The Impact of Common Property Right Forestry:

Evidence from Ethiopian Villages[1]

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Abstract

We use inverse probability weighting to examine the effects of a unique two-pronged common-property forestry program in the Gimbo district of Ethiopia, which includes Joint Forestry Management and improved non-timber forest product marketing efforts. The program was found to have affected household access to agricultural land, and, thus, reduced livestock holdings, due to program strictures. Furthermore, despite those reductions, there is evidence that the program had economically significant effects on other activities. Households were able to increase their earnings from non-timber forest products, partly due to an increased labor allocation toward non-timber forest product collection

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I. INTRODUCTION

The devolution of natural forest management to local communities has recently become more widespread, due to a growing recognition that local communities are likely to manage forest resources better than the state (Murty, 1994; Agrawal & Gibbon, 1999; Gauld, 2002). Decentralization, often in the form of Joint Forest Management (JFM), is also seen as a means of developing and upholding democratization, allowing people to engage in their own affairs (Agrawal & Ostrom, 2001). However, improving the management of forests and upholding democracy is likely to hinge on the ability of forest management decentralization to improve the standard of living of those who are dependent on forests; Angelsen & Wunder (2003) and Sunderlin et al. (2005), amongst others, believe forest management decentralization can reduce poverty.

Essentially, decentralization is intended to halt deforestation by restricting excessive forest harvest, limiting agricultural land expansion, and spurring investment in the forest stock. However, whether or not such reforms can offer sufficient investment incentives is uncertain. Although the shift from state management to co-management is a step in the right direction, insecure, incomplete and (often) incoherent property rights transfers from the state to local communities remains an important source of incentive incompatibility for communities (Behera & Engel, 2006). In some cases, the rents are shared with the state in the form of user fees (Kumar, 2002; Kajembe et al., 2003; Behera & Engel, 2006; Jumbe & Angelsen, 2006; Limenih & Bekele, 2008; Robinson & Lokina, 2012). The incentives could be even smaller, if we consider foregone income from deterred agricultural land expansion. Because of the restriction placed on forest clearing, due to JFM rules, a household foregoes income that could have been earned from new agricultural land. Household income would also be affected by agricultural or forest productivity, as well as any increase in the price of those outputs.[i] As part of the JFM program considered here, prices did increase, while forest or agricultural productivity could increase, due to decreased pressures placed on the forest. In a properly incentivized program, and, thus, one that is acceptable to participants, foregone forestry income is offset by forestry productivity gains and/or forestry product price rises. Similarly, the success of the program would depend on providing alternative incentives to farmers to eschew short-term gains in favor of medium- to long-term payoffs, while the benefits that accrue to community members must also serve as an incentive for monitoring and enforcement.

Since program success depends on the relative size of future returns compared to immediate losses, a program that more clearly offers future returns is more likely to be successful. One innovative design, and the one considered below, confers common property rights usufruct for non-timber forest products (NTFP) and augments it with improved marketing of these products. The present study evaluates one such JFM program in Ethiopia, described in detail in Section II. For the analysis, we exploit a policy (natural) experiment, in which some forest using villages were able to access JFM, while other similar villages were not.

While a sizeable body of literature on the commons has focused on examining the structure and functioning of long-enduring institutions for common property resource management (Lawson-Remer, 2012; Ostrom, 2005; Agrawal, 1994), only a few have employed quantitative analysis to draw conclusions about impacts. The few empirical studies assessing JFM effects and distributional outcomes have produced mixed evidence, but most of the evidence points to worsened welfare outcomes for the poor. Specifically, Jumbe & Angelsen's (2006) evaluation of JFM welfare impacts – based on monthly forest revenue – in two Malawian villages reveals contrasting welfare impacts across the villages. Similarly, Cooper (2008) finds that JFM increased per-capita consumption growth, as well as inequality, in Nepalese villages, where the programs were implemented. However, Basundhara & Ojha (2000) and Cooper (2007) conclude that there are significant welfare losses.

For the most part, the preceding studies consider programs involving local forest protection in exchange for benefits that could arise from long-term sustainable management – access to fuel wood and non-timber forest products (NTFP) – for own consumption. However, evidence of the effects of a "conservation by commercialization" program, is scant. Although one criticism of the following analysis is its inability to separate the effect of own consumption access rights from that of market linkages, the program examined does include both components, and, therefore, in the light of limited evidence around such programs, this program's efficacy deserves attention. Unfortunately, separating the effect of the two program components is not possible in this study, as requisite data for a program without marketing incentives is not available. More importantly, given the importance of future rewards in achieving local buy-in, a program that supports future returns is a logical program to implement, and, therefore, evaluate. As noted above, providing incentives to farmers to eschew short-term benefits in favour of medium- to long-term payoffs is likely to be important in determining the success of such programs, and, therefore, should not be ignored, when considering the multifaceted goals of afforestation and rural development.

Therefore, this study aims to evaluate the impact of a JFM program augmented by the provision of market-based incentives, through NTFP marketing. For the analysis, inverse probability weighting is used to identify the effects of the program. We applied these methods to data collected from households living proximately to program and non-program forests in selected villages of the Gimbo district, in southwestern Ethiopia.

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 augmented by market-based incentives, through the marketing of NTFPs. Given the widespread devolution of natural forest management throughout developing and emerging economies (Agrawal & Ostrom, 2001; Bluffstone, 2008), which is invariably based on theoretical predictions, as well as anecdotal evidence from local case studies, rigorous empirical analysis of the impact is needed to inform such policies. 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.

The remainder of the paper is organized as follows. Section II outlines the evaluation problem, as well as the context of the study. Section III describes the data collection efforts, while Section IV discusses the conceptual and econometric framework that informed the empirical strategies. Section V presents results and discusses those results. Finally, Section VI concludes the analysis

II. BACKGROUND, PROGRAM AND EVALUATION PROBLEM

Since the 1970s, Ethiopian natural forests were primarily owned and managed by the state, which led to the establishment of various state-owned protected Forest Priority Areas (Kubsa at al., 2003). These areas excluded local community input and, thus, were to be protected by hired forest guards; however, they were *de facto* open access forests, resulting in continued forest resource depletion (Limenih & Bekele, 2008). This realization incited the government of Ethiopia and NGOs to seek alternative policy instruments (Tesfaye et al., 2010; Kubsa et al., 2003). Against this backdrop, bilateral donors, such as GTZ and JICA, as well as NGOs, including Farm Africa/SOS-Sahel FARM-Africa, implemented Joint Forest Management (JFM) programs in different parts of the country. The overriding objectives of these interventions were two-pronged: halting deforestation and improving the livelihood (reducing poverty) of forest dependent communities, the latter to be achieved through bolstering the economic benefits provided by the forests. In Bonga, which is the site of this analysis, Farm Africa/SOS-Sahel implemented more than six JFM programs, covering about 80,066 hectares of natural forest (Jirane et al., 2008).[ii]

In light of the aforementioned objectives, Farm Africa/SOS-Sahel set intervention preconditions, targeting forests with high rates of deforestation as well as communities that depended heavily on those forests. Once identified, forest units were demarcated in the field. Within the provisionally identified forest units, information related to available forest resources was required, as was information related to past and present management practices. Finally, this information was collated and bolstered through an analysis of prevailing forest management problems, forest uses and forest user needs (Lemenih & 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. Importantly, for this analysis, these activities were most often associated with unemployed urbanites and a heavy concentration of individuals from the Menja tribe.[iii] These observations led Farm Africa/SOS-Sahel and local government to target JFM interventions towards forests surrounded by significant Menja populations (Lemenih & Bekele, 2008; Bekele & Bekele, 2005). Although the Menja population was the overriding eligibility criterion, other criteria, including the degree of agricultural encroachment, population pressure, the forest’s status, and the forest’s potential to produce non-timber forest products, were considered to a varying degree.

Once intervention sites had been identified, the remaining key elements of JFM intervention – crafting common property right forest management institutions (rules) and establishing enforcement mechanisms – were put in place. The process of rule setting and establishing the attendant community organization involved a range of complex procedures. Farm Africa/SOS-Sahel began the process with negotiations and discussions with all stakeholders. However, since skepticism regarding JFM was rife within both the local government and the local communities, Farm Africa/SOS-Sahel provided JFM training for all stakeholders (Bekele & Bekele, 2005); that training was offered at the level of the village, rather than the individual, which has implications for the subsequent analysis. In addition to problems related to skepticism, negotiations with regard to JFM participation and JFM forest boundaries were fraught with difficulties.

Whereas JFM 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 & Bekele, 2005).[iv] It is assumed that the village participation decision was determined by the perceived costs and benefits of JFM, a perception that is likely affected by training and other circumstances, driven, in large part, by program eligibility.

The formation of Forest User Groups (FUG) and Forest User Cooperatives (FUC) came into effect, following the framing of rules (institutions) and the setting up of organizational components (often known as nested enterprises in the commons literature).[v] Because JFM is a co-management system, determination of the authority for making constitutional, collective choices and operational level rules were not left to forest user collectives, alone; rather, they involved experts from Farm Africa/SOS-Sahel and local governments. At an operational level, the rules comprise of: (i) stipulations relating to quantity and the types of forest products allowed for use by members; (ii) stipulations concerning disposal procedures for commercially valuable NTFPs; (iii) enforcement rules surrounding protection from fire, vandalism (including unauthorized tree cutting) and agricultural encroachment (clearing forest for agricultural land acquisition), and (iv) forest development (management) rules regarding the planting of new trees for the enrichment of the existing forest.

Within the bounds of this intuitional framework, each individual member enjoys two kinds of rights over forest products: (1) a private right, and (2) a collective right. Privately, the forest can be used for livestock production, collecting wood for private use (including energy and farm implement construction), harvesting medicinal plants for own consumption, and beekeeping, all subject to management committee approval. The harvest of timber, forest coffee, and spices is a collective right, belonging to the FUC, leading to benefits that must be distributed across the membership, although 30% of total income is retained by the FUC (Bekele & Bekele, 2005; Lemenih & Bekele, 2008).[vi]

In terms of the causal chain, Gertler et al. (2010), activities of JFM intervention – program forest identification, community and personnel training, stakeholder's analysis and negotiation – gave rise to the FUC (rules and its organizational contents). The functioning of the FUC, in combination with marketing assistance, in turn, yielded a range of intended outcomes. Particularly, it has increased forest cover and NTFP productivity (Bekele & Bekele, 2005; Limenih & Bekele, 2008; Gobeze et al., 2009), reduced agricultural encroachment and forest fires (Limenih & Bekele, 2008), and slashed the extraction of forest resources for the production and sale of charcoal and firewood (Gobeze et al., 2009); we show, see Table 1, that these effects are observed in the villages selected for this analysis.[vii] Moreover, the program has improved farmer access to new, fair and sustainable market opportunities, enabling them to sell their products (coffee, honey, spices) at better prices (Schmitt & Grote, 2006). For example, program farmers, through the FUC, sell forest coffee at 87% higher prices than non-program farmers (Shemeta et al., 2012). Likewise, they sell honey at 70% higher prices compared to non-program farmers (SOS SAHEL, 2007). These price premiums are attributed to savings on transaction costs (searching, bargaining, etc.), as well as added value accruing from processing, at least in the case of honey.

III. CONCEPTUAL AND ECONOMETRIC FRAMEWORK

(a) Theoretical considerations

Our theoretical foundation follows from Roy (1951). Accordingly, farmers choose whether or not to participate in the JFM program, and that decision is assumed to depend on the farmer's expectation of the benefit of participation in the program, relative to the *status quo*. We assume households are unitary, as it is not possible to assume otherwise with the data that is available. Households are assumed to enjoy both consumption and leisure, and household budgets are determined by both income from farming activities, off-farm labor activities and profits from NTFP sales. Furthermore, profits are a function of both the price of NTFPs – which is higher for JFM participants than non-participants according to Shumeta et al. (2012) and SOS SAHEL (2007) – and the productivity of the program forest stock, from which NTFPs are collected. Previous literature finds that productivity is higher for JFM participants than non-participants (Bekele & Bekele, 2005; Limenih & Bekele, 2008; Gobeze et al., 2009). With these assumptions and previous results, household are expected to benefit from the program, due to increases in income arising from increased proceeds from NTFP. In turn, increased earnings may mitigate liquidity constraints associated with the engagement in off-farm activities, alleviating problems associated with accessing start-up capital or possibly augmenting existing off-farm activities. The size of that effect, though, is unknown. However, the labor-leisure tradeoff is driven by the usual factors that determine whether or not individual labor supply curves are backward bending, even in places where labor markets do not function that well. Thus, the effect of JFM on labor supply decisions is an empirical question.[viii]

Although labor supply decisions are an empirical question, JFM participation is assumed to arise from an expectation that household welfare is higher under JFM than under open access. That expectation is likely to be affected by a number of factors. For example, travel distances to either the JFM or alternative forest will impact this expectation, since travel distances affect the amount of time required to undertake NTFP collection. Other travel distances might also matter. It is plausible that households living closer to town and/or rural extension offices would be more likely to participate, as they would have better access to information. On the other hand, they may be more urban in their activities, and, therefore, less likely to participate. Experiences with other collective action programs are also likely to matter. Specifically, if past experiences were positive, households could be more willing to engage in additional collective action programs.

The household's supply of labor and pre-program off-farm labor market activities should also matter. If a household has more labor available, they may face lower opportunity costs of travelling further for NTFP collection. On the other hand, if the household is already contributing some of its current labor to the market, that household already has income sources that protect it from variances in agricultural income or may be less dependent on forests for their livelihoods. In either case, such a household should be more willing to participate in a program with uncertain longer-term benefits. Similarly, more educated households would tend to discount the future less and would more easily make the connection between open access use and land degradation. Thus, such a household is more likely to incorporate potential future benefits from JFM, and, therefore, participate in the program. The age of the household head could also influence decisions similarly. More elderly households consider a shorter future horizon, and, therefore, are less likely to participate in JFM. Finally, as noted in the description of the program, the state of the forest is also expected to affect participation. Therefore households living in a community with members of the Menja tribe are more likely to have access to the program, and, therefore, more likely to participate. This intuition underpins our empirical approach to inverse probability weighting, to which we now turn.

(b) Empirical considerations

For the empirical specification, let Y_{ij} be our household outcome of interest, which includes per capita expenditure, NTFP revenues and labor allocation decisions. If farmer $\mathrm{i} = \{1, 2, \dots, \mathrm{N}\}$ chooses to participate $(\mathrm{D_i} = 1)$, the relevant household outcome is Y_{i1}, while Y_{i0} is the relevant outcome for non-participating $(D_i = 0)$ households, where D_i represents treatment. In what follows, treatment is defined as being a member of a FUC, which was described in Section 2. Although participation is voluntary, and that must be taken into account in the analysis, it is also important to note that cooperatives are community-level decisions. However, we abstract from the community-level decision, and, instead, estimate post-intervention average impacts. We do so, recognizing the importance of controlling for as many pre-program differences as is possible, especially those underpinning program sites, while showing, in fact, that there was a treatment.

Assuming that the distribution of welfare outcomes, Y_{1i} and Y_{0i}, are independent of treatment D_i, given a vector of observed covariates X_i, a propensity score matching estimator for the average effect of treatment on the treated can be estimated. Intuitively, the goal of matching is to create a control group of non-JFM participants that is as similar as possible to the treatment group of JFM participants, although the groups differ in terms of their participation. Inverse probability weighting mimics the matching intuition through reweighting to make the participant and non-participant distributions look as similar as possible. Identification of the average effect of JFM on the program participants in this framework requires both strict ignorability of treatment, $(Y_{1i}, Y_{0i}) \perp D_i | P(X_i)$, and propensity score overlap, $0 < P(X_i) < 1$

(Rosenbaum & Rubin, 1983; Dehejia & Wahaba, 1999; Dehejia & Wahaba, 2002). The second assumption results in a common support, in which similar individuals have a positive probability of being both participants and non-participants (Heckman et al., 1999). The analysis, below, considers inverse probability weighted regression, where the probability is derived from a logit model inline with the propensity scores outlined above.

$$\text{prob}(D_i = 1|X_i) = \Lambda(X\Gamma) \qquad (1)$$

Variables in the model account for village level controls, such as the presence of the menja tribe, and household level controls. The latter include access to other forests, participation in other collective action programs, the household head's gender, age and education, categorical indicators of 'extra' men and women in the household who could work off-farm. In addition, we included a number of categorical indicators of distance to the closest: PFM forest, the rural extension office and road. Finally, we included recall information on pre-program livestock holdings and the household's experiences supplying labor to the community.

Based on the results from the above model, we applied inverse probability weighted (IPW) regression, wherein the propensity score is used to reweight the data. Due to the fact that IPW regression is a two-step process – propensity scores are first estimated to create the weights, as well as define overlap between comparison and control groups, and then the weighted regression is estimated – standard errors for the IPW parameters are estimated via bootstrap replications.[ix] Bootstrapping the inverse probability weighted regression does not suffer from the same matching bootstrap problems highlighted by Abadie & Imbens (2008). An important question arising, however, is whether or not the bootstrap should be clustered at the village level. Even ignoring two-step methods, as in the case of standard regressions, it is not completely obvious that pairs bootstrapping at the level of the cluster is an appropriate method for estimating standard errors, especially when there are as few clusters (10) as there are here (Wooldridge, 2003). Cameron et al. (2008) consider a wide range of methods, suggesting that wild cluster bootstrapping generally performs better when there are less than 20 groups and when clusters are not balanced. As we were unable to find a similar Monte Carlo analysis for appropriate bootstrap methods, when dealing with IPW, we bootstrap at the level of the cluster. As is common in observational studies (Cameron and Trivedi, 2005), we applied IPW regressions using two different counterfactuals: (i) all non-program participant households in the sample, and (ii) non-participants households residing within JFM villages.[x]

The regression model estimated is a standard treatment effects regression, wherein the outcome variable of interest is regressed on the treatment. In addition, we include the controls from the propensity score regression, see (1) and the related text, with the exception of pre-program control variables. We do this to control for any lingering covariate imbalance that could influence the estimates.

IV. 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. In half of the villages, there is a JFM program, while the villages are, otherwise, near each other and are quite similar. Sample frames for the survey were derived from the selected villages, via the lower level of local government unit, “kebele”. The analysis was based on randomly selected households from these villages: 200 from JFM villages and 177 from non-JFM villages.

With regard to the choice of outcome variables for the analysis, we use per capita consumption expenditure (per annum), including goods produced at home valued at village prices, rather than income for a variety of reasons, although we acknowledge that the program was meant to influence NTFP revenues and NTFP labor allocation, rather than income or expenditure. 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. Similarly, it is easily interpreted and widely used (Nguyen et al., 2007; Skoufias & Katatyama, 2011). 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, thus, may be more susceptible to under-reporting. More importantly, given that the major transmission mechanism through which the program is expected to impact households – through NTFP earnings and the potential to reallocate labor – we also considered related measures. These outcomes include: per capita revenue earned (per annum) from the sale of NTFP products, time allocated to NTFP collection, and time allocated to off-farm labor. The difference in the distribution of these variables across program participation gives some indication that the program could make a difference in the economic lives of program participants.

As already discussed above, survey respondents provided information on household characteristics, such as: age, education, gender, family size, household expenditure on various goods and services, household assets, household earnings from the sale of various goods and services, labor allocated to off-farm activities, distance to nearest town and distance to the nearest road. Additional information related to potential determinants of JFM participation was also collected, including: the presence of members of the Menja tribe, distance from the JFM forest, availability of alternative forests and experience with other collective action arrangements. Descriptive statistics of the survey data are presented in Table 2, and these statistics are separated by participation status.

In interpreting the information in Table 2, the final column is the most relevant column, as it describes mean differences. When considering the outcome variables, there is very little reason to believe that the program had any effect on households, although the signs tend to agree with our *a priori* expectations. Per capita consumption expenditure is larger for participating households, although the mean difference is not significant. Surprisingly, per capita revenue from NTFP is smaller for participating households, again, not statistically significantly so. We also observe that participant households allocate more labor to extract NTFP, but the mean difference is not statistically significant. Participating households are more likely to have a household member working off of the farm, and that difference is statistically significant. With respect to the following analysis, none of the preceding differences can be considered causal, as those differences do not account for other factors that are likely to have encouraged or discouraged participation in the program.

In terms of the variables that may have encouraged or discouraged JFM participation, there are a number that are statistically significant. Participating households are located in areas that are nearly 40% more likely to incorporate individuals from the Menja tribe; this is not surprising, given the way the program was developed. Participating households are located about 45 minutes closer to program forests, based on walking times. They are also nearly 10 minutes closer to the nearest road and 14 minutes closer to the nearest extension office, again measured by walking times. On the other hand, participating households were 10.5% more likely to have previously participated in other collective programs. Also, before the program was started, participating households were 6% more likely to have a household member working off of the farm. Finally, they own more livestock, as measured in tropical livestock units, and there are more working age females in those households.

Overall, the lack of balance, statistically significant mean differences, for some of the covariates between JFM participants and non-participants suggests that observed outcomes for the latter group cannot be used as a counterfactual for participant outcomes. The mean difference across outcome variables, which assumes random treatment assignment, is thus biased. This problem necessitates drawing on an alternative method of program evaluation. As noted above, we solve this problem via inverse probability weighting.

V. RESULTS AND DISCUSSION

(a) Determinants of JFM participation

Before analyzing JFM impacts, we first identify the determinants of household decisions to participate in the program, which have been estimated following logit models of the probability that a household is a program participant, and estimated for two separate comparison groupings. Table 3 reports the estimates and the marginal effects (calculated at the means of the data for each of the comparison groupings). Although the groupings differ, the results suggest that many of the household characteristics and village-level factors outlined in Section IV are statistically significant determinants of the decision to participate in the program, and these correlate strongly to the mean differences highlighted in Table 2. Furthermore, with a few exceptions, the marginal effects are quite similar across the comparison groups. In other words, the determinants of participation for all households in the sample are fairly similar to the determinants of participation for households that were eligible for the JFM program. Consistent with that discussion, the distance between the program forest and agricultural extension service office is negatively associated with household participation, suggesting that proximity to the extension office could have influenced the government's program location decisions, and, thus, influenced participation. Similarly, household's residing closer to the program forest were more likely to join the program, while those residing farther from alternative forests are also more likely to have joined, suggesting that opportunity costs associated with distance do matter. It is also true that households that have experience with other collective action programs are more likely to participate, presumably due to positive experiences. We also find that if a household member was engaging on off-farm employment before the program began, the household was more likely to participate in the program, presumably due to them being less dependent on forests for their livelihoods. Households headed by older individuals are less likely to participate; possibly, older individuals are more set in their ways and have a shorter time horizon over which to gain from the program.

(b) The impact of JFM participation

As noted earlier, if treatment assignment was completely random, it would be possible to simply compare the mean difference in the outcomes. Since participation is voluntary, and, therefore, random treatment assignment does not obtain, we, instead, consider conditional mean differences based on IPW regression. Before turning to the results, the underlying premises of IPW – confoundedness and overlap – must be considered. Figure 1 alludes to an appropriate magnitude of overlap. Although there are propensity scores that are too close to either zero or one, implying that regions too close to zero or one will not be part of the common support (see endnote ix), the majority of participants and non-participants have similar estimated propensity scores. Balance was also considered, although not reported here, in order to conserve space.[xi]

The primary results are contained in Tables 4-7. All of these tables contain IPW regression estimates of causal effects. In all cases, the analysis is bootstrapped at the cluster-level. Furthermore, each of the control groups is reweighted by $P/(1-P)$, where P is the estimated propensity score. Within each bootstrap replication, off-support propensity score estimates are dropped, such that the analysis focuses on those households, participant and non-participant, that are as similar as possible.

The results in Table 4 could be sensitive to the inclusion of additional variables; however, the results suggest that, when all households are used, such that reweighted non-participant households represent the control group, there is no evidence of a treatment effect. In agreement with the mean differences reported in Table 2, there is no statistically significant treatment effect identified for this comparison. Table 5, which contains doubly robust IPW regression estimates of the average treatment effect, confirms.

However, when turning to Tables 6 and 7, the results are rather different. Similar to Table 4, Table 5 contains results for only the treatment effect, without additional controls. On the other hand, Table 7, like Table 6, contains doubly robust IPW regression estimates of the average treatment effect. The counterfactuals for Tables 6 and 7, however, are based on reweighting the non-participant households that were located in villages initially eligible for the JFM intervention. In each of these tables, the results point to significant treatment effects. Per capita revenue per annum is statistically and significantly larger, approximately ETB 252-277 higher (ETB 12.63/USD at the time of the survey). Given that per capita revenue is approximately ETB 235, this represents an increase in excess of 100% per person per year. Presumably, part of the reason for that increase is the fact that participant households are devoting 3.5-3.7 additional hours per week collecting NTFP goods. As the average collection time in the sample is in the range of five hours, for non-participant households, the program impact is, again, substantial, nearly 70%. These results suggest that labor supply curves are not backward bending in this region of southwestern Ethiopia.

In terms of the results presented, we have been able to extend the analysis in two additional ways: (i) we considered alternative counterfactual groups, and (ii) we included a broader set of post-project covariates, other program participation. We find that these results are sensitive to the choice of counterfactual. Possibly, non-participant villages are quite different. One particular worry in any analysis of this sort is unobserved heterogeneity that might impact selection. Given the way the JFM program was rolled-out in this area, it is entirely likely that non-program villages were in better shape, environmentally, and, thus, households were in better shape, economically, such that no post-program implementation differences can be uncovered. Despite the sensitivity to choice of counterfactual, the direction and size of the program impacts are not particularly sensitive to the inclusion of a broader set of post-project covariates, although some variations in the magnitudes of these impacts are evident (see Tables 4-7).

(c) Discussion

In broader terms, our findings lend support to the propositions from the theoretical literature on the commons, which claims that common property rights can generate rent. In other words, it can mitigate externalities related to resource management. Thus, it could be a useful alternative to open access forestry management (Caputo & Luek, 2003; Agrawal, 1999; Gordon, 1954).

Empirically, our results also accord with Agrawal & Ostrom (2008); they conclude that effective decentralization reforms have increased local actors' benefits and rights in forests. We are able to show that this particular JFM program increases the time spent in forests, which is assumed to derive from these rights, and that it increases the earnings that they derive from these forests. Similarly, our findings agree with Chhatre & Agrawal (2009), who, through an analysis of 80 forest commons across 10 countries, find that decentralized forestry management is associated with greater household benefits, which we associate with increased revenues, along with forest carbon storage benefits, although the latter finding is beyond the scope of this research.

However, there has been little research into the effects of JFM reported in the literature. In comparison to the preceding studies, our results do not support Cooper (2008). Cooper finds that JFM increased the growth in per-capita consumption among program participating households in Nepal. Although we do find increased revenue generated from these forests, we are not able to empirically tie that to similar increases in per capita consumption. Our research is, however, congruent with Dasgupta (2006) and Wunder (2001), who argue that common property forestry has raised the welfare of participant households in India, although the programs they consider did include improved market linkages for NTFPs, and they did not consider JFM programs. However, our findings stand in sharp contrast to Jumbe & Angelesen (2006) and Basundhara & Ojha (2000), who reveal that JFM programs entailed significant welfare losses among program participants.

VI. CONCLUSION

Previous studies that have evaluated the impacts of Participatory Forest Management programs have found a wide variety of results that depend upon the study context and the employed methodology. Many Nepalese studies confirm that JFM succeeded in regenerating the forest resource (Edmonds, 2002; Yadav et al., 2003; Dev et al., 2003). Other, African and Asian studies, primarily, conclude that JFM offered mixed effects on welfare, but the majority suggest worsened welfare, especially for the poor (Kumar, 2002; Jumbe & Angelsen, 2005; Cooper, 2007; Adhikari, 2005). One reason for worsened welfare could arise from the fact that JFM places restrictions and regulations on forest-related livelihood options, mainly on harvesting (Larson & Pulhin, 2012). If not counterbalanced, these restrictions could lead to a decline in forest-based incomes (Schreckenberg & Luttrell, 2009). However, there is a dearth of evidence concerning, whether other JFM designs could alter that conclusion. One option, and the one considered in this analysis, includes improved market linkages for non-timber forest products.

The present study was motivated by this empirical gap. It set out to evaluate the effect of a JFM program that involved the decentralization of forestry management and was augmented by market linkage interventions. Our analysis has revealed that the decentralization exercise in southwestern Ethiopia, which combined JFM with improved NTFP market linkages, offers substantial revenue gains and labor allocation options, and, hence, provides incentives for program participants to protect the forests. The result implies that decentralization policies of this nature can provide alternative avenues of raising rural income, thereby promoting rural development; the latter of which is often confined to technological and market development interventions. Moreover, results from our initial analysis lend support to the general conclusion in the literature that forest management decentralization in the form of JFM halted deforestation.

The analysis was based on data collected in selected villages of the Gimbo district in southwestern Ethiopia. The potential outcome framework underpinned the analysis, through which, the causal link between program intervention and household welfare could be empirically investigated. Identification based on observed controls, via matching, and, subsequently, IPW regression, yielded an increase in average per capita revenue, and opened the door to labor reallocation within the household towards, especially NTFP collection activities. Unfortunately, due to the program design, we are not able to separately attribute the gains to either the change in forestry management arrangements or market access. However, we are led to believe, on the basis of this study's findings, as well as the restrictions imposed on forestry product harvests in the JFM forests, that the effects are most likely due to the market linkages associated with NTFPs harvested from JFM forests.

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Table 1. Difference-in-Difference Indication of Treatment

VARIABLES	Land Holdings	Livestock Holdings
Time (After Intervention = 1)	0.732***	1.071***
	(4.352)	(3.358)
JFM Participant	0.599	1.476**
	(1.592)	(2.066)
Difference-in-Difference	-0.708***	-0.858*
	(-2.972)	(-1.898)
Constant	1.649***	2.368***
	(6.202)	(4.693)
Observations	746	742
R-squared	0.044	0.017

Difference-in-Difference estimate of changes in land holdings and livestock holdings before and after JFM Intervention. Pre-program data based on recall response. All other variables in the data are only available after intervention, so no additional control variables are included in the model. Standard errors clustered by village. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

Table 2. Descriptive Statistics

VARIABLES		JFM-participant	Non-participant	Mean Difference
Per Capita Consumption	HH consumption per HH member	1,734***	1,692***	20.54
		(66.74)	(62.18)	(88.81)
Per Capita Consumption w/o Coffee	HH consumption per HH member without garden coffee	1,731***	1,688***	21.80
		(66.84)	(61.99)	(88.76)
Per Capita Revenue	NTFP Revenue per HH member	235.2***	239.0***	-8.206
		(34.89)	(27.64)	(42.85)
Per Capita Revenu w/o Coffee	NTFP revenue per HH member without garden coffee	232.2***	234.9***	-6.948
		(34.80)	(27.55)	(42.74)
Off-farm Labor (After JFM)	Off-farm labor market participation after JFM implementation	0.145***	0.0843***	0.0649**
		(0.0264)	(0.0209)	(0.0326)
NTFP Collection Time	Collection time for NTFP goods	5.671***	5.035***	0.683
		(0.345)	(0.321)	(0.457)
Distance to JFM	Distance to JFM forest in minutes	22.85***	69.05***	-45.62***
		(2.035)	(5.358)	(5.506)
Distance to Other Forest	Distance to non-JFM forest in minutes (only available for non-JFM households)	194.1***	137.6***	54.02***
		(6.294)	(8.002)	(10.03)
Male-Headed Household	Binary (=1, if HH headed by male)	0.922***	0.949***	-0.0271
		(0.0201)	(0.0165)	(0.0249)
Livestock Holdings (Before JFM)	Livestock holdings in Tropical Livestock Units before JFM implementation (Recall data)	4.088***	3.377***	0.618*
		(0.288)	(0.208)	(0.345)
Other Collective Action Participation	HH participated in previous collective action programs	0.156***	0.0562***	0.0971***

		(0.0272)	(0.0173)	(0.0310)
Household Head Age	Age of HH head	36.67***	35.80***	0.672
		(0.985)	(1.075)	(1.438)
Household Head Education	Education of HH head in years	2.218***	2.478***	-0.235
		(0.212)	(0.241)	(0.319)
Household Males 16-64	Number of HH males of working age	1.285***	1.264***	0.00
		(0.0490)	(0.0439)	(0.0638)
Household Females 16-64	Number of HH females of working age	1.352***	1.152***	0.177***
		(0.0504)	(0.0402)	(0.0626)
Off-farm Labor (Before JFM)	Off-farm labor market participation before JFM implementation (Recall Data)	0.123***	0.0730***	0.0595*
		(0.0246)	(0.0196)	(0.0309)
Distance to Extension	Distance to extension office in minutes	38.20***	52.40***	-14.13**
		(3.857)	(4.960)	(6.015)
Distance to Road	Distance to nearest road in minutes	23.00***	33.38***	-9.750***
		(1.923)	(2.840)	(3.308)
Distance to Town	Distance to town in minutes	69.02***	72.67***	-4.404
		(3.488)	(2.831)	(4.381)
Menja	Binary (=1, if village includes someone from Menja tribe)	0.788***	0.416***	0.379***
		(0.0307)	(0.0370)	(0.0471)
Alternative Forest Available	Binary (=1, if HH has access to alternative forest)	0.240***	0.483***	-0.237***
		(0.0320)	(0.0376)	(0.0485)
Observations		179	178	377

Mean of control variables, by JFM status, used in the analysis. Table includes mean differences, as well as tests of significant differences from zero. Standard errors clustered at the village level.

Table 3. JFM Participation and Marginal Effects Estimates

VARIABLES	All Households: Participants and Non-participants Logit Coefficients	All Households: Participants and Non-participants Marginal Effects	JFM Eligible Households: Participants and Non-participants Logit Coefficients	JFM Eligible Households: Participants and Non-participants Marginal Effects
Distance to JFM	-0.0278***	-0.00687***	-0.0171	-0.0034
	(0.0088)	(0.0021)	(0.0118)	(0.0033)
Other Forest Available	-0.9810***	-0.2420***	-1.1010***	-0.220**
	(0.266)	(0.0689)	(0.3470)	(0.105)
Male-Headed Household	-0.2610	-0.0645	0.4250	0.0847
	(0.5690)	(0.1430)	(0.434)	(0.0934)
Livestock Holdings (Before JFM)	0.1250***	0.0308***	0.144***	0.0287*
	(0.0220)	(0.0046)	(0.0352)	(0.0173)
Other Collective Action Participation	1.3790***	0.3410***	1.0220*	0.2040
	(0.4710)	(0.1160)	(0.594)	(0.172)
Household Head Age	-0.0102	-0.0025	-0.0123	-0.0025
	(0.0099)	(0.0024)	(0.0132)	(0.0030)
Household Head Education	0.0198	0.00488	-0.0004	-0.0001
	(0.0468)	(0.0116)	(0.0817)	(0.0163)
Household Males 16-64	-0.330**	-0.0814**	-0.501**	-0.0999
	(0.1650)	(0.0383)	(0.2150)	(0.0771)
Household Females 16-64	0.8300**	0.2050**	1.436***	0.286**
	(0.3600)	(0.0819)	(0.1950)	(0.1330)
Off-Farm Labor (Before JFM)	0.8005*	0.1990*	1.524***	0.304**
	(0.4410)	(0.1100)	(0.331)	(0.143)
Distance to Extension	-0.0049	-0.0012	-0.0051	-0.0010
	(0.0032)	(0.0008)	(0.0037)	(0.0009)
Distance to Road	-0.0090	-0.0022	-0.0127	-0.0025
	(0.0137)	(0.00341)	(0.0162)	(0.0032)
Distance to Town	-0.0038	-0.0009	-0.0004	-0.0001
	(0.0050)	(0.0013)	(0.0064)	(0.0012)
Menja	1.3930	0.3440		
	(1.4950)	(0.3740)		
Constant	0.5580		0.4610	
	(2.0120)		(1.0810)	
Observations	352	352	213	213

Propensity score estimates and marginal effects calculated at the means of the data of participation from Logit regression (participation = 1) for two samples One sample includes all households, the second sample includes only households initially eligible for JFM intervention. Standard errors clustered at the village level. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

Table 4. ATT Estimates from Bootstrapped Inverse Probability Weighted Regression: Counterfactual Represented by all Non-JFM Participants

VARIABLES	Per Capita Consumption	Per Capita Consumption w/o Coffee	Per Capita Revenue	Per Capita Revenue w/o Coffee	Off-farm Employment	NTFP Collection Time
JFM Participant	-52.18	-49.78	82.56	84.95	0.0167	2.391
	(383.2)	(385.0)	(175.8)	(176.5)	(0.183)	(2.064)
Constant	1,717***	1,709***	159.2*	151.9*	0.0965	3.388**
	(327.0)	(326.4)	(86.42)	(82.65)	(0.198)	(1.325)

Estimates from Inverse Probability Weighted regression, to reweight the non-participation group. Observations taken from full sample of households including those not initially eligible for JFM. Standard errors from 299 bootstrap replications, blocked at the village level. In each replication, only observations contained on the support were included, such that observations vary in each replication, and observations are not listed in the table. No additional control variables, other than those listed were included in this regression. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

Table 5. ATT Estimates from Bootstrapped Inverse Probability Weighted Double Robust Regression: Counterfactual Represented by all Non-JFM Participants

VARIABLES	Per Capita Consumption	Per Capita Consumption w/o Coffee	Per Capita Revenue	Per Capita Revenue w/o Coffee	Off-farm Employment	NTFP Collection Time
JFM Participant	-54.34	-50.00	65.53	69.87	0.00253	2.454
	(333.5)	(333.6)	(224.1)	(221.3)	(0.124)	(2.236)
Distance to JFM	9.494	9.532	2.057	2.095	0.0001	0.0137
	(10.82)	(10.81)	(6.524)	(6.511)	(0.0041)	(0.134)
Other Forest Available	304.7	282.3	49.13	26.79	0.0360	1.133
	(507.5)	(509.6)	(196.7)	(193.3)	(0.170)	(2.763)
Male-Headed Household	-24.66	-4.225	173.2	193.6	0.0154	2.226
	(748.2)	(759.7)	(255.4)	(257.0)	(0.218)	(2.542)
Other Collective Action Participation	-291.0	-294.8	-36.24	-39.98	-0.0410	1.344
	(648.0)	(637.7)	(275.3)	(274.3)	(0.200)	(3.760)
Household Head Age	-1.159	-1.056	-5.860	-5.757	0.0023	-0.0195
	(20.66)	(20.71)	(12.43)	(12.37)	(0.0072)	(0.302)
Household Head Education	-16.45	-17.85	-2.770	-4.170	0.0295	-0.0596
	(97.81)	(97.79)	(48.41)	(48.60)	(0.0490)	(0.385)
Household Males 16-64	155.5	158.3	95.00	97.77	0.0254	0.631
	(425.1)	(425.8)	(186.1)	(186.9)	(0.170)	(2.684)
Household Females 16-64	-1.270	-5.861	33.57	28.98	-0.0700	-0.365
	(854.0)	(854.3)	(211.6)	(211.8)	(0.277)	(4.504)
Distance to Extension	3.228	3.114	0.547	0.434	-0.00072	0.0062
	(13.89)	(13.82)	(4.487)	(4.504)	(0.0040)	(0.0414)
Distance to Road	-6.444	-6.343	-1.313	-1.211	-0.0011	-0.0184
	(17.18)	(17.18)	(5.288)	(5.275)	(0.0046)	(0.0749)
Distance to Town	-3.379	-3.348	0.578	0.609	0.00062	0.0332
	(8.437)	(8.407)	(8.130)	(8.154)	(0.0038)	(0.111)
Menja	188.7	202.1	-122.1	-108.7	-0.0760	-0.701

	(379.9)	(381.6)	(204.8)	(203.1)	(0.139)	(1.244)
Constant	1,449	1,419	57.56	28.28	0.0469	-0.591
	(1,227)	(1,234)	(693.9)	(698.6)	(0.469)	(10.62)

Estimates from Inverse Probability Weighted regression, to reweight the non-participation group. Observations taken from full sample of households including those not initially eligible for JFM. Standard errors from 299 bootstrap replications, blocked at the village level. In each replication, only observations contained on the support were included, such that observations vary in each replication, and observations are not listed in the table. Control variables, except for those representing pre-program data, from the propensity score model were included in these regressions. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

Table 6. ATT Estimates from Bootstrapped Inverse Probability Weighted Regression: Counterfactual Represented by all JFM-eligible Non-Participants

VARIABLES	Per Capita Consumption	Per Capita Consumption w/o Coffee	Per Capita Revenue	Per Capita Revenue w/o Coffee	Off-farm Employment	NTFP Collection Time
JFM Participant	-601.7*	-602.0*	252.2***	251.9***	0.154	3.486***
	(337.1)	(337.2)	(92.00)	(91.99)	(0.109)	(1.017)
Constant	2,442***	2,442***	40.92**	40.92**	0.0393	2.504***
	(294.8)	(294.8)	(18.42)	(18.42)	(0.101)	(0.789)

Estimates from Inverse Probability Weighted regression, to reweight the non-participation group. Observations taken from full sample of households including those not initially eligible for JFM. Standard errors from 299 bootstrap replications, blocked at the village level. In each replication, only observations contained on the support were included, such that observations vary in each replication, and observations are not listed in the table. No additional control variables, other than those listed were included in this regression. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

Table 7. ATT Estimates from Bootstrapped Inverse Probability Weighted Double Robust Regression: Counterfactual Represented by all JFM-eligible Non-Participants

VARIABLES	Per Capita Consumption	Per Capita Consumption w/o Coffee	Per Capita Revenue	Per Capita Revenue w/o Coffee	Off-farm Employment	NTFP Collection Time
JFM Participant	-329.3	-329.7	277.7**	277.3**	0.0610	3.680***
	(249.1)	(249.2)	(108.1)	(108.1)	(0.0547)	(1.190)
Distance to JFM	-0.290	-0.287	1.160	1.163	-0.0008	0.0221
	(5.666)	(5.667)	(2.558)	(2.560)	(0.0019)	(0.0305)
Other Forest Available	-678.3**	-678.1**	61.50	61.77	0.0604	0.0362
	(318.7)	(318.7)	(165.7)	(165.7)	(0.115)	(0.779)
Male-Headed Household	-441.4	-441.8	197.8	197.4	-0.0991	2.437*
	(459.0)	(458.9)	(156.9)	(156.9)	(0.162)	(1.377)
Other Collective Action Participation	411.3	412.0	326.4	327.1	-0.117	1.770
	(391.6)	(391.8)	(228.6)	(228.6)	(0.157)	(1.815)
Household Head Age	-6.517	-6.528	-4.903	-4.913	0.0046*	-0.0246
	(7.834)	(7.836)	(3.612)	(3.611)	(0.0027)	(0.0377)
Household Head Education	-53.07	-53.19	-77.44**	-77.57**	0.0737***	-0.266
	(52.27)	(52.25)	(38.19)	(38.19)	(0.0270)	(0.208)
Household Males 16-64	-66.91	-66.77	-93.35	-93.20	0.179**	-0.906
	(213.6)	(213.7)	(116.9)	(117.0)	(0.0872)	(1.026)
Household Females 16-64	-122.5	-122.5	23.69	23.74	-0.366**	1.609
	(423.4)	(423.5)	(116.2)	(116.2)	(0.157)	(1.895)
Distance to Extension	-4.646***	-4.646***	-0.794	-0.794	-0.0006	0.0346***
	(1.593)	(1.593)	(1.237)	(1.237)	(0.0006)	(0.0114)
Distance to Road	-11.02**	-11.01**	7.700*	7.711*	-0.0001	0.0027
	(4.946)	(4.944)	(4.012)	(4.013)	(0.0014)	(0.0189)
Distance to Town	4.391	4.394	0.193	0.195	-0.00045	0.0014
	(3.725)	(3.726)	(1.985)	(1.984)	(0.0014)	(0.0182)
Constant	3,314***	3,314***	-34.74	-34.71	0.234	-1.757

	(645.9)	(646.0)	(247.0)	(246.9)	(0.210)	(2.700)

Estimates from Inverse Probability Weighted regression, to reweight the non-participation group. All observations taken from sample of households originally eligible for JFM intervention. Standard errors from 299 bootstrap replications, blocked at the village level. In each replication, only observations contained on the support were included, such that observations vary in each replication, and observations are not listed in the table. Control variables, except for those representing pre-program data, from the propensity score model were included in these regressions. *** - Significant at 0.001, ** - significant at 0.01, * - significant at 0.05.

[i] The former may arise from technological change, such as the use of improved seed varieties, fertilizer, herbicides and other technologies, while the latter may arise from improved marketing of agricultural products.

[ii] JFM 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; Bekele & Bekele, 2005).

[iii] The Menja tribe in Bonga province is a minority ethnic group that is entirely dependent on forests for their livelihood. They are generally ostracized, and commonly referred to as fuelwood sellers (Lemenih & Bekele, 2008; Gobeze, Bekele, Lemenih. & Kassa, 2009; Bekele & Bekele, 2005).

[iv]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 & Bekele, 2008).

[v] The term “nested enterprise” refers to interrelated (sometimes hierarchical) organizational components that take on complementary sets of responsibilities (Ostrom, 2005).

[vi] Forest User Cooperatives are fully implemented and operational forest user groups (Jirane et al., 2008).

[vii] With the data that was available to us, it was not possible to show that prices were generally higher for NTFPs, or that agricultural land was more or less productive. However, it was possible to show that participant households had made smaller (relatively speaking) inroads into forests and were raising relatively fewer head of livestock. These are two of the important strictures in place, as part of the program;

therefore the results are suggestive of a treatment. The results suggest that the program was successful in halting deforestation arising from agricultural expansion into forests, supporting findings from previous qualitative studies in the area, similar to Edmonds (2002), who found that JFM has cut firewood extraction in Nepal.

[viii] Non-separable household models, under assumptions of imperfect or missing labor markets, yield labor demand and supply decisions that are jointly determined. Because farmers work for themselves, their demand for labor is also their supply of labor. Thus, increases in NTFP income yield higher opportunity costs for leisure and less demand for leisure, which means increased labor supply. Similarly, increased output prices lead to increased demand for variable input, labor.

[ix] Within each bootstrap replication, we require both the minimum and maximum probabilities of participation for participants and non-participation to be the same. In other words, we estimate our IPW regressions over a limited range of probabilities, denoted as the common support.

[x] JFM villages are villages where Farm Africa (the program implementing NGO) undertook village level training related to JFM, before program implementation. Despite the training, the program was not implemented in all the villages where training was received.

[xi] One option is to check if significant mean differences remain across the covariates, after matching. Another option, 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 & Kopeinig, 2008). 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 & Caliendo,

2007); thus, our results should be interpreted within the limits of this assumption.

Results are available from the authors, upon request.