UNIVERSITEIT VAN PRETORIA
UNIVERSITY OF PRETORIA
YUNIBESITHI YA PRETORIA

Welfare and Common Property Rights Forestry: Evidence from Ethiopian Villages

Dambala Gelo and Steven F. Koch

Working paper 277

March 2012

Welfare and Common Property Rights Forestry: Evidence from Ethiopian Villages*

Dambala Gelo† and Steven F Koch‡

March 30, 2012

Abstract

In this study, welfare impacts associated with a unique common-property forestry program in Ethiopia were examined. This program is different from other programs, because it is 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, southwestern Ethiopia. The program was found to raise the welfare of the average program participant households. Correcting for selection into the program led to both increased welfare and less precise estimates, as is common in IV analyses. The analysis 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 results suggest 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.

JEL Classification: Q23, Q28

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

*We are grateful to SIDA for financing the data collection of this project through EfD (EDRI/EEPFE). We are also grateful for comments received from Abebe Damte, Chitalu Chama and Gauthier Tshiswaka-Kashalala. The usual declaimer applies.

†PhD Recipient, Department of Economics, University of Pretoria, e-mail: daatu24@yahoo.com

‡Professor and HOD, Department of Economics, University of Pretoria, Private Bag X20, Hatfield 0028, Republic of South Africa, steve.koch@up.ac.za, (O) 27-12-420-5285.

1 Introduction

In recent decades, the devolution of natural forest management to local communities in several countries has become widespread, underpinned by a growing recognition that management decentralization is a low-cost policy instrument for natural forest stewardship. In other words, 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 plantied in trails and 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 is larger. Moreover, behavioural studies by Edmonds (2002), Yadev et al. (2003) and Bluffstone (2008) report reduced forest resource extraction efforts by programme 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 help control soil erosion and flooding, resulting in an increase in land productivity. Similarly, increased forest stocks reduce collection times associated with both timber products and non-timber forest products, potentially unleashing labour for other purposes. From a program perspective, on the other hand, government policies that support local organization, improved decision-making related to forest use, and increased local forest user participation in forest product markets, is likely to increase the returns associated with other household assets.[1]

However, the literature has not reached a consensus with respect to the previously described welfare benefits, partly because community forestry program development 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 (DID) panel study does find that decentralization has a positive impact on total income in China.

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

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. For welfare analysis, both matching and IV methods were ap-

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

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

plied 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.[3] These increased opportunities arise from complementary policy measures meant to help forest users access profitable market niches.

Therefore, this study contributes 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, Ethiopian Birr (ETB) 336.73, 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 2 discusses the background to common property forestry in Ethiopia, as well as the context of the study. Section 3 describes the data collection efforts, while Section 4 discusses the econometric framework that informed the empirical strategies. Section 5 presents results and discusses those results. Finally, Section 6 concludes the analysis.

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,

[3] Although it would be better to disentangle the impacts of each component of the program, by, for example, considering intermediate outcomes such as social network connections and access to markets, doing so is not possible in this study, as the requisite data is not available.

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 a certain level dependence on forest cover to protect against soil fertility is inevitable for some years to come. 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 resource, 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,[4] such as the GTZ and JICA, as well as NGOs, including Farm 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, Farm Africa/SOS-Sahel implemented participatory forestry management (PFM); more than six PFM programs have been established to improve the management of about 80,066 ha of natural forest (Jirane et al., 2008).[5]

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 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 prob-

[4] 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 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.

[5] 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).

lems, 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).[6]

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 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).[7] 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 household 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, which is driven, in large part, by program eligibility, i.e., whether there was a Menja population in the region.

Experts from Farm Africa/SOS-Sahel and local governments, in collaboration with FUG members, developed the Forest Management Plan (FMP), which includes forest protection, forest development, harvest quotas and benefit share

[6] 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.

[7] 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).

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.[8] 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).[9] 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.[10] 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.

3 Methodology

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

[8] 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.

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

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

propriately 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, as measured by per capita expenditure, associated with either participation in the program or maintaining the *status quo.* If farmer (household) $\{1, 2, ..., N\}$ chooses to participate ($D_i = 1$) in PFM, the relevant household outcome is $Y_{1i}; Y_{0i}$ is the relevant outcome for non-participating ($D = 0$) households. Importantly, only one of these outcomes is observed for any household, and, therefore, in regression format, $Y_i = Y_{0i} + D_i(Y_{1i} - Y_{0i}) + \eta_i = \alpha + \tau D_i + \eta_i$. If $Y_i \perp D_i$, as would be true in a randomized controlled trial, the impact of the program on household outcomes would be obtained from $\tau = E[Y_{1i} - Y_{0i}] = \bar{Y}_1 - \bar{Y}_0$. 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.

3.1 *Matching*

More generally, $E(Y_{1i} - Y_{0i} \lfloor D_i = 1) = E(Y_{1i} - Y_{0i}|D_i = 1) + E(Y_{0i}|D_i = 1) - E(Y_{0i}|D_i = 0)$, where the first term represents the average effect of PFM on the program participant, and the last two terms measure the effect of participation. Assuming positive sorting, such that farmers expecting to benefit from PFM choose to participate in PFM, the participation effect is expected to be positive, and, therefore, ignoring selection in the analysis would lead to positively biased treatment effects. However, assuming that the distribution of welfare outcomes, Y_{1i} and Y_{0i} are independent of treatment D_i, given a vector of covariates X_i, yields a matching estimator for the average effect of treatment on the treated. Compactly, this assumption is denoted as $(Y_{1i}, Y_{0i}) \perp D_i|X_i$; see Rubin (1973), Rosenbaum and Rubin (1983), Heckman, Ichimura, Smith and Todd (1998), Dehejia and Wabba (1999). Intuitively, the goal of matching is to create a control group of non-PFM participants that is as similar as possible to the treatment group of PFM participants, although the groups differ in terms of their participation.

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

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, 2005). 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). Furthermore, it is common to allow for more than one NN match. In this study, we allow for between one and five matches.

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 distance lies within the caliper and is the closest, in terms of the propensity scores (Caliendo and Kopeinig, 2005). 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 use of observations that may be bad matches (Caliendo and Kopeinig, 2005).

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 Imens 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 some 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, a binary variable, namely the presence of Menja people in one's village, is used as an indicator of the household's intention to treat, i.e., the presence of Menja people in the village is assumed to partly determine participation in the PFM, but not affect welfare, except through participation. 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 the PFM.[11] The exclusion restriction, although untestable, as in all IV applications, warrants further discussion. As the presence of the Menja tribe is associated with program eligibility, and the goal of the program was to improve the forest and household welfare outcomes, it is likely that eligible households were generally worse off than ineligible households. In that sense, any bias due to a violation in the exclusion restriction would tend to yield understated welfare impacts. For an upward bias to obtain in the analysis, the presence of the Menja tribe would need to be associated with better welfare outcomes for eligible households than ineligible households, which is likely if the intention to treat – forestry selection and training – is confounded. In particular, Menja settlement may follow other covariates, such as village access to roads, access to markets and the underlying condition of the forest, each of which can be related with the outcome through their effect on household income. Therefore, following Abadie et al. (2002), Abadie (2003) and Frölich (2007), IV exogeneity is assumed to obtain, upon conditioning over these covariates.

In what follows, we more carefully describe the causal effect of interest. The data is comprised of n observations, and the outcome variable Y_i, per capita expenditure, is continuously distributed. There is a binary treatment variable, denoted by D_i, as well as a binary instrumental variable, Z_i, representing the presence of Menja in the study village. Finally, the data includes a k x *1* vector of covariates X_i for each household. For concreteness, the following identification assumptions advanced by Abadie et al. (2002), Abadie (2003), Frölich (2007) and Frölich and Melly (2008) are outlined. To begin, the study population is partitioned according to treatment and eligibility, such that $D_{1i} > D_{0i}$ represents the complying subpopulation, $D_{1i} = D_{0i} = 0$ are the never-takers, $D_{1i} = D_{0i} = 1$ are the always-takers and $D_{1i} < D_{0i}$ represents the defiant subpopulation. Across these subpopulations, a number of assumptions are made. These assumptions include: (i) Conditional independence – $(Y_{1i}, Y_{0i}, D_{1i}, D_{0i}) \perp Z_i|X_i$, (ii), Monotonicity $-P(D_{1i} < D_{0i}) = 0$, (iii) Complier existence $-P_c(D_{1i} > D_{0i}) > 0$, (iv) Nontrivial assignment, or common support $-0 \leq P(Z_i = 1|X_i) \leq 1$ and (v) The existence of a first stage $-P(D_i = 1|Z_i = 1) \neq P(D_i = 1|Z = 0)$.

[11] 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.

Assumption (i) is a standard exclusion restriction, although it is conditioned on an additional set of covariates. Monotonicity, assumption (ii), requires that treatment either weakly increases with $Z_i \forall_i$ or weakly decreases with $Z_i \forall_i$. In our case, a household initially not in an eligible village would not be less likely to participate if the village were to become eligible, where eligibility is based on the presence of Menja. Assumption (iii) implies that at least some individuals react to treatment eligibility – the weak inequality is strict for some households – 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, i.e., that eligibility at least partly predicts participation. If these assumptions hold, the $LATE$ is identified; see Frölich (2007) and equation (1).

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

$LATE$ 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 The Data

Data for the analysis was obtained from a household survey undertaken in 10 Ethiopian villages, in October of 2009. The survey was designed for this study. 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. The selected non-PFM village was the closest available non-PFM village to the selected PFM village. Sample frames for the survey were derived from the selected villages via the lower level of local government, the kebele. We randomly selected 200 households from PFM villages and 177 from non-PFM villages.[12] The analysis was based on these households.

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

[12] Table 6 outlines the kebeles, villages, both PFM and non-PFM and number of survey respondents.

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 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 1 is the relevant column. As expected, 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.

For the sake of this study, we used per capita consumption expenditure rather than income as a welfare measure for the following reasons. First, by virtue of consumption smoothing, consumption expenditure fluctuates less in the short run compared to income. Second, consumption expenditure provides information over the consumption bundle that fits within the household's budget, although credit market access and household savings affect that budget (Skoufias and Katatyama, 2010). As such, consumption is generally believed to provide better evidence of the standard of living than income. Third, an income survey may not capture informal, in-kind or seasonal income and may be more susceptible to under-reporting. Unfortunately, the choice of per capita expenditure is not without problems. It would have been preferred to measure it as an adult equivalence, which takes into account differences between children and adults in terms of their nutritional and other requirements. However, inaccuracies in adult equivalent expenditure would result in sizable measurement errors, limiting its usefulness. Furthermore, per capita expenditure offers the benefit of ease of interpretation, and, hence, is widely used (Skoufias and Katayama, 2010).[13]

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.[14] 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.

5.1 *Matching*

Before turning to the results, the underlying premises of matching – unconfoundedness and overlap – must be considered. Table 1, previously alluded to, 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 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 algorithm is potentially guided by a broad set of criteria, primarily concerned with the quality of the match. Roughly speaking, that quality 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 3 provides information related to the quality of the different matching algorithms.[15] According to the results

[13] One might also consider other measures of welfare, such as happiness, which did not form part of the survey, time devoted to the collection of forest products, as done by, for example, Bluffstone (2008), but also not available in this data, or livestock holdings, as livestock holdings also relate to consumption smoothing, and, therefore, welfare. Program effects on livestock are considered in a companion piece. One could also consider disaggregated expenditure; however, disaggregated expenditure did not form part of the survey.

[14] According to Table 1, this difference is ETB45.40; however, it is insignificant.

[15] 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

reported in Table 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),[16] 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 treatment effects are available in Table 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 household per capita expenditure. The first row of the table repeats the estimate from the second row of Table 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 the program participants is estimated to range ETB295.68 to ETB 548.53, and each of the estimates are significantly different from zero.[17] 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.

5.2 *Matching Sensitivity and 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 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 5. The first column of the table contains an odds ratio measure of the degree of departure from the outcome that is

complete 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.

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

[17] Note that the present value of the ATT estimate multiplied by the size of relevant population yields the total benefit of the program interventions. Although comparing this quantity with the program cost would allow us to evaluate the cost-effectiveness of the program, we do not know the cost of the program for this region. However, the results could be used to create a cost-effectiveness measure, if the evaluator was willing to assume that the treatment effect was constant across the entire population. Furthermore, the results can be used to encourage participation amongst previously skeptical rural households.

assumed to be free of unobserved bias, Γ.[18] 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 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 preceding 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. These empirical strategies, results available in the last two rows of Table 4, yielded relatively higher estimates, compared to those previously reported. However, LATE applies only to the population of compliers, which is 46% of program participants, whereas matching applies to nearly the entire programme 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.

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

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

one-third of total welfare. This welfare effect has arisen from either rent generation associated with the property right regime change,[19] as proposed by Adhikari (2005), Murty (1994), Caputo (2003) and Cooper (2008), 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 to the positive welfare benefits that can be achieved from common property forestry programs that are reinforced with improved market linkages for forestry products.

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 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. Essentially, decentralized forest management combined with improved market integration for NTFPs provides alternative avenues for income generation, thus promoting rural development. However, the preceding research has not provided any information related to program impact equity, although Gelo and Koch (2011) argue that this program is not equity enhancing.

6 Conclusion

Previous studies that have evaluated the welfare impacts of common property forestry programs, have found 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. In comparison to the programme evaluations 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, as done here, revealed that common property forestry intervention has raised the average welfare, of participating households in the study villages. The results from the matching analysis revealed that the program has raised the welfare of the average program participant by ETB336.73. After controlling for endogeneity bias through IV, however, the programme was found to have raised the welfare of the average program complier by between ETB567.33 and ETB645.16.

Two policy implications were inferred from this evidence. First, the decentralization of natural forest management in combination with greater market

[19] The theoretical property rights literature maintains that common property rights generate resource rents and avoid rent dissipation that would occur under an open access regime.

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 currently operating under an open-access property right regime can raise rural household welfare, as measured by per capita expenditure. However, the results have not provided any evidence with respect to equity, a concern that warrants further study.

References

[1] Abadie , A., Angrist, J., and Imbens,G. (2002) Instrumental variables estimates of the effects of subsidized training on the quantile of trainee's earning, *Econometrca,* 1: 91-117

[2] Abadie , A. (2003) . Semi-parametric instrumental variable estimation of treatment response model. *Journal of Econometrics,* 113: 231-263.

[3] Adhikari, B. (2005). Poverty, property rights and collective action: Understanding the distributive aspects of common property resources management. *Environment and Development Economics,* 10: 7-31.

[4] Adhikari, B.(2004). Household characteristics and forest dependency: Evidences from common property forest management in Nepal. *Ecological Economics,* 48: 245-257.

[5] Agrawal, A. and Gibon, C. (1999). Enchantment and disenchantment: The role of community in natural resource conservation. *World Development,* 27: 629-49.

[6] Agrawal, A., and Ostrom., E. (2001). Collective action, property right and decentralization in resource use in India and Nepal. *Politics and Society,* 4: 485-514.

[7] Angelsen, A., and Wunder, S. (2003). Exploring forestry-poverty link: key concepts. Issues and research implications, CIFOR Occasional Paper, vol.4, CIFOR, Bogor.

[8] Angrist, J., Imben, G., and Rubin. D. (1996). Identification of causal effects using instrumental variables. *Journal of American Statistical Association*, 91: 444-472. Birthday

[9] Angrist, J., Graddy, K., Imben. G. (2000). The Interpretation of instrumental variable estimator in simultaneous equations models with an application to demand for fish. *Review of Economic Studies* 64: 499-527.

[10] Basundhara, B., and Ojhi, H., (2000). Distributional impact of community forestry: who from Nepal's community forests? *Forest Action Research Series* 00/01.

[11] Becker, S.O., and Caliendo, M. (2007). Sensitivity analysis for average treatment effects.*Stata Journal*, 7: 71–83.

[12] Bekele, M., and Bekele, T. (2005). Participatory Forest Management in Chilimo and Bonga, Ethiopia. An Evaluation Report, Farm Africa, Addis Ababa.

[13] Blomley, T., Pfliegner, K., Isango, J., Zahabu, E., Ahrends, A., and Burgess, N. (2008). Seeing the wood for the trees: An assessment of the impact of participatory forest management on forest conditions in Tanzania. *Fauna and Flora International* 42: 380-391.

[14] Bluffstone, R. (2008). Does better common property forest management promote behavioral change? On farm tree planting in the Bolivian Andes. *Environment and Development Economics,* 13: 137-170.

[15] Cameron, A.C., and Trivedi, P.(2005). *Microeconometrics: methods and applications*. Cambridge University Press.

[16] Caliendo, M. , and Kopeinig, S. (2005). Some practical guideline for the implementation of propensity score matching. *IZA Discussion Paper No. 1588*.

[17] Caputo, M., and Luek, D. (2003). Natural resources exploitation under common property right. *Natural Resource Modeling,* 16(1).

[18] Cobb-Clark , D.A., and Crossley, T. (2003), Econometrics for evaluation: An introduction to recent development. *The Economic Record,* 79: 491-511.

[19] Cooper, C. (2007). Distributional consideration of forest co-management in heterogeneous community: Theory and simulation. *Department of Economics Working Paper*, University of Southern California.

[20] Cooper, C. (2008). Welfare effects of community forest management: Evidences from the hills of Nepal. *Department of Economics Working Paper*, University of Southern California.

[21] Dasgupta, P. (2006). Common property resources as development drivers: A study of fruit cooperative in Himachal Pradesh, India. *SANDEE Working Paper No. 15-06*.

[22] Dehejia, R. H., and Wahaba,S. (1999). Causal effects in non-experimental studies: Reevaluating the evaluation of training programs, *Journal of the American Statistical Association*, 94: 1053-1062 Dehejia, R. H., and Wahaba,S (2002). Propensity Score Matching Methods for Non-Experimental Causal Studies *Review of Economics and Statistics* 84: 151–61.

[23] Delacote, P. (2007). Agricultural expansion, forest products as safety net and deforestation. *Environment and Development Economics*, 12: 235-249.

[24] Edmonds, E. (2002). Government-initiated community resources management and local resource extraction from Nepal's forests. *Journal of Development Economics,* 68:89-115

[25] Gobeze T., Bekele, M., Lemenih. M., and Kassa. H. (2009). Participatory Forest Management impacts on livelihood and forest status: The case of Bonga forest in Ethiopia. *International Forestry Review,* 11:346-358.

[26] Frölich, M. (2007). Nonparametric IV estimation of local average treatment effects with covariates. *Journal of Econometrics,* 139: 35-75.

[27] Frolich, M., Melly. B. (2008). Unconditional quantile treatment effect under endogeneity, *IZA Discussion paper* No. 3288

[28] Gauld, R. (2000). Maintaining centralized control in community based forestry: policy construction in the Philippines. *Development and Change,* 31: 229-254.

[29] Gelo, D., and Koch, S. (2011). Do rural households gain from common property forestry programs? QTE evaluation from Ethiopian villages. unpublished *manuscript*, Department of Economics, University of Pretoria.

[30] Heckman, J.J., Ichimura, H., Smith, J., and Todd, P. (1998). Characterizing selection bias as using experimental data. *Econometrica,* 66: 11017-1098.

[31] Heckman, J., LaLonde, R., Smith, J. (1999). The economics and econometrics of active labour market programs. *The Hand Book of Labor Economics,* North Holland, New York, pp: 1865-2097.

[32] Hirano, K., Imbens, G., Rubin, D., and Zhou, X.(2000). Assessing the effect of influenza vaccine in an encouragement design. *Biostatistics* 1: 69-88.

[33] Human Resources Development Canada, Evaluation and Data Development Branch. (1998). Quasi-experimental evaluation, SP-AH053E-01-98.

[34] Imbens, G., and Angrist, J. (1994). Identification and estimation of local average treatment effects. *Econometrica,* 62: 467-475.

[35] Jirane , T., Tadesse, T., and Temesgen, Z. (2008), PFM in Oromia and SNNP regions of Ethiopia: A review of experiences, constraints and implications for forest policy, in J. Bane, S. Nune, A. Mekonnen and R. Bluffstone (eds.), *Policies to increase forest cover in Ethiopia*, Proceedings of Policy Workshop, Environment for Development Environmental Economics Policy Forum for Ethiopia, Ethiopian Development Research Institute, Global Hotel, Addis Ababa.

[36] Jumbe, C., and Angelsen, A., 2006. Do poor benefit from devolution? Evidence from Malawi co-management programs. *Land Economics,* 84: 562–581

[37] Kassa, H., Campbell, B., Sandwall, M., Kebede, M., Tesfaye, Y., Dessie, G., Seifu, A., Tadesse, M., Garedew, E., and Sandwall, K. (2009). Building a future scenario and uncovering persisting challenges in Chilimo forest, Central Ethiopia *Journal of Environmental Management,* 90: 1004-1013

[38] Kassie, M., Shiferaw, B., and Muricho, G. (2010). Adoption of and impact of improved ground nut variety on rural poverty: evidence from rural Uganda. *EfD Discussion paper, series 10-11*

[39] Klooster, D., and Masera, O. (2000). Community forest management in Mexico: Carbon mitigation and biodiversity conservation through rural development *Global Environmental Change,* 10: 259-272

[40] LaLond, R., Heckman, J., and Smith, J (1999).The Economics and Econometrics of Active Labor Market Policies; in *The Handbook of Labor Economics*, eds. Orley Ashenfelter and David Card, Amsterdam: North-Holland

[41] Lee, J. (2005). Micro-econometrics for policy, programme and treatment effects.Oxford University Press, New York.

[42] Lemenih, M., and Bekele., M. (2008). Participatory forest management, best practices, lessons and challenges encountered: The Ethiopian and Tanzanian Experiences An Evaluation Report, Farm Africa, Addis Ababa.

[43] Mekonnen, A., and Bluffstone, R. (2007). Policies to increase forest cover in Ethiopia: lessons from economics and international experience. in J. Bane, S. Nune, A. Mekonnen and R. Bluffstone (eds.), *Policies to increase forest cover in Ethiopia*, Proceedings of Policy Workshop, and Environment for Development-Environmental Economics Policy Forum Ethiopia, Ethiopian Development Research Institute, Global Hotel, Addis Ababa.

[44] Mullan, K., Kontoleon, A., Swanson, T.M., and Zhang, S. (2009). Evaluation of the impact of the natural forest protection on rural livelihoods, *Environmental Management*, 45: 513-525

[45] Murty, M.N. (1994). Management of common property resources: Limits to voluntary collective action, *Environment and Resources Economics,* 4: 581-594.

[46] Nagendra, H. (2002). Tenure and forest conditions: Community forestry in the Nepal Terai, *Environmental Conservation,* 29: 530-539.

[47] Neumann, M. (2008). Participatory forest management in Oromia region of Ethiopia: a review of experience, constraints and implications for forest policy. in J. Bane, S. Nune, A. Mekonnen and R. Bluffstone (eds.), *Policies to increase forest cover in Ethiopia*, Proceeding of Policy Workshop, and Environment for Development- Environmental Economics Policy Forum Ethiopia, Ethiopian Development Research Institute, Global Hotel, Addis Ababa.

[48] Neupane, H. (2003). Contested impact of community forestry on equity: Some evidence from Nepal. *Journal of Forests and Livelihood,* 2: 55-62.

[49] Nune, S. (2008). Ethiopian Government efforts to increase forest cover: a policy oriented discussion paper, in J. Bane, S. Nune, A. Mekonnen and R. Bluffstone (eds.), Policies to increase forest cover in Ethiopia, Proceeding of Policy Workshop, Environment for Development - Environmental Economics Policy Forum for Ethiopia, Ethiopian Development Research Institute, Global Hotel, Addis Ababa.

[50] Rosenbaum, P.R., and Rubin, D.B. (1983). The central role of propensity score in observational studies for causal effects. Biometrika, 70: 41-75.

[51] Rosenbaum, P.R. (2002). *Observational Studies*. Springer, New York.

[52] Rosenbaum, R.P. (2005). Sensitivity analysis in observational studies *Encyclopedia of Statistics in Behavioral Science,* 4: 1809–1814.

[53] Roy, A.D. 1951. Some Thoughts on the Distribution of Income. *Oxford Economic Papers* 3: 135–46.

[54] Rubin, D. (1973). Matching to remove bias in observational studies, *Biometrics*, 29: 159-183.

[55] Sianesi, B. (2004). An evaluation of the active labor market programs in Sweden. *Review of Economics and Statistics,* 86:133-15.

[56] Skoufias, E., and Katayama, R.S. (2010). Sources of welfare disparities between and within regions of Brazil: Evidences from the 2002- 2003 household budget survey (POF), *Journal of Economic Geography*, pp. 1-22

[57] Smith, J., and Todd, P. (2005). Does matching overcome LaLonde's critique of non-experimental estimators? *Journal of Econometrics,* 125: 305-353

[58] Sunderlin, W., Angelsen, A., Belcher, B., Burger, P., Nasi, R., Santoso, L., and Wunder, S., (2005). Livelihood, Forests and conservation in developing countries. *World Development,* 9: 1383-1402.

[59] World Bank. (1994), Calub Gas Development Project, SAR

[60] World Bank. (2005). The Little Green Data Book. Washington, DC: World Bank.

[61] World Bank. (2008). Poverty and the environment: Understanding the linkage at the household level. Washington DC, World Bank.

[62] Wunder, S. (2001). Poverty alleviation and tropical forests—what scope for synergies? *World Development,* 11: 1817-1833.

[63] Yadev, N.P., Dev, O.P., Springate-Baginski O. and Sousan, J. (2003). Forest management and utilization under community forestry. *Journal of Forests and Livelihood,* 1: 37-50.

[64] Yau, L., Little, R. (2001). Inference for complier-average causal effect from longitudinal data and missing data with application to a job training assessment for the unemployed. *Journal of American Statistical Association,* 96: 1232-1244.

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

Variable	Description	PFM participant		Non-participant		
		Mean	SE	Mean	SE	Mean difference
totexp	Total household consumption expenditure in Ethiopian Birr (ETB)	9531.32	389.593	9000.756	337.464	530.564
cpc	Per capita consumption expenditure in Ethiopian Birr (ETB)	1732.09	66.5836	1686.69	59.263	45.397
ageb	Age of household head	36.905	0.997	35.887	1.030	1.017
gender	Household head gender	0.932	0.018	0.943	0.016	-0.010
hhedu	Education (grade attained) of household head	2.290	0.218	2.352	0.229	-0.061
dstpfm	Household distance to programme forest (in minutes)	23.083	2.042	65.85	4.962	-42.768***
offrmb	Whether a household participated in off-farm employment (yes=1)	0.128	0.025	0.071	0.018	0.057*
lndsz	Household landholding size in hectare	2.275	0.125	2.381	0.122	-0.106
wdlot	Whether a household owned private woodlot (yes=1)	0.497	0.037	0.530	0.035	-0.033
tlub	Household livestock ownership converted to TLU	4.120	0.283	3.447	0.202	0.673**
othpartcp	Whether a household ever participated in other collective actions (yes=1)	0.156	0.027	0.051	0.015	0.105***
dstextn	Household distance to extension office (in minutes)	38.223	3.845	51.61	4.530	-13.393**
dstothfrst	A household distance from a non-programme (alternative) forest	55.729	7.15	29.728	2.866	26.000***
mlfrc	Household labour-force (men)	1.284	0.048	1.266	0.041	0.018
fmlfrc	Household labour-force (women)	1.346	0.050	1.153	0.038	0.192***
Menja	Whether Menja people are present in one's (study unit's) village (yes=1)	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*
N		200		177		

Table 2. Propensity score estimates of the determinants of programme participation

VARIABLES	coefficient	Marginal effect
Household head's age	-0.008	-0.002
	(0.011)	(0.002)
Household head's gender	-0.336	-0.083
	(0.553)	(0.137)
Household head's education	0.022	0.005
	(0.052)	(0.012)
Female labour force	0.848***	0. 208***
	(0.307)	(0. 075)
Male labour force	-0.230	-0.056
	(0.258)	(0. 063)
Land holding size in ha	0.010	0.002
	(0.085)	(0.021)
Off-farm employment	0.842*	0.207*
	(0.490)	(0. 115)
Distance to agro extension office	-0.004*	-0.001*
	(0.002)	(0.001)
Woodlot ownership	-0.511*	-0.125*
	(0.282)	(0.068)
Livestock holding size in TLU	0.122***	0.030**
	(0.049)	(0. 012)
Distance from PFM forest	-0.028***	-0.006***
	(0.005)	(0.001)
Experience of other collective action	1.400***	0.329***
	(0.509)	(0 .103)
Distance from nearest town	-0.005*	-0.001*
	(0.003)	(0 .001)
Distance from nearest road	-0.008**	-0.002**
	(0.004)	(0.001)
Constant	0.281	
	(0.761)	
N	337	337

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Matching Estimator Performance

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

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

Table 4. Treatment effect estimates under different estimation strategies for welfare change

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

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5. Rosenbaum sensitivity analysis

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

Table 6. List of sample villages and their respective sample size

List of Kebeles	Number of villages	Name of villages	
		PFM villages	Non-PFM villages
Yebito (88)	2	Agama (58)	Mula - Hindata (30)
Bita Chega(49)	1	Dara (49)	
Michiti (80)	3	Beka (32), Matapha (24)	Chira - Botera (24)
Woka Araba (50)	1		Woka-Araba (50)
Keja Araba (47)	1		Keja-Araba (47)
Maligawa (63)	2	Sheka (37)	Sheko (26)
Total	10	200	177