Do ICDPs Work? An Empirical Evaluation of Forest-Based Microenterprises in the Brazilian Amazon

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ABSTRACT. *This paper evaluates public investments in forest-based microenterprises as part of an integrated conservation and development project (ICDP) in the Brazilian Amazon. We combine matching with regression to quantify the effects of program participation on household income, wealth, and livelihoods. We find that participation increased cash and total income and asset accumulation, suggesting that the microenterprises contributed to the development goals of the ICDP. There is no clear evidence, however, that the microenterprise program helped achieve the ICDP's conservation goals of shifting household livelihoods away from agriculture and into sustainable forest use.* (JEL O12, O13)

I. INTRODUCTION

The local welfare impacts of protected areas are a subject of increasing concern and scrutiny (Naughton-Treves, Holland, and Brandon 2005; CBD 2008; Andam et al. 2010). Integrated conservation and development projects (ICDPs) are a common strategy for promoting the social and economic development of people living in and around protected areas in the tropics (McShane and Wells 2004). ICDPs often focus on increasing the incomes of local people by involving them in new economic activities. These income-generation alternatives are designed to require intact ecosystems for inputs, draw labor out of environmentally harmful activities, and/or compensate for and thus encourage acceptance of legal restrictions on previously accessible natural resources. The expectation is that as these activities become more important in household portfolios, conservation goals will also be achieved by derived demand, "distraction" (Ferraro and Simpson 2002), or compensation. Thus, ICDPs seek to improve the welfare of local populations both as a means to conserve ecosystems and as an end goal.

While ICDPs remain a popular strategy, most assessments of the approach have found little evidence of success (Naughton-Treves Holland, and Brandon 2005). In one of the earliest and best-known reviews, Brandon and Wells (1992) said that many projects struggled to meet both their conservation and development goals. Barret and Arcese (1998) concluded that ICDPs "are not yet analytically or empirically sound approaches," while Wilshusen et al. (2002) argued that critics of ICDPs often do not compare them against a realistic counterfactual that recognizes the preexisting rights and traditions of resource use in most protected areas in the tropics. Garnett, Sayer, and du Troit (2007) find that ICDPs are more likely to be effective when there is good governance, an understanding of demographic trends and local resource limitations, and human capacity development. However, their conclusions—like much of the debate over ICDPs—are based on little rig-

Land Economics • November 2011 • 87 (4): 645–681
ISSN 0023-7639; E-ISSN 1543-8325

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orous, quantitative evaluation. There are a growing number of impact evaluations of protected areas (e.g., Andam et al. 2010), but these generally employ aggregate data to assess the total net effect of protected areas. Given the many options that park managers have for engaging local people, it is important to rigorously assess the causal impacts of specific conservation investments (Ferraro and Pattanayak 2006; Lund, Balooni, and Casse 2009).

The effects of a particular ICDP on household welfare and livelihoods depend on the specific activities promoted by the project, as well as the economic, environmental, and social context. Studies of traditional forest communities have shown that in spite of seemingly homogenous socioeconomic and environmental conditions, local people find diverse ways of meeting their needs. Coomes, Barham, and Takasaki (2004) argue that a weakness of conservation-development initiatives "lies in their founding on a limited understanding of the microeconomic logic that gives rise to livelihood heterogeneity among forest peoples." Gauging the implications of a proposed project, therefore, requires understanding the economic decisions facing local households.

This paper responds to calls for both rigorous impact evaluations of conservation interventions and better understanding of why and how these interventions work in the context of heterogeneous rural livelihoods. As Deaton (2009) and Ravallion (2007) point out, understanding heterogeneity is fundamental to evaluating development interventions. Specifically, we assess the patterns and effects of household participation in a forest-based microenterprise program as part of an ICDP in the Brazilian Amazon. Unlike many program evaluations, we draw on household production theory to develop a conceptual framework that guides the empirics by identifying factors expected to affect participation and outcomes. Consistent with most program evaluations, we estimate the mean impacts of the microenterprise program on households who participated. Specifically, we assess whether participation increases income and wealth and whether it shifts livelihoods away from agriculture. We accomplish this by combining propensity score matching with regression, and comparing the results of several different methods for imposing common support and weighting observations.

We find that participation in the ICDP microenterprises led to "development," as measured by more cash income, more total income, and greater gains in durable assets over the decade that the program was in place. This suggests that the microenterprises may be compensating households for restrictions imposed by the national forest, although we do not have sufficient data to empirically test this by constructing a counterfactual scenario "without" the entire national forest. In contrast, we find no effect on production of the key agricultural commodity (measured by either land area or output) and an economically small (and statistically less robust) effect on time spent collecting forest products, suggesting that the microenterprise program has not shifted livelihoods enough to generate "conservation" through derived demand or distraction.

II. THE CASE STUDY: THE TAPAJÓS NATIONAL FOREST AND PROMANEJO

The first national forest in Brazil was created in 1974 on the eastern bank of the Tapajós River in the Amazonian state of Pará. The government intended to use the forest for timber production and did not recognize the presence of approximately 20 traditional communities along the river inside the boundaries of the new national forest. Households in these communities depended on subsistence agriculture, employing traditional slash and burn practices as well as hunting, fishing, and collecting forest products. In the years following the declaration of the national forest, conflicts arose between forest residents and the government agency charged with managing the forest. These conflicts revolved around access to forest products and shifting cultivation. In 1997, on average, a quarter of the area within 5 km of each community had been cleared for agriculture. By that time, there were also active discussions between the administration of the national forest, the communities, and their advocates regarding the communities' legal status and use rights to natural resources. In a conservation law passed in 2000, the Brazilian government officially recognized the rights of traditional populations living within conservation units (Soares 2004). And in 1999, a new pilot project to showcase the ICDP concept in the Tapajós National Forest was launched.

ProManejo, or the Forest Resource Management Project, funded a series of activities in the Tapajós National Forest between 1999 and 2006 with support from the World Bank, the Brazilian government, and the German development bank, KfW. The activities supported by ProManejo were typical of ICDPs or community-based natural resource management, including community participation in management of the national forest and environmental education. The core activities also included initiatives to increase and diversify household income. Nongovernmental organizations led these initiatives by working with community associations to establish microenterprises for ecotourism and the production and sale of natural oils, artisanal wood products, and items made from a natural rubber fabric called “ecological leather.” The stated goal of this microenterprise component of the ICDP was to create economic alternatives based on multiple uses of the forest that would be economically and environmentally sustainable. We evaluate the microenterprise program specifically, examining its impacts on households over and above any impacts of ProManejo and other management and legal changes that applied to all communities and households in the national forest.

III. CONNECTING THE MICROENTERPRISE PROGRAM TO HOUSEHOLD DECISIONS

Households voluntarily participated in the microenterprises.[1] The decision to participate depended on the expected benefits, which in turn depended on the household’s assets, livelihood strategies, alternative economic opportunities, and skills congruent with program activities. For households in the Tapajós National Forest, their two key resources are labor and land. Thus, our point of departure for building a conceptual framework is to use the literature and our survey of households in the national forest (described in Section V) to understand labor allocation patterns and land tenure.

Households are engaged primarily in growing crops to consume and to sell. The staple crop, manioc, is cultivated using labor and hand-held implements. Households supplement income by working for others on a sporadic basis. Half of surveyed households had a member who participated in the labor market in the previous year, with an average participation of 19 days. In addition to growing manioc and working sporadically in the rural labor market, households extract and transform local natural resources to supplement consumption. About 80% collected forest products at least once in the previous year and reported that they would need at least R$100 to replace the value of forest products that they consume (one U.S. dollar was roughly equal to two Brazilian reais at the time of the survey). These statistics mask substantial heterogeneity in household livelihoods, also found by other studies in this same area. For example, Pattanayak and Sills (2001) found that forest dependence varied by wealth type: households with more cattle made fewer forest collection trips, while households with more possessions took more trips. Bowman, Amacher, and Merry (2008) also found lower forest dependence (specifically, less hunting) among households who own more cattle.

The communities occupy a swath of land that is within the bounds of the national forest but is now zoned to allow community land use. Within this zone, households originally established agricultural land by clearing forest. Further expansion of this agricultural area is constrained both by the distance to land not already cleared, and by limitations on deforestation imposed by traditional community norms and the official rules of the national forest. Specifically, households are supposed to

[1] Project documents and community meetings confirm that participation was open to all households and voluntary. While households could enter and leave the projects, it was apparently easier to join at the beginning. Both outside consultants and community leaders probably played a role in encouraging households to join and to stay engaged in the microenterprises. Other than this informal process, there was no systematic administrative targeting or selection of households.

obtain authorization from the Brazilian Institute of Environment and Renewable Natural Resources (IBAMA). The multistage authorization process requires IBAMA to evaluate requests using its land-use data base and rules that limit the size of agricultural areas (IBAMA 2004), followed by community meetings involving all interested parties. Community associations appear to more effectively limit the location and total area of land that can be claimed and cleared by a household. In this institutional context, there is no conventional market for land, and because households cannot claim legal title, there is no incentive to deforest to strengthen formal property claims, as suggested by Alston, Libecap, and Mueller (2000).

The ProManejo microenterprise program involved several activities that all centered on selling a forest good or service that local people could produce with some training. Community associations managed the microenterprises by making decisions about production and remuneration. Remuneration schemes varied by product and by community. In the ecotourism activity, tourists paid local trail guides a flat fee for guided hikes. In the production of artisanal wood products, natural oils, and ecological leather, participants earned a daily wage, which in some cases was adjusted for worker productivity. One report indicates that workers in the natural oils activities, for example, received R$15 to R$25 per day (Mazzetto Silva 2005), more than the going rate for casual agricultural labor of R$10 to R$15 per day.

Descriptive and anecdotal information from project documents suggests that enough tourist visits and product sales occurred to potentially have increased participant incomes, especially since there were previously no organized efforts to produce oils for sale or attract tourists to the communities. For example, in 2005 the ecological leather microenterprise, which involved only nine households, is reported to have earned a total of R$6,000 (sales minus taxes and transport costs). The natural oils microenterprise in São Domingos produced 250 L of oil in 2004, which it sold for R$50/L. And in 2006, 592 tourists visited the Tapajós National Forest and paid a total of R$3,600 to trail guides in Maguari alone, not including any payments to host households or purchases of handicrafts (Mazzetto Silva 2005).

A Model of Household Decisions

We develop a household model to guide discussion of what types of households are more likely to participate in the project and how participation affects labor allocation and land use. To focus on the core economic decisions facing households in the Tapajós National Forest, we adopt the following stylized facts. Prior to the project, households consume forest products, while they can both consume and sell agricultural products. The two inputs into production are labor and land. We abstract away from leisure and focus on the decision to allocate work hours (L) (cf., Bluffstone 1995; Tachibana, Nguyen, and Otsuka 2001). While we assume one market wage, we allow for market imperfections by limiting the amount of labor that households can sell. We treat household endowments of agricultural land as quasi-fixed: the household has T hectares of agricultural land but may use labor to clear additional land if there is sufficient incentive. Although households cannot sell land, they can always fallow or abandon crop land.

Households have a strictly concave utility function that depends on consuming a composite forest good (C_{f}) and a composite market good (C_{m}), which includes the agricultural good. Households can send members to work in the labor market or to produce agricultural or forest goods. To earn cash, households sell the agricultural crop or work for a wage. We suppose that the agricultural production function $A(\cdot)$ exhibits decreasing returns to scale, possibly from employing increasingly distant or marginal land or from the supervisory costs of employing nonhousehold labor. Production depends on labor and the household's agricultural land endowment (T) plus the land it decides to clear (T_{c}). Clearing a hectare of land uses ρ days of labor, which may vary due to differing distances and qualities of available forestland and differing institutional constraints to expanding agricultural area.

Nuts, fruits, oils, and resins are examples of forest goods that households can produce using labor and forest resources. We assume that households do not sell forest goods primarily because of the significant transaction costs of accessing the market.[2] Households collect and transform forest resources into the forest good for consumption according to a generic production function $F^i(\cdot)$ that at higher labor levels exhibits decreasing returns to scale locally as households must go further into the forest to collect more resources. Skill and knowledge required to produce the forest good vary across households (cf., Pattanayak and Sills 2001) as reflected in the household superscript on the forest good production function. We assume that households produce the forest good with family labor because of the difficulty of monitoring workers hired to harvest forest goods. A household's production of the forest good is then given by $F^i(L_f)$, where L_f is labor allocated to production of the forest good. More forest skills and knowledge makes labor more productive at every level by allowing a household to collect more forest goods or harvest more game per hour.

Households maximize utility subject to a cash income constraint, a forest-good consumption constraint, and an imperfect labor market constraint. The imperfect labor market constraint parameter (or slackness parameter) M_l reflects the limited possibility of households hiring out labor in the study area (cf., Pattanayak and Kramer 2001). Households choose how much labor to allocate to agriculture (L_a) and to forest-good production (L_f). Labor allocations determine the quantities of the market good and the forest good that a household can consume. We assume that households do not specialize exclusively in forest good production, in other words, the condition $L > L_f$ always holds.

Formally, the household solves

$$\underset{L_a, L_f, T_c}{Max}\ U(C_m, C_f),$$

subject to three constraints:

1. Cash constraint: $p_a A(L_a, T + T_c) + w(L - L_f - L_a - \rho T_c) \geq p_m C_m$
2. Forest-good consumption constraint: $F^i(L_f) \geq C_f$
3. Imperfect labor market constraint: $M_l \leq L_f + L_a + \rho T_c - L$

Household labor allocations determine how much of the market good and forest good it can consume:

$$C_m = \frac{p_a A(L_a, T + T_c) + w(L - L_f - L_a - \rho T_c)}{p_m};$$
$$C_f = F^i(L_f).$$

The three choice variables are agricultural labor, forest labor, and land to clear. The Lagrangian is

$$\mathcal{L} = U(C_m, C_f) - \lambda_1(M_l + L - L_f - L_a - \rho T_c), \quad [1]$$

where p_a is the price of the agricultural good that the households sells, w is the wage, and p_m is the price of the composite market good.[3] In the nonseparable case, an optimizing household will allocate agricultural and forest labor till the ratio of the marginal value products equals the ratio of marginal utilities:

$$\frac{p_a A_{la}}{p_m F^i_{lf}} = \frac{U_{cf}}{U_{cm}}, \quad [2]$$

where U_{cm} and U_{cf} are the marginal utilities of consuming the market good and the forest good, and A_{la} and F^i_{lf} are the marginal productivity of agricultural and forest labor. See Appendix A for first-order conditions. In the separable case, the labor market constraint does not bind ($M_l < L_f{}^* + L_a{}^* + \rho T_c^* - L$ at the optimum), and labor will be allocated to agriculture and land clearing according to market and technological parameters as shown in conditions [3] and [4]:

$$p_a A_{la} = w, \quad [3]$$

$$p_a A_T = \rho w. \quad [4]$$

[2] Commercial boats that offer transport to passengers and goods pass by many of the communities only once a week and take 3.5 to 12 hours to reach Santarém. Households never send goods unaccompanied to Santarém. While the same constraints apply to agricultural products, manioc flour is easy to store and transport and has a larger, more complete market with many buyers and well-known prices.

[3] p_a is a component of p_m.

Condition [4] is conceptually similar to condition [3]; a household will clear land until the marginal value product of an additional unit of land equals the cost of clearing it, ρw.

Introducing the ICDP

We conceptualize the ProManejo microenterprises as a single program to train local people in commercial production (e.g., in standardized units with labels) and develop a market for what was previously a nonmarketable forest good or service. We assume that participants receive a fixed program price p_p per unit of the program good. Recall that production of forest goods depends on a household's skill and knowledge. This carries over to the program, allowing a more skilled participant to produce more of the program good per hour. Some households, therefore, earn a higher effective "program wage" than others, where the program wage is $p_p F^i_{lf}$, the program good price multiplied by the marginal productivity of forest labor. Program rules require that households participate with family labor.

To reflect the limited markets for the program good, we impose a quota Q on sales by participant households. To further structure the model, we assume that the quota binds for participant households. That is, if an optimizing household has incentive to sell some units of the program good, it will sell Q units.

We focus on understanding which households are likely to participate, and the effects of households allocating labor to the project until the quota binds. A sufficient condition for participation is if the project wage exceeds the market wage w:$p_p F^i_{lf} > w$. If the condition holds, even a household that hired in labor prior to the project will have incentive to replace some family labor allocated to agriculture with hired workers and have the freed family labor participate in the program.

When the labor market constraint binds, a household compares the program wage against its shadow wage that results from its allocation of labor between agriculture, land clearing, and forest good production in the absence of the project. Given that the household would want to work more in the labor market but is constrained from doing so, the shadow wage will be lower than the market wage (see the model derivations in the appendix). If the labor market constraint binds, therefore, a household may participate even if the project wage is less than the market wage, $p_p F^i_{lf} < w$. In all cases, a key factor affecting participation is the household's forest skill and knowledge, which in turn affects their program wage, reflecting their competitive advantage in the program good.

If households decide to participate based on accurate information about the returns to labor (in all four options: L_m, L_f, L_a, and ρT_c), participation will increase total income. This in turn could increase consumption and/or wealth. The technological and resource constraints facing households determine how new income opportunities affect forest use and agricultural production. Unless the program absorbs enough labor to have a general equilibrium effect on the market wage, participation in the program reduces agricultural production and land clearing only for households for whom the labor market constraint was binding.[4] For these households, participation raises the shadow value of labor and, therefore, the cost of employing additional labor in agriculture or land clearing. Unconstrained households will make agricultural and land clearing labor allocation decisions based on the market wage. Unless the program raises the market wage, it will not affect labor allocations determined by conditions [3] and [4].

Because the program involves producing a forest good, the direct effect of participation is to increase labor allocated to the forest. However, because forest products require household labor, households may reduce time allocated to collecting subsistence goods from the forest in favor of time producing the program good. Again, because households use only family labor to produce forest goods, this result does not depend on whether the labor market constraint is binding.

[4] As with most program evaluations (Ravallion 2007), we do not consider possible spillover effects that could have affected the incomes of nonparticipants. Thus, our analysis underestimates the effects on participant households to the extent that there are positive spillovers to our matched comparison households.

To summarize, the model's key theoretical predictions are the following:

1. A household will participate in the microenterprises if the program wage exceeds the return from alternative activities. In the separable case, this requires $p_p F^i_{lf} > w$; in the nonseparable case, the program wage must only exceed the marginal value product of agricultural labor: $p_p F^i_{lf} > p_a A_{la}(L^*_a, T + T^*_c)$.
2. Assuming that the project does not affect the wage (i.e., no general equilibrium effect), participation reduces agricultural production and forest clearing only for households affected by the binding constraint on labor market participation, by raising their opportunity cost of labor (the nonseparable case). In the separable case, households could hire in labor to replace family labor reallocated from agriculture or forest clearing to the program. In both cases, participation increases total labor allocated to the forest but decreases the production of subsistence forest goods by increasing the opportunity cost of time.

IV. INTEGRATING THE CONCEPTUAL MODEL AND EMPIRICAL METHODS

Our empirical focus is twofold. First, which households participated in the microenterprises? Second, how did participation affect welfare and livelihoods? These two questions are inextricably linked, because the first determines what we observe about the second. For ex post evaluation of the ProManejo microenterprise program, we focus on the average impact of participation on participating households, commonly referred to in the impact evaluation literature as the "average treatment effect on the treated" (ATT). This is also informative for policy makers and NGOs interested in replicating the project elsewhere, because it assesses the impact relative to what would have happened had the project never existed.[5]

To estimate the impact, we must address the heterogeneity (in land, labor, and skills) that determines the potential benefits households derive from the program and, therefore, their participation in it. In particular, since households chose to participate, we must address the possible bias introduced by self-selection to construct a relevant comparison, that is, the counterfactual income, wealth, and livelihoods of participant households had they not participated in the program. Our conceptual model provides a guide to the variables that influence participation. These are employed first as covariates in probit and ordinary least squares (OLS) models (with robust standard errors to allow for heteroskedasticity),[6] in the most basic approach to answering our two questions. Then we match on observables and correct for any remaining differences in the covariates to obtain the best estimates of the causal effects of the program.

The relevant covariates include the household's labor endowment and other key assets that define a household's income-generating ability and the trade-offs it faces with participation. Our conceptual framework suggests that households with more labor are more likely to participate, since the costs and barriers to expanding agricultural land mean that the labor market constraint is more likely to bind, although it is also possible that income effects or variation in M_l across households could have the opposite effect.

Forest skills and knowledge also affect participation and, though unobservable, are related to observables. Our treatment of forest skills and knowledge follows ideas on capability formation (Heckman 2007). A child's development of capabilities depends on the parents' capabilities and the child's current capability. In our context, parents with forest skills can more easily invest in their child's forest skills. And since capability is self-reinforcing, development of forest skills early

[5] If the program were likely to be expanded to include households not yet participating, the average treatment effect or the average treatment effect on the untreated would also be relevant. However, funding for ProManejo has ended, and there is no discussion of a regionwide rollout of the microenterprise program with other funding.

[6] Because of the small number of villages and observations per village, we do not calculate clustered standard errors by village. Hansen (2007) shows that using a clustered robust approach works well with many clusters and many observations per cluster. There is no theoretical justification, however, for using cluster-robust methods with few clusters with equally few observations per cluster (Imbens and Wooldridge 2008).

in life fosters later skill formation. The initial decision to collect forest goods depends on household endowments. As the household spends more time in the forest it accumulates knowledge and skill and possibly a preference (lower disutility) for such work (Pattanayak and Sills 2001). The age of the household head, whether he was born in the area, his local experience (years living in the community), and his long-term livelihood strategy, which is captured by variables like extent of cattle pasture, should correspond to household accumulation of forest knowledge and skill. We therefore consider endowments and household background, rather than innate ability, to determine the unobservable that affects household production of the forest good.

We include community dummy variables to capture differences across communities, like quality of agricultural land, which may affect participation and outcomes. Including village dummy variables also helps to control for the factors that may have affected project placement. Not all communities had a microenterprise, and some had more than one. Placement was unsystematic, with a particular story for each microenterprise.[7]

We also include measures of social, human, and physical capital that may shift relative expected returns from program participation and alternatives. The location of the household in the community may proxy for social capital, which could be important for participation, since community associations manage program activities. The number of children living outside of the Tapajós National Forest may be negatively related to participation, because it expands households' economic opportunities if these children help to form alternative networks through which goods, capital, and labor flow, as described by Ehringhaus (2007). Formal education may be related to a household's ability to pursue nontraditional economic opportunities. More physical assets could mean that a household has more alternative economic opportunities and/or greater ability to participate in the program (e.g., canoes that facilitate collection and transport of forest products).

We assume that these covariates are sufficient to model the potential outcomes of participation (e.g., selection on observables) and use a mix of regression and matching techniques to identify the ATT for a variety of outcomes. Regression and matching may yield different results because of differences in weighting (due to matching with replacement) or common support (due to OLS use of treatment observations that do not have similar control observations) (Angrist and Pischke 2009). By using several different rules to trim the sample and by estimating both the simple difference in means and an OLS model with covariates, we test whether the results are robust to these methodological choices.

Under selection on observables, the expected value of the outcome for a participant household had it not participated can be represented by the outcome for a nonparticipant household that looks like that participant household in terms of observable variables. Methods that match participants and nonparticipants are increasingly used to quantify the impacts of conservation and development interventions (Bernard, Taffesse, and Gabre-Madhin 2008; Costello, Gaines, and Lynham 2008; Somanathan, Prabhakar, and Mehta 2009; Andam et al. 2010). Matching on the propensity score in particular reduces the dimensionality of the matching problem. In practical terms, this allows a rich specification of the propensity score, including higher-order polynomials (Rosenbaum and Rubin 1983).

Matching can be used to preprocess data prior to parametric modeling. For example, Crump et al. (2009) suggest trimming the sample based on the propensity score prior to regression estimation. As Ho et al. (2007) argue, this nonparametric preprocessing makes causal inference less model dependent, while

[7] For example, the "ecological leather" concept was introduced to Maguari by an Argentine traveler. The communities with microenterprises for producing natural oils had some households who had produced the oils previously. A nongovernmental organization with experience working in the community of Pini obtained ProManejo funding for the first wood-working activity. Initial success in Pini led members of other communities to solicit support from ProManejo for wood-working microenterprises (Mazzetto Silva 2005). In the case of ecotourism, the project administrators hired a consultant to define the project's "ecotourism plan," which targeted relatively accessible communities for the greatest involvement (IBAMA 2007).

TABLE 1
Community and Household Participation

Microenterprise	Communities with More Than One Participant	Participant Households
Ecotourism	11	58
Wood furniture	9	64
Natural oils	4	24
Ecological leather	1	9
Program	14	123

the second-stage parametric modeling can both reduce bias and increase efficiency.

More typically, participant households are matched with nonparticipant households (with replacement), and then the ATT is estimated directly from the difference in means. Under the assumption of selection on observables, matching identical (in terms of observable variables or the propensity score) households will yield unbiased estimates. In practice, however, matching techniques minimize differences in covariates between matched pairs (i.e., participants and nonparticipants) but do not eliminate all differences, especially in small samples with a limited number of control observations to draw from. Any remaining differences in covariates can generate bias. For example, if cattle holdings increase income and matched nonparticipants still had more cattle than participants, then the difference in cattle holdings would bias the income effect of participation downward.

Abadie and Imbens (2002) discuss how to correct for these remaining differences. Abadie et al. (2004) explain how to combine matching and regression to calculate a bias-corrected estimator, which provides the main point estimates for this paper. Their estimator matches control units with treatment units on a vector of covariates, weighted by the inverse variance matrix, with replacement. The outcome variable is then regressed linearly on the same covariates, using only the selected control units (weighted by the number of times they are matched), to obtain the coefficient vector $\hat{\boldsymbol{\beta}}_0$. Recall that we are asking about the counterfactual: had the participant household not participated, what would have been the expected value of the outcome. Thus, $\hat{\boldsymbol{\beta}}_0$ is multiplied by the difference in covariates between matched pairs and subtracted from the difference in outcomes between participants, $yi(1)$, and those of the matched nonparticipants, $\tilde{y}i(0)$, where 0/1 indicates participation status and the tilde indicates the outcome for the nonparticipant observation matched with the participant observation i. In summary, the sample average treatment effect on the treated (*SATT*) is estimated by

$$SATT = \frac{1}{n}\sum_{i=1}^{n}\left[y_i(1) - \tilde{y}_i(0) - \hat{\boldsymbol{\beta}}_0(\mathbf{x}_i(1) - \tilde{\mathbf{x}}_i(0))\right], \qquad [5]$$

Where $\mathbf{x}_i(1)$ and $\tilde{\mathbf{x}}_i(0)$ are vectors of covariates for the participant household i and the matched nonparticipant household, and n is the number of participant households.

V. DATA AND VARIABLE DEFINITIONS

Our empirical analyses rely on a survey of a random stratified sample of 355 households in 20 communities in 2006. This sample represents more than half of the area's population of 627 households. Table 1 shows how many communities and households participated in each microenterprise and in the overall program, according to the survey.

The program focused on increasing household income and wealth. We calculate both cash and total income. Cash income is computed by aggregating net cash earned from the host of activities available to households. The survey elicited the costs and net sales of all major activities, such as raising chickens, cows, or pigs and growing manioc, corn, or beans. Calculating total income requires that we assess the value of subsistence production. We use local prices to value agricultural production consumed. Because forest production is more diverse—both in terms of specific products and their units—we elicited this value directly from households by asking how much money they would need to replace goods taken from the forest. The response, though informative, is imprecise. For wealth, we construct two indices of physical assets using statistical[8] and price-based weights.

[8] For the statistical approach, we use the first principal component, calculated with weights that maximize the variance of a linear combination of asset holdings.

TABLE 2
Variable Descriptions

Variable	Description
Outcomes	
Cash Income[a]	Annual household cash income (wage income + sales − intermediate inputs)
Imputed Nonforest Income	Value of nonforest goods sold and consumed by household, imputed using local market prices
Forest Income	Total stated value of forest goods sold and consumed by household
Total Income	Nonforest Income plus Forest Income
Time Collecting Forest Products	Hours per month spent collecting forest products by household heads
Assets in 2006[b]	Price weighted index of household durables in 2006
Change in Assets 1997–2006	Change in price weighted index of household durables (1997–2006)
Index of Assets in 2006	Principal component score based on household durables in 2006
Manioc Production	Household manioc production in kilograms
Manioc Land	Hectares of manioc cultivated by the household
Agrochemical Expenditures	Expenditures on agrochemicals
Covariates	
Pasture	Hectares of pasture managed by household
Index of Assets in 1997	Principal component score based on household durables in 1997
Family Size	Number of individuals in the household
Born in Pará	Equals 1 if household head was born in the state of Pará
Education	Years of education of household head (1 = first grade, 14 = university)
Age	Age of head of household
Time Spent in Community	Percent of life that household head has lived in the community
Distance to School	Walking distance from household dwelling to community school
Children Out of National Forest	Number of children living outside the Tapajós National Forest

[a] All monetary values are in 2006 reais (the Brazilian currency).
[b] See Appendix B for a list of assets used in each index.

These indices include items ranging from domestic items like a gas stove to tools like a chainsaw (see Appendix B). The information on assets in 1997 is based on recall data, but is weighted by 2006 prices.

To evaluate impacts on livelihoods and potential implications for conservation, we consider indicators of household engagement in forest and agricultural production. The most obvious candidate indicators are full income from each sector. We also consider indicators that were easier for households to report: time allocation to the forest, the area under cultivation and quantity produced of the staple agricultural crop, and agrochemicals used in production. These outcomes, as well as the covariates of participation and its benefits, are defined in Table 2, and their descriptive statistics are in Table 3.

VI. RESULTS

We first estimate a probit model to see how household characteristics and assets affect the propensity to participate, where participation is defined as a household having participated (and possibly still participating) in one of the four microenterprises. Table 4 shows the results for covariates other than the community dummy variables. A total of 22 observations were dropped because two communities had no participants, suggesting a systematic lack of interest in or opportunity for program participation in those communities.

TABLE 3
Descriptive Statistics for Key Variables

Variable	Mean	Std. Dev.	Min.	Max.
Total Income	5,495	5,350	− 42	51,912
Cash Income	3,796	4,080	− 374	24,865
Assets in 2006	1,033	1,060	0	5,610
Change in Assets 1997–2006	348	986	− 2,490	5,100
Forest Income	1,290	3,497	0	50,000
Imputed Nonforest Income	4,205	3,954	− 90	25,938
Time Collecting Forest Products	4.7	10.0	0	84
Manioc Production	846	1,282	0	10,150
Manioc Land	1.4	1.3	0	13
Agrochemical Expenditures	42	99	0	1,116
Pasture	4.8	15.7	0	200
Family Size	5.5	2.6	1	15
Born in Pará	0.9	0.3	0	1
Education	3.6	2.4	0	14
Age	46.2	14.7	16	85
Time Spent in Community	0.7	0.3	0	1
Distance to School	12.1	15.0	0	120

TABLE 4
Program Participation Probit Model

Variable	Marginal Effect[a]	S.E.[b]
Pasture	− 0.0127**	0.0066
Index of Assets in 1997	0.1009**	0.0565
Family Size	0.2021	0.1329
Family Size Squared	− 0.0167	0.0101
Females 10 to 55	− 0.025	0.2371
Females 10 to 55 Squared	0.0119	0.0522
Born in Pará	0.8467***	0.391
Education	0.0129	0.038
Age	0.1065***	0.0387
Age Squared	− 0.0011***	0.0004
Life Spent in Community	1.1784	1.2741
Life Spent in Community Squared	− 1.2474	1.0674
Distance to School	− 0.0117	0.0074
Children Out of National Forest	0.0197	0.0368

Note: $N = 333$; likelihood value = –170; pseudo $R^2 = .225$. Intercept and coefficients on community dummies are not reported.

,* Significant at the 5% and 1% confidence level, respectively.

[a] Calculated at sample mean.

[b] Robust standard errors.

The program attracted households with middle-aged heads (the peak on the quadratic is 47), more assets, and less pastureland. The probability that the average household participates is about 0.85 higher if the household head was born in the state of Pará, the state where the study area is located. This may reflect local skills and knowledge, specifically with regard to forest goods, or preferences, such as enjoyment of work in the forest. Our other two proxies for forest knowledge and skills—*Age* and *Life Spent in Community*—are both positively correlated with participation, though only *Age* is statistically significant and the coefficient on *Age Squared* is negative. As predicted, the marginal effect of pastures is negative, with each hectare lowering the probability of participation by the average household by about 0.013. The finding that assets in 1997 are positively associated with participation is consistent with the earlier finding that households with more pos-

TABLE 5
Normalized Differences in Covariates Using Entire Sample

	Participants ($N=123$)		Nonparticipants ($N=232$)		
Variable	Mean	S.D.	Mean	S.D.	Nor-Dif
Pasture	3.4	7.8	5.5	18.6	− 0.01
Index of Assets in 1997	0.41	0.15	− 0.22	0.10	0.28
Family Size	5.5	2.5	5.4	2.7	0.00
Born in Pará	1.0	0.2	0.9	0.3	0.01
Education	3.76	0.20	3.53	0.16	0.07
Age	46.6	1.2	45.9	1.0	0.04
Time Spent in Community	0.75	0.03	0.72	0.02	0.05
Distance to School	8.4	0.9	14.0	1.1	− 0.28
Children Out of National Forest	1.73	0.22	1.65	0.16	0.02

sessions take more trips to the forests and, therefore, learn about the forest (Pattanayak and Sills 2001), creating skills that influence selection into participation.

Next we estimate the effects of participation on three categories of outcomes: income, assets, and livelihood strategies. Using OLS to regress an outcome variable on a participation indicator and a vector of exogenous covariates is a traditional—and potentially robust—method to control for selection on observables, especially if there are few differences in covariates across treatment and control groups. One measure of the difference in covariates between groups is the normalized difference in mean (Nor-Dif) calculated as

$$\text{Nor-Dif} = \frac{\overline{X}_1 - \overline{X}_0}{\sqrt{S_{x1}^2 + S_{x0}^2}},$$

where S is the standard deviation. Table 5 shows how the covariates vary across participants and nonparticipants in terms of normalized differences.

Only the means of distance to school and the asset index in 1997 differ across participant and nonparticipant (control) groups by more than 0.1 standard deviations. The small differences in covariates across treatment and control groups suggest that the OLS estimate of the average treatment effect would be informative. We employ a base OLS model of the form

$$y_i = \mathbf{x}_i{}'\boldsymbol{\beta} + \delta p_i + \varepsilon_i, \qquad [6]$$

where y_i is the outcome, $\mathbf{x}_i$ is the $k \times 1$ vector of covariates (including all variables in Table 5 plus community dummy variables), and p_i is a binary variable that indicates if household i participated. The first set of estimates of equation [6] uses the entire sample ($n=355$). For a second set of estimates, we follow Angrist and Pischke (2009) and estimate [6] using observations whose propensity score (of participation) is between 0.10 and 0.90, which holds for 243 observations (see Appendix C). We estimate the propensity score using the vector of covariates listed in Table 5, plus community dummy variables, and quadratic and cubic terms for continuous variables.

Table 6 shows coefficient estimates for δ using different outcomes as the dependent variable y. OLS estimated with the entire sample suggests that program participation had a positive and statistically significant effect (at the 10% confidence level) on total, cash, and forest income but had no effect on assets, manioc production, or time spent collecting forest products. Using the trimmed sample increases the point estimates for the previously statistically significant coefficients, and the statistical significance as well as the size of the coefficient on the change in the monetary value of the household's assets between 1997 and 2006.

Matching can also be used to identify treatment and control observations that are then compared directly. This differs from traditional OLS in that it imposes common support, is less dependent on parametric assumptions, and weights the observations

TABLE 6
Ordinary Least Squares Estimates of Delta

	Entire Sample ($n=355$)		Trimmed Sample[a] ($n=243$)	
y	Delta	S.E.[b]	Delta	S.E.[b]
Income				
Total Income	1,171*	599	1,447**	631
Cash Income	841*	430	916**	442
Assets				
Index of Assets for 2006	0.09	0.21	0.2	0.22
Assets in 2006	88	139	144	142
Change in Assets 1997–2006	173	131	239*	132
Livelihoods				
Forest Income	741*	434	967**	479
Imputed Nonforest Income	430	407	479	421
Time Collecting Forest Products	1.66	1.38	0.9	1.34
Manioc Production	43	176	60	178
Manioc Land	0.04	0.19	0.01	0.2
Agrochemical Expenditures	− 2.45	11.29	− 6.85	12.98

[a] Sample excludes households whose propensity score was less than 0.10 or greater than 0.90.
[b] Robust standard errors.
*,** Significant at the 10% and 5% confidence level, respectively.

differently (Angrist and Pischke 2009). We match with replacement and identify a single match for each treatment unit, first starting with the full sample and second after trimming the 10% of participants with the lowest density of propensity scores among potential controls.[9] As shown in Appendix C, matching makes for better comparisons: the distribution of the propensity score for the participant group mirrors the distribution of the propensity score for the matched nonparticipant group much more closely than that of the total nonparticipant group, even though the nonparticipant group already appears similar to the participant group according to the normalized differences in Table 4. Finally, we also employ the matching with regression estimator defined in equation [5] to correct for any remaining differences in covariates after matching. For the bias-correcting vector $\mathbf{x}_i$ in equation [5] we use the same vector of covariates as in equation [6], which includes all variables in Table 5, plus community dummy variables.

[9] Matching with replacement reduces bias by finding better matches (Abadie and Imbens 2002). While matching each treatment unit with several control units can reduce variance in the point estimate, a single match is generally recommended (Imbens and Wooldridge 2008).

In Table 7, we present three sets of results based on nearest neighbor matching. The first set corresponds to nearest neighbor matching on the propensity score with replacement and a single match. The second set comes from the same estimator but drops the 10% of treatment observations whose propensity score places them in the range of the propensity score where there are the fewest control observations. The third set uses all treatment observations and matches on the same covariates as in the probit participation model and the OLS regressions; however, the treatment effect estimate is adjusted by multiplying the parameter vector $\hat{\boldsymbol{\beta}}_0$ in equation [5] with any differences in covariate values remaining between matched pairs.

Similar to the OLS estimates, the results in Table 7 suggest that rigorous control of differences in observables—including imposition of common support—matters when estimating treatment effects. The first set of results yields only statistically significant effects of participation on forest income and time collecting forest products. Dropping the 10% of treatment observations where the propensity score density of the control observations is lowest strengthens the previous results and also yields a statistically significant effect on total income and a household's change in

TABLE 7
Average Treatment Effect on the Treated with Nearest Neighbor Matching

	Matching on Propensity Score[a]		Matching on Propensity Score, Trimmed Sample[b]		Bias-Corrected Estimator[c]	
Outcome	Coef.	S.E.[d]	Coef.	S.E.[d]	Coef.	S.E.[e]
Income						
Total Income	1,337	824	1,527*	850	1,488**	577
Cash Income	971	681	1,081	681	1,013**	467
Assets						
Index of Assets for 2006	− 0.07	0.41	− 0.10	0.41	− 0.01	0.18
Assets in 2006	90	247	156	234	264**	133
Change in Assets 1997–2006	267	202	360*	198	367**	143
Livelihoods						
Forest Income	946*	509	974*	550	1,068**	333
Imputed Nonforest Income	392	692	552	692	420	462
Time Collecting Forest Products	4.3***	1.53	3.4**	1.5	3.7***	1.42
Manioc Production	128	251	95	258	73	202
Manioc Land	0.14	0.26	0.1	0.3	-0.17	0.17
Agrochemical Expenditures	12	17	19	16	11	12

[a] Nearest neighbor matching on the propensity score using 1 match.
[b] Same estimator as previous column but drops the 10% of treatment observations where the propensity score density of the control observations is the lowest.
[c] This is the bias-corrected estimator defined in equation [5]. Matching is based on differences in covariates where the covariate vector is weighted by the inverse variance matrix.
[d] Homoskedastic standard errors.
[e] Standard errors allow for heteroskedasticity. For details on the procedure to calculate heteroskedastic standard errors for the bias-correcting matching estimator, see Abadie et al. (2004). Imposing homoskedasticity on the bias-corrected matching estimates does not change the statistical significance of results except for *Assets in 2006*, which becomes statistically insignificant.
*,**,*** Significant at the 10%, 5%, and 1% confidence level, respectively.

assets from 1997 to 2006. The bias-corrected estimator suggests that participation also increases cash income and value of assets in 2006. The statistical significance of all of the estimated effects is strongest using the bias-correction method.

As might be expected, the estimated impacts on forest income and total income are most robust across OLS and different matching techniques, including propensity score matching with radius and kernel techniques and with more neighbors (see Appendix D). The results for labor time allocated to the forest and cash income are nearly as consistent. Finally, the increase in asset value is found statistically significant only twice, but these significant results are obtained with the two "doubly robust" methods (Ho et al. 2007) that combine matching (to trim or define the sample) and OLS regression (to estimate the coefficient or adjust the impact estimate).

Overall, these "doubly robust" estimates in the last columns of Tables 6 and 7 are similar, despite employing significantly different weights, as shown in Appendix C. We focus our discussion on the bias-corrected matching estimates in Table 7. These suggest that through program participation, households earned on average an extra R$1,488 in total annual income, a 27% increase over the sample average total income of R$5,495. Likewise, participants increased cash income by R$1,013 or a 27% increase over the sample average cash income of R$3,796.

In the period 1997 to 2006, participants accumulated assets faster than nonparticipants, with an average increase more than double the sample average change in assets (R$348). The higher cash income coupled with the greater increase in the value of assets suggests that participants used some of their extra cash to accumulate assets. According to the bias-corrected estimator, participation also positively affected the level of assets in 2006, when weighted by prices, but not by the first principal component.

We test the sensitivity of the results from the bias-corrected estimator to selection on unobservables by performing a Rosenbaum bounds analysis on *Total Income*, *Cash In-*

come, *Change in Assets*, *Forest Income*, and *Time Collecting Forest Products*. All estimates are robust to some degree of selection on unobservables. See Appendix E for a discussion of the method and its results.

Our conceptual model suggests that participation would reduce nonprogram forest use. Empirically, households include most program-related forest use in their estimates of forest income and time allocation. Thus, the expectation is that program participation has a positive but potentially quite small effect on overall engagement in forest production. Participation does increase forest income, substantially and significantly. However, we find a statistically strong (p-value of 0.008) but economically small effect on the hours that household heads spent collecting forest products per week. The point estimate suggests that participation increases time spent in the forest only by about a half a day each month.[10]

One possible reason for the small net effect of participation on collecting forest products is a substitution effect involving risk. Pattanayak and Sills (2001) show that many households in the study area collect forest goods in response to shocks. The households that tended to collect forest products in their sample (i.e., those with few cattle, many possessions, and an older household head) are also the type of households who tended to participate in the program. Because program participation may help households ensure against shocks by providing a relatively stable source of cash income, it may reduce their need to use the forest as insurance and by extension their forest use.

Our conceptual model also implied that program participation would only affect agriculture and land clearing for households for whom the labor market constraint was binding. Ideally, we would like to identify constrained and unconstrained households and estimate hetereogenous treatment effects, but identifying constrained households without information on preprogram labor market participation would require strong assumptions. The average treatment effect, nonetheless, remains informative, as it reflects the net effect given the sample's composition of constrained and unconstrained households.

We find no evidence that program participation affected total nonforest income or production of the primary crop, manioc, as measured by output or area planted. Nor did participation affect household use of agrochemicals. If participants withdrew labor (or capital) from agriculture or reduced intensity of cropping, manioc production should fall. Furthermore, if expansion of agricultural lands is common, we might expect program participation to lessen demand for new agricultural lands, and by extension deforestation, by providing an economic alternative. If nonparticipants expanded their lands more than participants, we should see a negative relationship between participation and total output, as well as participation and total area. Instead, we find that participation does not affect production.

In sum, participation in the program increased forest income and slightly increased labor allocation to the forest, but had no perceptible impact on either total nonforest income or production of the staple crop. These findings are consistent with our conceptual framework, in that a small reallocation of household labor from agriculture to forest collection may not have a statistically significant effect on agricultural production, given other sources of variation. There are several possible explanations. The conceptual model suggests that participating households would hire workers to maintain agricultural production while shifting household labor to the program. To assess this possibility, we estimate equation [5] with outcomes (1) hiring out (days household spent working as day laborer in past year) and (2) hiring in (days of wage labor employed by the household). We find no significant impact on hiring out, and that par-

[10] It is possible that we underestimate hours spent in the forest. To limit the length of the interview and reduce recall bias, the survey asked about hours spent by the male and female household head in the forest in the previous month. Survey responses suggest that time allocated to collection of forest products was not systematically higher or lower than average in the month before the survey. We do not have information on how time allocation of other household members may have changed in response to participation. Time allocation by household heads, while a good indicator of household livelihood strategies, may not be precise enough to identify the effect of program participation on total household time in the forest.

ticipation actually reduced days of wage labor hired in by 8.8 (standard error of 3.8), thus ruling out this explanation. A second possibility is that households reallocate leisure time to participation in the program. This would happen if the substitution effect (between work and leisure) dominates the income effect from participation. We do not have the data necessary to evaluate this possibility. If this is the case, we may have overestimated the welfare benefits of the program, but we would not have missed any major shift in livelihoods.

The negative effect of participation on hiring in labor suggests that participant households are desisting from some activities. In the study area, households with modest amounts of land often purchase labor for specific activities like capital improvements. For example, the cropping system used in the study area involves occasionally rotating production to land left fallow for several years. Because of rapid vegetation growth, fallow land must be cleared to allow cultivation. Participating households may prefer to buy assets with their additional income instead of pursuing agricultural capital projects like clearing fallow land, planting fruit trees, or building livestock shelters. This suggests that program participation could affect manioc production in the longer term, especially if households become more confident that the economic opportunities generated by the program will persist.

VII. TOWARD A FULLER UNDERSTANDING OF ICDPS AND HOUSEHOLDS

Our model and empirics highlight how comparative and competitive advantages in household production of program "goods" influence participation. Results from the participation model suggest that there is no consistent correlation, positive or negative, between participation and household capital (including pastureland and durable assets). Instead, participation is related to households' portfolios of physical and human assets, which determine the opportunity costs and expected gains from participation. We find that wealth type, most likely reflecting a livelihood strategy, affects who participates. The program attracted middle-aged households with more durable assets, less pasture, and whose head was born in the region.

As with most ICDPs, the microenterprise program in the Tapajós National Forest had multiple welfare and conservation goals. Thus, we assess impacts on multiple outcome indicators. We conclude that participation increases both total and cash income by around 27%, which has ambiguous consequences for the Tapajós National Forest, depending on what households do with the additional income. There is some evidence, for example, that payments for ecosystem services provide liquidity for deforestation of unenrolled forestland (Alix-Garcia, Shapiro, and Sims 2010). In the Tapajós, we could ask whether households obtain better health care and send more family members to school, or whether they buy shotguns and chainsaws. This is likely to depend on which household members control the extra income (c.f., Pitt and Khandker 1998; Anderson, Locker, and Nugent 2002), suggesting a rich area for future research.

A common concern about integrating traditional households into the cash economy is that it will cause local skills and forest knowledge to atrophy. Our results suggest that forest-based microenterprises may have little effect on total time spent in the forest. However, collecting just one product for the market does not guarantee that a household will retain its knowledge about a diversity of subsistence goods. Thus, the concern still remains that the program may leave households with less ability to weather shocks by supplementing consumption with diverse forest goods.

The effect on income is not the only "development" benefit from the microenterprises. The most substantial benefit to households may be diversification of cash income sources, which was also an explicit program goal. Instead of replacing farming or labor market participation, the program adds a revenue source to household portfolios. In addition, it is a source whose returns should have little correlation with weather-related shocks to agricultural production.

One concern with a program that succeeds in substantially raising participant income is that it may have a boomerang effect, with the new economic opportunities inducing immigration. Since the projects began to generate revenue for participants around 2000 in the earliest cases, one would expect program-induced immigration to have increased since then. Our survey asked when the household head arrived in the community. Of the 355 households surveyed, 7% of household heads arrived in the community between 5 and 10 years ago, and exactly the same number arrived in the last 5 years. We also do not find evidence that the program altered overall population trends in the communities. Analysis of trends in the study communities reveals that the number of people per household changed little from 1997 to 2006 (Bauch and Sills 2007).

ICDPs seek to further conservation by promoting activities that increase demand for intact ecosystems, distract people from environmentally harmful activities, or compensate and encourage acceptance of new restrictions on resource use. The ProManejo ICDP involves all of these channels. For example, the ecotourism microenterprise requires relatively intact ecosystems to thrive; the artisanal wood activity, which uses fallen wood for raw material, better fits the conservation through distraction approach; and the program's overall investment in microenterprises could be viewed as compensation for restrictions on land clearing.

To make progress through any of these channels, an ICDP must increase welfare (often by increasing income), in order to generate derived demand for healthy ecosystems, distract people from deforesting, or offer something in exchange for accepting new restrictions on resource use. In our case study, the microenterprise program of the ICDP did increase income and asset accumulation, but the next link to conservation is ambiguous. While other forestwide components of ProManejo may have been more effective, there is no clear evidence that the indirect conservation strategy of forest-based microenterprises shifted household livelihoods away from their traditional focus on agriculture.

APPENDIX A: MODEL DERIVATIONS

The Lagrangian and associated first-order conditions for an interior solution are

$$\mathcal{L} = U\left(\frac{p_a A(L_a, T + T_c) + w(L - L_f - L_a - \rho T_c)}{p_m}, F^i(L_f)\right) - \lambda_1(M_l + L - L_f - L_a - \rho T_c),$$

$$L_a: U_{cm}\left(\frac{p_a A_{la} - w}{p_m}\right) = -\lambda_1, \quad [A1]$$

$$L_f: U_{cm}\left(\frac{-w}{p_m}\right) + U_{cf}F^i_{lf} = -\lambda_1, \quad [A2]$$

$$T_c: U_{cm}\left(\frac{p_a A_T - w\rho}{p_m}\right) = -\rho\lambda_1. \quad [A3]$$

Combining [A1] and [A2] shows that an optimizing household will allocate agricultural and forest labor so that the ratio of marginal utilities equals a price-scaled ratio of marginal labor productivity in agriculture and forest-good production:

$$\frac{p_a A_{la}}{p_m F^i_{lf}} = \frac{U_{cf}}{U_{cm}}. \quad [A4]$$

Likewise, under a binding labor market constraint, the amount of land cleared will be a function of the household shadow wage, which is equivalent to the value of loosening the labor market constraint, λ_1. Because the household is constrained from working more in the labor market, the market wage ceases to be the relevant opportunity cost of labor when deciding how much land to clear. Rearranging condition [A1] we see that $p_a A_{la} = w - \frac{\lambda_1 p_m}{U_{cm}}$, which means that under a binding labor market constraint, the wage will exceed the marginal value product of agricultural labor. Because the household's own shadow value of labor is less than the market wage, it faces a lower cost of clearing land compared to a nonconstrained household.

If the labor market constraint does not bind ($\lambda_1 = 0$), [A1] and [A3] reduce to

$$p_a A_{la} = w \quad [A5]$$

and

$$p_a A_T = \rho w. \quad [A6]$$

Equation [A5] says that the marginal value product of agricultural labor equals the wage. Similarly, [A6] requires the value of an additional unit of land to equal the cost of clearing a hectare of land (ρw).

APPENDIX B: COMPONENTS OF ASSET INDICES

TABLE B1
Assets

Asset Name
Included in price weighted index
Gas stove
Radio
Television
Sewing machine
Canoe
Bicycle
Chain saw
Motor boat
Fishing net
Additional assets used in principal component index for 1997
Electricity
Gun
Hand-held net
Additional assets used in principal component index for 2006
Clothes washing machine
Bed
Refrigerator
Clock
Satellite antenna dish
Car/truck
Motorcycle
Oxen cart
Gun
Hand-held net
Manioc processing equipment
Radio for communication

APPENDIX C: PROPENSITY SCORE HISTOGRAMS

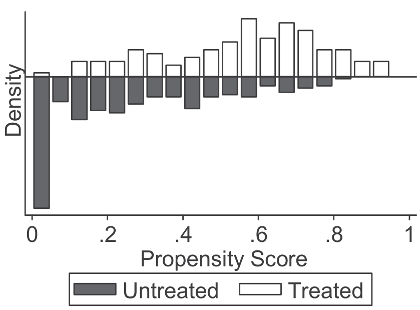

FIGURE C1
Unmatched Sample

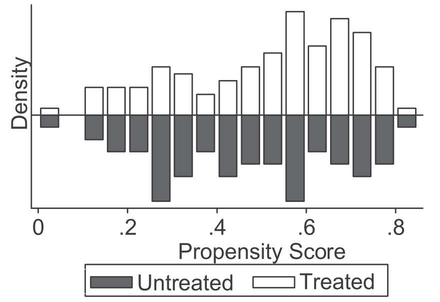

FIGURE C2
Sample Trimmed of Top and Bottom 10% of Propensity Scores

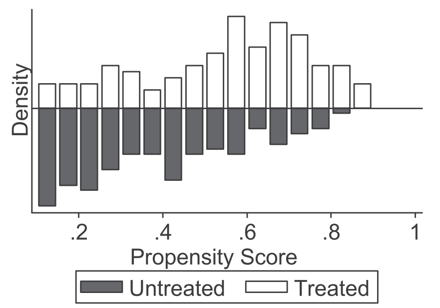

FIGURE C3
Matched Sample (Nearest Neighbor)

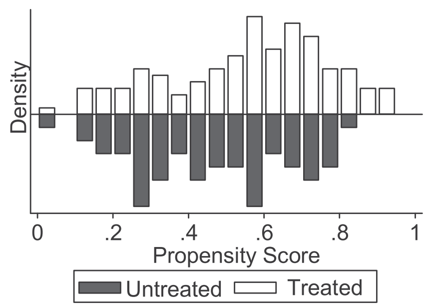

FIGURE C4
Matched Sample Trimmed of 10% of Treatment Observations with Least Number of Control Observations with a Similar Propensity Score

APPENDIX D: ALTERNATIVE MATCHING METHODS AND THE PROPENSITY SCORE

TABLE D1

Results from Alternative Matching Methods Using the Propensity Score

Outcome	Radius[a]		Kernel[b]		Nearest Neighbor (3)[c]	
	Coef.	S.E.[d]	Coef.	S.E.	Coef.	S.E.
Income						
Total Income	1,353*	761	1,501*	839	1,609**	721
Cash Income	1,041*	576	1,133*	636	1,210**	575
Assets						
Index of Assets for 2006	0.1	0.24	−0.10	0.26	−0.04	0.29
Assets in 2006	131	150	63	165	62	184
Change in Assets 1997–2006	157	141	236	152	201	163
Livelihoods						
Forest Income	976*	515	1,009*	550	1,017**	492
Imputed Nonforest Income	377	552	492	622	592	556
Time Collecting Forest Products	2.9**	1.4	3.05*	1.6	3.4**	1.7
Manioc Production	−16	183	13	203	76	234
Manioc Land	0.06	0.19	0.03	0.21	0.08	0.24
Agrochemical Expenditures	−4	14	0	16	1	13

[a] Radius matching with a caliper of 0.2.
[b] Kernel matching using an Epanechnikov kernel.
[c] Nearest neighbor matching using three closest neighbors.
[d] Homoskedastic standard errors.
*,** Significant at the 10% and 5% confidence level, respectively.

APPENDIX E: ROSENBAUM BOUNDS ANALYSIS

The Rosenbaum bounds analysis tests how sensitive results are to selection on unobservables. Gamma (γ) is a measure of selection on observables where $\gamma = 1$ indicates that unobservable variables have no effect on the propensity to participate apart from included observable variables. If $\gamma = 1.5$, for example, then unobservable variables make participants 50% more likely to participate than nonparticipants. For the bias-corrected matching estimates in Table 7, we use the "rbounds" command in Stata version 10.0 (StataCorp 2007) to estimate the value of γ at which a previously statistically significant result loses significance at the 10% level. The results are robust to a degree of selection on unobservables, though how much varies. The effect on *Cash Income* is the most sensitive, becoming statistically indistinguishable from zero if unobservables make participant households 20% more likely to participate than nonparticipants. The effect for *Time Collecting Forest Products* is the most robust, requiring $\gamma = 1.7$ for the estimated effect to be statistically insignificant at the 10% level.

TABLE E1

Sensitivity Analysis for Select Estimates from the Bias-Corrected Matching Estimates in Table 7

Outcome	Upper Bound of γ
Total Income	1.4
Cash Income	1.2
Assets in 2006	1.3
Change in Assets 1997–2006	1.3
Forest Income	1.5
Time Collecting Forest Products	1.7

Acknowledgments

We are grateful for funding from the U.S. Department of Agriculture Forest Service International Programs, the Amazon Institute for People and the Environment (IMAZON), and the University of Wisconsin–Madison LACIS Tinker Nave Field Research Grant program. We thank (without implicating) Brad Barham, Jan Borner, Jean-Paul Chavas, Jeremy Foltz, Martin Petrick, Mitch Renkow, Laura Schechter, Chris Tabor, Charles Towe, and two anonymous referees for comments. We received helpful feedback from seminar participants at AAEA, EAERE, NCSU CenREP, IMAZON, and MWIEDC. This work was completed while Weber was a graduate student in the Department of Agricultural and Applied Economics at the University of Wisconsin–Madison. The views expressed here are the authors' and should not be attributed to the Economic Research Service or the U.S. Department of Agriculture.

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