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Valuation of community forestry in Ethiopia: a contingent valuation study of rural households

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ABSTRACT. Community forestry projects in Ethiopia have been implemented using the top–down approach, which may have contributed to the failure of most of these projects. The so-called community plantations practically belonged to the government and the labour contribution of the local communities in the establishment of the plantations was mainly in exchange for wages. In this paper, we use the contingent valuation method to examine the determinants of the value of community forestry in rural Ethiopia and its feasibility, when the plantations are established, managed, and used by the communities themselves. The value elicitation format used is discrete question with open-ended follow-up which is closer to the market scenario our respondents are familiar with compared, for example, with the single discrete choice format. Unlike most other studies, we use a tobit model with sample selection in the empirical analysis of the bid function to correct for the effect of excluding invalid responses (protest zeros, outliers and missing bids) from the analysis. The analysis of the bid function shows that household size, household income, distance of homestead to proposed place of plantation, number of trees owned and sex of household head are significant variables that explain willingness to pay. We also find that there are significant differences in willingness-to-pay across sites. It is hoped that this study contributes to the limited empirical literature on community forestry in developing countries by indicating some of the conditions under which community plantations will be acceptable and feasible.

1. Introduction

In the face of continuing heavy dependence on woody biomass in the near future coupled with an estimated negative growth rate in the stock of forests, one possible way of satisfying increasing demand for woody biomass in Ethiopia is increasing its supply through tree planting. Thus, at a general level, it does not seem surprising to find project proposals for afforestation or reforestation in the country (World Bank 1984; ENEC 1986; EFAP 1993a). One such scheme is the introduction and expansion of community woodlots (forestry).[1]

[1] It should be emphasized, however, that this is not the only option available to satisfy increasing demand for woody biomass. Other potential alternatives or complementary options include energy substitution (such as electricity, kerosene, biogas and liquefied petroleum gas (LPG)), substitutes for construction such as bricks and energy efficiency improving measures such as improved stoves and cooking pots.

Community woodlots are not new to many Ethiopian peasants. Their history goes back to the second half of the 1970s when they were introduced largely as food-for-work projects in the drought-affected areas of Ethiopia. They emerged as a product of the environmental activism and awareness that developed immediately after one of the major famines in modern Ethiopian history. They also came after the seizure of power by the now defunct military-socialist government in 1974 that, among others, nationalized land in 1975 and created peasant associations (PAs) as the lowest administrative units in rural Ethiopia. This new land tenure system and administrative structure implied, at least in theory, that the PAs would have some area of land under their jurisdiction, part of which is allocated for individual use by peasants and part for communal use by members of the PA, such as communal grazing and browsing land. In practice, however, projects such as hillside plantations and structural soil conservation measures have been initiated and implemented using the top–down approach whereby the main decisions related to the establishment, management and use of the benefits of community forests were taken outside the communities themselves.[2] The labour contribution of the local communities in the establishment of the plantations was mainly in exchange for wages largely financed by the United Nations/World Food Program (UN-WFP) (WFP 1991, 1993; Yeraswork 1995). With a value of food committed by WFP that was estimated to be slightly over a quarter of a billion USD for the period 1975–90, it was the largest food-for-work project in Africa in terms of the resources committed (Yeraswork 1995: 5).

In 1991, following a change of government, the management and use of some of the plantations that survived was transferred to local communities (PAs). However, a number of them were destroyed, either in the transition between the two governments or immediately after the transfer to the local communities through lack of proper rules and regulations on their management and use (Yeraswork 1995).

In an evaluation of the soil conservation and afforestation program, Hoben (1995) observes, 'in retrospect, it is clear that much of this effort was wasted or counterproductive', and argues that the government and donors tried 'to justify the rapid, massive and widespread use of standardized environmental management "packages" without research on their environmental impact or their economic costs and benefits'.

Does such an experience mean that there is no need and no future for community woodlots in Ethiopia? Probably not since community woodlots could, among other things, minimize time spent to collect fuel and increase woody biomass availability for fuel and construction, particularly for the landless and those in short supply of labour for fuel collection. In addition they could contribute to mitigation of environmental degradation. With respect to tenure security, it would be more secure to introduce forestry programs at the level of the community compared with private plantations, particularly if redistribution of individual land by PAs

[2] For a discussion of different types of property right structures and their implications for incentive for the management of common property resources see, for example, Ostrom (1990).

is to continue. However, community woodlots would most definitely fail and be unnecessary if planned and implemented as in the past using a top–down approach, with little benefit for the community or community involvement in the decision making process. It is by now well established that the efficient use of common property resources depends very much on the specific rules, regulations and practices that are applied in their management (Ostrom 1990). As Kidane (1994) argues, lack of participatory approach in the planning and implementation of social forestry programmes is one major reason for the limited success of past efforts in the forestry sector in Ethiopia and 'the initiative for community forest development should emanate from the farmers themselves' (p. 15).

In this paper we use the contingent valuation method (CVM) to examine the determinants of peasants' willingness-to-pay (WTP) for community woodlots that are financed, managed and used by the communities themselves. Given past practices, where major decisions concerning the so-called community woodlots were made by non-members of the community, analysis of the determinants of WTP when woodlots are financed, managed, and used by the communities would help answer the question whether or not management is one important factor. The study may also be an addition to the limited literature regarding application of the CVM to social forestry issues in developing countries in general, and sub-Saharan Africa in particular.[3] From a methodological point of view, we use a tobit model with sample selection to test for selectivity bias that may arise from the usual practice of excluding (discarding) invalid responses (protest zeros, missing bids, and outliers) in the empirical analysis of theoretical validity of responses to the valuation question.

The rest of the paper is organized as follows. In section 2 we review some theoretical and methodological issues focusing on value elicitation question formats and the treatment of invalid responses (protest zeros, missing bids, and outliers) in the empirical analysis of theoretical validity of responses and implications for benefit measurement and inference. Survey design and implementation are discussed in section 3. Section 4 presents empirical analysis and discussion where we deal with tests of theoretical validity and selection bias. Welfare measures and aggregation are discussed in section 5. Section 6 concludes the paper.

2. Some theoretical and methodological issues

Community woodlots could be considered a quasi-public good in a certain social context. There are various benefits to be derived from community woodlots. Some accrue to individuals directly in the form of, for example, twigs, branches, and leaves for fuel, and the final harvest of wood for fuel or construction. Some are indirect which could take the form of external benefits and are more useful to the society at large, for example, soil conservation, carbon sequestration, and mitigating desertification. Given the nature of the property right structure, we do not know how much value peasants attach to community woodlots. We use contingent valuation which is a direct valuation method that uses surveys to elicit the value

[3] A brief review of the literature is presented in the next section of the paper.

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individuals attach to non-market goods. Since we are proposing community woodlots to be established and used by members of the community, the WTP format is used, given the property right structure (Mitchell and Carson 1989). The relevant welfare measure is therefore the Hicksian compensating surplus.

A number of theoretical and methodological issues and criticisms have been raised concerning the application of the contingent valuation method (CVM) in general and in valuation in developing countries in particular. A look into the literature indicates that, at least at the theoretical level, a large number of the criticisms, particularly those related to economic theory, are in one way or other, linked with problems in the details of specific studies, such as how the questionnaire was prepared and data collected and analysed. Even such issues as insensitivity of values to the scope of the good are, to some degree, linked with how studies are conducted (Carson, Flores, and Meade, 1996; Hanemann 1994a, 1994b, 1996).

An issue more directly relevant to our study has been the scepticism expressed, even by those who accept CVM as a non-market valuation method, about the usefulness of contingent valuation in a developing country context (Dixon and Sherman 1990). In 1996, Whittington (1996: 1) noted that ten years ago the conventional wisdom was that CVM simply could not be done in developing countries, mentioning 'problems related to posing hypothetical questions to low-income, perhaps illiterate respondents [that] were assumed to be [so] overwhelming that one should not even try'. However, it has been shown by a number of studies that CVM can actually be meaningfully applied to developing countries (Georgiou *et al.* 1997; Whittington 1996). Most of these studies seem to concentrate on water quality improvements and sanitation (e.g., Aguilar and Sterner 1995; Griffin *et al.* 1995; Wasike and Hanley 1998; Whittington *et al.* 1990; WBWDRT 1993). CV studies on forestry issues seem to be few and include: Köhlin (1997) who looks into WTP for social forestry in Orissa, India; Lynam, Campbell, and Vermeulen (1994) who study WTP for services from trees on communal lands in Zimbabwe; and Shyamsundar and Kramer (1996) who use a willingness-to-accept format for land-use restrictions associated with a newly established national park in Madagascar.

After reviewing experiences and problems in CV studies in developing countries, Whittington (1996: 9–10) states the existence of some contingent valuation researchers (counting himself as one of them) 'that believe it is easier to administer high quality contingent valuation surveys in some developing countries than it is in industrialised countries'. While such beliefs may not be shared by many, it is important to note, as Whittington does, that there are some problems that should be considered in relation to specific cultural values and practices in conducting CV studies in developing countries (Whittington 1996). In some cases, these considerations may be additional reasons for using approaches (methods) in the design and implementation of CV studies that are less common.

A common value elicitation method is the dichotomous choice format. As an alternative to dichotomous choice questions, the use of an open-ended follow-up question to a binary (close-ended) one has been proposed

and used by Mitchell and Carson (1989).[4] Moreover, Green *et al*. (1995: 21–2) argue that a binary question with open-ended follow up question 'provides far more information on WTP and information on plausibility of responses than alternatives such as the double referendum method'. Disadvantages of using the open-ended format identified in the literature include: (1) large non-response rates because of respondents' general tendency to have difficulty in giving an estimate of their maximum willingness-to-pay, partly because of their unfamiliarity with a market situation in which they state the maximum price they are willing to pay (Mitchell and Carson 1989); (2) respondents may report what they would not mind paying for the good instead of their maximum willingness-to-pay (Bishop, Heberlein, and Kealy 1983) and; (3) it may be prone to starting point bias if it is preceded by a binary question. While disadvantages (2) and (3) could be minimized by improved survey design and administration, we should note that the (double) referendum method also suffers from the third disadvantage. In addition, because of its dependence on the statistical method used to obtain welfare measures, the latter also suffers from a disadvantage similar to the second one, i.e., incorrect measure of welfare could be obtained. Moreover, while the problem of high non-response rate would be minimized when open-ended questions are preceded by close-ended ones (and, perhaps, in-person interviews used), the idea of unfamiliarity with the market scenario is not always a problem, particularly when open-ended questions are presented as a follow-up to a binary question. In fact such an elicitation format is closer to what the respondents for this study, and many others in Ethiopia and other developing countries, are used to than the referendum method in which the respondents, as buyers of a commodity, would first expect the price to be stated by the seller and then after some bargaining would decide on the final amount he or she would pay.[5]

After a comparison of open-ended willingness-to-pay against dichotomous choice, Schulze *et al*. (1996) conclude that 'CV researchers should not be restricted to use of dichotomous choice' and that the NOAA panel guidelines 'could have the side effect of freezing research by mandating procedures which are still under intensive investigation' (pp. 111–12). Although the referendum method is currently the most widely used value elicitation format, there does not seem to be a consensus on their use, partly because of reasons discussed above. Comparisons of these two formats continue (see, e.g., Brown *et al*. 1996).

[4] The discussion in this paper focuses on single and double bounded referendum methods (which are the most common methods currently in use) and the binary question with open-ended follow up which is the method used in this paper. Other methods include iterative bidding, open ended and payment card. For details on these see, e.g., Mitchell and Carson (1989).

[5] We should note that this value elicitation method is similar to what our respondents are used to particularly when they reject the offered bid and are asked to state their maximum WTP. Moreover, the bidding game might be much closer to what the respondents are familiar with (bargaining), although it has the disadvantage of boring the respondent when several bids are suggested until the respondent accepts the offered bid.

Another issue is related to tests of validity of CV estimates. A class of validity commonly considered in empirical analysis of CV data is theoretical validity—a type of construct validity—where an attempt is made to explain a benefit measure by variables to which, theory suggests, it should be related (Mitchell and Carson 1989). An issue that has received limited attention in the empirical analysis of theoretical validity and in benefit aggregation, is the treatment of invalid responses to the valuation question(s) in general and protest zeros and outliers in particular. As Carson (1991: 160) notes, the typical course of action (or the approach used most often) has been to discard these observations (protest zeros and outliers), which is clearly incorrect from a statistical point of view. Protest zeros (invalid zero responses) are usually selected based on responses to the question why individuals are not willing to pay, following the WTP question(s). On the other hand, outliers (invalid responses which are usually on the right side of the WTP distribution) are determined by the researcher based on measures such as the share of WTP in income or what is called α-trimmed mean (Freeman III 1993; Carson 1991; Mitchell and Carson 1989).[6]

Exclusion of both protest zeros and outliers may not be random which implies that the two groups (i.e., those excluded and those who are not) may be significantly different from each other. Under such conditions discarding invalid responses (protest zeros and outliers) may lead to sample selection bias which could have two consequences. First, the empirical analysis of the valuation function used to test for theoretical validity may give inconsistent parameter estimates similar to those stated in Heckman (1979) or Amemiya (1984, 1985). Second, the estimated benefit measures and hence the aggregated values may also be biased.

According to Edwards and Anderson (1987), A. Randall, J. Hoehn, and G. Tolley first suggested in 1981 that Heckman's analysis of censored samples was germane to selection bias in contingent valuation research (p. 170). Kaoru (1993) and Alvarez-Farizo, Hanley and Wright (1996, 1999) also use sample selection models to analyse willingness-to-pay. Mitchell and Carson (1989: 278) discuss the need for applying the Heckit procedure to test for sample selection bias when non-response is non-random and state that they 'know of no CV study that has attempted to use these techniques to correct for sample selection bias'. Smith and Desvousges (1987) use Heckman's two-step procedure to analyse the behaviour of respondents with positive responses but do not separate protest zeros from valid zeros. Shyamsundar and Kramer (1996) use a bivariate probit model by separating non-respondents from valid responses to test for selection bias due to non-response (which could also imply sample selection bias,

[6] When the share of WTP in income is used as a criterion, the rule would be to delete all responses representing more than X per cent of reported income, where the value of X could be chosen by the analyst based on an assessment of the likely importance to people of the environmental or resource change being valued (Freeman III 1993). On the other hand, Mitchell and Carson (1989) advocate the use of the α-trimmed mean, where the value of α is chosen by the analyst. In this case, a proportion of responses equal to the selected value of α is deleted both from the top and bottom of the rank-ordered responses.

though not related to protest zeros and outliers). Whitehead *et al.* (1993) use a combination phone/mail survey to test for non-response and sample selection bias using a bivariate probit model where protest non-responses are included as non-respondents. Wang (1997) provides a utility-theoretic interpretation of 'don't know' responses in contingent valuation surveys and used a maximum likelihood procedure to include 'don't know' responses in the estimation of willingness-to-pay functions and mean WTP. Edwards and Anderson (1987) argue that one reason sample selection bias could arise in contingent valuation studies is that 'researchers might create selection bias by culling "outliers" (i.e., observations with protest, endowment, and, otherwise, missing bids) from data sets in order to isolate a "solid core" of information that is consistent with the contingent market' (p. 170). They used Heckman's two-step procedure to test for selection bias from omitting zero and missing bids. Although it is not clear whether the zero bids they excluded are only protest bids, they find that there is no selection bias.[7] In this paper we have attempted to test for sample selection bias due to omission of invalid responses (i.e. protest bids, outliers, and missing bids).

3. Survey design and implementation

The data for this study came from a survey of a random sample of rural households in four Peasant Associations (PAs) in Ethiopia conducted in February 1996. At the time the field work was done these PAs had different experiences regarding community forestry. Two experienced community forestry, one had it during the former military-socialist regime but later this was destroyed in the transition between governments in 1991, and one never experienced it, although there is still some land that has been reserved for such a purpose. In addition to the CV questions on willingness-to-pay for new village woodlots, the questionnaire included other questions such as biomass fuel collection and consumption, tree growing behaviour and basic household characteristics and income. The total number of households surveyed was 480. In-person interview was used in the administration of the survey which, we may note incidentally, was the only feasible option in the selected study sites.[8]

A pilot survey of 47 households was conducted, the result of which was used, among others, to revise the questionnaire for the final survey. Given

[7] While the sub-section that deals with selection bias in their paper is titled 'Selection bias from omitting protest bids', they state in their discussion (including a footnote) that they excluded zero and missing bids and did not eliminate any bids by an outlier method. If they actually omitted only protest bids, as is suggested by the title of the sub-section mentioned above, and there are some zero bids used in the estimation of the WTP function in the second step of Heckman's two-step procedure, the parameter estimates may be inconsistent (Greene 1995). On the other hand, if they omitted all zero bids which may also include non-protest zeros, then the test is not for omission of protest bids and missing bids but for the difference between positive bids and others.

[8] One supervisor was employed at each site. Each supervisor had twelve interviewers under him. Both supervisors and interviewers were carefully trained. Cooperation to be interviewed was gained through the leaders of communities.

the problem of shortage of land that is common in the study areas, information on the area of land that could be used for the new community woodlots was also collected during the pilot survey. In the specification of the scenario for the CV questions, the nature of the good, the conditions under which it will be made available and the possible benefits to be derived by the respondents were described (see appendix). How much land would be covered by the community woodlots and where those woodlots would be located were indicated. In addition to the direct benefits, such as leaves and the final harvest, the respondents were also informed about possible indirect benefits such as reduction in the time they spend collecting fuel and possible reduction in consumption of dung and crop residues as fuel allowing their alternative use as natural fertilisers. Five starting prices, which were chosen based on answers to open-ended questions in the pilot survey, were assigned to respondents randomly and roughly proportionately. The starting prices were Birr 0.5, 1, 2, 5, and 6 per year.[9]

In order to take into account the problem of cash constraints in a semi-subsistence economy, the respondents were given the option of paying in cash or in kind, but most of them responded in cash. The value elicitation format used is discrete with open-ended follow-up questions. The discrete question was asked, based on the randomly assigned prices, excluding the final harvest from the benefit which was then included in subsequent open-ended question(s). A second open-ended question was also asked about willingness-to-pay for a community woodlot twice the size (in terms of the area of land covered) than that for the previous question(s).

4. Empirical analysis and discussion

As noted earlier, we need to check whether or not those who give invalid responses are in some ways different from those who do not, before deciding on whether or not to exclude invalid responses from the analysis. Using comparison of means, section 4.1 examines whether or not households with valid responses have different characteristics than those without. The empirical model and the data are described in section 4.2 while section 4.3 presents results and discussion.

4.1. Invalid responses and analysis of means

Out of 480 filled out questionnaires, about 11 per cent (55) were considered to have invalid responses to the valuation question. As is typically the case for in-person interviews, the item non-response rate was very low (about 1 per cent), and hence the two main reasons for invalid responses were protest zeros and outliers (as defined below). The selection of protest zeros was based on the response to a follow-up question to the valuation questions in which we asked the reasons for those who are not willing-to-pay. Responses stating that the government should pay or stating a dislike for communal property, (about 56 per cent of the invalid responses), were con-

[9] Given the fairly small sample of individuals covered in the pilot survey, we selected only 5 different starting prices.

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sidered protest responses.[10] Outliers, which were about 18 per cent of invalid responses, were determined as those whose willingness-to-pay (WTP) was over 5 per cent of their income and was over Birr 20 (which is over 330 per cent of the maximum starting price used) for the first open-ended question.[11] Though there is an element of arbitrariness, the decision on outliers seems to have excluded the most unreasonably high bids given the general income level of the respondents and the nature and amount of benefits to be derived from the good under consideration. About 16 per cent of the invalid responses were cases where the maximum willingness-to-pay was less than the starting price accepted by the respondent. This was in spite of the fact that the starting price applied to a quantity of the good that was smaller than what the maximum willingness-to-pay question applied to.

As discussed earlier, although the original sample of households was selected randomly, there is no reason to believe a priori that the sample that remains after exclusion of invalid responses is random. And to the extent that there is a systematic and hence non-random difference between cases to be discarded as invalid responses and the rest, there may be inconsistencies in estimated parameters in the valuation function and in aggregated benefits, making inferences invalid (or at least less valid). To test for differences between the two groups, we compared means of variables for the groups; the results are presented in table 1. We can reject the null hypothesis of equal means at the 5 per cent level of significance for starting price assigned to the respondent and household size, and at the 10 per cent level of significance for the number of young females and sex of the household head. In fact the difference in means of starting price in the two groups was significant even at the 1 per cent level.

Table 1. *Comparison of means of variables for respondents with invalid and valid responses to the valuation question*

Variable	*Mean for valid responses*	*Mean for invalid responses*	*t-statistic*[a]
Number of young females[b]	0.61	0.84	1.69*
Household size	4.75	5.42	1.98**
Starting price assigned	2.58	3.36	2.62***
Sex of household head[c]	0.85	0.93	1.92*
Number of observations	425	55	

***, **, * indicate significance at 1%, 5%, and 10% levels respectively.
[a] We first checked the mean values for equality of variances, the result of which was then used to determine the appropriate t-statistic used.
[b] Young females are defined here as those between 7 and 15 years of age.
[c] 1 for males and 0 for females.

[10] Most of those households that were considered as protest responses said the government should pay. Only 6 households said we do not like communal property. The results of the analysis do not change significantly when we consider those who said we do not like communal property as true zeros.
[11] The values of the outliers are 24, 25, 26, 27, 30, 35, 70, and 95. Three households gave a value of 25.

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A non-parametric analysis based upon a Mann–Whitney test suggested similar results. Having found evidence that those giving valid and invalid responses are different, estimation of a sample selection model is appropriate.

4.2. Empirical model and data description

Empirical model

In the empirical analysis of the determinants of willingness-to-pay (WTP), a function is typically estimated using some measure of the response to the WTP question as the dependent variable which is a function of variables expected or assumed to be the determinants of WTP. In our case, given the differences we found earlier between those with invalid and valid responses, we will use a tobit model with selectivity (Greene 1995) to examine more rigorously whether or not the difference is significant while at the same time we use the estimation results to discuss theoretical validity.

The model used takes the following form

$$Y^* = \beta'\mathbf{X} + \varepsilon \tag{1}$$

$$Y = 0 \text{ if } Y^* \leqslant 0$$

and $Y = Y^*$ otherwise

$$Z = \alpha'\mathbf{V} + u \tag{2}$$

$$Z = 1 \text{ if } Z^* > 0$$

and $Z = 0$ if $Z^* \leq 0$.

where Y is a vector of willingness to pay which is censored at 0; $\mathbf{X}$ is a matrix of explanatory variables that are hypothesised to influence willingness to pay; Z is a vector of a dummy variable which is 1 when the observation has a valid response and 0 otherwise; $\mathbf{V}$ is a matrix of explanatory variables which may influence the probability of giving a valid (invalid) response; α and β are vectors of unknown parameters to be estimated corresponding, respectively, to the matrices of explanatory variables V and X; ε and u are disturbances which could be correlated with correlation coefficient ρ and; Y^* and Z^* are latent variables corresponding to Y and Z respectively. Note that values of Y are observed when Z equals 1.

Note that Y is censored at zero for the sub-sample of households that gave valid responses. Thus, estimates obtained using Heckman's two-step estimation procedure, where OLS is used in the second step, would be not only inefficient but also inconsistent (Greene 1995). We therefore use maximum likelihood estimation. The existence of selection bias depends on whether or not there is a significant correlation between the error terms of equations (1) and (2) as measured by estimates of ρ and its standard error.

Data description

The descriptive statistics for variables included in the estimation are presented in table 2. Expecting that household size and age–sex composition could affect willingness-to-pay, these were included as regressors. Income,

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Table 2. *Descriptive statistics*

Variable	*Mean*	*Std. dev.*	*Min.*	*Max.*
Household size	4.75	2.07	1	12
Number of children[a]	0.92	0.95	0	4
Number of male youth[a]	0.64	0.81	0	3
Number of female youth[a]	0.61	0.75	0	3
Number of adult males[a]	1.34	0.87	0	6
Number of adult females[a]	1.24	0.59	0	4
Household income per annum in Birr[b]	1,604	1,340	226	10,482
Distance of homestead to plantation[c]	35	34.2	1	210
Starting price in Birr[b]	2.58	2.1	0.5	6
Value of livestock owned in Birr[b]	1,733	1,339	0	8,278
Number of trees owned	123	257	0	2,000
Sex of household head[d]	0.85	0.36	0	1
Education of head (= 1 if grades 1–6)	0.11	0.31	0	1
Education of head (= 1 if above grade 6)	0.05	0.22	0	1
Distance of homestead to the forest[c]	38.8	39	1	210
Reliance on market for wood consum (%)	0.05	0.22	0	1
Share of wood collected from forest (%)	0.46	0.44	0	1
Land 'owned' in hectares per household	1.22	0.16	0	1
Willingness to pay (Smaller plantation size) in Birr[b]	2.63	3.06	0	18
Willingness to pay (Larger plantation size) in Birr[b]	4.31	5.19	0	30

[a] Children are those below 7 years of age; the youth are between 7 and 15 and adults over 15.
[b] The exchange rate at the time of the survey was 1 USD = Birr 6.34.
[c] Distance is measured as walking distance in minutes.
[d] 1 if head is male and 0 if head is female.
Sample size = 425.

the value of livestock owned, sex, and education level of household head, and land size are another set of variables that are expected to influence willingness-to-pay. We also included measures of access to and/or dependence on other sources of wood for which we included distance of homestead to the forest, reliance on market for woodfuel consumption, share of wood collected from the forest in total wood consumption and number of trees owned by the household. Distance of homestead to the proposed place of plantation was included since it is an important factor expected to negatively influence willingness-to-pay. To test whether or not the stated willingness-to-pay is sensitive to the starting prices randomly assigned to respondents, starting price was also included. Moreover, since we asked willingness-to-pay for two plantation sizes, we also report willingness-to-pay for both the smaller and larger plantation sizes in table 2, the implications of which are discussed at the end of the next subsection.

4.3. Results and discussion

The empirical results for the tobit model with sample selection are presented in table 3. As indicated earlier in the discussion of the empirical model, the significance of ρ determines whether there is sample selection

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Table 3. *Parameter estimates of tobit model with sample selection*[c]

Variable	*Probit coeff.*	*Tobit (Corrected regression)*
Household size	−0.059*	−0.168*
	(0.0328)	(0.101)
Household income per annum		0.00046***
		(0.000157)
Distance of homestead to plantation		−0.0166***
		(0.0064)
Starting price	−0.0232	0.041
	(0.036)	(0.091)
Number of trees owned		0.0014**
		(0.0006)
Sex of household head[a]		1.08*
		(0.64)
Geltima site[b]		−1.37
		(0.97)
Amber site[b]		−2.05**
		(1)
Bulbulo site[b]		−0.005
		(0.97)
Filagober site[b]		−2.41***
		(0.98)
Constant	1.543***	2.06*
	(0.22)	(1.17)
Sigma		3.87***
		(0.15)
Rho		0.869***
		(0.184)
Number of observations	480	
Log likelihood function	−1092.02	

***, **, * indicate significance at 1%, 5%, and 10% levels respectively.
Figures in parentheses are standard errors.
[a] 1 if male and 0 if female.
[b] Note that one of the peasant associations was divided into two sites due to differences in topography and tree cover. Thus, Zemetin is the reference site (or the omitted site).
[c] The dependent variable in the tobit is willingness to pay in Birr, while for the probit, it is 1 for those who give 'valid' responses and 0 for those who do not.

bias due to exclusion of invalid responses. Since, as reported in table 3, ρ is significant at the 1 per cent level, we conclude that exclusion of invalid responses would lead to selection bias and hence the use of tobit models with sample selection is appropriate. Different sets of explanatory variables, listed in table 2, were included in both the probit and the tobit (corrected regression). Out of these, what is reported in table 3 is the preferred model using the likelihood ratio test (and t-test) in the selection of variables. We have also included site dummies in the tobit to examine inter-site differences in willingness-to-pay.

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Starting from the probit coefficients, we see that, although household size and assigned starting price were found to have significantly different means for the two groups (i.e. those with valid and invalid responses) using the Mann-Whitney test, only the former was significant at the 10 per cent level in the probit part of the tobit model with sample selection.

The negative sign of the parameter estimate for household size in the probit suggests that the probability of an invalid response increases with household size. Similarly, although insignificant, the negative sign of the parameter estimate for assigned starting price suggests an increased tendency for respondents to give invalid responses for higher starting prices than lower ones.

In the regression where willingness-to-pay is the dependent variable (column 2 of results in table 3 where tobit estimates are reported after correction for sample selection bias), among household size and age–sex composition variables, only household size was found to be significant with a negative parameter estimate. This suggests that willingness-to-pay for community woodlots decreases as household size increases. This is perhaps because of the effect of availability of labour that can be used to collect from other sources of wood instead of paying for community woodlots.

The parameter estimate for the income variable was significant and positive, as expected, indicating that richer households are willing-to-pay more. The variable that measures distance of homestead to the plantation had the expected significant negative parameter estimate, suggesting that households further away from the proposed place of plantation are willing-to-pay less than those closer to it. Because of lack of land at a central place and due to the scattered nature of rural settlements in our study areas, the walking distance from a household's homestead to the proposed plantation, as reported in table 2, was as much as three and half hours for some households. Thus, other things remaining constant, we would expect such households to be willing-to-pay less than those closer to the plantations.

The parameter estimate for starting prices was positive but not significant suggesting that there is no significant starting point bias. The number of trees owned by a household was found to have a significant positive effect on willingness-to-pay. One may expect that those who have more trees may be willing-to-pay less since it may be a substitute for wood obtained from proposed community woodlots. Such an expectation is based on at least two assumptions: that owners of trees have enough of what they need and that trees are planted only for 'current' consumption. However, there is an alternate viewpoint. Those who grow trees do so at least partly because they know how useful trees are and hence they are likely to support projects, such as the one proposed in this study, that will increase the availability of and access to trees; in this sense, they could be thought of as complements. Moreover, we can think of number of trees owned as proxies for wealth of households. Privately owned trees could also be considered by their owners as savings that could be used during bad days which implies that they would try to use other sources of wood as much as possible before resorting to their own trees. The empirical result obtained here supports these latter arguments.

Sex of household head was another variable found to be significant. Since the parameter estimate is positive, it implies that male headed households tend to be willing-to-pay more than female headed ones. This is perhaps because female headed households tend to be more prone to uncertainties in terms of income since in many cases their income comes from sharecropped out (rented out) land or some other form of non-labour income. They may also prefer to collect wood than pay for wood from community woodlots.

All site dummies were negative, two of them significant, suggesting that households in these sites were in general willing-to-pay less than those in the reference site, *Zemetin*. These differences in results for the different sites may be due to a number of factors, such as different experiences in community woodlots, availability of wood from private or other sources, weather condition, and suitability of the sites for the proposed plantations. These are discussed in the next section.[12]

5. Welfare measures and aggregation

We use mean willingness-to-pay as our welfare measure which implies using the Hicks–Kaldor potential compensation criterion or potential Pareto improvement. The mean is perhaps better than the median since the good dealt with is not a pure public good as exclusion is possible and a voting scheme may not be necessary.

A casual comparison of the average (mean) WTP across sites, reported in table 4, shows that they are different.[13] This conclusion is also confirmed

Table 4. *Average and aggregated benefit measures by site*

Name of site	*No. of households with valid responses*	*No. of households in the site*	*Mean WTP**	*Total WTP**
Amber	74	256	1.84	471
Bulbulo	100	1200	3.68	4416
Filagober	103	800	1.66	1328
Geltima	114	395	2.78	1098
Zemetin	34	223	3.74	834
Total	425	2874	2.63	7559

* The figures are in (Ethiopian) Birr. The exchange rate at the time of the survey was 1USD = Birr 6.34.

[12] The data we have allows for a scope test for within-subjects (and not between subjects). We compared mean WTP for two levels of the good using a t-test and Wilcoxon matched-pairs signed-ranks test. The results show that the difference was significant at 1 per cent level (using both parametric and non-parametric methods) suggesting that respondents were willing-to-pay more for a larger quantity of the good. We should note that while the within-subjects test has a stronger statistical power, the results of such a test may be influenced by respondents' attempt to be consistent. For the latter reason a between-subjects test would be more powerful, although not as powerful statistically as the within-subjects test. See Carson, Flores, and Meade (1996) and Smith (1992).

[13] The issue of sample selection bias which was discussed in the previous section (which also has implications for mean WTP) is taken up in the discussion of benefit aggregation below.

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by both parametric and non-parametric tests of differences between mean WTP. In particular, a Kruskal–Wallis test for differences across the means for all the five sites indicates significance of the differences at the 1 per cent level. Moreover, a Mann–Whitney pair-wise comparison of the means against those for the reference site we used in the previous section (i.e. *Zemetin*) shows that the means for *Amber* and *Filagober* are lower than those for the reference site at the 1 per cent level of significance. The mean for *Geltima* is significantly lower than that for the reference site at the 5 per cent level. On the other hand, the mean for Bulbulo was not significantly different ($p = 0.276$) from that for the reference site. The significance of differences in WTP between *Amber* and *Filagober*, on the one hand, and the reference site, on the other, persists even after controlling for differences in household characteristics, as reported in table 3. An implication of such inter-site differences in WTP is the need to take care in using the concept of benefit transfer where attempts are made to use benefit measures obtained from CV studies in one site to another similar site.

Our study sites are similar in many respects including farming techniques used, form of land ownership, the policy environment under which they operate, etc. However, there are differences in availability of common sources of wood, private wood trees, weather condition, suitability of land for tree planting, and experience with community woodlots, and these may have contributed to the differences in mean WTP across sites. For example, one possible explanation for the differences is greater accessibility to wood from the commons, particularly for *Amber*, where the difference is visible as the reference site is a neighbouring 'village'. On the other hand, the lower WTP for *Geltima* (though the difference is not as significant as for the other two sites) might be due, at least partly, to greater availability of wood both in community woodlots and privately owned ones. Thus, at the very least, one should use the estimated coefficients of the WTP function while doing benefit transfer, instead of considering the mean values directly from the raw data.

An important issue related to the measurement of welfare using WTP is aggregation of benefits. Mitchell and Carson (1989: 263) discuss the following four issues related to sample design and execution which should be examined in order to assess viability of benefit aggregation: population choice bias, sampling frame bias, sample non-response bias and sample selection bias. From these we can expect sample selection bias due to exclusion of invalid responses to be significant in this study.[14]

The literature on estimation of aggregate benefits using samples seems to have focused on the issue of non-responses and expanding benefit estimates for a population that is not necessarily the one sampled. This is, for example, what Loomis (1987) considers in a review of the practices in

[14] Since we used a random sampling procedure in the selection of the sample from a sampling frame which consists of a comprehensive list of households in the study sites (which is our population), we expect population choice bias and sampling frame bias to be insignificant. Moreover, as reported earlier, sample non-response was very small which was partly because we used in-person interview.

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expanding contingent value sample estimates to aggregate benefit estimates. In the presence of a non-random sample, Edwards and Anderson (1987) consider the issue of generalizing from the sample to the population as a sample selection problem and propose the use of Heckman's two-step procedure for detection and correction of the problem. Our earlier argument about inconsistency due to sample selection bias also applies to the issue of aggregation of benefits. This is because the existence of selection bias, due to exclusion of invalid responses, implies that in addition to the parameter estimates of the WTP (valuation) function, the mean WTP for the sample measured using only valid responses may also be incorrect. The aggregate benefit measures should therefore be compensated for the sample selection bias, or at least the presence of the bias, and its implications recognized.

6. Conclusions

In this paper we used the contingent valuation method to analyse the determinants of household valuation of community forestry in rural Ethiopia. Unlike most other studies, we use a tobit model with sample selection in the empirical analysis of the bid function to look into the effect of excluding invalid responses (protest zeros, outliers, and missing bids) from the analysis. The results of this study showed that income, household size, distance of homestead to plantation, number of trees owned, and sex of household head are important variables that explain WTP for community woodlots in rural Ethiopia. A comparison of WTP by site indicates that there are significant differences. An important policy implication of these results is the need to consider household and site specific factors when designing and implementing community forestry projects. In contrast to past practices in Ethiopia where the major decisions related to the establishment, management, and use of community plantations were made by non-members of the community, it is important to consider making the community the initiator, manager, and user of such plantations by selecting those that are more likely to support community plantations. Studies such as this one may contribute to identifying such groups. It should also be noted that such studies should also be complemented by other studies such as an examination of the costs of community plantations and a comparison of these costs with benefits measured using revealed, as opposed to stated, preference, and selection of value elicitation formats in a given cultural setting.

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APPENDIX. Willingness-to-pay (WTP) questions for new village woodlots (VWL)

You still have some land in your village that could be used for a VWL. The government does not have money to plant VWLs. If the village decided to plant a woodlot anyway it would be a (1.) site situated at _____, which is about ____ minutes away from your homestead. It would be a mixed plantation with many species but mainly Eucalyptus.

The benefits you would receive from this plantation would include the leaves and if the plantation is well managed it will also give a valuable harvest.

You should also consider the other benefits that arise from the plantation: that the pressure decreases on the natural forest and that the time you spend collecting fuel will be reduced significantly. You could also reduce your consumption of dung and crop residues as fuel which can then be used as natural fertilisers to your fields.

Considering all this, except the value of the final harvest, would your household be willing to pay (2.) ____ Birr per year or its equivalent in kind to establish and keep the proposed plantation?
3. Yes ... 1, No ... 2 _____
4. What is the maximum amount per year you would be willing to pay to establish and keep the plantation if you include the value of the harvest, equally divided among villagers?

If in cash, Birr	If in kind		
	Crop code **(a)***	Amount in kind	Unit **(b)***

5. What is the maximum amount per year you would be willing to pay to establish and keep the plantation if it instead was _____?

If in cash, Birr	If in kind		
	Crop code **(a)***	Amount in kind	Unit **(b)***

6. (If no WTP) why don't you pay anything for the establishment of the VWL? **(c)*** _____

7. If you instead had the option to contribute with labour, how much is the maximum number of labour days your household would be willing to supply per year to establish and maintain the plantation?
_____ days per year to establish.
_____ days per year for 10 years to maintain.

8. Remarks (if any) _____

* (a), (b) and (c) indicate where the list of codes (which are not reported here) for crops, units (of measurement) and reasons for not willing to pay, respectively, were presented immediately before the page in the questionnaire that contains the WTP questions to make it easier for interviewers.

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