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Decentralised forest management for reducing deforestation and poverty in low- and middle-income countries

A systematic review

March 2015

Systematic Review 16

Environment

3ie International Initiative for Impact Evaluation

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Decentralised forest management for reducing deforestation and poverty in low- and middle- income countries: a systematic review, was submitted in partial fulfilment of the requirements of grant SR4.1173 issued under Systematic Review Window 4. This review is available on the 3ie website. 3ie is publishing this report as received from the authors; it has been formatted to 3ie style. This review has also been published in the Campbell Collaboration Library and is available here.

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Funding for this systematic review was provided by the Norwegian Agency for Development Cooperation.

Suggested citation: Samii, C, Lisiecki, M, Kulkarni, P, Paler, L and Chavis, L, 2015, *Decentralised forest management for reducing deforestation and poverty in low- and middle- income countries: a systematic review*, *3ie Systematic Review 16*. London: International Initiative for Impact Evaluation (3ie).

Decentralised forest management for reducing deforestation and poverty in low- and middle-income countries: a systematic review

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March 2015

Abstract

We conducted a systematic review of studies on the impact of decentralized forest management (DFM) on deforestation and poverty in developing countries. The review is motivated by debates over whether the pursuits of conservation and poverty reduction in developing countries tend to conflict or whether they might be complementary. A search for rigorous evaluation studies identified eleven quantitative and nine associated qualitative evaluation studies on DFM. The methodological rigor of these studies varied widely, meaning that the evidence base for the impact of DFM policies is limited in both quantity and quality. Given the evidence available, we find little reason for optimism about the potential for current DFM approaches to achieve both conservation and poverty reduction benefits jointly. We call for the production of much better impact studies, employing randomized field experiments when possible, to assess whether the apparent incompatibility of conservation and poverty reduction might be overcome through programming innovations.

Summary

Background

Natural forest preservation in the tropics, and thus in developing countries, must be an element of any effective effort to manage climate change. Forests serve as natural carbon sinks, which help to mitigate the effect of other carbon emissions. However, forest cover is being reduced and it is estimated that deforestation is responsible for 10-17 per cent of global carbon emissions. Since 2007, governments have coordinated conservation efforts under the Reducing Emissions through Deforestation and Forest Degradation (REDD+) initiative, which has led to the implementation of various programs designed to reduce the amount of forested land converted to other purposes. Decentralized forest management (DFM) is one approach to forest management which has been widely implemented to reduce deforestation in developing countries. DFM programs relocate decision-making authority on forest use in the direction of forest communities, rather than central government actors.

While the primary goal of reducing deforestation is clear, the policy and academic literature debates the extent to which DFM programs in developing countries should incorporate goals of poverty reduction. Some argue that the targeting of poverty goals will undermine conservation effectiveness (e.g. because behavioral change among poorer households does not have as much potential to promote conservation as that of wealthier households or commercial entities). Others argue that targeting benefits toward the poor would contribute to conservation effectiveness by either promoting sustainable livelihoods or helping to legitimize conservation programming.

To move the debate around DFM beyond theoretical discussions and into better-informed, evidence-based discussion we examine the evidence on the effects of DFM programs on deforestation and welfare outcomes in low and middle income countries (LMICs), aiming also to assess whether these goals are at odds with each other.

Objectives

The first objective of this review is to assess the evidence on the effects of DFM programs on the conservation and poverty outcomes in LMICs. A second objective is to assess the extent to which these programs' effects on poverty in turn affect whether conservation benefits are realized. The third objective is to evaluate how institutional and social conditions (namely, inequality, institutional capacity, corruption, and democratic accountability) moderate the effects of DFM programs.

Selection criteria

The review includes studies of DFM programs that assess effects on (i) deforestation outcomes in forest areas in developing countries or (ii) poverty conditions of populations residing in communities that are proximate to natural growth forest areas in developing countries. We included studies using a range of measures for both deforestation (on-the-ground point samples, samples created from satellite imagery) and welfare (consumption, income, or income potential).

For a program to be considered a DFM program, de jure responsibility for managing natural forest resources must pass from centralized to local authorities. This responsibility must grant local authorities the right to grant concessions or establish use restrictions. We were

flexible as to the precise level of administration to which responsibilities are passed as well as whether or not the decentralization also involves granting local authorities the right to sell or transfer titles to forested lands.

For the quantitative synthesis we included (a) randomized studies or (b) quasi-experimental studies that employ strategies for causal identification with clearly delineated treated and control areas and use some method for removing biases due to non-random assignment of the intervention. Qualitative data is used in the synthesis to provide descriptions and context for interventions that are included in the quantitative synthesis. Such data were extracted from the quantitative studies themselves as well as qualitative studies that cover the same programs or settings as the quantitative studies.

Search strategy

To identify the articles included in this review, we searched a variety of databases using key words related to DFM programs. The set of databases and lists of keywords were assembled based on consultation with a Campbell Collaboration information retrieval specialist. We also carried out hand searches of key journals in relevant fields, using publisher search engines and references cited in papers accepted for review as well as references in review papers or thematically relevant papers identified during the search.

Data collection and analysis

We collected data on the study characteristics, findings, and moderators of all included studies. Risk of bias was assessed based on the guidance of the IDCG Risk of Bias Tool (version March 2012). We extracted qualitative information from both the included quantitative studies as well as qualitative studies that covered the same types of programs and contexts as our quantitative studies. We used such qualitative data to establish that conditions recorded in quantitative data are being interpreted correctly and to provide descriptions and context for interventions that are included in the quantitative synthesis. For effects on forest cover, whenever possible, we standardize them to annual forest cover change rates. For effects on material welfare and poverty outcomes we use percentage change over estimated average counterfactual outcome (e.g., for income effects, per cent change in income relative to the average income of the control group). For each hypothesis, we synthesized estimates using meta-analysis when the following conditions were met: (i) more than two studies meeting the quantitative inclusion criteria; (ii) effect sizes for common outcome constructs; and (iii) effects measured against similar comparators.

Results

Our database search returned 1272 articles on DFM programs. After eliminating articles that were not relevant to our hypotheses or conducted with appropriate methodological rigor, we were left with 12 studies of DFM programs. Of these, eight DFM studies were quantitative impact evaluations. The studies cover eight programs in seven countries (Bolivia, Ethiopia, India, Kenya, Malawi, Nepal, and Uganda).

The evidence base is limited both in terms of the number of eligible studies and the methodological quality of included studies. None of the studies are based on randomized experiments, and so the potential for hidden selection or confounding biases is the most pertinent problem. Few of the studies create comparison groups that allow them to address spillover and leakage of effects from program areas to non-program areas. Finally, none of

the studies investigated forest conservation and welfare effects jointly, which made it difficult to address our question of how these two goals relate.

Effects on Deforestation Outcomes

The included studies that report the effects of interventions on forest conservation outcomes included research on programs in Bolivia, India, Kenya, and Nepal, although only the studies from India and Bolivia report effects on forest cover change . These studies used a wide range of outcome variables (e.g., different forest types), so it is difficult to synthesize an average effect of these programs. Across the studies the reported effects of DFM programs on the annual forest cover change rate range from 0.026 percentage points (s.e.=0.060, 95% CI: [-0.09, 0.14]) for a study examining DFM and community forest use in India to 0.80 percentage points (s.e.=0.20, 95% CI: [0.41, 1.19]) for a study examining DFM-based administration of protected forests in Bolivia.

Effects on Human Welfare Outcomes

Three studies assess the effects of DFM on welfare or poverty outcomes, evaluating programs in Ethiopia, Malawi, and Uganda. They differed in the nature of the comparisons that they made, but all found that DFM did lead to a boost in either a households' forest or household income on average. Effects ranged from a 35 per cent increase in per capita consumption expenditure in Ethiopia (s.e.=9.44, 95% CI: [16.5, 53.5]) to a 2 per cent gain in Uganda (s.e.=2.36, 95% CI: [-2.63, 6.63]). Nevertheless, these average effects are not so helpful in assessing effects on poverty if they do not characterize consequences for poor households specifically. Indeed, the study of DFM programs in Uganda suggested that poor households in areas neighbouring DFM programs might have been harmed: among members of the lowest income quartile, the effect of devolution to local public institutions on income per capita was a 6 per cent reduction, although this was not statistically significant (s.e.=8.43, 95% CI: [-22.52, 10.52]), while devolution to local parastatals yielded a 10 per cent loss of income (s.e.=5.12, 95% CI: [-20.04, 0.04]).

The Role of Institutional and Social Conditions

We aimed to address a number of hypotheses regarding the influence of institutional and social conditions (inequality, institutional capacity, corruption, and democratic accountability) on the effects of DFM programs. However, due to limitations of the evidence base we were unable to assess these hypotheses. We did however extract qualitative data from included studies and associated qualitative studies that provide some insights into the role of institutional and social conditions in the context of PES programs

These studies highlighted issues of institutional capacity, describing situations where DFM programs did not have the ability to carry out their mandates. We also found qualitative support for the idea that democratically accountable DFM institutions may result in larger conservation effects. However, this hypothesis does rest on the possibly erroneous assumption that all forest edge community members favour conservation, an assumption that was challenged in qualitative accounts from Mexico.

Authors' conclusions

Limitations in the evidence base preclude definitive hypothesis tests, but we do find that DFM reduce deforestation rates. In terms of program effects on human welfare and poverty

outcomes, the evidence is very limited and we cannot conclude that the evidence indicates non-negative effects.

Our review aimed to assess the extent to which conservation and poverty reduction goals conflict, and the scope for “win-win” strategies that generate both significant environmental and human welfare benefits. Based on the evidence available, we do not find that an evidence-based case can be made for conservation and poverty-reduction goals being complementary in DFM programming.

Our final conclusion re-emphasizes the poor state of the evidence base for conservation programming. Much advanced scientific effort and extensive investment has gone into measuring forest conditions around the world. Relative to that, efforts to assess the effects of DFM programs on deforestation and poverty is limited and methodologically weak. Conservations researchers should look to recent work in development economics for guidance on executing field experiments that might provide more credible evidence (Banerjee and Duflo, 2011; Casey et al., 2012; Karlan and Appel, 2012).

Contents

List of figures and tables

Appendices figure

1. Background

1.1. Description of the problem

Deforestation has been estimated to be responsible for 10 to 17 per cent of carbon emissions (Intergovernmental Panel on Climate Change, 2007) and reducing the rate of deforestation is considered a key component of climate change mitigation efforts. It has been estimated that the net impact of cutting deforestation rates by 50 per cent by 2050 and keeping this level until 2100 would be similar to cutting annual fossil fuel emissions for almost 6 years (Gullison et al., 2007). Moreover, in an essay that helped to inspire the REDD initiative, Santilli et al. (2005) noted the importance of tropical natural forest protection for climate change, indicating that "current annual rates of tropical deforestation from Brazil and Indonesia alone would equal four-fifths of the emissions reductions gained by implementing the Kyoto Protocol in its first commitment period [i.e., 2008-2012]" (p. 267).

Thus, since 2007, governments have coordinated efforts to reduce deforestation through the Reducing Emissions from Deforestation and Forest Degradation (REDD) initiative, which consists of inter-governmental framework agreements to facilitate the protection of natural forests, with particular emphasis on natural forests in the tropics and, therefore, developing countries (that is, countries classified by the World Bank as low- or middle-income). The goal of REDD (now REDD+) is both to reduce carbon emissions resulting directly from deforestation and to preserve natural forests as carbon sinks so as to mitigate the effect of other carbon emissions on climate change (Intergovernmental Panel on Climate Change, 2007; Harris et al. 2012).

This has led to the implementation of various programs to reduce deforestation, including through decentralized forest management (DFM) programs. DFM find wide application around the world as part of government strategies to manage forest loss and climate change. Decentralized forest management (DFM) programs relocate decision-making authority on forest use in the direction of forest communities, although the extent to which such reforms empower forest community members depends on the institutional context (Tacconi, 2007). DFM exist alongside other types of programs including "community-based forest management", "protected areas" (that is, parks and reserves) and payment for environmental services (PES) as core components of government and privately led forest management efforts around the world (Angelsen, 2009).

Governments have applied DFM strategies domestically for decades to manage forests and prevent irredeemable loss of valuable endemic forest resources. Fundamental issues in policy debates over conservation strategies in developing countries are the extent to which conservation and poverty reduction goals conflict, how different conservation strategies fare in terms of such trade-offs, and the scope for "win-win" strategies that generate both significant conservation and poverty reduction benefits (Muradian et al., 2013; Sunderlin et al., 2005; Wunder, 2001, 2013). This review is organized around these issues.

Two core questions arise. First, how might these potential benefits from natural forest conservation in the tropics be realized? Second, how do different approaches to natural forest conservation relate to the pursuit of poverty reduction goals? While tropical forests are appealing as targets for conservation because of their high carbon storage density and lower (in absolute terms) opportunity costs of conservation, they are located primarily in areas of low- and middle-income countries where poverty is a central concern (Deveny et al., 2009;

Kremen et al., 2000; Sunderlin et al., 2005; Van Kooten and Sohngen, 2007). It is therefore crucial to understand whether conservation strategies require trading off on poverty reduction goals or whether there are strategies that allow for synergy in the pursuit of conservation and poverty reduction goals jointly. In this review, we address these questions with respect to DFM programs.

1.2. Description of the intervention

1.2.1. Description of DFM

Our definition of DFM follows Irawan and Tacconi (2009), defining it as the de jure transfer of control of state-managed forest resources from central government to local authorities. Forest management refers to decision-making over designating existing state-owned natural forest areas as protected versus available for conversion, timber harvesting, or other types of forest resource exploitation. The focus on de jure, rather than de facto, conditions is justified by the desire to study policy instruments that can be manipulated.

DFM resembles "community-based forest management" (CBFM), which involves statutory recognition of local communities' rights to manage forests (Agrawal and Angelsen, 2009). Whether DFM should be considered as distinct from CBFM depends on the goals of the analysis. Lawry (2012) conceptualizes a spectrum of strategies of "forest governance devolution," ranging from strategies involving no new property rights to forest communities (e.g., sharing royalty revenue between central and local administrations) to statutory recognition of forest communities' customary tenure rights over forest lands. Lawry proposes that forest governance strategies should be evaluated in terms of variation along this spectrum. While we agree that this approach is appealing conceptually, we also agree with Tacconi (2007) that DFM, in terms of deconcentration (or devolution) of formal government power, constitutes a distinct strategy relevant in many parts of the developing world. The distinction, in essence, is that DFM must involve the deconcentration of formal government authority.

In contrast, CBFM is often used in a manner that includes programs to develop community level capacities or provide resources to communities with no change to governing institutions. DFM and CBFM may coincide if local authorities that receive authorization to manage forests happen to be forest community leaders. Such would be the case when forest management is devolved to local authorities in autonomous indigenous areas. CBFM strategies have already been subject to systematic review (Bowler et al., 2010). What makes the most sense at this point is to assess the evidence base on DFM systematically in anticipation of a synthesis that then compares results across strategies and highlights important points of intersection.

A "DFM program", then, refers to significant de jure transfer of management responsibilities for natural forests, including authority to grant concessions or establish use restrictions, from centralized to local authorities. DFM programs have clear start dates that allow in principle for evaluation of their impacts. DFM programs are also in effect all over the world, including Bolivia, Ethiopia, Guatemala, Honduras, India, Kenya, Mali, Nepal, the Philippines, and Uganda. DFM "inputs" include the regulations that mandate deconcentration of forest management authority, human and material resources used to facilitate such deconcentration, and the forested lands to be managed. "Outputs" include the creation of local administrative capacities through hiring or reassigning staff into new roles, elaboration

of local regulations, and extent of forest lands covered by the new institutions. Again, the desired outcomes are improved conservation and, possibly, welfare.

1.3. How the intervention might work

1.3.1. Main hypotheses

Figure 1 is a schematic representation of the theory of change that we assess in this review. We embed the causal relationships between DFM and poverty/deforestation in Ostrom (2007)'s generic analytic framework for conservation dynamics. The framework defines the context in terms of the resource system, governance system, resource units, and resource users. In this review, the governance systems and resource users are the key areas of contextual variation that may moderate PES effects. Resource system (forest systems in developing countries) and resource units (forested land) are assumed to be of secondary concern once we condition on governance systems and resource users, with the latter understood as being potential agents of deforestation[1]. The causal arrows in the diagram do not characterize all conceivable causal relationships, just the ones that we seek to test. We have drawn a causal arrow that flows from poverty to deforestation, and not the other way around. This does not mean that we assume no effects of deforestation on poverty. It is meant to clarify the particular mediating relationship that interests us in this review.

A crucial question for conservation programs in developing countries is whether there might be synergies between the pursuit of conservation goals and poverty reduction goals or not. Pagiola et al. (2005) argues that coupling poverty goals with environmental protection goals in conservation programming may be inefficient for reaching either type of goal, and that in many instances the two objectives are orthogonal to each other, if not in conflict. While poorer members of forest edge communities stand to gain the most from poverty alleviation programming, they may not constitute the greatest deforestation threat. Such individuals may have relatively little means or incentive to engage in deforestation relative to large-scale farmers or logging interests. If so, making poverty alleviation in forest edge communities a priority may imply inefficient targeting of resources if the goal is the biggest conservation payoff (Wunder, 2005, p.12-14).

For DFM, while some research demonstrates that local administration may be better for administering policies affecting welfare of the poor (Bardhan and Mookherjee, 2005), it is not clear that this necessarily puts it at a comparative advantage in forest management relative to centralized management. Of course, there is a moral reason to couple poverty relief with conservation, although it does not presume the possibility of synergy: such a coupling would be imperative if conservation disrupts livelihoods of forest community members by limiting their ability to exploit resources for productive purposes, whether by themselves or as hired labor (Agrawal and Benson, 2011; Angelsen and Wunder, 2003; Chomitz, 2007, Ch. 3; Edwards et al., 2011; Porras, 2010; Wunder, 2005).

[1] This is mostly a semantic point: clearly aspects of the resource system will influence both deforestation rates and poverty conditions and will also affect the likely impact of DFM programs. For example, whether there are highly valuable timbers or whether there are mining opportunities. However, resource systems factors such as these operate through resource users' opportunity costs. Our argument is that *conditional* on opportunity costs, timber values and mining opportunities per se are of secondary importance. On the basis of economic theory, opportunity costs provide a sufficient statistic for incorporating such resource system factors into the analysis.

Arguments for synergy include those based on a sustainable livelihoods and political logic. With respect to sustainable livelihoods, the classic study by Vandermeer and Perfecto (1995) detailed tropical deforestation threats arising from forest edge communities' abandonment of sustainable forest use practices in the face of various pressures from commercial agriculture. Poverty relief for such communities is proposed as a way to arrest such dynamics. Politically, conservation strategies may be made more viable and effective if coupled with poverty alleviation. If DFM programs target only the interests of large-scale commercial enterprises, the result may be to exacerbate local inequality. Moreover, attaching poverty alleviation goals to conservation programs may help to minimize risks of hostilities, local level subversion, and corruption (Mapedza, 2006). Such risks have been demonstrated by rampant increases in deforestation in protected DFM areas in Indonesia (Burgess et al., 2012) and the regularity with which local corruption led to higher rates of deforestation following early DFM programs around the world (Ostrom 1990, p. 23).

Based on this theoretical discussion, the two most basic hypotheses that we wish to test are as follows:

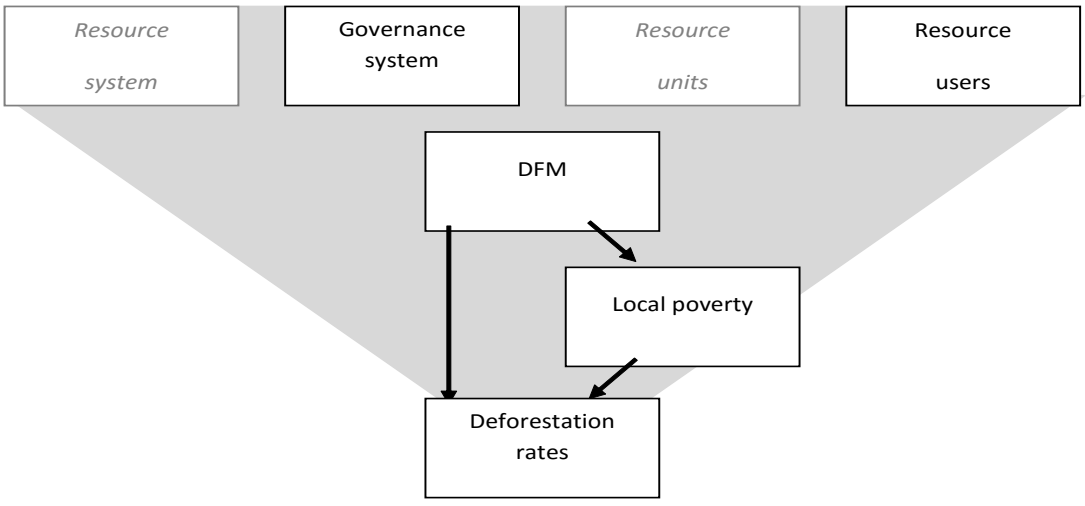
- H1: DFM reduces deforestation rates
- H2: DFM has non-negative impacts on local poverty levels.

The focus on non-negative, as opposed to "positive" impacts per se, reflects a primary concern to ensure that policy interventions do no incidental harm in association with ultimate goals, which in this case are taken to be reductions in deforestation.

Beyond these basic effects, we are interested in the possible mediating role of poverty conditions for deforestation outcomes. The sceptical take is that the two dimensions are orthogonal or even conflicting. The "synergy" position is that poverty consequences of conservation policies mediate effects on deforestation. The nature of the dilemma that pits attending to distributional concerns against targeting major agents of deforestation likely depends on two factors. First are levels of local inequality in terms of holdings and vulnerability due to cessation of deforesting activities (part of the "resource users" context). Thus, we have the following hypothesis:

- H3: DFM program functions to relieve poverty, the stronger its impact will be on reducing deforestation.

Figure 1: Illustrating the theory of change



1.3.2. Unintended consequences and moderating factors for DFM

The ostensible justification for DFM rests on two possible accounts. The first account emphasizes local information and administrative rationality: local authorities are assumed to be (i) more aware of local conditions, meaning that conservation or sustainable forest use policies can be more efficiently designed and executed, and (ii) more responsive to forest communities' interests, which in turn are assumed (perhaps inappropriately) to prefer maintaining forests (Tacconi, 2007). The combination of these conditions is expected to result in both improved consumption for forest communities and reduced deforestation. The second account emphasizes incentives for sustainable resource management (Gibson et al., 2000; Lawry, 2012).

In developing countries, national level administration of forests typically involves designation of national reserves whose use is controlled by national authorities. Forest and forest-edge communities are granted no rights to forest properties in many cases. In a situation of lax enforcement by national authorities, the incentives are for private actors to exploit the window of opportunity to extract forest resources as much as possible. This is in anticipation of national authorities eventually enforcing use restrictions or granting concessions to outsider firms. Decentralization typically involves (i) transferring management rights to local authorities, who are in a better position to monitor usage restrictions, and (ii) allowing these local authorities to tax concessions, thereby generating revenue for investing in local development. Under decentralization, local authorities may now have an incentive to manage forest resources sustainably to secure a steadier revenue stream. The result should be less forest degradation and reduction in levels of poverty.

The assumptions underlying these two "ideal type" accounts may be questioned, however. When authority is devolved to local districts, decisions about conservation must be made with respect to local, within-district needs and conditions. If such devolution means an end to access to resources outside the district, then local authorities may find it necessary to open up natural resources, such as forests, to exploitation in order to satisfy material needs. Burgess et al. (2012) found this to be the case as new districts were formed as part of the decentralization process in Indonesia. A recent example comes with the creation of the new province, North Kalimantan, in Borneo, Indonesia. North Kalimantan was carved out of East Kalimantan province. Because North Kalimantan was thus cut off from East Kalimantan's extractive industry revenues, fears arose that the North Kalimantan leadership would seek to exploit forest areas by allowing mining, clearing for agriculture, and logging[2].

In addition, by carving up territories into separately administered districts, consequences of environmental degradation that were once "internalized" by local authorities are made "external." For example, Lipscomb and Mobarak (2011) found that when territories in Brazil surrounding rivers were carved into separately administered counties, the amount of pollution that upstream users sent downstream increased. This was because the consequences of pollution downstream were no longer of concern to authorities managing upstream parts of the rivers. Similar effects may occur with forest management, for example if the benefits of maintaining riparian forests in one district accrue only to downstream users that reside in another district.

[2] See Diana Parker, "Indonesia's East Kalimantan loses forest area to new province," *Mongabay.com*, February 24, 2013.

Furthermore, knowledge, representation, and incentive problems may undermine the potential conservation benefits from DFM. Local authorities may have inadequate capacity to acquire the necessary knowledge to manage forests effectively. Assuming forest communities do prefer to reduce rates of deforestation, responsiveness to such interests depends on the extent to which local authorities owe their jobs to forest communities via elections or other channels of influence and whether organizations are present that advocate for local conservation interests (Kauneckis and Andersson, 2009). But forest communities may actually prefer more rapid deforestation, in which case accountability would work against conservation. Thus, the effects of DFM on deforestation are likely to depend on levels of public administration capacity, corruption, and the interaction of local democratic accountability and opportunity costs of conservation borne by forest communities.

1.3.3. Moderator hypotheses

We can state these points from the two sections above in terms of moderator hypotheses. The hypotheses are as follows:

- H4: DFM deforestation reduction impacts are positively moderated by the level of local administrative and enforcement capacity.
- H5: DFM deforestation reduction impacts are negatively moderated by levels of corruption in government.
- H6: When the opportunity costs of conservation borne by forest communities are sufficiently low, DFM deforestation reduction impacts are positively moderated by the level of local democratic accountability.

In testing these hypotheses, we control for variations in the design features of DFM programs. DFM programs vary in the extent to which local authorities are granted rights to offer concessions, profit from royalties, or sell property rights to forested lands. Differences across programs will reflect policy-makers' adaptation to contextual factors. In our analysis, we study how contextual variables, and in particular the moderating factors discussed above, affect the DFM design.

1.4. Why it was important to do this review

While the environmental science is clear in characterizing the potential gains from forest conservation (Santilli et al., 2005; Gullison et al., 2007), it remains for social scientists to provide insights into how institutions and incentives may be arranged to realize such potential (Gibson et al., 2000). DFM is among the most prominent national-level institutional approach to forest conservation (Angelsen, 2009). With respect to conservation per se, in theory, DFM has the potential to make forest protection more incentive compatible (Angelsen, 2009; Gibson et al., 2000). However, effects of programs implemented to reduce deforestation may depart substantially from projections, and therefore theory on its own does not provide a reliable guide for policy.

Thus, as Ferraro et al. (2011) and Ferraro (2011) explain quite convincingly, there is a need to move toward credible estimation of the effects of conservation programs to provide better guidance to decision makers. The extent to which such studies have been conducted to date is rather limited, but studies using quasi-experimental approaches do exist. These are

currently scattered in the academic and grey literature, but there is no systematic review of this evidence base.

This review complements a number of other systematic reviews assessing the evidence on interventions considered under the REDD+ initiative and other efforts to reduce deforestation. Bowler et al. (2010) assess the effects of community based forest management on environmental and human welfare outcomes. Part of the reason that we have decided to apply a tight definition of DFM that highlights differences with CBFM (focusing specifically on differences having to do with deconcentration of formal government authority) was to avoid overlap with this review. Geldman et al. (2013) assess the effects of protected areas on environmental outcomes, while Pullin et al. (2013) focus on the human welfare outcomes of protected areas. Finally, Samii et al. (forthcoming) was conducted in parallel to the current project and focus on the effects PES on environmental and human welfare outcomes.

There are also a number of other review based studies with similar objectives as ours. For instance, the volume edited by Angelsen (2009) contains chapters that describe varieties of forest conservation policies, including forest-oriented DFM programs, but these reviews do not apply replicable search and synthesis methods of a systematic review. Kauneckis and Andersson (2009) conduct a cross-national study of forest-oriented DFM impacts but the study only focus on Latin America. Our contribution above and beyond these studies is the use of systematic review methodology to assess the evidence from all developing countries.

2. Objectives

The overall objective of this review is to assess the evidence on the effects of DFM programs on conservation and poverty outcomes and to assess the extent to which the poverty outcomes of such programs in turn affect the extent to which conservation benefits are realized. More specifically, we seek to test the hypotheses set forth above, with hypotheses H1 and H2 being of primary interest. Hypotheses H4 through H6 are of secondary interest and in testing them we seek to evaluate how institutional and social conditions (namely, inequality, institutional capacity, corruption, and democratic accountability) moderate the impact of DFM programs. Our strategy for selecting studies will be targeted toward testing the four primary hypotheses as rigorously as possible. Table 1 relates each hypothesis to the types of evidence we will need. Such an assessment of impacts does not necessarily provide the basis for a full cost-benefit analysis of DFM programs. We acknowledge this limitation and propose that follow-up work should focus on filling in the cost side of the equation as a complement to the analysis that we provide in this report.

Table 1: Questions and types of evidence needed for the review

Hypothesis	Type of evidence
Main Hypotheses • H1: DFM reduce deforestation rates. • H2: DFM have non-negative impact on local poverty levels.	Quantitative data on forest conservation and host community poverty outcomes for sites with DFM and sites that constitute a plausible counterfactual. Qualitative accounts of whether the interventions operated as planned.
Mediator Hypothesis H3: The more a DFM program functions to relieve poverty, the stronger will be it impact on reducing deforestation.	Quantitative estimates of both poverty and deforestation impacts from DFM for at least a subset of cases to assess covariation between the two types of impact. Qualitative accounts of whether poverty benefits (disruption) contributed to compliance (non-compliance) and effective (ineffective) functioning of DFM programs.
Moderator Hypotheses • H4: DFM deforestation reduction impacts are positively moderated by the level of local administrative and enforcement capacity. • H5: DFM deforestation reduction impacts are negatively moderated by the level of corruption in government. • H6: Given that opportunity costs of conservation borne by forest communities are sufficiently low, DFM deforestation reduction impacts are positively moderated by the level of local democratic accountability.	Quantitative measures of local inequality, local capacity, corruption, local democratic accountability, and opportunity costs of conservation borne by forest communities for each study to assess covariation between these measures on the one hand and deforestation and poverty on the other. Qualitative accounts of how issues related to inequality, local capacity, corruption, or local democratic accountability affected the functioning and effectiveness of given DFM programs.

3. Selection Criteria

Our selection criteria are summarized in Table 2. Details are given in the following subsections.

3.1. Participants

The review includes only studies that focus on either (i) deforestation outcomes in forest areas in developing countries or (ii) poverty conditions of forest-dwellers and populations residing in communities that are proximate to natural growth forest areas in developing countries. "Forest" is defined as per the United Nations Food and Agricultural Organization Global Forest Resources Assessment:

> Land spanning more than 0.5 hectares with trees higher than 5 meters and a canopy cover of more than 10 percent, or trees able to reach these thresholds in situ. It does not include land that is predominantly under agricultural or urban land use. (Food and Agricultural Organization, 2010, p. 6)

"Developing countries" are those classified as lower income, lower middle income, or upper middle income by the World Bank in the year of the initiation of the program under study.

3.2. Interventions

The review includes studies of DFM programs. For a program to be considered a DFM program de jure responsibility for managing natural forest resources must pass from centralized to local authorities and this responsibility must grant local authorities the right to grant concessions or establish use restrictions. We were flexible as to the precise level of administration to which responsibilities are passed as well as whether or not the decentralization also involves granting local authorities the right to sell or transfer titles to forested lands. These differences are noted in our characterization of each DFM program below.

3.3. Outcomes

Outcomes of interest are (i) deforestation or (ii) poverty conditions of forest-dwelling communities. Similar to what Bowler et al. (2010) discovered, in our selected studies researchers varied in the precise metric that they used for deforestation impacts, including differences in operational definitions for deforestation or degradation and different types of data sources---for example, on-the-ground point samples or remote sensing samples from satellite or fly-over imagery (West, 2009; Achard and Hansen et al., 2013). We accepted whatever measure was used for the outcomes of interest as presented by the authors.

We sought to assess welfare outcomes in terms of effects on consumption, income, or income potential for members of forest communities residing below or just above the consumption-based, two-dollar per day purchasing power parity absolute poverty line (Ravallion et al., 1991). Such outcomes are typically assessed using household economic surveys or administrative data on consumption, food security, employment, or access to productive assets (Deaton, 1997). In the absence of such fine-grained data, we sought to look at studies that measure differential consumption or income impacts for "poor" versus "non-poor" households or communities. Again, we accepted whatever measure was used for the outcomes of interest as presented by the authors.

We also sought to pay attention to the potential impact of in- or out-migration on poverty outcomes. If a program causes outmigration among the poorest, then the resulting poverty level in the area may be less then was the case before the program. However, it would be inappropriate to take this to mean that the program helped to alleviate poverty.

Finally, we were particularly interested in identifying unintended effects of forest conservation programs on local poverty conditions. We also took note of whether studies accounted for spill-over effects such as deforestation "leakage" or "slippage" (Wu et al., 2001). Failure to account for such spill-over may result in a biased interpretation of the impact of a program.

3.4. Study Types

Table 1 above sketched out the types of quantitative data and qualitative evidence we included in this review. We prioritized identifying rigorous studies that address hypotheses H1 and H2. For quantitative synthesis, we sought well-designed experimental or quasi-experimental studies that use robust methods to construct approximations to the counterfactual for the areas or individuals subject to a DFM program, and then made comparisons between outcomes in the "treatment" group and outcomes in the approximation to the counterfactual for the treatment group.

We accepted for quantitative synthesis only (a) randomized studies or (b) quasi-experimental studies that employ strategies for causal identification with clearly delineated treated and comparison areas and used some method for removing biases due to non-random assignment of treatment. Such methods include: regression adjustment, difference-in-differences estimation, instrumental variables regression, fixed effects regression, regression discontinuity, matching, or inverse-propensity-weighted estimation. While application of such a method was sufficient for inclusion in our study, we appreciate that not all studies apply methods for causal identification with equal rigor and therefore we assessed the quality of all included studies (below we discuss the tools we used to assess study quality).

Quantitative studies that were excluded were those that failed to establish a credible approximation to the treatment group counterfactual. This included studies that relied exclusively on uncontrolled before-after comparisons or failed to adopt any of the above-mentioned methods of analysis to correct for selection bias and confounding. Qualitative data is used in the synthesis to provide descriptions and context for interventions that are included in the quantitative synthesis. Such data were drawn from the quantitative studies themselves as well as qualitative studies that cover the same programs or settings as the quantitative studies.

Table 2: PICOS inclusion criteria

Type	Criteria
Participants	Forest areas or forest communities in developing countries.
Interventions	DFM programs.
Comparisons	“DFM versus no DFM contemporaneous counterfactual”
Outcomes	Deforestation or poverty among forest communities.
Study types	Quantitative studies providing a robust counterfactual via randomized experiment or quasi-experiment or qualitative study with clear research objectives, original analysis, explanation of methods, and seeking to contribute to the academic conservation or social science literature.

4. Search Strategy

4.1. Electronic searches

Our search criteria were developed after initial scoping exercises with a Campbell Collaboration information retrieval specialist. We searched the set of databases, specialist websites, and search engines that Bowler et al. (2010, pp. 55-56) searched as well as others identified to possibly contain relevant content[3]. Our list of sources is given in an appendix below.

Search strings included the following:

> ("decentrali*" OR "co-manag*") AND ("forest" OR "deforest*" OR "ecol*" OR "ecos*" OR "environment*" OR "conservation")

To these keywords we also applied a lower- or middle-income filter based on the Cochrane EPOC filters (http://epocoslo.cochrane.org/lmic-filters). An example of a full search strategy is provided in section 10.1.4 of the appendix.

Some of the databases considered (for example, IDEAS, RUPES, and JSTOR) included search results for non-English language studies even when using English search terms and keywords. Relevance of such results were reviewed by native language speakers (the authors were able to cover French, Spanish, German, and Bahasa Indonesia). Ultimately, only English language studies met our inclusion criteria. We did not impose any date restrictions.

4.2. Other Searches

We carried out hand searches of (i) key journals in relevant fields as listed in the appendix using publisher search engines and (ii) references cited in papers accepted for review as well as in review papers or thematically relevant papers identified during the search. We had members of our advisory group and the specialist agencies listed in the appendix below review our search results to ensure that important studies were not missing from our search results.

[3] We apply the same strategy of reviewing only the first 100 hits for internet search engine results (but not academic database results), given that search engines typically return many thousands of results.

5. Data Collection and Analysis

5.1. Data collection and analysis

5.1.1. Selection of studies

The review team applied the PICOS inclusion criteria listed in Table 2 in three stages: first to titles to remove spurious citations, then to abstracts, and finally to full texts. For all stages, we maintained an account of the number of studies excluded, and the reasons for exclusion, by tracking references in an Endnote database. In the full text stage, excluded studies were tagged in terms of the PICOS criteria that were violated. All screening was done by two independent reviewers from the research team, with disagreements resolved by a third reviewer from the team. To ensure consistency in selection procedures, multiple reviewers reviewed a sample (of 50, for example) of citations and consistency was assessed. If agreement rates were below 90 per cent, we addressed any inconsistencies in interpretation of the criteria to assure at least 90 per cent rates of agreement.

5.1.2. Data extraction and management

For studies eligible for inclusion, we collected data on the study characteristics, findings, and moderators using a coding form (see appendix section 10.6). The data were double-entered into Microsoft Excel by the review team. While it would be ideal to have data on moderator variables measured at the level of the regions in which the programs under study are applied, such data were not typically available. Therefore, we obtained data on the moderator variables using the relevant country-level indicators from the World Bank Governance Indicators. In the end, because of the low number of countries represented, there was little that we could do with these moderator variables.

5.1.3. Assessment of risk of bias in included quantitative studies

Risk of bias was assessed based on the guidance of the IDCG Risk of Bias Tool (version March 2012)[4]. We appraised studies according to the following criteria:

1 Avoiding selection bias due to non-random assignment, non-exogenous source of quasi-experimental variation in assignment, no adjustment for differences in baseline measurements: We assessed this on the basis of whether or not the study worked with a source of exogenous treatment assignment.
2 Avoiding confounding bias due to lack of control for key confounders: Based on an initial reading of the studies, we concluded that key confounders included variables related to land quality, socio-economic conditions (namely, livelihoods, living standards, and access and size of markets for agricultural producers), and accessibility of treated land areas. We assessed whether studies accounted for all three types of confounders.
3 Avoiding motivation bias from measurement strategies that may be tainted by subjects' interest in presenting themselves in a positive light or telling researchers "what they want to hear." This was assessed as being satisfied if study conclusions

[4] We improved our risk of bias assessment over what we had proposed in the protocol to account for more specific nuances of the studies under consideration.

were drawn from effects estimated on non-self-reported data or data based using other measurement strategies that reduce motivation biases.

4 Accounting for potential bias due to spillovers: We assessed whether studies either evaluated units that were insulated from spillover or, in case where spillover was a likely concern, tried to estimate the extent to which spillover may bias naïve comparisons.

5 Free of selective outcome reporting and analysis fishing: We assessed whether studies clearly omitted results that might undermine the conclusions of the study or drew conclusions on the basis of methods that showed high potential for specification search.

6 Appropriate statistical inference due to proper calculation of standard errors and confidence intervals.

We coded each study on the basis of whether they clearly satisfied each of these conditions (coded as "yes"), clearly failed to do so ("no"), or whether it was impossible to judge ("unclear").

5.1.4. Measures of treatment effect

For effects on forest cover, whenever possible we standardize effect sizes to annual forest cover change rates following the proposals of Puyravaud (2003). For effects on material welfare and poverty effects, we use percentage change over estimated average counterfactual outcome (e.g., for income effects, percentage changes in income relative to the average income of the control group). Section 10.3 of the appendix provides the precise calculations for these standardized measures and associated standard error approximations.

When multiple estimates were presented in a given study, we first tried to select the estimate that posed the lowest risk of bias. For studies relying on "conditional independence assumptions" and using multiple regression or matching, this would be the estimate that either controlled-for or achieved the best balance on the largest set of pre-treatment covariates[5]. When there was no clearly defensible way to identify the single estimate in a study with the least risk of bias, we extracted all estimates and then perform our synthesis with the mean of the different estimates as well as the mean of the standard error estimates. This approach does not account for the dependence of the different effect estimates, although it avoids pitfalls in the use of standard approaches that assume independence[6]. (Section 10.9 of the appendix contains tables with all component effects used to construct the mean effects.)

Some of the studies that we included examine the same program, however the estimates that they present cover different time periods, cover different regions, and use independent data sources. As such we treat these as distinct (and statistically independent) estimates.

[5] As discussed in Lawry et al. (2013), in some cases, adding more covariates can actually increase the bias of an estimate, but this is something that is impossible to judge from the data.

[6] Initially we had use an inverse variance weighted averaging approach for synthesizing the different effect estimates. But, as a reviewer astutely pointed out, such an approach ignores the dependence between measures and results in synthesized standard errors that become artificially small as one increases the number of estimates. Our approach to using the mean of the effect estimates and standard errors was proposed as the least misleading way to synthesize effect estimates when there is no clear way to select one minimally biased estimate.

5.1.5. Unit of analysis issues

When the unit of analysis was at a lower level of aggregation than assignment units, standard error calculations should account for the attendant "clustering." We checked to be sure that this was done. In cases where it was not, we noted it in our risk of bias assessment and while we sought to correct them using standard formula in cases where the relevant problems arose the information was not available to do so.

5.1.6. Dealing with missing data and incomplete data

When studies did not report on endpoint or intermediate outcomes, the study authors were contacted to determine whether such outcome data did in fact exist and whether estimates could be produced. However, we did not receive data from any authors that would allow for the construction of effect estimates that went beyond what appeared in the original studies.

5.2. Data synthesis

5.2.1. Quantitative Synthesis

Our plan for the quantitative synthesis was guided by the hypotheses listed in Table 1. The "main hypotheses" (H1 and H2) require a synthesis of basic effect estimates on deforestation and welfare or poverty. For each hypothesis, the following conditions had to apply for a statistical meta-analysis to be justified (adapted from Wilson et al., 2011): i) more than two studies meeting the quantitative inclusion criteria with effect sizes for common outcome constructs AND ii) effects measured against similar comparators.

These conditions were not met, hence preventing meta-analyses. In our protocol, we also proposed a meta-regression approach for testing the moderator and mediator hypotheses. We could not implement this approach for lack of studies. Rather, we were forced to rely on qualitative information relevant for the included studies to comment on, rather than test, the moderator and mediator hypotheses. For similar reasons, we could not implement quantitative analyses of publication biases.

Our quantitative synthesis is limited to tables of effect estimates, forest plots with effect sizes from individual studies, and narrative discussion of trends in the size and direction of the effects reported by the studies. The narrative discussion highlights issues related to modes of measurement, nature of comparators, as well as moderator conditions that should be taken into account when comparing the different effect estimates. We also provide a critical assessment of methods that have been employed and provide concrete recommendations for how rigorous and comparable evidence might be generated in future research.

Use of qualitative data We extracted qualitative information from both the included quantitative studies as well as qualitative studies that covered the same types of programs and contexts (defined by our moderator variables) as our quantitative studies. We use such qualitative data to establish that conditions recorded in quantitative data are being interpreted correctly and that hypothesized, but difficult to measure, chains of events do in fact occur in linking explanatory factors to outcomes (Collier, 2011; Vajja and White, 2008). Our strategy was to search on hypothesis-specific keyword word stems in the articles for the mediator and moderator hypotheses outlined above. We used these search results to localize content that may be relevant to our hypotheses. We extracted whatever qualitative accounts or conclusions that were relevant to each of the hypotheses, and we used these to

provide insights in our narrative discussion. The keyword word stems that we used included the following:

- H3a & H3b: poverty or welfare.
- H4: equal, fair, rights, or property.
- H5a & H5b: capacity, monitor, technical, difficult, or governance.
- H6a & H6b: corrupt, rent, elite, capture, or profit.
- H7: politic, voice, democrat, participat, mobiliz, or accountab.

6. Results

Our search for qualifying studies followed the process presented in Figure 2. This search process identified 1272 on DFM using the search terms described in Section 4.1. Screening of abstracts had us narrow this to 193 DFM studies. Screening of full text papers reduced this first to a set of 8 quantitative and 3 qualitative DFM studies that met our inclusion criteria. We then conducted a second targeted search for other relevant and methodologically adequate qualitative studies that our initial search did not recover. We did this second targeted search by identifying any qualitative studies referenced in the bibliographies of the accepted quantitative studies, checking the websites of the quantitative study authors to see if they had produced complementary qualitative research, and then searching in same databases as in the initial search, using as search terms the names of the programs that were being evaluated in the quantitative studies. This yielded 2 new DFM studies. This yielded our final set of 8 quantitative and 4 qualitative DFM studies. Appendix section 10.7 provides information on studies that were excluded at the full text review stage. Tables 3 and 4 provide characteristics of the included studies, grouped by program.

Figure 2: Study search process

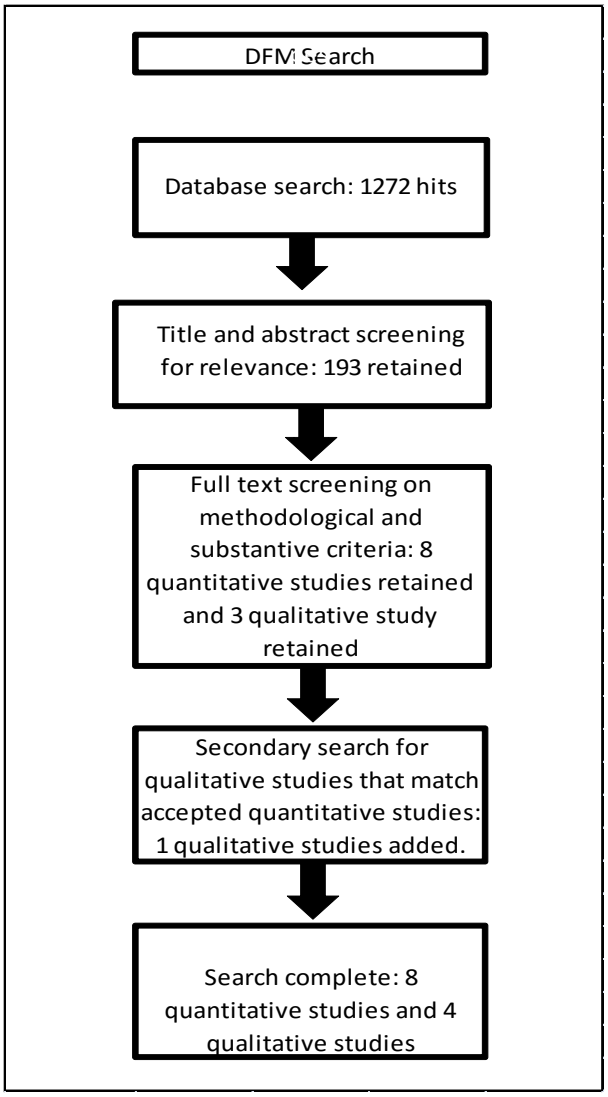


Table 3: Quantitative DFM study characteristics (grouped by program)

Program	Studies	Treatment Group	Time Period	Counterfactual Approximation	Forest Conservation Outcome	Welfare Outcome	Method of attribution	Sample
1931 India Establishment of Van Panchayats and 1976 Encouragement to Form Van Panchayats	Baland et al. (2010)	1931-2001 forest areas under VP management	1931-2001	regression adjusted forest areas under state protection	canopy cover (percent), basal area (m-sq./ha), basal volume (m-cu./ha), lopping (percent), regeneration (saplings>.5cm/ha), and firewood collection time (hours)		multiple regression	399 parcels in 83 villages in Uttaranchal
1931 India Establishment of Van Panchayats (VPs)	Somanathan et al. (2009)	1931-2000 forest areas under VP management	1931-2000	adjacent forest areas under state protection and matched forest areas under state protection	broadleaf forest percent crown cover and pine forest percent crown cover		adjacent area discontinuity and matching	1,091 GIS polygons and 270 strips of land straddling VP managed forest areas in Uttaranchal
1996 Bolivia Forestry Law muncipal forest management	Andersson and Gibson (2007)	1996-2000 forest areas in Santa Cruz with higher levels of municipal forest governance index	1996-2000	regression adjusted forest areas with lower levels of municipal forest governance index	proportion of forest area cut in total and then in permitted and unauthorized areas		multiple regression	30 municipalities in Santa Cruz
1996 Malawi World Bank and DFID sponsored forest comanagement program, operationalizing 1997 Forestry Act	Blessings et al. (2006)	1996-2002 households participating in forest management committees	1996-2002	matched and selection-model adjusted non-participating households in same area		monthly forest income in Kwacha	matching and selection modeling	404 households (including 182 participant households)
1993 Nepal Forestry Law and Establishment of Forest User Groups	Edmonds (2002)	1993-1995 households in communities with forest users groups	1993-1995	regression adjusted and matched households in communities without forest users groups, households that were next in line to receive forest users groups, and households in communities without forest users groups but that would be responsive to an instrumental variable measuring accessibility by forest staff	percentage change in baskets of firewood collected per month		multiple regression, matching, and instrumental variables	1200 households in 100 communities
2001 Ethiopia Farm Africa/SOS-Sahel Participatory Forest Management Program	Gelo and Koch (2012)	2001-2009 households residing in PFM communities	2001-2009	Regression- and matching-adjusted households from non-PFM communities responsive to presence of Menja people as an instrumental variable		Per capita consumption expenditure in Ethiopian Birr.	regression, matching, and instruments variables	377 households (including 200 participant households)
2003 Uganda Establishment of District Forestry Service	Jagger (2008)	2003-2006 household near forest areas under DFS management	2003-2006	Difference-in-difference and Tobit adjusted households near forests under national (Uganda Wildlife Authority) management		Annual forest income per capital in Ugandan Shillings and percent of income from forest	difference-in-difference and multiple regression	753 household observations in 2003 and 2007 in Rwenzori, Bugoma, and Budongo Forest Sites
2005 Kenya Forest Act and Devolution of Management to Community Forest Associations	Ogada (2013)	2005-2010 households participating in community forest associations on common matching support	2005-2010	matched non-CFA households	acerage under tree cultivation		matching	335 households (including 153 participant households)

Table 4: Qualitative DFM study characteristics

Program	Studies	Study Type	Data used
1931 India Establishment of Van Panchayats and 1976 Encouragement to Form Van Panchayats	Agrawal 2001	Historical and political economy analysis	Synthesis of various historical accounts
1993 Nepal Forestry Law and Establishment of Forest User Groups	Baral and Heinen 2007	Analysis of survey data	Household surveys in Bardia National Park and Suklaphanta Wildlife Reserve
2003 Uganda Establishment of District Forestry Service	Banana et al. 2002	Process evaluation and political economy analysis	Official documents and administrative records
2005 Kenya Forest Act and Devolution of Management to Community Forest Associations	Ongugo et al. 2008	Process evaluation and political economy analysis	Official documents and surveys and informal interviews in program areas

6.1. CHARACTERISTICS OF Included studies

The evidence base for the effects of DFM on deforestation and poverty is very limited and the included studies suffer from methodological shortcomings. We identified only a handful of quantitative studies meeting our inclusion criteria and they cover a small number of programs and contexts. Table 8 below displays various design features for the programs included for quantitative and qualitative synthesis. Section 10.2 in the appendix provides more detail on the programs.

6.1.1. DFM

We identified eight studies that met our quantitative inclusion criteria, covering seven programs in seven countries. These are described in Table 3. Among the studies analyzing deforestation effects, we identified five studies covering four countries and programs: India's Van Panchayats system, Bolivia's devolution of forest management to municipalities, Nepal's Forest Users' Groups, and Kenya's devolution of forest management to Community Forest Associations (see appendix section 10.2 for program descriptions). Three studies assess effects on human welfare outcomes, covering three countries and programs: Malawi's Forest Co-management Program, Ethiopia's Participatory Forest Management Program, and Uganda's District Forestry Service (they are described in appendix section 10.2).

6.1.2. Risk of bias in included studies

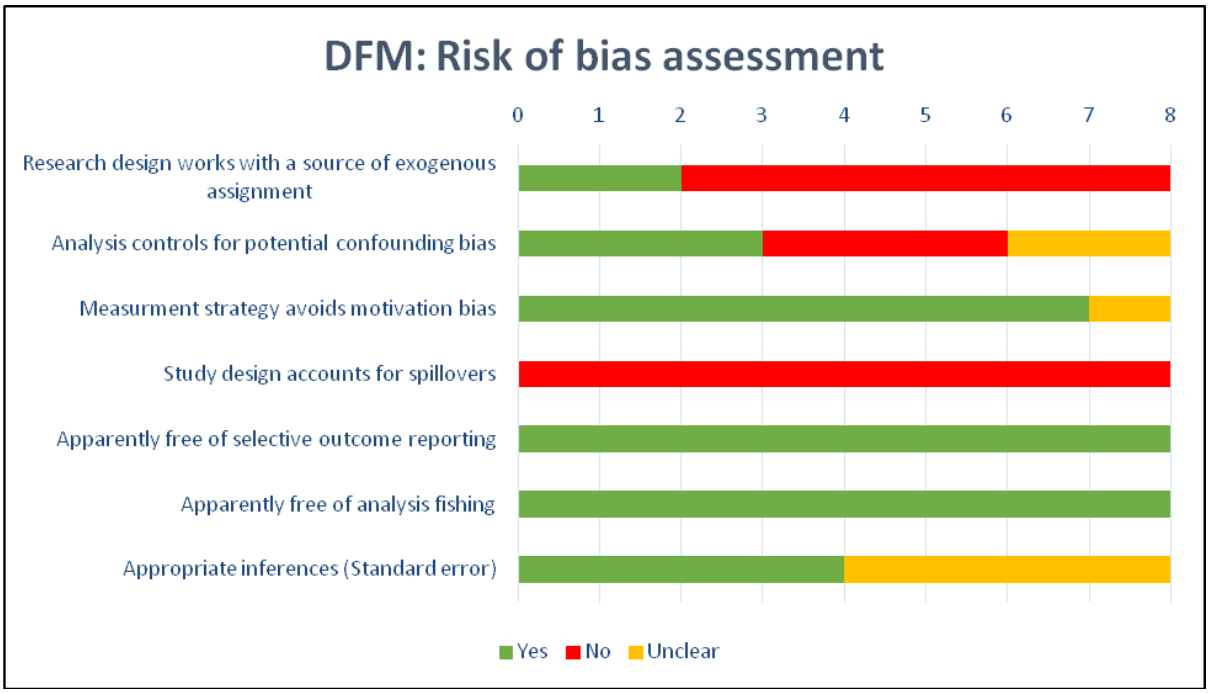
Table 5 shows the results of our risk of bias assessment, summarised for all included studies (study by study risk of bias assessment is available in appendix 10.8.1). None of the studies are based on randomized experiments and none take adequate account of spillover issues[7]. Two of the studies make use of a source of plausibly exogenous variation in treatment assignment: Somanathan et al. (2009) use the discontinuity in treated status between

[7] The Baland et al. (2010) study contains an analysis that proposes to account for spillover by including as a regression control a variable for the amount of available forest in a locality to which effects from treated areas might be displaced. However, this will not remove the bias due to such spillovers, rather it will result in an effect that averages over places that have more or less spillover related bias. The proper approach would have been to restrict the comparisons to parcels with little spillover potential.

treated parcels and those that are adjacent to the boundaries of the treated parcels, while Edmonds (2007) uses an instrumental variables strategy that uses accessibility from idiosyncratically located forestry field staff offices as an instrument. The Somanathan et al. empirical strategy is complicated by the possibility that spillovers will be most pronounced between adjacent parcels. Edmonds's instrumental variables strategy requires that we take on faith the plausibility of the arbitrariness in the placement of forest field staff offices.

Nonetheless, to the extent that these studies gave consideration to the possibility of selection on unobservables, they represent a step forward. This is especially so since the majority of studies did not include controls for factors associated with forested land quality, socio-economic conditions, and market accessibility conditions that were each shown across the studies themselves to be important in predicting the application of DFM policies or participation of households in DFM activities. Again, the methodological limitations of the evidence base are quite severe. While the Edmonds (2007) study stands out among the group for its methodological quality, we hesitate to call it a model because it used an idiosyncratic outcome measure (amounts of firewood collected per month) for forest conservation. A move toward experimental studies would be helpful, and this is a point that we discuss in more detail in our conclusions below.

Table 5: Risk of bias assessment



6.2. Effects of DFM on forest cover

Table 6 and Figure 4 show estimated effects on forest cover outcomes for DFM programs. The four studies use different types of outcomes, and so the synthesis of these effects is not straightforward. Figure 4 shows results for DFM programs in India and Bolivia, for which we could compute standardized effect sizes on forest cover change (taking mean of effect estimates for studies that provide multiple estimates of the same outcome construct). The Bolivia study by Andersson and Gibson (2007) did not consider a simple contrast between a treated and control area but rather used multiple regression to study the partial relationship between forest cover and the application of institutional resources to conservation as

measured by a "municipal forest governance index." For the purposes of this synthesis, we interpret their estimated regression coefficient in terms of a hypothetical intervention that increases the municipal forest governance index by one unit relative to the prevailing status quo. It was only the India and Bolivia studies that estimated effects on forest cover change directly.

The studies on India's Van Panchayats (community forest management committees that originated in a 1931 law) each examine forest areas surrounding communities in the northern Indian state of Uttaranchal (presently referred to as Uttarakhand). The studies differ in their study samples however: Baland et al. use outcome data from an on-the-ground survey administered in 83 villages administered in 2001, while Somanathan et al. use outcome data from satellite imagery taken in 1998. When reporting effects in Table 6, we split the Baland et al. (2010) into two sets of estimates, one that pools parcels covered by all types of "Van Panchayats" and another that tries to isolate effects in parcels covered by Van Panchayats that were established following a 1976 encouragement for their formation. As Table 6 shows, the estimated effects differ quite substantially, with the late-establishment Van Panchayats showing no effect, but pooled set of Van Panchayats showing quite pronounced effects. Baland et al. interpret this as evidence of long-term equilibration.

Baland et al. find from their pooled analysis of Van Panchayats that DFM is associated with an annual forest cover change rate that is 0.40 percentage points (s.e.=0.20, 95% CI: [0.01, 0.79]) improved over what would be the case otherwise (0.20). Somanathan et al. (2009) find that it is insignificant in broadleaf forests (point estimate 0.026 percentage points; s.e.=0.060, 95% CI: [-0.090, 0.140]) but 0.34 percentage points for pine forests (s.e.=0.18, 95% CI: [-0.01, 0.69]). Both of these studies measure effects in terms of overall forest cover change, whether due to deforestation or new forest growth.

Andersson and Gibson (2007) focus only on deforestation, and find that annual forest cover change is statistically insignificant on average (point estimate of 0.4 percentage points; s.e.=1.20, 95% CI: [-1.95, 2.75]) but 0.8 percentage points (s.e.=0.20, 95% CI: [0.41, 1.19]) improved over what the rate would be in the absence of the program in areas where cutting is not permitted (that is, a 0.8 percentage point decrease in the deforestation rate). In section 10.4 of the appendix, we present graphs that display the implied forest cover trajectories based on these effects. The results from India and Bolivia indicate that DFM's effects on forest cover differ depending on the type of forest being considered (broadleaf versus pine) as well as whether the forest area permits timber extraction or not (see the results from Andersson and Gibson 2007).

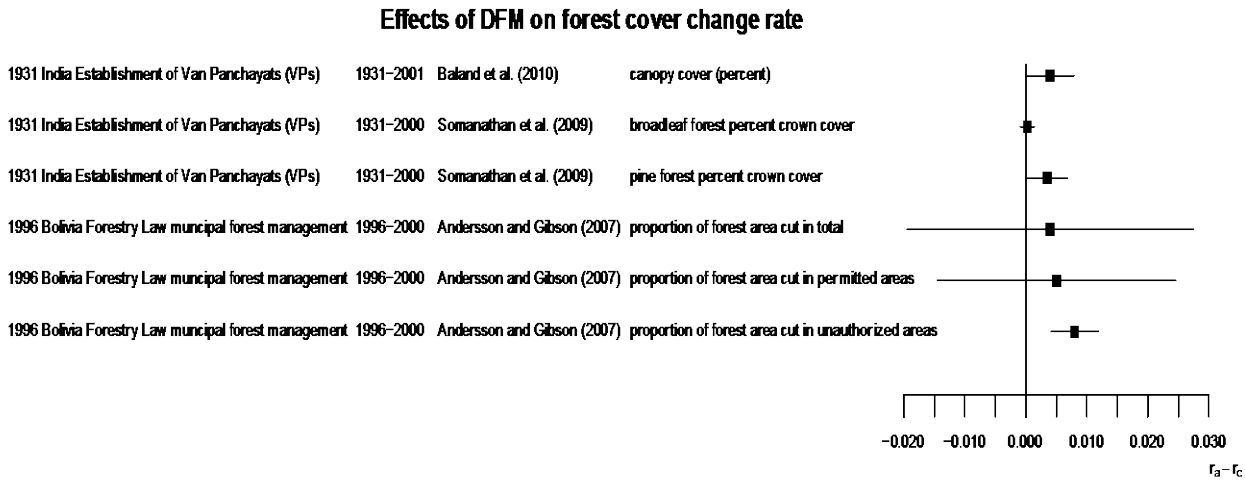
The other two studies looking at forest cover outcomes (from Nepal and Kenya) use outcome measures that cannot be translated into effects on natural forest cover. The Nepal study by Edmonds (2007) uses firewood extraction, while the Kenya study by Ogada (2013) uses acreage under tree cultivation. They each estimate significant beneficial effects from DFM based on these measures.

Table 6: Studies on DFM and forest conservation

Program	Studies	Treatment Group	Time Period	Counterfactual Approximation	Forest Conservation Outcome	Forest Conservation Effect (standard error in parentheses)	ra-rc (standard error in parentheses)	ra
1931 India Establishment of Van Panchayats (VPs)	Baland et al. (2010)	1931-2001 forest areas in Uttaranchal under VP management	1931-2001	regression adjusted forest areas under state protection	canopy cover (percent)	9.35 (4.3)	0.004 (0.002)	-0.014
					basal area (m-sq./ha)	2.47 (5.2)		
					basal volume (m-cu./ha)	194.33 (141.72)		
					lopping (percent)	-18.26 (4.56)		
					regeneration (saplings>.5cm/ha)	43.89 (116.12)		
					firewood collection time (hours)	-0.25 (0.3)		
1931 India Establishment of Van Panchayats (VPs)	Somanathan et al. (2009)	1931-2000 forest areas in Uttaranchal under VP management	1931-2000	*(mean of estimates)*	*broadleaf forest percent crown cover*	*1.15 (2.68)*	*0.0003 (0.001)*	*-0.006*
				(mean of estimates)	*pine forest percent crown cover*	*8.35 (3.95)*	*0.0034 (0.002)*	*-0.013*
1976 India Beginning of Encouragement of Formation of Van Panchayats	Baland et al. (2010)	1976-2001 forest areas under VP management	1976-2001	regression adjusted forest areas under state protection	canopy cover (percent)	0.06 (2.86)		
					basal area (m-sq./ha)	-12.56 (5.68)		
					basal volume (m-cu./ha)	-288.75 (227.27)		
					lopping (percent)	-6.7 (3.55)		
					regeneration (saplings>.5cm/ha)	-8.34 (66.83)		
					firewood collection time (hours)	-0.12 (0.23)		

1996 Bolivia Forestry Law muncipal forest management	Anderssson and Gibson (2007)	1996-2000 forest areas in Santa Cruz with higher levels of municipal forest governance index	1996-2000	regression adjusted forest areas with lower levels of municipal forest governance index	proportion of forest area cut in total	-0.01	(0.04)	0.004	(0.012)	-0.044
					proportion of forest area cut in permitted areas	-0.02	(0.03)	0.005	(0.01)	-0.040
					proportion of forest area cut in unauthorized areas	-0.03	(0.01)	0.008	(0.002)	-0.006
1993 Nepal Forestry Law and Establishment of Forest User Groups	Edmonds (2002)	1993-1995 households in communities with forest users groups	1993-1995	regression adjusted households in communities without forest users groups	percentage change in baskets of firewood collected per month	-11.26	(2.73)			
				kernel matched households in communities without forest users groups	percentage change in baskets of firewood collected per month	-10.02	(6.54)			
				households in communities that were next in line to receive forest users groups	percentage change in baskets of firewood collected per month	-27.15	(6.27)			
				households in communities without forest users groups but that would be responsive to an instrumental variable measuring accessibility to forest commission staff	percentage change in baskets of firewood collected per month	-33.52	(9.12)			
2005 Kenya Forest Act and Devolution of Management to Community Forest Associations	Ogada (2013)	2005-2010 households participating in community forest associations on common matching support	2005-2010	nearest neighbor propensity score matched non-CFA households	acerage under tree cultivation	0.43	(0.10)			
				kernel matched non-CFA households	acerage under tree cultivation	0.43	(0.10)			

Figure 4: Estimates of the effect of DFM on various types of forest cover change rates.



The text beside each estimate shows the program, timeframe of the program, study, and then the type of outcome measure used to derive the forest cover change rate effect. See below for explanation of the items in the forest plot. No random effects synthesis was conducted with these data given that the effects are reported for very different types of forest cover.

The small black squares show the point estimates and the horizontal lines running through the squares show 95% confidence intervals. Effects are measured in terms of changes to annualized forest cover change rates (see appendix section 10.3.2 for details.

6.3. Effects of DFM on poverty

We identified only three studies assessing the effects of DFM on welfare or poverty that met the quantitative inclusion criteria: Blessings et al. (2006) on Malawi's forest co-management programs over six years from 1996 to 2002, Gelo and Koch (2012) on Ethiopia's Participatory Forest Management program over eight years from 2001 to 2009, and Jagger (2008) evaluating Uganda's District Forestry Service and National Forestry Authority over three years from 2003 to 2006. These are described in Table 7 below. The studies differed in the nature of the comparisons that they made. Blessings et al. (2006) studied the effects of household participation in forest management committees. Their estimates of program impact varied considerably across a range of estimation methods, from a 2 per cent to 52 per cent boost to regular forest income for the whole sample, with a mean effect estimate of 26 per cent (s.e. = 11.5 percentage points, 95% CI: [3.46, 48.54]). Gelo and Koch as well as Jagger studied how being in a community located near a forest under DFM affected a household's regular income. Gelo and Koch's looked at the effect of forest proximity on per capita consumption expenditure. Their estimates also varied by method, ranging from 21 per cent to 59 per cent, with a mean effect estimate of 35 per cent (s.e.=9.44 percentage points, 95% CI: [16.5, 53.5]). Jagger, by contrast, focused on forest income. Her estimates varied in their precision, but tended to agree in magnitude at 2 per cent (s.e.=2.36 percentage points, 95% CI: [-2.63, 6.63]) and 16 per cent (s.e. = 6.12 percentage points, 95% CI: [4.00, 28.00]) for communities proximate to District Forestry Service and National Forestry Authority forests, respectively.

But these average effects are not so helpful in assessing effects on poverty if they do not characterize consequences for poor households specifically. The relevant considerations are program participation among poor households, benefits for poor households conditional on participation, and then effects on poor households in DFM areas even if they do not participate. On the first point, we obtained some qualitative insights from Baral and Heinen's (2007) study of DFM in plains region of Nepal. They find that "respondents who were economically better-off were more likely to participate that those who were poorer" (p. 525). Ogada's (2013) analysis of the correlates of participation in Kenya's Community Forest Associations found that access to credit was a strong predictor, which again would suggest a bias against poor households' participation.

Conditional on participation, Blessings et al. (2006) provide an optimistic assessment for participants in Malawi's Forest Co-Management program. They find that the benefits of DFM are stronger for households living below the national poverty line, both in absolute terms and in terms relative to poor households' average incomes. The mean of the absolute forest income effect estimates for poor households is MK 19.55/month (s.e.= 3.05, 95% CI: [13.57, 25.53]; MK 20 = $0.25), which while modest in absolute terms does imply about a 66 per cent (s.e. = 19.55 percentage points, 95% CI: [27.68, 104.32]) boost for poor households and is larger than the MK 10.56/month effect for the whole sample (equal to an 26 per cent income boost on average). Blessings et al. (2006) interpret these results to suggest, "forest co-management may help to improve the living standards of vulnerable households who participate in the program, but is not a long-term solution out of poverty" (p. 574).

The impact on poverty is not limited to effects for participating households however. One must consider how non-participating households are affected. Looking beyond DFM participants per se, Jagger finds that forest incomes are reduced by 6 per cent (s.e.= 8.43, 95% CI: [-22.52, 10.52]) in DFS areas and 10 per cent (s.e.=5.12, 95% CI: [-20.04, 0.04]) in NFA areas for households in the lowest income quartile, and that the positive overall effect is due to gains that are concentrated among wealthier households. In characterizing these perverse effects in the NFA areas, Jagger writes:

> *'The majority of forest income in the study area is from sawn wood, which is harvested and sold illegally. In this case, livelihoods have been improved, but due to the institutional failure of the National Forestry Authority to regulate and enforce rules regarding timber harvesting. The transfer of responsibilities for central forest reserves to the National Forestry Authority has not had the desired effect. Forests have improved the livelihoods, but only for relatively wealthy households accessing forest products illegally'* (2008, p. 26)

It is possible that for a given program poor participating households may benefit but that poor households in general, including non-participating households, are harmed. The Blessings et al. and Jagger studies provide evidence along these lines, but coming as they do from two different programs in two very different contexts, we cannot really compare the two studies. What is needed is further research that accounts for all three mechanisms through which the welfare of the poor may be affected.

Table 7: Studies on the effects of DFM on welfare and estimated income effects

Program	Studies	Treatment Group	Time Period	Counterfactual Approximation	Welfare Outcome	Welfare Effect (standard error in parentheses)		Percentage Effect (standard error in parentheses)	
1996 Malawi World Bank and DFID sponsored forest comanagement program, operationalizing 1997 Forestry Act	Blessings et al. (2006)	1996-2002 households participating in forest management committees	1996-2002	*(mean of estimates)*	*monthly forest income in Kwacha*	*10.56*	*(3.99)*	*26%*	*(11.50)*
		(low-income households)		*(mean of estimates)*	*monthly forest income in Kwacha*	*19.55*	*(3.05)*	*66%*	*(19.55)*
2001 Ethiopia Farm Africa/SOS-Sahel Participatory Forest Management Program	Gelo and Koch (2012)	2001-2009 households residing in PFM communities	2001-2009	*(mean of estimates)*	*Per capita consumption expenditure in Ethiopian Birr.*	*434.43*	*(136.41)*	*35%*	*(9.44)*
2003 Uganda Establishment of District Forestry Service	Jagger (2008)	2003-2006 household near forest areas under DFS management	2003-2006	Difference-in-difference and Tobit adjusted households near forests under national (Uganda Wildlife Authority) management	Annual forest income per capita in Ugandan Shillings	9838	(14684)	2%	(2.36)
		(lowest income quartile)				-17469	-26073	-6%	(8.43)
					Percent of income from forest	3.06	(4.57)		
	Jagger (2008)	2003-2006 household near forest areas under NFA management	2003-2006	Difference-in-difference and Tobit adjusted households near forests under national (Uganda Wildlife Authority) management	Annual forest income per capita in Ugandan Shillings	95972	(37256)	16%	(6.12)
		(lowest income quartile)				-27753	-14160	-10%	(5.12)
					Share of income from forest	6.37	(3.25)		

6.4. Intersection of poverty and deforestation impact

Our theoretical discussion above proposed that the conservation impact of a DFM program might be tied to its poverty impact. In order to address this possibility, one would need studies that evaluate both poverty and conservation outcomes jointly. Unfortunately, no such studies were identified. Surprisingly, there was no overlap in the programs covered by the quantitative studies on conservation and poverty, respectively, and so even general comparisons across studies are impossible. That being the case, we cannot address the mediation hypothesis.

Some studies reported on how prevailing conditions of poverty might moderate the impact of DFM programs. The DFM literature that we reviewed did not include any such studies on either the moderating or mediating impact of poverty conditions on conservation effectiveness.

6.5. THE ROLE of institutional and social context

The qualitative studies contain some insights on how the institutional and social context might influence the design and performance of DFM programs. For DFM programs, issues of resourcing local institutions and levels of participation and local accountability of such institutions feature prominently in qualitative accounts. We summarize the evidence from these accounts in the sections that follow.

6.5.1. Inequality

Our theoretical analysis suggests that inequality negatively moderates conservation impact of DFM programs. The literature reviewed does not explore the moderating effects of inequality on DFM programs.

6.5.2. Capacity

Our theoretical discussion proposed that the level of local state capacity positively moderates the conservation impact of DFM programs. We cannot assess this hypothesis quantitatively given the low number of studies. Also, the effect estimates do not vary enough for us to rank clearly the success of the programs and implementation periods. Nonetheless, qualitative accounts provide some useful insights.

In the case of DFM programs the same laws and policies that devolved forest management rights also created new decentralized institutions, and a key issue that many authors raise is the extent to which these institutions are properly resourced. Agrawal (2001) describes the Indian government "governmentalizing" communities by creating new regulatory bodies, calling the process "perhaps the most critical aspect of any program of environmental decentralization" (p. 209). Where decentralization of forest management came as part of broader decentralization of governance, such as in Bolivia, the reforms tended to come with increases in funding to local institutions, as described in Andersson and Gibson (2007):

> *'Through the reforms, many municipal governments' annual operating budgets increased by as much as 1,000 percent, and several went from a zero budget to tens of thousands of dollars in available resources, practically overnight.'* (p. 105)

These newly-empowered local institutions were then responsible, among other things, for monitoring forest land and enforcing environmental regulations. However, legal empowerment was not always accompanied by real-world capacities to enforce regulation. Somanathan et al. (2009) describe how the Van Panchayats can levy a fine, but are forced to engage the central government's legal system if the accused refuses to pay the fine—a difficult process which is rarely used.

In other DFM cases, resourcing problems appeared to seriously undermine performance. For example, Jagger's disturbing finding that gains from DFM went only to richer participants and came from illegally harvested timber may have been a result of poor local administrative capacity. Examining the same program, Banana et al. (2002) conducted field interviews with forest council members and discovered "a lack of interest by the local councils to take on extra duties of managing forest resources due to lack of motivation" (p. 12), with rule enforcement "gradually diminish[ing] at each successive lower level of governance" (p. 15).

Despite these potential issues surrounding administrative capacity, the studies included in this review reveal some promising aspects. Somanathan et al. (2009) find that India's Van Panchayat program has a significant effect on forest crown cover. However, they also find that the decentralized administration is associated with costs approximately 1/13th those of under state administration. These findings suggest that there is room to invest more in local DFM institutions while still spending less than the central government would by maintaining forest management authority.

6.5.3. Corruption

Our theoretical analysis suggests that corruption in government negatively moderates conservation impact of DFM programs. The literature reviewed does not explore the moderating effects of corruption on DFM programs. Some DFM programs, such as those in Indonesia (Potter & Badcock, 2001) were implemented to replace, in theory, the corrupt practices of previous regimes, but there is not currently any research into the level of corruption in the new system, or that system's impact on deforestation reduction.

6.5.4. Democratic Accountability

Our theoretical discussion proposed that the more democratically accountable the institutions administering DFM are, the larger the conservation impact will be, assuming that forest edge community members favour conservation. This hypothesis requires that the assumption holds of forest edge community members favouring conservation, an assumption that may not be valid in all contexts. When this does hold, the presumption is that responsiveness to such interests will increase DFM performance. Ongugo et al. (2008) support this idea with their qualitative study in Kenya of the apparently successful forest associations studied in Ogada (2013). Ongugo describes the methods by which Kenyan forest associations were formed. A large majority (82%) were formed when community members realized "the need to form associations with the aim of sustainable management of the forest." (p. 14). In comparison, 12 per cent were formed by governmental decree, and the remaining 6 per cent by local NGOs. Thus, we have a case of an effective DFM program that operated on the basis of broad-based participation and, presumably, accountability. Along similar lines, Baral and Heinen (2007) studied DFM programs in two different regions of Nepal. In one region, the groups responsible for managing forestland are formed in a more democratic, bottom-up manner than those of the other area, where groups are formed

by the central government. In the more democratic region, community members were more likely to participate in conservation activities.

In addition to fostering a feeling of participatory accountability in forest dwelling communities, transparent democratic processes may also increase the likelihood that forest councils will maintain records of their meetings, finances, and decisions. According to Agrawal (2001), this makes it easier for central government officials to monitor forest management and know when additional resources are needed and where.

6.6. Variation in the Program Design Features

We also documented variation in design features of the DFM programs covered by the eligible studies. The number of studies is too few to assess rigorously how such design features may affect program impact. Nonetheless, we can point out some patterns.

Table 8 shows how the DFM programs that we study differ in terms of the level of authority delegated to the local institutions formed under the DFM program. Specifically, we coded the authority level of local institutions as "high" if local authorities were granted rights to offer commercial concessions on forested lands, tax such concessions, or sell property rights to forested lands; authority levels were coded as "low" if such rights were not granted to local authorities. We find that the apparently beneficial effects of DFM programs in Bolivia, India, and Kenya were achieved despite low authority levels invested in local institutions. Jagger (2008) provides a within-country comparison on the consequences of different authority levels on welfare impact: the DFA was granted low authority, while the NFS was granted high authority. From above, we saw that the effects of the NFA program were much stronger, although in a way that suggests a concentration in the access of households to forest incomes (and thus possible contributions to increased inequality). Thus, from the DFM cases, we find that low authority levels appear to be associated with beneficial conservation impact (although we do not know whether the impact would have been larger with greater authority) and, from the one case that offers a direct comparison, more moderate welfare effects. Again, these results are only suggestive given the small number of cases.

Table 8: Level of authority invested in local institutions in DFM programs

Country	Program	Article(s)	Authority Level
Bolivia	1996 Forestry Law municipal forest management	Andersson and Gibson (2006)	Low
Ethiopia	2001 Farm Africa/SOS-Sahel Participatory Forest Management Program	Gelo and Koch (2012)	Low
India	1931 Establishment of Van Panchayats (VPs)	Baland et al. (2010) Somanathan et al. (2005)	Low
Kenya	2005 Forest Act and Devolution of Management to Community Forest Associations	Ogada (2012)	Low
Malawi	1996 World Bank and DFID sponsored forest comanagement program, operationalizing 1997 Forestry Act	Blessings et al. (2006)	Low
Nepal	1993 Forestry Law and Establishment of Forest User Groups	Edmonds (2002)	High
Uganda	2003 Establishment of District Forestry Service	Jagger (2008)	Low
Uganda	2003 Establishment of National Forestry Authority	Jagger (2008)	High

7. Authors' Conclusions

7.1. Summary of findings with respect to our HYPOTHESES

Our analysis sought to test two main hypotheses and then a set of mediator and moderator hypotheses:

Main hypotheses:

1. H1: DFM reduce deforestation rates.
2. H2: DFM have non-negative impact on local poverty levels.

Mediator hypotheses

3. H3: The more a DFM program functions to relieve poverty, the stronger will be it impact on reducing deforestation.

Moderator hypotheses

4. H4: DFM deforestation reduction impacts are positively moderated by the level of local administrative and enforcement capacity.
5. H5: DFM deforestation reduction impacts are negatively moderated by the level of corruption in government.
6. H6: Given that opportunity costs of conservation borne by forest communities are sufficiently low, DFM deforestation reduction impacts are positively moderated by the level of local democratic accountability.

Limitations of the evidence base preclude definitive tests of any of these hypotheses. With respect to hypotheses 1, the evidence suggests DFM reduce deforestation rates on average, although at a modest level. For hypotheses 2, we cannot say that the evidence indicates non-negative effects on poverty for DFM. This is a troubling finding, but it is based on only a handful of cases and therefore deserves much more empirical attention. We were unable to assess hypotheses 3. We found qualitative evidence in support of hypotheses 4 and 5, suggesting that the contextual conditions of inequality, local administrative and enforcement capacity, and corruption may undermine the effectiveness of DFM programs. However, in the absence of clear tests, these findings remain highly uncertain. With respect to hypothesis 7, we found evidence to show that, on the one hand, broad-based participation in local institutions tends to be associated with better outcomes under DFM but that, on the other hand, it may be problematic to assume that forest edge communities are especially interested in natural forest conservation as opposed to other ways of putting forest lands to use.

7.2. Implications for policy: Elusive win-win

Our review sought to address the fundamental issues of the extent to which environmental and poverty reduction goals conflict and the scope for "win-win" strategies that generate both significant conservation and poverty reduction benefits. We presented two sides of the argument about the extent to which conservation and poverty reduction goals ought to be married to each other.

We find positive, though modest, conservation effects, possible cost savings in implementation, and at least from one study (Blessings et al. 2006), evidence that poor households that participate in DFM institutions can benefit. Does this suggest that DFM provides a win-win solution? We are not so confident about this conclusion. When Jagger (2008) expanded the view to consider poor households in DFM areas, whether or not they participated, the results indicated a possible reduction in welfare. It is not clear whether this was due to especially bad implementation (as suggested by Banana et al. 2002) or whether this highlighted a fundamental feature of DFM programs. This is a question that requires further research. Until this is obtained, it is not clear that DFM programs provide an escape from conservation-poverty dilemmas that adversely affect the poor.

7.3. implications for research: Need for more rigorous research across contexts

Our final conclusion re-emphasizes the poor state of the evidence base for conservation programming. Much advanced scientific effort and extensive investment has gone into measuring forest conditions around the world. Relative to that, the evidence base on the ex post performance of DFM programs is limited in size and methodologically weak. Composed as it is of a few quasi-experimental studies of varying quality, the evidence base provides a very shaky foundation, likely tainted by selection biases, for environmental and development policy making.

Randomised studies of DFM are challenging to conduct because the decentralization processes to which DFM programs are attached typically occur as part of broader, nation-wide policy transitions. Nevertheless, DFM programs typically complement de jure decentralization with programs that help establish local capacities that allow for DFM's implementation. The phasing in of such establishment programs would seem to provide for ample opportunity for randomisation, and in fact, Edmonds (2002) used such naturally occurring phasing as the basis of his identification strategy. DFM researchers should look to recent research in development economics, and in particular on community-directed development programs, for models of research designs (Casey et al. 2012; see also Banerjee and Duflo, 2011 and Karlan and Appel, 2012).

The quasi-experimental studies covered in this review might be replicable for other countries and programs given tools such as Google Earth Engine's high resolution forest cover mapping (Hansen et al. 2013). Thus, there would seem to be ample opportunity to expand the coverage of these types of quasi-experimental studies around the world as formative research that might inform more finely targeted field experimental studies.

A substantive priority for future research is further examination of welfare impacts that extend beyond that of DFM institution participants, and include populations living in DFM adjacent areas. Moreover, studies should measure effects on both environmental and human welfare outcomes to allow for a comprehensive assessment of the overall impact of DFM programs. Quantitative studies should also collect data on context, implementation and costs.

The current evidence base excludes vast experience from other parts of the world. We were surprised to find no studies from either Indonesia or Brazil. Future research should focus on assessing the effects of DFM across a diversity of contexts, including in particular contexts

with high de-forestation rates. Finally, studies should also examine relevant moderators to inform the design and implementation of future conservation programs.

7.4. Limitations and deviations from protocol

Limitations of this study derive from the very few cases that the quantitative evidence base covers. The countries that we cover in this review exclude the major forested areas in the tropics, including the forests of the Amazon Basin, Indonesia, and the Congo Basin.

Details on the deviations from protocol are listed in section 10.5 of the appendix. The key point that we make there is that the very limited extent of the database prevented us from being able to do the type of thorough analysis of factors that moderate the effectiveness of DFM programs. Neither were we able to investigate directly how deforestation and poverty alleviation goals interact since we found studies that looked at effects on these outcomes jointly.

7.5. Acknowledgements

We thank the authors of the studies included in this review for their excellent work and the feedback they provided to us on earlier drafts of this review. We received enormously helpful feedback from three anonymous reviewers, the Campbell Collaboration International Development Coordinating Group Editor, Birte Snilstveit, and Methods Editor, Terri Pigott; members of an advisory group that included Michele de Nevers, Ilaria Firmian, Steven Lawry, Carlos Manuel Rodriguez, Jesus Quintana, and David Young; conference participants at the 2013 American Economic Association annual meeting, 2012 3ie Systematic Review Colloquium, and the 2014 Campbell Colloquium; and David Kaimowitz and Sven Wunder. Reema Badrinarayan, Harini Dedhia, Justin Epstein, Shomik Ghosh and Johan Gorr provided excellent research assistance.

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* indicates inclusion for quantitative synthesis.
# indicates inclusion for qualitative, but not quantitative, synthesis.

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Appendices

Search Strategy Appendix

Electronic search databases and websites

We reproduce the list of sources from Bowler et al. (2010, pp. 55-56):

Literature databases

- AgEcon (http://ageconsearch.umn.edu/)
- Greenfile (Ebsco)
- Agris (FAO - http://agris.fao.org/)
- RUPES (http://rupes.worldagroforestry.org)
- Science and Social Science Citation Index
- British Library for Development Studies
- Scopus
- Agricola
- CAB Abstracts
- EMBASE
- Science Direct
- EconLit
- JSTOR
- Directory of Open Access Journals
- IDEAS

Web search engines [NB: "jux2.com" is excluded from the original list]:

- http://www.google.com
- http://scholar.google.com
- http://scientific.thomsonwebplus.com/
- http://www.scirus.com (restricted to "web sources" only)

Specialist websites

- http://www.capri.cgiar.org/
- http://www.catie.org.ac.cr/
- http://www.cbnrm.net/
- http://www.cgiar.org/
- http://www.cifor.cgiar.org
- http://www.cof.orst.edu/org/istf/ftpp.htm
- http://www.communityforestryinternational.org/
- http://www.conservation.org
- http://www.dfid.gov.uk
- http://www.etfrn.org
- http://www.forestrycenter.org/
- http://forests.org/
- http://www.forestsandcommunities.org/
- http://www.ifad.org/
- http://www.iied.org

- http://www.indiana.edu/~iascp/
- http://www.iucn.org
- http://www.livelihoods.org
- http://www.www.macp-pk.org
- http://www.odi.org
- http://www.www.panda.org
- http://www.pfc.cfs.nrcan.gc.ca/
- http://www.rainforestportal.org/
- http://www.recoftc.org
- http://www.thegef.org
- http://www.tropenbos.nl/
- http://www.usaid.gov/
- http://www.waldbau.uni-freiburg.de/forlive/Home.html
- http://www.wcs.org

Specialist agencies contacted via email

- United Nations Development Programme (UNDP)
- United Nations Environment Programme (UNEP)
- World Bank
- African Development Bank (AFDB)
- Asian Development Bank (ADB)
- European Bank for Reconstruction and Development (EBRD)
- Inter-American Development Bank (IDB)
- International Fund for Agricultural Development (IFAD)
- UN Food and Agriculture Organisation (FAO)
- UN Industrial Development Organisation (UNIDO)

Field journals

American Economic Review

American Economic Journal: Applied

American Economic Journal: Economic Policy

American Journal of Political Science

American Political Science Review

Conservation Biology

Ecological Economics

Environment and Development Economics

Environment, Development and Sustainability

Environmental and Resource Economics

Forest Policy and Economics

Journal of Development Economics

Journal of Environmental Economics and Management

Journal of Environmental Planning and Management

Journal of Forest Economics

Journal of Politics

Journal of Public Economics

Journal of Regulatory Economics

Journal of Sustainable Forestry

Land Economics

Proceedings of the National Academy of Sciences

Resource and Energy Economics

Review of Economics and Statistics

Review of Environmental Economics and Policy

World Development

LMIC Filter

Below is the set of terms used to filter searches and limit results to studies carried out in low or middling income countries (LMICs):

AND (“Africa” OR “Asia” OR “Caribbean” OR “West Indies” OR “South America” OR “Latin America” OR “Central America” OR “Afghanistan” OR “Albania” OR “Algeria” OR “American Samoa” OR “Angola” OR “Argentina” OR “Armenia” OR “Azerbaijan” OR “Bangladesh” OR “Benin” OR “Belize” OR “Bhutan” OR “Bolivia” OR “Botswana” OR “Brazil” OR “Bulgaria” OR “Burkina Faso” OR “Burundi” OR “Cambodia” OR “Cameroon” OR “Cape Verde” OR “Central African Republic” OR “Chad” OR “Chile” OR “China” OR “Colombia” OR “Comoros” OR “Congo” OR “Costa Rica” OR “Cote d'Ivoire” OR “Cuba” OR “Djibouti” OR “Dominica” OR “Dominican Republic” OR “East Timor” OR “Ecuador” OR “Egypt” OR “El Salvador” OR “Eritrea” OR “Ethiopia” OR “Fiji” OR “Gabon” OR “Gambia” OR “Ghana” OR “Grenada” OR “Guatemala” OR “Guinea” OR “Guinea-Bissau” OR “Guam” OR “Guyana” OR “Haiti” OR “Honduras” OR “India” OR “Indonesia” OR “Ivory Coast” OR “Jamaica” OR “Jordan” OR “Kazakhstan” OR “Kenya” OR “Kyrgyzstan” OR “Laos” OR “Lebanon” OR “Lesotho” OR “Liberia” OR “Madagascar” OR “Malaysia” OR “Malawi” OR “Mali” OR “Malta” OR “Mauritania” OR “Mauritius” OR “Mexico” OR “Micronesia” OR “Moldova” OR “Mongolia” OR “Morocco” OR “Mozambique” OR “Myanmar” OR “Namibia” OR “Nepal” OR “Nicaragua” OR “Niger” OR “Nigeria” OR “Pakistan” OR “Panama” OR “Papua New Guinea” OR “Paraguay” OR “Peru” OR “Philippines” OR “Puerto Rico” OR “Rwanda” OR “Senegal” OR “Sierra Leone” OR “Sri Lanka” OR “Somalia” OR “Sudan” OR “Swaziland” OR “Tajikistan” OR “Tanzania” OR “Thailand” OR “Togo” OR “Tonga” OR “Tunisia” OR “Turkey” OR “Turkmenistan” OR “Uganda” OR “Uzbekistan” OR “Venezuela” OR “Vietnam” OR “Yemen” OR “Zambia” OR “Zimbabwe”)

Example of a detailed search strategy

To illustrate how the search strategy was applied, here are the steps used to search the AgEcon database (http://ageconsearch.umn.edu/) as applied on February 16, 2013:

- Go to database website: http://ageconsearch.umn.edu/
- Enter into the search text boxes the following:
    - Box 1: “decentrali* OR co-manag*” with search type “anywhere in record”
    - Box 2: AND “forest OR deforest* OR ecol* OR ecos* environment* OR conservation” with search type anywhere in record.
    - Box 3: AND enter sections of the LMIC filter shown above (the entire filter cannot be entered at once, so enter sections of the filter until all keywords have been used) with search type “anywhere in record”
- The search yields 12 hits with title and abstract information. Extract information and enter in search database (using Endnote).

Brief descriptions of programs included in quantitative synthesis

DFM

Andersson & Gibson 2007: The 1996 Forestry Law in Bolivia devolved forest management authority to municipalities. Each municipal government was charged with establishing a forest management plan (including granting concessions), then monitoring forestland and enforcing compliance.

Baland et al. 2010: Starting in 1931, the Indian government began allowing municipalities to form forest management councils called Van Panchayats. In the 1970s, the government began encouraging Van Panchayat formation as a primary method of forest management.

Blessings et al. 2006: In 1996, the World Bank and DFID sponsored a forest co-management program. Under this program, communities are organized into voluntary forest management committees tasked with boundary marking, firebreak maintenance, controlled early burning, selective harvesting, and the monitoring of illegal use of forest resources (including logging).

Edmonds 2002: In 1993, the government of Nepal transferred management of accessible forestland to local communities. The communities formed Forest Users Groups, which establish restrictions on forest use, grant concessions to and collect tax revenues from those selling forest materials, and maintain forest materials.

Gelo & Koch 2012: In 2001, Farm Africa and SOS-Sahel began a participatory forest management program where communities choose which households are eligible for the program. These households then decide whether or not to participate. Those that do form Forest User Groups, which work with Farm Africa/SOS-Sahel to create and enact Forest Management Plans.

Jagger 2008: In 2003, Uganda began reforming forest sector governance. Decentralized management authority devolved to two different bodies.

- District Forestry Service: Devolution of management rights to local governing bodies.
- National Forestry Authority: Devolution of management rights to for-profit parastatal organization.

Ogada 2013: The Forest Act of 2005 established the Kenya Forest Service, which devolved forest management authority to private sector forest conservation committees and community forest associations (CFAs).

Somanathan et al. 2009: Starting in 1931, the Indian government began allowing municipalities to form forest management councils called Van Panchayats. In the 1970s, the government began encouraging Van Panchayat formation as a primary method of forest management.

Effect sizes

Imputing standard errors

In some cases, standard errors are not reported but rather t-statistics, p-values, or sometimes only significance levels. When t-statistics were reported for an effect Δ, we computed the standard error as Δ /t. From significance levels, we imputed the standard error from a t-statistic equal to the quantile at the posted significance level---e.g., if an effect Δ was shown to have $p < .05$ for a two-way test, we imputed a t-statistic corresponding to the .975 quantile of the normal distribution (t = 1.96) and then a standard error corresponding to | Δ /t|. Generally speaking the formula for imputed standard errors (se.imp) from a two-sided p value under a normal approximation is as follows:

$$se.imp = \Delta / \mathbf{\Phi}^{-1}(1-.5*p),$$

where Φ-1 is the inverse CDF of the normal distribution

When no standard error, t-statistic, or statistical significance level was given, we imputed a p-value of 0.5 and then assigned the associated standard error, which is equivalent to assigned a standard error equal to (1/0.67)| Δ | = 1.48| Δ |. Imputing a p-value of 0.5 is not completely arbitrary, as it corresponds to the mean of the posterior p-value distribution under the null hypothesis, given a uniform prior over 0 to 1. In addition, such constant scaling will mechanically impute smaller standard errors for estimates closer to zero, in which case inverse weighted averages across numerous estimates will tend to drive the average toward zero; this again is consistent with assuming a prior of a null effect and updating it with vague information.

Standardized forest cover effect sizes

Puyravaud (2003) proposes a standardized measure of forest cover change based on the compound interest law,

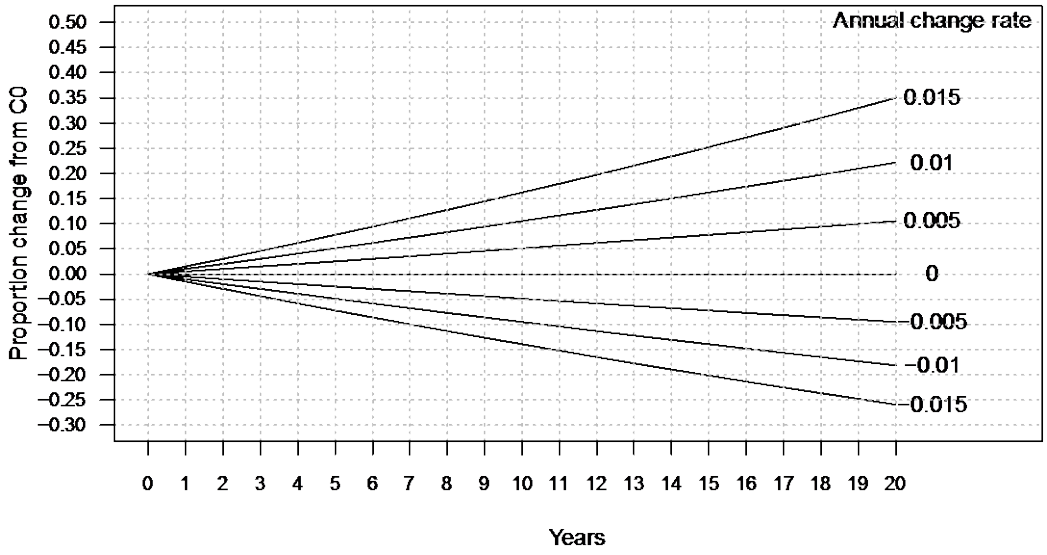
$$C = C_0 \exp[r(t_2-t_1)]$$

where C is the amount of forest cover at the time of follow-up, C0 is forest cover at baseline, r is the continuous rate of change per unit of time, and t2-t1 is the amount of time elapsed between periods t1 and t2. Taking the natural log of both sides and rearranging yields

$$r = \ln(C/C_0)/(t_2-t_1)$$

This measure of rate of change takes a sign that is positive for net forest cover growth and negative for net deforestation. The quantity 100r% is interpretable as the percent change in

Figure A. 1: Forest cover change under the compound interest law.



forest cover per period. For the studies considered above, we use year as the relevant period. Figure A.1 below shows how this annual rate of change translates into proportion change in forest cover for up to twenty years. Thus, a program that has the effect of sustaining a .01 increase in the annual rate of forest cover change (or, a .01 decrease in the deforestation rate) would induce on the order of a 10 percent increase in the extent of forest cover after ten years and 20 percent increase in the extent of forest cover after twenty years, as compared to the counterfactual of no program (at these small values of r, the annual change rate, and for these time scales, the compound interest law is almost perfectly linear in time).

Moving from this measure of forest cover change to a standardized effect measure may proceed as follows. We work with the difference between the actual forest cover change rate in the treated area and the counterfactual change rate for that area. Studies typically report forest coverage on an average-per-parcel basis. Given N parcels, then this does not affect the calculations as (C/N)/(C0/N) = C/C0. Using the a subscript to denote quantities for the actual treated area and c subscript for counterfactual quantities, then we note that

$$r_a - r_c = [\ln(C_a/C_0) - \ln(C_c/C_0)]/(t_2-t_1)$$

$$= [\ln(C_a/C_c)]/(t_2-t_1)$$

$$= [\ln((C_a/N)/\{[(C_a/N) - \Delta]\})]/(t_2-t_1),$$

where Δ is the estimated effect on mean forest cover change (in area units) in the N parcels, Ca/N is mean forest cover in the N treated parcels and (Ca/N) – Δ estimates mean

counterfactual forest cover in the treated parcels. Given a standard error for Δ denoted as se(Δ), an approximate standard error for the difference in rates that takes the treated area forest cover Ca as fixed is obtained via the delta method as

$$se(r_a - r_c) = se(\Delta)/\{[(C_a/N) - \Delta](t_2 - t_1)\}.$$

For studies that report effects in terms of proportion of fully forested parcels deforested, denoted as Δ_p, we have that the average pre-treatment forest cover in treated parcels,

$$P_0 = (C_0/N)/(A/N) = C_0/A,$$

is fixed to 1, where A denotes the sum of parcel areas. To compute $r_a - r_c$, we need the average actual post-treatment forest cover proportion in treated areas,

$$P_a = (C_a/N)/(A/N),$$

which we can also use to compute the counterfactual forest cover proportion,

$$P_c = (C_c/N)/(A/N) = P_a - \Delta_p.$$

Since Pa/Pc = Ca/Cc, we have

$$r_a - r_c = \{\ln[P_a/(P_c - \Delta_p)]\}/(t_2 - t_1),$$

with approximate standard error

$$se_p(r_a - r_c) = se(\Delta_p)/ [(P_c - \Delta_p)(t_2 - t_1)].$$

In cases where Pa is not reported, we impute a value using the treatment parcels deforestation rate in the most similar case where such information is provided.

Standardized consumption and income effects

We standardized consumption or income effects in terms of percentage change relative to the counterfactual. For studies that estimate effects using log of income or log of consumption expenditure as the outcome, then for an effect estimated as Δ l, the percentage change over the counterfactual is given by

$$PC = 100[\exp(\Delta_l) - 1]\%.$$

A delta method approximate standard error is given by

$$se_e(PC) = 100\ se(\Delta_l)\ \exp(\Delta_l).$$

For studies that use the raw income or consumption expenditure levels as the outcome, then for an effect estimated as Δ_r, the percentage change over the counterfactual is given by

$$PC = 100[\Delta_r /(T - \Delta_r)]\%,$$

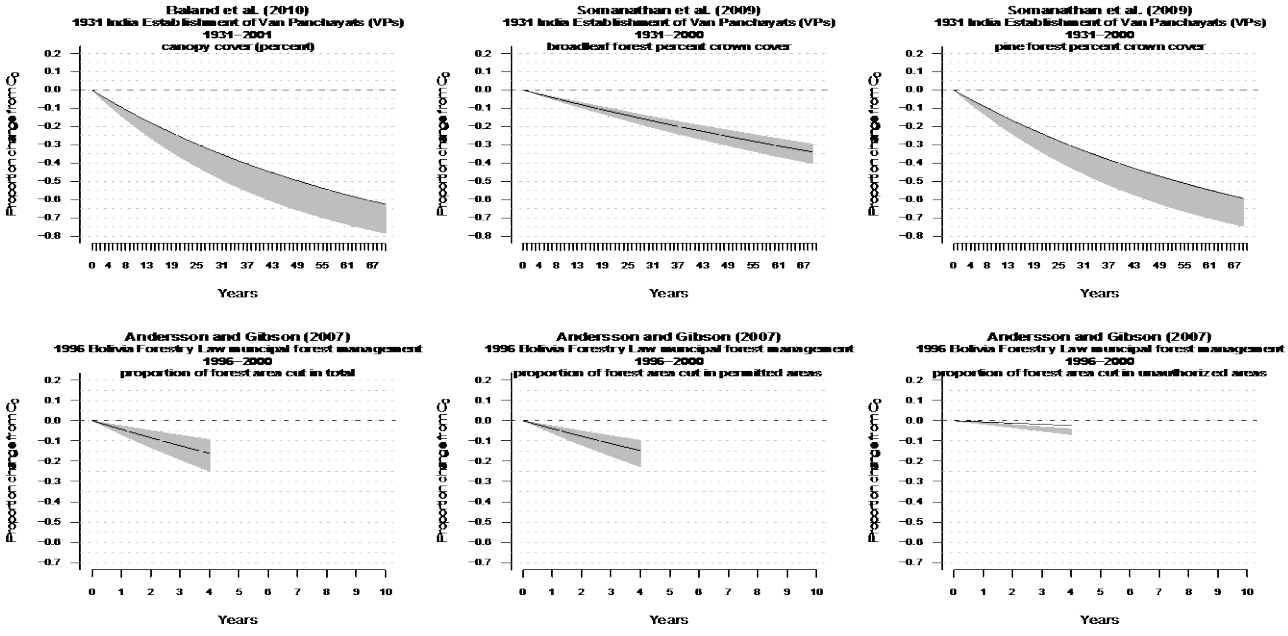
where T is the mean income level in the treatment group. A delta method approximate standard error is given by,

$$se_p(PC) = 100\ se(\Delta_r)[(T)/ (T - \Delta_r)^2].$$

Implications of Rate of Change Effects for Forest Cover Trajectories

Figure A.2 shows the implications of the rate-of-change effects for forest cover trajectories. The x axis shows years. The y axis shows proportional change in forest cover relative to the amount of forest cover that prevailed before the program was implemented (this baseline level of forest cover is denoted as C0, refer to the discussion in appendix section 10.3.2 on standardized forest cover change measures). A horizontal dashed reference line is drawn at 0 on the y axis. This reference line would correspond to no forest cover change over time. The black curves trace out the actual forest cover change trajectories in the program areas (treatment group) as reported in each of the studies. We trace out the change trajectory for the number of years that the program ran before the assessment provided by the study. Trajectories that run below the zero reference line imply forest loss; trends that run above the zero reference line imply forest gain. Each graph also displays a gray shaded area that corresponds to the 95% confidence interval for the estimated counterfactual change trajectory. That is, the gray shaded area translates the effect estimate from the study (ra – rc, in the notation from appendix section 10.3.2) into an estimate of what would have happened in the program areas had there been no program (thus, the counterfactual). If the black trajectory line overlaps with the gray shaded area, this means that the study found no statistically significant effect (at 95% confidence). When the black line does not overlap with the gray shaded area, this means that the effect of the program was statistically significant and thus the implied counterfactual trajectory is clearly distinct from what actually transpired in the program area. The titles for each graph show the study authors, the program, the observation period from which the estimates were derived, and the outcome that was used in the original analysis.

Figure A. 2: Implications of estimated effects of DFM for forest cover change. The y axis measures proportional change in forest cover relative to the amount of forest cover prior to the intervention (with this baseline forest cover denoted as C0). The black lines



Deviations from protocol

Following the terms of references provided by 3ie, our protocol proposed that we cover both DFM and payments for environmental services (PES) together in the same review. We had initially proposed, prior to drafting the original protocol, that the two programs be done separately, but that proposal was not accepted initially. However, the search, data extraction and analysis was conducted in parallel rather than integrated and once the review was completed C2 editors suggested to split the two interventions into two separate reviews for ease of interpretation.

Our protocol proposed that our risk of bias assessment code studies as “high”, “low”, or “unclear” risk of bias for each of the domains considered (exogenous assignment, control for confounding, avoidance of motivation bias, accounting for spillover, avoidance of selective outcome reporting, avoidance of analysis fishing, and appropriate statistical inference. We decided rather to indicate as to whether the study satisfied these criteria by indicating “yes”, “no”, or “unclear”, which can be interpreted as equivalent to the designations of “low”, “high”, and “unclear” risk of bias with respect to each of these domains.

Our protocol specified meta-regression analyses to test our moderator and mediator hypotheses and a set of quantitative publication bias analyses. We were unable to implement these as the number of eligible quantitative studies was too few.

The protocol also included a proposal for a set of descriptive and moderation analyses to assess external validity of our estimates. We were unable to implement these as the number of eligible studies and associated contexts was too few.

Study coding form

ITEMS/QUESTIONS LISTED IN THIS COLUMN	ENTER RESPONSES IN THIS COLUMN	INSTRUCTIONS
Coder Information		
Name of person filling in this form		*First Last*
Date that the form was begun		*MM/DD/YY*
General Study Information		
Author 1		*First Last*
Author 2 (if applicable)		*First Last*
Author 3 (if applicable) (If more than 3 authors, include only the first 3.)		*First Last*
Year of publication		*YYYY*
Intervention type		*PES or DFM*
Publication type		*Refer to the final-most publication. This is important because it determines whether the study was subject to peer review. Journal articles and book chapters should be straightforward to identify. Working papers will bear the mark of a wokring paper series. Reports will bear the mark of a commissioning or sponsoring organization. Unpublished works bearing no such series or organizational marks are considered as unpublished.*
Study Context		
Country		*Country name*
Year the intervention started		*YYYY*
Year of final outcome assessment/final data collection		*YYYY*
General Eligibility		
Does the paper report results of primary research on the impact of a PES or DFM program?		*Primary research means that it contains original data analysis, as opposed to summarizing analyses by others.*
Does the study assess impacts on at least one of the following: a. deforestation, b. material welfare in terms of poverty, consumption, or income?		*These are the outcomes of interest for our review.*
Does the study examine a developing country?		*Cf. the World Bank list of Low Income, Lower Middle Income, or Upper Middle Income countries: http://data.worldbank.org/about/country-classifications/country-and-lending-groups*
5. Does the study provide information on all of the following? a. research questions; b. data collection procedures; c. demographic or contextual characteristics of the subjects studied; d. analysis methods.		*Cf. the primary write-up of the study as well as any supporting materials (e.g., appendices, accompanying reports, etc.)*

PROCEED ONLY IF YOU ANSWERED "YES" TO ALL OF THE ABOVE.		
Eligibility for Quantitative Synthesis		
Were any of the following experimental or quasi-experimental methodologies used to assess impact of PES or DFM on deforestation or welfare (poverty, income, or consumption)? (Mark yes for each that applies.)		*These methods should be explicitly mentioned in the study write-up. If not, and if it is unclear whether any of the methods below were used, indicate "unclear", and then we can try to contact the authors.*
...randomized experiment or randomized control trial		
...regression adjustment to control for confounding variables		
...difference-in-differences methodology		
...instrumental variables methodology		
...panel/fixed-effects methodology		
...regression discontinuity methodology		
...matching or weighting methodology		
Were quantitative impact estimates reported on the following? (Mark yes for each that applies.)		*By "reported" we mean that they appear clearly in some kind of table or graph.*
...deforestation		
...income, consumption or poverty		
Is the study eligible for quantitative synthesis? (Mark yes *only if* an experimental or quasi-experimental methodology was used AND impact estimates were reported on at least one of the outcomes above.)		*The study is ineligible if you answered "no" to all of the above.*
Eligibility for Qualitative Synthesis		
Is the aim of the study clearly about the impact of PES or DFM?		
Does the study work from a theoretical framework?		*This would be to distinguish a qualitative study from, say, a journalistic account or a report that is mostly intended to advertise rather than scrutinise.*
Are original qualitative or quantitative data (e.g., quotes/paraphrasing from interviews, close observation, process tracing, etc.) used to support conclusions about impact, background conditions, or mediating factors?		
Is the study eligible for qualitative synthesis? (Mark yes *only if* you answered yes to all three of the questions above.)		

PROCEED ONLY IF THE STUDY QUALIFIES FOR QUANTITATIVE OR QUALITATIVE SYNTHESIS		
Design of the Intervention		
At what level do we have measured variation in intervention assignment or levels of exposure to the intervention? (This is the "unit of intervention" for the sake of our analysis.)		*That is, at what level do we have actual measured variation in program exposure or intensity. In some cases this is clear --- for example a household level program for which we have data from household about whether they indeed received the program. In other cases it is more subtle---for example, for programs that are the result of regional policies, the correct answer here is "region," even if they are targeted at households or communities within the region.*
PROCEED ONLY IF THE STUDY QUALIFIES FOR QUANTITATIVE SYNTHESIS		
Design of Quantitative Study (skip if study is not eligible for quantitative synthesis)		
What were the units of observation/data collection?		*That is, what are the "units" that are used in the data analysis. You can determine this by looking at the sample size. For example, if the intervention was applied in, say, 100 communites, but the data analysis uses data from 1000 households, then the unit of observation must be the household (while the unit of intervention would be the community).*
Is this a clustered study? (Mark yes if the units of observartion are nested within units of assignment---e.g., units of assignment are communities, but units of observation are households.)		*Continuing with the example above, that would be a clustered study, with the "clusters" being the communities, and an average of 10 households per cluster (10 households per community*100 communities = 1000 households).*
Does the analysis adequately account for any clustering in the design of the intervention or the study?		*That is, does the study indicate that they used "cluster robust", "multilevel", or "hiearchical" statistical methods?*
How many units of intervention were included in the study?		*In our running example, this would be the 100 communities.*
How many units of observation are included in the study?		*In our running example, this would be the 1000 households.*
How many different treatment/control conditions were studied?		*This number should always be 2 or higher. In some cases, a single study will examine different combinations of interventions. If there is only one intervention type and it is compared to a "control" condition, then you want to write "2" here.*
Describe the treatment conditions.		*Provide a concise description of the way the treatments were defined for the purposes of the statistical analysis that was performed.*
Were any significant imbalances in the baseline characteristics of the intervention recipient and non-recipient groups noted?		*Typically, a write-up will include some analysis of baseline characteristics. A properly executed randomized control trial should result in baseline characteristics being balanced over the treatment and control groups, although chance imbalances can sometimes occur. Quasi-experimental studies typically have baseline imbalances, and so it is a feature of the quasi-experimental methods to try to address the taint that this may introduce.*
If yes, for what variables was there imbalance?		
If yes, page, table, or figure number where this is reported.		
Did the study suffer from problems of unavailable data, non-response, or attrition?		
If yes, page, table, or figure number where this is discussed/reported.		
Pages, tables, or figures where baseline outcome data are presented		
Pages, tables, or figures where baseline sample characteristics are presented		

IDCG Risk of Bias Reporting (skip if study is not eligible for quantitative synthesis)		
Mechanism of assignment: was the allocation or identification mechanism able to control for selection bias?		
Group equivalence: was the method of analysis executed adequately to ensure comparability of groups throughout the study and prevent confounding?		
Hawthorne and John Henry effects: was the process of being observed causing motivation bias?		
Spill-overs: was the study adequately protected against performance bias?		
Selective outcome reporting: was the study free from outcome reporting bias?		
Selective analysis reporting: was the study free from analysis reporting bias?		
Other: was the study free from other sources of bias?		
Standard errors or confidence intervals: are appropriate standard errors or confidence intervals used?		
Quantitative Effect Estimates (skip if study is not eligible for quantitative synthesis)		
Deforestation		
Were effects reported for this outcome?		*By "reported" we mean that they either appear clearly in some kind of table or graph or they are discussed in the text with some indication of the nature of the estimated effects. For example, to save space authors will sometimes omit tables or figures for effect estimates that are statistically insigificant but they will indicate that such estimates were constructed and that nothing significant was found. We want to be sure to include these in our list of reported outcomes so as not to bias our analysis by only including "significant" effects.*
If yes…		
…what indicators were used?		*List the indicators, separating by semi-colon (e.g., "forest cover percentage; annual deforestation rate", etc.).*
…page, table, or figure number where the effects are reported:		*Provide adequate reference so that someone can locate the effect estimate easily.*
…where the effects found to be mostly positive, mostly negative, essentially zero, or mixed?		*Of course if only one indicator is used, summarize with respect to that indicator. If multiple indicators were used, try to summarize on this basis.*
…were the effects found to be mostly statistically significant, mostly insignificant, or mixed?		*Statistically significant effects are those what have a two-sided p-value of 0.10 or less. This is often indicated by "stars" or other embelishments in tables that report results. By "mixed" we mean that the study reports multiple estimates of this effect, some of which are statistically significant but at least half or most are not.*
Income, consumption, or poverty		
Were effects reported for this outcome?		
If yes…		
…what indicators were used?		
…page, table, or figure number where the effects are reported:		
…where the effects found to be mostly positive, mostly negative, essentially zero, or mixed?		
…were the effects found to be mostly statistically significant, mostly insignificant, or mixed?		

Excluded Studies

In the tables that follow, we list studies that were subject to full text search but were then excluded on the basis of substantive or methodological grounds. Studies are listed by first three authors, date of publication, and then reasons for exclusion.

DFM

Author 1	Author 2	Author 3	Publication Year	PES or DFM Intervention?	Deforestation or Poverty Outcome?	Developing Country?	Methodology Explained?	Quant. Criteria Satisfied?	Qual. Criteria Satisfied?
Agrawal, Arun			2001	Yes	No				
Agrawal, Arun	Ashwini Chhatre		2005	Yes	Yes	Yes	Yes		
Agrawal, Arun	Ashwini Chhatre		2007	Yes	Yes	Yes	Yes	No	No
Agrawal, Arun	Krishna Gupta		2005	Yes	No				
Anderson, Jon			2000	Yes	No				
Andersson, Krister			2002	Yes	Yes	Yes	Yes	No	No
Andersson, Krister			2004	Yes	Yes	Yes	Yes	No	No
Andersson, Krister	Clark C. Gibson	Fabrice Lehoucq	2006	Yes	Yes	Yes	Yes	No	No
Andersson, Krister	Elinor Ostrom		2008	Yes	Yes	Yes	Yes	No	No
Appiah, Mark			2001	No					
Arnold, J.E.			1998	Yes	No				
Assédio, Valerie	Eva Hajba	Stephen P. Mumme	2003	No					
Assies, Willem			1997	No					
Babili, H.I.	F.K. Wiersum		2010	Yes	No				
Balooni, Kulthushan	Juan M. Pulhin	Makoto Inoue	2008	Yes	Yes	Yes	No		
Balooni, Kulthushan	Makoto Inoue		2007	Yes	Yes	Yes	Yes	No	No
Banerjee, Ajit			n.d.	No					
Barr, Christopher	Eva Wollenberg	Godwin Limberg	2001	Yes	Yes	Yes	Yes	No	No
Becker, Laurence			2001	Yes	No				
Belbase, Narayan	Dhrubesh Chandra Regmi		2002	Yes	No				
Benjamin, Charles			2008	Yes	No				
Benjaminsen, Tor			1997	No					
Blair, Harry			1990	No					
Blomley, Tom	Kerstin Pfliegner	Jaconia Isango	2008	Yes	Yes	Yes	Yes	No	No
Bray, David	Camille Antinori	Juan Manuel Torres-Rojo	2006	Yes	No				
Bray, David	Leticia Merino-Perez	Deborah Barry	2005	No					
Bray, David	Peter Klepeis		2005	No					
Brooks, Jeremy	Margaret A. Franzen	Christopher M. Holmes	2006	Yes	Yes				
Brown, David			1999	Yes	Yes	Yes	Yes	No	No
Brown, H.	Steven A. Wolf	James P. Lassoie	2007	Yes	No				
Bruce, John	Anna Knox		2009	Yes	No				
Buys, Piet	Susmita Dasgupta	Uwe Deichmann	2006	No					
Capistrano, Doris			2012	No					
D'Silva, Emmanuel	B. Nagnath		2002	Yes	Yes	Yes	Yes	No	No
Dahal, Ganga			2003	Yes	Yes	Yes	No		
Dennawan, Ahmad	Heru Komarudin	Sian McGrath	n.d.	Yes	Yes	Yes	Yes	No	No
Diaw, M.C.	M.H. Assoumou	E. Dikongue	1997	Yes	No				
Domfeh, Dwame			2004	Yes	No				
Duncan, Christopher			2007	Yes	Yes	Yes	No		
Duran-Medina, Elvira	Jean-Francois Mas	Alejandro Velazquez	2005	Yes	Yes	Yes	Yes	No	No
Eghenter, Cristina			2000	Yes	No				
Eghenter, Cristina			2000	Yes	No				
EIÃ·as, Silvel	Hannah Wittman		2005	Yes	No				
EIÃ·as, Silvel	Hannah Wittman		2005	Yes	No				
Engel, Stefanie	Ramon Lopez	Charles Palmer	2006	Yes	Yes	Yes	Yes	No	No
Eloungou, Patrice			2003	Yes	Yes	Yes	No		
Ferguson, Ian	Cherukat Chandrasekharan		2005	No					
Ferguson, Ian	Cherukat Chandrasekharan		2005	No					
Fisher, Robert			n.d.	No					
Fisher, Robert			n.d.	No					
Foppes, Joost	Sounthone Kotphanh		2000	No					
Foppes, Joost	Sounthone Kotphanh		2000	No					
Fujita, Yayoi	Khamla Phanvilay		n.d.	Yes	Yes	Yes	Yes	No	No
Ghate, Rucha	Suresh Ghate		2010	Yes	Yes	Yes	Yes	No	No
Gibson, Clark	Fabrice E. Lehoucq		2003	Yes	No				
Gibson, Clark	Fabrice E. Lehoucq		2003	Yes	No				
Gregersen, Hans	Arnoldo Contreras-Hermosilla	Andy White	2005	No					
Gregersen, Hans	Arnoldo Contreras-Hermosilla	Andy White	2005	No					
Hayes, Tanya	Lauren Persha		2010	No					
Hayes, Tanya	Lauren Persha		2010	No					
Husain, Zakir	Dr. Rabindra N. Bhattacharya		2004	Yes	Yes	Yes	Yes	No	No
Irawan, S.	L. Tacconi		2009	No					
Irawan, S.	L. Tacconi		2009	No					
Iversen, Vegard	Birka Chhetry	Paul Francis	2005	Yes	Yes	Yes	Yes	No	No
Jagger, Pamela			2012	No					
Jagger, Pamela			2012	No					

Author 1	Author 2	Author 3	Publication Year	PES or DFM Intervention?	Deforestation or Poverty Outcome?	Developing Country?	Methodology Explained?	Quant. Criteria Satisfied?	Qual. Critria Satisfied?
JIANG, HONG			2006	No					
Jiang, Hong			2006	No					
Jones, Samantha			2006	Yes	Yes	Yes	Yes	No	No
Jumbe, Charles	Arild Angelsen		2006	Yes	Yes	Yes	Yes	No	No
Jumbe, Charles	Arild Angelsen		2006	No					
Jumbe, Charles	Arild Angelsen		2007	No					
Kaimowitz, David	Cristian Vallejos	Pablo B. Pacheco	1998	Yes	No				
Kaimowitz, David	Pablo Pacheco	James Johnson	1999	Yes	No				
Kajembe, G.C.	V.B.M.S. Kihiyo	A.Y. Banana	2000	Yes	No				
Kajembe, G.C.	V.B.M.S. Kihiyo	A.Y. Banana	2000	Yes	No				
Kassa, Habtemariam	Bruce Campbell	Mats Sandewall	2008	Yes	Yes	Yes	Yes	No	No
Kato, Gaku			2005	Yes	Unclear				
Kato, Gaku			2005	Yes	Unclear				
Kaunedois, Derek	Krister Andersson		2006	Yes	Yes	Yes	Yes	No	No
Kihiyo, Vincent			1998	No					
Kihiyo, Vincent			1998	No					
Klooster, Daniel			2000	No					
Klooster, Daniel			2000	No					
Klooster, Daniel	Omar Masera		2000	Unclear					
Klooster, Daniel	Omar Masera		2000	Unclear					
Kunce, Mitch	Jason F. Shogren		2005	No					
Kunce, Mitch	Jason F. Shogren		2005	No					
Lachapelle, Paul	Patrick D. Smith	Stephen F. McCool	2004	Yes	No				
Lachapelle, Paul	Patrick D. Smith	Stephen F. McCool	2004	Yes	No				
Lane, Marcus			2003	No					
Lane, Marcus			2003	No					
Lane, Marcus	G.T. McDonald	T.H. Morrison	2004	No					
Lane, Marcus	G.T. McDonald	T.H. Morrison	2004	No					
Lane, Marcus	Liana J. Williams		2008	Yes	No				
Lane, Marcus	Liana J. Williams		2008	Yes	No				
Larson, Anne			2002	Yes	No				
Larson, Anne			2002	Yes	No				
Larson, Anne			2003	No					
Larson, Anne			2003	No					
Larson, Anne			2003	No					
Larson, Anne			2003	No					
Larson, Anne			2004	No					
Larson, Anne			2005	No					
Larson, Anne			2010	No					
Larson, Anne			2010	No					
Larson, Anne	Pablo Pacheco	Fabiano Toni	2007	Yes	Yes	Yes	No		
Larson, Anne	Pablo Pacheco	Fabiano Toni	2007	Yes	Yes	Yes	No		
Lin-Long, Chun	Yilan Zhou		2000	Yes	No				
Long, Chun-Lin	Yilan Zhou		2000	Yes	No				
Lund, Jens	Thorstin True		2008	Yes	Yes	Yes	Yes		
Lybecker, Donna	Stephen P. Mumme		2002	Yes	No				
Lybecker, Donna	Stephen P. Mumme		2002	Yes	No				
Manasan, Rosario			2002	Yes	No				
Matose, Frank			2006	Yes	No				
Matose, Frank			2006	Yes	No				
Matose, Frank	Scolney Watts		2010	Yes	Yes	Yes	Yes		
McCarthy, John			2001	Yes	Yes	Yes	No		
McCarthy, John			2001	Yes	Yes	Yes	No		
McConnell, William	Sean P. Sweeney		2005	Yes	No				
Mercado, Elmer			2000	Yes	No				
Meshack, Charles	Bhim Ahdikari	Nike Doggart	2006	Yes	Yes	Yes	Yes		
Monshita, Akiko			2008	No					
Muhammed, Nur	Masao Koike	Farhana Haque	2008	Yes	No				
Mustafa, Usman			2007	No					
Mvondo, Samuel			2006	Yes	Yes	Yes	Yes	No	No
Nagendra, Harini			2002	Yes	Yes	Yes	Yes		
Nagendra, Harini	Mukunda Karmacharya	Birendra Karna	2005	Yes	Yes	Yes	Yes		
Nayak, Prateep	Filoret Berkes		2008	Yes	No				
Nguyen, Tan			2012	Yes	Yes	Yes	Yes	No	No
Nomura, Ko			2010	Yes	No				
Nsita, Steve			2005	Yes	No				

Author 1	Author 2	Author 3	Publication Year	PES or DFM Intervention?	Deforestation or Poverty Outcome?	Developing Country?	Methodology Explained?	Quant. Criteria Satisfied?	Qual. Criteria Satisfied?
Nyce, Christopher			2004	Yes	No				
Nygren, Anja			2005	No					
Obidzinski, Krystof	Christopher Barr		2003	Yes	Yes	Yes	Yes	No	No
Otua, J.	A.Y. Banana	N. Turyahabwe	1998	No					
Oliveira, Jose			2002	Yes	No				
Oliveira, Jose			n.d.	Yes	No				
Oyono, Phil			2004	Yes	No				
Oyono, Phil			2004	No					
Oyono, Phil			2004	Yes	No				
Oyono, Phil			2004	Yes	Unclear				
Oyono, Phil			2004	Yes	No				
Oyono, Phil			2005	Yes	Yes	Yes	Yes	No	No
Oyono, Phil	Jesse C. Ribot	Samuel Assembe	2007	No					
Pacheco, Pablo			2004	Yes	Unclear				
Pacheco, Pablo			2005	Yes	Unclear				
Palmer, Charles	Stefanie Engel		2007	Yes	Yes	Yes	Yes	No	No
Persha, Lauren	Tom Blomley		2009	Yes	Yes	Yes	Yes	No	No
Poffenberger, Mark			1993	Yes	Unclear				
Pragtong, Komon			2000	Yes	No				
Prasad, Ram	Shashi Kant		2002	Yes	Yes	Yes	No		
Pulhin, J.M.	M. Inoue	T. Enters	2007	Yes	Yes	Yes	No		
Ray, Biswajit	Rabindra N. Bhattacharya		2011	Yes	No				
Rhee, Steve			2000	Yes	No				
Ribot, Jesse			2009	Yes	Yes	Yes	Yes	No	No
Ribot, Jesse			2012	No					
Ribot, Jesse	J.F. Lund	T. Treue	2010	No					
Rival, Laura			2003	Yes	Yes	Yes	Yes	No	No
Rosyadi, Slamet	Regina Birner	Manfred Zeller	2003	Yes	No				
Sekhar, Nagothu			2000	Yes	No				
Shyamsundar, Priya	Racha Ghate		2011	No					
Sikor, Thomas	Tran Ngoc Thanh		2002	Yes	No				
Sikwese, Margaret			n.d.	No					
Sisawnto, W.	W. Wardojo	C.J.P. Colfer	2005	Unclear					
Southwold-Llewellyn, Sarah			2006	Yes	No				
Sudana, Made			2004	Yes	No				
Tewari, D.D.	A.G. Isemonger		1998	Yes	Unclear				
Toni, Fabiano			2011	Yes	No				
Tuan, Hoang			2006	Yes	No				
Veron, Rene	Garry Fehr		2011	Yes	No				
Vien, Tran	Nguyen Vinh Quang	To Xuan Phuc	2006	Yes	Yes	Yes	Yes	No	No
Wardell, D.	Christian Lund		2006	Yes	No				
Wiggins, Steve	Kofi Marfo	Vincent Anchirinah	2004	Yes	Yes	Yes	Yes	No	No
Wily, Liz			2002	No					
Wilman, Hannah	Charles Geisler		2005	Yes	No				
Xu, Jianchu	Jesse C. Ribot		2004	Yes	No				
Yasmi, Y.			2003	Yes	No				
Yasmi, Y.	C.Z. Anshari	H. Komarudin	n.d.	Yes	No				
Yasmi, Y.	J. Guernier	C.J.P. Colfer	2009	Yes	No				
Ylhaisi, Jussi			2003	No					
Zulu, Leo			2009	Unclear					

Detailed risk of bias assessments

DFM

Program	Studies	Treatment Group	Method of attribute-ion	Research design works with a source of exogenous assignment	Analysis controls for potential confounding due to land quality, socio-economic conditions, and accessibility	Measurement strategy avoids motivation bias	Study design accounts for spillover	Apparently free of selective outcome reporting	Apparently free of analysis fishing	Appropriate statistical inference (standard errors)
1931 India Establish-ment of Van Panchayats and 1976 Encourage-ment to Form Van Panchayats	Baland et al. (2010)	1931-2001 forest areas under VP manage-ment	multiple regres-sion	No	No	Yes	No	Yes	Yes	Yes
1931 India Establish-ment of Van Panchayats (VPs)	Somanathan et al. (2009)	1931-2000 forest areas under VP manage-ment	adjacent area disconti-nuity and matching	Yes	Unclear	Yes	No	Yes	Yes	Yes
1996 Bolivia Forestry Law municipal forest manage-ment	Anderson and Gibson (2007)	1996-2000 forest areas in Santa Cruz with higher levels of municipal forest gover-nance index	multiple regres-sion	No	Yes	Yes	No	Yes	Yes	Yes
1996 Malawi World Bank and DFID sponsored forest comanage-ment program, operational-izing 1997 Forestry	Blessings et al. (2006)	1996-2002 households participat-ing in forest manage-ment commit-tees	matching and selection modeling	No	Unclear	Yes	No	Yes	Yes	Unclear

Act										
1993 Nepal Forestry Law and Establish-ment of Forest User Groups	Edmonds (2002)	1993-1995 house-holds in communi-ties with forest users groups	multiple regres-sion, matching, and instru-mental variables	Yes	Yes	Yes	No	Yes	Yes	Yes
2001 Ethiopia Farm Africa/SOS-Sahel Participatory Forest Management Program	Gelo and Koch (2012)	2001-2009 house-holds residing in PFM communit-ies	matching	No	No	Yes	No	Yes	Yes	Unclear
2003 Uganda Establish-ment of District Forestry Service	Jagger (2008)	2003-2006 household near forest areas under DFS manage-ment	difference-in-difference and multiple regres-sion	No	Yes	Yes	No	Yes	Yes	Unclear
2005 Kenya Forest Act and Devolution of Management to Community Forest Associations	Ogada (2013)	2005-2010 households participat-ing in community forest associat-ions on common matching support	matching	No	No	Unclear	No	Yes	Yes	Unclear

Tables with all component effects used to construct mean effects

Deforestation effects

Program	Studies	Treatment Group	Time Period	Counterfactual Approximation	Forest Conservation Outcome	Forest Conservation Effect (standard error in parentheses)	ra-rc (standard error in parentheses)	ra
1931 India Establishment of Van Panchayats (VPs)	Baland et al. (2010)	1931-2001 forest areas in Uttaranchal under VP management	1931-2001	regression adjusted forest areas under state protection	canopy cover (percent)	9.35 (4.3)	0.004 (0.002)	-0.014
					basal area (m-sq./ha)	2.47 (5.2)		
					basal volume (m-cu./ha)	194.33 (141.72)		
					lopping (percent)	-18.26 (4.56)		
					regeneration (saplings>.5cm/ha)	43.89 (116.12)		
					firewood collection time (hours)	-0.25 (0.3)		
1931 India Establishment of Van Panchayats (VPs)	Somanathan et al. (2009)	1931-2000 forest areas in Uttaranchal under VP management	1931-2000	*(mean of estimates)*	*broadleaf forest percent crown cover*	*1.15 (2.68)*	*0.0003 (0.001)*	*-0.006*
				(mean of estimates)	*pine forest percent crown cover*	*8.35 (3.95)*	*0.0034 (0.002)*	*-0.013*
				adjacent forest areas under state protection	broadleaf forest percent crown cover	1.20 (2.80)	0.0003 (0.001)	-0.006
					pine forest percent crown cover	-2.40 (3.60)	-0.001 (0.001)	-0.013
				nearest neighbor matched forest areas under state protection	broadleaf forest percent crown cover	1.80 (3.00)	0.0004 (0.001)	-0.006
					pine forest percent crown cover	14.60 (4.70)	0.006 (0.002)	-0.013
				radius matched forest areas under state protection	broadleaf forest percent crown cover	0.50 (2.70)	0.0001 (0.001)	-0.006

					pine forest percent crown cover	12.00	(4.00)	0.005	(0.002)	-0.013
				kernel matched forest areas under state protection	broadleaf forest percent crown cover	1.10	(2.20)	0.0002	(0)	-0.006
					pine forest percent crown cover	9.20	(3.50)	0.004	(0.002)	-0.013
1976 India Beginning of Encouragement of Formation of Van Panchayats	Baland et al. (2010)	1976-2001 forest areas under VP management	1976-2001	regression adjusted forest areas under state protection	canopy cover (percent)	0.06	(2.86)			
					basal area (m-sq./ha)	-12.56	(5.68)			
					basal volume (m-cu./ha)	-288.75	(227.27)			
					lopping (percent)	-6.7	(3.55)			
					regeneration (saplings>.5cm/ha)	-8.34	(66.83)			
					firewood collection time (hours)	-0.12	(0.23)			
1996 Bolivia Forestry Law muncipal forest management	Anderssson and Gibson (2007)	1996-2000 forest areas in Santa Cruz with higher levels of municipal forest governance index	1996-2000	regression adjusted forest areas with lower levels of municipal forest governance index	proportion of forest area cut in total	-0.01	(0.04)	0.004	(0.012)	-0.044
					proportion of forest area cut in permitted areas	-0.02	(0.03)	0.005	(0.01)	-0.040
					proportion of forest area cut in unauthorized areas	-0.03	(0.01)	0.008	(0.002)	-0.006
1993 Nepal Forestry Law and Establishment of Forest User Groups	Edmonds (2002)	1993-1995 households in communities with forest users groups	1993-1995	regression adjusted households in communities without forest users groups	percentage change in baskets of firewood collected per month	-11.26	(2.73)			
				kernel matched households in	percentage change in baskets of	-10.02	(6.54)			

				communities without forest users groups	firewood collected per month			
				households in communities that were next in line to receive forest users groups	percentage change in baskets of firewood collected per month	-27.15	(6.27)	
				households in communities without forest users groups but that would be responsive to an instrumental variable measuring accessibility to forest commission staff	percentage change in baskets of firewood collected per month	-33.52	(9.12)	
2005 Kenya Forest Act and Devolution of Management to Community Forest Associations	Ogada (2013)	2005-2010 households participating in community forest associations on common matching support	2005-2010	nearest neighbor propensity score matched non-CFA households	acerage under tree cultivation	0.43	(0.10)	
				kernel matched non-CFA households	acerage under tree cultivation	0.43	(0.10)	

Income and poverty effects

Program	Studies	Treatment Group	Time Period	Counterfactual Approximation	Welfare Outcome	Welfare Effect (standard error in parentheses)		Percentage Effect (standard error in parentheses)	
1996 Malawi World Bank and DFID sponsored forest comanagement program, operationalizing 1997 Forestry Act	Blessings et al. (2006)	1996-2002 households participating in forest management committees	1996-2002	*(mean of estimates)*	*monthly forest income in Kwacha*	*10.56*	*(3.99)*	*26%*	*(11.50)*
				nearest neighbor matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	19.63	(4.61)	52%	(18.48)
				radius matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	0.986	(4.24)	2%	(7.63)
				kernel matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	18.34	(3.75)	47%	(14.04)
				stratification matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	3.29	(3.37)	5%	(5.84)
		(low-income households)		*(mean of estimates)*	*monthly forest income in Kwacha*	*19.55*	*(3.05)*	*66%*	*(19.55)*
		(low-income households)		nearest neighbor matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	25.62	(2.98)	98%	(22.54)

		(low-income households)		radius matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	14.04	(2.66)	37%	(9.66)
		(low-income households)		kernel matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	25.01	(3.45)	93%	(24.92)
		(low-income households)		stratification matched and selection-model adjusted non-participating households in the same area	monthly forest income in Kwacha	13.52	(3.12)	35%	(11.06)
2001 Ethiopia Farm Africa/SOS-Sahel Participatory Forest Management Program	Gelo and Koch (2012)	2001-2009 households residing in PFM communities	2001-2009	*(mean of estimates)*	*Per capita consumption expenditure in Ethiopian Birr.*	*434.43*	*(136.41)*	*35%*	*(9.44)*
				nearest neighbor (2) matched households from non-PFM communities	Per capita consumption expenditure in Ethiopian Birr.	295.68	(111.87)	21%	(7.38)
				nearest neighbor (3) matched households from non-PFM communities	Per capita consumption expenditure in Ethiopian Birr.	336.73	(101.53)	24%	(6.61)
				nearest neighbor (4) matched households from non-PFM communities	Per capita consumption expenditure in Ethiopian Birr.	327.62	(105.3)	23%	(6.89)
				nearest neighbor (5) matched households from non-PFM communities	Per capita consumption expenditure in Ethiopian Birr.	319.95	(101.91)	23%	(6.64)

				kernel matched households from non-PFM communities	Per capita consumption expenditure in Ethiopian Birr.	548.53	(148.61)	46%	(10.27)
				Parametric-regression adkisted households from non-PFM communities responsive to presence of Menja people as an instrumental variable	Per capita consumption expenditure in Ethiopian Birr.	645.16	(210.61)	59%	(15.76)
				Non-parametrically adjusted households from non-PFM communities responsive to presence of Menja people as an instrumental variable	Per capita consumption expenditure in Ethiopian Birr.	567.33	(175.01)	49%	(12.5)
2003 Uganda Establishment of District Forestry Service	Jagger (2008)	2003-2006 household near forest areas under DFS management	2003-2006	Difference-in-difference and Tobit adjusted households near forests under national (Uganda Wildlife Authority) management	Annual forest income per capita in Ugandan Shillings	9838	(14684)	2%	(2.36)
		(lowest income quartile)				-17469	-26073	-6%	(8.43)
					Percent of income from forest	3.06	(4.57)		
	Jagger (2008)	2003-2006 household near forest areas under NFA management	2003-2006	Difference-in-difference and Tobit adjusted households near forests under national (Uganda Wildlife Authority) management	Annual forest income per capita in Ugandan Shillings	95972	(37256)	16%	(6.12)
		(lowest income quartile)				-27753	-14160	-10%	(5.12)
					Share of income from forest	6.37	(3.25)		

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