

## 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<sup>1</sup>.

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

---

<sup>1</sup>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<sup>2</sup>. 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

---

<sup>2</sup>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<sup>3</sup>, such as the GTZ and JICA, as well as NGOs, including Farm

---

<sup>3</sup> 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., 2008 and 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

---

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)<sup>4</sup>.

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

---

<sup>4</sup>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)<sup>5</sup>. 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

---

<sup>5</sup>Primary users are those who use the forest more frequently, permanently or directly, whereas secondary users are those using 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<sup>6</sup>. 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)<sup>7</sup>. 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<sup>8</sup>. 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

---

<sup>6</sup>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

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

<sup>8</sup>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  $\tau = E[Y_{1t} - Y_{0t}] = \bar{Y}_1 - \bar{Y}_0$ . 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,

$$E(Y_{1t} - Y_{0t} | D_t) = E(Y_{1t} - Y_{0t} | D_t = 1) + E(Y_{0t} | D_t = 1) - E(Y_{0t} | D_t = 0),$$

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,  $Y_{1t}$  and  $Y_{0t}$  are independent of treatment  $D_t$ , given a vector of covariates  $X_t$ , yields a matching estimator for the average effect of treatment on the treated. Compactly, this assumption is denoted as  $(Y_{1t}, Y_{0t}) \perp D_t | X_t$ ; 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,  $(Y_{1t}, Y_{0t}) \perp D_t | X_t \Leftrightarrow (Y_{1t}, Y_{0t}) \perp D_t | P(X_t)$ , where  $P(X_t)$  is the propensity score, or propensity to treat, commonly estimated via logit regression. In other words,  $P(X_t) = E(D_t = 1 | X_t) = \mathbb{I}(X_t\beta + v_t > 0)$ , where  $\mathbb{I}$  represents a binary indicator function. To identify the average effect of treatment on the treated, in addition to the unconfoundedness assumption,  $(Y_{1t} | D_t = 1) \perp D_t | P(X_t)$ , overlap is also necessary, i.e.,  $0 < P(X_t) < 1$ . 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<sup>9</sup>. 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

---

<sup>9</sup>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  $D_{it}$ , as well as a binary instrumental variable,  $Z_{it}$ . Finally, the data includes a  $k \times 1$  vector of covariates  $X_{it}$  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  $D_{1it} > D_{0it}$  represents the complying subpopulation,  $D_{1it} = D_{0it} = 0$  are the never-takers,  $D_{1it} = D_{0it} = 1$  are the always-takers and  $D_{1it} < D_{0it}$  ( $D_{1it} = 0, D_{0it} = 1$ ) represents the defiant subpopulation. Across these subpopulations, a number of assumptions are made. These assumptions include: (i) Conditional independence –  $(Y_{1it}, Y_{0it}, D_{1it}, D_{0it}) \perp Z_{it} | X_{it}$ , (ii) Monotonicity –  $P(D_{1it} < D_{0it}) = 0$ , (iii) Complier existence –  $P_c(D_{1it} > D_{0it}) > 0$ , (iv) Nontrivial assignment, or common support –  $0 < P(Z_{it} = 1 | X_{it}) < 1$  and (v) The existence of a first stage –  $P(D_{it} = 1 | Z_{it} = 1) \neq P(D_{it} = 1 | Z_{it} = 0)$ .

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  $Z_{it}$ ,  $\forall i$  or weakly decreases with  $Z_{it}$ ,  $\forall i$ . 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).

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

*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<sup>10</sup>. 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

---

<sup>10</sup>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<sup>11</sup>. 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)<sup>12</sup>, 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

---

<sup>11</sup>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.

<sup>12</sup>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<sup>13</sup>. 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

---

<sup>13</sup>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 Table 4.4. 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,  $\Gamma^{14}$ . 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

---

<sup>14</sup>For ease of exposition, let the probability of program participation be given by  $P(x_i, u_i) = P(d = 1 | x_i, u_i) = e^{\beta x_i + \gamma u_i}$ . 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  $\gamma$ . 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  $\gamma$  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  $e^{\gamma}$  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,  $\Gamma$  measures the degree of departure from the random treatment assignment assumption that is inherent in matching (Kassie et al., 2011). If departure occurs at  $\Gamma$  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.<sup>15</sup> 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.

---

<sup>15</sup> 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 particular, the data used in chapter 2 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<sup>16</sup> 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

---

<sup>16</sup> 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 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 difference
		Mean	SE	Mean	SE	
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 (0.011)	-0.002 (0.002)
Household head's gender	-0.336 (0.553)	-0.083 (0.137)
Household head's education	0.022 (0.052)	0.005 (0.012)
Female labour force	0.848*** (0.307)	0.208*** (0.075)
Male labour force	-0.230 (0.258)	-0.056 (0.063)
Land holding size in ha	0.010 (0.085)	0.002 (0.021)
Off-farm employment	0.842* (0.490)	0.207* (0.115)
Distance to agro extension office	-0.004* (0.002)	-0.001* (0.001)
Woodlot ownership	-0.511* (0.282)	-0.125* (0.068)
Livestock holding size in TLU	0.122*** (0.049)	0.030** (0.012)
Distance from PFM forest	-0.028*** (0.005)	-0.006*** (0.001)
Experience of other collective action	1.400*** (0.509)	0.329*** (0.103)
Distance from nearest town	-0.005* (0.003)	-0.001* (0.001)
Distance from nearest road	-0.008** (0.004)	-0.002** (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*	pseudo-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=0.0025)	548.53	148.61	3.69**
Radius matching (r=0.005)	548.53	150.92	3.63**
Kernel matching(bwidth=0.01)	103.17	1150	0.09
Kernel matching(bwidth=0.0025)	548.53	152.84	3.58***
Kernel matching(bwidth=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 ( $\Gamma$ )	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  $\Gamma=1.7$  when p-values shift 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 analysis by 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.