Common Property Forestry and the Heterogeneous Distribution of Welfare: Evidence from Ethiopian Villages

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Abstract

Although extant literature has provided mixed empirical evidence on the welfare effects of common property forestry programs, they have rarely accounted for heterogeneity in those welfare effects. This paper aims at filling this gap in the literature. We assess the distributional impact of a unique common-property forestry program in Ethiopia. This program differs from other programs because it is two-pronged: it develops a Joint Forestry Management (JFM) and combines it with support for improved market linkages for non-timber forest products. The analysis is based on matching and instrumental variable (IV) methods of quantile treatment effects (QTE) evaluation using household data from selected rural villages of Gimbo district, in southwest Ethiopia. The results confirm that the intervention affect outcomes heterogeneously across the welfare distribution. Specifically, the program was found to raise welfare for only those above the median of the distribution. Thus, we infer that the current common property forestry management regime is not pro-poor, and, therefore, is not equity enhancing. Our analysis also revealed that such distributional bias of the program benefit arises from elite capture.

Keywords: Common Property Forestry, Quantile Treatment Effects, Welfare Distribution

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1. Introduction

One key hypothesis of the poverty-environment nexus is the proposition that the poor are both agents and victims of environmental degradation (Wunder, 2001 and Fisher, 2004). The implication of that proposition is that poverty alleviation ameliorates environmental outcomes, and vice versa.

In many poor regions, the poor heavily depend on the income derived from the natural resource base, such as village forests, grazing land and fisheries. These resources are typically managed openly, vindicating its lower entry costs relative to that of alternative income earning sources, i.e., the poor often lack access to other income generating resources, such as land, human capital and physical capital. Furthermore, compared to alternative income sources, natural resource earnings, such as those from non-timber forest products (NTFP), are lower but less variable (Wunder, 2001). Thus, given that the poor are epitomized by high rates of risk-aversion, sales of these products offer income insurance in an environment characterized by imperfect insurance and credit markets, providing another explanation as to why the poor would be more dependent on environmental resources than the non-poor (Delacote, 2007; Debela et al., 2012).

Unfortunately, such dependence on environmental resources leads to overuse (degradation) of the resource, which feeds back through further impoverishment of the poor resource users, an outcome often described as poverty-environment downward spiral (Angelsen, 1998). A corollary to this claim is that the poverty-environment trap can be broken through interventions that restrict excessive resource extraction. One such restriction is through defining and enforcing common property institutions. Theoretically, common property institutions improve resource conditions and generate resource rents, thereby reducing poverty, a contention that motivated the decentralization of natural forest management in many developing countries (Sunderlin et al., 2004 and Angelsen and Wunder, 2003). Whereas property right devolution within the context of tropical forest management has taken two major forms—Community Based Forest Management (CBFM)[1] and Joint Forest Management (JFM)—the present study focuses on a JFM program in Ethiopia, described in detail in Section 2. Invariably, JFM aims at forest conservation by placing significant restrictions on forest timber harvest, charcoaling, agricultural encroachment and other practices previously shown to lead to deforestation under the alternative property right regime of open access. However, several design options for JFM programs are available, and poverty reduction or welfare improvement prospects depend on these design options. Commonly, ownership rents associated with improved forest stocks, arising from property right decentralization in JFM do not accrue to the local communities; hence, there is a need to draw on alternative design options to incentivize forest protection. One possibility is to confer common property usufruct for non-timber forest products (NTFP), the harvest of which is environmentally less detrimental than timber harvesting or other forest uses (Arnold and Perez, 2001 and Wunder, 2001). However, due to the risks and high transaction costs associated with the marketing of these goods (Shillington, 2002 and Neumann and Hirsch, 2002), NTFP usufruct alone is not enough unless it can yield price premiums for the gatherers of NTFPs. In that sense, alternative design options are required, in which, for example, the public purse, as well as donor incentives, can be used to tackle these impediments, through technical, institutional and infrastructural interventions in the marketing of NTFPs, which would help JFM communities benefit from the growing national and international bent of market liberalization (World Development Report, 2008). For example, shortening the marketing chain or lowering the local differences between purchase and sale prices would leave more of the NTFP value in the hands of the extractors.

[1] CBFM involves abroad set of property rights transfer (exclusion, withdrawal and management), wherein local communities become the residual claimant on forest products, as well as the decision.

Although optimism is proliferating within policy circles that JFM has the potential to offer positive welfare benefits, especially amongst the poor, empirical evidence supporting this expectation is limited, at best, and leans towards worsened welfare outcomes for the poor. Jumbe and Angelsen (2006) conclude that common property forestry programs have contrasting welfare impacts across study villages in Malawi; importantly, though, welfare amongst the poor was worsened. Cost-benefit analyses, such as Basundhara and Ojhi (2000) and Neupane (2003) find negative net benefits for the poor, whereas Cooper’s (2007) computable general equilibrium analysis uncovers welfare losses for all income groups, although those outcomes are worse for the poor. Similarly, panel data evidence from Nepal shows increases in per-capita consumption, but greater inequality (Cooper, 2008). For the most part, the preceding studies consider JFM design options that involved local community forest protection in exchange for benefits that could arise from long-term sustainable management—access to fuelwood and non-timber forest products (NTFP) for own consumption. The Ethiopian program we consider, on the other hand, represents an alternative design option, as it includes both access rights for own consumption and the possibility for increased returns from NTFPs marketing.[2] Unfortunately, the existing literature has not uncovered poverty and income redistributive effect of this design option. Furthermore, uncertainties abound concerning whether participation in this scheme can be translated into moving up the income ladder, among rural poor program participants. First, the welfare outcomes of participation depend on household-specific resource endowments and returns to these endowments in harvesting and processing high value NTFP for marketing (Ainembabazi et al. 2012). Second, elite capture usually features in such programs (Nagendra, 2011; Iversen et al. 2006) and is likely to disproportionately impact the distribution of benefits

[2] This scheme is commonly described as conservation by commercialization in related literature (for example, see Evans, 1993 and Arnold , 2001)

from the program.[3] Third, before the advent of the program, the poor, to a larger extent, were used to making their living out of activities, such as the sale of charcoal and firewood, the restriction of which, as part of JFM operational rules, may affect them more negatively. The cost of being forced to give up these activities may not be outweighed by the purported gain from NTFP marketing.

It is this potential sensitivity over trade-offs that could yield heterogeneity within any particular rural population that motivates this investigation into the distributional implications of participation in a JFM that is augmented by marketing intervention. Moreover, a study of this nature has further import in sub-Saharan Africa (SSA), where poverty rates and income disparities are among the highest in the world (Fonta and Ayuk, 2013). From a policy perspective, understanding the potential income redistributive effects, if any, of JFM programs has the potential to help African policymakers restructure these programs to optimize their welfare benefits.

Furthermore, unlike extant literature, we draw on an alternative methodological basis for the analysis. To this point, the literature considers either mean treatment effects of the program or the ratio of costs to benefits for different income groups.[4] The focus on mean effects is common in much of the program evaluation literature, as mean outcomes have traditionally received more attention than the distribution of outcomes (Abadie et al., 2002; Firpo, 2007). Given the preceding proposition with respect to the environment-poverty nexus, it is clear that the interest

[3] Elite capture occurs when tenure system – whether customary or state-supported – fails to treat the elite (more powerful) and non-elite (less powerful) community members alike, with regard to applying rules and sanctions for resource use, or in ensuring that rights to the commons (particularly access rights) can be claimed (Fuys, et al. 2008). For the most part, elite capture is pervasive in JFM and participatory rural development programs, because institutions governing the programs are dominated by the wealthy, at the collective choice level, which results in the devising of operational rules that selectively benefit the wealthy.

[4] The major limitation of cost-benefit analysis is the assumption that a program accrues to each program participant the same benefits and costs.

lies beyond mean impacts, such that the distributional consequences of program interventions are of importance. In this analysis, we draw on recent advances in the estimation of the distribution of treatment effects, namely quantile treatment effects (QTE), to provide a wider indication of the welfare effects of interventions. QTE's ability to characterize the heterogeneous impacts of treatments across the outcome distribution makes it appealing in many economic applications (Frölich and Melly, 2008, and 2010), including this one.

Overall, our study moves the related literature forward in three major ways, in a bid to improve our understanding of the impacts of such programs. First, we provide comprehensive empirical evidence of heterogeneous program effects across the welfare distribution. Second, the variation across the welfare distribution is used to describe the pro-poor or anti-poor bias of the common property forestry program under consideration. Third, we test for the presence of elite capture in the program.

2. Common property forest management in south western Ethiopia

Much like citizens of 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, such that 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 modern energy development, Mekonnen and Bluffstone (2008) expect this dependency to continue, and, most likely, to grow. In addition to providing fuel for energy, the forest offers agricultural risk mitigation services, providing alternative sources of income (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 begun to implement a new set of forest policies (Mekonnen and Bluffstone, 2008). One of those policies is the decentralization of forest management to the communities located near those resources. Due to that policy, a number of programs have been implemented in Chilimo, Bonga, Borena and Adaba Dodola (Neumann, 2008 and Jirane et al., 2008), with the general objectives of arresting deforestation, while improving the welfare of those 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, 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, which is the site of this analysis, Farm Africa/SOS-Sahel supported the implementation of JFM; more than six JFM programs have been established to improve the management of about 80,066 ha of natural forest (Jirane et al., 2008).

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. We now describe the process through which JFM programs were established. Farm Africa/SOS-Sahel and local government targeted specific forests as candidate sites for the JFM programs. Once a forest unit had been targeted, the location of the forest was topographically identified and then demarcated in the field. Information related to available forest resources as well as past and present management practices was gathered. Finally, an understanding of prevailing forest management problems, forest uses and forest user needs was developed (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, and commonly referred to as fuelwood sellers (Lemenih and Bekele, 2008; Gobezeet al., 2009 and Bekele and Bekele, 2005). These observations led Farm Africa/SOS-Sahel and local government to select forests surrounded by significant Menja populations (Lemenih and Bekele, 2008; Bekele and 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 varying degrees.

Once sites for intervention had been selected, Farm Africa/SOS-Sahel began 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 and Bekele, 2005). 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 and Bekele, 2005; Farm Africa's FMP Manual, 2006). Program participation amongst eligible households, however, remained voluntary, as long as the households satisfied the eligibility criterion and undertook to abide by the JFM's operational rules. Eligible households that chose to participate in the JFM program formed Forest User Groups (FUG) for selected sites. Those choosing not to participate would revert to using the nearest non-JFM forest, which, in effect, is a forest that operates under the status quo; that forest is unregulated, and access is open to all.

Experts from Farm Africa/SOS-Sahel and local governments, in collaboration with FUG members, then developed Forest Management Plans (FMP) stipulating the rights and duties of program members involving forest protection, forest development, forest product harvest rules and benefit share rules (Jirane et al., 2008). The FMP is implemented by a management committee in the community which comprises of a chairperson, a deputy chairperson, a secretary, a cashier and an additional member. Commonly, each member would be required to participate in forest development (planting new trees for the enrichment of the existing forest), guard against fire, vandalism (including unauthorized tree cutting) and agricultural encroachment (clearing forest for agricultural land acquisition). In return, each individual member would enjoy two kinds of rights over forest products: (1) a private right and (2) a collective right. The private right relates to the use of the forest for livestock grazing, collecting wood for energy and farm implement construction, harvesting medicinal plants for own consumption, and beekeeping, all subject to management committee approval. The collective right relates to the harvest of honey, timber, forest coffee, and spices, which members deliver to their Forest User Cooperative (FUCo),[5] which sells the products on both national and international markets. The FUCo retains 30% of total income for investment and distributes the remainder across the membership as dividend (Bekele and Bekele, 2005; Lemenih and Bekele, 2008).

Possibly the most important aspect of the program is that NGOs, along with the regional government, provide FUCos with assistance in marketing, processing, grading, certification (e.g. green labeling of forest coffee), packaging of non-coffee NTFPs, storing, provision of price information and market access. Although there is no doubt that the additional assistance confounds the program's effects, the assistance is best viewed as a subsidy to engender participation in the program. Previous research has shown that forest cover and forest productivity have improved under the JFM (Bekele and Bekele, 2005 and Limineh and Bekele, 2008). However, that improvement, on its own, is not likely to offset either the participation cost or the costs of harvest restrictions imposed by the program that are generally necessary for the long-term revitalization of forest resources. To offset both the upfront participation costs and the long-term harvest restriction costs, a subsidy of this nature may be necessary. NTFP marketing and market linkage assistance for FUCs offers a potential subsidy. Although forest coffee, for example, sells for as much as ETB 60/kg in non-JFM regions, Shumeta et al. (2012) find that

[5] FUCos develop from FUGs, once the program becomes completely operational (Jirane, 2008).

revenues from the sale of forest coffee are not equally split; approximately 13% goes to farmers, while 87% goes to intermediaries in the supply chain. Moreover, the same study argues that intermediary average profits are as much as 40 times higher than that of farmers. If the JFM program can capture a further proportion of the sales price, and pass that on to the farmer, farmers can benefit from participation, as can the environment. If those gains cannot be realized, farmers will either not subscribe to the program or the program will suffer from attrition; regardless, the environment would be expected to continue to suffer.

3. The Data

Data for the analysis was obtained from a household survey, designed for this study, undertaken in 10 Ethiopian villages in October of 2009. The villages are located in the Gimbo District, which is in southwestern Ethiopia. Sample frames for the survey were derived from the selected villages, via the lower level of local government, the kebele. The analysis was based on randomly selected households: 200 from JFM villages and 177 from non-JFM villages. Table 1 outlines the kebeles and the villages within the kebeles, including JFM participants and non-participants, and the number of survey respondents in each.

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, total number of Menja households in the village, the Menja population density, distance from the JFM forest, availability of alternative forests and experience with other collective action arrangements. Finally, data related to the community, especially forest cover and population, was gathered.

Descriptive statistics of that data are presented in Table 2, and these statistics are separated by participation status; thus the differences give some indication with respect to the vector of propensity score control variables. Therefore, the final column of Table 2 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 away from program forests, based on walking times. They are also nearly 10 minutes away from the nearest road, again measured by walking times. However, these households are located 26 minutes (walking time) 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 the farm, 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.

With regard to institutional variables, respondents' perceptions concerning enforcement of forest management rules were gathered. Specifically, respondents were asked to rate their perceptions regarding the enforcement rules to four different statements on a five-point scale. This included a response to a question whether or not a respondent himself, other villagers in general and the villages' authorities monitor who takes what product from forests. Moreover, it included responses to a set of questions concerning the penalty instrument used to punish non-compliance with rules. The responses to these questions were coded as — strongly disagree, disagree, neutral, agree and strongly agree. It was from these responses that we constructed an index following (Bluffstone et al., 2008; Beyene and Koch, 2013) as a measure of the quality of forest management enforcement institutions to deter non-compliance. Bluffstone et al. (2008) argue that perception based-indices are useful for two reasons as measures of the institutional quality in surrounding common property resource (CPR) management. First, in face of a pervasive mismatch between stated policies and on-the-ground management practices in developing countries, perceptions have a potential to reflect the reality. Second, compared to objective measures of CPR institutional quality through interviews with village leaders or forest managers, perceptions has a better appeal, because in the former village leaders or forest manager have difficulties characterizing the details of CPR rules facing individual households in their villages.

For the purpose of this study, per capita consumption expenditure, including goods produced at home, which were valued at village prices, rather than income, was used 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, 2011); similarly, it is easily interpreted and widely used. 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. Unfortunately, the choice of per capita expenditure is not without problems. It might be preferred to measure it in adult equivalence, which takes into account differences between children and adults, in terms of their nutritional and other requirements. However, inaccuracies in adult equivalence would result in sizable measurement errors, limiting its usefulness**.**

4. Theoretical and Econometric Framework

The framework is grounded in Roy's (1951) occupational choice model. We assume that farmers choose to participate in the JFM program based on utility maximization. Farmers, who perceive comparative advantage from participation, are assumed to join the program; thus, treatment assignment is non-random. In particular, define V_{ij} as the utility received by household $i = \{1, \dots, N\}$ in treatment regime $j = \{0,1\}$, where 1 represents participation. Therefore, $D_i = 1(V_{i1} > V_{i0})$, where 1 is an indicator function yielding 1, when the condition in brackets is true, and 0, otherwise. Similarly, define Y_{ij} as potential welfare, household per capita expenditure, where Y_{i1} is JFM welfare and Y_{i0} is non-JFM welfare. The difference between Y_{i1} and Y_{i0} can be used to measure the differential welfare associated with participation. Unfortunately, there is a missing data problem, as welfare in both states cannot be observed for the same household; welfare can only be observed in one state or the other for each household depending on whether it participates or not. Furthermore, if treatment assignment is random, then a simple dummy variable regression can unravel the effect of treatment; however, given that participation is voluntary, non-random treatment assignment is likely to prevail. Hence, a more involved procedure is required to unravel the effect of treatment; something in the mould of the propensity score matching method.

4.1. *Quantile Treatment Effect under Exogeneity Assumption*

The quantity of interest in this analysis is the τ^{th} quantile of the potential outcome given as

$$Y_i = q(D, X, \tau) \quad (1)$$

Describing the observable covariates of the data, denoted by X_i, and the real number $\tau \in (0,1)$, representing the quantile index, the quantile treatment effect on the treated at the τ^{th} quantile is given as

$$QTT|_{D=1,x} = q_{1i|_{D=1,x_i}} - q_{0i|_{D=0,x_i}} = inf_q\{\Pr[Y(1) \leq q] \geq \tau\} - inf_q\{\Pr[Y(0) - q] \geq \tau\} \quad (2)$$

where inf denote inverse function. An important maintained identification assumption of the parameter QTT in (2) is the familiar *unconfoundedness* assumption of Rubin (1986) which characterizes selection into treatment regimes on the basis of observable covariates. This assumption necessitates that once we control for x_i, it follows that selection into the treatment regime is independent of potential outcome or compactly denoted as; $Y_i \perp D|x_i$ (Heckman et al., 1998; Dehejia and Wahba, 2002). The immediate consequence of this assumption is $Y_i \perp D|p(x_i)$ where $p(x_i)$ is the propensity score function estimated by using logit or probit model. Moreover, it should be assumed that there exist a common support or overlap assumption, which requires that individuals that share same values of X, should have a positive probability of being both participant and non-participant (Heckman et al., 1999).

This assumption underpinned Firpo's (2007) estimator for QTT, which involves two steps; first estimation of the probability of program participation or propensity score is estimated non-parametrically; at the second stage, QTE is estimated as a difference between two quantiles emerging as a solution of minimization problems, where minimands are now the sum of weighted check functions, which are convex empirical processes;

$$(\alpha, \delta_\tau) = argmin_q \sum_{i=1}^{N} \omega_{1,i|D=1}\, \rho_\tau(Y_i - \alpha - D_i\delta_\tau) - -argmin_q \sum_{i=1}^{N} \omega_{0,i|D=1}\, \rho_\tau(Y_i - \alpha - D_i\delta_\tau) \quad (3)$$

Where ρ_τ is a check function evaluated at real number *such* that $\rho_\tau(a) = \mathrm{a}(\tau - 1\{a \le 0\}$ (Koenker and Bassett, 1978) and δ_τ is the quantile treatment effect (QTE) at τ^{th} quantile. Introduction of the individual weight $\omega_{ji}, j = 0{,}1$ is what makes equation (2) different from standard (Koenker and Bassett, 1978) specification. The individual weights, given as

$\omega_{1,i|D=1} = \frac{D_i}{NP(X_i)}$, $\omega_{0,i|D=0} = \frac{1-D_i}{N(1-P(X_i))}$ are introduced to correct for differences in the distribution of observable covariates between the program participant (treatment) and non-participant (control) groups, and hence allowing comparison across similar households (Firpo, 2007).[6]

4.2. *Non-ignorable Treatment Assignment*

In the previous section, our underlying assumption of QTE identification is that the study units self-select themselves into the treatment regime on the basis of observed covariates. However, such assumption is simplistic if there are unobservable determinants of participation, meaning that treatment assignment is non-ignorable, the preceding estimators will be biased.[7] In that case,

[6] Note that the weights are computed from propensity score, $P(X_i)$, where X_ia vector of observable is household's covariates. Propensity score here plays the same role as in estimation average treatment effect in control for selection bias arising from differences in distribution in observable covariates over common support.

[7] An IV method is justified on the basis of a sensitivity test of the propensity score matching estimator in Gelo and Koch (2012), which used the same data set to evaluate the average effect of treatment on the treated.

like in standard average treatment effect identification, an IV approach is, instead, needed to identify QTE (Chernozhukov and Hansen, 2006 and Abadie et al., 2002).

In this study a combination of instrumental variables were employed. One set of instruments relates to eligibility, due to the presence of the Menja tribe. This set includes a dummy indicator for the presence of individuals from the Menja tribe, and provides an indication of the intention to treat. In addition to this dummy indicator, the population density of the Menja tribe, and the total number of Menja households in the village, were also used as indicators of the intention to treat. In other words, these Menja variables are assumed to partly determine participation in the JFM, but do not directly affect welfare. 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 JFM. However, program eligibility was associated with deforestation, which could be associated with reduced household income and per capita consumption; therefore, the intention to treat IV implied by these eligibility instruments could be related to the outcome, which would bias the treatment effects estimates towards zero. For an upward bias to obtain, the presence of the Menja tribe would need to be associated with better welfare outcomes distribution for eligible households than ineligible households, which is unlikely, given the structure of the program selection process. In particular, the Menja's settlement (presence in a village) may follow covariates, such as village's access to roads and markets and the underlying condition of the forest, each of which can be related with outcome variables-per capita expenditure through their effect on household's income. Following Frölich (2007) and Abadie et al. (2002), exogeneity (randomness) of IV is thus, assumed to obtain upon conditioning it over these covariates.

Furthermore, if the instruments affect participants in different ways, interpreting the resulting treatment effects estimates can be complicated. The IV treatment effect literature refers to this complication as a form of treatment effect heterogeneity (Imbens and Angrist, 1994; Angrist et al., 1996; Frölich, 2007; Heckman and Vytlacil, 2005 and Todd, 2008). The problem relates to the powerfulness of the instrumental variable, Z_i, in the sense that all study units (farmers) exposed to the training about the program could be induced to participate in the program. In other words, if Z_i is powerful enough, exogenous variation in Z_i, from $Z_i = 0$ to $Z_i = 1$, leads to variation in in D_i, from, $D_i = 0$ to $D_i = 1$. In many applications, however, this assumption is not plausible, as powerful IVs are not available. If Z_i is not powerful, we will no longer be able to estimate the causal parameter for the full population; instead, it can only be estimated for a subpopulation, which is induced by their exposure to the training (whose decision to participate changes when the status of exposition to training changes). Imperfect compliance in reaction to the IV, warrants the application of a heterogeneous treatment effects model, instead of a constant treatment effects model, in order to identify a causal parameter; in this case, that parameter is the local quantile treatment effect (LQTE) (Abadie et al., 2002).

In the interest of better exposition, we present the following characterization. We describe our data as being comprised of n observations of continuously distributed outcome variable$Y$, binary treatment variable, D, a binary instrumental variable, Z and a $k \times 1$ vector covariates X. For concreteness, we, once again, follow the identification assumptions advanced by Abadie et al. (2002) and Frolich (2007 and 2008). To start with, we first partition the study population into subpopulation owing to how the treatment variable, D, reacts to instrumental variable, Z; such that $D_{1i} > D_{0i}$ (compliers), $D_{1i} = D_{0i} = 0$ (never treated) $D_{1i} = D_{0i} = 1$ (always treated) $D_{1i} < D_{0i}$ (deifiers). The following assumptions are made across these subpopulations.

i. Conditional independence: $(Y_{1i}, Y_{0i}, D_{1i}, D_{0i}) \perp Z|X$

ii. Monotonicity: $P(D_{1i} < D_{0i})$

iii. Existence of compliers: $P_c(D_{1i} > D_{0i}) > 0$

iv. Nontrivial assignment (common support): $0 \leq P(Z = 1|X) \leq 1$

Assumption (i) is a standard instrumental variable assumption of exclusion restriction. However, it postulates that exclusion restriction and unconfoundness of instrumental variable is only possible (equivalent to random assignment of Z) if the instrumental variable is conditioned on covariates (Abadie, 2002 and Frolich, 2007, 2008). It implies that if conditioned on set of covariates, instrumental variable shouldn't affect the outcome of individual i but through treatment channel. This is, in fact, plausible assumption in our study as we found covariates that reasonably affect, Z as well as the outcome variable. The monotonicity assumption (assumption (ii)) requires that the treatment variable, D, either weakly increases with Z, $\forall i$ or weakly decrease with, Z, $\forall i$. The third assumptions implies that at least some individuals react to movement in instrument with the strength of instrument measured by the, P_c , defined as the probability mass of compliers. The last assumption requires that there exist propensity score of instrumental variable.

If assumptions (i) to (iv) hold simultaneously, then treatment effect (QTE) estimate, δ_τ, is identified and recovered from the unconditional quantile regression model in (3). Parameters $(\delta_\tau,\ \beta_\tau)$ in (3) are estimated as follows

$$(\alpha_{IV}, \delta_{IV},) = argmin_{(\alpha,\delta_{\tau,})} \sum_i^N [\omega^{IV} \rho_\tau (Y - \delta_\tau\ D|D_{1i} > D_{0i}] \qquad (4)$$

Where $\omega_{IV} = 1 - \frac{D_i(1-z_i)}{1-P(Z_i=1|X)} - \frac{(1-D_i)z_i}{P(Z_i=1|X)}(2D_i - 1)$ is a new individual weight introduced to correct self-selection biases.[8] Parameter α_{IV} represents the difference, $Y_{q1} - Y_{q0}$, in τ^{th} unconditional quantiles of compliers defined as all individuals who are responsive to a change in Z within the support of Z. It follows that quantile treatment effect applies to this group. It is equivalent to traditional local average treatment effect. In the interest of robustness, we will implement both conditional (cf: footnote 14) and unconditional quantiles distribution models in (2) and (3).[9]

Moreover, in the interest of testing robustness of results from binary IV analysis, we included another set of level instruments, such as the distance of the household from the JFM forest, the availability of alternative forests, and participation in other collective action programs in the analysis. We followed Chernozhukov and Hansen (2008), hereafter denoted as CH-IV, for identification of QTE under over-identified instrumental variable case. By assumption, each of these household level instruments is expected to influence participation, but not welfare. The existence of a first-stage, with respect to these household level instruments is not particularly problematic. Households that have to travel farther to use a program forest should be less inclined to participate, which is also true for households that have access to an alternative forest. Similarly, households that have participated in other collective action programs could be more inclined to participate in another program, although it is possible that previous negative

[8] Note that Firpo's (2007) estimator in (2) is the special case of (3) under the situation of exogeneity where, $D_i = Z_i$ (Frolich and Melly, 2008). Note also that conditionally endogenous QTE is recovered from the estimator; $(\delta_\tau, \beta_\tau) = argmin_{(\delta_{\tau,}\beta_\tau)} E[k\rho_\tau(Y - \delta_\tau D - X\beta_\tau | D_{1i} > D_{0i}]$, where $\omega_i = 1 - \frac{D_i(1-z_i)}{1-P(Z_i=1|X)} - \frac{(1-D_i)z_i}{P(Z_i=1|X)}$ is the individual weight to correct self-selection biases.

[9] By virtue of the definition, if we are interested in low quantile for example, the conditional quantile effect summarizes the effect for individual with low Y even if Y is high in absolute terms. But, the unconditional quantile effect summarizes the effect on relatively low absolute Y

experiences would reduce the inclination to participate. The exclusion restriction related to these instruments may, however, be more contentious. For example, as was the case with the eligibility instruments, households farther away from program forests may be located near forests that are less degraded. Importantly, in a simple regression of per capita consumption against forest cover, access to alternative forests and previous participation in other collective action programs, not reported here, no correlation was uncovered, providing evidence in favour of these exclusion restrictions at the household level.[10]

5. Results and discussion

In this section, we present the results of estimates from different empirical strategies. In order to draw conclusions about distributional consequence of the program, the analyses are aimed at investigating whether the welfare distribution of program participants is everywhere above that of the control group, whether the program impact on welfare distribution are positive at some point and negative at other and whether the impact on the welfare distribution are concentrated over some range (bottom or top) of the distribution.

As a benchmark, we first present the results of the unconditional quantile model of non-parametric matching methods under the exogeneity assumption followed by an IV method in Table 3. We then present the results of a binary IV conditional quantile. The unconditional quantile model estimated under exogeneity (see the first column of Table 3), shows that QTEs are only positive and statistically significant at the seventh decile. We observe that the results from this model weakly reject the null hypothesis of constant treatment effect.

[10] Note also that the validity of CH-IV estimator does not depend on D being statistically dependent on Z, it will be valid in cases of weak-instruments as formalized by, for example, Stock and Wright (2000) as well as in more general cases of partial- or non-identification (Chernozhukov and Hansen, 2008). Moreover, Gelo and Koch (2011), who used the same data set as this paper, employed 2SLS to estimate ATT using this set of IVs, found that they were valid instrument, when tested against Stock-Yogo 10% critical value.

However, different results emerge when we control for endogeneity bias. Column 2 of Tables 3 reports the estimates of the unconditional quantile IV model. We see an interesting result here that the program effects are statistically insignificant at lower quantiles. This is consistent with anecdotal evidences that the poor do not benefit from participation in such programs.

Tables 3 about here

Moreover, for higher quantiles, the QTEs are positive and statistically significant. The results here show that the program welfare impact at median is ETB 653.40 (USD 51.79), which compares well with parametric and non-parametric local average treatment effect (LATE) estimates of ETB645.16 and ETB567.33 respectively. Thereafter, QTE rises to ETB863.40 (USD62.56), ETB896.5 (USD 68.44), ETB806.0 (USD63.89) and ETB 1,268 (USD100.51) at 0.6, 0.7, 0.8 and 0.9 deciles respectively. Two observations can be made here; first the program is largely heterogeneous within this group of welfare distribution as QTE widely ranged between ETB 653.4 - ETB1,268. Second, QTE increases progressively as we move up the welfare distribution beyond median welfare level.

We infer the following from these analyses. First, the results reject the null hypothesis of constant treatment effect along welfare distribution in favour of heterogeneous program effects. Second, statistically insignificant QTE for the bottom half of the distribution supports the hypothesis that the program is not pro-poor. Moreover, statistically significant QTE for the top half of the welfare distribution prove that the program welfare accrual is biased towards non-poor or conceivably rich program participants.

Results from the conditional quantile QTE with intention to treat IV estimation are presented in Table 4. Here again, QTEs estimates of the program at lower quantiles are not statistically significant suggesting that the program has not offered welfare gain to participants corresponding to these welfare points. However, QTE estimate at median welfare and above are all positive and statistically significant confirming that participants at these points of welfare distribution are the main beneficiaries of the program intervention. The results show that the program has raised median welfare by ETB407.9 (USD 32.33) which is lower by 37.57% compared to QTE estimate of IV-unconditional distribution. Moreover, it is lower than LATE estimates. The results also show that the program has raised the 75^{th} and 95^{th} quantiles of welfare by ETB550.5 (USD 43.63) and ETB979.8 (USD77.66) respectively.

Table 4 about here

We now turn to the result of CH-IV estimator. The results are presented in table 5. Here again we see that QTE tells the same story as estimates of unconditional and conditional quantile QTE with intention to treat IV estimation; that QTEs increases progressively as we move up the welfare distribution and that the QTE estimates are statistically significant at median welfare level and above suggesting that the program has raised welfare level corresponding to these points.

Table 5 about here

However, QTEs estimates here are substantially higher than their corresponding QTEs in the unconditional and conditional quantile QTE with intention to treat IV estimators. Median estimates here, however, appears to be higher than LATEs estimates reported earlier.

In nutshell, the results from both conditional and unconditional quantile distribution models reject the hypotheses of constant treatment effects and pro-poorness of the common property forestry management intervention. However, the results from either empirical strategy suggest that the benefit offered by the program intervention is skewed toward the rich subgroup of the population. These results led us to the conclusion that common property forestry management intervention in our study villages could not be defended on equity grounds, although it has raised the welfare (albeit heterogeneous) of some of the participant households.

We now return to the relation of our program impact evidences to prevailing evidences so far. Our results of heterogeneous program impacts, as opposed to constant program impacts across the welfare distribution, lend support to Adhikari (2004; 2005) and Cooper (2008), who concluded that common property forestry program is heterogeneous and hence has worsened inequality in Nepal. Moreover, the finding that the program impacts are concentrated in the top half of the welfare distribution without any bearing in the lower half of the distribution, support the empirical conclusion that the program is not pro-poor (Jumbe and Angelsen, 2005; Basundhara and Ojhi, 2000; Malla, 2000 and Neupane, 2003). However, our findings stand in sharp contrast with Herbert et al. (2013), who finds that participation in charcoal production under open access forest management regime in Uganda has increased income of the poor relative to non-poor charcoal producers. The observation that the poor are better off under open access forest management regime implies that instituting common property right management of the forest attenuate income of the poor, but bolsters that of the non-poor, an outcome to which our finding lends support.

But, the evidence that the program benefits are unequally distributed in favour of rich program participants begs the question of why we observe such an outcome.
Observers in the field contend that unequal program benefits distribution largely has to do with local power relations as mediated by wealth distribution and differential opportunity cost of program participation across income groups (Cooper, 2008 and Malla, 2000). Restriction of extraction of forest products and mandatory labour contribution demanded by the program management disproportionately disfavour the landless and those with limited opportunity of livelihood diversification or alternatives (Cooper, 2008).

Second, wealthier households are often the ones who dominate decision making and management activities of common property forestry programs (Maskey, Gebremedhin and Dalton, 2003 and Agrawal, 2001), opening up the opportunity for elite capture. This result is also obtained by Adhikari (2006) who observed that, although rich households bear higher transaction cost (management cost) of common property management, taking this cost as percentage of resources appropriation cost revealed that the poor incurs relatively higher transaction cost of management as compared to middle-wealth and rich households. Given that management decision being dominated by the wealthy, as for example in terms of fixing rate of resource harvest, pricing of the product in case of cooperative marketing, elite capture is inevitable such that such decisions selectively benefit the wealthy. In light of these backdrops, we now return to uncovering whether this outcome is underpinned by elite capture (see Table 6). In effect, we test whether the forest management enforcement institution is associated with welfare outcome across welfare distribution. We found that the strength of enforcement institution offers greater welfare benefit and that these effects happen both at lower (second quantile) and upper (sixth and seventh quantiles) points of welfare distributions. In fact, this is consistent with our priori. However, when we interact institutional variable with treatment variables (program participation), the results show that, across quantiles above median – the welfare points that supposedly corresponds to elite group members, the strengths of enforcement institution attenuates welfare benefit obtained from participating in JFM program. Conversely, the result shows that the weaker is the enforcement institution, the higher will be program benefit accruing to this group of program participants suggesting that institutional dodge has enabled elite group to siphon greater share of resource rent (benefit) generated by JFM program.

Overall, our analysis establish that the program benefits are skewed to the rich households, through the mechanism of capture elite capture as the result of weak institution of JFM enforcement rules.

6. Conclusion

This study is aimed at examining the distributional consequences of common property forestry management intervention. The analysis drew on data from 200 randomly selected program participant households and 177 non-participants households in selected villages of Gimbo district, southwest Ethiopia.

As opposed to existing literature, which often employ cost-benefit analysis (CBA) to evaluate distributional impacts of this intervention (Adhikari, 2005 and 2004; Basundhara and Ojhi, 2000), we implemented the potential outcome model (POM) framework model to establish a causal link between the program intervention and household's welfare and its distributions outcomes. Particularly, we employed quantile treatment effect (QTE) evaluation of the program intervention, we believe, would warrant stronger conclusion compared to extant cost-benefit analysis literature. We implemented QTE methods under different empirical identification strategies; under exogeneity assumption and IV-method to account for endogeneity bias.

The results of the QTE analyses, irrespective of the identification strategies, rejected the hypothesis of constant treatment effect in favour of heterogeneous treatment effect across welfare distribution. This would have been concealed in average treatment effect evaluation study. Tellingly, the program welfare effect has been concentrated on the top half of welfare distribution without bearing effect on the bottom half of the same. In other words, the results reinforce the contentions that the current common property forestry institution has not benefited the poor but has operated in favour of the non-poor. In effect, the apparent optimism, maintained by some observers, that common property forestry institutions offers equitable benefit is not supported.

There is the need to consider redesign of the program to ensure that its impact would reach out to the poor before implementing it as a dual policy of forestry management and rural development. Both command and control (CC) and incentive based (IB) approaches may be envisaged to redesign the program institutional structure. With regard to the former, regulations must be instituted and enforced to ensure greater participation of poor households in management decisions in cases where local elites manipulate management decision in their favour. An alternative option is to include leasehold and private property right within common property right structure which may bring about efficient and equitable transferability of property right (Adhikari, 2004). This voluntary exchange of rights within the bound of common property right structure may benefit poor people as was conjectured by Baland and Platteau (1996). In fact, these are interesting areas of empirical future researches.

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Table 2 .Descriptive statistics for post-project variables and welfare measures

Variable	Description	JFM participant		Non-participant		
		Mean	SE	Mean	SE	Mean difference
totexp	Total household annual consumption expenditure (in ETH Birr)	9531.32	389.593	9000.756	337.464	530.564
cpc	Per capita annual consumption expenditure (in ETH Birr)	1732.093	66.5836	1686.69	59.263	45.397
gender	Household head gender	.932	0.018	.943	0.016	-0.010
agea	Age of household head	43.916	1.019	43.244	1.023	-0.671
hhsize	Household size	5.899	0.165	5.7346	0.154	0.164
tlua	Household livestock ownership converted to TLU)	4.256	0.193	4.501	0.215	-0.244
lndsza	Household landholding size in hectare	2.300	0.110	2.412	0.114	-0.111
edumax	The maximum years of schooling in the households	6.257	0.220	6.707	0.220	-0.450
offrma	Whether a household participated in off-farm activities (yes=1)	0.145	0.026	0.082	0.019	0.063**
wealth	Whether a household has a corrugated house (yes=1)	.251	0.032	0.239	0.030	0.011
hhedua	Education years of schooling of household head	4.5	0.208	5.108	0.307	-0.608*
dsttown	Household distance to the nearest town	69.379	3.509	72.454	2.693	-3.074
dstroad	Household distance to the nearest road	23.639	1.935	32.295	2.614	-8.656***
malefa	Household labour-force (men)	1.449	0.055	1.478	0.059	0.028
femalefa	Household labour-force (women)	1.378	0.051	1.338	0.046	0.04
crdta	Whether a household has participated in credit market (yes=1)	0.307	0.034	0.219	0.029	0.087**

Table 3 Unconditional QTE

VARIABLES	QTE-exogenous	QTE-IV method
Quantile_1	91.71	-159.0
	(217.8)	(340.7)
Quantile_2	253.8	-95.40
	(219.3)	(261.5)
Quantile_3	285.4	207.2
	(277.7)	(309.8)
Quantile_4	179.9	229.7
	(298.4)	(357.4)
Quantile_5	350.8	653.4*
	(483.7)	(414.4)
Quantile_6	558.9	863.4**
	(674.7)	(388.4)
Quantile_7	844.7**	896.5*
	(422.6)	(489.4)
Quantile_8	576.7	806.0*
	(476.9)	(426.7)
Quantile_9	480.9	1,268**
	(530.7)	(539.0)
%Compliers		38.7
Observations	359	337

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

Table 5 conditional quantile binary IV- QTE estimates

VARIABLES	5^{th} coefficient	25^{th} coefficient	50^{th} coefficient	75^{th} coefficient	95^{th} coefficient
partcp	107.8	268.8	407.9**	550.5**	979.8*
	(226.5)	(1.450)	(171.2)	(230.2)	(521.3)
sex	-161.1	-3.051	11.69	-368.6	-522.0
	(613.3)	(-0.392)	(1,397)	(655.6)	(1,334)
wealth	67.08	19.76	176.6	51.93	-119.3
	(205.9)	(0.857)	(206.8)	(264.4)	(606.6)
offrma	189.6	72.24	232.3	382.5	91.00
	(275.0)	(0.920)	(175.0)	(258.9)	(776.7)
agea	2.561	-163.5***	-8.402	-7.346	-30.87*
	(7.741)	(-3.282)	(6.423)	(8.954)	(18.57)
tlua	29.78	-4.282	38.66	66.00	1.025
	(35.03)	(-1.246)	(41.50)	(43.38)	(98.22)
lndsza	43.52	-2.322	76.67	107.2	175.9
	(70.50)	(-1.580)	(74.81)	(138.5)	(127.1)
hhsize	-114.8	13.73	-172.1***	-204.2***	-239.1**
	(79.48)	(0.457)	(40.70)	(44.79)	(110.6)
hhdstroadmin	-1.092	56.88	-5.387	-5.800	-11.63
	(3.895)	(0.123)	(4.502)	(6.873)	(9.558)
hhdstwnmin	-1.339	113.3	-3.818***	-5.686***	-7.161
	(1.593)	(0.653)	(1.338)	(2.153)	(6.115)
edumax	12.78	254.4	39.21	28.70	83.90
	(27.97)	(1.060)	(41.97)	(43.86)	(112.5)
Constant	1,264	2,039***	2,502*	3,419***	5,737***
	(780.4)	(3.245)	(1,494)	(807.9)	(2,050)
% compliers	44.5	44.5	44.5	44.5	44.5
Observations	337	337	337	337	337

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

Table-5 CH-IV- QTE estimates

Variables	*Quanti-1*	*Quanti-2*	*Quant-3*	*Quanti-4*	*Quanti-5*	*Quanti-6*	*Quanti-7*	*Quanti-8*	**Quanti-9**
partcp	-292.5	345.8	182.6	26.83	767.3**	1,358**	1,277**	1,558**	3,266**
	(363.3)	(404.3)	(345.1)	(298.4)	(343.3)	(548.3)	(393.9)	(459.6)	(614.6)
offrma	317.0	349.6	269.7	35.50	14.35	50.83	-11.56	-67.59	-82.48
	(221.0)	(223.0)	(215.8)	(191.3)	(252.5)	(247.1)	(254.7)	(341.7)	(358.6)
lndsza	-23.50	12.29	9.990	-22.84	8.847	23.80	80.12	9.938	-5.459
	(81.40)	(96.36)	(102.3)	(71.12)	(85.82)	(124.7)	(82.24)	(119.8)	(113.6)
agea	-5.929	-1.220	-2.313	-2.063	-1.541	-2.866	-4.759	-1.137	-16.59
	(4.914)	(5.494)	(4.857)	(5.557)	(5.712)	(7.810)	(6.071)	(13.49)	(12.89)
sex	152.6	191.7	173.2	202.0	30.68	211.2	236.1	-1,959*	-1,953
	(414.3)	(297.7)	(287.1)	(216.2)	(346.6)	(506.8)	(753.3)	(1,126)	(1,434)
hhsize	-92.50	-126.3**	-129.6**	-140.6**	-192.5*	-136.7*	-140.7**	-153.0*	-157.9*
	(76.14)	(58.90)	(54.17)	(46.76)	(49.50)	(72.84)	(65.64)	(91.99)	(84.10)
wealth	118.7	190.1	221.8	77.86	190.4	269.9	310.8	299.2	287.9
	(234.3)	(230.5)	(234.4)	(271.4)	(245.5)	(346.2)	(254.8)	(310.9)	(357.9)
hhdstwnm	-3.424**	-4.008**	-3.564*	-3.549**	-2.827	-3.709	-2.993	-4.935	-9.341**
	(1.491)	(1.962)	(1.992)	(1.696)	(2.263)	(3.642)	(2.283)	(3.235)	(3.831)
hhdstroad	-0.0935	-0.0817	-1.249	-3.455	-7.018**	-5.554	-7.828	-6.469	-10.04
	(2.766)	(2.388)	(3.731)	(3.964)	(3.524)	(4.892)	(5.026)	(6.239)	(7.928)
tlua	33.44	17.35	23.14	48.80	46.70	60.27	45.18	70.78	99.39
	(34.53)	(39.40)	(41.10)	(29.96)	(40.93)	(42.56)	(37.29)	(47.50)	(76.35)
edumax	18.58	7.898	0.0281	25.92	42.04	24.25	8.720	-7.844	-100.2
	(33.12)	(38.11)	(34.30)	(32.38)	(35.82)	(42.99)	(35.73)	(45.98)	(64.56)
Constant	1,735**	2,230**	2,131**	2,024**	2,833**	2,903**	2,721**	5,499**	8,953**
	(812.4)	(563.3)	(592.7)	(546.1)	(851.9)	(1,298)	(782.6)	(1,454)	(1,863)
Observation	270	270	270	270	270	270	270	270	270

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

Table. 6 CH-IV- QTE estimates with institutional variable

VARIABLES	(1) coefficient	(2) coefficient	(3) coefficient	(4) coefficient	(5) coefficient	(6) coefficient	(7) coefficient	(8) coefficient	(9) coefficient
partcp	1,880	4,360*	394.1	704.5	3,850	2,402***	2,439***	2,633*	5,620***
	(433,206)	(2,332)	(837.0)	(779.3)	(14,975)	(728.5)	(753.5)	(1,723)	(1,792)
enforce2	405.5	708.8***	95.66	108.3	575.7	293.2*	240.7*	148.7	156.2
	(72,177)	(231.1)	(121.2)	(122.7)	(2,335)	(169.3)	(133.0)	(298.1)	(357.5)
interaction2	-278.9	-750.6	-97.81	-118.9	-672.1	-497.4**	-483.8**	-442.5*	-719.9*
	(72,217)	(457.0)	(165.8)	(141.7)	(2,507)	(194.1)	(194.7)	(301.3)	(375.5)
offrma	271.3	415.3*	326.0	36.64	3.434	-3.685	-60.85	-349.9	218.4
	(282.8)	(229.5)	(228.9)	(197.6)	(209.5)	(177.2)	(232.6)	(302.8)	(762.0)
lndsza	-44.14	75.75	79.56	-22.73	-1.935	64.21	47.66	-30.88	-68.93
	(102.6)	(104.9)	(96.43)	(87.90)	(100.5)	(76.72)	(84.15)	(106.3)	(206.8)
agea	-7.285	-3.835	-4.338	-2.043	-2.640	-2.704	-3.020	-1.240	-6.772
	(5.903)	(5.067)	(4.503)	(5.368)	(5.636)	(8.963)	(9.128)	(11.04)	(23.77)
sex	-112.8	-126.9	-6.848	188.7	-1,706	-235.5	-158.8	-2,148	-1,779
	(441.7)	(552.5)	(308.7)	(377.2)	(7,786)	(585.2)	(593.0)	(1,663)	(2,213)
hhsize	-110.3*	-121.5	-145.6**	-144.0***	-125.8*	-112.1	-92.53	-120.3	-178.4
	(65.68)	(86.07)	(64.82)	(49.36)	(72.90)	(71.36)	(81.02)	(75.63)	(145.0)
wealth	-81.25	173.2	171.5	86.07	155.2	140.5	179.7	497.7**	192.6
	(266.1)	(224.4)	(213.0)	(264.5)	(193.5)	(313.0)	(350.7)	(252.9)	(559.6)
hhdstwnmin	-3.565*	-4.006	-4.262**	-3.584**	-2.667	-4.685***	-4.371***	-5.077**	-8.723*
	(1.964)	(3.165)	(2.111)	(1.609)	(3.290)	(1.321)	(1.615)	(2.588)	(4.708)
hhdstroadmin	-2.967	-1.654	-2.078	-3.199	-8.134	-3.819	-3.149	-1.789	-8.857
	(4.113)	(5.877)	(2.983)	(2.480)	(7.139)	(3.174)	(2.471)	(6.886)	(10.24)
tlua	58.49	7.183	11.35	49.77	34.25	48.20	65.26	108.7**	96.74
	(46.97)	(47.62)	(44.73)	(36.26)	(37.47)	(38.04)	(40.59)	(49.80)	(150.4)
edumax	25.40	4.970	7.626	23.13	9.208	16.84	33.34	4.369	-84.67
	(37.75)	(37.41)	(34.11)	(36.08)	(47.16)	(55.80)	(50.08)	(41.47)	(110.6)
fc	237.3	-17.76	171.4	208.6	436.2*	266.7	73.07	-433.7	-1,711*
	(346.3)	(305.4)	(273.9)	(297.9)	(239.9)	(597.0)	(546.7)	(484.2)	(896.0)
totvlpop	-0.0403	-0.0460	0.0464	0.0100	-0.155	-0.0924	-0.0541	-0.0509	-0.165
	(0.299)	(0.432)	(0.151)	(0.111)	(0.259)	(0.194)	(0.207)	(0.225)	(0.254)
Constant	-367.0	-1,677	1,664	1,468	1,095	1,206	1,187	4,431**	8,293***
	(432,675)	(2,265)	(1,183)	(1,313)	(5,098)	(1,289)	(1,273)	(1,880)	(2,533)
Observations	200	200	200	200	200	200	200	200	200

Standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$