; another is the LCM. In this study, in addition to the base CL methodology, we apply both RPL and LCM to analyze preferences related to the establishment of community forestry programs. The

RPL generalizes the CL by allowing coefficients to vary randomly over individuals (Train, 1998), and, therefore, the model relaxes IIA and can represent any substitution pattern. Furthermore, the RPL explicitly accounts for unobserved heterogeneity (Train, 1998; Carlsson et al., 2003). The LCM, on the other hand, allows coefficients to vary across subgroups of the population and also relaxes IIA.

In our model, we assume a linear-in-parameters specification for indirect utility, where we denote the individual with subscript *q*, the choice with *i*, and the choice set with *t*[2].

$$V_{iqt} = \alpha_{iq} + \gamma_i s_q + \beta_q x_{iqt} + \varepsilon_{iqt} \tag{5}$$

In (5), α_{iq} is the individual alternative-specific intercept that captures the intrinsic preference for the alternative, γ_i captures systematic preference heterogeneity related to socioeconomic characteristics, where s_q is a vector of socio-economic characteristics; β_q captures systematic preference heterogeneity related to program attributes, x_{iqt} is the vector of attributes (including costs) for alternative *i* and ε_{iqt} is stochastic accounting for observational deficiencies, due to unobservable components in the model that are assumed to be uncorrelated with the observed components.

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[2]In the choice experiment, each individual is asked to choose amongst three different alternatives on four separate occasions; therefore, we observe each individual four different times.

3.2.2.1 Conditional Logit. Under the appropriate error term assumption, (5) is a logit model with both alternative-varying and alternative-invariant regressors. As such, it constitutes a mixed logit model. However, following the literature, we restrict $\gamma_i = \gamma$ and $\alpha_{iq} = \alpha$, since the real interest is in attribute preferences, rather than alternative-specific effects[3]. Assuming $\beta_q = \beta$ and that each of the errors is identically and independently distributed (IID) type 1 extreme value, a CL can be estimated based on the following probability model.

$$P_q(i) = \frac{\exp\left(\beta x_{iqt} + \alpha + \gamma s_q\right)}{\Sigma_{i=1}^{N} \exp\left(\beta x_{iqt} + \alpha + \gamma s_q\right)} \quad (6)$$

Estimation can proceed with data that is pooled over all of the choice experiments.

3.2.2.2 Random Parameters Logit. The RPL, on the other hand, extends the CL, by allowing the coefficient vector β_q, to vary across the population according to the density $f(\beta|\theta)$, where θ is a vector of the parameters of the distribution, while the CL assumes that the preceding density is degenerate. Assuming the error terms are IID type 1 extreme value, an RPL (Train, 1998) can be specified. Following Carlsson et al. (2003), the conditional probability of alternative i for individual q in choice situation t can be specified.

$$P_q\left(it \middle| s_q, x_{iqt}, \beta_q\right) = \frac{\exp\left(\beta_q x_{iqt} + \alpha + \gamma s_q\right)}{\Sigma_{i=1}^{N} \exp\left(\beta_q x_{iqt} + \alpha + \gamma s_q\right)} \quad (6)$$

One of the maintained assumptions in the RPL inherent in (6) is that individual utilities vary, but are stable across the different choice experiments (Train, 1999).

[3]A more general model allowing for alternative-specific effects has been estimated, but is not reported here. Importantly, the alternative-specific effects do not change the underlying conclusions.

Given (6), the conditional probability of observing a sequence of choices is simply the product of the individual choice probabilities for each choice set. Denoting $j(q,t)$ as the sequence of choices from all of the experiments, the conditional probability can be written, as in (7).

$$\pi_q \left(j(q,t) \middle| s_q, x_{iqt}, \beta_q\right) = \prod_{t=1}^{T} P_q \left(it \middle| s_q, x_{iqt}, \beta_q\right) \tag{7}$$

Due to the variation in the β_q parameter vector, the preceding conditional probability needs to be integrated over the assumed density, in order to arrive at the unconditional probability in (8).

$$P_q \left(\theta \middle| s_q, x_{iqt}\right) = \int \pi_q \left(j(q,t) \middle| s_q, x_{iqt}, \beta_q\right) f(\beta|\theta) d\beta \tag{8}$$

However, the integral in (8) does not have an exact solution. Therefore, we estimate via simulated maximum likelihood (Revelt and Train, 1998; Train, 1999). Furthermore, it is assumed that there is correlation between the randomly distributed parameters, and, therefore, we estimate the full variance-covariance matrix of the parameter vector, $\boldsymbol{\Sigma}_\beta$, assuming a normal distribution, i.e., $\beta_q \sim N\left(\bar{\beta}, \boldsymbol{\Sigma}_\beta\right)$. Given the Cholesky decomposition of the variance-covariance matrix, $\beta_q = \bar{\beta} + C\eta_q$, where η_q is a vector of standard normals. In other words, we estimate both $\bar{\beta}$ and C, such that $CC' = \boldsymbol{\Sigma}_\beta$.

Despite the desirable properties of RPL, allowing for individual heterogeneity and correlation across alternatives, it is subject to restrictive assumptions. In this case, those assumptions are based on the assumed distribution of the coefficient vector. The two most common are: (a) the log-normal distribution and (b) the normal distribution. However, there is no rule of thumb to select the distribution. As with any sort of model misspecification, the estimated results could be biased if the distribution is mis specified (Carlsson et al., 2003)[4].

3.2.2.3 Latent Class Models. One way to resolve the aforementioned problem associated with the potential misspecification of the underlying parameter distribution is to avoid, as much as possible, distributional assumptions, relying, instead on either non-parametric or semi-parametric methods. The LCM, which is semi-parametric, offers one such avenue. The LCM largely resembles the RPL by allowing for preference heterogeneity, although the heterogeneity is modeled as discrete parameter variation (Greene and Hensher, 2003), rather than continuous variation. Individuals are sorted into classes or segments of the population, which are not observed by the analyst[5].

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[4]Ferrini and Scarpa (2007) undertake a Monte Carlo study of the effects of some aspects of misspecification; however, they focus primarily on CL and nested logits applied in CE studies.

[5]In Latent class model (LCM),individual choice behavior is dependents on observable attributes of the goods in question and latent heterogeneity that varies with factors such as general attitude, perception and socio-economic characteristics of individuals and yet unobservable to analyst (Boxall and Admamowicz, 2002, Greene and Henseher, 2003). In describing a class (segment), suppose that we have a population sample of $N,$ individuals partitioned into M segment defined as a set of individuals or subsample, $S \in N$ having different utility function in the sense that vectors of parameters, β *and* μ are not specific to an individual but common to all individual in that class (segment). However, it is not possible a priori to unequivocally classify each individual into either of the class, $S_i,$ $S_j \ldots \ldots \ldots \ldots S_m$, but rather it is determined by a latent membership likelihood function that classify each individual in a given class. This classification function is related to factors such as general attitude, perception and socio-economic characteristics of individuals (Boxall and Admamowicz, 2002). The determination of the number of classes is then data-driven as it relies on statistical criterions. Note that the number of class ranges between 1 and $N.$ When the number segment is 1, which implies preference homogeneity, LCM collapse to CL. However, when the number of segments equals $N,$ it turns out that each individual is a segment, where the choice problem is modeled by random parameter logit (RPL).

Assuming that there exists *R* segments in the study population, and that an individual belongs to segment $r \in \{1,2,\dots,R\}$, the utility function in (5) can be re-specified to account for segmentation.

$$V_{(iqt|r)} = \alpha_{(iq|r)} + \gamma_{(i|r)} s_{(q|r)} + \beta_{(q|r)} x_{(iqt|r)} + \varepsilon_{(iqt|r)} \tag{9}$$

Utility parameters are, thus, segment specific (Boxall and Adamowicz, 2002). Maintaining the parameter restrictions applied in the CL model, and assuming that μ_r represents a segment-specific mean, the segment-specific choice probability restates (6).

$$P_{(q|r)(i)} = \frac{\exp\left(\mu_r\left(\beta_r x_{(iqt|r)} + \alpha_r + \gamma_r s_{(q|r)}\right)\right)}{\sum_{l=1}^{R} \exp\left(\mu_l\left(\beta_l x_{(iqt|l)} + \alpha_l + \gamma_l s_{(q|l)}\right)\right)} \tag{10}$$

Following Swait (1994), we define the probability that an individual *q* is in segment *r* as given by an additional multinomial logit probability.

$$\Lambda_{qr} = \frac{\exp\left(\tau\rho_r s_q\right)}{\sum_{l=1}^{R} \exp\left(\tau\rho_l s_q\right)} \tag{11}$$

In (11), s_q, as before, is a vector of socio-economic variables, ρ_r is a vector of parameters and τ is a scale factor. Therefore, the joint probability that a randomly chosen individual *q* chooses alternative *i* and is in segment *r* is given by the multiplication of the probabilities in (10) and (11).

$$P_q(i) = \sum_{r=1}^{R} \Lambda_{qr} P_{(q|r)(i)} \tag{12}$$

3.3. Experimental Design and Survey Data

3.3.1 Experimental Design. This study presents the results of a valuation exercise, using CE to elicit household perceptions of the welfare effects of potential community forestry programs in selected sites in Ethiopia. The Ethiopian Federal Ministry of Agriculture, in collaboration with the World Bank previously selected our study sites for possible intervention, due to high levels of deforestation and increased demand for woody biomass in these areas (World Bank, 2008). Within these sites, we administered a CE survey on a sample of 600 randomly selected heads of households, in which respondents were asked to choose their preferred option from a choice set containing the status quo and two alternative scenarios with different levels of community forest improvements. Each respondent was asked to provide answers across four different choice sets, yielding a total of 2400 observations. Community forestry alternatives were constructed in a way that the respondent is forced to make trade-offs. To this effect, the experimental design removes dominant choice sets in which one alternative is strictly better than another, since little would be learned from such choices[6]. The CE questions were designed around four attributes of community forestry: tree species mix, type of place for plantation of the woodlot, the wood harvest quota and the cost to the participant.

Although CE has some desirable properties, compared to CVM, such as the reduction of some of the potential biases found within CVM, as well as allowing for the possibility of testing for internal consistency (Adamowicz et al., 1998 and Alpizar et al. 2001), CE is not a panacea. Appropriate CE methods require careful design, selection of the attributes, the attribute levels and the choice contexts. Furthermore, careful survey design, implementation and appropriate sampling methods are required

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[6]For detailed design principles of choice experiments, see Huber and Zwerina (1996).

to guarantee the best results. Therefore, the CE methodology was underpinned by a meta-analysis of the literature; a pilot study of the methodology was also undertaken to determine if the potential participants understood the survey.

Initially, determination of the appropriate plantation attributes and their levels was considered, after examining EFAP (1993), Mekonen (1999, 2000), Shylendra (2002), Jaggar and Pender (2003), Gebremedhin et al. (2003) and Babulo (2007). The preceding research identified area enclosures, multipurpose trees and eucalyptus plantations as the major forest types used for private woodlots and community plantations across Ethiopian farming communities. Moreover, this research provided estimates of the approximate annual demand for household fuel wood consumption. To obtain additional and complementary information, we consulted researchers specializing in community forestry, including experts from the Wondo-Genet College of Forestry and the Awassa College of Agriculture at Hawassa University. Finally, we conducted focus group discussions. The combination of these processes helped determine a wider range of attributes and their levels. We then chose attributes that are more important for both farmers and policymakers; ourchoices are describedin Appendix 3.A. The attributes and levels were used to create choice sets using the orthogonal main-effect design in SAS, which results in 24 different choice sets blocked into four combinations of three, based on the D-optimal criterion (Kuhfeld, 2001).

Once the CE had been developed, a pilot study was conducted to determine if the survey population could be reasonably expected to cope with four different sets containing three potential choices each. The pilot survey, conducted across 60 household heads revealed no problems, and, therefore, we proceeded. Within the survey, households were randomly assigned to one version of the questionnaire, comprising of four choices sets; consult Appendix 3.B for an example of one version of the questionnaire. Before proceeding with the questionnaire, the purpose of establishing a community forest was briefly explained. In order to make the scenario as realistic as possible, a suitable area of land within each study site was identified and its size specified; respondents were told that a community forest could be established on that site. Subsequently, respondents were told that we were interested in their views regarding the options that were available for that community forest, and the attributes used in the choice experiment were explained. Each respondent was provided with a separate fact-sheet describing the attributes and their levels. The fact sheet contained pictures illustrating the levels of the attributes in the choice sets, and these illustrations were verbally described. Pictorial and verbal descriptions are especially useful, considering the literacy levels of most rural Ethiopians.

In each choice set, respondents were asked to choose their preferred option. The choice set always included the status quo – the base alternative – in which a community plantation would not be established and there would be no cost implication, as well as two alternatives containing differing community plantation attributes. Each alternative is identified by the tree species (namely: eucalyptus, multipurpose trees and a mixture of the two) or area enclosure. For every choice set there corresponded two alterative forest types. The entire CE survey, thus, involved choosing between the following combinations; eucalyptus versus multipurpose forest, eucalyptus versus mixed species, eucalyptus versus area enclosure, multipurpose forest versus mixed species forest, multipurpose forest versus area enclosure and mixed species forest versus area enclosure. The forest type could be the same between two or more choice sets, although the level of other attributes would be different[7]. Although respondents were not specifically asked whether or not they would be willing to participate in a community forestry program, the proportion of respondents always choosing the status quo provides some information related to protest votes and non-participation. In addition to preference elicitation questions, we also interviewed the same respondents to elicit data on socio-economic variables. Moreover, data on the density of existing forest cover was obtained from a spatial GIS survey.

3.3.2 Description of the Data. The socio-economic data used in the empirical analysis is described in Table 3.1. Although the primary purpose of the analysis is to examine preferences for various types of community forests, it is expected that individual, household and community level characteristics are likely to affect the demand for various community forest attributes. Therefore, the survey collected information on the gender of the household head, household livestock holdings (in TLUs, 1TLU=250kg), household size, the age of the household head, the education of the household head and a measure of household access to alternative forests, including natural forests, private woodlots or community plantations. Our measure of access to

[7]For example, in the choice set presented in Appendix 3.B, the respondents must choose between an area enclosure and a multipurpose forest, wherein the former represents alternative 1 and the latter represents alternative 2. The first alternative yields 15 loads for each household annually, is to be established on degraded land and requires a contribution of ETB30 from each household. The second alternative, on the other hand, yields 30 loads annually for each household, is established on productive grazing land and requires a contribution of ETB 30 from each household.

alternative sources of forest products and services is the per-hectare biomass per-capita, obtained from GIS data.

We expect that increased access to alternative forests will tend to reduce the demand for community forestry attributes, because alternative forests can serve as a substitute for community forestry. Similarly, we expect that increased wealth, in terms of livestock, is negatively associated with community plantation participation, and, therefore, community plantation attributes, because community plantations demand greater labor contributions. Since wealthy households have higher opportunity costs for labor, it is relatively costly for wealthy households to participate in community plantations. A similar expectation holds for the household head's level of education. For the remainder of the variables, we do not have any *a priori* expectations.

3.4. Empirical Results

As outlined in Section 3.2, we estimated the utility function parameters β_q using CL, RPL and LCM. In all models, a common intercept and common socio-economic effects across the alternatives were included. Furthermore, preferences were assumed to be stable across the alternatives, although in the RPL and LCM, preferences were assumed to be heterogeneous. We assumed all attributes in the RPL were normally distributed, with the exception of the cost. The cost parameter was assumed fixed for two reasons: (a) the distribution of the marginal willingness to pay for an attribute is the same as the distribution of that attribute's coefficient – in other words, allowing costs to be randomly distributed creates unnecessary complications – and (b) we wish to restrict the cost effect to be non-positive for all individuals (Carlsson et al., 2003 and Phanikumar and Maitra, 2006).

3.4.1 Utility Parameter Estimates

The results from the empirical analyses are presented in Table 3.2. As expected, and the reason for considering multiple models, the utility parameter estimates are quite different across the three models. In terms of comparing the models, the CL is nested within the RPL, and the RPL is statistically preferred to the CL at the 0.001 significance level $(2\Delta\ell = 76.36 > \chi^2_{6,.001} = 22.45)$, where ℓ refers to the estimated log-likelihood. The significant result is partly due to the degree of correlation between the attribute coefficients (presented in Table 3.5). Furthermore, the RPL results imply significant preference heterogeneity, as the estimated standard deviations for both Eucalyptus plantation and area enclosure attributes are significant.

In terms of the LCM, estimates are presented for three segments of the population, as the Bayesian Information Criterion selected three segments. It is also possible to compare the CL and the LCM, since the CL is nested within the LCM; in terms of the comparison, the LCM is statistically preferred to the CL at the 0.001 level of significance$(2\Delta\ell = 172.1 > \chi^2_{24,.001} = 51.18)$. The difference between the LCM and CL is driven, at least in part, by the statistically significant class probabilities, which attest to the presence of discrete preference heterogeneity. However, the RPL is not nested within the LCM, and, therefore, no comparison test is available for these two models.

Given the estimated preference heterogeneity in the RPL and across population segments in the LCM, as well as the statistical significance of both the RPL and the LCM over the CL, we are led to prefer the models that allow for heterogeneity. Regardless of which model is considered, the estimated effect of cost is negative, which is line with economic theory; as costs rise, demand falls. However, the estimated cost coefficient is only significant for the CL, and for segments two and three in the LCM. For the rest of the attributes, preferences for all of the attributes are positive and significant in the CL, while all attributes, except for the Eucalyptus plantation, yield positive and significant preference coefficients. However, the RPL offers more information, as the estimates imply that preferences for both Eucalyptus plantations and area enclosures are heterogeneous amongst the survey respondents. Although the estimated coefficients for the attributes in each of the segments are not similar, the LCM agrees with the RPL results in supporting significant preference heterogeneity. Interestingly, survey respondents in segment two have a preference for the location of the plantation (on degraded land), while Eucalyptus plantations, multi-purpose tree plantations and area enclosure are strictly not preferred. For segment three, Eucalyptus forests and multi-purpose trees are also not preferred, while harvesting quotas and plantation location (on degraded land) are strictly preferred. Segment one is distinct in that this segment of the population is indifferent, in a statistical sense, to each of the attributes considered in the CE.

Although Eucalyptus is commonly observed in the Ethiopian rural landscape, the observed variation in preferences could arise from the following factors. First, fuel wood and poles from Eucalyptus are mainly sold, although Eucalyptus is also used for farm implements and dwelling construction. If farmers are not well integrated into markets for the sale and purchase of Eucalyptus poles, and other forests are available for farm implement production and construction purposes, farmers are less likely to view Eucalyptus as a favourable community forest alternative. Second, there is a common view amongst farmers, policymakers and agricultural experts in Ethiopia that Eucalyptus, regardless of its type, degrades land allocated to its plantation (Jagger and Pender, 2003). Therefore agricultural experts and rural development workers could easily influence farmers, affecting their preferences. Our results lend support to Mekonnen (2000), who found that the WTP for Eucalyptus community woodlots varied across study villages in Ethiopia.

With respect to area enclosures, the CL and RPL parameter estimates suggest that farmers prefer area enclosures to be part of the community forestry program[8]. However, the RPL results further indicate that preferences for this form of forestry are heterogeneous amongst farmers, a conclusion that is supported by the wide variation in LCM estimates across classes[9]. Specifically, segment two respondents strictly do not prefer area enclosures, while segment one and segment three respondents are indifferent. The heterogeneity that is observed in relation to area enclosures is likely to be related to study site variation, in terms of land use and agro-ecology. Area enclosure interventions are limited to woodlands, wherein semi-dry and dry agro-ecology can reclaim wasteland and/or improve grass and woody biomass production, according to Tefera et al. (2005) and Mekuria et al. (2010). Open grazing on communal land is typically associated with this form of agro-ecology, and therefore, households in those sorts of areas may prefer area enclosures.

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[8]In the survey questionnaire, area enclosure is described as restricting use of grazing area, notably hillsides, until the vegetation (trees and grass) sufficiently regenerates. The community sets aside such areas, fences and guards it against encroachment. At a later stage, the wood and grass products are harvested and shared among community members based on the rule set by thecommunity council.

[9]For better exposition to interpretation of class (segment), we refer to footnote 5 above.

Finally, we turn to the socio-economic variables that were included in the analysis[10]. As expected, our alternative forest access measure significantly reduces the demand for community forestry, at least in the CL and the RPL. However, there was significant heterogeneity in the estimates across the LCM; segment two's demand increased, while segment three's demand decreased. There is also significant heterogeneity in preferences related to livestock ownership; it is a significant and positive determinant of community forestry demand amongst segment two respondents. Recalling that segment two also had a strong preference for plantations on degraded land, the estimates related to forest density and livestock ownership support the contention that grazing needs are an important component of community forest preferences, i.e., farmers with more livestock are concerned that the community plantation may compete with grazing land, which is the major source of livestock feed in rural Ethiopia. Finally, the sex of the household head does not follow a constant pattern. Male household heads have reduced demand for community forestry in the CL and segment two of the LCM, but increased demand in the RPL and in segment three of the LCM.

3.4.2 Welfare Measures

The preceding coefficient estimates, although interesting, are not easily interpreted, due to the differences in the estimation models and the scale factor associated with these values (Greene and Hensher, 2003). In order to improve interpretability,

[10]Household's size, education and the age of the household head were not included in the analysis. When included, the variance-covariance matrix became singular; furthermore, when included separately, no statistically significant effects were observed.

marginal rates of substitution between the attributes were computed, using the negative of the cost coefficient as the denominator. The distribution of these ratios is obtained via the Krinsky-Robb (Krinsky and Robb, 1986) method[11]. These ratios can be interpreted as the average marginal WTP for a change in each attribute (Hannemann, 1984; Train, 1998; Carlsson et al., 2003 and Greene and Hensher, 2003). The calculated marginal rates of substitution between the attributes are presented in Table3. 3.

Compensating-variation welfare estimates, WTP, from both the CL and the RPL models, show that all of the choice attributes offer a positive welfare gain, as expected from the positive utility parameter estimates. In each of these models, the average welfare gain from area enclosure community forestry is the largest, followed by multipurpose community forestry, while harvest quotas provide the smallest welfare gain; the ranks for the remaining attributes vary by model. Furthermore, the average WTP is larger for the RPL model than the CL model.

As expected, given the differences in utility parameter estimates across segments in the LCM, the average marginal WTP varies widely across the segments. However, the estimates are more precise. Average WTP estimates are positive for nearly all attributes in segment one, with the exception of plantations on degraded land, but all are smaller than for either the CL or RPL estimates. On the other hand, segment two estimates for Eucalyptus, multipurpose forests, area enclosures and harvest quotas are negative, while mixed species forests and plantations on degraded land all have

[11]In this method, coefficients are drawn several times from the asymptotic normal distribution of the parameter estimates; fare equivalents are calculated for each of these draws (Krinsky and Robb, 1986). The method is less computationally burdensome than bootstrapping (Carlsson et al., 2003).

positive WTP averages. These results suggest that a community forest established on grazing land is a welfare increasing option for this group. It further implies that this group is not concerned with grazing land shortages. Otherwise, they may feel that a community forest established on wasteland is not relatively productive.

One feature, however, generally arises from the results: area enclosures offer the highest average welfare among the study participants, when segment two of the LCM is ignored. Babulo (2007) notes that, in addition to on-site production of wood and grass, area enclosures provide significant off-site services, such as downstream soil erosion reduction and decreased damage in reservoir storage volume and, thus, greater water supplies. Cost-benefit analysis studies by Babulo (2007) and Mekuria et al. (2010) in Tigray, northern Ethiopia, suggest large welfare effects; area enclosures yielded significant net present values (NPV), even when the opportunity cost of alternative land use was considered[12]. Moreover, qualitative studies have revealed that farmers in different parts of Ethiopia positively perceive area enclosures, feeling that area enclosures will benefit them (Shylendra, 2002; Tefera et al., 2005 and Mekuria et al., 2010). Although the preceding research does not analyze the welfare effects of area enclosures relative to alternative community forestry attributes, their conclusions are in line with our finding that area enclosures are perceived to provide extensive

[12]Babulo (2007) found that area enclosures yielded a NPV of ETB 1,579/ha and ETB 3,089/ha, when the opportunity cost of alternative land use was considered, and, when it was not considered, respectively. The estimates rose to USD 837 (approximately ETB 10,558.76) when the carbon sequestration benefit was also taken into account and the opportunity cost of alternatives land use was considered (Mekuria et al., 2010). Note that during survey time the exchange rate between USD and ETB was 13.8ETB/USD.

benefits to our study's participants[13].

*3.4.3 Choice Elasticities.*In addition to WTP results, another useful comparison across models is the estimated choice (share) elasticity (Greene and Hensher, 2003). Table 3.4 presents the implied elasticities calculated from the CL, RPL and LCM models for both the cost and harvest quota attributes. The direct elasticity represents the relationship between a percentage change in the attribute level and the percentage change in the proportion (share) of choices for an alternative, in which the level of the attribute has been changed. In particular, the numbers in the column headed CL imply that a one percent increase in the cost of community plantation in alternative one leads to a 0.150 percent reduction in the proportion of those choosing this alternative. Likewise, a one percent increase in the community plantation harvest quota in alternative one leads to a 0.099 percent increase in the proportion of those choosing this alternative. Similar interpretations can be drawn for the rest of the figures in the table.

As with the rest of the results discussed so far, these elasticities differ across the models considered, as did the reported parameter estimates and average WTP estimates. Given the smaller and more precise WTP estimates, it is not surprising that the share response estimates for the LCM are less sensitive, when compared to the CL and RPL estimates. However, in all of the reported results, the absolute value of the

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[13]Part of the reason could be that area enclosure is often practiced in acacia wood lands, where acacia, being a leguminous species and when remained undisturbed for sufficient time, enriches soil through nitrogen fixation and hence increase productivity of other vegetation while at the same time they can be used a fodder and fuel woods themselves.

estimated elasticity increases when comparing alternative one to alternative two, regardless of the attribute considered. Similarly, as required by economic theory, the estimated elasticity for the cost attribute is negative; an increase in the cost in any alternative reduces the probability of that alternative being chosen. Furthermore, harvest quota elasticities are positive, supporting the *a priori* expectation that increased harvest quotas are associated with increased preferences for the alternative, much as we might expect of the income elasticity for a normal good.

3.4.4 Discussion. Ethiopia has a long history of initiating and implementing community forestry programs, primarily due to environmental activism that developed in the 1970s (Mekonnen, 2000). However, these experiences have generally been deemed a failure, because of a top-down intervention approach. For example, hillside enclosures and plantations on communal land were implemented in the past, within food-for–work schemes; however, the management and planning associated with these interventions were made outside of the community. Recently, though, the incumbent Ethiopian government has developed a different approach, emphasizing community-based resource conservation and management, as part of its rural development policies (Benin et al., 2002). This change has led to efforts for stimulating and organizing collective action with regard to the establishment of area enclosures and plantations or woodlots. In many parts of the country, area enclosure development, as well as community woodlot development, has been carried out in a more participatory process. Although local Departments of Agriculture identify the area to be enclosed or planted, the operational rules are formulated through a general meeting of the community members (Gebremedhin et al., 2003 and Fekadu, 2008).

This increase in community participation has led to the need for research into the types of attributes that are preferable to potential community forest participants, which we address here. In a nutshell, the literature on community forestry programs has mainly focused on how uncertainties regarding ownership and access to community forest plantations, market and demographic pressures led to their widespread failure. Many observers contend that the failure to incorporate local valuations of community forests has been a contributing factor to that failure (Gebremedhin et al., 2003). However, designing a community forestry program that maximizes social welfare and raises acceptance within the community has, so far, not featured in the discussion. The present study extends that literature by addressing this empirical paucity. Particularly, the application of the CE method has provided additional understanding of the relative household valuations of various attributes, information that is crucial to the design of programs and incentives that are more likely to lead to the successful implementation of such programs, and, hence, yield the greatest welfare benefits for communities. In addition to providing estimates of values of alternative forest programs, this study revealed that the valuation varies across individuals, or at least groups of individuals. Therefore, our results support the claim that one size does not fit all. In other words, targeting community forestry programs towards the community meant to benefit from the program has great potential to improve the efficacy of community forestry programs.

Considering our results within the context of the literature, a number of parallels can be drawn. Specifically, many of the attributes of the proposed community forests offer substantial welfare benefits to our study's participants. In other words, maintaining land use in its initial state is less preferred, and, therefore, reclaiming the land for the purposes of a community plantation improves average household welfare. This result provides additional support to the findings made by Mekonnen (2000), Köhlin (2001), Carlsson et al. (2003), Jagger and Pender (2003), Köhlin and Amacher (2005), Babulo (2007) and Mekuria et al. (2010), in which community forestry offers significant welfare benefits. Moreover, the heterogeneity of estimated welfare impacts supports Mekonnen (2000), who finds that community plantation WTP values varied across geographical locations in Ethiopia

Two important forestry policy implications can be drawn from this study. First, a comparison of marginal willingness to pay for attributes contributes to the understanding of the relative importance that respondents hold for them. Second, the results offer insight into the differential impacts of various program interventions, as well as the economic value of such interventions. Knowledge of these differences can be used to improve the design of community plantation alternatives. Focusing on those attributes with higher average welfare impacts will increase the acceptance of the community forestry program in the local community. Given that a wide variety of attribute bundles can be included in various community plantation programs, such programs will have distributional consequences, and these differential impacts can be taken into account for equity considerations.

3.5. Conclusion

The research presented here was based on a choice experiment designed to identify the welfare impact of various community plantation attributes, including: harvest quotas, plantation land type (degraded land or productive communal grazing land), plantation tree species (Eucalyptus, multi-purpose, mixed species of Eucalyptus and multi-purpose), area enclosures and the contribution that each household would have to make in order to establish and manage the plantation. Individuals were asked to choose their preferred option from a choice set containing the current situation (as described by current levels of attributes) and two potential alternative scenarios with different levels of improvements in environmental quality that would be contained in the community plantation. From the CE survey, estimates of utility function parameters, the average marginal value of the different attributes, and choice elasticities for a particular set of attributes were generated from CL, RPL and LCM models. The results are indicative of significant preference heterogeneity, suggesting that community forestry programs should be designed for the community, in which the program is to be placed.

We found that there are considerable trade-offs between various attributes of community plantations, and that these trade-offs vary across the choices models implemented. In the CL and RPL, all of the choice attributes were associated with increases in average welfare, although the RPL point estimates were consistently larger. Although all of the attributes raised welfare, welfare gains were highest for the area enclosure attribute, suggesting that area enclosures should be an important feature of community forestry programs in this study area. Furthermore, the productivity of the community plantations, as measured by its harvest quota, and establishing the community plantation on wasteland, rather than on grazing land, were both found to increase the average welfare across the study population, suggesting that productivity improvements are also important features to be included in the design of community forestry programs in the study area.

However, the strength of the CL and RPL results require caveats. In particular, the

LCM identified three distinct classes of farmers in the study area, and preferences were found to vary significantly across these segments of the study population. Although one segment of the population mirrors the CL and RPL results, the other two segments did not. This finding provides further evidence that, in fact, one size does not fit all, such that local participation in the development of community forestry programs will strongly influence the success of those programs. Despite the heterogeneity observed in the analysis, some comforting consistencies, with respect to economic theory were also observed. Specifically, increases in the cost of the program reduce the demand for community forestry, while increases in productivity increase the demand for the community forests.

Table3.1. Descriptive Statistics of Socio-economic Variables

Variable	Description	Mean	S.D.	Min	Max
Forest density	Per-hectare biomass per-capita	0.254	0.497	0	2.96
TLU	Animal holdings in TLUs	8.64	6.529	0	42.03
Sex of HH head	=1 if respondent is male	0.89	0.30	0	1
Age of HH head	AGE of household head	45.43	12.74	23	90
HH size	Household size	6.48	2.42	1	15
Education of HH head	Household head's education in years	5.50	2.94	0	14

Source: Author's analysis, Note that S.D stands for standard deviation.

Table3.2.Utility Parameter Estimates from CL, RPL and LCM

Variables	Conditional logit	Random parameter logit		Latent class logit		
		Coeff	Standard error	Class1	Class2	Class3
Eucalyptus	0.5642***	0.3390	2.1750 *	29.5428	-58.6562***	-1.2962***
	(0.120)	(0.345)	(1.434)	(27.216)	(13.611)	(0.226)
Multi-purpose tree	0.7604***	0.6655***	0.5926	45.0979	-46.8906***	-1.0319***
	(0.1209)	(0.159)	(0.513)	(42.539)	(10.904)	(0.196)
Mixed	0.4319***	0.4014***	0.5924	27.3682	1.9794	0.0620
	(0.164)	(0.160)	(0.513)	(27.90)	(1.344)	(0.197)
Area enclosure	0.7600***	0.6410***	1.6280***	67.7238	-5.4242***	68.6786
	(0.125)	(0.164)	(0.828)	(63.733)	(1.453)	(66.603)
Harvesting quota	0 .0042	0.0237***	0.0064	1.5523	-0.8247***	0.0611***
	(0.004)	(0.005)	(0.019)	(1.476)	(0.191)	(0.007)
Planting place	0.2663***	0.4654***	0.0269	-0.0228	50.9629***	0.8521***
	(0.086)	(0.133)	(0.222)	(1.935)	(11.465)	(0.168)
Cost	-0.0068*	-0.0050		-0. 7981	-0.6097***	-0.0710***
	(0.004)	(0.005)		(0.838)	(0.140)	(0.006)
Gender	-0.0557	0.1940**		-6.8327	-4.6345**	0.5623***
	(0.181)	(0.106)		(5.400)	(1.328)	(0.138)
Forest density	-0.1651	-0. 1717*		-0.1466	2.2033*	-0.478***
	(0.097)	(0.117)		(1.000)	(1.195)	(0.158)
Livestock	-0.0037	-0.0041		-0.1511	1.9655***	-0.0130
	(0.003)	(0.004)		(0.144)	(0.449)	(0.008)
Respondent size	600	600				
Observation size	2400	2400				
Class probability				0.403***	0.220***	0.375***
				(0.017)	(0. 022)	(0. 018)
Log-likelihood	-1575.76	-1537.58		-1489.71	-1489.71	-1489.71

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

Table 3.3: Average Marginal Willingness to Pay for Attributes

Attribute	Conditional logit		Random parameter logit	Latent class logit		
	Without covariates	With covariates		Class1	Class2	Class3
Eucalyptus	168.44	82.76	190.38	37.01	-96.19	-18.24
	(81.12)	(50.84)	(97.06)	(7.37)	(7.01)	(3.79)
Multi-purpose tree	189.30	111.55	206.03	56.50	-76.9	-14.52
	(90.98)	(65.70)	(111.01)	(8.21)	(6.32)	(3.25)
Mixed	178.17	63.36	204.05	34.28	3.24	0. 87
	(90.98)	(46.50)	(69.83)	(2.69)	(2.102)	(2.75)
Area enclosure	194.97	111.68	220.04	84.84	-8.89	966.94
	(95.10)	(64.50)	(107.72)	(13.02)	(1.703)	(938.25)
Harvesting quota	1.46	0.624	1.83	1.94	-1.35	0. 86
	(0.82)	(0.84)	(3.72)	(0.25)	(0.133)	(0.10)
Planting place	26.56	39.06	21.8154	-0.028	83.57	11.99
	(11.40)	(31.16)	(59.31)	(2.44)	(4.30)	(1.59)

Source: Author's analysis, Standard errors in parenthesis

Table3.4. Implied Direct Share Elasticities

Alternative	CL	RPL	LCM
(i) cost			
1	-0.150	-0.138	-0.088
	(0.10)	(0.13)	(0.09)
2	-0.152	-0.145	-0.103
	(0.09)	(0.10)	(0.08)
(ii) harvest quota			
1	0.0999	0.119	0.059
	(0.07)	(0.11)	(0.13)
2	0.109	0.133	0.071
	(0.06)	(0.09)	(0.15)

Source: Author's analysis, Standard deviations in parenthesis

Table3.5. Correlation Matrix for Random Parameters from RPL

	Eucalyptus β_1	Harvest quota β_2	Planting place β_3	Area enclosure β_4	Multi-purpose tree β_5	Mixed species β_6
Eucalyptus β_1	1					
Harvest quota β_2	-0.942	1				
Planting place β_3	-0.187	0.223	1			
Area enclosure β_4	0.824	-0.708	0.386	1		
Multi-purpose tree β_5	0.291	-0.416	0.522	0.496	1	
Mixed species β_6	0.088	0.086	0.894	0.721	0.643	1

Source: Author’s analysis

Chapter 4

The Welfare Effect of Common Property Forestry Rights: Evidence from Ethiopian Villages

Abstract

In this study, welfare impacts associated with a unique common-property forestry program in Ethiopia were examined. These programs are different from other programs, because they are two-pronged: a community forest is developed and additional support is provided for improved market linkages for the community's forestry products. The treatment effects analysis is based on both matching, which assumes random treatment assignment, conditional on the observable data, and instrumental variable (IV) methods, which relax the matching assumptions. Data for the analysis is taken from selected villages in Gimbo district, south-western Ethiopia. The program was found to raise the average welfare of program participant households. Correcting for selection into the program led to both increased welfare impacts and less precise estimates, as is common in IV analyses. The results underscore the benefits to be derived from expanding the current forestry management decentralization efforts, although these benefits, given the design of the program, cannot be separated from the benefits to be derived from increasing market access for forestry products. However, the evidence suggests that placing property rights in the hands of those closest to the forest, combined with improved forest product market linkages, offers one avenue for both rural development and environmental improvement.

Keywords: *community forestry, treatment effects, IV, matching and Ethiopia*

4.1. Introduction

In recent decades, the devolution of natural forest management to local communities in several countries has become widespread. Such widespread devolution has been underpinned by a growing recognition that management decentralization is a low-cost policy instrument for natural forest stewardship, i.e., local communities are likely to manage forest resources better than the state (Murty, 1994; Agrawal and Gibbon, 1999 and Gauld, 2003). Furthermore, decentralization is seen as a means of upholding democratisation, allowing the people to engage in their own affairs (Agrawal and Ostrom, 2001). Finally, the decentralization of forest management is believed to have the potential to reduce poverty (Angelsen and Wunder, 2003 and Sunderlin et al., 2005).

The literature contains ample evidence that community forestry is beneficial for forests, in particular, and the environment, in general. Klooster and Masera (2000) argue that natural forest management under a common property regime is preferred to plantation forestry and park development, when it comes to carbon sequestration and biodiversity conservation, although Nagendra's (2002) conclusion is less supportive. He reports that Nepalese forests under community management appear to be less biodiverse than national forests and national parks, even though timber tree densities are roughly similar. However, Bekele and Bekele (2005) find increased forest regeneration and reduced agricultural encroachment in Ethiopia, which they associate with decentralized management. Kassa et al. (2009) and Gobeze et al. (2009) also observe increased forest regeneration, as well as increased biomass production and enrichment – trees being planted in trails and on bare patches – in Ethiopia. Blomley et al. (2008) uncover similar successes in Tanzania. They find that the decentralization of natural forest management leads to increased forest stocks; also, there are more trees per hectare, while both the mean height and mean diameter of trees are larger. Moreover, behavioural studies by Edmonds (2002), Yadev et al. (2003) and Bluffstone (2008) report reduced forest resource extraction efforts by program households, due to decentralization, implying increases in the forest stock.

The aforementioned forest condition improvements are assumed to improve rural household income and, thus, reduce poverty. For example, increases in the forest stock may increase the return to other natural and human assets (World Bank, 2008). Improved forest cover can also protect the quantity and quality of water, which could favourably impact household health and labour productivity. Increased forest cover may even help control soil erosion and flooding, resulting in an increase in land productivity. Similarly, increased forest stocks reduce collection times associated with both timber products and non-timber forest products, potentially unleashing labour for other purposes. From a program perspective, on the other hand, government policies that support local organization, improved decision-making related to forest use, and increased local forest user participation in forest product markets is likely to increase the returns associated with other household assets[1].

However, the literature has not reached a consensus with respect to the previously described welfare benefits, partly because community forestry program development

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[1]Governments may subsidize access to profitable market niches, such as coffee, rubber or spices, which have wider international appeal. Similarly, local and international governments may offer transfer payments in exchange for greater forest protection and the global public goods related to that protection.

involves trade-offs and direct investments. In particular, community forestry implies deterred harvest rates and foregone agricultural encroachment, as well as investments in the form of enrichment – planting trees on trails and patches that would otherwise be used for livestock grazing (Kassa et al., 2009) – resulting in increased forest stocks. Therefore, resource rents can accrue to the community, rather than being dissipated under an open access regime; however, there is a trade-off between the immediate returns arising from grazing and the use of open forest resources and the future returns associated with more dense forests. More problematic, however, is that the welfare outcomes described in the literature appear to be either negative or, at the very least, anti-poor. Jumbe and Angelsen (2006) conclude that Malawian programs of this nature have contrasting welfare impacts across their study villages; importantly, they find lower welfare outcomes for poor people in their study. Basundhara and Ojhi's (2000) and Neupane's (2003)cost benefit analyses also find negative net benefits for the poor. Cooper's (2007) CGE analysis uncovers a welfare loss for all concerned, although outcomes for the poor are even worse. The only positive results come from Cooper (2008) and Mullan et al. (2009), although even Cooper's result is only partially positive. Using panel data from Nepal, Cooper (2008) finds increased per-capita consumption, as well as increased inequality. However, Mullan et al.'s (2009) difference-in-differences panel study does find that decentralization has a positive impact on total income in China.

In other words, although it has been maintained that community forestry institutions have the potential to benefit rural households and protect the environment, only limited support of the first part of that hypothesis has been uncovered. Importantly, though, those uncovering decentralization benefits have applied recent advances in micro-econometric methods to deal with the identification problems associated with treatment effects. However, those studies have employed different identification strategies; as such, they are difficult to compare to each other. Furthermore, this literature has not generally distinguished between various decentralization intervention typologies[2]. As noted earlier, decentralization may be complemented by government policies that support the local communities; thus, there is a need to uncover evidence regarding the impact of these combined programs. Although it would be better to disentangle the impacts of each component of the program, doing so is not possible in this study.

It is these issues that motivate the present study. In particular, this study aims to evaluate the impact of decentralized community forestry management on rural household welfare. Both matching and IV methods were applied in the analysis. Matching methods capture the average effect of treatment on the treated (ATT), while requiring rather restrictive identification assumptions. IV, on the other hand, is employed to account for treatment heterogeneity, through the estimation of local average treatment effects (LATE), via both parametric and non-parametric specifications. We applied these methods to data collected from households living close to forests in selected villages of the Gimbo district, in southwestern Ethiopia. Unlike existing studies, we study a specific type of decentralization intervention, a community forestry program that is accompanied by increased commercial opportunities for non-timber forest products. These increased opportunities arise from

[2]One notable exception is Dasgupta (2006), who examines common property rights along with a market linkage program related to fruit cooperatives in India. He finds that this combined program raises welfare

complementary policy measures meant to help forest users access profitable market niches. Therefore, this study contributes to the literature by adding to the small, but growing, literature related to the evaluation of environmental policies in developing and emerging countries, while providing evidence of the effect of decentralized forestry management programs that are accompanied by complementary policies. Furthermore, this study contributes to the debate regarding the potential for community forestry management to yield positive welfare outcomes for the program's participants. Our results provide support for the hypothesis that decentralized forestry management, combined with a complementary market access policy, has the potential to raise the welfare of program participants, and that result is robust to specification. According to the matching estimates, welfare has increased by, on average, ETB336.73, although that average increases to between ETB567.33 and ETB645.16, when controlling for program participation effects.

The remainder of the paper is organized as follows. Section 4.2 discusses the background to common property forestry in Ethiopia, as well as the context of the study. Section 4.3 describes the data collection efforts, while Section 4. 4 discusses the econometric framework that informed the empirical strategies. Section 4.5 presents results and discusses those results. Finally, Section 6 concludes the analysis.

4.2. Common Property Forestry Management in Southwestern Ethiopia

As in a number of developing and emerging economies, Ethiopians depend heavily on forest resources, and the reasons for that dependence are many. Ethiopia's modern energy sector is not well developed, and, therefore, biomass fuel consumption incorporates 96% of total energy consumption (Mekonnen, 1999, Mekonnen and Bluffstone, 2008), 82% of which comes from fuel wood (World Bank, 1994). Given the lack of development with regards to modern energy, Mekonnen and Bluffstone (2008) expect this dependency to continue, arguing that it will likely grow. In addition to the demand for energy, the lower adoption rate of modern agricultural inputs amongst peasant farmers means that forest products, especially fodder, are needed for fertilizer. Finally, the forest provides a safety net to cope with agricultural risk, providing alternative sources of income, which helps alleviate rural household liquidity constraints (Delacote, 2007).

In recognition of the importance of forest resources and the realization that deforestation rates, currently at 8% (World Bank, 2005), are not likely to decrease soon, Ethiopia has recently reviewed its long-standing forestry policies and begun to implement a new set of policies (Mekonnen and Bluffstone, 2008). One of those policies is the decentralization of natural resources, especially forest, management to the communities located near those resources. From that policy, a number of programs have been implemented in Chilimo, Bonga, Borena and Adaba Dodola (Neumann, 2008 and Jirane et al., 2008). The general objectives of these programs are to arrest deforestation, while improving the welfare of those who are largely dependent on the forest for their livelihoods. Although the 2007 Ethiopian forestry policy supports decentralization (Mekonnen and Bluffstone, 2008 and Nune, 2008), bilateral donors[3], such as the GTZ and JICA, as well as NGOs, including Farm

[3] The Deutsche Gesellschaft für Technische Zusammenarbeit (German Technical Cooperation), GTZ, is a bilateral agency mainly engaged in urban and rural development and environmental protection endeavors in Ethiopia. The

Africa/SOS Sahel, are also supporting these programs. These external actors have provided financial support and helped mediate between the local communities and the local and regional governments. In Bonga, for example, participatory forestry management (PFM), also called common property forestry management (CPFM), was implemented by Farm Africa/SOS Sahel. In Bonga, more than six PFM programs have been established to improve the management of about 80,066 ha of natural forests (Jirane et al., 2008). PFM formation has undergone a series of steps. Those steps include: identifying forest units to be allocated to forest user groups (FUGs); defining forest boundaries, through government and community consensus; and facilitating the election of PFM management teams (Neumann, 2008; Jirane et al., 2008and Bekele and Bekele, 2005).

As might be expected, donor involvement hinges, in part, on whether or not the donor believes the program will be successful. Therefore, Farm Africa/SOS Sahel set intervention preconditions focusing on the possibility of success. Effectively, the level of local community and government concern over the current forest situation and the donor's perception of the degree of forest exploitation are important components of these preconditions. Once a forest unit has been provisionally accepted, further efforts are undertaken. The location of the forest needed to be topographically identified, and

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Japan International Cooperation Agency, JICA, provides technical cooperation and other forms of aid that promote economic and social development. Farm Africa is a UK based registered charity, which operates mostly in eastern Africa, focusing on agricultural development and, to some extent, on natural resource management. SOS-Sahel is also a UK based registered charity focusing, primarily on operations in Africa's arid regions, such as the Sahel.

then demarcated in the field. Further, information related to available forest resources was required, as was information related to past and present management practices. Finally, it was necessary to develop an understanding of prevailing forest management problems, forest uses and forest user needs (Lemenih and Bekele, 2008).

A number of observations emerged from this multi-step process. Importantly, agricultural encroachment into forests, illegal logging, and the harvest of fuel wood, for either direct sale or charcoal production, stood out as major deforestation threats, and these activities were most often associated with unemployed urbanites and a heavy concentration of individuals from the Menja tribe. The Menja tribe in Bonga province is a minority ethnic group that is entirely dependent on forests for their livelihood. They are generally ostracized, while being referred to as fuel wood sellers (Lemenih and Bekele, 2008; Gobeze et al., 2009 and Bekele and Bekele, 2005). These observations led Farm Africa/SOS Sahel and local government to target PFM interventions towards forests surrounded by significant Menja populations (Lemenih and Bekele, 2008, Bekele and Bekele, 2005)[4].

Once intervention sites had been identified, Farm Africa/SOS Sahel began negotiations and discussions with all stakeholders. However, since skepticism regarding PFM was rife within both the local government and the local communities, Farm Africa/SOS Sahel provided PFM training for all stakeholders (Bekele and

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[4]Although the Menja population was the overriding eligibility criterion, other criteria, including the degree of agricultural encroachment and the forests' potential to produce non-timber forest products, were considered to a varying degree.

Bekele, 2005). In addition to problems related to skepticism, negotiations with regard to PFM participation and PFM forest boundaries were fraught with difficulties. Whereas PFM membership is meant to include those who actually use a particular area of the forest – regardless of their settlement configuration, clan and/or ethnicity – membership negotiations involved both collective and individual decisions. The result was that the entire community was allowed to determine eligibility based on customary rights, as well as the existing forest-people relationship, which includes the settlement of forest-users, the area of forest-use, and whether or not forest-use was primary or secondary (Lemenih and Bekele, 2005)[5]. Program participation amongst eligible households, however, remained voluntary, as long as the household satisfies the eligibility criterion and abides by the PFM's operational rules. Eligible households that chose to participate form Forest User Groups (FUGs), although not all eligible households participated. Those choosing not to participate in the FUG must revert to using the nearest non-PFM forest, which, in effect, is a forest that operates under the *status quo*; that forest is unregulated, and access is open to all. It is assumed that participation is determined by the perceived costs and benefits of the PFM, a perception that is likely affected by training and other household-specific circumstances.

Experts from Farm Africa/SOS Sahel and local governments, in collaboration with FUG members, develop the Forest Management Plan (FMP), which includes forest

[5]Primary users are those who use the forest more frequently, permanently or directly, whereas secondary users are thoseusing the forest less frequently and those who are located farther from the forest boundary (Lemenih and Bekele, 2008).

protection, forest development, harvest quotas and benefit share rules (Jirane et al., 2008). The FUG elects their management team, and that team comprises of a chairperson, a deputy chairperson, a secretary, a cashier and an additional member. This team oversees the implementation of the FMP and deals with non-compliance[6]. Members of the FUG, after obtaining permission from the management, are entitled to harvest several forest products for their own consumption and sale. FUG members use the forests for grazing, collect firewood, and extract wood for construction material and farm implements (Lemenih and Bekele, 2008). Other non-timber forest products (NTFPs), such as honey, poles, forest coffee, and a variety of spices belong to Forest Users Cooperatives (FUCos)[7]. Each FUCo member collects and delivers these products; the FUCo, in turn, supplies them to national and international markets. Proceeds are disbursed to members in the form of a dividend[8]. Moreover, FUCos receive significant government assistance, including eco-labelling for forest coffee, the provision of price information and technical assistance. Technical assistance is provided for the marketing, processing and packaging of non-coffee NTFPs.

4.3. Methodology

The program evaluation literature distinguishes between process evaluation and summative evaluation (Cobb-Clark and Crossley, 2003). The former refers to whether

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[6]Available evidence from Bekele and Bekele (2005), Lemenih and Bekele (2008) and Gobeze et al. (2009) suggests that PFM has improved forest conditions. The production of non-timber forest products (NTFP) is greater, while notable forest regeneration, increased forest density and increased biodiversity have also been observed. Similarly, agricultural encroachment, charcoal production and illegal logging have all fallen

[7]FUGs are entry-level coordinating bodies. However, complete operationalization of the program results in promotion from FUG to FUCo (Jirane et al., 2008)

[8]The FUCo retains 30% of total income as a reserve (Bekele and Bekele, 2005, Lemenih and Bekele, 2008).

the program has worked as planned, while the second method measures a program's success in meeting its goal (Human Resources Development Canada, 1998). This study is based on the latter, where success is measured in terms of household outcomes, and measurement depends on counterfactuals. Program impact is defined as the difference between the observed outcome and the counterfactual outcome – the outcome that would have obtained had the program not been taken-up (Rubin 1973; Heckman et al., 1998 and Cobb-Clark and Crossley, 2003). As is well understood in the program evaluation literature, counterfactuals are unobservable; at any point in time an individual is either in one state or the other. Heckman et al. (1998) refer to this as a missing data problem. Experimental and non-experimental approaches are commonly used to identify suitable counterfactuals, thereby overcoming the missing data problem. In the experimental approach, study units are randomly selected into both groups, such that program impacts are estimated as the mean difference between group outcomes. In this study, however, a quasi-experimental approach is followed, accepting that program participation is not random. As such, appropriately controlling for participation decisions is tantamount to identifying the program impact.

The theoretical foundations follow Roy (1951). Accordingly, farmers choose whether or not to participate in the PFM program, and that decision is assumed to depend on the farmer's expectation of the welfare associated with either participation in the program or maintaining the *status quo*. If farmer (household) $i = \{1,2,\dots,N\}$ chooses to participate $(D_i = 1)$ in PFM, the relevant household outcome is Y_{1i}; Y_{0i} is the relevant outcome for non-participating $(D_i = 0)$ households. Importantly, only one of these outcomes is observed for any household, and, therefore, in regression format, $Y_i = Y_{0i} + D_i(Y_{1i} - Y_{0i}) + \eta_i = \alpha + \tau D_i + \eta_i$. If $Y_i \perp D_i$, as would be true in a randomized controlled trial, the impact of the program on household outcomes would be obtained from $\tau = E[Y_{1i} - Y_{0i}] = \overline{Y}_1 - \overline{Y}_0$. However, since participation is voluntary, the outcome is not likely to be independent of the treatment choice; therefore, additional assumptions are needed in order to estimate the treatment impact.

4.3.1 Matching. More generally, $E(Y_{1i} - Y_{0i}|D_i) = E(Y_{1i} - Y_{0i}|D_i = 1) + E(Y_{0i}|D_i = 1) - E(Y_{0i}|D_i = 0)$, where the first term to the right of the equation represents the average effect of treatment on the treated, and the last two terms measure the effect of selection into treatment. Assuming positive sorting, such that farmers expecting to benefit from the program choose to participate in the program, the selection effect is expected to be positive, and, therefore, ignoring participation in the analysis would lead to positively biased treatment effects. However, assuming that the distribution of welfare outcomes, Y_{1i} and Y_{0i} are independent of treatment D_i, given a vector of covariates X_i, yields a matching estimator for the average effect of treatment on the treated. Compactly, this assumption is denoted as $(Y_{1i}, Y_{0i}) \perp D_i | X_i$; see Rubin (1973), Rosenbaum and Rubin (1983), Heckman, Ichimura, Smith and Todd (1998), Dehejia and Wabba (2002). Intuitively, the goal of matching is to create a control group that is as similar as possible to the treatment group, although the groups differ in terms of their treatments.

Operationalization of matching, however, can be rather complicated, as there are a number of ways to create matches. Furthermore, if the covariate vector contains many variables, there may be too many dimensions upon which to match. A common solution to this problem is to apply propensity score matching (Rosenbaum and Rubin, 1983), accordingly, $(Y_{1i}, Y_{0i}) \perp D_i | X_i \Leftrightarrow (Y_{1i}, Y_{0i}) \perp D_i | P(X_i)$, where $P(X_i)$ is the propensity score, or propensity to treat, commonly estimated via logit regression. In other words, $P(X_i) = E(D_i = 1|X_i) = \mathbb{I}(X_i\beta + v_i > 0)$, where $\mathbb{I}$ represents a binary indicator function. To identify the average effect of treatment on the treated, in addition to the unconfoundedness assumption, $(Y_{1i}, Y_{0i}) \perp D_i | P(X_i)$, overlap is also necessary, i.e., $0 < P(X_i) < 1$. The second assumption results in a common support, in which similar individuals have a positive probability of being either participants or non-participants (Heckman, LaLonde and Smith, 1999). The analysis, below, considers general propensity score matches, as well as nearest neighbor matches, caliper matches, kernel matches based on propensity score.

Nearest neighbor (NN) matching is the most straightforward algorithm. In NN matching, an individual from the non-participant group is chosen as a matching partner for a treated individual, if that non-participant is the closest, in terms of propensity score estimates (Caliendo and Kopeinig, 2008). Typically, two types of NN are proposed; NN with replacement and NN without replacement. In the former, an untreated individual can be used more than once as a match, while, in the latter, a non-participant is considered only once as a matching partner. The choice between the two is determined by the standard trade-off between bias and efficiency. Specifically, NN with replacement trades reduced bias with increased variance (reduced efficiency), whereas the reverse is true of NN without replacement (Smith and Todd, 2005).

NN matching, however, may risk bad matches, if the closest neighbor is far away, a problem that can be overcome by caliper matching. Closeness in caliper matching is specified through the imposition of tolerance levels for the maximum propensity score distance; that tolerance is referred to as a caliper. Matches are only allowed, if the propensity score difference lies within the caliper and is the closest, in terms of the propensity scores (Caliendo and Kopeinig, 2008). Unfortunately, there is no obvious theory for choosing the appropriate caliper (Smith and Todd, 2005).

Both NN matches and caliper matches share the common feature of using only a few observations from the comparison group to construct treatment counterfactuals. Kernel matching, which uses a non-parametric weighting algorithm, provides an alternative. Kernel matches are based on a weighted average of the individuals in the comparison group, and the weight is proportional to the propensity score distance between the treated and untreated. The advantage of kernel matching is greater efficiency, as more information is used; however, the disadvantage is that matching quality may be limited, due to the use of observations that may be bad matches (Caliendo and Kopeinig, 2008).

4.3.2 LATE. If there are unobservable determinants of participation, meaning that treatment assignment is non-ignorable, matching estimators will be biased. Under non-ignorable assignment to treatment, IV approaches are, instead, needed (Frölich, 2007; Angrist et al., 1996 and Imbens and Angrist, 1994). The major distinction made in the IV treatment effect literature is between constant treatment effects and heterogeneous treatment effects (Angrist et al., 1996), although identification in both approaches hinges on random assignment of the instrument (Frölich, 2007). In many applications, however, the instrument is not obviously randomly assigned; therefore, an alternative identification strategy conditions the instrument on a set of exogenous covariates to yield a conditionally exogenous instrument (Angrist et al., 2000;Hirano et al., 2000; Yau and Little, 2001; Abadie, 2003 and Frölich, 2007).

In the present study, the presence of Menja households is used as an indicator of the intention to treat. As noted earlier, the Menja tribe was an important attribute of the forestry selection process, which further resulted in the provision of training with regard to PFM forestry[9]. Selection, and the resulting training, was expected to help households understand the potential benefits and costs associated with program participation. However, it is likely that intention to treat – forestry selection and training – is correlated with other covariates, such as village access, access to roads, and the underlying condition of the forest. Therefore, in the analysis, we assume that the intention to treat is randomly assigned upon conditioning over these covariates. Moreover, compliance is not perfect, which warrants the application of heterogeneous treatment effects, in order to identify the local average treatment effect (LATE), as outlined by Imbens and Angrist (1994) and Angrist et al., (1996).

In what follows, we characterize the casual effect of interest. The data is comprised of n observations, and the outcome variable Y_i is continuously distributed. There is a

[9]Within the data, 182 households from villages surrounded by selected forests participated in the program, whereas 81 households chose not to participate in the program. On the other hand, 96 households from non-selected villages did not participate, while 18 households from non-selected villages chose to participate. Although the split is not perfect, possibly due to information externalities, selection and training (intention to treat) is strongly associated with participation decisions.

binary treatment variable, denoted by D_i, as well as a binary instrumental variable, Z_i. Finally, the data includes a $k \times 1$ vector of covariates X_i for each household. For concreteness, the following identification assumptions advanced by Abadie et.al (2002) and Frolich (2007) are outlined. To begin, the study population is partitioned according to treatment and eligibility, such that $D_{1i} > D_{0i}$ represents the complying subpopulation, $D_{1i} = D_{0i} = 0$ are the never-takers, $D_{1i} = D_{0i} = 1$ are the always-takers and $D_{1i} < D_{0i}\,(D_{1i} = 0, D_{0i} = 1)$ represents the defiant subpopulation. Across these subpopulations, a number of assumptions are made. These assumptions include: (i) Conditional independence – $(Y_{1i}, Y_{0i}, D_{1i}, D_{0i}) \perp Z_i\,|X_i$, (ii) Monotonicity – $P(D_{1i} < D_{0i}) = 0$, (iii) Complier existence – $P_c(D_{1i} > D_{0i}) > 0$, (iv) Nontrivial assignment, or common support – $0 < P(Z_i = 1|X_i) < 1$ and (v) The existence of a first stage – $P(D_i = 1|Z_i = 1) \neq P(D_i = 1|Z_i = 0)$.

Assumption (i) is a standard exclusion restriction, although it is conditioned on an additional set of covariates. Monotonicity, assumption (ii), requires that treatment either weakly increases with Z_i, $\forall i$ or weakly decreases with Z_i, $\forall i$. Assumption (iii) implies that at least some individuals react to treatment eligibility, and the strength of that reaction is measured by P_c, the probability mass of compliers. Nontrivial assignment, assumption (iv), requires the existence of a propensity score. The final assumption, assumption (v), requires the intention to treat to provide information that is relevant to observed treatment status. If these assumptions hold, the $LATE$ is identified; see Frölich (2007) and equation (1).

$$LATE = E(Y_1 - Y_0|D_1 > D_0) = \frac{E[[E(Y]_1|X = x, Z = 1) - E(Y_0|X = x, Z = 0)]}{E[[E(D]_1|X = x, Z = 1) - E(D|X = x, Z = 0)]} \quad (1)$$

$LATE$ has a causal interpretation, but only for the subpopulation of compliers. Unlike most related applied studies, we implement both parametric and non-parametric specifications of (1), with the latter aimed at relaxing distributional assumptions. For brevity, we skip further discussions of these specifications and, instead, refer to Frölich (2007).

4.4. The Data

Data for the analysis was obtained from a household survey undertaken in 10 Ethiopian villages, in October of 2009. The villages are located in the Gimbo District, which is in southwestern Ethiopia. Survey sites were purposive, in the sense that five PFM villages and five non-PFM villages were selected from a list developed in consultation with the local government, as well as Farm Africa/SOS Sahel. Because the selected non-PFM village was the closest available non-PFM village to the selected PFM village, the village selections partly resemble the selection process for randomized controlled trials.

Respondents provided information on household characteristics, such as: age, education, gender, family size, household expenditure on various goods and services, household earnings from the sale of various goods and services, as well as the labor allocated to harvesting forest products and to other activities. Additional information related to potential determinants of PFM participation was also collected. This information included household circumstances prevailing immediately before the inception of PFM, such as household assets, the household head's education and age, participation in off-farm employment, ownership of private trees, access to extension services and experiences related to alternative collective action arrangements. We also gathered information related to the distance the household was from both PFM and alternative forests. Finally, data related to the community was gathered, including population, ethnic structure, forest status and location.

Descriptive statistics of that data are presented in Table 4.1, and these statistics are separated by participation status; thus the differences give some indication with respect to the vector of control variables to be used to estimate propensity scores. Therefore, the final column of Table 4.1 is the relevant column. As expected from theoretical argument alluded earlier total expenditure and per capita expenditure are larger for the participating households, although the mean difference is not significant. Also, given the way the program was handled, it is not surprising that participating households are located in areas that are nearly 40% more likely to incorporate individuals from the Menja tribe. Therefore, it is expected that this instrument will perform adequately.

In terms of potential observable controls for participation, there are a number of significant differences between participant and non-participant households. Participating households are located nearly 43 minutes closer to program forests, based on walking times; these same households are located just over 13 minutes closer, also based on walking times, to the nearest agricultural extension office. They are also nearly 10 minutes closer to the nearest road, again measured by walking times. However, these households are located 26 minutes (walking time) farther away from the nearest non-program forest. On the other hand, participating households were 5.7% more likely to have a household member working off of the farm, more women in the household are working, and they were 10.5% more likely to have previously participated in other collective programs. Finally, they own more livestock, as measured in tropical livestock units.

4.5. Results and Discussion

This section focuses on the welfare impact of program participation. As noted earlier, if treatment assignment was completely random, it would be possible to simply compare the mean difference in welfare outcomes[10]. Since participation is voluntary, and, therefore, random treatment assignment is not likely to obtain, we instead consider conditional mean differences, based on matching, as well as instrumentation. We consider each, in turn, below.

4.5.1 Matching. Before turning to the results, the underlying premises of matching – unconfoundedness and overlap – must be considered. Table 4.1, as previously alluded, includes an initial balance test, the results of which point to wide differences between participating and non-participating households. Therefore, in order to match and balance the data, program participation was estimated via logit regression. Propensity scores, the predicted probabilities of participation, were used as the matching basis. The logit results, presented in Table 4.2, offer rather similar conclusions to those derived from comparing covariate mean differences, although the ability to simultaneously control for multiple covariates within the regression does yield some differences.

Since a wide range of matches is considered in the analysis, the match quality across these different algorithms deserves attention. The final choice of the matching

[10]According to Table 4.1, this difference is ETB45.40; however it is insignificant

algorithm is potentially guided by a broad set of criteria, primarily concerned with the quality of the match. Roughly speaking, the quality of the match depends on whether or not the propensity score has a similar distribution across the treatment and control groups. One approach is to check if significant mean differences remain across the covariates, after matching. Another approach, suggested by Sianesi (2004), is to re-estimate the logit regression using the matched sample. After matching, there should be no systematic difference between covariates, and, thus, the pseudo-R^2 should be fairly low (Caliendo and Kopeinig, 2008). In the same vein, a likelihood ratio test of joint significance can be performed. The null hypothesis of joint insignificance should be rejected before matching, but not after matching. Table 4.3 provides information related to the quality of the different matching algorithms[11]. According to the results reported in Table 4.3, four of the five NN matches resulted in balance for all of the covariates, as did one of the kernel matching algorithms. Furthermore, matched sample sizes were largest for the NN matches. Therefore, based on balancing NN(2) through NN(5)[12], as well as kernel matching with a bandwidth of 0.0025, perform the best.

Although a subset of the proposed matching estimators perform better than the others, match-based treatment effects (on the treated) were estimated. The results are

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[11]The important columns are the second and third columns. As 14 variables were included in the analysis, a test result of 14 in the second column suggests that the matching yields balance. The numbers in that column represent the number of insignificant mean differences, after matching. Furthermore, the pseudo-R^2 results contained in column 3 suggest that, with two exceptions (caliper = 0.01 and kernel bandwidth = 0.01), the re-estimated propensity score models have very limited explanatory power.

[12]NN(2), for example, refers to an algorithm that includes the two nearest matches

available in Table 4.4. Ignoring the last two rows for now, as they are related to unobserved effects, the results generally point to significant and positive welfare benefits, as measured by households per capita expenditure. The first row of the table repeats the estimate from the second row of Table 4.1, which is based on simple mean differences. This naïve estimate suggests that there is a positive, but insignificant welfare benefit. However, after controlling for program participation, assuming that treatment assignment is ignorable, the conclusion changes. For the best matches, NN(2)-NN(5) and 0.0025 bandwidth kernel matching, the program's average impact on program participants is estimated to range from ETB295.68 to ETB 548.53, and each of the estimates are significantly different from zero[13]. Given that average per capita expenditure for participating households is approximately ETB1732.09, the program impact accounts for between 17.8% and 31.7% of per capita household expenditure.

4.5.2 LATE

Although a number of matches perform rather well, by the aforementioned standards, it should be noted that matching is based on an intrinsically non-testable assumption, conditional independence (Becker and Caliendo, 2007). However, if treatment assignment is non-ignorable, conditional independence is not appropriate, and match-based treatment effects are biased. The sensitivity of the estimates to uncontrolled bias could be either large or small (Rosenbaum, 2005). Although it is impossible to estimate the magnitude of the bias, it is possible to test the robustness of the matching

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

estimates to potential unobserved variables. Rosenbaum's (2002) bounding approach is used in this analysis to examine the sensitivity of the match-based treatment effects estimates with respect to potential deviations from conditional independence. The results of that sensitivity analysis are presented in Table4.4. The first column of the table contains an odds ratio measure of the degree of departure from the outcome that is assumed to be free of unobserved bias, Γ[14]. The second column contains the upper bound *p*-value from Wilcoxon sign-rank tests examining the matched-based treatment effect for each measure of unobservable potential selection bias. As the estimated ATT values are positive, discussed below, the lower bound, which corresponds to the assumption that the true ATT has been underestimated, is less interesting (Becker and Caliendo, 2007) and is not reported in the table. From the table, we see that unobserved covariates would cause the odds ratio of treatment assignment to differ

[14]For ease of exposition, let the probability of program participation be given by $P(x_i, u_i) = P(d = 1 | x_i, u_i) = e^{\beta x + \gamma u_i}$. Therefore, the odds that two matched individuals, say m and n, receive the treatment may be written as $e^{\gamma(u_m - \gamma u_n)}$. Thus, two individuals with the same observable covariates may have differing program participation odds, due to differing unobserved effects, and the odds are influenced by the factor γ. If there is no difference in unobservable covariate or if these covariates don't affect participation, treatment assignment is random conditional on the covariates. Thus, the Rosenbaum test assesses the required strength of γ or $u_m - u_n$ to nullify the matching assumption. Placing the condition within bounds, yields $e^{-\gamma} \leq e^{\gamma(u_m - u_n)} \leq e^{\gamma}$, implying that e^{γ} can be used to assess that strength. For example, if $\gamma = 0$, $e^{\gamma} = 1$, or $\Gamma = 1$, which implies that there is no problem. If, on the other hand, $\Gamma = 2$, one subject is twice as likely as another to receive the treatment, because of unobserved pretreatment differences As such, Γ measures the degree of departure from the random treatment assignment assumption that is inherent in matching (Kassie et al., 2011). If departure occurs at Γ values near 1, the matching estimate is highly sensitive to potential unobserved effects (Rosenbaum, 2005).

between the treatment and control groups, once we reach a factor of about 1.7. Therefore, we conclude that there is strong evidence that the matching method estimates are highly sensitive to selectivity bias. However, as Becker and Caliendo (2007) note, this sensitivity result is a worst-case scenario. It does not test for unobserved factors; rather, it indicates that the program effect confidence interval would include zero, if the unobserved covariates cause the program participation odds to differ by a factor of 1.7.

The implied sensitivity of the results to potential unobserved effects led us to further consider IV methods for treatment effect identification. Therefore, we implemented an IV model to control for endogeneity bias using both parametric and non-parametric specifications, following Frölich (2007). The reported estimates are based on the presence of individuals from the Menja tribe within the local population.[15] These empirical strategies, results available in the last two rows of Table 4.4, yielded relatively higher LATE estimates, compared to the previously reported ATT estimates. However, LATE applies only to the population of compliers, which is 46% of program participants, whereas matching applies to nearly the entire program participant population. The parametric LATE is ETB645.16, whereas the nonparametric counterpart is ETB567.33. Given these values, program impacts account for between 32.7% and 37.2% of program participant household per capita expenditure.

[15] In further analysis, two instruments were included, the presence of Menja and household experiences with collective action; the resulting LATE was ETB611.28. However, when household experiences with collective action is used as the lone IV, the resulting LATE is negative.

Note that the welfare effect of common property management of natural forests reported here is about 20 folds larger than that of community forestry program presented in chapter 2. This raises the question that why such substantial differential could exist. The following explanations are in order. First, the two programs are inherently different in the sense that they provide different products. The proposed community forestry studied in chapter 2, involved eucalyptus woodlots, which was proposed to provide outputs such as timberproducts (wood) for fuel, construction material and agricultural implements and leaves for fodder and medicinal use.However, the PFM program considered in the present chapter provides not only these products (wood for fuel, construction, farm implements and leaves for fodder and medicinal use), but also a range of non-timber products including honey, poles, forest coffee, and a variety of spices. Consequently, the latter is likely to earn higher income than the former. Moreover, the non-timber products from PFM are high value products because they enjoyed additional demands from national and international markets in addition to local market demand and household self-consumption. For example forest coffee commands high prices in international market by virtue of being eco-labeled. Second, the PFM program involved not only production of the products mentioned and hence the income thereof, but also substantial marketing through the so called forest user's cooperative, earning the concerned households additional income.

Third, the difference in welfare effect estimates between the two programs has to do with the difference in methodology followed in either chapter. In chapter 2, the sampling of households covered larger geographic and socio-economic scopes. In particularly, the data used in chapter2 was generated from households selected from four regions (provinces)of Ethiopia, where within each region ten districts and a total of forty villages were selected from all districts combined. However, in the present chapter, data was generated from households selected from ten villages in a single district. Such difference in sampling scope reflects differences in economic environment such as markets and other institutions, technological and environmental opportunities and constraints which are likely to result in welfare effect difference between the two programs.

4.5.3 Discussion. The results from the analysis imply that the decentralization of natural forest management, when combined with market access support for NTFPs, has substantially raised participant household welfare, accounting for approximately one-third of total welfare. This welfare effect has arisen from rent generation[16] associated with the property right regime change, as proposed by Adhikari (2005), Murty (1994), Caputo (2003) and Cooper (2008), and from increased profit opportunities arising from improved market linkages, as proposed by Wunder (2001), or from both. Importantly, our result reinforces Dasgupta's (2006) analysis. In other words, there are now at least two studies providing empirical support for the positive welfare benefits that can be achieved from common property forestry programs that are reinforced with improved market linkages for NTFPs.

In terms of policy, we are not able to directly comment on whether or not welfare impacts are driven by the change in forestry management arrangements, or market

[16] Theoretical property right literature maintains that common property rights generate resource rents and avoid rent dissipation under an open access regime (Caputo, 2003, Agrawal and Ostrom, 2001 and Ostrom, 1999).

linkage opportunities. However, it is possible to infer that maintaining *status quo* state-centralized forest management under poorly integrated market conditions for NTFPs is socially wasteful. Moreover, decentralized forest management combined with improved market integration for NTFPs provides alternative avenues for income generation, thus promoting rural development. With respect to long-term implications, a number of other possibilities arise. First, increased returns from natural forests, due to either increased forest productivity or its product price rise, simultaneously create an incentive s to cooperate as well as incentive to free-ride (cheat) among forest users as the gain from either behavior increases under common property management. The net effect of increased return to improved forest management on the forest resources base and its sustainable use is thus, ambiguous. However, increased current income may provide opportunities to invest in alternative ventures such as financial capital, human capital (education, health) and physical capital related, to off-farm employment and farming. In turn, the accumulation of one or a combination of these capitals, depending the magnitude and volatility of respective return to them relative to that of common property forests, can lessen the household's dependency on natural forests, an outcome that function in favor of the sustainability of the forest resources base. One caveat is, however, in order. The preceding research has not provided any information related to program impact equity. Future research should examine equity potential of these programs.

4.6. Conclusion

Previous studies based on statistical analysis, cost-benefit analysis and CGE have evaluated the welfare impacts of common property forestry programs, finding a wide variety of results that depend upon the study context and the employed methodology. Motivated by these uncertainties, the present study set out to evaluate the welfare impact of a common property forestry program that resulted in the decentralization of forestry management and was augmented by market linkage interventions. The analysis was based on data collected in selected villages of the Gimbo district in southwestern Ethiopia. We implemented the potential outcome framework to examine the causal link between the programme intervention and household welfare outcomes. Compared to the program evaluation literature previously applied in this area, such as that by Jumbe and Angelsen (2006), Cooper (2008) and Mullan et al. (2008), we employed both matching and IV methods.

Controlling sample selection bias through propensity score matching and IV methods, however, revealed that common property forestry intervention has raised the average welfare of participating households in the study villages. The results from the matching method revealed that the program has raised the average welfare of the typical program participant by ETB336.73. After controlling for endogeneity bias through IV, however, the result showed that the program has raised the average welfare by between ETB567.33 to ETB645.16.

Two policy implications were inferred from this evidence. First, the decentralization of natural forest management in combination with greater market linkage (commercialization) for forest products can be used as an alternative rural development policy instrument. Second, the evidence points to the importance of expanding the current practice of decentralization to other areas under an open-access property right regime of natural forest management to raise rural income and halt deforestation. However, future research should examine the equity implications of the program.

Table4.1. Descriptive statistics for baseline covariates and household welfare measures

Variable	Description	PFM participant		Non-participant		
		Mean	SE	Mean	SE	Mean difference
totexp	Total household consumption expenditure in Ethiopian Birr (ETB)	9531.32	389.593	9000.756	337.464	530.564
cpc	Per capita consumption expenditure in Ethiopian Birr (ETB)	1732.09	66.5836	1686.69	59.263	45.397
ageb	Age of household head	36.905	0.997	35.887	1.030	1.017
gender	Household head gender	0.932	0.018	0.943	0.016	-0.010
hhedu	Education (grade attained) of household head	2.290	0.218	2.352	0.229	-0.061
dstpfm	Household distance to programme forest (in minutes)	23.083	2.042	65.85	4.962	-42.768***
offrmb	Whether a household participated in off-farm employment (yes=1)	0.128	0.025	0.071	0.018	0.057*
lndsz	Household landholding size in hectare	2.275	0.125	2.381	0.122	-0.106
wdlot	Whether a household owned private woodlot (yes=1)	0.497	0.037	0.530	0.035	-0.033
tlub	Household livestock ownership converted to TLU	4.120	0.283	3.447	0.202	0.673**
othpartcp	Whether a household ever participated in other collective actions (yes=1)	0.156	0.027	0.051	0.015	0.105***
dstextn	Household distance to extension office (in minutes)	38.223	3.845	51.61	4.530	-13.393**
dstothfrst	A household distance from a non-programme (alternative) forest	55.729	7.15	29.728	2.866	26.000***
mlfrc	Household labour-force (men)	1.284	0.048	1.266	0.041	0.018
fmlfrc	Household labour-force (women)	1.346	0.050	1.153	0.038	0.192***
Menja	Whether Menja people are present in the study village	0.798	0.030	0.403	0.035	0.395***
hhdstwnmin,	Distance to town in minutes	68.51	3.43	72.91	2.71	-4.40
hhdstroadmin	Distance to nearest road	23.21	1.86	32.96	2.72	-9.75*

Table4.2. Propensity score estimates of the determinants of program participation

VARIABLES	coefficient	Marginal effect
Household head's age	-0.008	-0.002
	(0.011)	(0.002)
Household head's gender	-0.336	-0.083
	(0.553)	(0.137)
Household head's education	0.022	0.005
	(0.052)	(0.012)
Female labour force	0.848***	0. 208***
	(0.307)	(0. 075)
Male labour force	-0.230	-0.056
	(0.258)	(0. 063)
Land holding size in ha	0.010	0.002
	(0.085)	(0.021)
Off-farm employment	0.842*	0.207*
	(0.490)	(0. 115)
Distance to agro extension office	-0.004*	-0.001*
	(0.002)	(0.001)
Woodlot ownership	-0.511*	-0.125*
	(0.282)	(0.068)
Livestock holding size in TLU	0.122***	0.030**
	(0.049)	(0. 012)
Distance from PFM forest	-0.028***	-0.006***
	(0.005)	(0.001)
Experience of other collective action	1.400***	0.329***
	(0.509)	(0 .103)
Distance from nearest town	-0.005*	-0.001*
	(0.003)	(0 .001)
Distance from nearest road	-0.008**	-0.002**
	(0.004)	(0.001)
Constant	0.281	
	(0.761)	

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

Table4. 3. Matching Estimator Performance

Matching estimator	Balancing test*	pseoudo-R2	matched sample size
Nearest-neighborhood			
NN(1)	12	0.303	160
NN(2)	14	0.084	160
NN(3)	14	0.057	160
NN(4)	14	0.067	160
NN(5)	14	0.069	160
Radius caliper			
0.01	11	0.459	51
0.0025	11	0.030	117
0.005	12	0.110	117
Kernel			
band width 0.01	11	0.459	51
band width 0.0025	14	0.038	117
band width 0.005	12	0.061	117

Source: Author's analysis

*Number of covariates with no statistically significant mean difference between matched samples of program participants and non-participants

Table4.4. Treatment effect estimates under different estimation strategies

Estimator	ATT/LATE	Standard deviation	t-statistics
Simple mean difference	45.397	88.85	0.51
Nearest neighbor(1)	359.35	131.56	2.73 ***
Nearest neighbor (2)	295.68	111.87	2.64***
Nearest neighbor (3)	336.73	101.53	3.32***
Nearest neighbor(4)	327.62	105.30	3.11***
Nearest neighbor (5)	319.95	101.91	3.14
Radius matching(r=0.01)	103.17	1070	0.09
Radius matching(r=0025)	548.53	148.61	3.69**
Radius matching (r=0.005)	548.53	150.92	3.63**
Kernel matching(bwdth=0.01)	103.17	1150	0.09
Kernel matching(bwdth=0.0025)	548.53	152.84	3.58***
Kernel matching(bwdth=0.005)	548.53	154.91	3.54***
IV-parametric	645.16	210.61	3.06**
IV-nonparametric	567.33	175.01	3.24***

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

Table4.5. Rosenbaum sensitivity analysis

Program-participation odd ratio (Γ)	Upper bound p-value from Wilcoxon sign-rank test
1	0.017335
1.1	0.026368
1.2	0.037451
1.3	0.050448
1.4	0.065171
1.5	0.081406
1.6	0.098931
1.7	0.11753
1.8	0.136995
1.9	0.157137
2	0.177784

Source: Author's analysis

Note that the unobserved covariates would cause the odds ratio of treatment assignment to differ between the treatment and control groups, once we reach a factor of about Γ=1.7when p-values shifts from its lower value ($p < 0.1$) to higher values($p \geq 0.1$)

Chapter 5.

General Conclusion

Deforestation, spurred by market failures associated with incomplete property rights, is pervasive in many developing and emerging economies. Recent theoretical literature maintains that, compared to other alternatives, common property rights appear superior on efficiency and equity grounds. Not only are common property rights expected to curb deforestation, but also they are expected to improve welfare through correcting the attendant externalities. These theoretical developments have, at least partially, along with broader political decentralization, been responsible for a wave of property rights regime shifts. Community forestry (Bluffstone and et al., 2008 Cooke-St.Clair et al. 2008, Köhlin and Amacher, 2005,Gebremedhin et al., 2003, Mekonnen, 2000, and Köhlin, 1998) and state-community property rights arrangements (Sikor, 2005, Larsson and Ribot, 2004, White and Martin, 2002 and Agrawal and Ostrom, 2001) are recent examples of those shifts, and are seen as effective policy instruments in the mitigation and reversal of deforestation.

However, empirical evidence, either supporting or refuting the theoretical predictions, are largely missing, anecdotal or inconclusive. The lack of an empirical consensus warrants further attention, with the presumption that further analysis can further inform both academic and policy debates surrounding the subject.

It is in light of this motivation that the present thesis set out to empirically evaluate the fundamental welfare outcomes associated with common property rights forestry. Its focus on identification of the true parameter of program welfare effects with the aid of advanced econometrics, significantly deepens and broadens the conclusions of the extant literature to inform debates surrounding forestry policy in developing countries. Particularly, this thesis contributes to literature in the following major ways. First, by drawing on an application of double-bounded contingent valuation (CVM), it engages with the valuation of perceived welfare gains that could arise from the establishment of community forestry programs in selected Ethiopian villages, while controlling for potential anomalous response elicitation germane to such studies. Our simultaneous test of two major hypotheses of whether the program is welfare-improving and whether the valuation could have been influenced by biases arising from anomalous preferences revelation significantly extends the CVM literature in developing countries. Particularly, our analysis provides relatively better (less-biased), as well as more precise estimates, of the welfare impacts of community forestry programs to inform policy, compared to the related CVM literature, including Carlsson et .al(2004), Köhlin (2001) and Mekonnen(2000).

The thesis adds to the literature by relaxing the assumption that one size fits all maintained in the CVM literature (Carlsson et al., 2004, Köhlin, 2001, and Mekonnen, 2000), cost-benefit analysis literature (Mekuria et al., 2010,Babulo, 2007 and Jagger and Pender, 2003) and the treatment effects literature (Köhlin and Amacher, 2005). It is to be recalled that each of these studies has provided an important contribution to our understanding of the welfare impacts of community forestry in developing countries, but they have reached that conclusion through the assumption that community forestry is a single-typed program. These studies generally recommended a blanket approach for implementing such programs, a contention we describe as one size fits all. In our analysis, we account for this limitation by assuming otherwise, such that community forestry programs have multiple attributes, including the size of the stake of the program, the cost of implementation, the composition of the plantation and the quality of communal land to be allocated. We tested two hypotheses; whether the relative household valuation of these attributes is the same across attributes and whether preferences related to the various attributes is homogenous among individual farmers and across groups of farmers. The analysis helps us identify salient community forestry program attributes, in the sense that these are the attributes that peasant farmers prefer to have included in the program. Such information can help design a community forestry program that enjoys wider acceptance by the community and, hence, maximizes social welfare. The analysis employed choice experiment methods to estimate the welfare associated with selected community plantation attributes. Additionally, we applied recent advances in discrete choice econometric models to test for preference heterogeneity and the sources of that heterogeneity.

Moreover, the thesis is also an important addition to the growing literature examining quasi-experimental evaluations of welfare outcomes that can be attributed to common property natural forest management programs. *Inter alia*, recent literature has employed propensity score matching(Mullan et al, 2009, Jumbe and Angelsen, 2006), panel data models and IV methods (Cooper, 2008) to identify the average welfare improvements that can be ascribed to the decentralization of natural forest management. 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. In recognition of these limitations, we employed a combination of matching and IV methods to identify the causal impact of the program. An application of the IV method, based on a single binary IV, allowed us to account 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. These empirical strategies thus, allow us to take the conclusions of the extant literature a step further in identifying the correct program impact, thus informing both policy and academic debates surrounding whether common property right forestry management can effectively revive rural development, while helping to protect the environment.

The analyses were based on the data obtained from rural household survey in selected villages of rural Ethiopia during 2009. The first two analysis chapters were based on the survey data generated through 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. Moreover, it was extended to include decisions between multiple community forestry program scenarios for the choice experiment valuation exercise. 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 (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.

Several findings emerged from this research. The results from the first chapter indicate that community forestry programs offer sizeable welfare benefits and that these benefits are incorrectly estimated, if preference anomalies are not accounted for in the analysis. This is one of the first studies to consider double-bounded CVM anomalies in a developing country setting, although the results agree, to some extent, with the international literature, which finds similar anomalies. In the second chapter, the results suggested that welfare largely hinges on the attributes included in the community forest. Moreover, the results pointed to significant differences in attribute preferences across the study population, suggesting that programs should be tailored to the communities in which the program is to be implemented.

A set of policy implications can be drawn from these evidences. First, the results provide support to furtherance of community forestry programs as alternative forestry policy instrument to improve rural household's welfare. Moreover, such welfare gain can be maximized, if the program's design identify the attributes that the local community participation promote the most (promote local participation) and account for preferences heterogeneity of the selected attributes and then tailor the program implementation.

In the third chapter, after controlling for selection biases and treatment-effect heterogeneity, the result implied that common property rights applied to natural forest management raises participant welfare by between 19.96% and 33.63%. Given these results, it was reasonable to conclude that common property forestry management is capable of reviving rural development, in addition to its ability to protect the environment, the latter of which has been uncovered in related research.

In nutshell, evidences from our analyses reinforce theoretical conjecture that common property right institution is defended on efficiency ground as it increases social welfare through preventing resources rent dissipation associated with open access exploitation. It follows that traditional state property right (*de facto* open access regime) of natural forest management and communal grazing land management under open access property regimes are socially wasteful resource management alternatives. These inferences vindicate a prompt shift from these alternatives towards resources management regime under common property right to improve both social welfare and environmental outcomes. However, there is a real need that the usefulness of such policy move should be seen in light of its equity implication.

We suggest future research in the following areas; in contrast to static treatment effect evaluation model employed in this thesis, one could implement a structural dynamic choice model and hence identify dynamic treatment effect. The interest here resides in testing the hypothesis of whether household's duration of program participation (time to treatment) can have varying welfare impact across households.

Second, one can extend the analysisby testing the hypothesis of whether the treatment effect is heterogeneous across the welfare distribution to draw inference about distributional consequences of the program. As opposed to average treatment effect evaluation, quantile treatment effect (QTE) evaluation will be a useful empirical strategy to test this hypothesis.

Third, given that common property forestry management is often plagued by free-riding, it remains intriguing to identify alternative enforcement mechanisms that foster compliance with its rules of provision and appropriation. To that effect, one can carry out a framed field experiment to evaluate various enforcement mechanisms of common property forestry management, which may include standard monitoring mechanism, collective taxation and ostracism.

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Appendix 2.A. CVM questionnaire

Suppose that your "got" development committee (GDC) proposes that the "got" need to establish new community forest plantation ("got" Woodlot) on the "got" communal grazing land. Also suppose that this plan is endorsed by the "Kebele" administration and district office of agriculture.

The community forest plantation offers you the following benefits:

You get fuel wood and it reduces your household time required to collect fuel wood from distant woodlands and/or other forests. The time saved can be used for agricultural activities, marketing or social activities. Moreover, it allows you to use crop-residue and animal dung for your farm soil management instead of using them for fuel. In addition, it provides animal feed (fodder) particularly during dry season when fodder from communal woodland is hardly available. You can also use leaves of the plantation for medicinal purposes. When harvesting age of the plantation reaches, you can share timber products of the plantation for construction material and agricultural implement. You can either use these products for yourself or sell them to generate cash income depending on your need. But, note that the communal grazing land used for the forest plantation is not going to be used for grazing any longer like it is being used now for the long years to come.

The proposed woodlot has the following characteristics:

species mix: Eucalyptus

harvest quota: __30____meter cube

type of place: x grazing land

Also note that the government doesn't have enough funds to finance the project and the establishment can be possible only if the "got" community contribute money for the establishment and management costs of the forestry..

The contribution is required from the community for:

establishing community nursery or purchasing seedlings

site preparation; clearing the site, digging hole and fencing

employing guards to protect against theft

It is also important to note that the control and the management of the money contributed are entrusted to the development committee and the committee cannot divert this fund to any other purpose by law. Note that the money will be collected by the committee after main crop harvest during each year.

The contribution is paid from each community member household annually for the five consecutive years.

When we talk to other people in your village, we have found people who vote for the project and those who vote against it. Both of them have good reasons to vote that way.

Those who vote for the project say that;

-Having increased forest products is worth a cost

-They are tired of walking over long distance to fetch fire wood and other forest products

-They want to reduce their farm fertility loss by applying more manure(dung) and crop residue instead of using them for fuel

- They need supplement feed source for their cattle particularly during dry season

-Timber products for construction and farm implements are getting ever scarce

Those who vote against the project say that;

-Community forest plantation reduces grazing areas of their animals

- They would rather save money and spend on other things

-They own private woodlots and alternative community forest

-They cannot afford time to attend series of meetings to take care of the community forest plantation.

We would now like you to weigh the benefits and cost associated with the woodlot establishment described above and answers the following questions:

1. Before I go on, do you have any questions about the plan to establish a community forest plantation?

Yes………………1,

No… ……………2(go to 2)

1.1 What would you like to know?

If the respondent asks about costs, tick here and say: " we will come to that in a moment."

2. Do you want to contribute for the community forest?

Yes………………1,   No………………2

2.1 If no why? Use code A

3. As we said earlier the cost required for the establishment of the plantation is not known in advance. However, if you are a decision maker in your household and asked to contribute Birr __________ annually for five consecutive years, would your household be willing to contribute the money?

1. Yes ➔ Go to 4

2. No ➔ Go to 5

4. What if you are asked instead Birr__________annually for five consecutive years, would your household be willing to contribute the money?

1. Yes   2. No

5. What if you are asked instead Birr__________annually for five consecutive years, would your household be willing to contribute the money?

6. What would be the maximum annual premium that your household would be willing to pay?___________Birr

6.1 To enumerators: *probe if the answer is yes to 4 or 5 and the maximum willingness to pay in 6 is less than the amount he agreed to pay in 4 or 5* as follows;

Why is it that the maximum annual premium that your household would be willing to pay is less than the amount you initially agreed to contribute? Use code B

Appendix 3.A. Attributes and Levels Used in the Choice Experiment

Attribute	Description	Levels
Cost	The total cost for the individual if the alternative was chosen	Br/year 0, 30, 48, 62
Forest type	The forest program can have single tree species, multipurpose species , a mix of both or a combination of herbaceous and wood species	eucalyptus only, multipurpose tree only, mix of eucalyptus and multipurpose tree, area enclosure
Type of place	Describes the quality(degraded or not) of a place where the community plantation is to be planted	Waste land(communal), productive communal grazing land
Harvesting quota	The amount of wood biomass that a household would be allowed to harvest per year from the community forest	0 , 15load/year, 30load/year

Appendix 3. B. Example Choice Set

Forest Attributes:	**Alternative 1 (Current state)**	**Alternative 2**	**Alternative 3**
Forest type	Same as today	 Area closure	 Multi-purpose forest
Harvest Quota	Same as today	 15 load	 30 load
Type of Place	Same as today	 Degraded land	 Grazing land
Total cost per household	Zero	 Birr 30	 Birr 30
Indicate the option you prefer most (Tick one)	☐	☐	☐