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Improving the estimation of local welfare costs of conservation in low-income countries using choice experiments: Empirical evidence from Madagascar

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"IMPROVING THE ESTIMATION OF LOCAL WELFARE COSTS OF CONSERVATION IN LOW-INCOME COUNTRIES USING CHOICE EXPERIMENTS: EMPIRICAL EVIDENCE FROM MADAGASCAR"

A thesis for a joint degree of Doctor of Philosophy

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"The only thing I know is that I don't know enough"

Paraphrasing Socrates

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SUMMARY

Discrete choice experiments (DCEs) are increasingly used for ex-ante evaluations of environmental policies but their validity and reliability are largely untested in low-income settings. My thesis examines whether DCEs provide valid and reliable estimates of welfare impacts in these contexts and comprises a systematic review and three field tests of DCE in a new REDD+ (Reducing Emissions from Deforestation and forest Degradation) project and national park in eastern Madagascar. I first conducted a systematic review of empirical evidence on the reliability and validity of DCEs when valuing non-market environmental goods from the past 13 years. I found limited and mixed evidence and mostly recommend the use of more qualitative, interdisciplinary and deliberative approaches. The review informed the design of my empirical DCE survey, which elicited local people's preferences for the right to clear forests for swidden agriculture, cash payments, and support for improved rice farming. I used this survey and rigorous qualitative debriefings to investigate three aspects of reliability and validity of DCEs. I first surveyed households who have varying experience of restrictions on swidden agriculture to explore the validity of DCE in estimating the costs of conservation restrictions ex-ante. I found that experience of forest protection matters; households who have been exposed to forest protection for a comparatively longer period had significantly higher welfare costs for restricting forest clearance than those who are less experienced. I conclude that although DCE can elicit current preferences in my study context, DCE is not a valid ex-ante tool for estimating compensations for such a long-term and complex intervention. I then used a within-subject design to evaluate whether giving respondents more time to deliberate influences DCE responses. I found that deliberation significantly affected individual-level preference parameters and marginal willingness-to-accept (WTA) estimates, but the effect depended on their literacy. Illiterate respondents' WTA significantly increased post-deliberation while literate respondents' decreased. Although respondents' decisions to revise their choices mostly stemmed from more reflection, I also found evidence of strategic behaviour among more literate respondents. Greater time to deliberate may improve welfare estimates with illiterate respondents, but may have risks when used with others. Finally, I compared responses to the WTA and willingness-to-pay (WTP) DCE formats. While it is well recognised that the choice of format can influence welfare estimates, I showed that it also influenced which attributes are significant. The WTA format is more appropriate in my study context based on three criteria; its content validity, its acceptability to respondents, and respondents' ability to pay. I also found that local people were generally averse to state protection and strongly demand secure forest tenure. My thesis makes an important methodological contribution to the advancement of DCE techniques. It also has major implications for how forest conservation policy may be devised in low-income countries, including devolution of secure forestland tenure to local people and genuinely negotiating conservation with forest users.

RESUMÉ (DANISH SUMMARY)

Valgeksperimenter (DCE) er en metode, som i stigende grad benyttes til ex ante vurdering af miljøpolitikker, men deres validitet og pålidelighed er stort set ikke undersøgt i lav-indkomst lande. Min afhandling undersøger hvorvidt DCE giver valide og pålidelige estimater for velfærdskonsekvenser i disse sammenhænge. Den består af et systematisk overblik og tre felteksperimenter af DCE i et nyt REDD+ (Reducing Emissions from Deforestation and forest Degradation) projekt, som også er en nationalpark, i det østlige Madagascar. Først lavede jeg et systematisk studie af hvilken empirisk evidens der er for DCEs pålidelighed og validitet til brug for værdisætning af ikke markedsomsatte goder de sidste 13 år. Jeg fandt begrænset og flertydig evidens herfor og anbefaler derfor brugen af mere kvalitative interdisciplinære og deliberative tilgange. Jeg brugte erfaringerne fra dette studie til at designe min empiriske DCE undersøgelse som søgte at afklare folks præferencer for retten til at rydde skov for landbrugsjord, monetære betalinger og støtte til forbedret risdyrkning. Jeg brugte dette studie og grundige kvalitative debriefings til at undersøge tre aspekter af DCEs pålidelighed og validitet. Først undersøgte jeg husholdninger som har forskellig grad af restriktioner på landbrugsjord til at undersøge validiteten af DCE estimater for omkostninger ved beskyttelsesrestriktioner. Jeg fandt at erfaringer med skovbeskyttelse har betydning for estimaterne; husholdninger som har erfaring med skovbeskyttelse i en relativt længere periode havde signifikant højere velfærdsomkostninger for restriktioner i muligheden for at rydde skov end husholdninger som havde færre erfaringer. Jeg konkluderer, at selvom DCE kan afsløre nuværende præferencer i den undersøgte sammenhæng, så er DCE ikke et validt ex ante redskab til at estimere kompensationsstørrelser for så langtrækkende og komplekse tiltag. Dernæst brugte jeg et indenfor-individ design til at evaluere hvorvidt det at give respondenter tid til at tænke efter påvirker DCE-svarene. Jeg fandt at denne tænke-efter-tid påvirkede præferenceparametrene på individniveau og willingness-to-accept (WTA) estimaterne signifikant, men påvirkningen var afhængig af læsefærdigheder. Analfabetiske respondenters WTA var signifikant større efter tænke-efter-tid end før, mens det modsatte var tilfældet for de der kunne læse. Selvom respondenternes beslutning om at ændre deres valg i vid udstrækning stammede fra dybere refleksioner, så fandt jeg også evidens for strategisk adfærd blandt de mere der kunne læse. Følgelig konkluderer jeg at tænke-efter-tid kan forbedre velfærdsestimater med analfabetiske respondenter, men det kan være problematisk når det bruges hos andre. Til sidst sammenlignede jeg WTA og willingness-to-pay (WTP). Det er en etableret viden at valgformatet påvirker størrelsen på velfærdsestimater, så det jeg viser er at det også kan påvirke hvilke attributter der er signifikante. WTA formatet er mere korrekt i mit studies kontekst ifølge en række kriterier. Jeg finder også at lokale folk generelt er imod statslig beskyttelse og har en stor efterspørgsel på sikring af rettigheder til skov. Min afhandling er et vigtigt metodisk bidrag til DCE og har stor betydning for miljøpolitikker i lav-indkomst-lande.

ABBREVIATIONS

AIC	Akaike's information criterion
CVM	Contingent valuation method
CAZ	Corridor of Ankeniheny-Zahamena
DCE	Discrete choice experiment
HIC	High-income country
LCM	Latent Class Model
LIC	Low-income country
LR	Likelihood ratio
MWTA	Marginal willingness to accept
MWTP	Marginal willingness to pay
PA	Protected area
PES	Payments for Ecosystem Services
REDD+	Reducing Emissions from forest Degradation and Deforestation
RPD	Random parameter difference
RPL	Random Parameter Model
SP	Stated preference
SR	Systematic review
WTA	Willingness to accept
WTP	Willingness to pay

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Chapter 1. INTRODUCTION

1.1 Research background and rationale

1.1.1 Forest conservation in developing countries

Although the poverty-biodiversity relationship is complex and differs from case to case, there is mounting evidence that areas identified as 'biodiversity hotspots' occur mostly in regions with severe poverty (Fisher and Christopher 2007). Low incomes also tend to mean low monetary opportunity costs of forest conservation. Thus conservation in low-income countries (LICs) appears to have higher cost effectiveness (Balmford et al. 2002) and conservationists have used this observation to justify focusing international conservation attention on biodiversity in LICs. However, this apparent high cost effectiveness does not take into account the huge disparities of income between wealthy countries, which benefit from conservation, and the countries that bear the opportunity costs (Balmford and Whitten 2003; Bawa et al. 2004). Low incomes in biodiversity rich countries mean that costs represent a higher percentage of local incomes (Angelsen et al. 2014) and a more serious welfare impact. When costs and benefits are weighted according to the relative incomes of the stakeholder groups, the net present value of forest conservation may become strongly negative (Hockley and Razafindralambo 2006).

In addition to the high cost-effectiveness case for forest conservation in LICs, rapid habitat loss and extinction in biodiversity hotspots have heightened the need to protect forests in LICs (Brooks et al. 2002; Butchart et al. 2010). To preserve biodiversity, governments often set aside natural forestlands in protected areas (PAs). The number and extent of PAs have increased rapidly in the last decades (Jenkins and Joppa 2009), particularly in LICs. Forestlands gazetted in PAs are often the customary lands of local communities, whose livelihoods are deeply entwined with forest use and clearance (Sunderlin et al. 2005). Whether, or to what extent, PAs harm local people is a controversial topic and the empirical evidence is weak and sparse. Nevertheless, the establishment of PAs restricts access and use of forest resources and therefore may threaten human rights and social welfare if compensation mechanisms are ineffective, and many examples in the literature support such claims (e.g. Brockington 2003; McShane 2003; West et al. 2006). However, impacts will vary from case to case, and there is evidence to the contrary (e.g. Andam et al. 2010; Foerster et al. 2011). In addition to welfare effects, it is likely that local resentments caused by displacement or restrictions pose major obstacles to conservation (Agrawal and Redford 2009). Importantly, social welfare losses due to delayed or

incomplete compensations may be as irreversible as species extinctions (Harper 2002), i.e. irreversibility is not a purely environmental phenomenon.

PAs have attempted to use rural development projects to compensate local costs as part of the Integrated Conservation and Development Projects (ICDPs) paradigm but such projects have a high failure rate (Brandon and Wells 1992; McShane and Newby 2004). Alternatively, some conservation actions have been pursued through community-based natural resource management and related concepts, sometimes successfully (e.g. Treue and Lund 2008). Nonetheless, in many cases, the rights of communities over resources have been tightly constrained; and community-based management has not been able to compensate communities for their opportunity costs (Dressler et al. 2010). More recently, Payments for Ecosystem Services (PES) schemes have emerged as innovative conservation tools by providing incentives to local communities whose activities would otherwise threaten the provision of ecosystem services; thus increasing the acceptability of conservation policies (Ferraro 2001). However, most emerging evidence on the evaluation of PES to date are from countries where the institutional framework is well defined and where land is individually owned (e.g. Muñoz-Piña et al. 2008; Pagiola 2008). PES is yet to succeed where institutions are weak and property rights unclear or contested. Indeed, preliminary results suggest that PES is not providing a better outcome for local people than previous approaches and that compliance is mostly obtained by coercion (Milne and Adams 2012). Recently, the Reducing Emissions from Deforestation and forest Degradation (REDD+) concept, described as the world's largest PES by Corbera (2012), has received substantial donor support and interest in LICs. REDD+ projects could potentially finance the expansion of PAs and the enforcement of strict restrictions (Brandon and Wells 2009; Harvey et al. 2010). However, the effectiveness of REDD+ in adequately compensating local people has also been questioned as REDD+ payments are not likely to be based on opportunity costs but on a price per unit of carbon or other services (such as biodiversity benefits) (Busch 2013). Since REDD+ schemes are involuntary at the local level, there is no reason to expect payments to cover opportunity costs. Moreover, REDD+ social safeguards are vulnerable to elite capture and fail to compensate those who are most harmed by conservation actions (e.g. Poudyal et al. 2016).

1.1.2 Estimating local welfare costs of forest conservation in least developed countries: Issues and challenges with conventional valuation techniques

Restricting access to forests or displacing people without adequate compensations can delegitimise conservation actions (Brockington et al. 2006). While the local costs of conservation and the need to compensate them are being increasingly recognised; the few studies estimating the opportunity costs

of forest conservation have faced the considerable challenges of applying conventional valuation techniques to LIC contexts. Conventional economic analyses such as household production functions or market price methods (e.g. Norton-Griffiths and Southey 1995; Kremen et al. 2000; Boerner et al. 2009; Fisher et al. 2011) are problematic in the context of LICs where formal property and use rights are rarely aligned with customary rights, factor markets are undeveloped, and domestic production and non-cash transactions predominate. Thus, land values (or shadow values of lands) are extremely difficult to estimate in a LIC context (particularly when forestlands are formally state-owned) and studies relying exclusively on land values may be unreliable (e.g. Chomitz et al. 2005; Naidoo and Adamowicz 2006). Furthermore, studies measuring livelihoods and environmental dependence are often based on rapid standardized questionnaire surveys with relatively large sample sizes (e.g. Pouliot and Treue 2013; Babigumira et al. 2014): seldom are follow-up debriefings with respondents used to compare or cross validate findings.

The net welfare costs of forest protection can be defined as the value of the foregone activity to a farmer (swidden agriculture on newly cleared land), minus the benefits he will receive from whatever he did instead (including ecological services from intact forest). Assessing these net costs is therefore challenging because one has to project what would have happened if a conservation policy had not been implemented and/or what will happen if protection is strictly enforced. This is problematic and requires a good understanding of individual's behaviour in alternative scenarios. Stated preference techniques can offer one approach to dealing with the problems of making projections, through the construction of hypothetical scenarios (Freeman 2003). Discrete choice experiments (DCE) and the contingent valuation method (CVM) are the two most commonly used stated preference techniques, they allow analysts to generate welfare estimates of environmental benefits or damages based on respondents' stated willingness-to-pay (WTP) or willingness-to-accept (WTA) estimates[1]. The main difference between DCE and CVM is the way that respondents' stated values are inferred from their responses. In contingent valuation, these values are stated directly while in DCEs, analysts infer these values from the choices or trade-offs that respondents make.

While CVM may be better suited to policy scenarios which cannot be easily decomposed into their characteristics (Colombo et al. 2005), DCEs allow policy makers to value different attributes of a policy, and to explore different types of compensatory mechanisms (e.g. cash vs agricultural extension

[1] Hanley and Barbier (2009) and Freeman (2003) provide an introduction to the use of stated preference techniques in environmental valuation.

support) (Hanley et al. 2001). By inferring policy impacts from the trade-offs that respondents make, DCE also avoids asking direct questions[2] about the policy being valued and therefore may be useful when valuing sensitive goods, such as illegal activities (Moro et al. 2013). Although DCEs are increasingly used in environmental valuation, they are likely to suffer from a number of limitations that affect CVM (Hanley and Barbier 2009). Even in developed countries where the majority of DCE studies have been undertaken, their validity and reliability are much disputed (Bateman et al. 2002; Freeman 2003). The criticisms stem mostly from the hypothetical nature of DCEs which, although offering greater freedom in the construction of the scenario, may lead to the well-documented "hypothetical bias" (Loomis 2011).

1.1.3 Defining and measuring welfare changes: Basic theory

Welfare economics uses people's preferences to understand how policies affect an individual's welfare and assumes that individuals are the best judge of their welfare. People thus express their preferences through the choices and trade-offs that they make; if an individual prefers bundle A over bundle B, bundle A therefore provides a higher level of welfare than bundle B. Instead of taking a substantive position about what is good for people, welfare economics simply infers these relative values from what people reveal as their preferences.

The property of "substitutability" is at the core of the economists' concept of welfare (Freeman 2003). Substitutability means that if the quantity of one element in an individual's bundle is reduced, it is possible to increase the quantity of some other element so as to leave the individual no worse off because of the change. Substitutability therefore establishes trade-off ratios between pairs of good that affect an individual's welfare. Most of the tools used by neoclassical economists to model individual's welfare and predict individual behaviour in and outside markets rest on the assumption of substitutability. In order to use the same standard of measurement for all individuals to measure trade-off ratios, the standard needs to be a good from which everyone derives positive utility[3], i.e. something that everyone prefers to have more of rather than less. In economics, the standard of

[2] The sensitive good or policy can be hidden within a pool of available goods, in contrast to its central role in contingent valuation

[3] Utility is a modelling construct that refers to the satisfaction that an individual gets from the consumption of a good, it cannot be directly measured from observations of behaviour but can only be inferred by observing individuals' preferences over potential choices (Freeman 2003. Welfare economics, a choice-based concept, builds on the utilitarian approach to understand how policies affect an individual's welfare.

measurement is often money as it finely divisible and is a particularly effective substitute good (Bateman et al. 2002). However, monetary prices may not always be available, in which case the trade-off ratios can sometimes simply be interpreted as expressions of economic values. For example, Moro et al. (2013) have estimated the trade-off ratios that individuals make between illegal hunting and alternative livelihoods.

Values based on substitutability can be measured in terms of willingness to pay (WTP) or willingness to accept (WTA), which measure the amount of other goods that an individual is willing to substitute for the good being valued. Measures of welfare loss for changes in the quantities of imposed goods (i.e. unalterable quantities or qualities) are the compensating and equivalent surplus measures[4]. In measuring welfare loss from a new environmental regulation that decreases respondents' utility (e.g. forest use restrictions in this thesis), compensating surplus asks what compensating payment would make the individual indifferent between the original situation and the new policy. The measure CS is interpreted as the minimum payment that an individual would require (i.e. WTA) to induce that individual to voluntarily accept the new environmental regulation. Conversely, equivalent surplus (ES) asks what reduction in income would lead to the same utility change as the new regulation. The ES measure has also been described as the maximum amount that an individual would be willing to pay (WTP) to avoid welfare loss.

Neoclassical welfare economics makes some further assumptions about welfare; individuals seek to maximise their benefit and minimise their cost and individuals' preferences are rational, i.e. they are both complete and coherent (Bateman et al. 2002). By 'complete', individuals should be able to express a preference for any good or say they are indifferent between any pair of goods. By "coherent", preferences that are elicited for any person must be internally consistent as viewed in the light of some properties of preferences[5]. However, some critics of economic valuation find its assumptions too restrictive and question whether all behaviour can be translated into measures of rational preferences, that is, it might be unrealistic to assume that rational choice theory always provides a sufficient description of actual behaviour (Herrnstein 1990). While some scholars claim that irrational preferences may be attributable to random errors or flaws in the survey design (e.g. Carson

[4] Compensating and equivalent variation are closely related to the compensating and equivalent surplus, respondents cannot adjust the consumption of a given environmental good (or consumption bundles) in the latter as is generally the case with public goods (Freeman 2003).

[5] For example, transitivity imposes a 'consistency' requirement by enabling a ranking or ordinal mapping onto the options that an individual faces. For example, if a farmer strictly prefers rice to cassava and prefers cassava to corn, the transitivity assumption requires him/her to also prefer rice to corn.

et al. 2001), others argue that researchers may need to modify the assumptions underlying welfare economics or resort to accepting the possibility that for some public and/or unfamiliar goods, respondents may not be able to provide coherent and stable preferences (Bateman et al. 2002).

Critics also argue that neoclassical welfare economics fails to recognise a wider range of values (e.g. O'Neill et al. 2008; Lo and Spash 2013; Kenter et al. 2015). For example, people may object to trading off values that relate to culture or ethics against money. By focusing on *how much*, economic valuation gives little attention to the *reasons* underlying valuation decisions, which include for example rights, cultural beliefs, identities, and narratives (Spash et al. 2009). When decisions are made at the community level, the summation of individual values may not represent these shared values; instead, the appropriate institution for articulating such values may be public fora in which participants discuss what is best for society at large[6] (Ward 1999). It has also been argued that such a forum for valuation is better suited to address fairness or legitimacy issues or to consider the rights of future generations (Wilson and Howarth 2002). Also, in many LICs, many people considerably rely on ecosystems to support subsistence livelihoods, which may imply that the values expressed in economic markets may not represent the true value of an environmental service (Kenter et al. 2011; Christie et al. 2012). For example, local people who heavily rely on subsistence farming may highly value nutrient cycling services. However, their stated willingness to pay is severely constrained by their monetary income and may not reflect the actual value of the services. Yet, if they do not take income constraints into account, their stated values suffer from hypothetical bias, i.e. their stated preferences would differ from their actual behaviour under real economic circumstances. In response to these fundamental criticisms, deliberative monetary valuation has been proposed as an improved approach that combines the advantages of monetary valuation with elements of deliberative processes. Where monetary valuation fails to elicit the values of interest altogether, they have sometimes been quantified in non-monetary units or described in qualitative terms (Christie et al. 2012; Kelemen et al. 2014).

1.1.4 The discrete choice experiment method: Reliability and validity issues

The term “stated preference” has come to refer to any survey-based study in which respondents are asked to state or express their individual preferences (preferences being understood as in section

[6] However, using a public forum to elicit shared values may not be unproblematic (particularly in a LIC context) because ‘shared values’ may not always be shared by all present, i.e. it is unclear whether the outcome of such public forum is more public-spirited than preferences expressed by respondents in questionnaire surveys. Such discussion is however a controversial question in political philosophy and is beyond the scope of my thesis.

1.1.3). Stated preference techniques are used when there is no relevant information which has been generated by markets. Other techniques, known as revealed preference methods infer respondents' preferences from information from markets that are associated with the good or service being valued. While stated preference techniques are particularly valuable for non-market goods, their potential is not limited to these. Stated preference techniques may also be used to simulate or predict individuals' behaviour in the marketplace or in real-life situations.

The DCE, or choice modelling, is a stated preference technique that asks respondents to express preferences across some sets of alternatives. It is based on the idea that any good or policy can be described in terms of its characteristics or attributes, and the levels that these take. This idea is rooted in Lancaster's model of consumer choice, which proposed that consumers derive satisfaction not from goods themselves but from the attributes they provide (1966). For example, a lake can be decomposed in terms of its turbidity, its recreational facilities (fishing, swimming), and its ecological quality. Changing attribute levels will essentially result in a different good being valued, DCE therefore focuses on changes in these attributes. DCE originates in market research and transport literatures and has thereafter been used to value various environmental goods and services. One of the earliest applications of DCE in economics is that of Beggs and colleagues (1981) which estimated the values of characteristics of bundles of cars, including electric vehicles. Pioneering DCE studies in environmental valuation include Adamowicz's (1994) work on measuring passive use values and that of Hanley et al. (1998a) estimating the economic value of conservation benefits in Scotland. DCEs are also increasingly used in developing countries, although the evidence base is smaller than in industrialised countries. Bennett and Birol (2010) report many applications of the method to value a wide range of environmental goods in the developing world from conservation programmes to recreation demand, the case studies generally suggest that asking poor and illiterate respondents in developing countries complex questions about their preferences is feasible provided that immense efforts are put into designing and implementing the DCE survey.

The DCE method has appealed to environmental economists by allowing the valuation of individual attributes of environmental goods that are seldom traded in markets (Hoyos 2010). However, DCEs remain controversial because of their hypothetical nature and reliance on a market analogy. Critics have long questioned their reliability and validity and debates are ongoing even among stated preference practitioners (e.g. Hanley and Barbier 2009; Carson and Groves 2011). The reliability and validity of DCEs are potentially even more problematic in LICs (Whittington 2010) where particular challenges prevail. LICs are frequently characterized by low literacy rates, language barriers, difficulties in explaining hypothetical scenarios, and lack of understanding of local people's livelihoods

and behaviour (Bennett and Birol 2010; Christie et al. 2012). Issues with the reliability and validity of stated preference techniques have been widely acknowledged in textbooks, reviews and position papers (e.g. Mitchell and Carson 1989; Bateman et al. 2002; Freeman 2003; Carson and Hanemann 2005; Carson and Groves 2007; Hess and Daily 2014; Lancsar and Swait 2014). Given the controversies over the DCE method, a systematic review of the empirical evidence from environmental studies that have incorporated tests of the reliability or validity of the DCE method may help reduce subjectivity. Chapter **2** provides the first systematic review (SR) of empirical evidence from studies that have incorporated tests of the reliability or validity of the DCE method when valuing non-market environmental goods[7].

In attempts to improve the validity of DCEs, economists have tended to devote their energy to measuring and dealing with hypothetical bias[8] (e.g. List et al. 2006; Taylor et al. 2010), but such research has not questioned what motivates respondents' answers in the first place. Qualitative approaches may provide a better understanding of individuals' stated preferences, the motives behind these preferences, as well as the mechanisms by which respondents make choices. The combination of DCEs with qualitative debriefing techniques (focus groups or individual interviews) can be particularly important in cross validating DCE results (Powe 2007). Debriefing approaches, although rarely used, can be a powerful tool to explore respondents' thought processes and "the conceptualizations, attitudes, perceptions, values and properties of the good valued" (Powe et al. 2005, p514). Qualitative techniques may not only be used as exploratory tools to aid the design of DCE surveys, but can also be used to explore the theoretical assumptions of the method, i.e. whether DCE respondents are attending to all attribute levels across each of the alternatives presented to them and whether they have any objections to the features of the survey scenarios. I tackle these issues in chapter **3**.

One major assumption of stated preference scholars is that individuals have pre-defined or well-informed preferences for large environmental interventions and these preferences can be elicited on the spot within a conventional survey situation. A major criticism of the DCE method is that respondents are unlikely to have pre-existing or complete preferences over complex or unfamiliar

[7] In other fields (transport, marketing or health economics), I am not aware of any studies which systematically reviewed empirical evidence on the reliability and validity of DCEs.

[8] Hypothetical bias is the difference between stated preferences and actual behaviour under real economic consequences. In the environmental DCE literature, since markets are lacking for many goods, hypothetical bias has been measured by matching hypothetical DCE results with the outcomes of a DCE survey requiring actual payments.

environmental issues, thus violating the rational choice theory (Mas-Colell et al. 1995). They may also be inexperienced or lack the knowledge to comprehend the importance of environmental problems for their ex-post welfare and therefore may not provide meaningful preferences. To improve the reliability and validity of DCEs, most efforts have focused on the provision of "more adequate and appropriate information", for example through the careful development of an ecosystem service framework (e.g. Barkmann et al. 2008) or by using group-based deliberative techniques (e.g. Alvarez-Farizo and Hanley 2006; Christie et al. 2006; Robinson et al. 2008; Shapansky et al. 2008). However, determining the ideal level or depth of information provision deemed adequate or sufficient for valid preferences is not only difficult (Spash 2002), but is also highly context specific. In chapter **3**, I surveyed respondents with varying degrees of experience of forest conservation to investigate whether inexperienced respondents can accurately predict the actual welfare impacts of *teviala* restrictions.

Group-based or consensus-based deliberative approaches (also qualified as "participative and deliberative approaches" to valuation in Christie et al. 2012) in particular, while increasing learning for participants, may produce values which are hard to interpret in terms of individual utility maximisation in line with neoclassical welfare theory[9] (Bunse et al. 2015). Also, the facilitation provided by the group moderator, or the exposure to expert witnesses or extensive information in a focus group or workshop setting may affect the extent to which stated preferences reflect participants' true preferences, instead valuation workshops may result in externally-induced preferences from the considerable influence of moderators or domination by a few participants[10]. The "time-to-think" protocol (e.g. Whittington et al. 1992; Cook et al. 2007) could avoid the drawbacks of participatory valuation and allow each individual to speak out and think, free from wider group influence or social norms prevailing in group-based valuation approaches. Such "time-to-think" protocols could mimic reality better since respondents can talk to other household members and the survey setting is less restricting (Whittington 2010). To the best of my knowledge, no environmental DCE study to date has explored

[9] For example, valuation workshops used by Alvarez-Farizo and Hanley (2006), Christie et al. (2006), Robinson et al. (2008) which combined elements of the citizen's jury approach with choice experiments, selectively sampled jurors from a given population and asked them to respond as citizens thinking of the welfare of the community rather than individual consumers of environmental goods. Furthermore, the relatively small sample size used by such group valuation techniques (N<50) may hamper standard parametric statistical analysis(Bunse et al. 2015).

[10] Market stall techniques (e.g. Macmillan et al. 2002) may also suffer from the same issues (moderator bias and influence of dominant participants) where the scenario presentation is provided within a group setting, and respondents are encouraged to ask questions and to discuss the issues in small groups before revealing anonymously their individual preferences.

the effect of giving more time to think[11] and discuss with others on DCE responses. I address such knowledge gap in chapter **4**.

Finally, an important reliability and validity issue, relatively underexplored in the DCE literature is the choice of the correct format for measuring welfare changes. There is widespread evidence of a consistent discrepancy between an individual's willingness to pay (WTP) for a good and his willingness to accept (WTA) compensation to forego the same good. The valuation literature suggests that WTA will generally exceed WTP measures and the WTA-WTP ratio ranges from 1.95 to 10.41 (Horowitz and McConnell 2002). The WTA-WTP disparity has been observed for private and public goods, as well as market and non-market goods in real, hypothetical and experimental settings (Horowitz and McConnell 2002; Tunçel and Hammitt 2014). The few existing DCE studies that examined the WTA – WTP discrepancy allowed respondents to trade both improvements and deterioration in the levels of attributes against the reference level, entitling them to both 'buy' or 'sell' the attributes (Hess 2008; Bateman et al. 2009; Lanz et al. 2010; Glenk 2011). While such designs allow a measure of WTA/ WTP ratio, they overlook whether the target population actually perceives a property right over the good being valued in the first place. Scholars also tend not to consider how the likely sources of the WTA-WTP disparity might inform the choice of the correct elicitation measure (Kim et al. 2015b). I only know of one DCE study that used a between-subject design where respondents were asked to answer either WTP or WTA valuation scenarios (MacDonald et al. (2010) examined the WTA-WTP disparity for changes in the reliability of household water services). In chapter **5**, I explore which valuation format (WTA/WTP) is most appropriate for estimating the welfare losses from forest conservation policy in LICs.

1.2 Madagascar as a case study site

Madagascar, the fourth largest island in the world (after Greenland, New Guinea, and Borneo), is one of the world's most significant biodiversity hotspots, meaning that it supports extremely high biodiversity levels, an exceptionally high rate of species endemism (Mittermeier et al. 1997), yet faces considerable threats from deforestation. Madagascar is also one of the poorest countries in the world where more than 92% of the population live under 2 USD a day and is ranked 155th out of 187 countries on the Human Development Index (World Bank 2014). More than two thirds of the population

[11] The effect of the time to think treatment could be expected to be more pronounced in DCE than in CV because of the complexity of trading off different attributes.

(estimated at 23.6 million in 2014) live in rural areas, and more than three quarters of the population are engaged in natural resource dependent livelihood activities (Carret 2013).

While the underlying drivers of forest loss in Madagascar are much more complex than are usually assumed[12] (Scales 2014a), the expansion of agricultural land at the forest frontier remains the major pressure on Madagascar's forests (Muttenzer 2010). Although rural people have long migrated to urban areas to seek alternative sources of income, the highly subsistence nature of many households, rapid population growth in rural areas, as well as the very low adoption of alternative livelihood options still motivate most Malagasy farmers to clear new forests for agriculture (Kull 2004). Swidden agriculture (known locally as *tavy*) is a central livelihood strategy for many rural Malagasy farmers, it is an efficient and low-input agricultural technique and represents a strategy to manage risks to food security amidst climatic hazards or market challenges (Harper 2002). *Tavy* also takes on an important cultural meaning, the slash and burn process is often accompanied by rituals and blessings (Hume 2006). Clearance of primary forest in the *tavy* system, known specifically as *teviala* has been the focus of conservation actions in Madagascar for centuries.

Madagascar's environmental policy has been considerably shaped by a predominant deforestation narrative; the island is widely believed to have lost 90 percent[13] of its original forest cover (e.g. Harper et al. 2007), with *teviala* and pastoral fire playing major role – thereby justifying the need for rapid conservation actions (Kaufmann 2014; Scales 2014b). Under strong donor pressure (Horning 2008b), Madagascar's then President Marc Ravalomanana tripled the country's protected area network from 1.7 million hectare to 6 million hectare (known as the Durban vision), to cover 10% of the country's total land area (Corson 2014). As Madagascar's National Parks agency lacked the institutional capacity and resources to oversee the expansion programme, the government has promoted a more flexible approach to the management of new protected areas by contracting out collaborative management agreements between the Directorate General of water and forests and its regional offices, and non-governmental organisations such as Conservation International. Despite the multiple-use objectives of these newly established protected areas (IUCN category V and VI) which also incorporate the

[12] Madagascar's deforestation narrative has been reduced to simplistic accounts, with poverty and population growth frequently identified as the main drivers. However, while population growth (through migration) and poverty can act as a stimulus for forest loss, they are not the underlying driver of deforestation but are themselves the result of social, economic and political drivers (Scales 2014).

[13] However, the 90% deforestation 'fact' has little empirical basis and is ultimately based on the assumption that Madagascar was more or less entirely covered in forest before human arrival (McConnell and Kull 2014).

sustainable use of natural resources, evidence suggests that they have largely failed to benefit[14] local livelihoods (e.g. Sander and Zeller 2007; Marie et al. 2009; Gardner 2014). Deforestation also persists, with an estimated average annual rate of 1.26% for the eastern rainforest[15] between 1990 and 2005 (Verified Carbon Standard 2013). The "emergency approach" (Marie et al. 2009) of the Durban Vision also significantly limited public consultation (Corson 2012) and in many cases, new protected areas were established without a sufficient understanding of their local welfare impacts or the local socio-ecological system (Gardner 2012).

Teviala has been criminalized in Madagascar since colonial times, during which *tavy* and all burning were strictly banned. State control over forest resources continued post-independence, but was seen as illegitimate by rural communities possessing de facto access to forests based on customary and ancestral rights (Antona et al. 2004). *Tavy* is often viewed as the "necessary evil" by the Malagasy government (Kull 2004, p225): forest clearance has always been considered a threat to Madagascar's unique biodiversity, yet, the government recognised the necessity of fire (including *teviala*) to rural farmers' subsistence. *Tavy* legislation during the second republic under President Ratsiraka (1975-1991) was considerably more pro-fire than the colonial[16] legislation and characterised by a politically pragmatic tolerance instead of prescribing a complete ban (Kull 2004). Pasture fires and *tavy* permits (including *teviala*) were delivered by local authorities throughout Ratsiraka's government, but in practice, the *tavy* permit system functioned imperfectly if at all. The actual number of fires outstripped the authorisations; most burners never sought a *tavy* permit and the actual forest area cut and burned for agriculture was at least ten times greater (Ramamonjisoa 2001). Consequently, deforestation by *tavy* sharply increased in the 1970s. The loosening and quasi-absence of *tavy* regulations in the second republic was mostly attributed to the political turmoil and deep economic crises striking the country during that period (Kull 2004). Foresters hardly had the means or political support to go on tour and monitor *tavy* permit compliance; "the political message at that time was to 'do as you please'" (Kull 2004, p176), and politicians were even encouraging *teviala* to ensure re-elections (*ibid*). Even to this

[14] While ecotourism activities for example may potentially provide an alternative livelihood to forest clearance for some local people, the benefits are highly seasonal and accrue only to a small subset of the population, mostly those who are literate and have better access to markets (Duffy 2008).

[15] Where forest is defined as an area of trees greater than seven metres in height with greater than 30% crown closure.

[16] The first republic (1960-1972) was also remembered as a time when a system of tavy authorisations existed and the legislation was less repressive than the colonial period, although fire enforcement was stronger than during the second republic and still reflected the previous colonial practices (1896-1960).

day, while the issuing of *tavy* authorisations officially ended in the mid-1990s (Kull 2004), and *teviala* is strictly prohibited on paper, the enforcement of the *teviala* ban is still weak (if not inexistent in many remote areas) and rural farmers continue to clear forests to expand their agricultural lands, taking advantage of the state's weaknesses (*ibid*).

In many regions in Madagascar, forestlands (particularly those outside protected areas or in newly established protected areas) are not de facto subject to well-defined formal property right regimes. Local systems of customary tenure frequently mix with, and evolve in response to, formal state-claimed ownership (Muttenzer 2006). Local people often burn forestlands to establish claims on the deforested lands, i.e. the rights to cultivate a piece of land belongs to the person who first cleared the land and his descendants (ibid). These lineage-based rights often evolve and it is common that different families in the lineage claim individual rights to certain parcels. Over time, many additional complex factors have shaped the history of *tavy*. In particular, migration, population growth, land scarcity and market incentives have significantly impacted the evolution of local customary property rights in the corridor of Ankeniheny-Zahamena. Different groups of people have migrated to the corridor at different times, searching for new forest frontiers to settle, and often claiming customary rights on the standing forestlands. These claims determine who has rights to clear the land, depending on who has arrived first. However, this system of first settlers and customary rights is sometimes violated by recent migrants[17] who forcefully clear forests ignoring previous settlers' subtle claims, which lack a legal basis. Thus, weakly enforced state ownership can undermine local tenure institutions, potentially increasing deforestation (Horning 2005; 2008a).

1.3 Study sites

When scarce resources may be used for equally deserving or competing purposes, ex-ante analysis which involves predicting the economic consequences of alternative policy actions may aid decision-making at an early stage of the policy cycle. While ex-ante analyses of agricultural and environmental regulations are common in Europe (e.g. Hertin et al. 2009; Helming et al. 2011), they are quite rare in LICs. The predictions of the local welfare impacts of forest conservation using DCEs may be invaluable to the design of compensation schemes. I surveyed households who have limited experience of conservation restrictions in one site to assess ex-ante the welfare impacts of conservation restrictions. However, respondents who lack experience of a given policy may not be 'affective forecasters', i.e.

[17] Locally known "*mahery loha*"

they may not be able to accurately predict the utility impacts of the policy (Kahneman and Sugden 2005). To check the validity of these ex-ante analyses, I also conducted an 'ex-post analysis' by surveying a matched site where local people are more experienced in conservation restrictions and whose preferences would be influenced by what actually happened[18].

I selected two study sites in the eastern escarpment of Madagascar (figure 1.1) with similar forest characteristics (i.e. situated in the same ecological zone with similar topographic and altitudinal characteristics and market access), but which primarily differ in local people's experience of forest conservation. The first one, the corridor of Ankeniheny-Zahamena (CAZ), is one of the new protected areas set up in Madagascar following the country's commitment to triple its protected areas, it encompasses one of the largest remaining blocks of rainforest in Madagascar, which spans 382,000 hectares. While the corridor has had temporary protection since 2005, people have only been exposed to conservation restrictions for a relatively short period (5 years) and the corridor was formally given protected area status only in April 2015 (Republic of Madagascar 2015). The CAZ protected area has been regarded as one of the top conservation priorities of the island and is co-managed by the Directorate General of water and forests in Madagascar, Conservation International and local community associations. It is the site of a REDD+ project (Reducing Emissions from Deforestation and Degradation) mostly financed by the World Bank's BioCarbon Fund. Carbon sequestration through REDD+ is envisioned to provide the long term funding for the corridor. My study site, the *Fokontany* of Ampahitra*,* is located in the south west corner of the corridor.

In Mantadia national park, my second study site, conservation restrictions have been enforced at a comparatively stronger level than CAZ for more than 20 years. Mantadia protected area was first established in 1989 as Madagascar's third national park and received its final protection status in 1994. The Mantadia national park is a 9,875 hectare protected area in eastern Madagascar, which is exclusively managed by Madagascar National Parks. The two study sites represent humid tropical forests, they are both characterised by steep terrain and dense undergrowth vegetation and are home to some of the island's famed biodiversity. Major pressures in these study regions include expansion of agricultural lands through forest clearance as well as illegal logging and artisanal mining. Local people's main livelihoods are based on swidden agriculture and extraction of forest products. While the distance as the crow flies between Ampahitra and Mantadia is only 40km, respondents from the

[18] I did not measure the actual consequences of the policy (as may be understood by 'ex-post-analysis') but examined the ex-ante preferences of more experienced respondents for the continuation of conservation restrictions.

two sites could not have communicated because of the very poor road conditions and remoteness of these sites. Even within each site, the communication between respondents was limited because of the widely scattered hamlets and location of households, with some isolated households taking 4-5 hours of walk from the nearest hamlet.

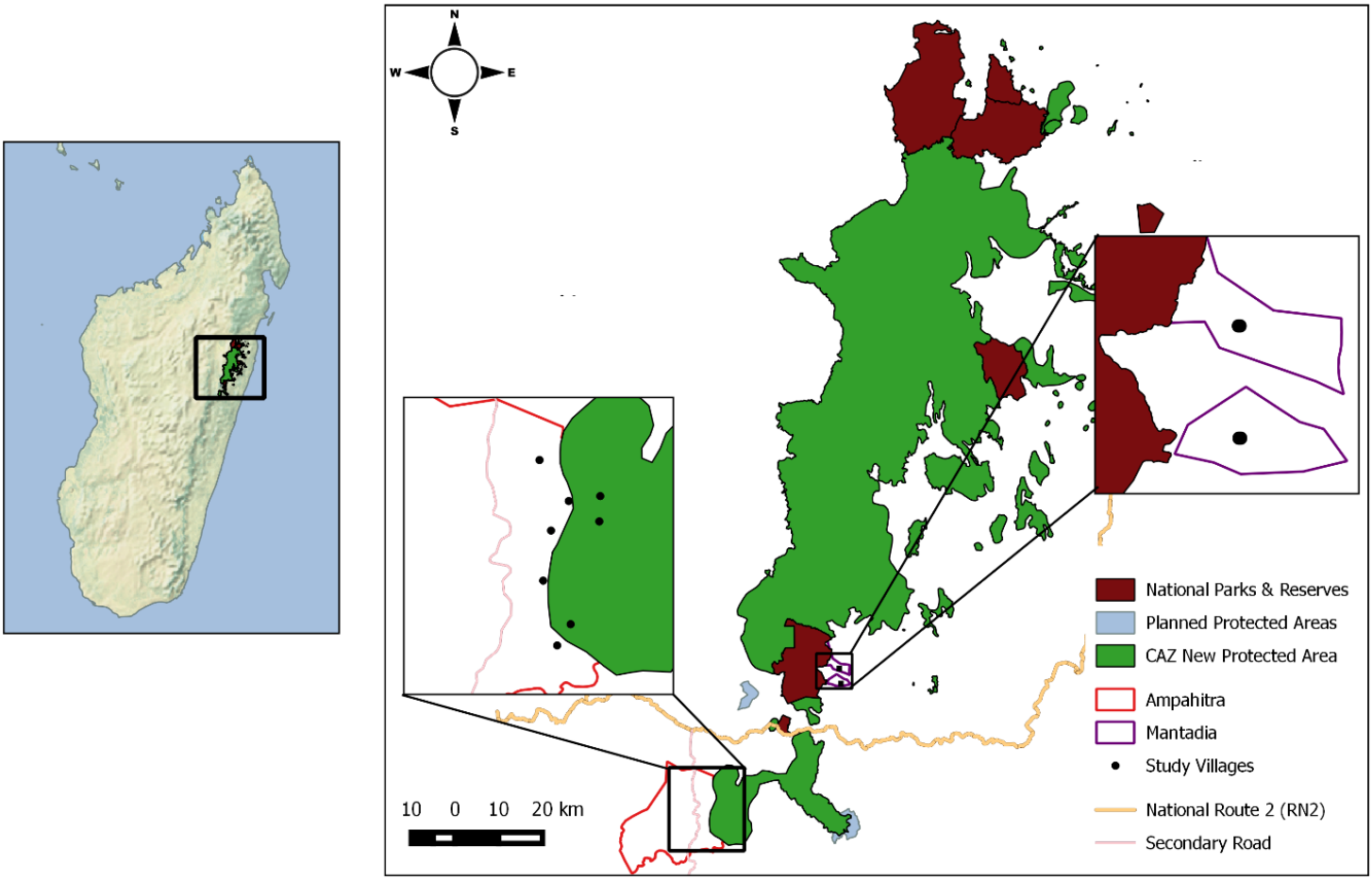

Figure 1.1: Study sites: Ampahitra (CAZ New Protected Area) and Mantadia (National Park)

1.4 Thesis aim and research questions

The overall aim of this thesis is to examine the validity and reliability of the DCE method for assessing the local welfare impacts of forest conservation in a low-income context.

Drawing upon both quantitative and qualitative methods, I address the following research questions (RQs):

> ***RQ1:*** *What is the evidence base on the validity and reliability of DCE in valuing environmental non-market goods?*
>
> ***RQ2:*** *How valid are ex-ante valuations of the welfare impacts of forest conservation using DCE? And do the DCE results have high theoretical and content validity?*
>
> ***RQ3****: Does more time to think and discuss with others influence responses to a DCE assessing the local welfare impacts of forest conservation?*
>
> ***RQ4:*** *How do the patterns of responses differ between the WTA and WTP formats? Which format is best for estimating the welfare impacts of conservation? What are respondents' attitudes to conservation restrictions and property rights over forestlands?*

These objectives are met in the course of four self-contained manuscripts (chapter 2 to 5). The contents of each chapter are outlined below. Data for chapters 3, 4 and 5 were collected during one main field work season (five months from June to October 2014) following a shorter trip to select sites and pilot survey instruments between February and March 2014. I summarise in figure 1.2 the different field treatments and sample sizes.

Chapter 2 systematically reviews the empirical evidence on the reliability and validity of DCEs (**RQ1**) in valuing non-market environmental goods. I developed a detailed search protocol to guide the review and searched two large databases, the ISI Web of Science (WoS) and Econlit. The systematic review aims to suggest areas of improvement and inform the development of contemporary guidelines in environmental DCE. It is mostly addressed to those who might commission, conduct or rely upon the results of DCE studies in applied environmental settings. The field-based empirical **chapters** (**3, 4 and 5)** build on the recommendations of the systematic review on the need to use more qualitative, interdisciplinary and deliberative approaches in the assessment of the reliability and validity of DCEs.

Chapter 3 examines the effect of prior experience of forest protection on households' preferences for compensation measures using DCE and qualitative debriefing approaches (**RQ2**). If respondents' preferences are affected by their prior experience of conservation restrictions, DCE may not be a valid method for measuring ex-ante the welfare impacts of forest conservation and required compensations. This chapter also aims to examine the theoretical and content validity of the DCE results by assessing how well they conform to the assumptions of the method. **Chapter 3**'s data come from both sites, Ampahitra which is part of the corridor of Ankeniheny-Zahamena and Mantadia national park.

Chapter 4 examines whether giving respondents in Mantadia National Park additional time (one day) to evaluate their responses to the DCE and discuss with other household members affects DCE results (**RQ3**). It also builds on the systematic review in **chapter 2**. The paired DCE responses are compared using hierarchical Bayesian procedures, socio-economics characteristics are controlled for, and methodological implications discussed.

Chapter 5 examines the patterns of responses (sign and significance of attributes) to the WTA and WTP format, assesses the performance of the two formats on a set of criteria and investigates respondents' attitudes to conservation and their perceptions of property rights in Ampahitra site (**RQ4**). **Chapter 5** addresses an important validity and reliability issue in stated preference literature and provides an applied case study in forest conservation in Madagascar. The WTA-WTP comparison was purposefully conducted in Ampahitra where the WTP scenarios were more plausible given the current land practices in the area (i.e. existing land trading and leasing agreements) and the higher proportion of migrant households (about 40% of the Ampahitra residents have moved to the area within the last 10 years against only 3% in Mantadia). Determining the most suitable welfare measure for estimating the local welfare impacts of forest conservation in this site also has additional applied relevance since it may inform the planning of compensations for the new Protected Area.

The thesis discussion (chapter 6) summarises the research findings and contributions, and discusses commonalities of the main thesis chapters. The section then highlights the limitations and strengths of the research methods and suggests potential areas for future research. It finishes with the policy implications of the thesis in the wider context of conservation and development in LICs as well as in practically designing and conducting DCEs in a LIC setting.

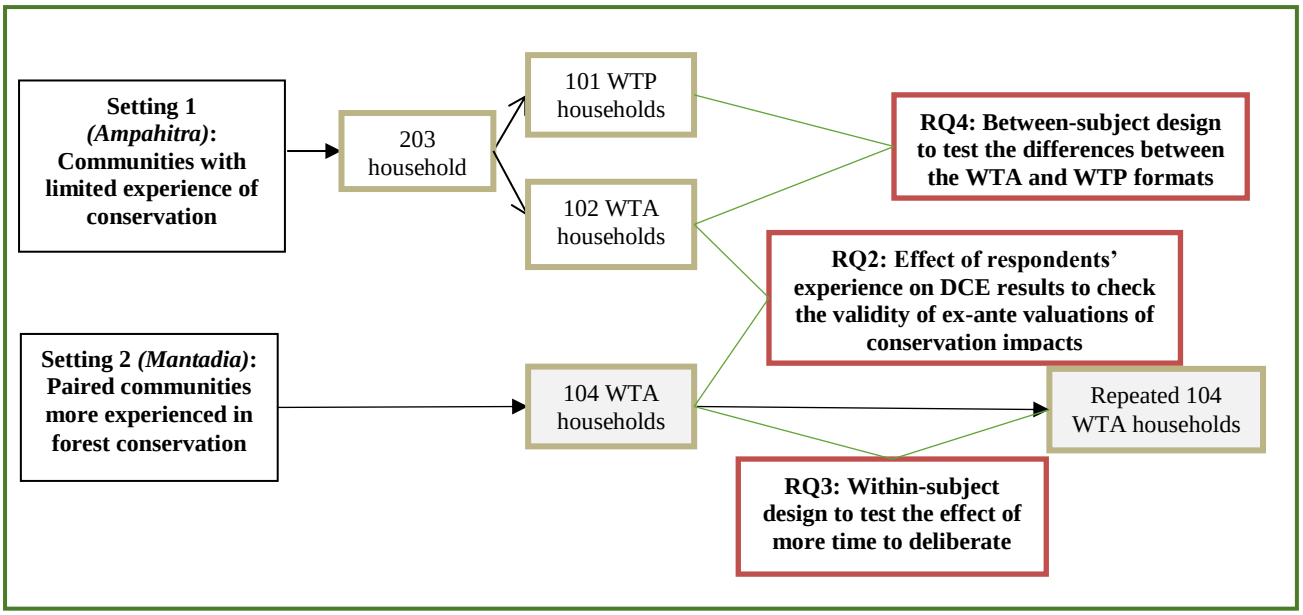

Figure 1.2: Study design

1.5 Authorship

All chapters in this thesis have been prepared as manuscripts for peer reviewed journals. Since the papers have multiple authors, I use the pronoun 'we' rather than 'I' throughout chapters **2** to **5**. My contributions are detailed in the authorship declaration for each chapter (appendix 8).

This thesis is integrated into the work package 6 of an ESPA-funded project (Ecosystem services for Poverty Alleviation), the p4ges project ("Can payments for ecosystem services reduce poverty?"). P4ges seeks to address the main research question: *How can international ecosystem service payment schemes most effectively reduce poverty in low income countries, given bio-physical, economic and political realities?* The objective of p4ges work package 6 is to estimate the magnitude and distribution of the net welfare impacts of alternative payments for ecosystem services approaches at local scales. While I designed and led the DCE survey, p4ges work package 6 used production function approaches to estimate the opportunity costs of forest conservation in Ampahitra. P4ges also used the same DCE design in three additional sites. An additional research question from the collaboration is: *How do estimates of local costs obtained through DCEs (stated preferences) compare with those from revealed preference data (household production and consumption functions estimated from in-depth agricultural surveys)?* The agricultural household surveys are still currently being processed and data analysis is scheduled to start in May 2016.

Chapter 2. A SYSTEMATIC REVIEW OF THE RELIABILITY AND VALIDITY OF DISCRETE CHOICE EXPERIMENTS IN VALUING NON-MARKET ENVIRONMENTAL GOODS[19]

ABSTRACT

While discrete choice experiments (DCEs) are increasingly used in the field of environmental valuation, they remain controversial because of their hypothetical nature and the contested reliability and validity of their results. We systematically reviewed evidence on the validity and reliability of environmental DCEs from the past thirteen years (Jan 2003- February 2016). 107 articles met our inclusion criteria. These studies provide limited and mixed evidence of the reliability and validity of DCE. Valuation results were susceptible to small changes in survey design in 45% of outcomes reporting reliability measures. DCE results were generally consistent with those of other stated preference techniques (convergent validity), but hypothetical bias was common. Evidence supporting theoretical validity (consistency with assumptions of rational choice theory) was limited. In content validity tests, 2-90% of respondents protested against a feature of the survey, and a considerable proportion found DCEs to be incomprehensible or inconsequential (17-40% and 10-62% respectively). DCE remains useful for non-market valuation, but its results should be used with caution. Given the sparse and inconclusive evidence base, we recommend that tests of reliability and validity are more routinely integrated into DCE studies and suggest how this might be achieved.

[19] **Rakotonarivo, O.S.** Schaafsma, M. Hockley, N. A systematic review on the reliability and validity of discrete choice experiments in valuing non-market environmental goods (in press). Journal of Environmental Management.

2.1 Introduction

It is frequently argued that improvements in environmental management require monetary valuation of environmental goods so that they are considered in decision-making (e.g. Jones-Walters and Mulder 2009). Stated preference (SP) techniques offer an attractive valuation approach, particularly for environmental goods which are seldom traded in markets, and they have predictably become widely used for non-market valuation (Adamowicz 2004). However, critics have long questioned their reliability and validity; that is whether they give consistent results across different survey designs that might be used to measure the same quantity and whether they measure what they are intended to (Bateman et al. 2002; Freeman 2003). Their hypothetical nature is at the heart of the controversy: since respondents are asked to answer hypothetical questions, hypothetical bias may arise, i.e. respondents' expressed preferences may differ from their actual behaviour under real economic circumstances (Hausman 2012).

The two most popular SP techniques are the contingent valuation method (CVM) and the discrete choice experiment (DCE) method (Freeman 2003); the latter is the focus of this paper. CVM usually involves a single binary choice or open-ended question and was the dominant method for valuing non-market environmental goods in the 1990s. Latterly, DCEs have become widespread among environmental practitioners (Birol and Koundouri 2008; Carson and Czajkowski 2014). DCE originates in the market research and transport literatures, and is rooted in Lancaster's (1966) model of consumer choice, which proposed that the satisfaction that consumers derive from goods could be disaggregated into the good's various attributes. One of the main advantages of DCE over CVM is its ability to value the individual attributes characterizing a good or a policy, which may be more useful from a management perspective (Hanley et al. 2001). While DCE may potentially ameliorate some of the problems of CVM, it is likely to suffer from a number of similar limitations of CVM (Hanley et al. 2001) as well as new ones.

Issues with the reliability and validity of SP techniques (in particular CVM) have been widely acknowledged in textbooks, reviews and position papers (e.g. Mitchell and Carson 1989; Bateman et al. 2002; Freeman 2003; Carson and Hanemann 2005; Carson and Groves 2007). In particular, the design and analysis of DCE surveys have long been examined (e.g. Hanley et al. 1998a; Louviere et al. 2000a; Bennett and Blamey 2001; Hensher et al. 2005; Louviere et al. 2011; Hess and Daily 2014). Despite increasing efforts to tackle various reliability and validity aspects of DCE methods, DCE studies are still viewed with suspicion and debates are ongoing about various reliability and validity aspects even among SP practitioners (e.g. Hanley and Barbier 2009; Carson and Groves 2011; Hess and Daily

2014; Lancsar and Swait 2014). Nevertheless, DCEs remain widely used (e.g. Willis et al. 2003; Boatman et al. 2010; Christie et al. 2010). In order to reduce subjectivity, and given the controversies over the DCE method, it is vital that evidence on the reliability and validity of these DCE studies is robustly synthesised so that those who might commission, conduct or rely upon their results in applied environmental settings comprehend its implications. Accordingly, this paper provides the first systematic review of empirical evidence from studies that have incorporated tests of the reliability or validity of the DCE method when valuing non-market environmental goods. This review also suggests areas for improvement and informs the development of contemporary guidelines in environmental DCE[20]. Applications of DCE in low-income and lower-middle-income countries[21] (LICs) may encounter further challenges to validity and reliability, as problems with low literacy rates, language barriers, difficulties in explaining hypothetical scenarios, and relatively low respondent exposure to surveys may be more prominent (Bennett and Birol 2010; Christie et al. 2012). We have therefore specifically identified and highlighted evidence from, and implications for, DCEs conducted in LICs.

Systematic reviews have been developed in response to calls for a more rigorous and systematic approach to identifying and synthesising evidence that could inform policy (Haddaway and Pullin 2014). Systematic reviews have the potential to enhance awareness of how much evidence is available in different parts of a field which can be useful for environmental management (e.g. Laurans et al. 2013). Unlike a conventional literature review, a systematic review follows a detailed, transparent, and reproducible search strategy, defined a priori (Pullin and Stewart 2006), thereby aiming for completeness and objectivity in summarising the knowledge base. Systematic reviews have also been used to address methodological issues. However, in environmental management we are aware of only two systematic reviews that assessed methods: Petrokofsky et al. (2012) compared the accuracy and precision of methods for measuring carbon stocks, while Le Gentil and Mongruel (2015) assessed the methods and tools used to inform coastal zone management. While using systematic reviews to investigate the efficacy of research methods is still in its infancy, it may prove to be valuable for many methodological questions in environmental economics. We only know of two studies which used a systematic approach to review the application of SP methods in environmental valuation and these

[20] Leading experts in the European Association of Environmental and Resource Economics (EAERE) are currently establishing such guidelines and standards for SP environmental valuation to promote broader acceptance of the method (see the session entitled “Emerging guidelines for stated preference methods in policy analysis” at the 21st Annual Conference).

[21] We used the World Bank’s classification throughout this paper (http://data.worldbank.org/about/country-classifications/country-and-lending-groups accessed in August 2013). High income countries (HICs) refer to high income and upper-middle-income countries while LICs are low-income and lower-middle-income countries.

concentrated on the *usage* of CVM (Carson 2011) and DCEs (Mahieu et al. 2014), rather than the reliability and validity of the methods. A secondary aim of this paper is therefore to consider the suitability of the systematic review approach for methodological questions in environmental valuation. In section 2, we develop a conceptual framework for reliability and validity. Methods are presented in section 3 and results are reported and discussed in section 4, together with implications for researchers and decision-makers. We conclude in section 5.

2.2 Reliability and validity of the discrete choice experiment method: Conceptual framework

The term “discrete choice experiment” is used throughout the review to avoid ambiguity, as suggested by Carson and Louviere (2011). The term “choice experiment” has different meanings in other disciplines such as biology and physics. To avoid confusion with the long-standing dichotomous CVM, we only cover DCE methods which involve more than a single choice set and allow analysts to estimate the marginal value of changing attributes as well as the total value of a good. Complete ranking techniques or other variants such as “best worst choice” or “pick any” techniques are often explicitly distinguished from DCE by SP researchers and are not covered by this systematic review, nor is “conjoint analysis” which originated from rating and rankings techniques that are generally inconsistent with economic demand theory (Louviere et al. 2010). Reliability refers to the degree of reproducibility of the results while validity refers to the degree to which the method is truly measuring what the researcher intended it to (Bateman et al. 2002; Freeman 2003). It may not always be possible to clearly separate tests of reliability from validity tests because the two concepts are related; low reliability limits the overall validity of a test, and a lack of validity manifests itself in unreliable responses that vary with factors to which they should be robust (Davidshofer et al. 2005). The different types of validity tests are also not mutually exclusive but should be seen as focusing on different validity aspects. We have, however, attempted to distinguish them in the framework that follows. Table 2.1 summarizes the key concepts of reliability and validity testing.

Table 2.1: Typology of validity and reliability testing in DCE studies

TESTS OF			METHODS
RELIABILITY	**Within-subject design**		- Use of the test-retest approach at two different points in time - Use of deliberation or increased exposure to information - Small changes in the background scenario - Small changes of DCE attributes or levels - Use of different choice experiments designs.
	Between-subject design (split sample)		
VALIDITY	**EXTERNAL**	**Criterion**	Comparison with actual (field) or simulated (laboratory) market experiments or non-hypothetical DCEs
		Convergent	Comparison with other methods such as hedonic pricing or contingent valuation
	INTERNAL	**Theoretical**	Examination whether DCE responses conform to the standard axioms of rational choice theory: continuity (compensatory decision making as opposed to lexicographic or discontinuous preferences), transitivity, monotonicity, and stability (including order effects) Scope and embedding tests Use of qualitative techniques (e.g. verbal protocol or debriefing interviews or focus groups) to assess the above
		Content	Use of debriefing questions or qualitative techniques to assess respondent behaviour or perceptions: - Protest attitudes: Objection to the policy scenario, disbelief in the credibility of the valuation scenario, or rejection of the payment vehicle - Belief in the consequentiality of the survey - Respondent's stated or rated comprehension

2.2.1 Reliability

DCEs are reliable if they give consistent results across different surveys that might be used to measure the same quantity (Freeman 2003). Studies testing for reliability usually survey the same individuals (within-subject design) or two independently drawn samples from the same population (between-subject or split-sample design). In the DCE literature, we identified five general ways to check for reliability: i) the test-retest approach using the same survey at two different points in time (e.g. Liebe et al. 2012; Schaafsma et al. 2014), ii) test of deliberation or greater exposure to information on DCE results (e.g. Robinson et al. 2008; Kenter et al. 2011), iii) test of framing effects or small changes in the background scenario (prior to choice sets) (e.g. Carlsson et al. 2010; Tonsor and Shupp 2011), iv) test of small changes to DCE attributes or levels (e.g. Bateman et al. 2009; Solino et al. 2012), and v) comparisons of the results of different experimental design characteristics (e.g. Rolfe and Bennett 2009; Baskaran et al. 2013). The first reliability check (i) is concerned with the temporal stability of stated values while the four others (ii to v) involve the simultaneous or subsequent use of two slightly different DCEs. The sensitivity of results to small changes in DCE survey instruments may be systematic

and eventually predictable. Until then, we argue that these checks are important because decision-making often relies on the results of a single DCE survey. A systematic review of the outcomes of these tests therefore provides insights into the importance of methodological differences and how DCE surveys might usefully be improved.

2.2.2 Validity

Validity consists of i) external[22] validity (sometimes referred to as "concurrent validity" and including criterion and convergent validity) and ii) internal validity (theoretical and content validity). External validity tests involve comparisons with instruments other than a DCE survey while internal validity tests focus on the core assumptions of the DCE methods.

2.2.2.1 External validity testing

Criterion validity refers to the extent to which preferences elicited by the DCE method are related to another measure (a 'criterion') which is considered to be "true", or at least closer to the theoretical construct of the investigation, such as data from real or simulated markets (Bateman et al. 2002). It is therefore directly concerned with hypothetical bias. However, for non-market environmental goods, the validity of market behaviour as a true measure of welfare might often be contested and for many environmental goods, no valid criterion measure can be observed. Therefore, some DCE researchers have used "real" or "non-hypothetical" DCE designs where respondents are presented with the same choices as in the hypothetical CE and then informed that one of the choices will be drawn randomly and will be binding, i.e. they will either have to pay or be paid the amount of money of the chosen alternative (e.g. Ready et al. 2010; Taylor et al. 2010).

Convergent validity refers to the correspondence between measures obtained by different methods (Freeman 2003). In convergent validity testing, no method can be presumed superior to the other: two experiments that deliver the same estimates might just be equally invalid. DCE results can be compared with one of three alternatives: revealed preferences (e.g. travel cost models, production function approaches, hedonic pricing) (e.g. Scarpa et al. 2003); CVM or complete contingent ranking techniques (e.g. Caparros et al. 2008; Christie and Azevedo 2009); or other valuation methods which

[22] External validity in this review is different from the concept of external validity in the scientific literature generally, which refers to the extent to which the findings of a study can be legitimately transferred from one context to another (Brewer 2000).

may not be consistent with random utility theory such as multi-criteria analysis (e.g. Moran et al. 2007) or a simple attribute ranking exercise (e.g. Azevedo et al. 2009).

2.2.2.2 Internal validity testing

DCE results are said to be theoretically valid if respondents' choices do not deviate from the assumptions of standard rational choice theory (on which DCE methods are based), as defined by four axioms of utility maximisation (Mas-Colell et al. 1995). i) The "continuity axiom" refers to the use of compensatory decision-making rules i.e. attending to all the attribute levels across each of the alternatives and choosing the most preferred alternative within a choice task instead of using heuristics. Attribute non-attendance has also been referred to as discontinuous or lexicographic preferences (see Colombo et al. 2013 for a review in the environmental DCE literature). ii) Monotonic preferences require that, holding the levels of all other attributes equal, respondents should never prefer worse levels to better levels of an attribute (e.g. lower price in a WTP format should be preferred to a higher price). iii) The transitivity axiom requires that if a respondent prefers option A over option B and option B over option C, then he must prefer option A over option C. iv) The stability axiom[23] requires that when a respondent chooses an alternative A over an alternative B, he does not reverse his preference if presented with the same choice set later on. Stability testing also encompasses tests of order effects i.e. the influence of the order in which choice sets are presented to respondents (e.g. Day et al. 2012).

Other tests of theoretical validity concern sensitivity to scope. In DCE, sensitivity to scope broadly presumes that respondents should be willing to pay more for a large effect than for a subset of that effect (Carson and Czajkowski 2014). Within-subject tests of sensitivity to scope assess whether a change in one or more attribute levels in a given alternative influence WTP significantly. Such within-subject tests may be judged to be weak; external scope tests which use a split sample design and compare WTP across samples from the same population are viewed as stronger tests (Rolfe and Wang 2011). Scope tests are conceptually different to tests of monotonicity; failure to pass scope tests might not always indicate non-monotonicity: it may indicate satiation which is strictly compatible with the monotonicity axiom (Banerjee and Murphy 2005). We included within subject and split sample scope tests.

[23] Stability here is different from the temporal reliability defined in section 2.1. In practice the difference is between stability within a survey (i.e. across different presentations at the same time) vs stability across identical presentations over time.

Bateman et al. (2002, p305) refer to studies with high content validity as those in which the survey descriptions and questions are "conducive and sufficient to induce respondents to reveal valid stated values". We identified three measures of the content validity of the DCE method: i) protest responses, ii) perceptions of consequentiality, and iii) comprehension of the DCE. Measures of protest responses aim to identify respondents who object to some features of the survey or the valuation scenario and are distinguished from zero-bids. Protest responses often concern distrust towards the payment vehicle or beliefs regarding the credibility of the policy scenario (e.g. Meyerhoff and Liebe 2009). Measures of perceived consequentiality examine whether respondents care about the survey outcomes and view them as consequential: i.e. having real policy impact (e.g. Vossler et al. 2012). Respondents' comprehension of the valuation exercise is either self-reported by respondents or rated by researchers (e.g. Barkmann et al. 2008).

Lack of theoretical and content validity can be identified in respondents' choices or self-reported by respondents in follow-up statements. The lack of validity has been measured by: i) the percentage of respondents showing violations of the utility axioms or perceiving a lack of content validity, ii) the effect on willingness-to-pay (WTP) estimates of, for example, removing the inconsistent choices from the analysis, or iii) entering an additional variable into the econometric specification that captures the lack of theoretical or content validity (e.g. Alemu et al. 2013). Qualitative methods can also be used to assess both theoretical and content validity of DCE. These include verbal protocols during the completion of the valuation task (Arana and Leon 2009) or debriefing interviews after the DCE exercise (through focus groups or individual qualitative interviews) (e.g. Powe et al. 2005).

2.3 Methods

The systematic review process generally comprises five steps: the development of a protocol to guide the review, screening or inclusion criteria, quality appraisal, data extraction, and synthesis (Pullin and Stewart 2006). As the primary objective of this review is to examine the evidence on the reliability and validity of the DCE method, we selected studies which met the inclusion criteria and whose survey design is judged sufficiently robust to answer our review question, but did not further appraise the quality[24] of the selected articles given the limited evidence base. We sent the review protocol to six DCE experts and practitioners, three of them reviewed it and provided valuable comments on the selection criteria and search strategy.

2.3.1 Systematic review protocol and search strategy

We used the conceptual framework developed in section 2 to generate a set of search terms that were included in a search string formatted according to requirements for searching in the Web of Science (WoS) and EconLit databases. Following experts' recommendations, we used a set of 24 references (Supplement 1) as a 'test library' to check whether the search strings captured the expected studies, and, if not, what terms would have included them and how many other relevant studies using those new terms might add. We used an iterative checking process to validate the search terms and reduce the risk of missing relevant studies. The final search string employed (figure 2.1) was defined after 15 iterations and was judged to be sufficiently diverse to capture different phrasings of the reliability and validity of DCE. The search terms ensured a balance between the proportion of hits that are relevant (referred to in the systematic review literature as "specificity") whilst ensuring that all available literature was captured ("sensitivity"). We conducted the initial search between 20 July and 20 August 2013 by entering the search terms (into two databases: i) WoS (*https://webofknowledge.com*/), one of the world's largest databases of scientific papers and (ii) Econlit *(http://www.aeaweb.org/econlit/icon.php)*, the database of the American Economic Association. The search was updated on 24-29 February 2016, using WoS only, since EconLit had returned only three additional includable articles in the initial search. After removing duplicates, articles were assessed against our inclusion criteria (see 3.2) first using titles and keywords, then abstracts, then full texts. At each stage any potentially includable studies were retained for the next stage. Included studies are

[24] Quality appraisal involves the scoring of each relevant study against a set of pre-established criteria or "quality hierarchy". These criteria often involve subjective judgements about the relative importance of different sources of bias (for more details, please see Pullin and Stewart 2006).

described in the synthesis tables (Supplement 3), which report the type of validity and reliability checks, the good valued, the location, the sample design and sample size, the econometric methods used and the methods used to test for the equality of marginal willingness to pay (MWTP) / willingness to accept (MWTA) estimates.

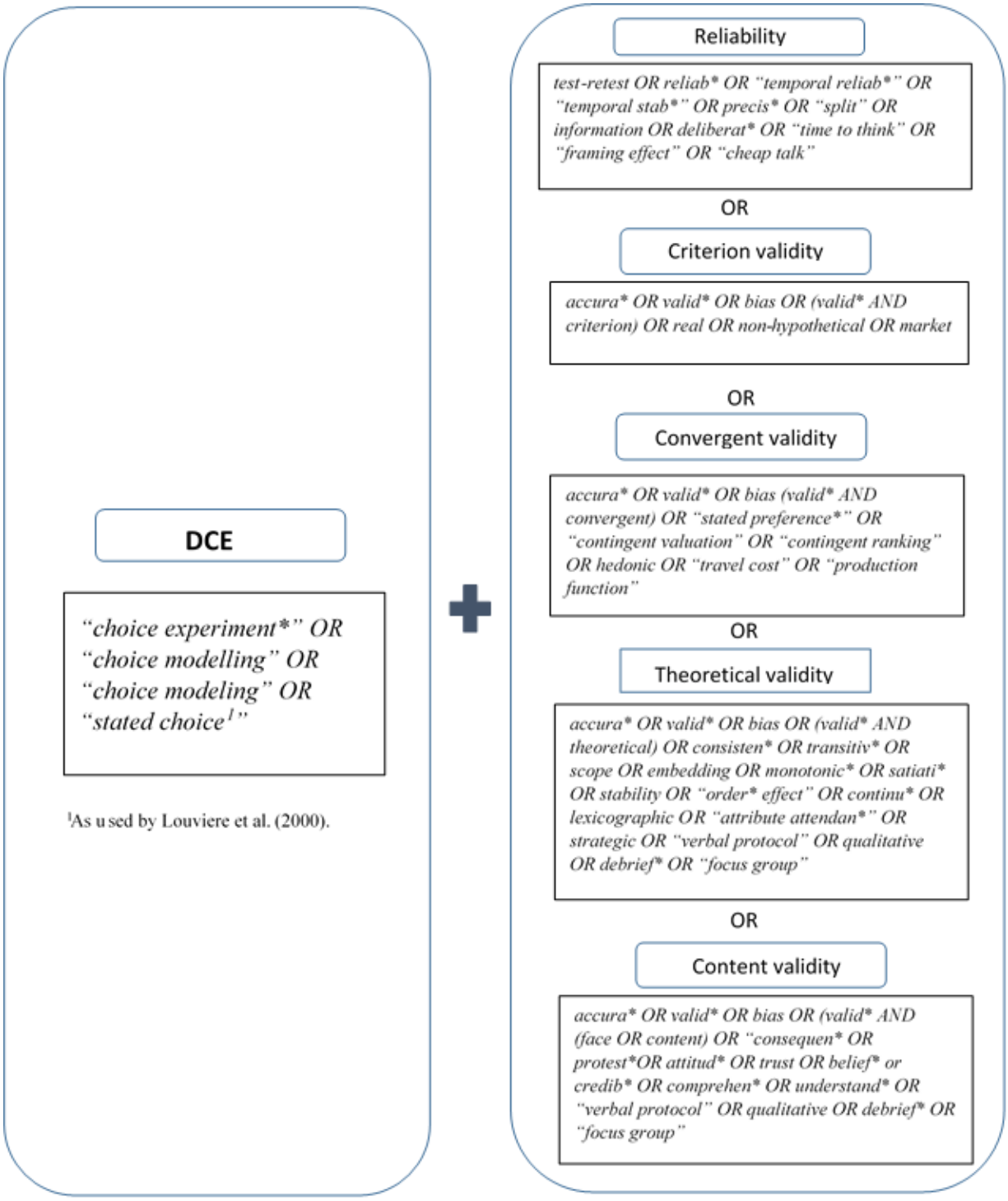

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Figure 2.1: Search strings (combination of sub-strings from DCE and different approaches to reliability and validity testing using Boolean operators)

2.3.2 Inclusion criteria, data extraction and synthesis

To be included in the review, studies had to satisfy the following criteria. They had to test for the validity or reliability of the DCE results, and must have been published between January 2003 and February 2016. The time span was restricted to capture the most recent studies as DCE and SP techniques have advanced over the years and are evolving fast. The object of valuation or type of good being valued was restricted to non-market environmental goods or non-market environmental attributes of market goods, including both use and non-use values. "Non-market" refers to goods that do not have an observable market price and are not sold or bought directly in the market (e.g. the regulation of water or air quality, or recreational and spiritual benefits - See Millenium Ecosystem Assessment 2005). Non-market attributes of market goods include for instance the ecological component of certified coffee beans (e.g. Carlsson et al. 2010; Tonsor and Shupp 2011), where organic production may be supposed to produce public goods as well as private benefits to the consumer. Only original DCE applications were included in the analysis, and benefit transfer studies, meta-analyses or discussion papers were excluded. Only papers in English were included.

To be included, qualitative studies must have explicitly reported results in a manner which allows an assessment of reliability/validity to be made. Studies which carried out focus groups or other qualitative methods simply to assist in drafting DCE surveys were excluded. Studies which only included robustness checks (Smith 2007), which examine model fits or the robustness of results to different assumptions such as the treatment of unobserved heterogeneity or different model specifications (e.g. Campbell et al. 2011; Christie and Gibbons 2011; Torres et al. 2011) were also excluded. Instead, we focused on the design and administration of DCE surveys, and on how respondents perceive and answer them, rather than on data analysis. Similarly, we excluded studies that only tested common prior expectations such as the relationship between WTP estimates and income (Bateman et al. 2002). Such tests are routinely handled in data analyses and are ambiguous tests of validity[25]. We excluded respondents' self-reported certainty about their choices since low certainty may represent a real feature of respondents' preferences not a lack of validity. Likewise, we excluded comparisons of MWTP and MWTA estimates because the WTP-WTA disparity is not prima facie evidence of lack of reliability of the DCE method but may instead reflect underlying preferences consistent with Hicksian theory (Kim et al. 2015b). Conversely, while comparing the effect of

[25] We distinguish such tests from those described in section 2.2.2, which concern assumptions on which the DCE method is based.

alternative survey administration modes on DCE results (e.g. Olsen 2009) rightly qualifies as reliability testing, it is beyond of the scope of this systematic review which focused on survey design.

Different outcome elements were extracted from the included studies depending on the types of reliability or validity tests. Reliability, criterion and convergent validity testing often produce comparisons of attribute parameters (or utility coefficients), MWTP/MWTA or compensating surplus estimates between split samples. When comparing attribute parameters between two samples, we included outcomes which used the Swait and Louviere sequential testing procedure (1993) to account for differences in scale factors[26]. In logit models, the scale parameter (inversely related to the variance of the error term) is jointly estimated and hence confounded with the attribute parameters in the utility function (Louviere et al. 2000a). Three tests for equality of MWTP/MWTA estimates were used in the reviewed studies; i) confidence intervals, ii) performing a simple t-test, iii) using the complete combinatorial method (Poe et al. 2005). The first two tests can give biased outcomes if normality assumptions are violated: t-tests in particular might underestimate the level of significance of differences in WTP (*ibid*). Nevertheless, we included studies that used any of the three tests, but noted the approaches used by authors (Supplement 3). Studies are too heterogeneous to permit a quantitative meta-analysis. Instead, using the full synthesis tables (Supplement 3), we describe the state of evidence by highlighting the number of studies providing a yes or no answer to the questions of interest. We do not present effect sizes, which would be uninformative because both the context and the non-market environmental good being valued differed across studies.

[26] We note that in addition to the Swait-Louviere sequential procedure, there are also less common methods used by other fields (transport and health economics) to control for scale differences such as the procedure proposed by Ben-Akiva and Morikawa (1990), in which observations from separate (groups of) choice tasks are used simultaneously to maximize a joint likelihood function; and the Bradley and Daly (1994) one-step estimation approach of Ben-Akiva and Morikawa, which can be implemented using a nested logit (the logit-based scaling approach).

2.4 Results and discussion

Searches in August 2013 returned 2350 articles from WoS and 2600 from Econlit. After removal of duplicates 2865 articles remained, and 995 of these were identified as potentially relevant from the title and keywords. 285 articles were retained after abstract-level screening, and 78 after initial full text assessment. The updated search in February 2016 resulted in 1104 articles, of which 59 articles were fully assessed. In total, 107 articles (29 were from the update) were included after the final stage of full text assessment, from which the outcomes of interest were extracted. The most common reasons for the exclusion of articles at this final stage included not valuing non-market environmental goods or non-market environmental attributes of market goods (e.g. Lusk and Schroeder 2004; Hess et al. 2012), absence of a test of reliability or validity (e.g. Beharry-Borg et al. 2009; Bush et al. 2009), including only robustness checks (e.g. Campbell et al. 2011; Christie and Gibbons 2011; Torres et al. 2011), discussions or theoretical articles (e.g. Carson and Groves 2007; Carlsson 2010), or not using a hypothetical DCE (e.g. Gracia et al. 2011; Michaud et al. 2013). Of the 107 studies retained, 12 articles (11%) were conducted in LIC and one is a working paper, the remainder were all in peer-reviewed journals. Supplement 2 indexes all 107 studies by their IDs; these studies are synthesized in supplement 3 and the studies excluded at the final stage of full text assessment along with the reasons for exclusion are reported in Supplement 4.

We found 56 and 65 articles incorporating reliability and validity tests respectively (14 tested both) (figure 2.2). Twenty-eight articles produced more than one outcome of reliability and/or validity tests, the total number of test outcomes was 173 (93 and 80 outcomes of reliability and validity tests respectively).

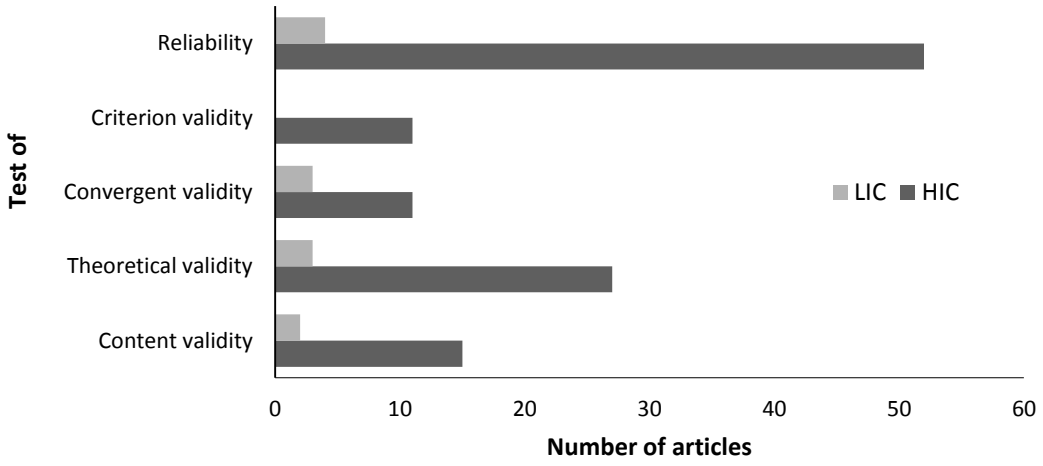
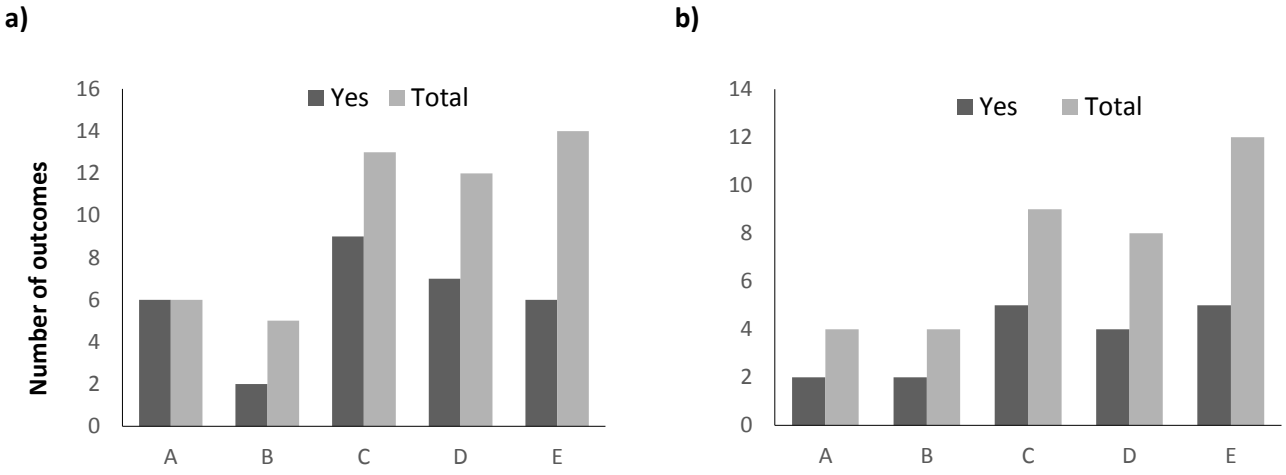


Figure 2.2: Number of articles incorporating validity and reliability tests in low-income (LIC) and high-income (HIC) countries

2.4.1 Reliability

Of the 87 outcomes of reliability tests (from 50 articles, of which only three were conducted in LICs), 39 (45%) found a significant difference between treatments: 20 (40%) for MWTP/MWTA and 19 (51%) for attribute parameters (figure 2.3). Six outcomes (from six articles, all but one in HICs) were neither comparisons of attribute parameters nor MWTP/MWTA estimates. Respondents' choices were not altered by deliberation in a HIC setting (S75), whereas the good valued became incommensurable with money following deliberation in a LIC context (S51). A "cheap talk script" significantly increased the percentage of respondents who chose the status quo option (S14 and S60). Likewise, different design characteristics (number of choice sets, alternatives, attributes, levels and the range of levels) reflecting different levels of complexity significantly affected choice outcomes (S14 and S66). Only three outcomes were derived from a WTA survey (S10, S50 and S39) and 67 outcomes (72%) from a between-subject design. This mixed evidence on the reliability of DCE is not unexpected, since survey research has long demonstrated that small changes in the design or wording can significantly affect outcomes (Schuman and Presser 1981). It should also be remembered that statistically significant results may not be economically significant, and vice versa. Nevertheless, if two similar designs (each of which might be considered good practice) yield different results, decision-makers must apply appropriate caution in relying on the results of any single DCE study.



A: Test-retest: Are DCE results stable over time?
B: Are DCE results robust to deliberation?
C: Are DCE results invariant to small changes in the background scenario prior to choice sets?
D: Do small changes of DCE attributes or levels have no effect on the DCE results?
E: Are DCE results unsusceptible to different design characteristics?

Figure 2.3: a) Outcomes comparing MWTP/MWTA estimates, b) Outcomes comparing equality of attribute parameters (at 5 % significance level) - *Note: Yes responses to questions A – E indicate evidence consistent with the reliability of DCE i.e. attribute parameters or MWTP/MWTA estimates were not significantly different at the 5% level.*

2.4.2 Validity

2.4.2.1 Do DCEs predict behaviour in real transactions?

Eleven articles used some criterion validity testing (S4, S14, S17, S47, S48, S60, S71, S76, S94, S102, S106) producing 13 outcomes (figure 2.4). They were all conducted in HICs and 11 outcomes used non-hypothetical DCE as the criterion. None of these 11 outcomes supported the criterion validity of DCE: hypothetical bias varied from 50% to 100%. However, three of these 11 studies (S14, S60, and S71) also used a "cheap talk script" to "mitigate" hypothetical bias but only the first two succeeded (i.e. similar behaviour was found in real and hypothetical settings). One study (S76) found criterion validity only when data were weighted by respondents' certainty. In S102, hypothetical bias was no longer significant when only respondents who believed their answers could influence policy decisions were included in the analysis. However, S17 still found significant hypothetical bias after adjusting for consequentiality. The remaining two outcomes (both from S4) used an experimental market to value the environmental features of a market good (a detergent) and compared hypothetical DCE shares with the experimental market shares. The study found the same market shares one month after the goods were traded in the market, but different shares after four months[27].

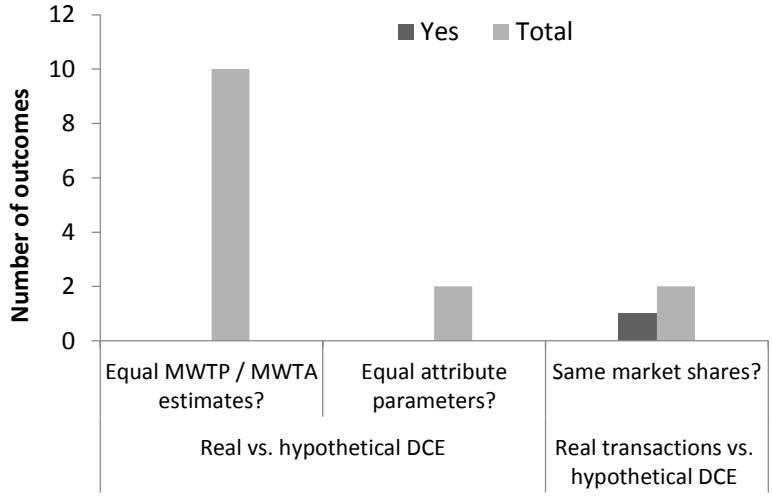


Figure 2.4: Criterion validity tests: Do DCEs predict behaviour in real transactions? Note: Yes responses indicate that the outcomes are consistent with criterion validity (i.e. MWTP/MWTA estimates, attribute parameters or market shares were not significantly different between the hypothetical and real / simulated treatments at the 5% level).

[27] However, the difference after four months could be due to changes in market conditions that occurred during those four months. That is, the difference does not necessarily invalidate the DCE.

While these results suggest that DCEs are unlikely to predict respondents' behaviour in non-hypothetical situations, they must be interpreted with caution since the laboratory or controlled experiments with which DCEs were compared may themselves fail to predict behaviour outside the lab (Carlsson 2010). The use of students in many of these tests (S14, S17, S347, S48 and S94) bears little resemblance to the diverse contexts in which DCE are used. For many non-market goods, a simulated market may not provide a true criterion measure of welfare impacts, that is actual behaviour may not be the "gold standard" against which DCE outcomes should be assessed, when the goal of the DCE is to estimate welfare impacts rather than predict market behaviour, or when there are no intentions to create real markets. Likewise, when the good is associated with non-utilitarian values (Lo and Spash 2013; Kenter et al. 2015), "real" DCEs may not reflect full welfare effects.

2.4.2.2 Does DCE produce the same results as other methods?

Fourteen articles (producing 14 outcomes) tested for the convergent validity of DCE (S1, S6, S21, S26, S28, S33, S34, S67, S68, S73, S74, S87, S100), of which three were conducted in LICs (S73, S87, S100). The evidence generally supported consistency between DCE and other SP methods (figure 2.5). Two out of six outcomes comparing DCE and CVM did not find convergent compensating surplus estimates (S26 and S74). Equality of compensating surplus estimates depended on the specification of the utility function (S67) and the econometric modelling used (S28). Comparisons with other methods gave mixed outcomes: four contingent and qualitative ranking studies produced the same preference orderings as DCE (S6, S34, S68, S73); while multi-criteria analysis techniques produced different preference rankings than DCE (S68). MWTP estimates from DCE and hedonic pricing method were not shown to be statistically different (S87), whereas a significant difference was found in a comparison with the travel cost method (S1). While these results generally provide evidence of convergent validity between DCE measures and other SP approaches (CVM and contingent rankings), they only indicate 'validity by association', that is neither method can claim to be measuring the true value of the underlying construct (Bateman et al. 2002).

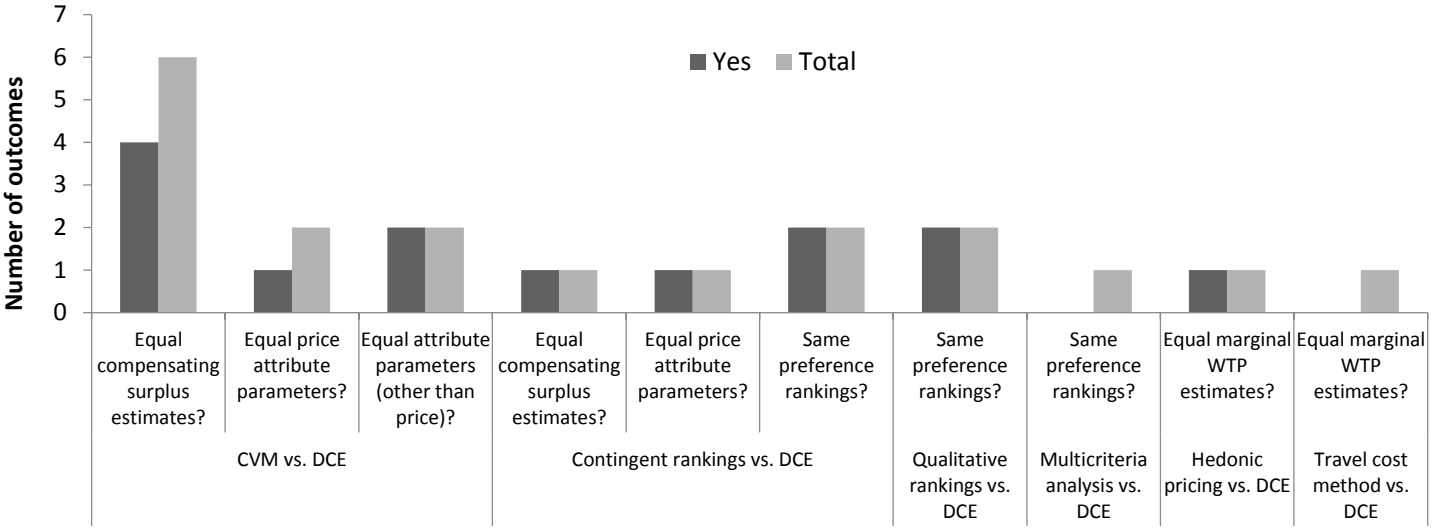


Figure 2.5: Convergent validity tests: Does DCE produce the same results as other methods? Note: Yes responses indicate evidence consistent with convergent validity i.e. (MWTP/MWTA estimates - attribute parameters or market shares were not significantly different between the hypothetical and real / simulated treatments at 5% level[28])

2.4.2.3 Do DCE results conform to theoretical expectations?

We found 30 articles that tested for the theoretical validity of DCE (of which three were conducted in LICs) producing 34 outcomes. Twenty-eight outcomes tested whether DCE results conformed to rational choice theory and six tested scope effects.

Twelve outcomes were quantitative measures of consistency with the continuity axiom as self-reported by respondents in follow-up statements (S2, S20, S24, S29, S30, S40, S53, S72, S84, S85, S97, S104). Between 15-100% of respondents reported that they did not behave as assumed by the axiom. Removing or accounting for discontinuous preferences in the analysis had mixed effects on WTP estimates; S24, S72, S84, and S97 found no systematic differences in MWTP between two models with and without consideration of ignored attributes whereas S20, S53, and S85 found the opposite. Accounting for stated partial attendance ('sometimes ignored') had statistically significant effects on both estimated preferences and welfare measures (S29). Non-attendance to alternatives due to unacceptable attribute levels occurred in 14% of the choices in S30. Design dimensions such as the number of choice sets, alternatives, levels and the level range did not affect stated attendance to attributes (S30 and S104). Two studies (S53 and S104) also employed econometric methods to reveal attribute non-attendance ex-post and found that a much smaller proportion of respondents was

[28] Here, we report compensating surplus estimates for CVM

inferred to have attended to all attributes, compared to self-reported attendance. Two studies used qualitative techniques to test whether DCE results conform to the continuity axiom (S5 and S75). Post-DCE focus group results suggested that respondents attended to all the attribute levels across each of the alternatives, although some admitted that making trade-offs was difficult (S75). The verbal protocol and thinking aloud techniques showed that 66% of the sample followed compensatory rules, and greater emotional intensity significantly increased the likelihood of using non-compensatory decision making rules (S5).

Three studies reported measures of monotonicity, finding 25%, 21% and 1% of respondents violating that axiom respectively (S93, S86 and S65). The exclusion of choices that deviated from the monotonicity axiom resulted in reduced MWTP estimates in S86. Unstable preferences were diagnosed for 6-28% of respondents (S17, S18, S19, S84, S86, S93). Excluding respondents who violated the stability axiom from the analysis significantly lowered MWTP estimates in S86 but not in S84. S35, S95 and S96 found evidence of order effects, i.e. systematic changes in respondents' preference parameters related to the position of the choice task or the nature of options defined by attribute levels in previous tasks. While S32 found evidence of learning, attribute parameters and MWTP were not statistically different between choice tasks. Only one study assessed responses relative to the transitivity axiom and found that 17% of respondents showed intransitive preferences (S17). Six articles examined whether DCE conforms to expectations regarding scope effects. When examined across samples, four out of five DCE outcomes were not sensitive to scope (S43, S45, S61, and S70), while S58 found sensitivity to scope, except for the MWTP of one attribute for which test failure may be explained by its diminishing marginal values. In within-sample tests, MWTP of some attributes was sensitive to scope, while MWTP for others was not (S43, S70).

2.4.2.4 Do respondents find the DCE survey descriptions plausible, consequential, and comprehensible?

In total 17 articles (producing 20 outcomes) reported quantitative measures of the content validity of DCE, two of them were conducted in LICs (S7 and S12). Fifteen articles identified protest responses. 'Protesters' were defined as those who objected to the policy scenario in most studies (S7, S8, S77, S40, S44, S45, S46, S63, S64, S70, S95, S101, S103), those who perceived a lack of credibility of the hypothetical scenario (S8 and S90), and those who rejected the payment vehicle (S12). Respondents protesting ranged from 2% to 58% of the total sample in HICs and reached 90% in one of the LIC studies (S12). In one study (S7), respondents' average comprehension was rated at 3.1 (on a five point ascending scale) by enumerators, while in another (S90) 40% of the respondents stated that they did not understand the valuation task and 17% found making choices between alternative management

options confusing. Three studies assessed the perceived consequentiality of the DCE survey and found that 35%, 53%, and 90% of the respondents believed that the study could have an impact on policy (in S68, S75 and S16 respectively). Only one article (S75) used qualitative techniques (post-DCE focus groups) to test for all three (protest responses, consequentiality, and comprehensibility). Participants in the focus group debriefings asserted that they considered their budget constraints and found the link between expected outcomes and the proposed policy realistic. However more than half of participants found the choice task difficult, with too much information.

2.4.3 Future directions in testing the reliability and validity of DCEs

The limited evidence base calls for greater attention to reliability and validity testing of DCEs in environmental valuation. We found that only 55% of the reliability outcomes passed the test. Reliability tests are essential to assess the robustness of results, and arguably as fundamental as calculating confidence intervals. Since DCE researchers may not be able to assess a priori how different information or designs will affect choices, tests of reliability should be incorporated into DCEs whenever resources allow. More specifically, we see research potential in the identification of minimum levels of deliberation for reliable preference elicitation in different contexts and more test-retests (see figure 3a). Kenter et al. (2011), for example, conducted a DCE with rural and illiterate respondents and experimented with communal deliberative workshops to improve the quantity and quality of information available to participants. As between-sample tests currently dominate, more within-sample tests would strengthen the current evidence base.

Criterion validity is the least tested, yet often violated form of validity according to this review, with only 1 out of 13 outcomes passing the test. We recommend that whenever a reasonably valid and feasible criterion is available, DCE researchers should strive to measure hypothetical bias and investigate its sources. In other circumstances, methods should be developed to elicit value components that real markets and “real” DCE may not unveil, for example through participative and deliberative approaches (Spash 2008) or mixed methods (Powe 2007). Ultimately, for many non-market environmental goods no suitable criterion may ever become available. In such circumstances, SP techniques like hypothetical DCE may be the only option for monetary valuation, even if their criterion validity is untestable.

For convergent validity, 14 out of 19 outcomes passed the test, mostly when DCE is compared with other SP methods (CVM). Whilst there are many CVM vs revealed preference comparisons (Carson et al. 1996), only two studies compared DCE results with revealed preferences (hedonic pricing and travel cost method), with mixed findings (S1 and S87). We therefore concur with Lancsar and Swait’s (2014)

recommendations for health economics: opportunities remain to compare revealed preference data with DCE estimates in environmental and resource economics, even though market failures undisputedly exist and revealed preference data cannot be presumed to provide a closer approximation to the "truth" than DCE data. Another avenue for further convergent validity testing, of which we found no existing study, would be to compare preferences revealed in response to interventions (e.g. randomized controlled trials where feasible) with those elicited by DCEs conducted prior to implementation. Ex-ante predictions from the original DCE could then be compared with ex-post revealed preference outcomes (Lancsar and Swait 2014).

Theoretical validity tests, in particular attribute-attendance and sensitivity-to-scope tests, are the most prevalent validity tests conducted to date, yet also often contested (Adamowicz et al. 2014). DCE analysis assumes behaviour compatible with rational choice theory, and deviations from rational choice theory have implications for analysis and interpretation. In particular, non-attendance to attributes has been a central issue in the examination of the theoretical validity of DCE as failure to identify and account for attribute attendance may bias welfare estimates and respondents' utility functions. Research from other fields suggests that respondents do not fully ignore attributes as they self-report but instead place lower importance on them, which need not be zero (Hess et al. 2013; Balcombe et al. 2014). Also, as insensitivity to scope has been extensively demonstrated for CVM (e.g. Carson and Mitchell 1993) and was found in five out of six DCE studies in this review, we recommend that DCE researchers build in tests of how the environmental good is presented to the respondent whenever the results are suspected to be insensitive to scope.

Whether rational choice theory is a useful model of human behaviour has been much disputed by behavioural psychologists (e. g. Herrnstein 1990) and economists equally assert that rational choice theory may not always correctly predict human behaviour. Recent advances in DCE modelling have suggested different ways to account for deviations from utility axioms (e.g. Campbell et al. 2011), however, they may not be a panacea if the level of non-conformities is unacceptably high. Once again, subjective judgments about what is acceptable must be made. Likewise, alternative choice theories or models which relax the assumptions of rational choice theory may be used (e.g. regret minimizing theory models, Thiene et al. 2011), however, they may pose problems for aggregation if the assumptions of the social welfare function used are violated. Ultimately, if DCEs are to be useful to policy and a lack of theoretical validity is a major concern, DCE researchers ought to gain a better understanding of the disparate and context-dependent ways in which respondents make choices (Loomes 1999) as well as the factors or processes explaining violations of rational choice theory and how they relate to respondents' characteristics (Adamowicz et al. 2014). We recommend using

qualitative approaches in combination with DCEs to make full use of key concepts in cognitive psychology and decision-making (Carlsson 2010), for instance, to gain better understanding of choice processes and mechanisms (e.g. Clark et al. 2000; Powe et al. 2005). Qualitative approaches have so far been scarcely used to test the validity of DCE, because of concerns about the lack of generalizability and unclear economic interpretation of the results (Johnston 2009) or possibly a lack of experience with qualitative approaches among DCE researchers. We believe that DCE and environmental valuation can benefit considerably from interdisciplinary approaches (Powe 2007; Lancsar and Swait 2014).

Evidence on the content validity of DCE is sparse with only 20 outcomes, which may be an artefact of our systematic review protocol, but could also imply a high level of undiagnosed protest beliefs and a need for more routine measurement. If a high number of respondents across DCE studies hold protest beliefs toward the payment vehicle or the policy scenario, this challenges the usefulness of the method in environmental decision-making. Similar concerns apply to perceived inconsequentiality and difficult-to-comprehend DCE survey designs, which may result in random responses instead of choices that would maximize utility. In particular, the identification of protesters is subjective and case study specific, and there is no agreement on how to handle protest responses in econometric modelling (Meyerhoff et al. 2014). We have observed a move towards ever more sophisticated econometric model specifications to analyse DCE data, but argue that survey design remains very important for improving DCE's reliability and validity. The use of debriefing questions is a simple but useful diagnostic tool to examine content validity, but we found them to be rarely reported. However, as with self-reported attribute-attendance, scholars have questioned the extent to which respondents' self-reported measures are reliable (Hess and Beharry-Borg 2012).

Whilst the evidence is too heterogeneous to identify environmental goods for which reliability and validity are particularly problematic, we want to highlight the importance of testing reliability and validity in LICs for which we only found 12 studies. The scant evidence may be attributed to cost considerations, or a greater focus on delivering valuations commissioned for policy work rather than investigating methods (Whittington 2010). At least until more evidence emerges, researchers should be particularly cautious when designing DCEs in LICs given the additional challenges that DCE researchers may face in these countries (Mangham et al. 2009).

Finally, we see deliberative methods as a promising approach to understanding reliability and validity both in LICs and HICs. However, group-based deliberative approaches should be treated with caution since they may create scope for researcher-induced bias particularly when deliberation is used as part

of the DCE. The “time-to-think” protocol (e.g. by Cook et al. 2007, a health economics' application) could avoid some of the drawbacks of participatory valuation and allow each individual to speak out and think free from wider group influence or social norms prevailing in group-based valuation approaches. Such “time-to-think” protocols could mimic reality better since respondents can talk to other household members and the survey setting is less restricting (Whittington 2010).

2.4.4 Limitations of the systematic review approach

Although we took care to avoid missing relevant articles e.g. by using a training set, the search strings may be insufficiently sensitive to capture all available studies on the reliability and validity of DCE in the non-market environmental valuation literature. Adding more search terms might have permitted a more sensitive search, but would have been at the cost of specificity (Pullin and Steward, 2006). The diverse ways in which reliability and validity are conceptualised and reported in the literature prevent a more comprehensive search without much greater resources. The use of consistent terminology in validity and reliability testing would assist future systematic reviews. Nevertheless, we believe that the results are representative of studies testing reliability and validity and provide a good assessment of the extent to which the peer-reviewed literature has reported empirical evidence of the reliability and validity of DCEs. Given the diversity and relative paucity of studies, especially the very small sub-samples for specific types of validity tests, we did not attempt a meta-analysis. Moreover, the very different contexts, treatments and DCE designs prohibit us from identifying factors that determine whether a specific method made DCEs more likely to be reliable and/or valid. Unless determining these factors was specifically the focus of a controlled test (within a study), such an analysis would need a large number of studies to control for confounding variables.

2.5 Conclusions

We systematically reviewed studies from 2003 to February 2016 that incorporated tests of reliability and validity of the DCE method when valuing non-market environmental goods. DCE results are frequently susceptible to modest changes in survey designs and poorly predict respondents' actual behaviour (albeit in somewhat artificial conditions). As expected, DCE outcomes were consistent with other SP based methods (mostly CVM) that share the same underlying theory. Overall, the evidence shows that a considerable proportion of respondents' choices were inconsistent with the utility axioms assumed by DCEs, and evidence on the content validity of DCE is sparse. These results demonstrate a need to increase the evidence base on the reliability and validity of DCE in the environmental valuation literature. As DCE researchers always face uncertainties and difficulties in designing surveys, replicating reliability and validity tests would inform best practices in terms of alternative design approaches and give users of the DCE results, whether for policy-making or benefit transfer exercises, a sense of the level of confidence one can have in those results.

Despite the diverse, scant and inherently subjective nature of the evidence on the reliability and validity of DCE, it is sufficient to suggest considerable caution when using DCEs to inform decision-making. Arguably, the debate on the reliability and validity of DCE and other SP methods may never be settled as no decisive experiment exists (Whittington 2010). Judgments about reliability and validity depend on not only the statistical significance of test results but also their economic importance. They are therefore specific to the context and intended uses of DCE, which are extremely diverse. In many environmental contexts, SP techniques may be the only valuation method available, and we expect that DCEs will continue to attract significant resources. However, their reliability and validity are still questionable and therefore require a similar level of attention. In particular, combining DCEs with revealed preference data is one promising research avenue in the environmental field that has been little explored. Likewise, the use of participative and deliberative processes, qualitative approaches, and other interdisciplinary techniques offer opportunities for improving the DCE method.

SUPPLEMENT 1: TEST LIBRARY

Reliability / validity check	Author	Year	Title
Reliability	Alvarez-Farizo et al	2007	Choice modelling at the "market stall": Individual versus collective interest in environmental valuation
Reliability	Bateman et al	2009	Reducing gain-loss asymmetry: A virtual reality choice experiment valuing land use change
Reliability	Bliem et al.	2012	Temporal stability of individual preferences for river restoration in Austria using a choice experiment
Reliability	Carlsson et al	2011	Is Fairness Blind?--The Effect of Framing on Preferences for Effort-Sharing Rules
Reliability	Day et al	2012	Ordering effects and choice set awareness in repeat-response stated preference studies
Reliability	Kenter et al	2011	The importance of deliberation in valuing ecosystem services in developing countries-Evidence from the Solomon Islands
Reliability	Kragt	2013	The Effects of Changing Cost Vectors on Choices and Scale Heterogeneity
Reliability	Liebe et al	2012	Test retest reliability of CE in environmental valuation
Criterion validity	Bosworth and Taylor	2012	Hypothetical Bias in Choice Experiments: Is Cheap Talk Effective at Eliminating Bias on the Intensive and Extensive Margins of Choice?
Criterion validity	List et al	2006	Using Choice Experiments to Value Non-market Goods and Services: Evidence from Field Experiments
Criterion validity	Ready et al	2010	Using Respondent Uncertainty to Mitigate Hypothetical Bias in a Stated Choice Experiment
Criterion validity	Taylor et al	2010	Exchange Rules and the Incentive Compatibility of Choice Experiments
Convergent validity	Christie and Azevedo	2009	Testing the Consistency Between Standard Contingent Valuation, Repeated Contingent Valuation and Choice Experiments
Convergent validity	Scarpa et al	2003	Valuing indigenous cattle breeds in Kenya: an empirical comparison of stated and revealed preference value estimates
Theoretical validity	Campbell et al	2008	Incorporating discontinuous preferences into the analysis of discrete choice experiments
Theoretical validity	Scheufele and Bennett	2012	Response Strategies and Learning in Discrete Choice Experiments
Theoretical validity	Lew and Wallmo	2011	External Tests of Scope and Embedding in Stated Preference Choice Experiments: An Application to Endangered Species Valuation
Theoretical validity	Shapansky et al	2008	Assessing information provision and respondent involvement effects on preferences
Content validity	Barkmann et al	2008	Confronting unfamiliarity with ecosystem functions: The case for an ecosystem service approach to environmental valuation with stated preference methods
Content validity	Meyerhoff and Liebe	2008	Do protest responses to a contingent valuation question and a choice experiment differ?
Content validity	Vossler et al	2012	Truth in Consequentiality: Theory and Field Evidence on Discrete Choice Experiments
Qualitative techniques	Arana and Leon	2009	Understanding the use of non-compensatory decision rules in discrete choice experiments: The role of emotions
Qualitative techniques	Powe et al	2005	Mixing methods within stated preference environmental valuation: choice experiments and post-questionnaire qualitative analysis

SUPPLEMENT 2: INCLUDED STUDIES

IDs	References
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S2	Alemu, M. H., M. R. Morkbak, S. B. Olsen and C. L. Jensen (2013). "Attending to the Reasons for Attribute Non-attendance in Choice Experiments." Environmental & Resource Economics 54(3): 333-359 DOI: 10.1007/s10640-012-9597-8.
S3	Alvarez-Farizo, B., N. Hanley, R. Barberan and A. Lazaro (2007). "Choice modeling at the "market stall": Individual versus collective interest in environmental valuation." Ecological Economics 60(4): 743-751 DOI: 10.1016/j.ecolecon.2006.01.009.
S4	Arana, J. E. and C. J. Leon (2009). "Understanding the use of non-compensatory decision rules in discrete choice experiments: The role of emotions." Ecological Economics 68(8-9): 2316-2326 DOI: 10.1016/j.ecolecon.2009.03.003.
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S14	Bosworth, R. and L. O. Taylor (2012). "Hypothetical Bias in Choice Experiments: Is Cheap Talk Effective at Eliminating Bias on the Intensive and Extensive Margins of Choice?" B E Journal of Economic Analysis & Policy 12(1): 58-58 DOI: 10.1515/1935-1682.3278.
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S61	Luisetti, T., I. J. Bateman and R. K. Turner (2011). "Testing the Fundamental Assumption of Choice Experiments: Are Values Absolute or Relative?" Land Economics 87(2): 284-296.
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Chapter 3. QUALITATIVE AND QUANTITATIVE EVIDENCE ON THE TRUE LOCAL WELFARE COSTS OF FOREST CONSERVATION IN MADAGASCAR: ARE DISCRETE CHOICE EXPERIMENTS A VALID EX-ANTE TOOL?[29]

ABSTRACT

If protected areas (PAs) aim to achieve conservation through the enforcement of use restrictions, they will inevitably impose local welfare costs. Despite decades of recognition of these costs, evidence suggests that compensation is often delayed, incomplete or non-existent. Predicting the welfare impacts of PAs before their establishment could help with the design of compensation schemes. Discrete choice experiments (DCEs) are increasingly used for ex-ante evaluations but their validity is largely untested in low-income settings. Using a case study of a new REDD+ (Reducing Emissions from Deforestation and Forest Degradation) project in eastern Madagascar, we explore the validity of DCE in two ways: i) whether DCE results are affected by respondents' experience of conservation, ii) whether DCE results have high theoretical and content validity. We surveyed households who have varying degrees of experience of restrictions on swidden agriculture. We also qualitatively debriefed a sub-sample of respondents to better understand their thought processes. Latent class analysis shows that conservation experience significantly influences DCE outcomes. Households more experienced with forest protection are not willing to trade-off rights to clear forest for swidden agriculture with any compensatory interventions whereas less experienced households highly favour support for alternative agricultural techniques and a secure right to clear one hectare of forest. Although the latent class model shows apparent non-attendance to some attributes (e.g. cash payments), qualitative debriefings suggest that respondents instead expect relatively low or no utility from the given attributes and hence have theoretically valid preferences. Similarly, the DCE has generally high content validity. We conclude that although DCE can elicit current preferences in this context, DCE is not a valid ex-ante tool for estimating compensations because people who lack experience of restrictions may be unable to estimate the actual welfare impacts of such a long-term and complex intervention. Our study suggests that it is hard to robustly estimate compensation in advance of an intervention, there is therefore a need to rethink conservation approaches, and the feasibility of achieving fair compensations for conservation-imposed restrictions.

[29] **Rakotonarivo, O.S.** Jacobsen, J.B., Larsen, H.O., Jones, J.P.G., Nielsen, M.R., Ramamonjisoa, B.S., Mandimbiniaina, R., Hockley, N. Qualitative and quantitative evidence on the true local welfare costs of forest conservation in Madagascar: Are discrete choice experiments a valid ex-ante tool? Under review at World Development.

3.1 Introduction

Conserving biodiversity through the establishment of protected areas (PAs) has been the foundation of conservation in the tropics. The number and extent of PAs have increased rapidly in the last decades (Jenkins and Joppa 2009), particularly in least developed countries where they are viewed as an urgent response to the increasing loss of biodiversity. Although there is a wide range of PA categories, most involve some degree of restrictions on access to natural resources which may have negative impacts for the welfare of local communities dependent on those resources. REDD+ (Reducing Emissions from Deforestation and Forest Degradation) is resulting in a further increase in tropical forest areas where access restrictions are imposed on local resource users (Ghazoul et al. 2010).

Despite decades of recognition of these local costs, compensation measures are often delayed, incomplete or non-existent (e.g. Cernea and Schmidt-Soltau 2006). Attempts to provide compensations started with integrated conservation and development projects (ICDPs) in the 1980s, which promoted rural development projects but which generally failed to achieve development on a scale commensurate with the costs (Brandon and Wells 1992). Community-based natural resource management (CBNRM) and related concepts have been pursued to enable communities to participate in the management of natural resources and benefit from these resources (Brosius et al. 1998). Although there are successful cases, CBNRM has often not been able to compensate for the opportunity costs of protection (e.g. Berkes 2004). More recently, Payments for Ecosystem Services (PES) schemes emerged, with an aim to internalise the benefits that people obtain from ecosystems using market or quasi-market exchanges (Grieg-Gran et al. 2005). However, emerging evidence suggests that this is not providing a better outcome for local people and that compliance is mostly obtained by coercion (e.g. Milne 2012). The REDD+ concept, which can be seen as a carbon-focused PES, could be a substantial means to finance the establishment of a new wave of PAs (Harvey et al. 2010). However, the effectiveness of REDD+ social safeguards in adequately compensating local people has also been questioned (Chhatre and Agrawal 2009).

The perceived urgency of conservation may have resulted in the dearth of ex-ante assessment[30] and lack of consideration of alternative policy options and the inclusion of the views of the affected population. All of these may have contributed to compensation failures. In this context, predicting the

[30] Ex-ante assessment methods predict the impact of a program or policy prior to their implementation (Todd and Wolpin 2008).

welfare impacts of PAs before their establishment could provide valuable evidence to improve compensation. However, a major constraint is finding robust methods to estimate welfare impacts in advance. Discrete Choice Experiments (DCEs), a stated preference valuation technique[31] (Freeman 2003), can offer one approach to estimating welfare impacts ex-ante through the construction of hypothetical scenarios (e.g. Cranford and Mourato 2014). While DCE may be prone to hypothetical bias (Hensher 2010b), it may help decision makers predict how respondents would adapt to a policy change and devise compensation mechanisms that would integrate the affected population's needs. Besides, by inferring policy impacts from the trade-offs that respondents make, DCE avoids asking direct questions about the policy being valued and therefore may be useful when valuing sensitive goods, such as illegal activities (e.g. Moro et al. 2013; Nielsen et al. 2014). We conducted a DCE survey with rural households in eastern Madagascar affected by forest conservation to investigate the trade-offs local people would make between the right to clear new forests for swidden agriculture, cash payment compensation, and support for improved rice farming.

Although DCE methods are increasingly used in environmental valuation, their validity, especially in low-income rural settings, is largely untested (Whittington 2010). First, this paper aims to assess the validity of ex-ante DCE conceptualised as the degree to which the method is measuring what the researcher intends it to measure (Bateman et al. 2002). If researchers' aim is to measure the welfare impacts of forest conservation to inform the design of compensation policies, validity therefore concerns how well the DCE method, as an ex-ante impact assessment tool, can achieve this[32]. How well ex-ante assessment can predict the impacts of conservation may depend on the effect of respondents' prior experience with the policy. If experience of forest conservation significantly influences DCE outcomes, this casts doubt on the validity of DCE as a method for predicting welfare impacts and required compensations[33]. Complex and long lasting interventions such as forest

[31] Choice experiments and contingent valuation methods are stated preference techniques which are a set of valuation methods which allow analysts to generate welfare estimates of environmental benefits/damages based on respondents' stated willingness-to- pay or willingness-to-accept estimates. (See Freeman 2003 for an introduction to SP techniques in environmental valuation).

[32] In this study, we define validity as a property of a method *given* a particular intended application of that method. Thus a certain DCE may be valid if the objective is to reveal people's current preferences, but the same DCE may be invalid if the intention is to use it to decide how much respondents must be paid in compensation (or what type of compensations) so that they are left no worse off.

[33] In economics, these compensations are termed "compensating variation" which is the amount of money needed to render a person indifferent to a policy change that would make them worse-off (e.g. price increase or quantity decrease), i.e. it measures the amount of money required to maintain a person's economic welfare, at the level it was at before the change (Freeman 2003).

conservation may have long-lasting effects on household wellbeing which are hard for a respondent to estimate in advance. If less experienced respondents are ill-informed, using their preferences to design compensation measures would be flawed.

Second, this paper aims to examine the theoretical and content validity of our DCE results by assessing how well they conform to the assumptions of the method. The first assumption relates to the continuity axiom of rational choice theory which postulates that DCE respondents need to attend to all attribute levels across each of the alternatives and make "compensatory" trade-offs (Campbell et al. 2008; Campbell et al. 2011). However, it may be difficult to distinguish genuine attribute non-attendance (that is ignorance of an attribute because of an incomprehensible survey design or other concerns not captured or raised by the DCE survey) from a no (or low) preference for given attributes (i.e. low attribute importance translated into a low or zero utility) (Hess et al. 2013). The former is a violation of the continuity axiom, the latter is not. What is observed in DCE results, e.g. apparent patterns of non-attendance to some attributes, may not always tally with respondents' thought processes, and qualitative debriefings that directly examine decision processes, i.e. how people say they made decisions, can help disentangle such issues (Powe et al. 2005; Arana and Leon 2009). Here, we test the extent to which our results conform to the continuity axiom by exploring the processes through which respondents arrive at their choice decisions using qualitative debriefing interviews with a sub-sample of respondents.

Another assumption of the DCE method pertains to the content validity of DCE, i.e. whether the survey descriptions and questions are "clear, plausible, and unbiased" so that respondents are motivated to reveal their true preferences (Bateman et al. 2002). If respondents protest some features of the survey scenario, they may not have the incentives to accurately state their true welfare costs (e.g. Meyerhoff and Liebe 2009). If respondents for instance distrust or misperceive the payment vehicle i.e. the means through which the policy outcome is delivered (Morrison et al. 2000), or they do not believe that their responses could potentially influence policy (Vossler and Watson 2013), their choice decisions may not be valid indications of their preferences. In our qualitative debriefing interviews we examined the extent to which a perceived lack of plausibility of the payment vehicle or consequentiality of the DCE survey may bias the results.

This is the only DCE study we know of that investigates the validity of the DCE results in a low-income setting by explicitly looking at the effect on people's choices of varying exposure to a complex and long-term intervention (in this case restrictions on land use). We also believe it is the only study in a low-income context to enrich a DCE survey with rigorous qualitative data collection approaches which

can considerably help understand the psychological processes leading to respondents' answers. These improvements to established economic valuation techniques for use in a low-income setting have broad applicability for environment and development researchers. However the paper also has significant implications for conservation policies and the design of compensation measures around PAs including PES and REDD+ projects.

3.2 Methods

3.2.1 Case study and sampling procedure

Madagascar's protected area network has been recently expanded from 3.1% of Madagascar terrestrial surface area (1.8 million hectares) in 2003 to 10% in 2012 and now covers most of the remaining natural habitat. The local swidden agricultural system known as *tavy* (De Wilde et al. 2012) has been regarded as the main driver of deforestation in eastern Madagascar. At low population densities *tavy* may be sustainable, but population growth and other internal and external have put this practice in opposition to conservation objectives (Scales 2014a). Clearance of primary forest in the *tavy* system is known specifically as *teviala* and is the main focus of conservation goals and policies. *Teviala* has been criminalized in Madagascar since colonial times, however enforcement has often been weak (Kull 2004). With the support of international donors, the state is currently making a renewed attempt to outlaw *teviala* and coercively enforce the ban. De facto, many forestlands are not subject to well-defined formal property right regimes, though local systems of customary tenure frequently mix with, and evolve in response to, formal state laws (Muttenzer 2010).

To investigate the effect of experience of forest conservation on local welfare costs, we purposefully selected two sites in the eastern rainforests of Madagascar which differed in their exposure to forest protection but were otherwise similar in terms of forest characteristics, market access and remoteness. In one site, Ampahitra, de facto enforcement has been relatively weak until recent conservation measures. Ampahitra is part of the corridor Ankeniheny-Zahamena ('CAZ') which is a new protected area formally gazetted in May 2015 following temporary protection since 2005. It is co-managed by Conservation International and community associations. The other site, Mantadia has a long history of strict conservation and is part of the Andasibe-Mantadia protected area established 20 years ago and managed by Madagascar National Parks. The dominant and indigenous ethnicity in these two study sites are *Betsimisaraka.*

In each site we identified villages that were close to the forest and therefore affected by conservation. In Ampahitra this included all eight villages east of the main road in the *fokontany*[34] of Ampahitra. In Mantadia we included all villages in the *fokontany* of Vohibazaha and Volove. There is poor information available on the location and size of communities in much of rural Madagascar making it

[34] *Fokontany* is the smallest administrative unit in Madagascar

difficult to develop a rigorous sampling frame. Since no map or census of households was available, we constructed the sampling frame in three steps: i) key-informant interviews with *fokontany* authorities to develop a sketch-map of villages, ii) key-informant interviews in villages to collect information on households and hamlets, iii) visits to hamlets in person to cross check information and record GPS locations ensuring that no isolated household was missed out. Building the sampling frame took approximately 50 person-days in each site and approximately 1/3 of the total field work time. We identified in total 417 households in Ampahitra and 241 households in Mantadia and selected a random sample of households stratified by village with the aim of interviewing at least 100 households in each site. Ultimately, 102 and 104 households were surveyed in Ampahitra and Mantadia respectively.

3.2.2 Choice experiment design

The attributes and levels (table 3.1) were informed by three focus group discussions and pilot testing of the design with 50 respondents in nearby villages (the selection of attributes and levels is described in appendix 1, the questionnaire is presented in appendix 2, and the DCE experimental design in appendix 5).

Table 3.1: Attributes and levels of the DCE (reference levels in bold)

Attributes	**Description**	**Levels**	**Coding and Notation**	**Hypotheses** (Expected sign of coefficients with WTA estimates in brackets)
Total cash donations framed as development assistance (3,080 MGA = 1 USD)	The cash donations were framed as development assistance that the household would receive. Review of secondary data and previous literature estimating the local costs of deforestation aided the selection of the payment levels (e.g. Shyamsundar and Kramer 1996; Ferraro 2002).	**0**, 3, 6, 9, 12, 15 ($x10^6$ MGA)	Cash (continuous variable)	More cash increases the average respondents' utility (+)
Number of annual instalments over which the household will receive the total payments	The three levels of instalments allow an estimation of the respondents' discount rates and provides information on the respondents' ability to invest money.	**1**, 10,20	Dummy-coded: Installment10 and installment20	Higher number of installments is expected to decrease the average respondents' utility (-) (due to discounting)
Support for improved rice farming	This attribute is introduced as a sustainable and modern agricultural package that includes productivity enhancing practices such as the use of fertilisers, insecticides and/or herbicides. It involves digging and possibly the construction of terraces for slopes and precludes the use of fire as a way to maintain fertility while not fallowing the land. It also includes material support (e.g. improved seeds, wheelbarrow, spades, etc.).	**No,** yes	Dummy-coded: Support for improved rice farming coded as 1	Improved agricultural practice may increase the average respondents' utility (+)
Teviala (clearance of new forestlands for agriculture)	This attribute has three levels: i) no *teviala* (i.e. strict enforcement of restrictions), ii) a permit for one hectare of *teviala* (a one-off opportunity), iii) free *teviala* (similar to pre-colonial times before criminalization of *teviala*, and de facto to more recent periods of little or no enforcement).	**Free *teviala*** (open forest frontier), 1ha of *teviala* permit and no *teviala* (strict protection),	Dummy-coded: *teviala* 1ha, no *teviala*	Restrictions on *teviala* are expected to decrease the average respondents' utility (-)

3.2.3 Data collection

The DCE survey was piloted in three phases between February and June 2014 in villages near the sampled villages. The actual survey was carried out between July to October 2014. The questionnaire was administered by OSR[35], RM[36] and three enumerators who all held at least a bachelor's degree in agricultural sciences from the University of Antananarivo. Enumerators received two weeks of training from OSR on the theoretical underpinnings of the DCE method, ethical considerations, and how to conduct the survey. Field activities were also supervised by NH[37] who speaks fluent Malagasy and has more than five years' experience of similar field work. Our unit of analysis is the household and interviews were conducted with the household head, his spouse and other household members.

The questionnaire comprised three sections: 1) Socio-economic characteristics of the household including education, household features, land holdings and characteristics, other household assets, and wealth indicators (such as food security), 2) DCE survey. 3) Follow-up questions which examined five aspects of the valuation exercise, four were measured on a five-point Likert scale while the last one is a binary question i) Plausibility of the survey scenario, particularly the payment vehicle, ii) Trust in the institution that is to deliver the cash donations, iii) Consequentiality of the valuation exercise (i.e. how much respondents believe the results would be used to inform policy), iv) Perceptions of the support for improved rice farming, and v) Perceptions of the benefits of forest protection.

OSR also conducted debriefing interviews the following day with a sub-sample selected to represent the full range of DCE responses (N=25 from 206 respondents) to examine their decision making processes. The interview guide focused on how respondents organized their thoughts and arrived at their choice decisions. OSR enquired specifically about how their lives would be under different scenarios, within which timeframe they were conceptualizing the policy being valued (restriction of forest clearance for swidden agriculture), whether cash transfers would substitute for *teviala* both at the practical level (ability to invest money) and at the philosophical level (e.g. cultural values), how much they valued freedom of choice, whether they were also thinking of other aspects exogenous to the DCE survey (e.g. experience of outside interventions), and how much they considered the effects

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[36] Rina Mandimbiniaina

[37] Neal Hockley

of their choices on their future descendants and on wider society. The number of interviewees was determined by data saturation.

We explained the DCE survey to respondents using dolls and large pictures (Appendix 4) which helped respondents engage with the survey and framed it as a game to desensitize the illegal nature of *teviala* (an approach used by Nielsen and colleagues, (2014) when valuing illegal bushmeat hunting in Tanzania). The full survey took one to two hours per household with some warm-up steps to give respondents some practice and ensure they understood the task of making trade-offs in a DCE survey. Our study protocol was reviewed and approved by the Bangor University's Ethics Review Committee.

3.2.4 Data analysis

3.2.4.1 Analysis of DCE results

Since respondents may have heterogeneous preferences, we estimated a latent class model (LCM) using the pooled dataset to identify the sources of heterogeneity and segments of respondents with similar preferences (see appendix 6) (Boxall and Adamowicz, 2002). Choosing the number of classes for the LCM involves a trade-off between model simplicity and explanatory power, and should be informed by the significance of parameter estimates, analyst judgment regarding the interpretability of the model results, the Akaike Information Criteria (AIC) (Scarpa and Thiene 2005). Based on these criteria, we selected a 4-class model.

The utility function of an individual *n* facing a choice between two experimentally created alternatives and a reference level alternative can be described as:

$$U_{ni} = \begin{cases} V(ASC, X_{ni}, \beta_k) + \varepsilon_{ni} \text{ if } i\text{=reference level alternative, otherwise,} \\ V(X_{ni}, \beta_k) + \varepsilon_{ni} \end{cases} \quad \textbf{(3.1)}$$

Where U_{ni} is the utility function for individual *n*, for alternative *i*. *V* is the observed indirect utility, which is a function of X_{ni}, a vector of observable attributes and associated fixed parameters β_k. We specify an alternative specific constant (ASC) for the reference level, and free *teviala*, and a Gumbel distributed error term ε_{ni} as a means of capturing the unobservable factors beyond attributes present in the choice sets. We specify the utility function (U_{ni}) of an individual n of the alternative *i* as:

$$U_{ni} = \beta_1\, cash + \beta_2\, installment\ 10 + \beta_3\, installment\ 20 + \beta_4\, support\ for\ improved\ rice\ farming + \beta_5\, teviala\ 1\ ha + \beta_6\, No\ teviala + \varepsilon_{ni} \quad \textbf{(3.2)}$$

The latent class model is estimated as four conditional logit models, in which the class membership probability is estimated simultaneously. The class membership probability can further be explained by

possible sources of heterogeneity across segments. We included the household-level experience variable to explain segment membership as well as six relevant socio-demographic variables (table 3.3).

The household-level experience is a composite variable combining two parameters:

i. The number of years the site has been de facto exposed to forest protection.

For respondents in Mantadia this is 20 years as the Park was formally established in 1994. Ampahitra first received provisional protection status in 2007 but this was formalized only in October 2013 (Ruta 2014). Penalization of two residents for *teviala* was reported by respondents from 2009 so we classified respondents in Ampahitra as having experienced 5 years of forest protection from 2009 to 2014 (but the results are not sensitive to varying this from 4 to 7 years).

ii. The immigration status of the individual household, i.e. how long the household has resided in the area.

The composite household-level experience variable takes the smaller value of any of these two variables, e.g. if the household resides in Ampahitra but has been living in the area for only 3 years, its household-level experience is 3 years whereas it equals 5 if the household has been in the area for 8 years.

Attitudinal variables in the class membership probability function may create endogeneity problems (Hess and Beharry-Borg 2012) . Attitudinal data are actually functions of latent attitudes, i.e. they are not exogenous to the choice variables and are not a genuine expression of fundamental attitudes (Provencher and Moore 2006). We therefore estimated the ex post individual segment membership probabilities and used this to calculate probability weighted values for these variables (Hess et al. 2011) using equation 3.3:

$$X_{segment1} = \frac{\sum_{n=1}^{N} \pi \,.\, X_n}{\sum_{n=1}^{N} \pi_{n1}} \qquad \textbf{(3.3)}$$

Where *N* is the number of respondents, *X* is the value of the attitudinal variable, and π_{n1} is the estimated probability of respondent *n* falling into segment 1, computed from the segment allocation model. Data were analysed with Nlogit 5.0 and Stata 12.

3.2.4.2 Qualitative debriefings

Interviews, which lasted from 30 minutes to one hour, were audio recorded after obtaining consent and professionally transcribed for thematic analysis (Bryman and Burgess 1994; Braun and Clarke 2006; Kvale and Brinkmann 2009). We used a coding scheme intended to generate themes or general patterns which answer our two main research questions: *1) Can DCE predict local welfare costs and required compensations (as measured by the effect of respondents' experience of forest conservation on respondents' preferences), 2) Do DCE results have high theoretical and content validity (i.e how well do DCE responses conform to the assumptions of the method?)*. OSR assigned codes to data segments using Nvivo 10, which were then grouped into larger themes. Codes and themes were constantly revised based on new insights from data analysis. RM cross-checked the codes, the quality of the transcriptions, and checked the veracity of the translation of the extracts.

3.3 Results

3.3.1 Sample characteristics

Households across the two sites were similar in most socio-economic characteristics (table 3.2). In both sites mean household size is 6 and household heads averaged less than three years of schooling. Food security is low with households having sufficient food for only half of the year on average. However there were differences in variables which may be affected by conservation restrictions: only 5% of household heads in Mantadia were migrants compared to 75% in Ampahitra, of which half had arrived in the last 10 years. 96% of households accessed at least one of their plots by inheritance in Mantadia versus 30% in Ampahitra, while only 17% accessed their lands by forest clearance in Mantadia versus 42% in Ampahitra. This confirms the weaker enforcement of forest protection and consequently high immigration and recent land clearance in Ampahitra. The *Betsimisaraka* ethnic group forms 98% of the total sample in Mantadia and 80% in Ampahitra. Table 3.3 presents the summary statistics of the seven socio-economic variables included in the LCM as well as the household-level experience composite variable.

Table 3.2: Socio-economic characteristics

Descriptive statistics of socio-economic variables		Ampahitra (N = 102)		Mantadia (N = 104)		p-value
		Mean	*Std. Dev*	Mean	*Std. Dev*	
Household head's age (years)		38.87	*14.56*	41.49	*15.26*	0.21
Household age (years)		12.65	*10.52*	14.12	*10.38*	0.35
Education: Years of official schooling of the household head		2.15	*2.78*	2.98	*2.03*	0.11
Household size		5.79	*2.42*	5.23	*2.50*	0.10
Quantity of seeds used in the tavy plots (in kapoaka - as a proxy of land holding)		208.91	*198.27*	240.29	*170.87*	0.23
Walking distance to the centre of the fokontany (in minutes)		150.98	*64.12*	161.67	*66.33*	0.32
Total livestock owned by the household (in tropical livestock unit - Chilonda and Otte 2006)		0.76	*1.80*	0.59	*0.86*	0.09
Food security (number of months that the household has enough to eat)		5.87	*3.13*	7.51	*2.93*	0.08
Was the household head born in this village? (as an indicator of immigration status)	1=YES; 0=NO	0.25	*0.43*	0.60	*0.21*	0.00**
Access to at least one of the plots is by inheritance	1=YES; 0=NO	0.31	*0.46*	0.96	*0.19*	0.00***
Access to at least one of the plots is by forest clearance	1=YES; 0=NO	0.41	*0.49*	0.17	*0.38*	0.00***
The respondent household uses a paddy field	1=YES; 0=NO	0.26	*0.44*	0.18	*0.39*	0.00***
The respondent household uses perennial crop field	1=YES; 0=NO	0.63	*0.48*	0.81	*0.39*	0.00***
Ethnicity (1=YES; 0=NO)	Betsimisaraka	0.79	*0.40*	0.98	*0.14*	0.00***
	Bezanozano	0.14	*0.34*	0.00	*0.00*	
	Merina	0.03	*0.17*	0.01	*0.10*	
	Mixed	0.03	*0.17*	0.00	*0.00*	
Household has firewood as the only source of light	1=YES; 0=NO	0.18	*0.38*	0.04	*0.19*	0.00**
Has the household benefited from the World Bank development project?	1=YES; 0=NO	0.17	0.05	0.17	0.08	0.31
Do respondents have experience of the technical rice farming project? 3: "I have done it", 2: "I have seen it", 1: "I have heard about it", 0: "I have never seen nor heard about it"	Scale 0 to 3	0.75	0.92	0.98	1.07	0.06

A chi-square test was applied to compare the distributions of the dummy coded variables between the two samples and mean comparison t-tests for continuous variables. Note: *, ** ➔ Significance at 1%, 5% levels**

Table 3.3: Covariates explaining LCM segment membership

Variables	Description		Summary statistics Ampahitra (N = 102)	Mantadia (N = 104)
Household-level experience	Composite numeric variable composed of the site's history of conservation and the length of the household's residence at the site.	Range	1-5	2-20
		Mean	4.3	19.32
		Std. dev	1.2	3.17
Young households	Binary variable indicating 5 or less years of household formation (highly correlated with the age of the household head) [1=YES; 0=NO]	YES	32 (31%)	40 (38%)
Literacy	Binary variable indicating whether the household head is literate. [0=NO; 1=YES]	YES	56 (55%)	82 (78%)
Tavy seeds	Numeric variable measuring the quantity of rice seed required to plant the households' swidden agriculture (*tavy*) plots, measured in *kapoaka* (a local unit of measurement, roughly equivalent to a cup). This variable is used as a proxy for the area of swidden agricultural plots.	Range	0-1900	0-1600
		Mean	208	225
		Std. dev	175	184
		Median	201	180
Livestock owned	Numeric variable indicating the total livestock ownership of a household measured in 'Tropical Livestock Units' (Chilonda and Otte 2006). This variable is used as an indicator of household wealth.	Range	0-8.23	0-6.25
		Mean	0.86	0.63
		Std. dev	1.9	0.86
		Median	0.10	0.17
Beneficiary of World Bank project	Recent World Bank development projects in 2013 aimed at compensating households potentially affected by forest protection and encouraging pro-conservation attitudes. Each beneficiary was provided with either beekeeping, poultry or agricultural support. This variable is used as an indicator of experience of development interventions [1=YES; 0=NO]	YES	17 (17%)	18 (17%)
Experiences of the improved rice farming	Binary variable indicating whether household has any experience of the improved rice farming [1=YES; 0=NO] (1=household has implemented it or seen others doing it, 0= household has only heard about it or has neither heard nor seen it)	YES	22 (22%)	39 (37%)

3.3.2 Latent segments and their characteristics

The alternative specific constant (ASC) takes the value of 1 for the alternative describing the reference level (set as no cash, no support for rice farming and free *teviala)*, in all four segments, the ASC is not significant (table 3.4), indicating that the full value of the proposed alternatives are captured by the attributes.

The household-level experience of forest conservation and other socio-economic variables are significantly associated with preference heterogeneity (table 3.4). Segment 1 households ("holdouts", 33% of households) prefer the free *teviala* scenario regardless of compensation and are composed

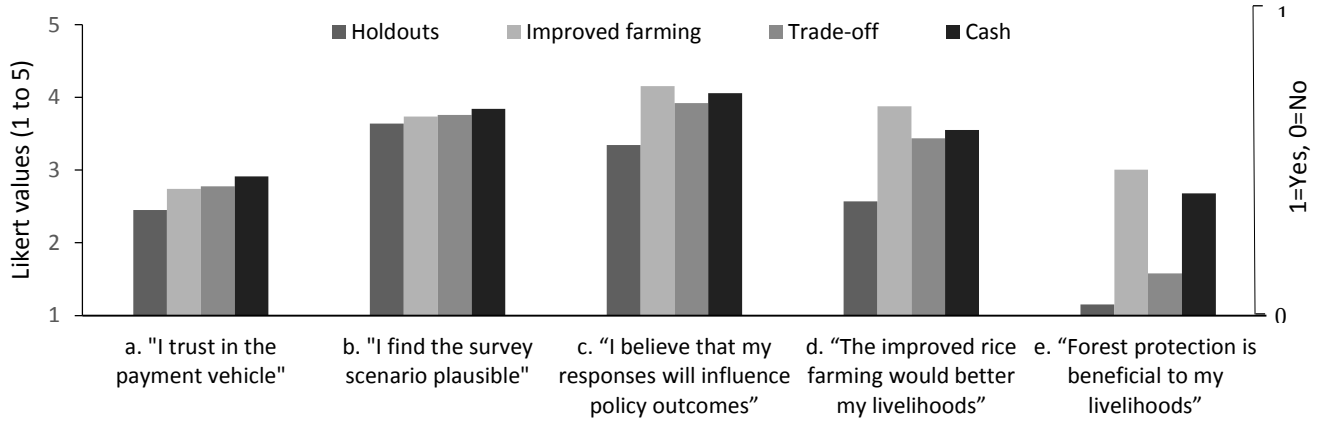
mostly of more experienced households who are more likely to have been left out of the World Bank funded development projects. Conversely, households in segment 2 (“improved farming”, 30%) are likely to be relatively less experienced in forest use restrictions and preferred secure rights over one hectare of *teviala* to an open forest frontier. They also positively and highly value the technical and material support for improved rice farming. These inexperienced households are likely to have benefited from the development project. Respondents in segment 3 (“trade-offs”, 15%) are composed of households with and without experience of conservation restrictions who traded off the cash donations with use restrictions, i.e. they positively value the cash payments and get disutility from swidden agriculture restrictions. This segment is composed of newly formed households with young household heads. Finally, members of segment 4 (“cash”, 21%) value only the cash payments (highly significant and positive), and comprise experienced and inexperienced households. These cash preferring respondents express a non-monotonic preference for the instalment attributes, favouring the medium timeframe (10 years) to one lump sum payment and strongly disfavouring longer timeframes (20 years). Literacy rate, respondents’ experience of support for rice farming, tavy seeds and livestock owned do not significantly explain segment membership.

Table 3.4: Latent segments and their determinants. The model is estimated on the pooled dataset. For each latent segment, the coefficients show the effects on utility of changes in each attribute level on the average respondent relative to the reference level of no cash, no support for rice farming, and free *teviala*. For the instalment attributes, the base level is one year.

	SEGMENT 1: "Holdouts"		**SEGMENT 2 "Improved farming"**		**SEGMENT 3 "Trade-offs"**		**SEGMENT 4 "Cash"**	
	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error
Cash	-0.112	0.180	0.068	0.098	.136**	0.071	.389**	0.214
Instalment 10	1.332	1.257	0.820	1.700	0.181	0.508	1.411**	0.624
Instalment 20	1.987**	0.892	1.138	0.821	0.592	0.516	-2.215**	0.793
Support for improved rice farming	-0.299	0.608	4.447***	1.208	0.171	0.600	-0.380	1.091
Teviala 1ha	-3.264***	0.85	4.978***	1.141	-1.095*	0.827	-0.795	1.726
No *teviala*	-6.229**	2.748	1.118	1.851	-1.801**	1.420	-1.175	3.119
ASC	0.929	0.812	-2.188	1.666	-0.369	0.904	-3.446	1.259
Segment size (%)	33.3		30.2		15.4		21.1	
	Explanatory variables of class probability							
	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error	Coefficient	Std. error
Constant	-1.532*	0.798	1.325*	0.697	-3.153**	1.491	Fixed	
Household-level experience	**0.093*** **	**0.038**	**-0.137*** **	**0.050**	**0.038**	**0.061**	**Fixed[38]**	
Young household (1=yes, 0=no)	1.004	0.736	0.230	0.791	2.256**	0.899	Fixed	
Literacy (1=literate, 0=illiterate)	0.082	0.699	-0.242	0.656	1.040	0.985	Fixed	
Land holdings (tavy plots proxied by seeds)	0.003	0.001	0.001	0.001	.004	0.002	Fixed	
Livestock owned (tropical livestock unit)	0.041	0.015	0.035	0.030	.009	0.001	Fixed	
Experiences of the technical rice farming (1=yes, 0=no)	-0.556	0.570	-0.673	0.649	-0.293	0.7918	Fixed	
Beneficiary of World Bank projects (1=yes, 0=no)	-0.269*	0.776	1.590*	0.917	-0.515	1.319	Fixed	
Log-likelihood	-847.067							
McFadden's pseudo R^2	0.3767							
AIC/n	1.450							
Obs.	1236 (N = 206)							
Note: ***, **, * ➔ Significance at 1%, 5%, 10% level.								

[38] Note that the parameters for segment 4 ('Cash') were fixed due to their normalization during estimation.

The follow-up data show that the majority of respondents in the four segments reported neutral views toward the independent institution which is to manage the cash donations over time (figure 3.1). They all tended to believe the cash payments to be plausible, with no large disparity between the four segments. Segment 1 "holdouts" scored the lowest for the belief in the consequentiality of the valuation exercise, as well as for the perceptions of the technical rice farming and the ecological benefits of forest protection whereas segment 2 "improved farming" believed in the consequentiality, was very optimistic toward the support for rice farming and positively valued forest protection. These patterns generally support the choice patterns of these segments.



a.	**Trust in payment vehicle**	"I trust that the independent institution will transparently and effectively manage the cash donation over time"
b.	**Plausibility of the cash payments**	"A donor genuinely interested in development would donate cash instead of usual development projects (e.g. improved rice farming)"
c.	**Perceived consequentiality of the DCE survey**	"I believe that the results of this research would be used to inform policy on forest conservation"
d.	**Perceptions of the support for rice farming**	"I believe that support for improved rice farming would better my livelihoods"
e.	**Perception of the benefits of forest protection**	"Forest protection provide benefits which are important to my livelihoods"

***Statements a. b, c, d are based on a five-point Likert scale: 1='strongly disagree', 2='disagree', 3= 'neither disagree nor agree', 4='agree', 5='strongly agree' Statement e is based on a binary question (1=Yes, 0=No)**

Figure 3.1: Probability weighted attitudinal variables calculated from the class membership probabilities of the latent segment model

3.3.3 Respondents' rationale for their choice decisions

For each of our qualitative debriefing interviews, we assigned each interviewee (n=25) to one segment based on the highest ex-post individual class membership probability from the LCM) (table 3.5).

Table 3.5: Socio-economics of the interviewees and LCM segments (n=25)

ID	LCM segments	Years exposed to de facto enforcement (average)	Indigenous (Yes=1, No=0)	Years of residency	Household-level experience	Site	Ethnicity: *Betsimisaraka* (Yes=1,No=0)	Household age	Household head age	Own paddy fields (Yes=1,No=0)	Tavy lands owned (average seeds)	Beneficiary of compensatory project (Yes=1,No=0)	Member of community associations (Yes=1,No-0)
I1	Holdouts	5	0	1	**1**	Ampahitra	1	2	22	0	150	0	0
I2	Holdouts	5	0	1	**1**	Ampahitra	1	1	21	0	100	0	0
I3	Holdouts	20	1	NA*	**20**	Mantadia	1	23	47	0	500	1	1
I4	Holdouts	20	1	NA	**20**	Mantadia	1	15	45	0	300	1	0
I5	Holdouts	20	1	NA	**20**	Mantadia	1	20	53	0	300	0	0
I6	Holdouts	20	1	NA	**20**	Mantadia	1	30	58	0	100	0	0
I7	Holdouts	20	1	NA	**20**	Mantadia	1	6	37	0	200	0	0
I8	Improved farming	5	0	10	**5**	Ampahitra	1	3	21	0	480	1	1
I9	Improved farming	5	0	13	**5**	Ampahitra	0	20	45	0	225	1	1
I10	Improved farming	5	1	NA	**5**	Ampahitra	1	18	46	0	100	0	0
I11	Improved farming	5	0	14	**5**	Ampahitra	0	16	38	1	100	1	1
I12	Improved farming	5	1	NA	**5**	Ampahitra	1	3	20	0	500	0	1
I13	Improved farming	5	0	4	**4**	Ampahitra	0	13	45	0	150	1	1
I14	Improved farming	5	0	7	**5**	Ampahitra	1	15	37	0	100	0	0
I15	Improved farming	5	0	16	**5**	Ampahitra	1	13	60	1	50	0	0
I16	Improved farming	20	1	NA	**20**	Mantadia	1	10	63	1	250	1	0
I17	Improved farming	20	1	NA	**20**	Mantadia	1	10	34	1	200	1	0
I18	Trade-offs	20	0	4	**4**	Mantadia	0	10	42	0	425	1	0
I19	Trade-offs	20	0	NA	**20**	Mantadia	1	16	39	0	500	0	0
I20	Trade-offs	20	1	NA	**20**	Mantadia	1	1	24	0	150	0	0
I21	Cash	5	0	7	**5**	Ampahitra	1	17	30	1	50	0	0
I22	Cash	20	1	NA	**20**	Mantadia	1	40	55	0	200	1	1
I23	Cash	20	0	2	**2**	Mantadia	0	1	21	0	100	0	0
I24	Cash	20	1	NA	**20**	Mantadia	1	8	36	1	150	1	1
I25	cash	20	1	NA	**20**	Mantadia	1	41	64	1	160	1	1

*Years of residency: NA if respondent is indigene

Below we highlight similarities and differences within each segment. We also describe any outliers in the "holdout" and "improved farming" segments (i.e. respondents with similar patterns of DCE responses but who differed from the rest of the segment on the household-level experience of conservation).

3.3.3.1 Segment 1 "Holdouts" (33% of the sample)

This segment is mostly composed of more experienced households, i.e. households who have been exposed to restrictions for a relatively longer period. They (n=7, I1-I7 – table 3.5) are not willing to trade-off *teviala* with either cash or support for rice farming.

Holdouts express rational utility maximizing arguments in favour of *teviala*: many claimed (I1, I2, I4, I5) that the crops they will be able to harvest from an open forest frontier scenario and the utility they would thereby expect far outweigh the cash payments offered. They believe that *teviala* is sustainable and can continue across many generations, they are confident that they will be able to produce the highest and most reliable yields from forest lands given the expertise they have acquired over centuries of trial and error. They don't value cash payments due to their limited opportunity to invest them (such as remoteness and lack of market access).

Some (I5 to I7) also emphasised the important cultural values of *teviala* practices, how they make a living is deeply rooted in their cultural norms and they find it hard to imagine alternative ways of life. They also talked of the importance of compliance with *tavy* rituals and submission to traditional authorities (*tangalamena)* with regards to access to new lands.

Holdouts also anchored their choices on concerns for their future descendants' needs. Land from *teviala* is seen as the most valuable inheritance they can leave their children and they considered accepting cash as a self-centred behaviour, betraying their future descendants' rights and needs:

> "So supposedly, we will receive cash for 10, 20 years, and that's it? Then what are we supposed to do since we cannot do *teviala* anymore, what would happen to my children and my grandchildren? Let it be very clear, if cash payments will flow every year, forever, just as people who worked for the government (*fonctionaire*) are still receiving retirement pension, then we would be in, otherwise, no." I6 (indigene, 58 years old, Mantadia)

This segment also expressed concern that only the elites and the socially well-connected households would benefit from external help, leading to suffering by the most vulnerable groups often most affected by restrictions on land uses. Interestingly, this group does not recognize any ecological

benefits from the forests. These perceptions were not altered by further probing about examples of regulating services such as climate regulation, erosion control, and cleaner air.

Many interviewees in this segment (I3 to I7) were distrustful of the proposed novel agricultural technique which they believe is not suited to the conditions under which they farm as exemplified by the statement: "rice cannot be grown without fire, you simply can't." (I7, indigene, 37, Mantadia). They have been disappointed by the training provided and the lack of follow up provided by similar interventions in the past and claimed that these new techniques require substantial start-up funds and support that external agents failed to deliver. This contradicts the LCM results which suggest that the experience of support for improved rice cultivation does not significantly explain the grouping identified[39].

Two outliers (I1 and I2) exhibited the same choice patterns as segment 1 but are young households in Ampahitra, recent migrants and hence lacking long experience of use restrictions. They share the concerns of the rest of the segment over land scarcity but for these outliers, choices are principally driven by land scarcity caused by the rapid immigration and high level of conflict over access to land rather than experience of forest use restrictions. As I2 (migrant, 22, Ampahitra) said:

> "I came too late, there are no unowned forests left nearby, we have to borrow fallow lands which are not very fertile. As I recently got married, I haven't got my share, I have to acquire my own lands to feed my family, but I do not dare clear forests as they all have owners."

3.3.3.2 Segment 2 "Improved farming" (30%)

These households have been exposed to forest use restrictions for a relatively shorter period. They (n=10, I8-I17) positively prefer a one-off, one hectare, legal forest clearance permit to strict protection and unanimously associate this with secure individual land tenure which they value highly. Since competition for new forestlands is becoming increasingly fierce where forest protection is weakly enforced, the longer established households or indigenes among this group believe that legal forest tenure would better enable them to establish claims over forestlands. They fear losing their forest and fallow lands to the ongoing influx of migrant smallholders. They despise the military enforcement of strict forest protection and argue that they are in a much better position to protect their forests than any state representatives as long as they have legal tenure.

[39] Experiences of the improved rice farming is insignificant in all four segments, i.e. the four segments identified by the LCM are not significantly different with regard to their experience of the improved rice farming.

Many of this segment claim that they do not intend to clear the one hectare of forestlands but preserve it for their descendants who can clear it if they do not have better options.

> "The way we perceive things now, our children may perceive it differently, they may no longer want to protect these forests. I think this will depend on their education and whatever alternative livelihoods they find. For instance, if they get some education, they may find other options, who knows, they may decide to move to town. Otherwise, they may just clear these forests. Only fate will decide…" I12 (indigene, 20, Ampahitra)

These households also highly favour the support for improved rice farming, although their experience of such agricultural interventions is generally limited. These respondents, especially migrants, often associate the proposed improved rice farming simply with paddy fields and digging, a practice they are familiar with. Indigenes are also willing to try the approach but feel the topography with few exploitable valleys constrains the likely success. Nonetheless, they perceive the technical and material support for agricultural intensification as more bankable than cash.

This segment also slightly preferred the strict forest protection attribute to an open forest frontier (although not statistically significant). Perceived societal benefits of forest conservation (positive externalities including ecological services) contribute to these preferences, as well as the fear of losing land to the ongoing influx of migrants.

Like the 'holdout' group, this group tend not to value cash payments highly due to their perception of the limited opportunities to invest cash (n=10). Some (I9, I16, and I17) mentioned they would prefer communal investment (such as improved market access through road construction or health centres). One respondent (I9) associated the cash donations with credit which he perceived as extremely damaging and prone to hidden agendas, noting that nothing is ever free.

Just as for the holdouts, two outliers differ from others assigned to this latent class (I16 and I17) as they have experienced at least 20 years of strict forest protection. Their main rationales do not significantly diverge from the majority in that "improved farming" segment as they view forest protection as something that their future descendants and the world should benefit from. The one hectare of *teviala* permit is perceived as a shrinkage of the national park, and recognition of people's needs, i.e. they aspire to a secure right to clear that they would not necessarily exercise.

3.3.3.3 Segment 3 "Trade-off" (15%)

Segment 3 "trade-off" is composed of both experienced and inexperienced households. These households (n=3, I18-I20) seem to have traded off the cash donations with use restrictions, i.e. they

are willing to accept cash compensations to offset the foregone benefits of future forest use restrictions.

The less experienced household head interviewed in this group (I18) shows explicit economic reasoning, trading-off the amount of cash with the revenue he would get from forest clearing. He also clearly differed from the previous two segments in the value he gave to cash payments and can envisage investing cash to generate returns. A recent immigrant from the *Merina* ethnic group (from the generally more developed region around the capital), ascribed his ability to invest money to his previous experience of urban life and his alternative sources of income (his wife is a government teacher). This household head does not intend to do *teviala* at present but would be attracted by the opportunities to clear forests should the forest frontier be open.

> "If forest frontier is open, anyone, whether the wealthy or the poor, those who have got lots of lands and those who are deprived will go there and farm in the forest, to be honest, even me, we must do *teviala* if restriction is lifted, since there is no longer any obstacle." I18 (recent migrant, 42, Mantadia)

The more experienced respondents in this segment (I19 and I20) on the other hand recognised the value of cash compensation and insisted that such compensation was a right. Just as the state and the society have the rights to benefit from forest protection, they believe that they too should be also entitled to sufficient compensations.

> Since they don't even allow us to farm on the fallow lands that our ancestors left for us, they must compensate us. Otherwise, we will forcefully enter the park, we will fight for our rights, it is a sacrifice that I am ready to make for the next generation. We are actually entitled to get 50% of the park tourism revenue, but that's obviously not enough! This money was not even enough for the school construction. 50% of the park tourism revenue is not enough for four *Fokontany*. Receiving money as compensation for forest protection is fine but small amounts of money don't make sense." I19 (indigene, 39, Mantadia).

Table 3.4 further suggests that respondents assigned to this class are likely to be younger households. Despite their willingness to trade-off rights to *teviala* with cash, some of them raised concern about ongoing need for land.

> "I am willing to accept cash donations since I won't be able to do *teviala* anymore, but money alone is not enough because it is very difficult to use it here, particularly if you don't have lands that you can cultivate. So ideally, we should get both new lands and money so that we can invest the money in our cultivation." I20 (indigene, 24 years old, Mantadia).

Households in this segment generally express neutral preferences for the improved rice farming. They prefer cash they can use as they wish over externally defined development projects.

3.3.3.4 Segment 4 "Cash" (21%)

Respondents belonging to this class expect a highly positive utility from the cash payments and seem indifferent to any forms of forest use restrictions and improved rice farming. They comprise both experienced and inexperienced households (n=5, I21-I25).

Some households (I21 and I23) are not interested in *teviala* at all – they associate *teviala* practices with an undesirable nomadic lifestyle. Others (I22, I24, and I25) emphasise that if they had alternative livelihoods, they would happily abandon *teviala*. Some have already started seeking alternative agricultural techniques to adapt to forest use restrictions e.g. by converting small valleys to paddy fields. They believe that paddy fields will last and be transferable to future descendants whereas clearable forests won't be available forever.

Households in this segment are eager to intensify agriculture on paddy fields. They often see paddy fields and improved (rain fed) rice farming as two conflicting practices that cannot go hand in hand given their limited resources and capital. They are relatively uncertain about the prospective yields from the improved rice farming and will only adopt if demonstration projects are successful:

> "They need to implement pilot projects first, if they are successful, people will be automatically in, long speeches are useless, people just want to see by themselves." I25 (indigene, 64, Mantadia)

This segment highly values cash which they currently feel constrained by: "Like a foreigner trapped in one island, our hearts are longing for so many things but we cannot find dry land to move over" (I25). Cash compensation would enable them to afford to send their children to the nearest town, to pay their school fees and living costs. One young household head, I23, (21 years old, recent migrant, MDT) expressed his intention to migrate to urban areas and open a small shop.

The utility that these households expect to get from the instalment attribute is non-monotonic i.e. when compared to a lump sum payment, they strongly prefer medium time frame or 10-year time horizon but highly disfavour the long time horizon (20 years). They ascribe the preference for the medium timeframe to their inability to invest money and the volatility of cash. The longer timeframe is perceived as less trustworthy.

Responses suggest that this group did not consider any societal values in their choice decisions but were instead mostly concerned about their households' well-being:

> "If I disclose my choice to others, they won't be happy with me, but I do not care and anyway, you assured me that my answers will be kept secret, as the saying goes: Roosters fighting in the tomb, both strive to stay alive." I23 (recent migrant, inexperienced, 21, MDT)

3.3.4 Are the theoretical assumptions of the DCE method met?

The qualitative debriefings suggest mixed evidence on the continuity axiom which requires that respondents should attend to all attribute levels across each of the alternatives and trade them off while evaluating their preferred choices. In many cases, we find a no (or low)-value of the attribute rather than a non-attendance (or total ignorance), albeit violation of the continuity axiom did happen in a few cases.

There are for instance some cases (e.g. I1, I2, I4, I5 in segment "holdouts") where, although the choices made suggest respondents were fixating on a single attribute rather than attending to all attributes (known as lexicographic preferences), the information provided in the qualitative debriefings shows their decision making processes were consistent with the continuity axiom. They did attend to the cash attribute and weighed up the values of *teviala* against the cash payments. Likewise, I10 and I11 (assigned to the "improved farming" segment) reported that they compared the cash payments with the utility they would get from legal forest tenure. Trade-off is most explicit among segment 3, as I18 stated:

> "So let's see, if I receive a payment of nine million Ariary over 10 years, which amounts to nine hundred thousand Ariary per year, and even if I still continue farming here, that won't be profitable. But then if it goes up a little bit, to let's say 12 or 15 million, then it may be more attractive. If I get for instance 12 million just as a lump sum payment, that would be really ideal, that would definitely be my preferred choice…. If I get 15 million, I will build a house in town, then I will rent it out, I can still continue living here, so that I can get a monthly revenue on top of my crop revenues. If I receive the payments over let's say 20 years instead of a lump sum, I will then invest part of it in some lucrative activities like poultry, and then keep some in the bank, so by the end of 20 years, I will have saved large sums of money." I18 (recent migrant, 39, Mantadia).

Similarly, respondents' accounts suggest that instead of exhibiting lexicographic preferences, they rather expect lower utility from other attributes. Holdouts (segment 1) claimed for instance to have considered the cash donations but simply do not value cash given its limited value in the context in which they live. As I3 attested:

> "If we were sure that the cash would really cover all our needs and if we were confident that investing it would generate returns and be profitable, we might have liked it, but the reality is quite different." I3 (indigene, 47, Mantadia)

A few interviewees, however, admitted overtly to having not attended to some attributes. For instance, I6 and I7 (holdouts) clearly expressed that they ignored the cash payments not because the utility levels of the cash are low or zero (as their choices might imply) but because they do not believe that the cash payment would really happen in reality. Their accounts suggest feelings of

disappointment towards external agencies promoting alternative livelihoods or implementing compensation measures, they for instance claim to have not perceived any benefit from the park's establishment but instead have experienced destitution. They (I5, I6, I7) also anchored their choices on the incommensurable cultural value of *teviala* by stating "nothing will ever compensate us for *teviala*, it is our identity".

Similarly, some segment 4 households ("cash") did not consider the *teviala* attribute at all in their choice decisions. For instance, I23 is only concerned about how he can best invest the cash to generate returns. I22 and I24 pointed to the very small likelihood of an open forest frontier; "forests are already well protected, the government is very determined to protect it". They also genuinely favour forest protection and would like their future descendants to enjoy the forests' multiple benefits; "I want my children to have real-life experience of lemurs' songs, I don't want lemurs to become an ancient history that they would only hear on the radio once all the forests are gone."

We find that experience of forest conservation or compensatory interventions is not systematically associated with violations of the continuity axiom. Instead, respondents' thought processes suggest that apparent non-conformity to the continuity axiom does not imply invalidity of respondents' choices, i.e. they represent their genuine preferences. For instance, if they do not attend to cash, it actually implies that they have genuine zero preference for that attribute.

With regard to the content validity of our DCE survey, i.e. the extent to which respondents found the payment vehicle plausible or believed in the consequentiality of their choices, the qualitative debriefings suggest that most of the interviewees' choices (n=19 out of 25, 76%) conform to the assumptions of the method. Although a few interviewees (I5, I6, I7) explicitly raised issues with regard to the plausibility of the cash payment scenario (e.g. "Getting that much money or any aid at all from faraway donors is utopia") or the survey scenario (I22 and I24), the majority of respondents did perceive their choices as consequential and were actively engaged with the DCE survey. Most respondents were also pleased to be consulted about their preferences and aspirations. As I25 asserted:

> "The choices were so appealing, we thoroughly enjoyed doing it, it is the first time that a researcher asked us about what we really need and want. I25 (indigene, experienced, 64, Mantadia).

3.4 Discussion

3.4.1 The effect of experience of forest use restrictions and compensatory interventions on welfare impacts of forest conservation

Our results suggest that households' prior experience with forest use restrictions has a strong influence on both their appraisal of the welfare impacts of future restrictions and expectations of compensatory mechanisms. Our results therefore suggest caution is needed in using DCE as a means of estimating compensations for long term and complex projects such as forest conservation. Although two segments (trade-off and cash – 36% of the total sample) are composed of both experienced and inexperienced households, their rationales substantially differ and are anchored in their experience of forest use restrictions. Our results are consistent with previous studies which show that experience with a good or policy markedly influences both WTP estimates and the predictability of respondents' preferences (e.g. Adamowicz 1994; Ferrini and Scarpa 2007; Hanley et al. 2009).

Our findings could be considered evidence of experiential learning (Kolb 1984). Experienced households gave many accounts of why they value their rights to continue *teviala* practices owing to a solid knowledge of the no *teviala* policy in question and their negative experience of development interventions. Their lack of interest toward the improved rice farming is consistent with the high failure rate of such interventions. A number of studies (e.g. Scott 1998; McConnell 2002; Pollini 2009) have described how agricultural development interventions are often not taken up due to requiring unrealistic labour inputs and because farmers cannot afford to invest in something unproven. While one can conclude that they may be biased against novel alternatives, we argue that their preferences are the outcomes of learning and knowledge gained through adaptive processes (Denrell and March 2001; Fazey et al. 2005; Fazey et al. 2006).

The incommensurability of *teviala* among experienced households is consistent with anthropologists' findings in Madagascar that deforestation is often driven by a desire to attain customary tenure, since agricultural land is usually private, while forest land often is not (Keller 2008). The high cultural value of *teviala* to *Betsimisarika* farmers is also important (Hume 2006; Desbureaux and Brimont 2015). The reason why many respondents treat cash as dubious and unreliable is supported by Sandel (2000). That is, their indifference towards the cash is anchored in the very reasons why it is useful, i.e. it is fungible, anonymous, and easily transported. The strong bequest value of *teviala* practices, i.e. the value they placed on ensuring higher utility for the future generation from forest clearing may explain the highly positive utility of the 20-years' time horizon (table 3.4). Forest clearing is viewed over a long term planning horizon, not to address the immediate households' needs but those of future

descendants. Such findings challenge the pervading myth that indigenous communities have a high discount rate or high time preference rate (e.g. Holden et al. 1998; Poulos and Whittington 2000).

In contrast, households with limited experience of forest conservation exhibit substantially less aversion to losing their rights to continue *teviala* than more experienced respondents. This is in stark contrast to the "endowment effect" theory (Thaler 1980) which predicts that an individual values a good more highly if his/her rights toward the good have been established (de facto *or* de jure). Psychological feelings that can be interpreted as regret (Loomes and Sugden 1982) may explain this result, i.e. more experienced households regret losing the previous open forest frontier that they used to benefit from and anticipate regret in relation to the continuation of tightened forest protection.

Respondents who are inexperienced in terms of exposure to conservation restrictions ("improved farming" segment), have also been affected by their experience of open access, and their strong preferences for the legal forest tenure may originate from their inability to exclude others (Ostrom 1999). In Ampahitra there has been very rapid recent immigration of people from a variety of ethnicities (*Merina*, *Betsimisaraka* and *Bezanozano*) attracted by land availability. These inexperienced households' strong preference for the improved rice farming is consistent with the positive correlation between individual land tenure and adoption of more efficient land management practices reported by previous scholars (e.g. Barrows and Roth 1990). This is also supported by some respondents, although indigenous, appear more inclined to adopt new techniques only after they have freely benefited from an open access situation and realized that forests are becoming increasingly scarce.

3.4.2 Theoretical and content validity of DCE results

Although we found violations of the continuity axiom among six debriefed respondents, most gave accounts of decision making processes which conform to the axiom. Our qualitative findings suggest that most apparent patterns of attribute non-attendance are in fact theoretically valid, i.e. genuinely represent respondents' anticipated welfare impacts. Deleting these responses would bias the DCE outcomes and resulting policy implications (Lancsar and Louviere 2006).

Other studies using focus group debriefings of stated preference techniques (Clark et al. 2000; Powe et al. 2005) or verbal protocol analyses (Schkade and Payne 1994) (all in developed country settings) found that many respondents' willingness-to-pay figures are not consistent with rational choice theory and instead represent resistance to the commodification of nature. While it is clear that these values do not reflect the worth of the good being valued, Clark et al. (2000) argue that they are rational

preferences and should still be considered "economic", as the definition and importance placed on rationality depends on the approach to consumer theory to which one ascribes.

We concur with Hess et al. (2013) and Balcombe et al. (2015)'s conclusions which call for the reappraisal of previous DCE studies showing or inferring significant shares of attribute non-attendance and using alternative modelling approaches to accommodate this issue. In our study context, although the insignificance of the cash among the "holdouts" and "improved farming" segments poses significant complications for the computation of monetary willingness-to-accept estimates, they accurately represent how forest protection affects local livelihoods. Our findings, together with previous literature (e.g. Hensher 2010a), suggest that DCE researchers may need to re-engage with the psychology of decision making and look more into processes, i.e. how respondents construct their choices and what constitute actual violations of the assumptions of rational choice theory. As Gregory and Slovic (1997: p3) put it, *"truth may ultimately reside in the process of the evaluation, rather than the outcome"* and DCE researchers can gain deeper understanding of respondents' thought processes using qualitative methods.

The satisfactorily high content validity of our results can be mostly attributed to the considerable efforts we put into piloting and explaining the valuation survey to respondents. Given the high illiteracy rate of population in the study area, we had to proceed with lengthy warm-up steps[40] to ensure that respondents understood the tasks of making trade-offs in a DCE survey. The use of dolls and large pictures helped desensitize the illegal nature of *teviala* practices and encouraged respondents to actively engage with the DCE survey.

3.4.3 Study design

It is important to note that our research design means it is not possible to isolate completely the effect of experience on the welfare impacts of forest conservation (Pullin et al. 2013). Randomizing an intervention (such as exposure to forest use restrictions and compensatory projects) would enable isolation of potential confounding variables, such as immigration status from experience of forest use restrictions, but would be impractical and would raise ethical concerns as they may involve long-

[40] Although these lengthy explanations considerably eased respondents' comprehension of the valuation exercise, we acknowledge that this may have also biased respondents' choice decisions. We are however unable to control for such potential bias due to the inexistence of widely accepted theoretical model for how individuals' priors about their preferences for a policy (and their experience of the policy) interact with information provided in a survey.

lasting negative effects. We also argue that immigration is not exogenous to conservation restrictions. Our quasi-experimental approach (comparing people in communities differing in their experience of forest use restrictions matched on important socio-economic parameters) gives valuable insights but we are unable to fully separate the effect of experience of conservation from the experience of compensatory projects. Longitudinal data with unambiguous baseline measures from recently established protected areas may overcome such challenges in the future, however practicalities may remain a problem.

3.5 Policy implications

There have been a number of commitments stating that local people living at the forest frontier, many of whom are extremely poor and marginalised, should not be negatively affected by efforts to conserve forests for the global benefits they provide (Martin et al. 2013). Estimating how much or what type of compensation is appropriate is challenging as those with experience of forest use restrictions provided very different appraisals of the local welfare impacts of future conservation restrictions than those who lack such experience. Thus, although we find good evidence that the DCE can be successfully used in a rural developing country context with low literacy to elicit current preferences (high theoretical and content validity of the DCE survey), ex-ante valuations of welfare impacts of conservation may not accurately estimate compensations necessary for forest use restrictions.

When respondents are more experienced with forest protection, neither cash nor support for improved rice project would compensate them for *teviala* restrictions. On the other hand, when respondents lack experience of conservation, they may be ill-informed and hence less able to estimate the likely welfare impacts of such a long lived and complex intervention. If it is hard to accurately estimate in advance the costs of an intervention which may affect people negatively, this calls into question the viability of the model of equitable coercive conservation. We argue that there is a need to rethink conservation approaches, particularly the urgency which with interventions are implemented and the feasibility of achieving fair compensation for coercive measures.

The strong bequest motive for maintaining the rights to clear land amongst experienced and inexperienced respondents alike suggests that secure forest tenure may slow down deforestation. This is a well-known result in fisheries economics, where harvesting is higher in open access fisheries and slow down when fishers are granted individual property rights (Holzer 2015). Many have argued similarly that promoting legal titles to land in forested areas with weak property rights can be similarly

effective (e.g. Chhatre et al. 2012). While giving secure common tenure over forests to stable communities (i.e. with a tradition of communal and shared ownership) might slow deforestation without any ban on *teviala*, individual legal tenure may be prioritized when the situation is not an idealised village with clearly defined common rights. Either way, conservation can then be negotiated through PES schemes where local people's participation is genuinely voluntary. This recommendation is driven not only by a concern for social justice and equity (Hellum and Derman 2004), but also by a pragmatic assessment that local forest dwellers have the greatest impact on resources and also the most to lose from non-sustainable uses of these resources. When ex ante estimates of compensation are very difficult to achieve, voluntary PES schemes may actually have lower transaction costs than trying to achieve fair compensations if markets are competitive and property rights enforceable (Pagiola et al. 2005). However, formalizing individual ownership may be open to elite capture and embed inequalities. We argue, however, that any mechanisms aiming at achieving equitable compensations would likely face the same constraints.

Where conservation is imposed on local people, and forest tenure remains with the government, great care needs to be taken in developing approaches to adequately compensate for welfare impacts of conservation restrictions. We argue that the welfare impacts of forest use restrictions must be continually monitored. Otherwise local livelihoods may be seriously harmed. Compensation using cash, while a preferred option for some households, has significant limitations because of the limited opportunities for investment in remote rural areas, and lump sum payments are most problematic. Those most likely to be affected negatively by conservation restrictions may be least able to invest cash to generate returns. However, the reluctance of some groups to engage in improved agricultural techniques, and the very low values they place on such interventions, should also suggest caution when providing development projects as compensation schemes. Such schemes need to promote development interventions which will be effective in the region but also accepted locally. Ultimately, tackling remoteness will be key to ensuring the viability of any alternatives to *teviala*.

Chapter 4. DOES MORE TIME TO DELIBERATE AFFECT RESPONDENTS' BEHAVIOUR IN A DISCRETE CHOICE EXPERIMENT? ASSESSING THE WELFARE IMPACTS OF FOREST CONSERVATION IN MADAGASCAR[41]

ABSTRACT

Respondents to discrete choice experiments (DCEs) may lack pre-defined or well-informed preferences for large environmental interventions. Low literacy rates may additionally challenge the application of DCEs in developing countries. Using a within-subject design and qualitative debriefing approaches, we evaluate whether more time to think and discuss with others influences responses to a DCE assessing the local welfare impacts of forest conservation in Madagascar. We used Bayesian analyses to investigate the effect of more time to deliberate on individual-level parameters within a model with random taste variation. Giving respondents more time significantly affects individual-level preference parameters and marginal willingness-to-accept (WTA) estimates. The effect of deliberation on households WTA depends on the literacy, with WTA of illiterate respondents significantly increasing post-deliberation, and the reverse for literate respondents. Qualitative debriefings provide limited evidence for carry-over effects and suggest extensive deliberation within household members even among respondents who did not revise their choices. While respondents' decisions to revise their choices mostly stemmed from more reflection, we also found evidence of strategic considerations. We discuss the implications of our results for the application of DCEs in developing countries.

[41] **Rakotonarivo, O.S.** Jacobsen, J.B., Gibbons, J.M., Andrianantenaina, N.S., Hockley, N. Assessing the welfare impacts of forest conservation in Madagascar: Does more time to deliberate affect respondents' behaviour in a discrete choice experiment? Manuscript submitted for publication.

4.1 Introduction

Since the discrete choice experiment (DCE) method entered the environmental field in the 1990s, it has expanded the boundaries of non-market valuation, allowing the valuation of individual attributes of environmental goods that are seldom traded in markets (Hanley et al. 1998b). However, a major criticism of the method is that respondents are unlikely to have pre-existing preferences for complex or unfamiliar environmental issues, thus violating the completeness axiom of rational economic theory (e.g. Alvarez-Farizo and Hanley 2006). They may instead construct their preferences from the cues or information provided within the survey context. They may also be ill informed or lack the knowledge to comprehend the importance of environmental problems for their welfare and therefore may not provide meaningful preferences that can be elicited within the time scale of a stated preference survey.

One important but relatively under-studied issue is whether individual preferences elicited on the spot within a conventional survey situation represent respondents' actual preferences (Cook et al. 2012). Most DCE studies, particularly those administered in person, require respondents to give immediate responses, and may therefore impose considerable time restrictions on respondents. Respondents may not have enough time to deliberate[42] about what they would choose in real situations. For example, when respondents are asked to state their willingness to accept compensations for foregoing a good, they may overstate their compensation estimates relative to what they would accept in real life. This often leads to hypothetical bias, which arises when respondents' elicited preferences differ from their decisions in real life.

In real life, people usually need to sleep on important matters before making decisions (Bos et al. 2011). Respondents may need more time to gain a better understanding of the good being valued and the DCE survey and carefully evaluate the full implications of their decisions. Before making a decision involving long-term or complex interventions, which may have a significant effect on their welfare, they would also usually consult other household members or others outside their households. Even if respondents are familiar with a good or a policy, deliberation with others may help them construct their preferences and therefore reflect better real world decision making (Svedsater 2007). Besides the potential mitigation of hypothetical bias, giving respondents more time to deliberate has been

[42] We used the term 'time to deliberate' instead of 'time to think' as used by earlier work (Whittington et al. 1992; Cook et al. 2007) to explicitly allow for the possibility of discussion with others in addition to the process of thinking over the good being valued.

shown to improve the internal validity of DCEs (Cook et al. 2007). Cook et al. (2007) also found evidence of lower willingness-to-pay (WTP) estimates among respondents who had time to deliberate compared to those who did not. When answering a contingent valuation question, more time to deliberate also reduced WTP estimates and respondents' stated uncertainty (e.g. Whittington et al. 1992; Svedsater 2007; Cook et al. 2012).

Lack of prior or well-informed preferences is likely to occur even with well-educated individuals in developed countries, but DCEs may face extra challenges in least developed countries where literacy rates are low and where it may be harder to explain hypothetical scenarios to the average respondent (Whittington 2004; Christie et al. 2012). In rural developing country settings, giving respondents more time to deliberate may give them room to make better use of their cognitive skills and abilities to comprehend and interpret numerical tasks. As better understanding is essential to better choice decisions (Peters 2012), deliberation may improve the validity of DCE responses. Answering a simple contingent valuation survey scenario may be daunting for an illiterate rural respondent (Whittington 2004), and the computational burden of making trade-offs in DCE may place unrealistic expectations on respondents. Deliberation may also mediate interviewer effects such as respondents' tendency to say yes easily or give responses that they think interviewers may want to hear (Whittington 2010), or other strategies to quickly escape the restricted and intimidating interview situation. Interviewer effects are most prevalent in face-to-face interviewing, still the most common way to administer household surveys in developing countries (e.g. Whittington et al. 1992; Cook et al. 2007). Respondents may also be more encouraged to ask questions, request additional information or raise any issues about the survey when given the opportunity to deliberate.

In this study, we conceptualise 'deliberation' as the act (or process) of carefully thinking over and/or discussing with others in order to make a decision. Deliberation does not necessarily require interaction with others outside the household, i.e. individual households may change their previous choices simply as a result of reflecting upon the hypothetical scenarios and consulting other household members. Deliberation may also offer opportunities for social learning[43] through discussion with friends, relatives or neighbours outside the household. Others' opinions or judgments may validate or influence respondents' views and attitudes toward the good being valued (Zimbardo and Leippe 1991). More time to deliberate may reduce the cognitive overload of the DCE design as households

[43] This is similar to the conceptualisation of deliberation in judicial or social settings where different actors exchange arguments during which ideas and perceptions change through persuasion (Habermas 1981).

can clarify among themselves what they are valuing and why. As we primarily seek to elicit households' preferences as opposed to single individuals, giving respondents the chance to deliberate may also free intra-household negotiations from the enumerator's presence. Welfare estimates are also likely to be underestimated if the policy being valued is considered sensitive or illegal as is often the case with environmentally damaging behaviours like swidden agriculture, and deliberation without the enumerator may help to counteract this. However, more time to deliberate may also encourage strategic behaviour (Carson and Groves 2011), i.e. respondents may use the additional time to consider how their responses could influence proposed policies in their favour. For example, instead of choosing their most preferred alternatives across choice sets, they may instead opt for different options for fear of missing out from a given compensation policy. Likewise, discussion with others outside the households may also allow respondents to reach an agreement and hence disclose collective preferences instead of revealing their individual values.

This article aims to evaluate whether giving respondents additional time to deliberate affects their responses to a DCE survey valuing the local welfare impacts of forest conservation in Madagascar. The DCE survey was designed to assess the trade-offs that local people adjacent to a protected area make between: clearing new forests for swidden agriculture, cash payments over varying timeframes, and support for improved rice farming. We investigate the effect of more time to deliberate at the choice task level, on individual-level utility parameters, and on marginal willingness-to-accept (WTA) estimates. We also conducted in-depth qualitative debriefing interviews with a sub-sample of respondents after the second round of DCE to explore the mechanisms by which more time to deliberate shapes individual values and hence influences preferences (e.g. potential for strategic answers). Previous studies using the time-to-deliberate approach in stated preference techniques have relied exclusively on rapid follow-up questions[44] to understand respondents' motivations for changing or not their first elicited answers.

Despite the potential advantages (and drawbacks) of more opportunities to deliberate in improving the validity of DCE answers, we only know of two health economic studies (Cook et al. 2007, Johnson et al. 2010) which have empirically tested the effect of more time to deliberate, as well as three environmental contingent valuation studies (Whittington et al. 1992; Svedsater 2007; Donfouet et al.

[44] Whittington et al. (1992) added a single open-ended "why" question at the end of the second survey to unravel respondents' rationale for revising or no their answers. Johnson et al. and Cook et al. assumed about what had happened during the deliberation with the sample that was given additional time without any retrospective questioning.

2015). Cook et al. (2007) and Johnson et al. (2010) used a split sample design to isolate the effect of more time to deliberate (one day) on DCE responses. Our DCE study shares the same motivations as Cook et al. (2007) and Johnson et al.'s studies (2010) in health economics, but instead uses a within-subject design. We used hierarchical Bayes estimation which greatly simplifies the estimation and inference compared to classical procedures (maximum simulated likelihood) and gives more accurate estimates of individual-level utility parameters for small sample size (Train 2003). While all previous research looking at the effect of deliberation asked WTP questions (e.g. Whittington et al. 1992; Cook et al. 2007; Svedsater 2007), this study is also the first we know of which elicits WTA answers.

In the next section, we describe our deliberation treatment, the choice experiment survey design, data collection, and sampling method. Section 3 defines the modelling framework. We then present the results in section 4 and discuss them in the last section.

4.2 Research design

4.2.1 Deliberation treatment

To examine the effect of giving respondents more time to deliberate, we used a test-retest design, i.e. we administered the same DCE instruments to the same households (N=104) the following day, exactly in the same order. After the first round of the survey, we gave the following instructions to respondents:

> "We encourage you to reflect on your choices tonight and tomorrow, you will still have the opportunity to revise your answers and ask any questions or raise any issues regarding the survey. We also encourage you to discuss with each other (spouse), relatives, neighbours, and friends. Although you may receive valuable comments from others outside your household, keep in mind that that we are only interested in your own household opinion and preferences. That is, the answers you are to give tomorrow have to be the values for your own household. The second interview tomorrow is very important for our research."

Our design differs from previous test-retest studies, which aim to test the stability of respondents' preferences at two different points in time mostly for benefit transfer purposes (e.g. Liebe et al. 2012; Schaafsma et al. 2014). While these test-retest procedures using long time intervals may minimize the potential for recall (or carry-over effect i.e. where respondents simply anchor their revised responses on their initial decisions), deliberation will not be sustained over long time periods[45]. As we are interested in how DCE responses are influenced by more time to deliberate, we instead opted for a shorter time frame (one day) and thus minimise changes in other potential determinants of DCE responses. While our design does not allow us to statistically isolate the true effect of more time to deliberate from recall effect, we explore the extent of this effect in the debriefing interviews. We argue that recall may be less prevalent in DCE surveys than in contingent valuation studies since respondents are asked to complete a number of choice cards.

Split-sample designs (such as the one by Cook et al. 2007) may help reduce recall effects. In these designs, respondents were split into two subsamples, one split is administered the DCE survey whereas the other split is introduced to the survey, given one practice choice card, and asked to

[45] While it is difficult to completely separate deliberation from temporal instability of preferences, deliberation seems unlikely to be sustained over periods of more than a few days without further prompting. Therefore, temporal instability is likely to dominate in research design with a long gap between test and retest, while deliberation might be expected to dominate over short time periods.

complete the survey only the following day. However our approach of showing the full set of choice cards and eliciting respondents' choices at the first round of DCE survey in the presence of the enumerators may be essential to familiarise respondents with the tasks of making trade-offs and deepen deliberation overnight, particularly in a developing country setting where respondents are less familiar with surveys. In addition, the use of a split sample design requires a much larger sample size, and does not enable researchers to examine how the very same individuals with specific norms and values are affected by deliberation. Furthermore, leaving the choice cards with the respondents overnight (as per Cook et al.), may allow them to jump ahead in the survey instrument in unobserved ways, which may lead to the violation of a major assumption of DCE, i.e. that respondents should consider the choice cards independently, and not compare alternatives across choice sets (Hensher et al. 2005). Like Cook et al. and previous contingent valuation research investigating the effect of more time to deliberate, we are unable to statistically isolate the effect of reflecting upon and discussing the issue (i.e. we cannot distinguish which particular aspect of deliberation triggered the change). We therefore explore these issues in the qualitative debriefing interviews.

4.2.2 Choice experiment design

The DCE survey aimed to assess the trade-offs that respondents make between compensation interventions and the ability to clear new forests for swidden agriculture (termed *teviala*). *Teviala* has been considered as the main driver of deforestation in Madagascar and hence the main focus of conservation actions (De Wilde et al. 2012). *Teviala* has been criminalized in Madagascar since colonial times, yet enforcement has often been weak (Kull 2004) even in long-established protected areas such as Mantadia National Park (our study site), mostly because of recurrent political instabilities combined with the lack of viable alternatives. We asked respondents to choose between a reference level alternative (forest protection is formally lifted and households can freely do *teviala*) and two alternatives with the following attributes: i) A monetary attribute which was framed as cash donations or development assistance from a major foreign donor that would be managed and distributed by an independent institution[46], ii) Number of annual instalments over which the household will receive the payments, iii) Support for improved rice cultivation and iv) *Teviala* attribute (table 4.1). The attributes and levels (table 1) were informed by the literature and three focus group discussions and were piloted with 50 respondents.

[46] The selection of such institutional mechanisms was informed by the piloting of the DCE survey and three focus groups in which an independent institution was perceived as most reliable by the majority of participants compared to local government or local community representatives.

Table 4.1: Attributes and levels of the DCE (reference levels in bold)

Attributes	Description	Levels	Coding and Notation
Total cash donations framed as development assistance (3080 MGA = 1 USD)	Review of secondary data and previous literature estimating the local costs of deforestation aided the selection of the payment levels (e.g. Shyamsundar and Kramer 1996; Ferraro 2002).	**0**, 3, 6, 9, 12, 15 (x10^6 MGA)	Cash (continuous variable)
Number of annual instalments	The three levels of instalments allow an estimation of the respondents' discount rates and provides information on the respondents' ability to invest money.	**1**, 10,20	Dummy-coded: Installment10 and installment20
Support for improved rice farming	This attribute is introduced as a sustainable and modern agricultural package that includes productivity enhancing practices such as the use of fertilisers, insecticides and/or herbicides. It involves digging and possibly the construction of terraces for slopes and precludes the use of fire as a way to maintain fertility while not fallowing the land. It also includes material support (e.g. improved seeds, wheelbarrow, spades, etc.).	**No,** yes	Dummy-coded: Support for improved rice farming coded as 1
Teviala (clearance of new forestlands for agriculture)	This attribute has three levels: i) no *teviala* (i.e. strict enforcement of restrictions), ii) a permit for one hectare of *teviala* (a one-off opportunity), iii) free *teviala* (similar to pre-colonial times before criminalization of *teviala*, and de facto to more recent periods of little or no enforcement).	**Free *teviala*** (open forest frontier), 1ha of *teviala* permit and no *teviala* (strict protection),	Dummy-coded: *teviala* 1ha, no *teviala*

We combined alternative levels of the four attributes in choice tasks using an efficient design that seeks to minimize the standard error of the coefficients to be estimated (see Ferrini and Scarpa, 2007). The fractional factorial design was optimised for d-efficiency for the multinomial logit model using Ngene 1.1.1, and based on information on the signs of the parameters obtained from the piloting (Scarpa and Rose 2008). The main purpose of this design was to ensure more reliable parameter estimates despite the relatively small sample size that was achievable given the field conditions (Rose and Bliemer 2013). The design generated 12 choice tasks which were divided into two blocks; each respondent was presented with six choice tasks. Respondents were randomly assigned to one of the two blocks in the experiment.

Each choice task was composed of three alternatives including the reference level alternative (figure 4.1). DCE surveys usually include a status quo option (or do nothing or opt out) but since a status quo option (households' own current "levels" for each attribute) would vary enormously across respondents[47], we opted for a reference level where protection is totally lifted. Elucidating a status quo alternative would suffer from the problem of requiring respondents to reveal their current participation in *teviala*.

ALTERNATIVES	A	B	C (reference level)
Total cash payments (10^6 MGA)	3 x 10^6	6 x x 10^6	NONE
Number of installments	**10**	**20**	-
Technical rice farming	**YES**	**NO**	**NO**
Teviala option	YES BUT only on **1ha**	NO X	YES
Choice			

Figure 4.1: Example of choice card

4.2.3 Sample design and survey data collection

We surveyed 104 households adjacent to Mantadia national park, a protected area established for 20 years in the eastern rainforest of Madagascar. These households were sampled from all villages in the two *Fokontany* of Vohibazaha and Volove bordering the park, belonging to the Commune of Ambatovola. Prior to sampling, we had to construct the sampling frame since no map or census of households was available. We first conducted key-informant interviews to collect information on households and hamlets, and drew sketch maps of the locations of individual households. We thereafter visited hamlets in person to cross check key-informants' statements, refined the sketch map of individual households location, and recorded GPS markers. We then selected a random sample

[47] Some respondents may be more inclined to take risks and engage in *teviala* activities while others may be more fearful or disinclined to do *teviala*. Respondents may also face or perceive different levels of enforcement.

of households (N=104) stratified by village. Building the sampling frame took approximately 30 person-days, approximately 1/3 of the total field work time. The sample size was severely constrained by the remoteness of many households, which often required up to seven hours of walking to reach. Surveys took place in respondents' field huts, which are located near their temporary fields often in isolated settings. Swidden agriculture, a major livelihood activity in the area, is characterised by a constant moving between fallow lands and/or clearing of primary forests.

The survey focused on the preferences of households represented by couples, i.e. the household head and his spouse; interviews were conducted with both members whenever feasible (200 individuals over 104 households). The DCE survey was piloted in three phases in February, March and June 2014 in nearby villages. The actual survey was carried out between Mid-august to October 2014 by three enumerators, including OSR. In addition to the DCE survey, we also collected some socio-economic characteristics of the household including education, household features, land holdings and characteristics, other household assets, and wealth indicators (such as food security). All respondents also answered one open-ended follow-up question about their motivations for revising or not their responses on day 2 and several other closed debriefing questions. The full survey took one to two hours per household including some warm-up steps to give respondents some practice and ensure they understood the task of making trade-offs in a DCE survey. The study protocol was reviewed and approved by Bangor University's Ethics Review Committee.

4.2.4 Qualitative debriefings

To explore the effect of deliberation and respondents' rationale or motives for revising or not their answers, we conducted debriefing interviews after the second round of DCE survey with a sub-sample of households[48]. Preliminary thematic analysis of the data as the interview progressed indicated that data saturation had been reached with 14 interviewees. The interviews which on average lasted for 45 minutes, were audio recorded with respondents' consent and transcribed. The data were then thematically analysed (Braun and Clarke 2006). We developed an initial coding framework using Nvivo 10 which we expanded following the identification of new themes. To ensure coding was comprehensive and reliable, the coding was also cross-checked independently by another co-author for a random sample of the data (4 interviews) against the framework.

[48] These are the same households debriefed in Mantadia in **chapter 3**. The interview guide used in **chapter 3** was supplemented with additional questions to specifically address this chapter's aim on the effect of deliberation on DCE responses.

4.3 Modelling framework

4.3.1 Modelling the effect of more time to deliberate at the choice task level

We administered the same choice sets to respondents at the first and second rounds of DCE survey which makes pairs of choice sets directly comparable at the aggregate and individual level. To investigate the effect of more time to deliberate at the choice task[49] level, we applied a sequential testing procedure in line with Swait and Louviere (1993) and using the classical maximum likelihood estimation in Nlogit 5. We first estimate a separate mixed logit model for two choice tasks, and a pooled model across the pair of choice tasks in which scale parameters Λ^n are allowed to vary, i.e. the scale parameter of the second model is rescaled to the one of the first model so that the preference parameters equality can be tested. The relative scale parameter $\frac{\Lambda^2}{\Lambda^1}$ used to rescale the second model is obtained from a grid search procedure optimizing the log likelihood for the pooled preference parameters. The standard chi-square distributed Likelihood Ratio (LR) test is as follows:

$$-2(LL^{pooled} - (LL^{c1} + LL^{c2})) \text{ with d.f. } |\beta| - 1 \qquad \textbf{(4.1)}$$

where $|\beta|$ is the number of imposed parameter restrictions

If the LR tests suggest that preference parameters are equal across the two choice tasks, a test for scale parameters equality can then be performed. Otherwise, we can only conclude that preference (as reflected in the beta-parameters) and/or variance (as reflected in the scale-parameters) are significantly different between the two choice tasks but we cannot isolate which since scale and preference parameters are jointly estimated (ʎβ). To test for scale parameter equality, a mixed logit model for the pooled model (choice tasks at the first and second DCE survey) is estimated in which equal scale parameters are imposed, i.e. $\Lambda^1 = \Lambda^2$. The log likelihood of that pooled model with equal scales is then compared to that of the pooled model allowing for varying scales:

$$-2(LL^{equalscale} - (LL^{pooled})) \text{ with d.f. } 1 \qquad \textbf{(4.2)}$$

Equal scale parameters between a pair of choice sets elicited at the first and second rounds of DCE surveys would imply that choice variance remain unchanged as households are given overnight to deliberate. If the relative scale parameter for a given choice task at day 2 increases, the relative

[49] Choice tasks are synonyms of choice sets in which the household is asked to choose their most preferred alternative.

variance of choice task at day 2 compared to choice task at day 1 decreases (equation 4.3). Hence, an increase in the scale parameter would indicate reductions in preference uncertainty, i.e. choices become less random. We also contrast the relative scale parameters between pairs of choice tasks at day 1 and 2 to enumerators' ratings of respondent certainty which were recorded after each choice task.

$$\left(\frac{\Lambda^2}{\Lambda^1}\right)^2 = \frac{(1/\Lambda^1)^2}{(1/\Lambda^2)^2} = \frac{\pi^2/6(\Lambda^1)^2}{\pi^2/6(\Lambda^2)^2} = \frac{\sigma_1^2}{\sigma_2^2} \qquad \textbf{(4.3)}$$

4.3.2 Modelling the effect of more time to deliberate on individual-level utility parameters

We used Bayesian procedures to estimate the individual-level utility parameters within a mixed logit model (described by Train 2003). The mixed logit approach introduces preference heterogeneity by 'individualizing' preferences; each respondent has a possibly unique set of preference parameters. As it is not practical to estimate the parameter vector governing the behaviour of individual respondents, preference parameters are instead defined as random draws from a joint distribution and mixed logit models estimate a distribution of these parameters from the full sample (see Train 2003). Mixed logit models also eliminate the assumption of independence from irrelevant alternatives[50] which is a restrictive assumption of the standard logit model. Bayesian procedures estimate the distribution of coefficients in the population and combine such information with the individual respondents' choices to derive posterior or conditional estimates of the individual respondents' tastes. Bayesian estimation outperforms the classical maximum likelihood simulation in three ways. First Bayesian procedures do not require maximisation of the simulated maximum likelihood function and therefore imposes less computational burden. In addition to problems of convergence, there is often no guarantee that the global maximum has been reached with the chosen starting value. Second, Bayesian estimation procedures are more consistent since they do not rely on the number of draws used in simulation approaches (Train 2003). Third and last, they can easily produce individual-level parameters and hence the calculation of a random parameter difference between the first and revised individual-level utility parameters.

All parameters are specified as random and given a normal distribution, except the price parameter which is fixed. While responses to the price parameter are very likely to vary across respondents

[50] This independence means that the unobserved portion of utility for one alternative is unrelated to the unobserved portion of utility for another alternative.

according to unobservable factors (i.e. factors independent of observable socioeconomic variables), fixing the price parameter greatly facilitates the estimation of WTA estimates[51] and hence avoids implausibly large WTA estimates which is often the case when the price parameter is set as random (Scarpa et al. 2008).

The utility function of an individual *i* facing a choice between two experimentally created alternatives and a reference level alternative in choice task *t* can be described as:

$$U_{ij} = \begin{cases} V(ASC, X_{ijk}, \beta_i) + \varepsilon_{ijt} & \text{if } j\text{=reference level alternative, otherwise,} \\ V(X_{ijk}, \beta_i) + \varepsilon_{ijt} \end{cases} \quad \textbf{(4.4)}$$

Where U_{ij} is the utility function for individual *i*, for alternative *j,* β_i is a vector of preference parameters that vary over households in the population (rather than being fixed) and are hence treated as random variables, X_{ijk} is a vector of observed variables that relate to the alternatives *j* and respondents *i*, and ε_{ijt} is a Gumbel distributed error term, ASC is an alternative specific constant (ASC) for the reference level.

On day 1, we specify the utility function (U_{ij}) of an individual *i* of the alternative *j* as:

$$U_{ij\ (day\ 1)} = \beta_{i\_day1}\ X_{ijt\ (day1)} + \varepsilon_{ijt\ (day1)} \quad \textbf{(4.5)}$$

$$U_{ij\ (Random\ parameter\ difference)} = (\beta_{i\_day2} - \beta_{i\_day1})Xijt + \varepsilon_{ijt\ (day\ 2-day\ 1)} \quad \textbf{(4.6)}$$

$$U_{ij\ (day\ 2)} = U_{ij\ (day\ 1)} + U_{ij\ (Random\ parameter\ difference)} \quad \textbf{(4.7)}$$

Bayesian procedures are often called hierarchical Bayes since they involve a hierarchy of parameters. β_i is a vector of individual-level parameters for individual *i* which describe the tastes of that individual. In the absence of any prior knowledge the parameters β_i are assumed to be normally distributed in the population with mean vector $\tilde{\beta}$ and covariance matrix Σ. From Bayes rule, the joint posterior distribution of the parameters is proportional to the individual likelihood times the prior distribution of the model parameters and can be expressed as:

$$p(\{\beta_i\}, \bar{\beta}, \boldsymbol{\Sigma} | \text{ data}) \propto \prod_{I=1}^{N} L_i(\beta_i)\, N(\beta_i | \bar{\beta}, \boldsymbol{\Sigma})\, p(\bar{\beta}, \boldsymbol{\Sigma}) \quad \textbf{(4.8)}$$

[51] The analytical expression for WTA calculation involves a ratio where the denominator is the price coefficient. When the price parameter is fixed, the distribution of WTA estimates may be inferred directly from the distribution of the non-price coefficient

where α means "is proportional to", *i* refers to the i^{th} household out of *N*, $Li(\beta_i)$ is the likelihood of household *i*'s data conditional on β_i and $N(\beta_i|\tilde{\beta}, \Sigma)$ is the random effect distribution indexed by the parameters, $\tilde{\beta}$ and Σ; $p(\tilde{\beta},\Sigma)$ is a prior distribution placed on $\tilde{\beta}$ and Σ.

Draws from this joint posterior distribution are obtained through Gibbs sampling. That is, a draw is taken from the conditional posterior of each parameter, given the previous draw of other parameters (see Casella and George 1992). Under unrestrictive conditions, the mean of the Bayesian posterior of a parameter is asymptotically equivalent to the maximum likelihood estimator of the parameter. Similarly, the variance of the posterior distribution is the asymptotic variance of this estimator. Hence, the results obtained by Bayesian procedures can be interpreted from a purely classical perspective. We present the results the same way as that of classical estimation by giving the mean estimates and the 95% confidence intervals for each parameter. The model was estimated in R 3.2.1 using the RStan package (Stan Development Team 2015). We also accounted for the effect of socio-demographic variables (table 4.2) by interacting them with the random parameter differences (equation 4.7). We were particularly interested in whether literacy significantly affects the effect of more time to deliberate.

The derivation of the marginal rate of substitution is straightforward and leads to WTA estimates. They are defined as follows:

$$WTA = \frac{\beta_i}{\beta_{price}} \quad \textbf{(4.9)}$$

Where β_i are the attribute coefficients and β_{price} are the price coefficients.

The standard errors and the 95% confidence intervals for these estimates of mean marginal willingness to pay are obtained by using the Delta method (Hensher et al. 2005). Hierarchical procedures produce a unique set of coefficients for each respondent. We can calculate average WTA for any number of different subgroups (e.g. by literacy level) by linking these coefficients with each respondent's socioeconomic data. To compare the WTA estimates between day 1 and day 2, we cannot rely on a direct comparison of 95% confidence intervals for the mean WTA estimates using paired t-tests as non-overlapping confidence intervals may be biased indicators of the significance of differences in estimated means (Poe et al. 2005). To test for WTA differences between day 1 and day 2, we use the complete combinatorial method suggested by Poe et al. (2005) to develop precise confidence bounds for the difference between the means. This is a non-parametric test that involves empirically estimating the confidence interval around the difference of the mean WTA values.

4.4 Results

4.4.1 Respondents' characteristics

Our study site, the commune of Ambatovola, is characterised by low-income levels, lack of infrastructure, chronic poverty and livelihoods highly dependent on forest resources. Mean household head age is 41 and although 78% of the total sampled household heads are literate, they average less than three years of schooling. Food security, which is a common wealth indicator used in least developed countries, is low with households having sufficient food for only half of the year on average. Average livestock holding is also very low (0.35 tropical livestock units using Chilonda and Otte 2006). Small-scale swidden agriculture is the main livelihood activity in the region and the clearance of new forestlands for agriculture is one of the main drivers of deforestation. Residents derive their livelihoods mainly from rain-fed rice farming, only 18% of the sample had access to irrigated lands. Almost all respondents reported to have thought overnight about their choices and discussed them within the household, only 12.5% talked with people outside the household. Table 4.2 describes the variables used as covariates in the mixed logit model.

Table 4.2: Covariates used in the mixed logit model

Variables	Notation used in figure 6 and 7	Mean (sd)
Tropical livestock unit (TLU)	TLU	0.35 (1.06)
Household head age	Age	41.49 (15.25)
Household head literacy (1= literate, 0 otherwise)	Literacy	0.78 (0.15)
Deliberation with others outside households	Outside household	0.125 (0.33)

4.4.2 Effect of more time to deliberate at the choice task level

Since respondents completed exactly the same choice tasks on day 1 and day 2, we first examined the raw response data on a household basis. Hence, we made six sets of comparisons based on the six choice sets per household. 42 households (40% of the total sample) changed their responses to at least one choice set, while only 9.6% altered their responses to at least four choice sets (figure 4.2).

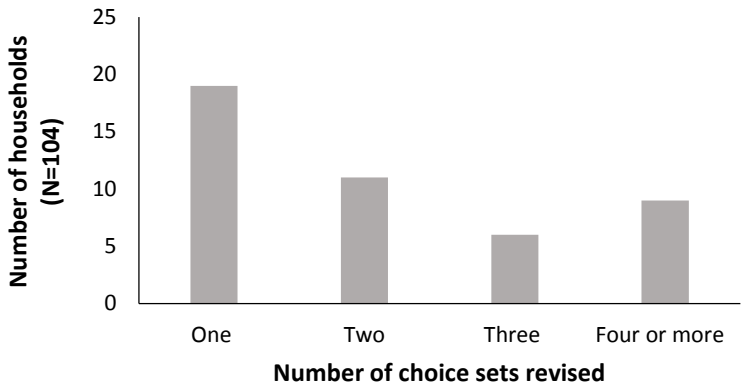


Figure 4.2: Number of households who revised their responses on day 2

To account for the stochastic nature of DCE responses (i.e. preferences parameters are also defined by unobservable factors), we tested for equal preference and scale parameters between paired choice sets using the Swait and Louviere test procedure (1993). As the DCE design used two blocks of six choice cards, table 4.3 presents the results of 12 paired comparisons. For all choice task comparisons between day 1 and day 2, the results show that the underlying preference structures are stable (column 5 in table 2). Thus, respondents do not seem to change their rates of trade-off between the attributes making up the policy alternatives when given overnight to reflect on their first choices and/or discuss with others within or outside the household. These results suggest that four or more changes made by 9.6% of the sample (figure 4.2) did not translate into significantly different utilities between day 1 and day 2 at the aggregate level.

Results suggest that differences in preference uncertainty between day 1 and day 2 as measured by the relative scale parameters or relative variance are choice set-specific, i.e. for choice set 1 in block 1 and choice sets 2, 4, and 5 in block 2, choices became more random on day 2 whereas we observe an increase in preference precision for the remaining choice sets overnight. However, these patterns are not statistically significant at 5 % level (column 10 in table 4.3).

Enumerators also rated respondents' certainty for each choice set. Responses were identified on a 5 point Likert scale where 5 means that respondents seemed very certain and 1 means that they seemed completely uncertain. We are interested in whether respondents became more certain and confident when given more time to reflect more on their choices or discuss with others. Results (figure 4.3) suggest that rated choice certainty measures are generally very high (>4) and are not statistically different across each pair of choice tasks between day 1 and day 2, supporting the results of the relative scale parameters (table 4.3).

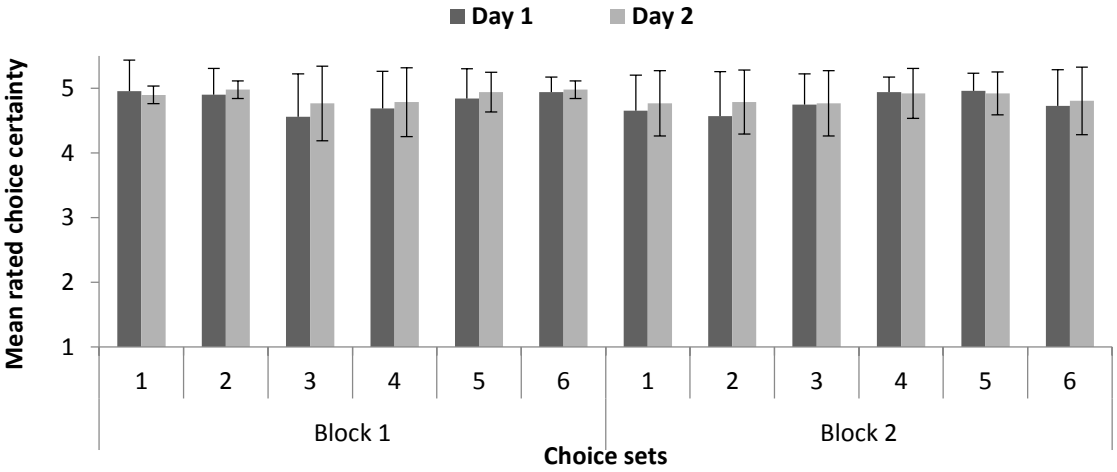


Figure 4.3: Respondents' choice certainty rated by enumerators

Certainty codes (scored by the enumerators for each choice set using a 5 points Likert scale)

1: Very uncertain, changed his/her choice many times, asked the interviewers to re-explain the choice cards again one or two more times, took a long time to finally make up their mind
2: Uncertain, the household took also very long but a little bit quicker than 1
3: So, so, seems a bit uncertain but at least when they made his/her choices eventually, they seemed certain.
4: Certain of his/her choice, the respondent was quick and didn't request any additional explanations
5: No doubt at all, relatively quick, made up his/her mind after the first round of explanation.

Table 4.3: Test results for equality of preference and scale parameters between choice tasks on day 1 and day 2

Block	Choice task	Nobs	1	2	3	4	5	6	7	8	9	10
			LL day 1	LL day 2	LL pooled[a]	LR-test (8 d.f.)[b]	Reject H_0= $\beta_1 = \beta_2$	Rel. scale λ_2/λ_1	Rel. var. σ_2^2/σ_1^2	LL pooled[c]	LR-test (1 d.f.)[d]	Reject H_0= $\lambda_1 = \lambda_2$
1	1	52	-50.30	-56.15	-107.39	1.88	No	0.50	4.00	-108.79	2.8	No
	2	52	-47.63	-42.13	-90.17	0.82	No	1.00	1.00	-90.17	0.00	No
	3	52	-45.40	-41.66	-87.41	0.70	No	1.05	0.91	-87.49	0.16	No
	4	52	-48.44	-43.16	-91.6	0.00	No	1.35	0.55	-91.97	0.74	No
	5	52	-55.64	-55.63	-111.59	0.64	No	1.00	1.00	-111.59	0.00	No
	6	52	-43.10	-37.40	-80.60	0.20	No	1.35	0.55	-81.05	0.90	No
2	1	52	-51.76	-46.86	-98.64	0.04	No	1.45	0.48	-99.08	0.88	No
	2	52	-52.81	-56.67	-109.44	-0.08	No	0.35	8.16	-110.34	1.80	No
	3	52	-50.82	-50.80	-101.65	0.06	No	1.00	1.00	-101.65	0.00	No
	4	52	-51.16	-55.95	-107.12	0.02	No	0.5	4.00	-108.09	1.94	No
	5	52	-54.13	-55.22	-109.62	0.54	No	0.75	1.78	-109.69	0.14	No
	6	52	-45.57	-46.60	-92.87	1.40	No	1.00	1.00	-92.87	0.00	No

[a] Pooled mixed logit model allowing scale parameter to vary
[b] Critical value for chi-square test with 8 degrees of freedom at 5% level is 15.30
[c] Pooled mixed logit model forcing the scale parameters to be equal
[d] Critical value for chi-square test with 8 degrees of freedom at 5% level is 3.841

4.4.3 Effect of more time to deliberate on individual-level preference parameters

The mixed logit model on day 1 suggests that utility significantly decreases if forest clearance is restricted to only a one-off one hectare permit or totally prohibited (no clearance). Respondents positively valued the total cash donations, and the support for improved rice farming. They preferred that payments were spread over 20 years to a lump sum payment, ceteris paribus. We estimated random utility parameter differences (RPD) for each attribute between the first and second DCE responses (figure 4.4) while accounting for differences in scale factors between day 1 and day 2. The relative scale parameter which is the scale for day 2 relative to day 1 (which is fixed to 1) is 0.91 with a 95% confidence interval of [0.65 – 1.25] suggesting that that there is no difference in unobserved variance or respondents' uncertainty between day 1 and day 2 at the aggregate level. The log likelihood (-1368.43) and its 95% confidence interval [-2974.69; -2400.08] imply that the model is statistically significant. The random parameter differences (computed from equation 4.7) suggest that none of the attributes significantly differs between day 2 and day 1, i.e. the utility parameters observed on day 1 are not significantly different from those on day 2 indicating that more time to deliberate does not significantly affect respondents' preferences for each attribute at the aggregate level.

The 20-year instalment indicates switching of patterns between day 1 and day 2 while the improved rice project individual parameters indicate a consistent reduction on day 2 at the lower end (figure 4.5). The one hectare permit pattern is more of a hybrid while the ASC[52] suggests that respondents who are more reluctant to enter a policy (i.e. those who prefer the reference level scenario of "no protection") become even more reluctant on day 2 whereas those who are keen to enter a policy alternative become keener.

[52] Such interpretation however warrants caution as all the variables are dummy-coded except the cash donations, which means that they are correlated with the ASC.

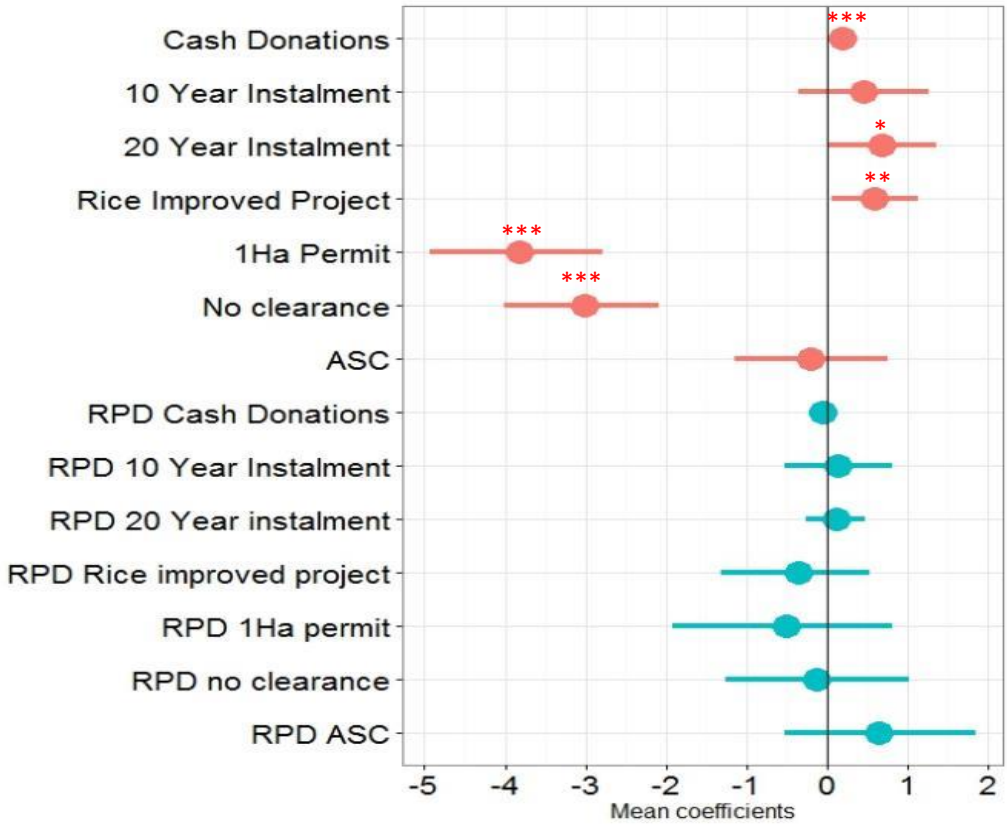


Figure 4.4: Effect of more time to deliberate on individual-level preference parameters - Utility parameters on day 1 and random parameter differences (RPD) between day 2 and day 1. Note: *, **, * ➔ Significance at 1%, 5%, 10% level**

Examining the individual-level parameters (Figure 4.5) from which the random parameter differences were calculated, we however observe that the effect of more time to deliberate is variable, i.e. more time to deliberate does not have a uniform effect on everyone as the direction of the change varies.

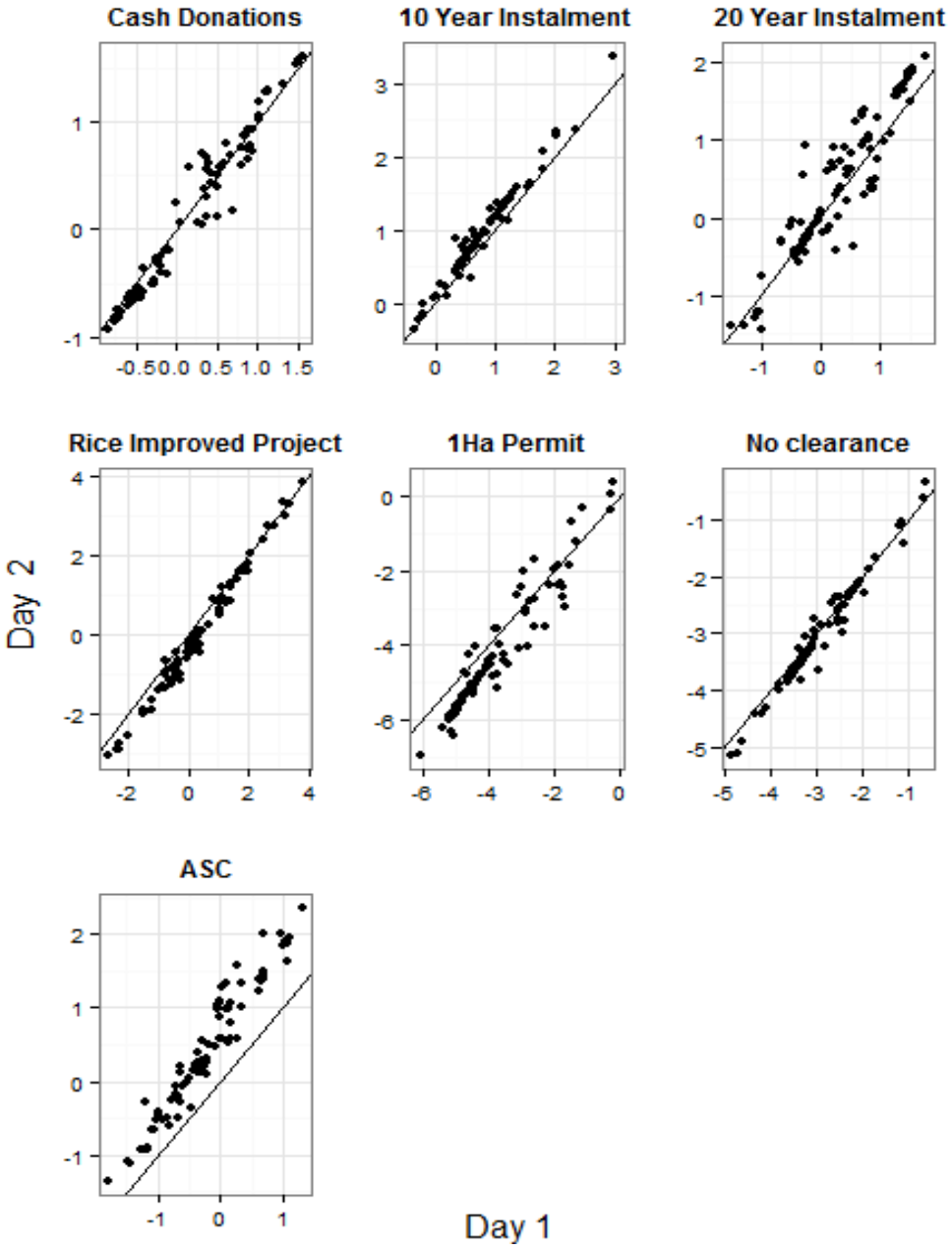


Figure 4.5: Individual random parameters for each attribute on day 1 and day 2

If we control for socio-demographic variables in the model, the average utility that illiterate households expect from the strict forest protection significantly decreases on day 2 (mean RPD_No Clearance = -1.58, figure 4.6), i.e. the negative utility of strict protection on illiterate households becomes more pronounced on day 2. On the other hand, literate respondents are more likely to revise upwards the utility from the strict protection (no clearance) when given overnight to deliberate i.e. literate respondents became more inclined to prefer strict forest protection to an open forest frontier

on day 2[53], all other things equal. Literate household heads also favour the cash donations less when given the chance to revise their answers whereas those who are illiterate expect greater utility from cash donations on day 2. However the mean RPD_Cash donations for both literate and illiterate households[54] are not significantly different from zero at 5% level.

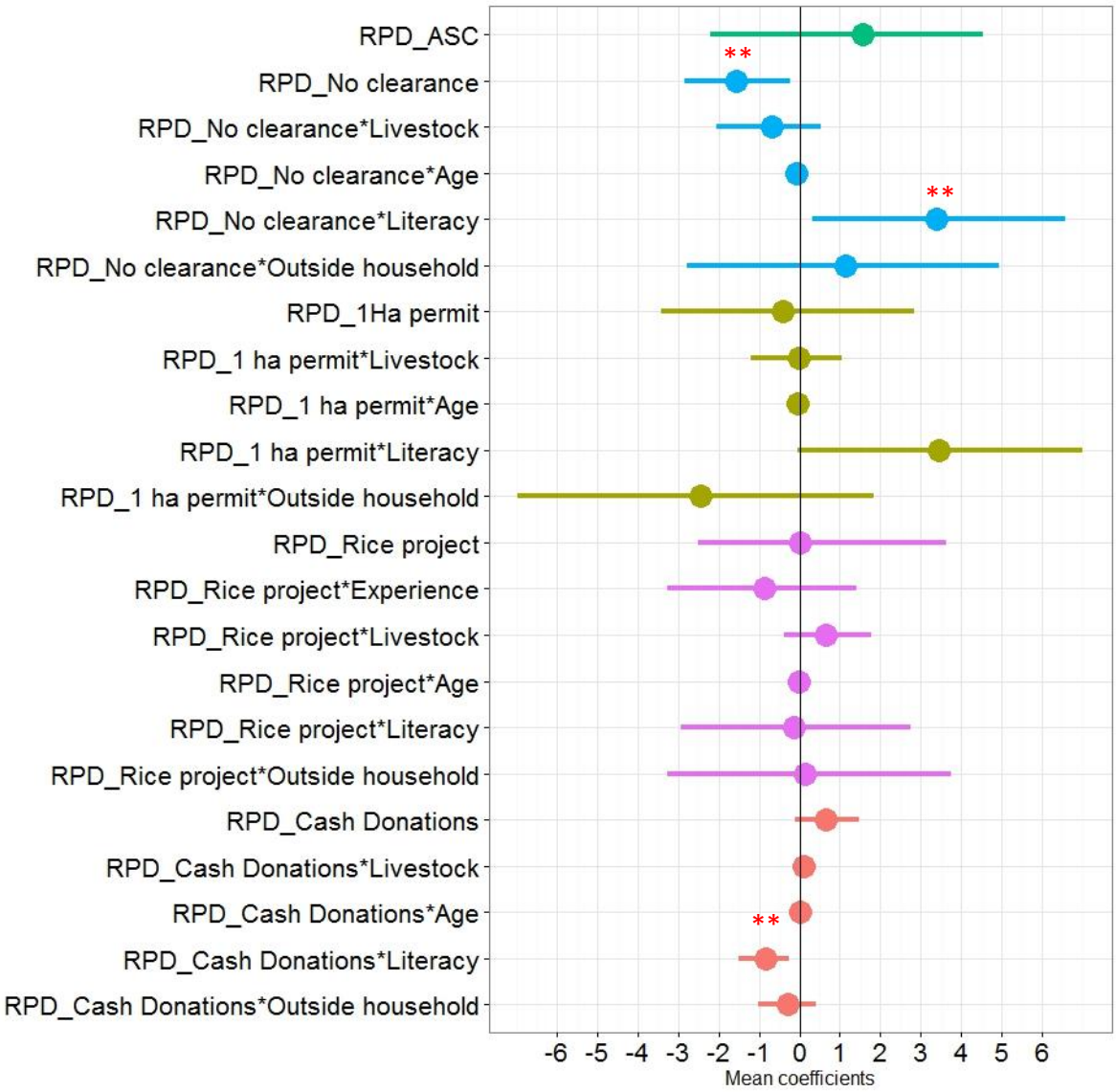


Figure 4.6: Mixed logit model results where the random parameter differences are interacted with covariates, ** indicates significance at 5% level

[53] Mean RPD_No clearance for literate households = mean RPD_No Clearance + mean_RPD_No Clearance * Literacy = -158 + 3.38 = 1.80 which is statistically different from zero at 5% level, p=0.035

[54] The utility coefficients of literate and illiterate households for the cash donations attribute are represented by the RPD_Cash donations and RPD_Cash donations + RPD_cash donations*Literacy, respectively.

4.4.4 Effect of more time to deliberate on marginal WTA estimates

We estimated mean WTA compensations for a total prohibition of forest clearance and one hectare of forest clearance permit (figure 4.7). Mean respondents' WTA compensations for strict forest protection were significantly higher on day 2 than day 1 at 10% level whereas they are not statistically different for one hectare of forest clearance permit. More time to deliberate does however have a large and significant effect on respondents' WTA compensations when the latter are decomposed by different groups of respondents. Less literate respondents significantly revised their WTA estimates upwards when given overnight to deliberate whereas literate respondents required smaller compensations both for strict forest protection and 1 ha of forest clearance permit. Also those who reported that they discussed with others outside the household expressed significantly smaller compensation estimates for forest clearance restrictions on day 2, whereas those whose discussion was limited within the household stated larger compensations (although differences are not statistically different) (figure 4.7).

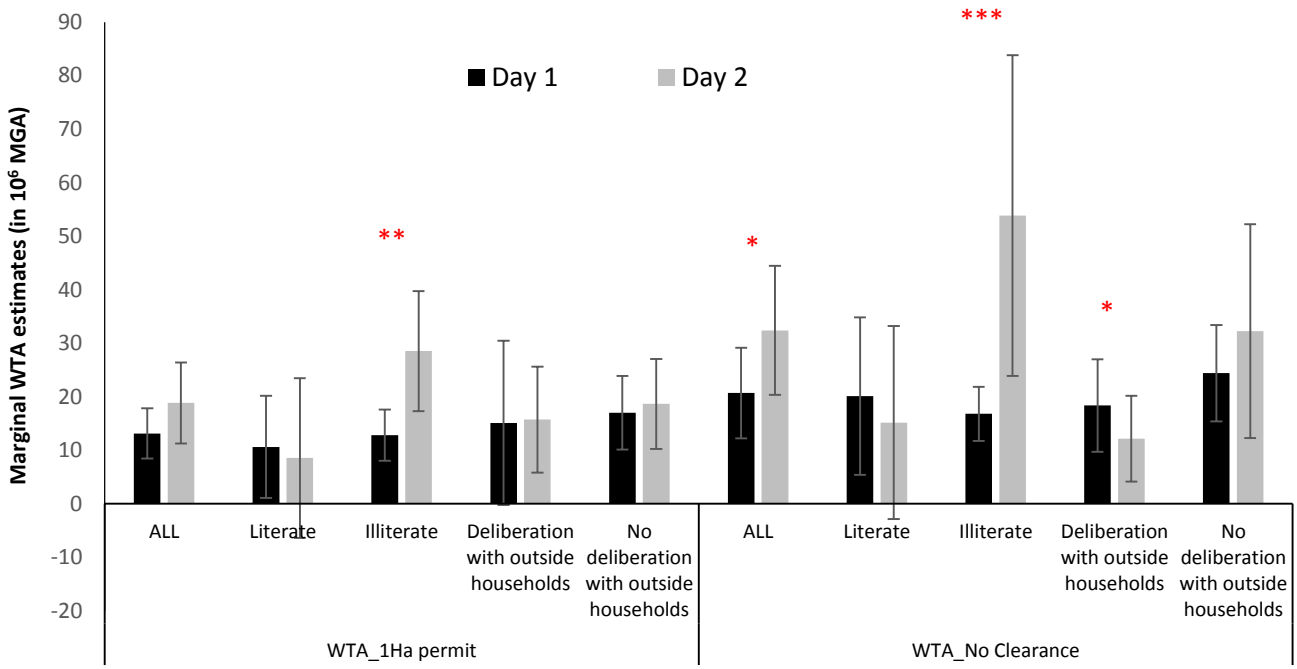


Figure 4.7: Marginal WTA estimates (in 10^6 MGA; 3080 MGA = 1 USD) on day 1 and 2 for the total sample and by literacy and deliberation with others outside the household. Note: *, ** ,* ➔ Significance at 1%, 5%, and 10% levels**

4.4.5 Qualitative debriefings

4.4.5.1 Respondents' characteristics

Table 4.4 presents the characteristics of the sub-sample of households who were debriefed after the second round of DCE survey to understand the mechanisms by which more time to deliberate might have influenced their responses. The raw response data show that four of them changed responses to at least four choice sets, two of which were literate (I5 and I10), two are less literate (I11 and I12).

In what follows, we first describe the accounts of households who made fewer than four changes on day 2 and then report the motivations of households who made 4 or more. We used also the open-ended follow-up question asked to all respondents about why they had (or had not) revised their answers.

Table 4.4: Characteristics of the qualitatively debriefed respondents (N=14)

ID	Changed responses		Literate	Years of education	Household head age	TLU	Food security	Deliberated with others outside the household
	1-3	≥4						
1	1	0	Yes	5	42	0.49	6	No
2	0	0	Yes	5	63	0.10	12	No
3	0	0	Yes	6	39	0.06	11	No
4	0	0	Yes	5	34	0.08	7	No
5	0	1	Yes	1	55	0.80	12	Yes
6	1	0	Yes	11	47	0.50	12	No
7	1	0	Yes	3	45	6.00	10	No
8	0	0	Yes	3	21	0.00	5	No
9	1	0	No	0	53	2.20	8	No
10	0	1	Yes	6	24	0.04	6	Yes
11	0	1	Yes	2	54	0.10	5	No
12	0	1	No	0	37	0.06	7	No
13	0	0	Yes	1	36	0.02	5	No
14	0	0	Yes	3	64	0.74	10	No

4.4.5.2 Debriefing findings: Why did households' behaviour remain unchanged overnight?

Among the 14 debriefed households, we found only one case (I8) where the household clearly showed evidence of recall, the follow-up question asked to all respondents suggest that four other households showed similar behaviour. The household head in I8 simply tried to remember his first answers and avoid contradicting himself, without any evidence of deliberation or discussion with other household members or others outside the household. As I8 asserted:

> "What the tongue has twisted can no longer be straightened." (I8, literate, 21 years)

Although more time to deliberate did not affect the choices of 10 debriefed households (see table 5), the majority of their accounts suggest evidence of some reflection over the choices made on day 1. They all also reported to have spent time discussing the first elicited choices within their households.

> "I still spent time ruminating carefully over the questions, because you know when someone asks you something, you just answer without giving it too much thought. You then think about all the details and what the implications are only when that person walks away from you. (I2, literate, 63)

None of these households reported to have discussed with their neighbours or others outside their households (table 4.4). The additional time provided them with more opportunities to digest the survey scenarios and ask questions.

> "The fact that you are independent of the government made us think last night. Where did you find the money to buy the gifts that you gave us yesterday? Who will benefit from your research? Are they only those that you interviewed or will you be able to help the whole community?" (I3, literate, 39 years)

Their decisions not to revise their responses mostly stem from the high certainty that they felt about their first answers. There are indications their preferences had been fully constructed at the first round of DCE survey. They all seem to have a crystallized idea of what different survey scenarios (e.g. restriction of forest clearance, cash donations, or an open forest frontier) would entail for their livelihoods (e.g. I1 to I4, I9, I11, I13, I14). Choice decisions on day 1 appeared to be robust for these respondents and reliable for inclusion in decision-making.

> "Life here always ends with lands, everything you do revolves around lands…lands (accessed through forest clearance) are the most valuable inheritance you can leave to your children, as long as I breathe, nothing is ever better than an open forest frontier… I am grieving that people are put in jail because they are making a living from the forestlands which are Zanahary's creation, not the government's property." (I9, illiterate, 53 years)

> "The cash would enable me to invest in my perennial crops, my main constraints are insecticides and fertilisers. I have no interest in clearing natural forests. I don't need one additional day or one more year to reflect over such decision." (I14, 64)

4.4.5.3 Debriefing findings: What motivated households to revise their choices?

More time to deliberate resulted in more polarised responses mostly among less literate respondents, i.e. some households stopped trading off the no forest clearance policy with the cash donations or the improved rice project on day 2 and consistently chose the reference level alternative (i.e. open forest frontier) (e.g. I11, I12). These households claimed that they do not cast doubts on the plausibility of the survey scenarios but their decisions to revise their choices stemmed mostly from more reflection and extensive discussion within their household. Additional time made them realise that the proposed alternatives to forest clearance are neither sustainable nor economically viable.

> "We decided to revise our responses because we are not good with pencil and paper. We ruminated over the impacts of forest protection, and our abilities to use the cash, we now believe that an open forest frontier is the best should we really have the freedom to choose." (I12, illiterate, 37 years)

Some respondents' accounts however suggest evidence of strategic considerations. More time to deliberate encouraged them to reflect more on the consequentiality of the DCE survey (i.e. how much they believe the results would be used to inform policy) which incentivized them to develop some strategies. Such behaviour mostly affected those who discussed their choices with others outside their household and those who are literate (e.g. I5 and I10). Interaction with other households promoted

awareness of the heterogeneity of choices as well as other people's strategies and encouraged them to revise choices in a way that they thought would make their preferred policy option more likely.

> "There might be some people who traded-off forest clearance with money but who are still hoping to infringe the restrictions. It would be unfair to those who rejected the payments. As everyone may ultimately get one policy, I decided to switch to a more explicit trade-offs between forest protection and cash. "(I5, literate, 55 years)

Likewise, I10's decision to significantly revise choices stem from a greater consideration of the global benefits of forest conservation in addition to concerns of missing out on the cash donations:

> "Anyway, the forest is a global wealth, and the all world's lives depend on it. We worry that if we do not accept the money, we will be left with nothing if forest protection is strictly enforced. So after we discussed between ourselves, we decided to trade-off forest clearance restriction with the cash compensations." (I10, literate, 24 years)

4.5 Discussion

This paper examines the role of giving respondents additional time (one day) to evaluate their responses to a discrete choice experiment assessing the welfare impacts of forest protection in Madagascar. We found that more time to deliberate does not affect aggregate responses at the choice task level. Similarly, additional time to deliberate does not affect utility parameters for the aggregate sample. However, we found that the effect of deliberation depends on literacy: literate respondents favoured more the strict forest protection to an open forest frontier when given overnight to deliberate whereas less literate respondents' preferences for the strict protection (no clearance) significantly decreased on day 2. We also found that WTA estimates were significantly higher for illiterate households when given overnight to deliberate whereas literate respondents and those who discussed with others outside the household required smaller compensations (in terms of marginal WTA estimates) for strict forest protection on day 2.

Our results are different from previous stated preference studies (e.g. Cook et al. 2007, Whittington et al. 1992) using a WTP valuation format which suggest that respondents who were allowed time to reflect over the survey scenario bid significantly less than those who were not. Conversely, on average, our respondents' marginal WTA compensations for strict forest protection increased when given overnight to deliberate. An important explanation for the results found in this study may be the use of WTA format as opposed to its WTP counterpart in previous studies. More time to deliberate may have allowed WTP respondents to better factor their budget constraints in their decisions leading to lowered WTP estimates (e.g. Cook et al. 2007, Whittington et al. 1992). Whittington et al. (1992) argue that their follow-up questions provided limited support for strategic behaviour among respondents who had time to think and their stated values therefore represent better measures of respondents' preferences for the good being valued (WTP for improved water services). In our case study however, polarisation of preferences mostly among less literate respondents and strategic behaviour occurring mostly among literate respondents may have driven the observed WTA estimates upwards and downwards respectively on day 2. While more than 50% of the respondents in Cook et al. (2007) have attended secondary school (6-12 years of schooling), respondents' average years of education (3.04 years) in Whittington et al. (1992) were very similar to ours.

We also do not find that respondents' certainty increased on day 2 (either measured by the relative scale parameter or rated by enumerators) as opposed to previous studies which have shown an increase in respondents' stated certainty (e.g. Svedsater 2007; Cook et al. 2012). As people generally value losses (as measured by respondents' WTA) more highly than gains (measured by respondents'

WTP) (Knetsch 1997), we argue that respondents may have already put considerable efforts in answering our WTA survey on day 1 and had more fully constructed their preferences than they would have done in a WTP format where they are asked to pay for a good or a policy. Our respondents are also very familiar with the policy being valued which has major impacts on their livelihoods and on which they possess more crystallised attitudes and values. The low effect of deliberation on utility parameters in this study may also be attributed to the use of visual aids and extensive warm-ups steps before the actual choice sets. The warm-ups gave respondents some practice and helped establish trust with the enumerators. Given the sensitive nature of the good we valued, warm-up steps were critical in desensitizing forest clearance as well as in ensuring that respondents understood the task of making trade-offs. Although these lengthy explanations considerably eased respondents' comprehension of the valuation exercise, we acknowledge that this may have also biased respondents' choice decisions. Too little information can mean that the respondent has an inadequate understanding of the scenario considered, but too much information may also confuse respondents or distort their true preferences.

The qualitative debriefings suggest that the lack of the effect of more time to deliberate on preference parameters for the aggregate sample is mostly explained by respondents' high certainty on day 1. Heightened reflection had nonetheless encouraged others to express more polarised choices when given more time. Such findings are not new in other areas of social science, responses to attitude surveys tend to become more polarised and conservative when respondents are given more time to evaluate the questions (Kaplan and Miller 1977). Other respondents' motivations to revise choices on day 2 also stemmed from strategic considerations, their revised answers heavily hinged on responses that could influence proposed policies in their favour instead of expressing their true values. These strategic respondents are mostly composed of literate household heads and those who had discussed with others outside the household, corroborating the modelling results (figure 8).

Our design however presents challenges which warrant caution in the application of our findings and for further research. The use of independent pre-test and post-test control groups (Teisl et al. 1995) might help isolate the effect of more time to deliberate from recall effects without lessening the deliberation time provided to the same individuals. However, such a design requires a larger sample size. Likewise, randomizing the order of choice cards might also reduce recall, but since order effect was shown to be significant in DCE studies, this could confound the effect of more time to deliberate unless the sample size was large enough to isolate this (Day and Prades 2010). Our test-retest design presents a considerable advantage over the split sample design used by Cook et al. (2007) and others as it allowed the very same respondents to reflect on how they changed their choices. The use of

qualitative debriefings, which focused on how and why respondents amend their preferences, proves to be invaluable in understanding the effect of deliberation on stated preference responses.

4.6 Conclusion

Evidence of more extensive deliberation among less literate respondents on day 2 suggests that these respondents may considerably benefit from more time to deliberate. More time to deliberate might not only increase respondents' comprehension of the tasks of making trade-offs in DCEs, but it also provides more opportunities for deliberation within the household. Deepened deliberation would also better mimic the real life situation in which people sleep on important matters before making decisions (Bos et al. 2011). However, the presence of strategic behaviour among more literate respondents on day 2 suggests that revised values may not be improved measures of respondents' preferences overall[55]. As the qualitative debriefings suggest that strategic considerations mostly stemmed from discussion with others outside their households, our findings warrant caution on the value of participatory or group-based deliberative techniques in general where participants consult with each other. If the objective is to include non-monetary or non-economic values such as shared or communal values appealing to social justice or legitimacy issues (e.g. Jacobs 1997; Ward 1999; Wilson and Howarth 2002) or to produce plural values which are deemed to have greater "democratic credentials" (Spash 2008; Lo and Spash 2013), then group-based deliberative techniques may be extremely useful. If, however, researchers are primarily concerned with improving the elicitation of individual monetary values within the neoclassical economic paradigm, approaches promoting dialogue among respondents may not be ideal.

The time to deliberate approach used in this study may provide less educated DCE respondents more opportunities to reflect on the survey questions. However, the overall effect may not be significant and strategic considerations may be a concern. Where resource or time constraints do not allow the testing of more time to deliberate within individual DCEs in least developed countries where low literacy may be a problem, extensive warm-ups and the use of visual aids may help reduce the cognitive burden of the DCE survey. Giving respondents more opportunities to think and discuss with other household members may result in better preferences for respondents who have lower cognitive abilities, but may not always make a difference where respondents are familiar with the good being valued and take the survey very seriously. Testing the effect of more time to deliberate for unfamiliar

[55] We however note that there's no evidence that strategic behaviour was absent from day 1, only that some individuals engaged in some strategic behaviour on day 2.

goods or complex environmental interventions which may significantly impact people's livelihoods should therefore be encouraged. Also, as our findings are not easily transferable to industrialised settings where respondents' socio-economic characteristics are in sharp contrast with ours, it would be useful to do further testing of the effect of more time to deliberate in different contexts and income settings.

Chapter 5. WILLINGNESS-TO-PAY OR WILLINGNESS-TO-ACCEPT? CONTESTED PROPERTY RIGHTS IN FOREST CONSERVATION IN MADAGASCAR[56]

ABSTRACT

Where property rights over forest resources are not explicit or are contested, the effectiveness of conservation may be undermined and it can be difficult to estimate the welfare impacts of forest conservation on local people. In particular, researchers face the dilemma of estimating respondents' Willingness To Pay (WTP) for rights to forests, or their Willingness To Accept (WTA) compensations for foregoing these rights. This paper has three objectives: 1) to examine the differences in the patterns of responses to the WTA and WTP formats, 2) to assess the performance of these two formats in this context on three criteria: their content validity, their acceptability to respondents, and respondents' ability to pay, 3) to investigate respondents' attitudes to conservation restrictions and property rights over forestlands. We elicited responses to both formats using a split sample design and qualitative debriefing interviews. The DCE survey examined the welfare impacts of forest protection within a REDD+ project in the eastern rainforest of Madagascar. We found that the format affected respondents stated preferences: 86% of WTA respondents strongly favoured support for an improved rice project and secure tenure for one hectare of forestlands whereas 53% of the WTP respondents showed the opposite preferences. The WTA format was perceived to be more plausible and consequential, led to fewer protest responses, and was more appropriate given very low incomes. Seventy-three percent of respondents did not accept the legitimacy of state protection and strongly aspired to secure forest tenure. The use of WTP may thus be inappropriate even if respondents do not hold formal rights over the resources. Our findings also suggest that current conservation models may not be viable and achieve just compensations.

[56] **Rakotonarivo, O. S**., Jacobsen, J.B., Poudyal, M., Rasoamanana, Hockley, N. Willingness-To-Pay or Willingness-To-Accept? Contested property rights and protest beliefs in forest conservation in Madagascar. Manuscript submitted for publication.

5.1 Introduction

By forming and restoring soils, forests have underpinned agriculture worldwide (Sunderlin et al. 2005). The removal of forest cover provides access to fertile soils to millions of small farmers in the tropics, and has therefore supported their livelihoods for decades (*ibid*). In most low-income tropical countries, the conversion of natural forests to small scale swidden agriculture has been described as the main proximate cause of deforestation (van Rijnsoever et al. 2015). Primary forests continue to be used for swidden cultivation (Laurance et al. 2014; Kim et al. 2015a). Small farmers often view swidden agriculture as a low labour, low capital, and risk minimising farming strategy promising greater flexibility than more intensive agricultural systems that require onerous investments and technical training (Nielsen et al. 2006; Scales 2014a). Clearing forests for swidden agriculture may provide higher returns to local communities than leaving them standing (Dove 1983; Godoy et al. 2000). Local people may therefore incur welfare losses from conservation actions restricting forest clearance.

Protected areas are seen as a major conservation tool for preserving biodiversity. The continuing habitat loss in the tropics has motivated their expansion and the setting of more stringent protection targets (Perrings et al. 2010). However, much of the protected area network in low-income countries is characterised by considerable confusion and dispute over property rights. While governments often have de jure ownership of forestlands in many tropical countries, they have often been unable to enforce this claim owing to complex factors including funding shortfalls, recurrent political instability and exceptionally high levels of corruption (Bruner et al. 2004; Struhsaker et al. 2005). In addition, state ownership is often contested by indigenous communities who claim customary rights over forestlands through settlement (White and Martin 2002). Despite long-standing customary ownership rights, local communities may only be devolved the responsibility to manage forest resources (Dressler et al. 2010). Property rights to forestlands are clearly a key and contentious issue in forest conservation in many tropical countries.

Ambiguous property rights also pose challenges to the use of stated preference techniques in the ex-ante valuation of the welfare impacts of forest use restrictions. While discrete choice experiments (DCE) have been successfully used to value local people's willingness-to-accept compensations to reduce illegal hunting activities in Tanzania (Kaczan et al. 2013; Moro et al. 2013; Nielsen et al. 2014), asking willingness-to-accept questions when local people do not perceive any rights over the good being valued leads to biased results (Freeman 2003). Property rights are theorised to be the most important criterion determining the choice between willingness-to-pay and willingness-to-accept formats. Theoretically, for a given policy which negatively affects an individual's utility, the maximum

amount that an individual would be willing to pay to avoid this policy is a measure of equivalent surplus (ES), i.e. the monetary payment (or the equivalent in-kind) that would lead to the same utility as without the policy (Freeman 2003). In other words, if local people are not entitled to the rights to own and clear forests for swidden agriculture, ES would be a measure of their willingness-to-pay to avoid incurring losses from forest use restrictions. Conversely, if local people perceive property rights over forestlands, they will need to be compensated for the negative impacts of forest use restrictions on their livelihoods; compensations required by local communities to voluntarily tolerate the welfare loss is a measure of compensating surplus (CS), i.e. the monetary gain (or in-kind gains) that would make them as well off as without the policy. For the remainder of this article, property rights refer to a complete set of rights[57] over forestlands as defined by Schlager and Ostrom (1992, p250-251 referring to access, withdrawal, management, exclusion and allienation rights).

The choice of the welfare measure (CS vs. ES[58]) matters because they are predicted to be unequal as an individual's WTP is constrained by his/her ability to pay whereas an individual's WTA compensation does not have an upper limit (Freeman 2003). CV and ES have been shown to be consistently different (Horowitz and McConnell 2002; Tunçel and Hammitt 2014). While the effect of the format on welfare estimates has been well demonstrated, the choice of format might also affect the relative importance of the attributes valued in a DCE survey (as suggested by their sign and statistical significance) – the nature of such differences being less researched. The first aim of this paper is therefore to examine the differences in the patterns[59] of responses between the WTA and WTP formats. We elicited responses to two analogous DCE surveys which are both designed to value the welfare impacts of use restrictions ex-ante, i.e. the scenarios asked respondents to evaluate expected future losses from forest conservation policy. We used a between-subject design; half of the sample was asked their WTP to avoid the welfare losses by purchasing legal permits for forest clearance while the other half was asked their WTA compensations to forego forest clearance rights. Both DCE surveys assessed the trade-offs that local people make between their rights to clear new forests for agriculture, cash payments and support for improved rice farming.

[57] Households may however customarily perceive less than these full sets of rights and the reality often involves a complex operationalisation of these bundles of rights (e.g. Muttenzer 2006).

[58] A compensating surplus is estimated by: "How much money would just compensate you for tolerating X?" (a WTA question), whereas "How much money would you be willing to pay to avoid X?" (a WTP question) elicits the equivalent surplus.

[59] While both the WTA and WTP valuation scenarios asked respondents the amount of money that would make them as well-off as without forest conservation, our objective is not to strictly compare measures of compensating and equivalent surplus.

A second aim of this paper is to assess the performance of the two formats for estimating the welfare losses from forest conservation policy in low-income countries on three criteria. The first criterion comprises measures of content validity, i.e. respondents' attitudes towards the features of the survey scenario in each format. For example, whether respondents found the survey scenarios plausible or believed in the consequentiality of the survey (i.e. whether respondents care about the survey outcomes and view them as having real policy impact, see Carson and Groves 2011; Vossler et al. 2012). Therefore, the best format results in the least problematic attitudes to the survey. The second criterion concerns their acceptability to respondents measured by the rate of refusal to trade off in a format due to a lack of compatibility between respondents' ethical beliefs and the given format. When property rights to forestlands (or other resources) are contested, respondents may have ethical beliefs towards the policy that conflict with the selected format resulting in high rates of refusals to trade-off, also known as "protest responses[60]" or "status-quo effect" (Meyerhoff and Liebe 2009). The best format therefore minimises the percentage of respondents who are not willing to trade-off different attribute levels because of ethical beliefs. The third criterion pertains to budget constraints; a very low substitutability between the good and money may severely constrain respondents' ability to pay, which might argue against the WTP format. In addition to a set of quantitative follow-up questions to the DCE survey and respondents' socio-demographics, we also address this objective with qualitative debriefing interviews with a sub-sample of respondents.

Our third aim is to investigate respondents' attitudes to conservation restrictions (irrespective of the valuation format) and perceptions of property rights, and discuss the policy implications for REDD+ (Reducing Emissions from forest Degradation and Deforestation) policy. REDD+ is involuntary for most local people, who may be coerced into accepting it (Corbera 2015). As such REDD+ may lack legitimacy and undermine social justice (Corbera 2012; Martin et al. 2013). Strict enforcement of restrictions in

[60] Meyerhoff and colleagues (2008; 2009; 2014) defined protesters as "respondents who reject or protest against some aspect of the constructed market scenario". Mitchell and Carson (1989, p166) defined them as respondents who "refuse to play the game" economists want them to play. While treatment of protest responses can significantly influence the results of valuation studies, there is no agreement about what qualify as "protest responses" nor about the treatment of protest responses in subsequent analyses. Definitions of protest response are case study specific; a respondent who would be defined as a protester in one study might be seen as a valid zero response in another study. For example, respondents who considered the act of paying unfair were categorised as protesters by Strazzera et al. (2003) whereas Jakobsson and Dragun (2001) did not consider fairness but defined protesters as those who objected to assessing nature in monetary terms. In this study, while responses from respondents who expressed a disbelief in the legitimacy of the state's forest conservation policy may be qualified as "protest responses"; and can be argued to be an underlying attitude which is not directly related to the survey, we abstained from using the term "protest" given the ongoing debates and controversies associated with that term.

such a context may also impose local welfare losses that may not be mitigated by proposed compensation schemes (Martin et al. 2013). Justice principles enshrined in forest conservation policies in the tropics may not often align to local perceptions of just and legitimate environmental management[61] (Martin et al. 2014). Investigating respondents' attitudes and ethical beliefs towards forest conservation may therefore have important implications for conservation approaches in the tropics.

Our study site is part of the Ankeniheny - Zahamena corridor REDD+ project in Madagascar which was formally gazetted as a protected area in 2015. Before then, the forest corridor was subject to successive waves of immigration mixing with well-established customary rights, and resulting in the emergence of new rules and property rights regimes which are clearly misaligned with the formal property rights vested exclusively in the state. These customary regulations determine who has rights to clear the land and how, depending on whether the individual belongs to an indigenous lineage, longer established settlers or more recent migrants (Pollini et al. 2014). In the next section, we review the literature on the WTA – WTP disparity and situate the novel contributions of this study. We then describe the valuation scenarios and data analysis in sections 3 and 4 respectively. Results are presented in section 5, followed by the discussion and conclusion in sections 6 and 7.

5.2 The WTA – WTP disparity: Review of the literature

Discrete choice experiments, a stated preference approach, have appealed to environmental economists by allowing the valuation of individual attributes of environmental goods without relying on existing markets. The construction of hypothetical scenarios is particularly useful for valuing environmental interventions in remote areas of developing countries where factor markets are undeveloped, and customary rights are rarely aligned with formal property rights. DCEs have been largely used to estimate respondents' willingness-to-pay for new environmental policies such as the preservation of a wide range of ecosystem services in developing countries (e.g. Barkmann et al. 2008; Beharry-Borg et al. 2009; Do and Bennett 2010; Kenter et al. 2011; Mombo et al. 2014). DCEs estimating local people's willingness-to-accept compensations for environmental interventions are scarcer, most of them value agri-environmental schemes in industrialised settings (e.g. Espinosa-Goded et al. 2010; Christensen et al. 2011; Cranford and Mourato 2014), and we know of few DCE

[61] For example, the "risk-based approach" of the social safeguard standards by World Bank, which focusses on mitigating the negative impacts of REDD+ may not be shared by local people who instead demand help to "live with such risk" (Martin et al. 2014, p168) i.e. an approach that goes beyond no harm and improves their welfare.

applications in developing countries which evaluate WTA compensations for conservation restrictions (Kaczan et al. 2013; Moro et al. 2013; Mulatu et al. 2014; Nielsen et al. 2014).

There is widespread evidence of a consistent discrepancy between a person's willingness to pay (WTP) for a good and his willingness to accept (WTA) compensation to forego the same good. The valuation literature suggests that willingness to accept will generally exceed willingness to pay measures and the WTA-WTP ratio ranges from 1.95 to 10.41 (Horowitz and McConnell 2002). The WTA–WTP disparity has been observed for private and public goods, as well as market and non-market goods in real, hypothetical and experimental settings (Horowitz and McConnell 2002; Tunçel and Hammitt 2014). Standard Hicksian economic theory provides two explanations for the WTA-WTP disparity (Randall and Stoll 1980). The first concerns the income effect: WTP is strictly limited by budget constraints while WTA is not. The size of the difference between WTA and WTP responses therefore depends on the magnitude of the income elasticity of WTP, i.e. the responsiveness of WTP to changes in income. The income elasticity of WTP largely depends on the elasticity of substitution between the policy being valued and money (Bateman et al. 2002). The second is related to the first but involves a closer examination of the theory of preferences and relates to the availability of substitutes for the good being valued (Hanemann 1991; 1999): The income elasticity of WTP can be expressed as the ratio of income elasticity of demand for an environmental good being valued (*q)* and the aggregate *Allen-Uzawa* elasticity of substitution between *q* and its composite substitutes (denoted σ_q); if the good being valued has few close substitutes (as is often the case with unique and irreplaceable resources), i.e. the elasticity of substitution is low, σ_q can be close to zero leading to a high value of the income elasticity of WTP, and hence a large disparity between WTA and WTP values.

The WTA-WTP disparity may also reflect limitations in the standard theory; prospect theory provides the most prominent alternative (Kahneman and Tversky 1979). People define gains and losses based on a reference point, normally the status quo, and losses measured relative to this reference point have greater subjective significance than gains. Thaler (1980) proposed an extension and generalisation of the prospect theory to choices not involving uncertainty, by postulating an endowment effect on individuals' valuation functions. The endowment effect refers to the notion that goods are considered to be more valuable when they are part of a person's endowment than when not in the endowment, all else equal (Kahneman et al. 1991).

Another alternative explanation for the WTA-WTP disparity is the lack of information, and respondents' uncertainty, about their true values for complex or unfamiliar goods (Hoehn and Randall 1987). An individual who is uncertain about the true value of an environmental change would tend to

state a WTA that is greater than his true WTA, or a WTP that is less than his true WTP. Thus, on average the difference between stated WTA and WTP would be greater than the true difference. Since it is unlikely that respondents have formed refined preferences for most environmental goods, the problem of uncertainty may not be resolved entirely. Another possible contributor to the disparity is strategic bias (Bateman et al. 2002). When respondents are asked a WTP valuation question, they may engage in bargaining behaviour and express an offer that is lower than their true preferences in the hope of getting the good at a price below the maximum they would be really prepared to pay. Likewise, some respondents may react as a seller when inquired about their WTA to forego some good, and ask more than their true values in the hope of getting a better deal. A further possible factor (which may also be viewed as a strategic behaviour), is "protest responses". If a respondent disagrees with the policy being valued, for example he has a moral objection to paying for nature conservation in a WTP survey because he thinks those who would damage nature should be the ones paying; he may state zero WTP estimate or be unwilling to trade-off the attributes described in a DCE survey. Such protesting therefore exerts a downward influence on WTP values. Similarly, if a given policy has other less tangible negative impacts on a respondent that are not reflected in the monetary nature of a stated preference survey, a respondent may say that there is no amount of money that he would accept in compensation, which often result in exceedingly high WTA estimates.

Despite being a major validity issue in environmental non-market valuation and particularly stated preference techniques, there are few DCE studies[62] which explicitly examine the WTA-WTP asymmetry for the same good or policy. Most existing DCE studies designed the survey to allow respondents to trade both improvements and deterioration in the levels of attributes against the reference level, entitling them to both 'buy' or 'sell' the attributes (e.g. Hess 2008; Bateman et al. 2009; Lanz et al. 2010; Masiero and Hensher 2010; Glenk 2011). While such designs explicitly allow a measure of WTA-WTP ratio, they have not explicitly framed the valuation questions in terms of WTP and WTA, nor have they elicited whether the target population actually perceives a property right to the good being valued. By explicitly asking respondents to think in terms of receiving or paying money, and following up with debriefing questions, researchers may identify alternative explanations to the WTA-WTP

[62] Most of the previous studies examining the WTA-WTP discrepancy are also contingent valuation studies which only allow the valuation of equivalent or compensating surplus for a given environmental change. As DCE enables the estimation of marginal WTA or WTP estimates associated with a change in one attribute level, it provides a richer setting for the assessment of the WTA-WTP disparity.

disparity that have been to date less researched. We only know of one DCE study which used a between-subject design where respondents are explicitly asked to answer either a WTP or WTA valuation scenarios; MacDonald et al. (2010) examined the WTA-WTP disparity for changes in the reliability of household water services.

5.3 Study design

5.3.1 Study site and design

Tavy is the widely used term for swidden agricultural in Madagascar whether in primary or secondary forests. Clearance of primary forest in the tavy system is known specifically as *teviala* and has been the flashpoint for environmental conflict in Madagascar for centuries[63]. The Corridor Ankeniheny-Zahamena Protected Area aims to reduce deforestation in the eastern region of Madagascar and has been regarded as one of the top conservation priorities of the island. It is the site of a pilot REDD+ project financed by the World Bank's BioCarbon Fund. It encompasses one of the largest remaining blocks of rainforest in Madagascar which spans 382,000 hectares and was formally granted a category VI protected area status in April 2015 (Republic of Madagascar 2015). It is co-managed by the Ministry of Environment in Madagascar, Conservation International, and local community associations. Major pressures include expansion of agricultural lands through forest clearance as well as illegal logging and artisanal mining (Ratsimbazafy et al. 2011).

Our study focuses on one *fokontany* (the smallest administrative unit in Madagascar), Ampahitra, in the south-west corner of the corridor where most farmers in the study area rely on *tavy* for their subsistence. Since no map or census of households was available, we worked with key informants at the *fokontany* level to construct a sketch map showing locations of all villages in the study area. We identified eight villages along the border of *Ankeniheny-Zahamena* corridor. We visited each village and carried out detailed mapping of the households ensuring that no isolated household was missed out. We identified in total 417 households residing within our study site across the eight villages. With the aim of interviewing a minimum of 200 households in total (at least 100 for each survey format), we randomly sampled at 65% allowing for replacement from each village (proportional random sampling) and surveyed 203 households in total. Surveyed households were randomly allocated one of the two DCE formats (WTA or WTP), resulting in a total of 102 WTA and 101 WTP responses. Of the

[63] Section 1.2 (last two paragraphs) of the introductory chapter describes the history of *tavy / teviala* regulations in Madagascar.

sampled households who were approached for the survey, only two declined to be interviewed, and three withdrew from their interviews before completion. The DCE surveys were piloted in three phases between February and June 2014 in nearby villages. The actual surveys were carried out from June to August 2014.

We also conducted qualitative debriefing interviews with a sub-sample of respondents after they had completed the DCE questionnaire surveys (N=11 and 9 for the WTA[64] and WTP sample respectively, i.e. 11% and 9% of the total sample). Interviews took place the day after the DCE survey with both the household head and his spouse[65] whenever feasible, and lasted from 30 to 90 minutes. Interviewees were purposefully recruited to represent the full range of DCE responses to both the WTA and WTP surveys and the interview guide was aimed at understanding respondents' motivations for their preferences as well as their thoughts' processes. Similar inquiries have been carried out by a handful of environmental DCE studies, but hitherto have been confined to developed countries (e.g. Clark et al. 2000; Powe et al. 2005), mostly using focus group discussions. The focus group setting also allows participants to deliberate and share understandings or any issues raised by the DCE. However, in our situation where a very sensitive issue is at stake (illegal swidden agriculture), we felt that individual interviews were more appropriate and avoided the influence of other participants in a focus group setting. The number of interviewees was determined by data saturation, i.e. we progressively built up a consistent representation of respondents' views and perspectives until a point was reached when no new information was retrieved. Interviews were audio-recorded with respondents' consent.

5.3.2 Choice experiment design

The attributes and levels (table 5.1) were informed by three focus group discussions and pilot testing of the design with 50 respondents (See Appendix A for the DCE experimental design). The DCE questionnaires were administered by a team of five enumerators who all held at least a bachelor's degree in agricultural sciences from the University of Antananarivo. While we used the same format for the WTA and WTP survey, the magnitude of the ES and CS are not strictly comparable because the payment levels differ in the two formats. The payment levels were determined by intensive piloting[66]

[64] The WTA debriefed sample (N=11) are the same households debriefed in Ampahitra in chapter 3. The interview guide used in chapter 3 was supplemented with additional questions to specifically address this chapter's aims on the differences in the patterns of responses to the WTA and WTP formats.

[65] 95% of the surveyed households are male-headed.

[66] The WTP format particularly necessitated three stages of piloting.

and in the WTP format are influenced by respondents' ability to pay as well as other contextual factors such as the current land trading or leasing agreements in the study site. Although we aimed to have the same payment levels, piloting demonstrated this to be impossible: an acceptable level of trading off in each format necessitated that the payment levels in the WTP format are three times lower than those in the WTA format[67]. While we are not able to directly compare the magnitude of the ES and CS, we compare the relative attractiveness of the DCE attributes between the two formats and identify the socio-demographic and attitudinal variables driving respondents' choices.

The questionnaire comprised three sections: 1) Socio-economic characteristics of the household including education, household features, land holdings and characteristics, other household assets, and wealth indicators (such as food security); 2) DCE survey; and 3) Six follow-up questions. The first four follow-up questions concerned the valuation scenarios and were measured on a five-point Likert scale while the last two were related to attitudes toward the policy at hand and were binary questions: i) Trust in the payment vehicle, ii) Plausibility of the survey scenario, iii) Perceived consequentiality of the DCE survey (i.e. how much respondents believe the results would be used to inform policy), iv) Perceived ability to negotiate compensations with the government, v) Perceptions of the benefits of forest protection, and vi) Belief in the legitimacy of forest conservation policy. While our split sample design might not allow us to isolate all the possible contributors to the WTA-WTP disparity mentioned in section 2, we explored these issues using the quantitative and qualitative debriefings.

[67] Bateman et al. (2002, p390) also commented that policy makers should use different WTA and WTP values because the WTA-WTP disparity reflects real and robust characteristics of people's actual preferences.

Table 5.1: Attributes and levels of the DCE (reference levels in bold)

Attributes	**Description**	**Levels**	**Coding**	**Hypotheses** (Expected sign of coefficients with WTA estimates in brackets)
WTA format: Total cash donations framed as development assistance (3080 MGA = 1 USD)	The cash donations were framed as development assistance that the household would receive. Piloting, review of secondary data and previous literature estimating the local costs of deforestation aided the selection of the payment levels (e.g. Shyamsundar and Kramer 1996; Ferraro 2002).	**0**, 3, 6, 9, 12, 15 (x10^6 MGA)	Continuous variable	More cash increases the average respondents' utility (+)
WTP format: Total cash payments made to the government	The cash payments would give individual households forest clearance permits. Payments would be made to the government and levels were determined by significant piloting, as well as the current land trading or leasing agreements in the study area.	**0,** 0.5, 1, 1.5, 2, 3 (x10^6 MGA)	Continuous variable	More cash decreases the average respondents' utility (-)
Number of annual instalments over which the household will receive / pay the total payments	The three levels of instalments allow an estimation of the respondents' discount rates and provides information on the respondents' ability to invest money.	**1**, 10,20	Effect-coded	Higher number of installments is expected to decrease the average respondents' utility (-) (in the WTA and increase utility in the WTP format)
Support for improved rice farming	This attribute is introduced as a sustainable and modern agricultural package that includes productivity enhancing practices such as the use of fertilisers, insecticides and/or herbicides. It involves digging and possibly the construction of terraces for slopes and precludes the use of fire as a way to maintain fertility while not fallowing the land. It also includes material support (e.g. improved seeds, wheelbarrow, spades, etc.).	**No support,** Support	Effect-coded	Improved agricultural practice may increase the average respondents' utility (+)
Teviala (clearance of new forestlands for agriculture)	This attribute has three levels: i) no *teviala* (i.e. strict enforcement of restrictions), ii) a permit for one hectare of *teviala* (a one-off opportunity), iii) free *teviala* (similar to pre-colonial times before criminalization of *teviala*, and de facto to more recent periods of little or no enforcement).	WTA format: **Free *teviala* ,** 1ha of *teviala* permit, and no *teviala* or strict protection WTP format: **No *teviala*,** 1ha of *teviala* permit, and Free *teviala* or open forest frontier	Effect-coded	Restrictions on *teviala* are expected to decrease the average respondents' utility (-)

5.3.3 Valuation scenarios

The background scenario of the WTA survey was presented to respondents as[68]:

"Please consider a major foreign donor who would like to provide you with some development assistance. The donor would like to provide you with support for improved rice cultivation technique. This project specifically targets rice cultivation on hills and its main objective is to maintain soil fertility. You would get technical support from the start till the end of the first year's production as well as start-up materials… Next consider that the donor lets you freely choose which development assistance you find best for your livelihoods. Therefore, the donor also offers to give you some cash payments that you can invest in any alternative income generating activities of your choice. Such cash payment would be managed by an independent external institution such as an access bank which will provide you with savings accounts…. Next, please consider that the government would make it possible for you to get a permit to clear forests on one hectare of forestland. This would be new additional land, still very fertile for which you have a legal and enforceable title (no risk of being penalized by the government) Please know that the forest clearance permit on one hectare is a one-off opportunity. Likewise, the rice cultivation technique would be a one-off project…. Next, please consider that the government would make it possible for you to get a permit to clear forests on an unlimited forestland (i.e. not limited to one hectare). This would be similar to President Ratsiraka's government (1975-1991) where teviala permits were formally granted to rural farmers. That would mean that the government would stop enforcing forest protection and you can think of it as an open forest frontier.

So if you were offered the choice below, which one would you choose? I.e. which one would be the best option for your livelihoods?

ALTERNATIVES	**A**	**B**	**C** (reference level)
Total cash donations (MGA)	3×10^6	6×10^6	NONE
Number of installments	**10**	**20**	-
Improved rice farming	**YES** ✓	**NO** X	**NO** X
Forest clearance option	YES BUT only on **1ha**	NO X	YES ✓
Choice			

Figure 5.1: Example of choice card in the WTA format

[68] Here translated in English from Malagasy

Thus, in the WTA format, the reference level alternative is an open access scenario, whereas in the WTP format, it is a strict protection scenario. The WTP scenario was presented as follows:

Please consider that you will be given the opportunity to invest in improved rice farming which can support your livelihoods. This project specifically targets rice cultivation on hills and its main objective is to maintain soil fertility. You would get technical support from the start till the end of the first year's production as well as start-up materials... Next, please consider that you would also be able to buy a permit to clear one hectare of forestlands. This would be like new additional land, still very fertile for which you have a legal title (no risk of being penalized by the government). Please know that the teviala permit on one hectare is a one-off opportunity, i.e. your household would be given the chance to buy it only once in your lifetime, likewise, the rice cultivation technique would be a one-off project. Note that you can pay only after harvest. But please we would like to kindly remind you to carefully consider whether you would be really able to afford the one you choose. Know that you would be paying the government through state agents, and the permit would be legal. Note that the Fokontany and independent stakeholders would also be involved to ensure transparency…. Next, please consider that the government would make it possible for you to buy a forest clearance permit on unlimited forestland (i.e. not limited to one hectare). This would be similar to President Ratsiraka's government (1975-1991) where teviala permits were formally granted to rural farmers. That would mean that the government would stop enforcing forest protection and you can think of it as an open forest frontier.

So if you were offered the choice below, which one would you choose? I.e. which one is the best option for your livelihoods?

ALTERNATIVES	**A**	**B**	**C** (reference level)
Total cash payments (MGA)	1×10^6	2×10^6	NONE
Number of installments	**10**	**20**	-
Improved rice farming	**YES** ✓	**NO** X	**NO** X
Forest clearance option	YES BUT only on **1ha**	YES ✓	NO X
Choice			

Figure 5.2: Example of choice card in the WTP format

5.4 Data analysis

5.4.1 Treatment of attitudinal data

Exploratory factor analysis is a multivariate statistical method used to understand the nature of constructs that underlie the data. It aims to generate factor scores that represent the underlying constructs by condensing a large number of variables into a smaller set of latent variables or factors (Thompson 2004). We conducted exploratory factor analyses separately for each WTA and WTP sample using the six DCE follow-up statements (cf. section 5.3.2). The factor analyses show that for both samples, only the first four statements (trust in the payment vehicle, plausibility of the survey scenario, perceived consequentiality of the survey, perceived ability to negotiate compensations with the government) loaded highly on the same factor. The one-dimensional factor solutions[69] explained 44% and 42% of the total variance in the WTA and WTP samples respectively. We therefore constructed an additive index "attitude scale" for each respondent in each sample based on the first four statements related to the valuation scenarios, ranging from 4 to 20 (using the five-point scales). The attitude scale had a high level of internal consistency, as determined by a Cronbach's alpha[70] of 0.70 and 0.71 for the WTA and WTP samples respectively. A smaller score on the attitude scale corresponds to more problematic attitudes towards the features of the survey scenarios. We used the attitude scale, perceptions of the benefits of forest protection, and belief in the legitimacy of state's protection in the discrete choice models to explain preference heterogeneity.

5.4.2 Analysis of the DCE data

The choice experiment method was initially developed by Louviere and Hensher (1982) and Louviere and Woodworth (1983) and is one of the four variants of stated preference choice modelling approaches (Bateman et al. 2002). It is theoretically based on Lancaster's model of consumer choice (Lancaster, 1966). Lancaster proposed that consumers derive satisfaction not from goods themselves but from the attributes they provide. The statistical analysis of choice experiments is based on random utility theory (McFadden 1983). According to this theory, the utility of a choice is comprised of a

[69] The factor analysis results suggest that the first four statements are measuring the same latent construct whereas perception of the benefits of forest protection and beliefs in the legitimacy of forest protection measure different constructs, perhaps relating more to the policy rather than the DCE survey.

[70] Cronbach's alpha can be used to measure the reliability of an attitudinal scale. It is used to determine how much the items on a scale (i.e. the four statements) are measuring the same underlying dimension. A higher value indicates a higher level of internal consistency. A 'reliable scale' — as a widely accepted rule of thumb — is indicated by an alpha value of at least 0.70 (DeVellis 2003).

deterministic component (*V*) and a stochastic component (*ε*), which is modelled to follow a predetermined distribution. According to this framework, the utility function of an individual *i* facing a choice between two experimentally created alternatives and a reference level alternative can be described as:

$$U_{ni} = \begin{cases} V(ASC, X_{ni}, \beta_k) + \varepsilon_{ni} \text{ if } i\text{=reference level alternative, otherwise,} \\ V(X_{ni}, \beta_k) + \varepsilon_{ni} \end{cases} \quad \textbf{(5.1)}$$

Where U_{ni} is the utility function for individual *i*, for alternative *n. V* is the observed indirect utility, which is a function of X_{ni}, a vector of observable attributes and associated fixed parameters β_k. We specify an alternative specific constant (ASC) for the reference level. We effect coded all the categorical coefficient utilities to avoid inherent problems associated with using dummy coding when including a fixed comparator in DCEs (Bech and Gyrd-Hansen 2005). The sign and significance of ASC cannot be interpreted when variables are dummy coded as the ASC coefficient may be associated with the utility of the base levels of the utility coefficients instead of representing the utility of the ASC per se.

The conditional logit model assumes the same preference structure across individuals, it may result in biased estimates and incorrect predictions if there is variability in the households' preferences. The random parameters logit (RPL) model or mixed logit model allows utility coefficients to be random variables to reflect preference heterogeneity in a population. In estimating RPL models, the analyst specifies a particular distribution of preferences prior to estimation, with normal, log normal and triangular distributions typically being used (Hensher et al. 2005). In our case all parameters were assumed to be normally[71] distributed across respondents. If there is statistically significant variation in preferences for a particular attribute, this shows up as a statistically significant parameter estimate for its standard deviation (representing the spread of preferences around the average respondent). The model presenting the best fit was selected as measured by improvements in McFadden's pseudo-R^2 and Akaike Information Criterion (AIC).

While the RPL model incorporates and accounts for heterogeneity mostly at the individual level, the latent class model (LCM) identifies the sources of heterogeneity at the segment level, i.e. it identifies

[71] The normal distribution is symmetrical around a mean and permits standard deviations that can result in a change in sign throughout the full range. Such distribution approximates best our "empirical truth" where respondents' preferences are highly heterogeneous and utility coefficients may be both positive and negative. The normal distribution is well documented whereas the uniform and triangular are less well-known.

a number of latent classes of respondents with distinct preferences for the policy alternatives being valued. The LCM thus typically assumes that preferences are uniform within groupings of individuals, but vary between these groupings. In many cases the sources of heterogeneity between segments relate to the characteristics of individual respondents such as socio-demographics as well as attitudes and perceptions (Boxall and Adamowicz 2002). We also estimated a latent class model for each format and found that a 2-class model fitted the data from the two formats best according to AIC statistics and our judgement regarding the interpretability of the model results.

We used the attitude scale (defined in 5.4.1) as a covariate explaining class membership of the LCM. The remaining two attitudinal statements were also added as covariates explaining class membership in the latent class model, these statements refer to respondents' belief in the legitimacy of state's protection, and local perceptions of benefits from forest protection. While attitudinal data may be endogenous to the choice data and not a genuine expression of fundamental attitudes (Provencher and Moore 2006), including them in the model allows a pragmatic check of which attitudes are associated with differences in the response patterns between responses to the WTA and WTP formats.

5.4.3 Analysis of the interview debriefing data

Each interview was professionally transcribed for the purpose of theoretical thematic analysis (Braun and Clarke 2006). In contrast to an inductive approach in which the identified themes are strongly linked to the data themselves, a theoretical thematic analysis is more explicitly analyst-driven, i.e. the coding maps onto the asymmetry between the WTA and WTP formats. We used a coding scheme intended to generate themes or general patterns which answer our research questions: *Which format is best for estimating the welfare impacts of conservation? What are respondents' attitudes to property rights over forestlands and conservation restrictions?* Codes and themes were constantly revised based on new insights from data analysis using Nvivo 10.

5.5 Results

5.5.1 Sample characteristics

Table 5.2 presents the socio-economic characteristics of the two randomly selected split samples. The two samples differ significantly only with respect to literacy rate and ethnicity at the 10% significance level. The average official years of schooling (2 – 2.5 years) are, however, not statistically different between the two samples. In the WTA sample, *Betsimisaraka*, which is the indigenous and dominant ethnicity in the study site, account for 79% of the total whereas this share is 66% in the WTP sample. Due to the different distributions of ethnicity in the two samples, the ethnicity variable is included in our discrete choice models to explain choice heterogeneity and to ascertain whether potential differences with regard to preferences in the two samples are caused merely by an overall impact of the differing valuation format, or if there is an ethnicity effect.

Table 5.2: Socio-economic characteristics

	Descriptive statistics of socio-economic variables		WTA sample (N = 102)		WTP sample (N = 104)		*t-value* [a] *χ2(1)* [b] *z-value* [c]	*p-value*
			Mean	*Std. Dev*	Mean	*Std. Dev*		
a	Household head's age (years)		38.87	*14.56*	37.36	*13.51*	*.519* [a]	*.60*
a	Household size		5.79	*2.42*	6.07	*2.71*	*-.790* [a]	*0.43*
a	Quantity of seeds used in the swidden agricultural plots (in *kapoaka* - as a proxy of land holding)		208.91	*198.27*	238.62	*201.0*	*-.960* [a]	*.513**
a	Total livestock owned by the household (in tropical livestock unit - Chilonda and Otte 2006)		0.76	*1.80*	0.95	*2.04*	*-.704* [a]	*.482*
c	Food security (number of months that the household has enough to eat)		5.87	*3.13*	6.15	*3.21*	*-.575* [c]	*.565*
a	Education: Years of official schooling of the household head		2.15	*2.78*	2.24	*2.53*	*-.269* [a]	*.78*
b	Literacy indicating whether the household head is literate	1=YES; 0=NO	0.51	*0.50*	0.64	*0.48*	*2.719* [b]	*.084**
b	Immigration status: Has the household moved to the village within the last 10 years?	1=YES; 0=NO	0.44	*0.49*	0.32	*0.47*	*2.609* [b]	*.102*
b	Ethnicity = *Betsimisaraka*	1=YES; 0=NO	0.79	*0.40*	0.66	*0.47*	*3.392* [b]	*.056**
b	Has the household received the World Bank safeguard development project?	1=YES; 0=NO	0.17	0.05	0.21	*0.41*	*.856* [b]	*.355*
c	Do respondents have experience of the improved rice cultivation? 3: "I have done it", 2: "I have seen it", 1: "I have heard about it", 0: "I have never seen nor heard about it"	Scale 0 to 3	0.75	0.92	0.78	0.88	*-.436* [c]	*.663*

(a) Mean comparison t-tests were applied to compare continuous variables between the two samples, (b) chi-square tests were used to compare the distributions of binary variables and (c) Mann-Whitney-U tests for ranked variables. Note: *, ** ,*➔ Significance at 1%, 5%, and 10% levels**

5.5.2 Difference in response patterns to the WTA and WTP format

We found that in both formats, the price coefficient had the expected sign, higher payments would significantly increase and decrease respondents' utility in the WTA and WTP formats respectively (table 5.3). We cannot compare the size of marginal WTA and WTP estimates because of different payment levels in the two formats and also possible differences in scale. In the following we only interpret their sign and significance. Respondents positively and significantly valued the rice project in the WTA format. They preferred one hectare of forest clearance permit to an open forest frontier and they preferred to receive payments spread over ten years (compared to a lump sum or 20 years). Conversely, in the WTP format respondents were indifferent to support for improved rice farming and between closed forest frontier and *teviala* permits (on one hectare or unlimited as in an open forest frontier scenario), and also indifferent between instalment options. The standard deviations of the cash as well as forest clearance attributes (one hectare of permit and open or closed forest frontier in the WTA and WTP respectively) were highly significant in both formats, this implies that there is significant heterogeneity in preferences for these attributes. The ASC representing strict forest protection is negative and highly significant in the WTP format, suggesting that moving away from the closed forest frontier scenario would increase the average households' utility. In the WTA sample, the ASC is negative and significant at 5 % level indicating that households preferred a change compared with the fixed alternative of open forest frontier, ceteris paribus.

Table 5.3: Random parameters logit model (RPL) results. Mean effects show the effects on utility for discrete changes in each attribute for the average respondent away from the same baselines of no cash donation, no improved rice project, and open forest frontier for the WTA format, and no cash payments, no improved rice project, and closed forest frontier for the WTP format. Standard deviation parameters show the spread in preferences around this mean effect for each attribute and level change. All parameters are set as random with a normal distribution. Note: *, **, * ==> Significance at 1%, 5%, 10% level.**

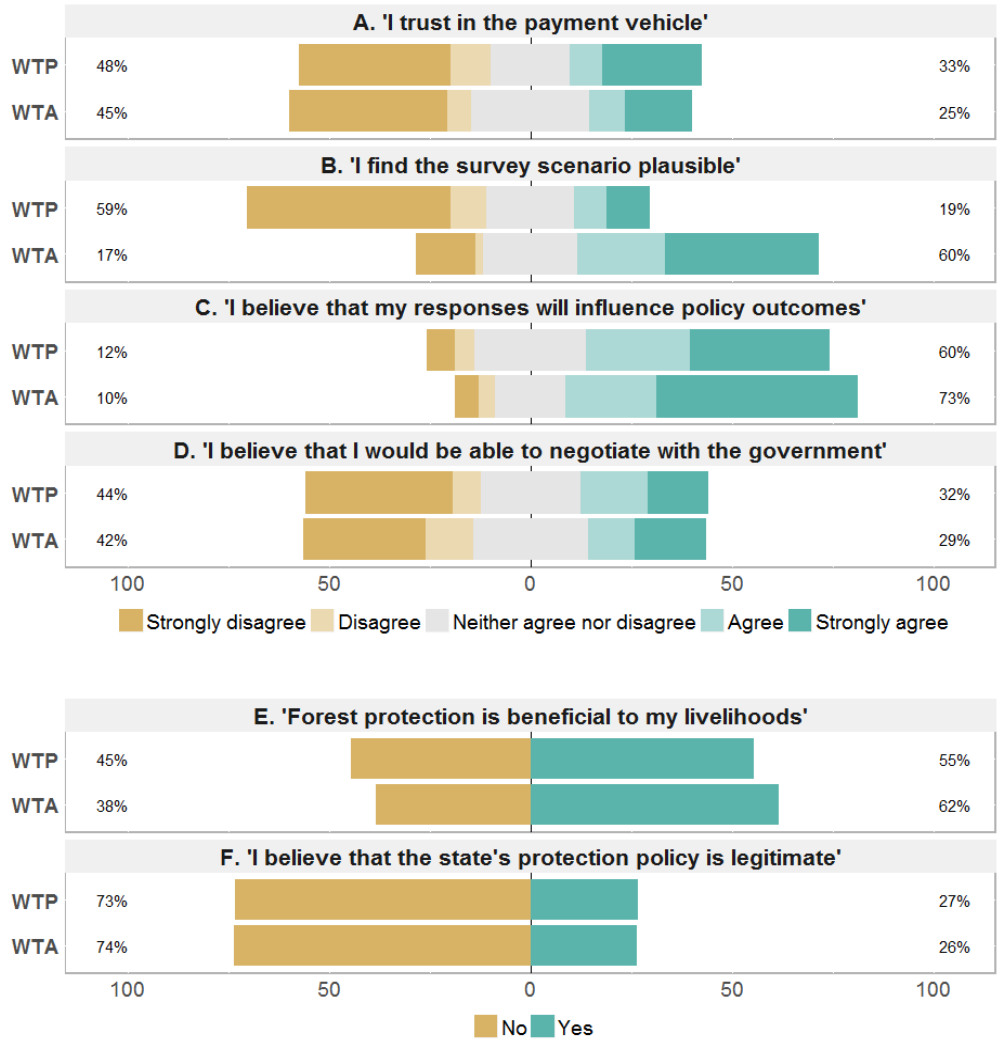
	WTA		WTP	
	Coefficient	**Standard error**	**Coefficient**	**Standard error**
	Random parameters			
Total cash donations (WTA) or payments (WTP)	.120***	0.041	-1.787***	0.308
Instalment = 10 years	.710**	0.330	0.467	0.258
Instalment = 20 years	0.314	0.316	-0.100	0.271
Improved rice farming	.854***	0.163	0.097	0.175
Permit 1ha	1.079**	0.431	-0.363	0.334
Closed forest frontier for WTA or open forest frontier for WTP	-0.460	0.582	-.957	0.559
ASC (reference level alternative)	-1.257**	0.604	-1.356***	0.452
	Standard deviation estimates			
Stdev Total cash donations	.167***	0.029	1.776***	0.254
Stdev Instalment = 10 years	.762*	0.462	0.144	0.773
Stdev Improved rice farming	1.329***	0.405	0.222	0.747
StDev Permit 1ha	.773***	0.177	.4775***	0.147
Stdev Closed forest frontier for WTA or open forest frontier for WTP	1.825***	0.342	.821**	0.428
Stdev ASC	0.193	0.555	1.677***	0.451
Log-likelihood	-462.8.46		-498.85	
McFadden's pseudo R2	.31		.25	
AIC/n	1.56		1.69	
Nobs	612 (N=102)		606 (N=101)	

5.5.3 Performance of the two formats on three criteria: Content validity, acceptability and budget constraints

5.5.3.1 Content validity of the two formats

Measures of the content validity of the two formats are presented in figure 5.3. The two split samples differed only in their beliefs towards the plausibility of the survey scenarios and their beliefs in the consequentiality of the survey, i.e. whether they view the survey outcomes as having real policy impact (Mann Whitney test, z-value=-6.57, p<0.005 and z-value=-1.95, p=.061 respectively). Half of the WTP sample strongly disbelieved that the state would be selling forest clearance permits (Likert value = 1) whereas only 15% of the total WTA sample did not believe in the cash donations scenario. The WTP sample, however, expressed slightly higher trust toward the payment vehicle (although not statistically significant), than its WTA counterpart: 33% of the WTP sample believed that payments to state's representatives would ensure secured ownerships over forestlands (Likert values= 4 and 5) whereas only 26% of the WTA sample had confidence in the transparency of the cash donations management. The share of respondents who believed in their ability to negotiate compensations with the government is similar between the two splits (19% and 20% for WTA and WTP respectively), and so is the percent of respondents who believed that state's protection is not legitimate (71 and 77% for WTA and WTP respectively).

To examine whether statistical differences in the plausibility of scenarios and perceived consequentiality of the survey between the WTA and WTP sample are associated with the socio-economic characteristics of the households (for example literacy or prior experience of external projects such as safeguard projects by the World Bank), we compared these statements for different sub-groups (i.e. by literacy level or experience) using Mann-Whitney U tests. We found that only prior experience of safeguard projects affect consequentiality beliefs; households who have received safeguard projects have significantly higher belief in the consequentiality of the WTP survey (median Likert value = 4) than those who have not (median Likert value = 5) (z-value=-2.49, p=0.013).



A. Trust in payment vehicle	WTA	"I trust that the independent institution will transparently and effectively manage the cash donation over time"
	WTP	"If the government sold me a permit, I trust that the government would honor that permit forever."
B. Plausibility of the scenarios	WTA	"A donor genuinely interested in development would donate cash"
	WTP	"The idea of the government selling me a permit to do *teviala* is plausible." (likelihood of the state selling permit in the valuation exercise)
C. Perceived consequentiality of the DCE survey		"I believe that my responses will influence policy outcomes"
D. Perceived ability to negotiate with the government		"I believe that I would be able to negotiate compensations or other requests with the government"
E. Perception of the benefits of forest protection		"Forest protection provides benefits which are important to my livelihood"
F. Belief in the legitimacy of state's conservation policy		In your opinion, which forest management policy is more legitimate: 1) You do not have the rights to forestlands and pay to be able to do *teviala*, that is state protection is legitimate) 2) You do have the rights and need to be paid not to do *teviala*, i.e. state's protection is not legitimate) (1= State's protection is legitimate; 0= State's protection is not legitimate)

Figure 5.3: Diverging stacked bar charts of the follow-up attitudinal data - Statements A. B, C, D are based on a five-point Likert scale: 1='strongly disagree', 2='disagree', 3= 'neither disagree nor agree', 4='agree', 5='strongly agree' Statement E and F are based on a binary question (1=Yes, 0=No).

5.5.3.2 Acceptability of the two formats and budget constraints

The acceptability of each format was measured by the rate of refusal to trade off due to a lack of compatibility between respondents' beliefs towards the policy being valued (belief in the legitimacy of state's conservation policy) and the given format. We specified a latent class model with two-segments for each format (table 5.5[72]), the characteristics of the latent class explanatory variables are summarised in table 5.4. We labelled segment 1 "traders" in both formats because the response patterns indicate that households traded off the payments with support for improved rice farming and/or forest clearance. WTA traders (86%) preferred secure rights over one hectare of *teviala* to an open forest frontier. They also positively and highly valued the support for improved rice farming. Traders in the WTP sample (47%) stated positive willingness to pay for the improved rice project and for forest clearance permits (both on one hectare and unrestricted clearance). They also strongly favoured longer timeframe (20 years) to one lump sum payment. Conversely, segment 2 respondents, labelled "non-traders", appeared to be unwilling to trade-off the payments with other attributes. WTA non-traders (14%) were negatively affected by partial or strict forest protection (one ha permit and closed forest frontier) but were indifferent to the payments while WTP non-traders (53%) were unwilling to pay for forest clearance permits[73]; the strict forest protection reference level (ASC) would appear to significantly increase their utility.

The WTA non-traders are more likely to have problematic attitudes towards the survey scenarios, and are less likely to believe that forest protection would provide benefits that would positively impact their livelihoods than WTA traders. Non-traders in the WTP format are mostly composed of households who strongly disbelieved in the legitimacy of forest protection, they are also more likely to be composed of households who indicated problematic attitudes towards the survey scenarios (as measured by the attitude scale), are less food secure and who belong to the *Betsimisaraka* ethnic group. Neither food security nor ethnic group however significantly explained class membership in the WTA sample.

[72] We also interacted the covariates in table 5.4 with the ASC of the RPL model for each format, the results (shown in Appendix 7) corroborate the LCM results in table 5.5.

[73] The sign of the one hectare permit and open forest frontier utility coefficients (negative and highly significant relative to the baseline of closed forest frontier) as well as the sign of the ASC for the non-traders' WTP segment (table 6) seem to suggest a positive WTP for forest protection. The qualitative evidence (section 5.4) however suggests that they are negatively affected by forest protection and their responses actually suggest a protest behaviour (i.e. rejection of the hypothetical scenario).

Table 5.4: Covariates used in the latent class models

Variables	Description		Summary statistics	
			WTA (N= 102)	WTP (N= 101)
Food security	Numeric variable indicating the number of months a household has sufficient food for two good meals per day.	Mean	5.8	6
		Std. dev	5	6
		Median	3.13	3.21
Ethnicity	Binary variable indicating whether the household head is *betsimisaraka* (the dominant and indigenous ethnic group in the study site) [0=NO; 1=YES]	YES	81 (79%)	67 (66%)
Attitude scale (from the factor analysis in 4.1)	Additive index measuring protest attitudes towards the survey scenario ranging from 4 to 20 (using the five point scales) (A smaller score on the attitude scale corresponds to more problematic attitudes towards the survey scenarios.	Mean	13.06	11.34
		Std. dev	3.48	3.55
		Median	13	12
Perceptions of the ecological benefits of forest protection	Binary variable indicating whether the household perceives any ecological benefits from forest protection [0=NO; 1=YES]	YES	63 (38%)	56 (55%)
		(missing)	7	4
Belief in the legitimacy of state's protection	Binary variable indicating whether the household believes that the state's protection is legitimate [0=NO; 1=YES]	YES	30 (29%)	23 (23%)
		(missing)	3	2

Table 5.5: Latent class models. Mean effects show the effects on utility for discrete changes in each attribute for the average respondent away from the same baselines in table 5.3

	WTA				WTP			
	SEGMENT 1: "Non-traders"		SEGMENT 2: "Traders"		SEGMENT 1: "Non-traders"		SEGMENT 2: "Traders"	
	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.
Total cash donations (WTA) or payments (WTP)	-0.063	0.166	.055**	0.027	-1.70***	0.352	-0.704***	0.175
Instalment = 10 years	1.279	1.684	0.140	0.202	-0.066	0.516	0.428	0.262
Instalment = 20 years	1.859	1.611	0.104	0.198	-0.811	0.412	0.556**	0.268
Improved rice farming	0.528*	0.440	.551***	0.092	-0.406	0.276	.457*	0.379
Permit 1ha	-2.19*	0.987	1.182***	0.281	-1.50***	0.485	0.819**	0.408
Closed forest frontier for WTA or open forest frontier for WTP	-2.45**	0.991	0.390	0.465	-2.54***	0.766	1.294**	0.560
ASC (reference level alternative)	1.278	0.972	-1.031***	0.350	-1.68***	0.624	-0.651	0.408
Segment size (%)	14%		86%		53%		47%	
	Explanatory variables of class probability							
	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.	Coef.	s.e.
Constant	0.848	2.025	Fixed		3.66***	1.390	Fixed	
Food security	-0.118	0.327	Fixed		-0.222*	0.088	Fixed	
Betsimisaraka	0.106	0.909	Fixed		1.05*	0.564	Fixed	
Attitude scale	-0.173*	0.090	Fixed		-0.198*	0.084	Fixed	
Perception of ecological services	-1.470**	0.686	Fixed		-0.180	0.552	Fixed	
Belief in the legitimacy of state's conservation policy	-0.135	0.784	Fixed		-2.02***	0.683	Fixed	
Log-likelihood	-451.00				-489.98			
McFadden's pseudo R^2	.33				.26			
AIC/n	1.53				1.68			
Obs.	612				606			

Note: ***, **, * ➔ Significance at 1%, 5%, 10% level.

5.5.4 Discrepancies and similarities in WTA and WTP accounts and respondents' attitudes towards conservation policy: Insights from the qualitative debriefings

We assigned each interviewee to either the trader or non-trader segments of each survey format based on the highest ex-post individual class membership probability[74] from the LCM results (table 5.5). We first contrast non-traders' accounts in the WTA format with those in the WTP format and then highlight the divergence and similarities between the WTA traders' rationale and that of the WTP traders. While highlighting below the similarities and differences between and within the WTA and WTP format, we also underline respondents' attitudes to conservation policy. The interviewees' characteristics are summarised in table 5.6.

[74] We used the estimated probability of household *n* falling into the taker or protester segments from the LCM results to assign households to either segment.

Table 5.6: Interviewees' socio-economic and attitudinal characteristics

ID	Format	Segment	Betsimis araka	Food security (months)	Literate	Household head age	Trust in payment vehicle	Plausibility of the scenarios	Consequentiality of the DCE survey	Ability to negotiate compensations with the government	Perception of the benefits of forest protection	Belief in the legitimacy of forest conservation policy
							1='strongly disagree', 2='disagree', 3= 'neither disagree nor agree', 4='agree', 5='strongly agree'					
I1	WTA	Trader	Yes	12	No	21	3	3	3	2	1	1
I2	WTA	Non-trader	Yes	7	No	40	1	1	1	1	0	0
I3	WTA	Trader	No	8	Yes	45	3	5	4	1	1	0
I4	WTA	Trader	Yes	12	Yes	46	2	4	5	3	1	0
I5	WTA	Trader	No	10	No	38	3	1	4	1	1	0
I6	WTA	Non-trader	Yes	4	No	21	4	4	4	3	0	0
I7	WTA	Trader	Yes	6	No	20	3	4	4	2	1	0
I8	WTA	Trader	No	8	Yes	45	5	4	5	1	1	0
I9	WTA	Trader	Yes	10	Yes	37	3	4	3	3	1	0
I10	WTA	Trader	Yes	5	Yes	30	1	3	1	2	1	1
I11	WTA	Trader	Yes	6	Yes	60	3	3	3	3	1	0
I12	WTP	Trader	No	12	Yes	32	1	3	2	3	1	0
I13	WTP	Trader	No	7	Yes	27	4	3	4	3	1	0
I14	WTP	Non-trader	Yes	5	Yes	51	4	2	5	4	1	0
I15	WTP	Non-trader	No	5	Yes	23	2	1	3	1	1	0
I16	WTP	Non-trader	No	3	Yes	52	1	1	5	2	1	0
I17	WTP	Trader	Yes	9	Yes	38	4	5	3	4	0	0
I18	WTP	Trader	Yes	12	No	74	2	3	4	3	1	0
I19	WTP	Trader	Yes	7	Yes	52	2	3	1	1	0	1
I20	WTP	Non-trader	Yes	3	Yes	52	1	1	5	1	1	0

5.5.4.1 WTA and WTP non-traders: Forest clearance as a necessity amidst a declining standard of living

WTA and WTP non-traders share some common rationales for their responses; they both experienced hardship from strict forest protection. Even if WTP non-traders were not willing to pay for forest clearance permits, they claimed that the enforcement of strict forest protection, which would be materialised on the ground in the presence of armed law enforcement, would make their living much more precarious than the current enforcement levels. Non-traders in both formats also argued that they cared about forest protection but could not afford it. They both viewed forest clearance as a necessity amidst a declining standard of living and the ongoing influx of migrant smallholders.

However WTA and WTP non-traders' account differed on some areas. WTA non-traders (I2 and I6) claimed that the revenue they would get from clearing forests far outweighed the compensation levels. They asserted that cash is fleeting and *teviala* is much more sustainable. Unexpectedly, their accounts did not seem to allude to any objections to the survey scenarios, particularly, trust in the cash donations or plausibility of the scenarios, yet, the LCM suggest this group are more likely to have problematic attitudes towards these survey scenarios. Their preferences were instead anchored in the critical importance of new lands to their current households' livelihoods and their future descendants. They however expressed a lack of ability to negotiate compensations with the government.

> We don't really have the choice, do we? We've never had the choice, so whatever the government decides, we will have to go with it, even if the government gives as little as 600 Ariary, we have no say, anyway, the government won't listen to us locals hidden below the leaves." (I2, WTA)

On the other hand, interviewees among the WTP non-traders (I14, I15, I16, I20) were strongly averse to paying for forest clearance rights, which they asserted as already theirs. They strongly objected to the state's protection and claimed that they must not pay for something they have been protecting for years from recent settlers. They appear determined to assert their rights over forestlands. As I15 (WTP) stressed:

> "The valuation exercise was very disturbing, because if I pay for something, that implies that I don't own that thing yet, I cannot purchase what's already mine. Asking me to pay is so illegitimate because I have protected these forest patches and my efforts involved lots of sacrifice."

In addition to strong beliefs about their rights to forestlands, these WTP non-traders were also averse to paying for forest clearance which they saw as their subsistence livelihood. They claimed that asking local forest dwellers to pay for a precarious living as *teviala* is highly nonsensical and unrealistic because it ignores the very reasons for its practice (i.e. their poverty-stricken status). Instead of paying, they argued that they should be provided with some livelihood support. WTP non-traders also claimed

that the sale of forest clearance rights would be too prone to elite capture as those with higher purchasing power (mostly the non-*betsimisaraka* migrants who have other non-agricultural sources of income) would monopolise the market.

Only one interviewee among the WTP non-traders mentioned that their ability to pay was constrained by their income and risks. They also asserted that if they had the means to pay for forest clearance or for the improved rice farming, they would rather invest money in buying fallow lands or additional labour.

> "You know that there are some good years and some bad years, so if ever we are unable to pay, the government will withdraw the permit and we will be left with nothing. Or could it be that the government will be more indulgent to such cases? I don't think so, an agreement is an agreement….The support for the improved rice project is particularly very risky, we cannot simply risk starving for one whole year because we were too busy digging soils which will only yield meagre crops". (I16, WTP).

Some respondents were hoping for reduced costs given the number of households who would be willing to pay and the likely benefits that will accrue to the government.

> "There will be so many people who would be interested in the forest clearance permits, so imagine, if there are just 20 people who are willing to pay 20,000 Ariary that would already amount to a fair sum of money. So that implies that we should only pay the minimum, even nothing because we are the ones who need help, not the government." (I14, WTP)

A WTP non-trader expressed very low beliefs in the survey scenarios, particularly, the likelihood of a forest clearance permit, given that forest protection seems to have nowadays a burgeoning interest among conservationists and the international community.

> "The forest frontier is closed and there will never be such things that it will be open for people to farm. So let it be as it used to be always and instead help us find other income generating activities." (I20, WTP)

5.5.4.2 WTA and WTP traders: A strong demand for secure forest tenure

The interviewed WTA and WTP traders strongly aspire to secure legal tenure over forestlands. Both groups expressed that strict forest protection would result in severe hardship among local forest dwellers. They also expressed a strong aversion to state protection claiming that the state is unable to enforce protection and they are too vulnerable to the state representatives' manipulation.

> "I cannot imagine what would happen if this military protection becomes a reality. You surely know how gendarmes work, they will just impose whatever they want on us, and who are we to discuss or fight with them? They will always win, and they will restrict everything, they won't even allow us to take firewood." (I19, WTP)

> "The first thing that came to my mind was: will the state be able to protect these forests, with all its problems and its instability? You cannot rely on the state to do anything. Since I lived here 25 years ago, our request to get a government-hired teacher has remained vain, our children cannot to go to school because parents cannot afford teachers' fees." (I8, WTA)

Traders in both formats seem to care about forest protection and claimed that they want to "breed" their forests. Nonetheless, they aspired to have the freedom to choose forests' fate.

> "If only people have legitimate rights to own forest patches and protect them, life will be so much easier and conflicts with recent migrants will be reduced….But you can never predict what others think, I do intend to breed mine, but others may decide differently depending on their circumstances, as the saying goes: even if yams grow on the same valley and use the same nutrients, there will always be ugly yams." I10, WTA

However, while WTA traders (I1, I2-I5, I7-I11) only aspired to legal tenure on one hectare of forest clearance and feared a tragedy of the commons situation in an open access scenario, WTP traders were willing to secure rights not only for one hectare permit but also for an unrestricted access to forestlands; the act of paying made them assume that they were entitled to have more rights. One hectare of permit would allow WTA respondents to exclude others and assert their rights over forestlands as opposed to a simple customary ownership which are often disputed by recent settlers. WTP takers (I12-I13, I17-I19) were willing to pay for forest clearance permits to leave a legacy of natural forestlands with their future descendants, they however begrudged having to pay for these rights.

> "There is simply no way that we agree to relinquish our rights to these forestlands, it is out of the question. But if we really have to pay for our descendants, then we will pay, although we strongly feel that we should not have to pay because we protected these forests." (I19, WTP)

WTA traders were very receptive to the support for improved rice project and stated that they would invest the cash mostly in the improved agricultural techniques. While WTP traders were also willing to pay for the rice project, they seemed to face considerable budget constraints.

> "Since forest clearance will be strongly prohibited, we will have to start finding other alternatives. There is no other way round using fertilisers and using improved techniques but we could not afford the payments." (I13, WTP)

5.6 Discussion

5.6.1 How do the patterns of responses differ between the WTA and WTP formats?

We found that the utility coefficients that are statistically significant differ between the WTA and WTP formats. The WTA respondents strongly favoured support for the improved rice project and secure tenure for one hectare of forestlands relative to no support and open forest frontier respectively, whereas the WTP respondents expressed no significant preferences for either the improved rice farming or *teviala* permits. Also, WTP respondents had surprisingly no preference for delaying payments whereas WTA respondents significantly preferred that the payments (cash donations) were spread over 10 years instead of a lump sum payment, due to a limited ability to invest cash for the future (Rakotonarivo 2016). While the WTP and WTA formats have been shown to affect welfare estimates (e.g. Bateman et al. 2009; Lanz et al. 2010), this study has provided evidence that the valuation format can also affect the response patterns, i.e. the relative importance of different attributes.

It is possible that the different response patterns observed between the WTP and WTP formats could have been reduced with lower payment levels in the WTP format. Nonetheless, the disparity may remain because of respondents' strong disbeliefs in the legitimacy of state's protection (as suggested by both the quantitative and qualitative debriefings). We also made significant efforts when developing the valuation scenarios to ensure that the rates of refusals to trade-off between the two formats are not an artefact of respondents' budget constraints. We instructed respondents in the WTP format that they could pay after harvest time (in cash or in baskets of rice) if they run short of cash. We also used WTP bids that are at least three times smaller than the WTA bids.

5.6.2 Which format is best for estimating the welfare impacts of conservation?

This study also aimed to assess the performance of the WTA and WTP formats in our study context on three criteria: respondents' perceptions of the survey itself; whether respondents were unwilling to trade off different attributes due to moral beliefs; and the effect of budget constraints. We found that the WTA format outperformed the WTP format on all three criteria. The WTA format elicited fewer problematic perceptions than the WTP. Only 15% of the total WTA sample did not find it plausible that a donor genuinely interested in development would donate cash whereas 50% of the WTP sample strongly disbelieved that the state would be selling forest clearance permits (figure 1). Similarly, 73% in the WTA sample viewed the survey outcomes as having real policy impact against 60% of the WTP sample.

The WTP format resulted in higher rates of refusals to trade-off forest clearance permits with payments (53% against 14% in the WTA format) and this did not seem to be explicable simply by the payment levels: respondents' disagreement with the legitimacy of forest protection was highly significant in explaining refusals to trade-off in the WTP survey (table 4) but not in the WTA survey. This is corroborated by the qualitative debriefings which suggest that respondents considerably begrudged paying for forest clearance, because such payments would ignore their rights and past efforts to conserve the forest.

Finally, the qualitative debriefings support the argument that the WTP format is problematic in our study context because respondents' ability to pay is severely constrained. Forest protection results in large negative welfare impacts; *teviala* provides barely enough for subsistence living and its substitutability with money is critically low. The qualitative findings suggest that although respondents highly value forest conversion to agricultural lands, *teviala* may not produce much surplus, but has a high labour efficiency which cannot be easily monetised, that is, it produces agricultural crops with minimal drudgery compared to improved agricultural techniques (Pollini 2009; Scales 2014). Swidden agriculture has also many advantages that are not easily substitutable by other alternatives (such as irrigated paddy fields) (*ibid*). In effect, swidden agriculture allows households to minimize climatic risks (e.g. flooding or cyclones) associated with lowland agriculture while paddy fields require significant inputs of labour or capital (Pollini 2012). Given the very slow rate of technological change (agricultural intensification) it is likely that swidden agricultural practices will remain widespread in the coming years as long as convertible forestlands are available.

Most stated preference surveys ask respondents their willingness to pay for a policy change, these are appropriate when respondents do not perceive any property rights over the good being valued, or when the value of the policy is likely to be small relative to their income (Freeman 2003). However, our results suggest that even where de jure forest ownership rests with the government, suggesting that WTP should be estimated (Mitchell and Carson 1989), respondents may hold strong protest beliefs that conflict with the WTP format and researchers should thus also consider estimating WTA. In the literature, other arguments against the use of the WTA format include the possibility of strategic behaviour and extremely high WTA estimates that are inconsistent with neoclassical preferences (e.g. The NOAA panel - see Arrow et al. 1993). However, the qualitative debriefings do not suggest any evidence of strategic considerations. Our study therefore suggests that the WTA format may outperform the WTP format in a rural developing country context, which emphasises the importance of at least considering both formats. Kim et al. (2015b) argued that where the WTA-WTP disparity genuinely reflects respondents' underlying preferences, the choice of the correct welfare measure

should be based on the explanations for the WTA-WTP disparity in addition to property rights structure and the nature of the change from the 'status quo' or business as usual position. That is, if a researcher can identify the likely sources of the disparity arising in a given situation (e.g. lack of substitutes, bounded rationality, reference dependence, value learning or protest responses), they can make an informed decision about the appropriate measure to use.

5.6.3 What are respondents' attitudes to conservation restrictions and property rights over forestlands?

Finally, this study aimed to investigate respondents' attitudes to conservation restrictions and property rights over forestlands. 73% objected to state protection, arguing that it leads to a tragedy of the commons, and that they have been protecting forests by restricting, if not completely stopping, forest clearance. Most WTA traders (86% of the total sample) shared the WTP traders' strongest aspiration, which is to secure their customary rights over forestlands. Since strict protection has only been recently enforced in the Ankeniheny-Zahamena corridor and forest clearance used to be the legitimate way to claim new resources and territory (Muttenzer 2006), the strong loss aversion exhibited by the WTA and WTP traders towards forestlands ownerships may not be unexpected. These results do not support other scholars' interpretations that the WTA and WTP formats both accentuate feelings of loss aversion, but in different dimensions (Bateman et al. 2002). That is, that by explicitly asking respondents to think in terms of paying money, WTP prompts loss aversion behaviour in the dimension of money whereas WTA prompts thoughts related to loss aversion in the dimension of the good being valued. We found that both WTA and WTP traders are loss averse with regard to the same dimension, the good being valued, i.e. their rights to forestlands. The WTP households' responses primarily reflected their beliefs about the legitimacy of the state's conservation policy.

The results indicate that the current model of coercive conservation (that is REDD+ building upon protected area regimes in which clearing is strictly prohibited and forestlands are state-owned assets) and the provision of compensations for the costs of restrictions may not be viable. Since REDD+ is involuntary for most local people, coercive conservation lacks procedural legitimacy and may not achieve full compensation. If local people perceive the state's protection policy as illegitimate or unjust, they may resist conservation actions and engage in environmentally harmful behaviours (Holmes 2003; Milner-Gulland and Rowcliffe 2007) or they may incur loses that would not be mitigated by most common compensatory schemes (e.g. cash or in kinds). An explicit recognition of customary rights may be more effective at slowing down deforestation than the current coercive conservation model embedded in REDD+ policy. This recognition may be materialised by establishing secure

forestland tenure and enabling owners to exclude migrants and outsiders. As there is currently an increasing trend to link carbon rights with liability and tenure rights (Loft et al. 2015), our study highlights the importance of viable property rights arrangements over forestlands in REDD+ implementation (Dokken et al. 2014; Sunderlin et al. 2014). As local communities may not be able to afford forest conservation (Godoy et al. 2000), conservation may then be negotiated with landowners, such as the model embedded in conservation contracts or agreements in many industrialised countries (Guignet and Alberini 2015; Liebe et al. 2015). When property rights are explicit and locally perceived as legitimate, contract negotiations would provide room for local people to claim incentives (cash or in-kind) for conservation efforts.

5.7 Conclusions

The patterns of responses to the WTA and WTP formats significantly differ. The WTA format is more suitable in our study context because it was perceived to be more plausible and consequential, it minimises the rates of refusal to trade off because of ethical beliefs, and it is not disadvantaged by severe budget constraints. Most respondents strongly aspired to secure tenure and argued that they have better capabilities to protect forests than the government. Respondents in both WTA and WTP formats were very reluctant to relinquish their rights over forestlands and more than 70% of respondents in both formats perceived the state's conservation policy as illegitimate.

Researchers using DCE in similar contexts should not simply use de jure property rights to determine which format to use. The choice of format may substantially affect welfare estimates, which attributes are significant, and the content validity and willingness to trade off in the survey. An inappropriate valuation format may seriously compromise efforts to determine appropriate compensation levels for coercive conservation. Conservationists and REDD+ proponents should reconsider coercive models of conservation (even with compensation) where these align very poorly with local people's beliefs about customary rights. Otherwise they risk alienating local people, thus undermining a very significant driver of conservation: local people with secure property rights to the forest.

Chapter 6. DISCUSSION

This thesis comprises four standalone but interdependent chapters. I first used a rigorous and systematic approach to identifying and synthesising the empirical evidence on the reliability and validity of DCEs (Chapter **2**). Building on the results of the systematic review, I used primary data from two study sites, a national park established for 20 years and a new REDD+ pilot project site, both in the eastern region of Madagascar. The field-based empirical chapters **3, 4, 5** used both carefully-designed quantitative analysis and rigorous qualitative debriefing approaches, and make an important contribution to the advancement of DCEs for use in LIC context. However they also have strong practical relevance for those designing development interventions or seeking to compensate the costs of conservation restrictions.

In this chapter, I first summarise the main research findings and contributions, and discuss the commonalities across the main thesis chapters. I then highlight the limitations and strengths of my research methods and suggest potential areas for future research. Next I discuss the policy implications of my thesis in the wider context of conservation and development in LICs. I finally propose some practical recommendations for designing and conducting DCE in a LIC setting which may be relevant to both DCE practitioners and policy makers.

6.1 Chapter summaries, contributions, and commonalities

6.1.1 Chapter 2: A systematic review of the empirical evidence on the reliability and validity of discrete choice experiments in valuing non-market environmental goods

Chapter 2 systematically reviewed empirical evidence from studies that have incorporated tests of the reliability or validity of the DCE method. The review was limited to environmental valuation applications. I first developed a conceptual framework of how reliability and validity could be assessed in DCE. I then used a reproducible and transparent search protocol and reviewed the past 13 years' literature (January 2003- February 2016) using two databases; the ISI Web of Science and Econlit.

Only 107 met the inclusion criteria at a later stage of full text assessment, of which 12 articles were conducted in LICs. The results show that the evidence on reliability is mixed: in 45% of the outcomes small changes to the survey design yielded different results. Since DCE researchers may not be able to assess, a priori, how different information or designs will impact choices, tests of reliability should be incorporated into DCEs whenever resources allow. Evidence of the lack of criterion validity (none of the ten DCE outcomes matched those of non-hypothetical DCEs) is also a concern. However,

measuring hypothetical bias may be of limited value for many non-market environmental services which have no suitable criterion. I recommend that whenever the criterion itself is reasonably valid and feasible, DCE researchers should strive to measure hypothetical bias and investigate its sources. Otherwise, methods should be developed to elicit value components that real markets and real DCE may not reveal, for example through deliberative approaches or mixed methods. DCE outcomes are consistent with other SP based methods (mostly CVM) that share the same underlying theory. As I only found two studies which compared DCE results with revealed preferences (Scarpa et al. 2003; Abildtrup et al. 2015), such comparisons (e.g. with hedonic pricing. travel costs, production functions, etc.) represent key research avenues in the assessment of the external validity of DCEs. Finally, evidence on theoretical and content validity suggests that an important proportion of respondents' choices were inconsistent with the utility axioms assumed by DCEs. Ultimately, if DCEs are to be useful to policy, and if a lack of theoretical and content validity is a major concern, DCE researchers should strive to understand and examine the factors or processes explaining violations of rational choice theory or lack of content validity and how they relate to respondents' characteristics.

To my knowledge, this is the first systematic review of the empirical evidence on the reliability or validity of the DCE method when valuing non-market environmental services. Systematic reviews for methodological questions in environmental economics are relatively novel (I am not aware of any). This review can enhance awareness of how much evidence is available in the DCE field particularly for those who might commission, conduct or rely upon the results of DCE studies in applied environmental settings. Key knowledge gaps identified by the SR were the critical need to use more qualitative, and interdisciplinary approaches in the assessment of the reliability and validity of DCEs. I only found two studies (Powe et al. 2005; Arana and Leon 2009) which used qualitative approaches (focus group debriefings and verbal protocol) to assess the theoretical ad content validity of DCE. Qualitative debriefings provide a better understanding of the meaning of the values elicited, the complexity of issues and difficulties within DCE methods which would be difficult to achieve using purely quantitative methods. My empirical chapters (**3, 4, 5**) have all used qualitative methods to improve our understanding of key aspects of DCE reliability and validity. The SR results also show that SP for environmental services are often dependent on the elicitation context. This may have implications for the use of DCE in policy analysis in terms of identifying how environmental policy is implemented and financed. I addressed this important issue by surveying two samples that differ in their experience of forest conservation in **chapter 3**. One important suggestion that also stems from the SR is the need to use more deliberative approaches to address respondents' lack of pre-defined or well-informed preferences for environmental services or policies; I took this recommendation on board in **chapter**

4. Although comparison of WTA and WTP estimates was not included in the SR, the often observed empirical divergence between WTA and WTP measures of welfare change constitutes an important validity issue and continues to be a topic of interest in environmental economics. In **chapter 5**, I examine the disparity in the qualitative patterns of responses to the WTA and WTP format.

6.1.2 Chapter 3: Qualitative and quantitative evidence on the true local welfare costs of forest conservation in Madagascar: Are discrete choice experiments a valid ex-ante tool?

Chapter 3 is my first field-based empirical chapter, its rationale mostly stems from the significant effects of elicitation context on DCE results as I identified in **chapter 2**. **Chapter 3** aimed to assess the validity of ex-ante DCE in predicting the local welfare costs of conservation, and hence in informing the design of compensation policies. Validity is conceptualised as the degree to which the DCE method measures what it is intended to measure; in this case, the level and type of compensation required to maintain respondents' welfare levels despite forest conservation policies that would otherwise make them worse-off. I sampled households from two sites in the eastern rainforests of Madagascar, Ampahitra and Mantadia, which differ in their exposure to forest protection but are otherwise similar in terms of forest characteristics, market access and remoteness. **Chapter 3** also aimed to examine the theoretical and content validity of the DCE results by assessing how well they conform to the continuity axiom (which assumes that DCE respondents need to attend to, and explicitly trade-off, the attribute levels across each of the alternatives), and the extent to which respondents object to some features of the survey scenarios, and believe in the consequentiality of the DCE survey. I found that greater household experience with forest use restrictions was associated with larger welfare impacts of future restrictions. I also found that despite apparent evidence of non-attendance to some attributes from the modelling results, qualitative debriefings suggested that respondents instead expected relatively low or no utility from those attributes and hence had theoretically valid preferences. I believe that this is the only study in a low-income context to enrich a DCE survey with rigorous qualitative debriefing data to assess the validity of DCE results. The significant effect of prior experience of conservation indicates that it is hard to accurately estimate in advance the costs of an intervention which may affect people negatively. These results suggest that caution is needed in using DCE as a means of estimating compensations for long term and complex projects such as forest conservation. I argue that conservation practitioners and policy makers may need to question the viability of the current coercive conservation model which urgently seeks to establish protected areas, with no, or insufficient effort to assess the likely impacts of conservation actions on local people.

6.1.3 Chapter 4: Assessing the welfare impacts of forest conservation in Madagascar: Does more time to deliberate affect respondents' behaviour in a discrete choice experiment?

Chapter 4 examined the effect of more opportunity to deliberate (24 hours) on respondents' behaviour in the same DCE survey assessing the local welfare impacts of forest conservation. It builds on **chapter 2**'s recommendations on the importance of deliberation and debriefing approaches in the examination of the reliability and validity of DCE. Most DCE surveys (and household surveys in general) ask respondents to give immediate responses. However, respondents may lack the knowledge to comprehend the importance of environmental policies for their welfare and therefore may not have meaningful preferences that can be elicited within the time scale of the survey. Lack of prior or well-informed preferences is likely to present an even bigger challenge in least developed countries where literacy rates are low. I used data from Mantadia National Park where local households have had at least 20 years of prior experience with strict use restrictions. I used a test-retest design, i.e. I administered the same DCE instruments twice to the same households (N=104) one day apart. I also conducted debriefing interviews with a sub sample of respondents after the repeated DCE survey to explore the mechanisms by which more time to deliberate shapes individual values and hence influences preferences (e.g. potential for strategic answers or recall). I used hierarchical Bayes estimation which greatly simplifies the estimation and inference compared to classical procedures (maximum simulated likelihood) and gives more accurate estimates of individual-level utility parameters for small sample size. I found that more time to deliberate or discuss with others did not affect responses at the choice task level or individual utility parameters for the aggregate sample. However, literacy significantly influenced the effect of more time to deliberate; literate respondents and those who discussed with others outside the household required significantly smaller compensations (in terms of marginal willingness-to-accept estimates) for strict forest protection on day 2 whereas WTA estimates were significantly higher for illiterate households when given overnight to deliberate.

Chapter 4 suggests that more time to deliberate may give DCE respondents with low education levels more opportunities to reflect on the survey questions. However the overall effect may not be significant and deliberation might encourage strategic behaviour by other respondents. However, the finding of no aggregate effect should not discourage testing the effect of more time to deliberate for unfamiliar goods or complex environmental interventions. Further testing of more time to deliberate in different contexts and income settings would help build a wider evidence base on the effect of deliberative approaches. The effect of more time to deliberate is also likely to depend on a range of factors such as the prior experience of the respondents, and other attitudinal factors. It would be

interesting to test whether more time to deliberate could have lessened the effect of prior experience with the policy being valued (**chapter 3**). Also, deliberation in the sense of social learning (Reed et al. 2010), i.e. getting inexperienced and experienced people together, would be interesting. Similarly, as differences in the response patterns between the WTA and WTP formats investigated in **chapter 5** may be caused by the lack of information and respondents' uncertainty[75] about their true values, giving more time to deliberate to respondents may help respondents carefully reflect on the characteristics and implications of the good being valued which would therefore be expected to reduce the disparity between the WTA and WTP responses.

6.1.4 Chapter 5: Willingness-to-pay for use rights and willingness-to-accept compensations for foregoing use rights: Property rights issues in forest conservation Madagascar

While **Chapter 3** suggested that DCE is not a valid ex-ante tool for estimating compensations because people who lack experience of restrictions may be unable to estimate the actual welfare impacts of forest conservation, I found good evidence that the DCE survey had high theoretical and content validity and could successfully elicit current preferences. Likewise, the low rate of changes in respondents' choices shown in **chapter 4** also corroborates the suitability of my DCE design in eliciting current preferences. **Chapter 5**, I used a case study of a recent REDD+ project in eastern Madagascar where households have limited experience of forest conservation[76]. **Chapter 5** was motivated by the ambiguous and contested property rights over forest resources in developing countries. Natural forests are state-owned yet local people perceive customary rights over them. Past research in other contexts shows that the choice between the two formats significantly affects valuation results and has important policy implications. I used a split sample design and elicited each measure from separate but analogous DCE survey scenarios valuing local people's WTP for forest clearance permits and their WTA compensations for forest clearance restrictions. I found that the valuation format can also affect the response patterns, i.e. the relative importance of different attributes. Respondents' disbelief in the legitimacy of state's conservation policy strongly conflicted with the WTP format. Using the latter is therefore arguably inappropriate even if respondents do not hold formal rights over forestlands. The sampled households strongly aspired to secure tenure and believed they were in a better position

[75] An average individual who is uncertain about his true preferences for an environmental change would tend to state a WTA that is greater than his true WTA, or a WTP that is less than his true WTP. Thus, on average, the difference between stated WTA and WTP would be greater than the true difference (Hoehn and Randall 1987).

[76] Investing the WTA-WTP disparity in Mantadia where local people are more experienced in conservation restrictions would also be interesting. Experiences of the policy being has been found to decrease the observed divergence between WTA and WTP (List 2004).

to protect forests than the state or its representatives. Such findings may imply that an explicit recognition of forest tenure may be more effective at slowing down deforestation. **Chapter 5** also cautions against the often-misconceived and imposed framings of justice and legitimacy in conservation approaches in the tropics (Martin et al. 2014). **Chapter 5** reinforces **chapter 3**'s main policy implications on the importance of securing forestland tenures and negotiating conservation through PES schemes where local people's participation is genuinely voluntary, property rights are explicit, and where forest conservation policies are locally perceived as legitimate.

6.2 Limitations and strengths of my research design and methods

Validity in socio-economic research is a multidimensional ideal (Roe and Just 2009). Approaches to designing social surveys often require a trade-off between internal and external validity (*ibid*). Internal validity is concerned with whether the observed correlations are causal while external validity can be defined as the ability of a researcher to generalise the results of a given study to other similar settings (Campbell 1957). This trade-off is central to the design of **Chapter 3** which examined the effect of experience of conservation using a natural experiment, i.e. comparing a group of respondents which have been exposed to the treatment with other similar respondents who have not. As I was not able to randomly assign the treatment (experience of conservation) to participating subjects, I am not able to isolate causal effect nor can I even attempt to match respondents with respect to key characteristics (for example using propensity score techniques, see Austin 2011). However, in my study context, randomizing forest use restrictions over the population of interest, or conducting an experiment on experience over several decades raises important ethical issues as well as being infeasible. While the correlation between experience of conservation and DCE responses may be spurious as the immigration status may affect both the hypothesised stimulus (experience of conservation) and the hypothesized response (respondents' preferences), I argue that immigration is not exogenous to conservation restrictions. One difficulty in identifying the effects of experience of forest conservation is their endogeneity: factors (such as migration or experience of outside interventions) that influence an individual's preferences could have affected the nature of his/her experience in the first place. My study design may have higher external validity[77] and be better suited to identifying temporal treatment involving learning or adaptation than more controlled experiments (such as randomised controlled trials) which are often of limited duration. This trade-off between internal and external

[77] The natural experiment that I used is rich of many contextual elements found in real-life settings, hence, there is a larger likelihood that these contexts could increase the applicability of my results.

validity equally applies to **chapter 4 and 5** which examined important issues in respondents' real life settings as opposed to laboratory experiments.

My approach to studying the validity and reliability of DCEs (using three field-based case studies: **chapters 3, 4 and 5**) produces results that are likely to be very context dependent (as are many DCE studies in the literature). One may argue that I cannot generalise on the basis of these individual cases or that they are of limited use in advancing the DCE method. However, I concur with Flyvbjerg (2006) that context-dependent knowledge is critically important because it is central to learning processes and inherent in social science[78]. While I cannot generalise from my case studies, their conclusions are relevant to other similar contexts in tropical countries (to the extent that the context in which respondents make decisions is similar to my study context) and that they contribute to the accumulation of a wider body of evidence that may ultimately allow a formal generalisation of my findings.

In empirical economics, validity is defined as "the best available approximation to the truth of a given proposition, inference, or conclusion" (Trochim 2006). Yet the real welfare costs of conservation are not and may never be observable. Therefore, any attempts to examine the validity of DCEs in such contexts have to resort to assessing DCEs against some measures that one hopes are good proxies for real validity. For instance, households' stated welfare losses based on their ex-ante preferences (**chapter 3**) may not be valid proxies for the compensation needed to offset the negative welfare impacts of forest conservation. Similarly, DCE responses elicited at day 1 (**chapter 4**), or households' preference elicited by the WTP format (**chapter 5**) may not be valid approximation to the true welfare costs of conservation. Even if one limits 'truth' to current preferences (as those expressed by less-experienced respondents), the real welfare costs of conservation will never be known due to the hypothetical nature of my DCE design, i.e. households may misreport their true valuation of the impacts of forest protection.

The use of the mixed methods approach in my thesis (DCEs combined with qualitative and quantitative debriefing approaches) may help increase the validity of my conclusions in **chapter 3, 4, and 5**. The qualitative debriefings, in particular, provide useful methodological insights into how to improve the design of future DCE studies. For example, in **chapter 3**, the debriefings offered important insights about whether the DCE results are consistent with theoretical expectations, which would not have

[78] As Flyvbverj (2006) put it: "Social science has not succeeded in producing general, context-independent theory and, thus, has in the final instance nothing else to offer than concrete, context-dependent knowledge."

been available through quantitative data alone. Any inability to trade-off attributes will restrict the margins at which a good or a policy can be valued using the DCE method. The findings (of high theoretical validity) suggest that qualitative debriefings constitute a critical validity tool which may not always corroborate the quantitative results. In **chapter 5**, the debriefings provided more in-depth insights into respondents' beliefs towards the policy being valued (state forest protection). A better understanding of respondents' motivations may help DCE researchers identify potential pitfalls in the study design. The evidence of strategic behaviour on day 2 which is linked to deliberation with others outside the household in **chapter 4** may indicate that limiting respondents' discussion within the confine of the household may help elicit more valid preferences. However, since the qualitative debriefing findings were obtained from a sub-sample of respondents who had answered the DCE survey, I can only elucidate mechanisms or processes that have been used by at least one respondent, generalisation of the qualitative findings must then be verified using quantitative methods. For example, the strategic behaviour observed in **chapter 4** and its association with group deliberation could be formally tested using an experimental approach.

Despite the roles of debriefing approaches (whether qualitative or quantitative) in supplementing the DCE survey and explaining respondents' rationale for their answers (and their important roles even in industrialised settings), they have been criticized as potentially clouding respondents' real motivations (Fischhoff and Furby 1988; Schkade and Payne 1994; Fischhoff et al. 1999). Instead of giving an accurate representation of how respondents made choices, they can reflect how people think, having had time to rationalise the situation. Such retrospective responses may express motivations that are susceptible to social desirability or conformity bias[79], and reflect further thoughts having subsequently acquired additional knowledge and more time for reflection. Also, retrospective debriefing techniques are prone to individuals' short-term memory which would imply that the processing of the information may be too superficial. While additional knowledge and the maturation of thoughts and preferences may be a desired purpose of qualitative debriefing approaches, it may be useful to distinguish the actual motivations of respondents from "post-hoc rationalisations" (Powe 2007). Fischhoff et al. (1993) argued that the use of concurrent verbal protocols or thinking aloud techniques in which respondents are asked to verbalise their thoughts while answering the stated preference survey are more appropriate in assessing respondents' thought processes and strategies (Schkade and Payne 1994; Whitty et al. 2014). However, these "concurrent" approaches are also not without criticisms as

[79] Social desirability or conformity bias refers to a situation in which the answer does not reflect the respondent's own view but rather is driven by a social-norm (Nederhof 1985)

they provide limited scope for probing responses during the interview without distracting respondents from the task of answering the valuation question (Powe 2007). The high cognitive demands of DCEs imply that it would be unrealistic to expect them to consider the issues and simultaneously provide an eloquent discussion of how they are making choices.

Given the challenging field conditions and my limited resources, I could not afford a bigger sample size, which would have presented many key advantages. A larger sample size would most importantly allow the testing of interactions between treatments, and more reliability or validity treatments such as the randomization of the order of choice cards or the use of independent pre-test and post-test control groups in the examination of the effect of time to deliberate (Teisl et al. 1995). A larger sample size would also be more representative of the target population and capture greater diversity and outliers. Although outliers or extreme observations complicate data analysis, accounting for them offers a more accurate picture of the characteristics of the population. Surveying a larger sample size would also increase information and result in more precise parameter estimates. Increasing the sample size (for example in the examination of the effect of more time to deliberate - **Chapter 4**) can also allow a greater power to detect a significant effect of the treatment, however, large sample sizes may not always be necessary as they may detect economically or practically insignificant effects.

The econometric methods used to analyse DCE data are evolving fast and this thesis might have benefited from more advanced modelling procedures. For example, advanced integrated choice and latent variable models might be used to incorporate endogenous attitudinal variables in the choice model and result in more accurate parameter estimates (Hess and Beharry-Borg 2012). I have observed a move towards ever more sophisticated and novel econometric model specifications to identify or control for choice behaviour that does not conform to random utility theory or to accommodate endogeneity or unobserved taste heterogeneity such as utility in willingness to pay space (e.g. Scarpa et al. 2008), the use of spatial sorting models combining discrete choice modelling capabilities and high resolution spatial data (Allen Klaiber and Phaneuf 2010), the use of eye tracking to test for attribute non-attendance (e.g. Balcombe et al. 2015), random regret models (e.g. Thiene et al. 2011) providing an alternative to random utility maximization-theory. Nevertheless, while these innovations are noteworthy, there are still opportunities for further enhancements in survey design on which my thesis has mostly focused, and in investigating whether these innovations improve the prediction of welfare impacts.

6.3 Further research

6.3.1 The need to widen the evidence base on the reliability and validity of DCEs both in developed and developing countries

The systematic review (**chapter 2**) finds a limited evidence base on various reliability and validity issues. My empirical chapters on the roles of qualitative debriefings, the effect of more deliberation and the WTA – WTP disparity have all demonstrated that context matters, and that the effect of a treatment (e.g. more time to deliberate) may be rather complex. Thus a better understanding of the reliability and validity of DCEs will depend on a wider evidence base across diverse contexts and settings, each of which may lack generalisability in itself. While this may not be encouraged by peer-reviewed journals which often prioritise novelty in the methods, a wider evidence base will help highlight the possible implications for those who might use DCE results and inform best practices. As DCE researchers always face uncertainties and difficulties in crafting and designing hypothetical surveys, replicating reliability and validity tests across different contexts would give policy makers more confidence (or undermine their confidence depending on the results) in using the DCE results in terms of alternative research design approaches (Whittington 2002).

6.3.2 The potential for supplementing individual interview debriefings with group debriefing approaches

A key advantage of focus group meetings is that they enable discussion between participants. Through information sharing, participants can gain a better understanding of different issues in the DCE survey. Sensitive issues like swidden agriculture may however be confidential and best explored in an individual setting where responses are not influenced by the comments of other participants and where respondents can reflect better on their preferences. Focus group debriefings such as those used by Powe et al. (2005) and Clarke et al. (2000) may be best used to consider general issues. A combination of both approaches, group meetings and individual debriefings may provide a more holistic understanding of different issues. However, DCE practitioners need to be aware of the considerable influence of moderators and the risk of domination by a few participants. While proponents (e.g. Ward 1999) argue that these problems can be overcome through adequate group moderation and structuring of the processes, these are issues can never be completely eliminated. I conducted five focus group debriefings alongside the individual debriefings. Focus group participants (N=42) were different from the interviewees and their selection aimed at reaching a good balance between diversity of opinions and a wide range of socio-economic characteristics. The number of groups was determined by the point of saturation which was reached when conducting focus groups

did not longer yields additional insights about the issues of interest. Data were transcribed but have not been analysed yet. I intend to write up a manuscript which aims to explore the potential for supplementing the individual debriefing findings with group debriefings. The latter appeared to succeed well at enabling discussion between respondents and revealed a wide range of issues complementary to (but also conflicting with) the individual debriefings.

6.3.3 The value of group-based valuation techniques or valuation workshops

While group-based deliberative valuation methods are at odds with neoclassical economics by producing communal or plural values, environmental economists need not restrict themselves to the neoclassical model of human behaviour (Spash 2008). Placing respondents in a valuation workshop context may yield values which are of critical importance to the management of natural resources. In the context of community based natural resources management, forest resources are often conceptualised as "commons" and valuation workshops may reveal collective, agreed and shared values amongst the stakeholders involved. In LICs, individual householders rarely make any significant decision about land on their own, they often have many people they consult with (i.e. decisions are often made at the lineage or extended family level). As experimental economics has often demonstrated the ineffectiveness of microeconomic theory in explaining behaviour concerning collective goods (Ostrom 2000), group-based approaches to valuations may add additional relevant perspectives to rational choice theory. For example, some individuals may use norms of trust and reciprocity (termed "conditional co-operators") instead of pursuing self-interests when participating in communal projects (*ibid*). Where individuals trusted other community members more, they were much more likely to cooperate in sustainable resource management (Cardenas 2004). More generally, the issue of whether individual and group valuations provide similar or complementary information has not been widely studied and warrants further research. Undeniably, local people may express a wide range of preferences (individual willingness to pay or shared values), either as individuals, as individuals in a group setting or as a group (Christie et al. 2012).

6.3.4 Informing the design of payments for ecosystem services (PES) in the context of weak institutions: Choice-experiment based approaches and behavioural economic games

Despite the widespread interest in PES/REDD+, the available evidence to guide policy is limited. The success of PES actually depends on how well the principles of PES suit varied circumstances and contexts. The majority of PES to date have been implemented in countries where the institutional framework is well-defined and where land is individually owned (e.g. Muñoz-Piña et al. 2008; Pagiola 2008). Designing PES in situations of weak institutions is challenging, yet these contexts are dominant

features of biodiversity hotspots where threats to ecosystem services are the highest (Chomitz et al. 2007) and where conservation actions are viewed as most urgent by the international conservation community (Myers et al. 2000). For instance, where property rights are not clearly defined or contested, it is challenging to determine who should be paid, and how contracts can be legally enforced. Empirical evidence is needed if PES interventions are to be appropriately designed and achieve positive impacts for both ecosystems and local livelihoods.

DCE can offer one approach to exploring the preferences of potential PES recipients for different mechanisms. They have been recently used to inform the design of PES among ecosystem services providers in developing countries but I only know of a few applications in Africa (Kaczan et al. 2013; Mulatu et al. 2014; Kassahun and Jacobsen 2015). While these all examined local preferences for the payment type and conditionality levels of PES, they were not primarily designed to examine various institutional foundations of the PES programs and focused more on valuing the incentives needed for the supply of ecosystem services. This research gap may be addressed using a DCE survey to assess local preferences for various institutional mechanisms. Such DCE would complement my thesis which suggests that securing local tenure is the best route to conservation, compared to coercively-based REDD+. As DCE may be prone to hypothetical bias (Hensher 2010b), a convergent validity analysis may be conducted by comparing its results with a behavioural economic game investigating local communities' behaviour in a range of PES treatments. Economic games have rarely been used to test the effectiveness of environmental interventions in developing countries (Cardenas 2004; Travers et al. 2011). These quantitative methods can also be combined with group-based and individual qualitative debriefing approaches to better understand respondents' thoughts processes and motivations.

6.4 Policy implications

In this section, I first discuss the policy implications of my thesis in the wider context of forest conservation in LICs. I then provide an overview on the acceptability of the DCE survey in policy making.

6.4.1 Policy implications in the wider context of forest conservation in low-income countries

Chapter 3 suggests that DCEs can successfully elicit current preferences in a low-income country with low literacy rate. However, ex-ante valuations of conservation impacts may not accurately estimate compensations because more experienced households stated significantly larger welfare impacts of future conservation restrictions than those who are less experienced. Less experienced households

may be ill-informed and hence less able to estimate the likely welfare impacts of such a long lived and complex intervention. Compensation using cash, while a preferred option for a few households (15% of the total sample), has significant limitations because of the limited opportunities for investment in remote rural areas. Likewise, while some households perceived the technical and material support for agricultural intensification as more bankable than cash, those who are more experienced were often distrustful of these novel agricultural techniques. As it is hard to accurately estimate in advance the costs of an intervention which may affect people negatively, effectively compensating the local costs of forest conservation may be unachievable under current conservation regimes (as in protected areas).

Chapter 5 suggests that the response patterns to the WTA and WTP formats largely differed and the use of WTP is inappropriate even if respondents do not hold formal rights over forestlands. Most respondents were very reluctant to relinquish their customary rights over forestlands. Giving respondents more time to deliberate (as investigated in **chapter 4**) might have lessened the effect of prior experience with the policy being valued (**chapter 3**) or helped respondents carefully reflect on the characteristics and implications of the good being valued which may in turn reduce the disparity between the WTA and WTP responses (**chapter 5**). However, more deliberation is not a panacea as respondents may develop strategic considerations and its effects may vary across different groups of respondents.

C**hapter 3 and 5**'s findings may therefore imply a need to rethink conservation policy. While a whole array of alternative approaches may be envisaged, I only explore below two main possibilities. The first appeals to the role of legislation while the second suggests a radical change of current conservation practices through the establishment of secure forestland tenure and the negotiation of conservation with owners. The traditional policy to limit forest clearance within protected areas, both in Madagascar and globally, is to impose an outright ban. Strictly enforcing the ban on clearing on forest clearance may be necessary to conserve natural forest resources in Madagascar and other LICs. Population growth and migration will increase the pressure on the remaining primary forests and local people will tend to convert forests to agricultural lands which is overall more profitable than sustainable resource exploitation. Coercion may therefore be the only way to stop or slow down deforestation - at least in the short run. Coercion may be seen as a simple or inexpensive approach, requiring only forest guards. However, as strict protection may result in very large welfare losses for

local people (**chapter 3 and 5**[80]**)** who are already extremely poor (having sufficient food for only half of the year on average), the ban must be associated with compensation which must outweigh the benefits that local people expect from an open frontier scenario. While various compensatory approaches have emerged in the past two decades, compensations often proved to be insufficient[81] (see section 1.1.1), and they have not accounted for likely variation of preferences and knowledge over time as local people gain more experience of restrictions (as I demonstrated in **chapter 3**). One way around this dilemma may be to over-compensate local farmers (e.g. by tripling or quadrupling their stated or estimated opportunity costs) to ensure that they do not incur any harm from future conservation actions. Over-compensations are also more likely to go beyond 'doing no harm' by improving the livelihoods of the poorest people in forest dependent communities, a desired attribute of REDD+ policy as explicitly stated in the Paris Agreement in December 2015 (UNFCCC 2015).

However, over-compensation would not only be a costly approach, but may also be very vulnerable to elite capture and may fail to reach the most vulnerable or those who are most affected by the restrictions (e.g. Pascual et al. 2014; Poudyal et al. 2016). Also, as strictly enforcing the ban on clearing using military forces was strongly perceived as illegitimate by the surveyed households (**chapter 5**), it may not succeed in stopping deforestation in the long term and may eventually face local resistance as has been evidenced in other African countries (Leach and Scoones 2015). Furthermore, given the political instability and corruption prevailing in Madagascar (and other LICs), the enforcement of laws strictly prohibiting forest clearance may reinforce the already existing power imbalance between conservation agencies and poor rural people as well as between rural people and the (often corrupt) state agents.

The significant effect of respondents' experience of conservation **(chapter 3)** on their stated welfare losses suggests that achieving fair and equitable compensations for coercive conservation measures may not be feasible. As welfare losses and human rights may need to be put before conservation emergency, an explicit recognition of customary rights may be more effective and legitimate at slowing down deforestation than the current coercive conservation model embedded in REDD+ policy.

[80] as I showed in **chapter 3 and 5,** such large welfare impacts are frequently expressed verbatim by local people as a "deep economic crisis" In Malagasy: "*fahasahiranana lalina"*

[81] However, insufficient compensations may not be exclusively attributed to the lack of reliable and valid estimates, but rather lack of willing or funds, and lack of political power of communities to demand them, and corrupt institutions. Thus compensation is structurally unlikely to happen, regardless of whether we have valid opportunity cost estimates.

Local people's strong aspirations to secure tenure, their claimed better capability in protecting forest resources as well as their inability to close access to migrants given their lack of formal tenure (**chapter 3 and 5**) all corroborate the need for such recognition. Farmers also asserted that stopping or slowing down forest clearance is in the interests of many local people and they did recognise the negative consequences of land degradation such as the need to walk long distances to find firewood or forest products for construction. **Chapter 3** also provide strong qualitative evidence that local people feel the opportunity costs of conservation restrictions over long periods of time, and can even perpetuate across generations as forest clearance is generally seen as a long term acquisition strategy. These findings may imply that if local people were given control over their forestlands, they would deforest slowly.

The work package 6 of p4ges (Poudyal et al. *in prep*, an ongoing project I am involved in) used the same DCE survey (WTA format) in two additional sites to explore the magnitude and distribution of the local opportunity costs of conservation restrictions seeking to slow deforestation driven by small-scale agricultural expansion. One site is affected by the new corridor of Ankeniheny-Zahamena (CAZ) protected area (Sahavazina *fokontany*) while another site is a long-established protected area (Zahamena national park). The median net present value of the costs per household, based on 450 choice experiments, is 2500 USD per household over 20 years across four sites (Ampahitra, Mantadia, Sahavazina, Zahamena). These costs (125 USD per year on average) represent 10-50 % of the household's annual incomes in the affected region. Publically available documents reveal that compensations offered under REDD+ social safeguards for the new CAZ protected area amounted to a one-off 120 USD per household (World Bank 2012). These compensations hugely underestimated our local welfare costs estimates and affected only a very small proportion of the population around the CAZ protected area. Our data suggests more than two thirds of affected people in the region have likely gone uncompensated. These findings cast doubt on whether effective compensation for coercive conservation measures (as in REDD+ policy) is likely. Compensating the local welfare costs of conservation restrictions may be very costly and challenging to achieve, especially given the difficulties of stimulating meaningful development through microprojects in remote areas, high transaction costs of identifying those eligible, lack of political power of communities to demand compensations, and the unwillingness of some people eligible to compensation to self-identify because of fear of sanctions (Poudyal et al. 2016).

My thesis and the p4ges project findings (Poudyal et al. 2016, Poudyal et al. in prep) highlight the need to reconsider coercive models of conservation (even with compensation) where these align very poorly with local people's beliefs about customary rights. A legal recognition of customary rights may

be materialised by establishing secure forestland tenure and enabling owners to exclude migrants and outsiders. A lack of recognition of customary tenure may incentivise local people to clear forests faster than they would do if they were given secure rights. Secure tenure would also ultimately reduce the transaction costs of negotiating compensations (Pagiola et al. 2005). Devolving secure tenure, including both legal ownership and rights to clear forest if they wish, would be fundamentally different from community based resource management approaches. Securing of forestland tenure must explicitly take into account the diversity and realities of the contexts. While giving secure common tenure over forests to stable communities (i.e. with a tradition of communal and shared ownership) might slow deforestation without any ban on *teviala*, individual legal tenure might be prioritized when the situation is not an idealised village with clearly defined common rights. While the amount of legislation recognizing or strengthening the forest and land rights of indigenous peoples and communities has increased significantly in the tropics since Rio 2012, the bulk of the progress has mostly been made in Latin America (Rights and Resources Initiative 2009; 2012b) and Africa is lagging behind. The very slow rate at which forest tenure reform is happening in Africa may be rooted not only in governments' persistence in claiming ownership over forestlands, but may also be caused by international agendas which may not create favourable opportunities to give due urgency to forest tenure reform (White and Martin 2002; Rights and Resources Initiative 2012a).

Secure forestland tenure may then be accompanied by voluntary conservation contracts negotiated directly with forest landowners. The considerable challenge of delivering effective compensations for forest use restrictions justifies an approach based on negotiated payments, just like in many industrialised countries, instead of compensation or mitigation of coercive regimes. While concepts similar to conservation contracts or agreements (payments for ecosystem services) have received greater attention in the conservation literature for almost two decades (Ferraro 2001), only very few have explicitly tackled the challenging institutional context of forest conservation in least developed countries where forest users are often not de jure landowners (Veronesi et al. 2015). When property rights are explicit and locally perceived as legitimate, contract negotiations would allow local people to claim incentives (cash or in-kind) for conservation activities. Local people would also have the freedom to choose their own livelihood strategies. Violations of contract conditions would imply the stopping of payments. Payments may also account for changing opportunity costs and be renegotiated over time (Kaczan et al. 2013; Veronesi et al. 2015). Such voluntary conservation contracts are also argued to be more cost-effective than previous indirect support for conservation (Ferraro and Kiss 2002). Great reluctance to adopt such policy recommendations among conservationists and governments alike is certainly to be expected. The social costs of forest conservation are often

underestimated and advocacy for nature conservation often reveals a lack of awareness of the high price that local people have to pay. These conservation advocates often assume that small farmers should not have a say in the bargaining about nature and should not be compensated for activities which are already illegal[82]. However, as conservation works better with local communities' support, a radical change in current practices may be needed. If these social costs are not put at the forefront of conservation actions, local people may resist conservation. Human-wildlife conflicts documented in the tropics (Dickman 2010) testify to the importance of recognising and resolving the social costs of conservation.

The well-documented failure of agricultural intensification projects in the tropics also suggests that governments and their partners need to radically change the way they implement these projects (Reardon et al. 1999). The availability of natural resources permitting a subsistence lifestyle may hinder intensification (Boserup 2005). I believe that agricultural intensification will occur in any scenario; when forest resources run out and users must innovate in response, or if the global conservation community is prepared to provide the much needed financial and technological expertise to support the transition before the exhaustion of natural forest resources. There might be no other way to tackle deforestation than providing local communities with the needed capital and technical support. Agricultural intensification is a long term and onerous endeavour, it requires investments (in labour, capital and techniques), and the costs of such investments are often hugely underestimated by agricultural development interventions. Such agricultural support would need to complement conservation contracts and must also be given a high priority; i.e. agricultural intensification alone are unlikely to be sufficient to divert from deforestation[83]. Most households in this study recognised that improved rice cultivation is essential when land fertility is decreasing and access to new forestlands is limited. While other examples around the world suggest that local communities have naturally developed new agricultural practices to adapt to the shortening of fallow periods (Carswell 1997; Sunderlin et al. 2005), external support is needed if intensification is to become more attractive to poor rural farmers (e.g. in terms of labour productivity, risk minimising strategy) than clearing forests for swidden agriculture. Agricultural interventions will also need to be

[82] These activities were originally made illegal by regimes that lacked any democratic legitimacy, often colonial regimes who sought to appropriate natural resources (Kull 2004).

[83] After all, if successful, agricultural intensification just increases the value of land but combined with negotiated cessation of deforestation, they can work (although they could increase the price of conservation contracts)

promoted in the local context. As Pollini (2007, p670) put it, "what matters is to implement research (and new interventions) in the farmer's conditions, not in the farmer's location." In addition to adequate support for agricultural intensification, remoteness should be explicitly tackled with the creation of new roads. As roads may also favour the intrusion and capture of benefits by outsiders, secure forestland tenure may need to precede the creation of roads. Tackling remoteness will also increase the ability of small farmers to use cash payments from conservation contracts to generate more profitable returns than unsustainable agricultural practices. Finally, the solutions that I propose to tackle the deforestation crisis in the tropics (securing of forestland tenure, conservation contracts, agricultural intensification and opening up access for remote communities) are only half the battle, governments must also get children of least developed countries into school and provide them with a better learning environment. Improving education in least developed countries will equip rural people with higher financial literacy, will help increase their ability to use and invest money, and may facilitate industrialisation and urbanisation away from the forests.

6.4.2 Acceptability of DCE within policy making: insights from field restitutions

I went back to the study sites in Mantadia[84] National Park (*Fokontany* of Volove and Vohibazaha) in December 2015 for two weeks to share some key messages from the DCE findings (mainly from **chapter 3** on the effect of prior experience of forest conservation on households' preferences). The restitutions were done via community meetings; preliminary invitations were sent out to the villages via a letter addressed to the *Chef de Fokontany*. I used a PowerPoint file and a small field projector to convey the messages in an easy to understand form. The use of the projector resulted in high turnout, about 50 and 40 participants showed up in Volove and Vohibazaha, respectively. In addition to reporting my study findings, the restitutions also aimed to seek feedback from villagers on the validity of the findings, and the acceptability of the valuations within policy making. I explore these two issues in turn the following paragraphs.

I asked participants in both *Fokontany* to discuss my interpretations of the results. Most participants agreed with the extremely high welfare impacts of restricting forest clearance and about their scepticisms towards their ability to use and invest cash. They also reiterated the important bequest value of forest clearance, i.e. the importance of lands acquired by forest clearance for their future descendants' needs. However, they acknowledged that local communities' preferences are not

[84] I could not do the restitution in Ampahitra because of time constraints. P4ges team is planning to conduct these restitutions later in May, this year.

homogenous. One important issue which was also raised in both meetings is the very low inclination of local people towards the improved rice cultivation, which is mainly motivated by the failure of past attempts to fund the real costs of these new agricultural techniques. About half of the participants in Volove asserted that they are constrained to engage in forest clearance activities because of the lack of viable alternatives. The saying used by one participant *"Teviala rice is like a thorn in the corpse, you are not at peace eating it"*[85] illuminates well how much they care for lemurs but cannot afford to spare their habitats. Almost 90% of participants in both *Fokontany* also complained about the fact that only a small proportion of local communities (mostly the local park committees which are only composed of 16 members) benefited from livelihood supports from the government. They argued that these benefits should accrue to everyone in the community since everyone is left destitute from the park's restrictions. Most participants were also very much in favour of my proposed policy recommendations on the need to rethink conservation approaches and the potential for securing forestlands tenure and negotiating conservation through voluntary PES schemes. They asserted that they would strongly approve such an approach and the government should seriously consider trialling it, even only on limited forest areas in the national park. They also raised the potential role of locally managed ecotourism in supplementing PES revenues and in supplying a lucrative revenue stream for local people.

I also enquired about the acceptability of the DCE results as a means of representing local people's preferences in policy decisions, i.e. whether participants view the DCE as an acceptable approach, and to what extent they feel their responses are a reliable source of information that can guide policy making. This was also interwoven with debates about issues such as the extent to which they can voice their concerns and aspirations and be heard and acted upon. The majority of participants in both meetings considered the valuation approach acceptable and suggested that their responses are accurate enough to inform actual decision making. They also trusted that their responses would be used appropriately and appreciated that the DCE provided them the opportunities to express their opinions and the freedom to choose what is best for their livelihoods. Participants seem desperate to voice their opinions and influence the establishment of a more responsive government. They claimed that they are currently marginalised from policy decisions and hoped that the DCE may offer one way to change this. I also requested an audience with Madagascar National Parks in the capital,

[85] This saying means that when something is earned using a deplorable mean or involving a significant cost, the person does not derive any satisfaction from it.

Antananarivo in December 2015 to present the same results but have not had any responses despite follow-up emails and calls.

6.5 Practical recommendations for designing and conducting DCE in a developing country setting

Designing and conducting DCEs in developing countries are not as straightforward as many environmental and resource economists might assume. Explaining hypothetical scenarios to illiterate respondents can be extremely overwhelming and in this section, I examine some of the issues and challenges I experienced and the important lessons learned. My aim is not to offer a generalised or streamlined guideline that would be applicable to every situation. The objective of this section is to highlight some challenges and issues that DCE practitioners and policy makers need to be aware of while conducting or using DCEs in developing countries particularly in rural or remote areas. This section aims to supplement Whittington's work (1998; 2002; 2004) on the administration of contingent valuation methods in developing countries with issues particular to DCEs as well as additional issues I faced when valuing a sensitive policy in remote areas. Such practical issues are hardly found in other textbooks or journal articles (e.g. Louviere et al. 2000b; Hensher et al. 2005; Bennett and Birol 2010) but need greater consideration from academics, decision makers and international organisations.

I first discuss some issues that have arisen in the design of the DCE survey I conducted in eastern Madagascar, these include the use of adequate wording in the framing of the scenarios, managing expectations while ensuring workable scenarios, striking a balance between neutral and credible scenarios. I then move to the implementation of the DCE survey and highlight the importance of selecting and training enumerators, the use of lengthy warm up steps, the questionnaire set-up, the importance of visualisations (as well as issues that need to be considered in their use), the importance of debriefings among the field team. I end with some additional ethical considerations which deserve special attention in developing countries: the challenges in obtaining informed consent in rural areas ensuring a balance between promises of anonymity and rigorous sampling, the need to compensate respondents for their time, and the importance of restitutions. This section is an interpretative essay, it is not meant to be exhaustive but to provide the reader with insights into some of the issues that can be encountered in developing countries.

6.5.1 Survey design

6.5.1.1 Cultural considerations and involvement of local researchers

The importance of question wording has been well-documented in the literature (Warnecke et al. 1997; Tourangeau et al. 2000). Researchers need to pay careful attention to the wording of DCE surveys in a culturally different population and the psychological processes that the wording may entail. Involving local researchers who understand their own cultural norms, the language in the design and piloting, as well as translation and administration of surveys, may have more potential to minimise the risks associated with cultural sensitivities than researchers from industrialised countries (Christie et al. 2012). I argue that involving local researchers at the early stages of the survey design is critically important. While intensively training enumerators is also essential, it should not substitute for the early involvement of local researchers. This recommendation highlights the need to build local researchers' capacity to execute and significantly contribute to research, and the need to carefully plan the timeframes needed to conduct the work effectively (Fazey et al. 2010). DCE research needs local researchers with the skills, time and confidence/power to influence the research design. However, researchers from outside a country should not *assume* that urban, educated nationals of that country will necessarily have a strong understanding of or empathy with, rural residents, just because they share the same language (more or less). Also, they may bring prejudices and conflicts of interest just like foreigners can.

As a researcher from Madagascar where I conducted the DCE survey, I first developed a version of the questionnaire in my native language Malagasy, while paying particular attention to the dialect used in the study site (*Betsimisaraka*), with which I am also familiar. My supervisor, Neal Hockley, who can also speak Malagasy, revised the questionnaire and helped detect any problems with the wording. The Malagasy questionnaire was then piloted and was significantly amended. The final version of the questionnaire after two piloting phases was then translated to English, the final stages of the piloting helped detect any further problematic terms. It was particularly difficult to convey the meaning of the term "hypothetical" in Malagasy. As hypothetical was not straightforwardly translatable in Malagasy, I and my research assistants had to establish the frame in respondents' minds and asked them to imagine different scenarios that are distinctly different from the reality, situations which don't exist yet but they can imagine as real ("*zavatra mbola tsy misy fa andeha vinavinaintsika hoe tena misy*"). We had to frame the hypothetical scenario as a game to desensitize the illegal nature of *teviala* (an approach used by Nielsen and colleagues, (2014) when valuing illegal bushmeat hunting in Tanzania) and used the local term "*kilalaon-tsaina*" (which literally means mental games). We used simple and

natural language and careful wording of the explanations and questions adjusting to the customary way of orally presenting questions in the study area. Crafting the right language for the valuation scenarios is not an easy task and warrants careful consideration, they often encompass nuance that is lost in translation and this confirms the importance of involving local researchers at an early stage in the survey design.

6.5.1.2 Framing of the valuation scenarios

- **Managing expectations while ensuring workable scenarios**

"A good valuation scenario is designed to be realistic and for respondents to take the hypothetical choice seriously" (Whittington 1998, p26). There was a risk that framing the DCE survey as a game would undermine the consequentiality of the DCE survey (whether respondents make choices as if they have real economic consequences). However, it helped avoid raising any expectations with regard to any future benefits generated by our survey and also helped desensitize *teviala*. We also avoided mentioning links to policies as people tended to exhaustively and emotionally discuss issues of property rights, forest protection and its negative impacts on their livelihoods. We do not have any doubt that respondents considered the consequentiality of the valuation exercise, in fact, many tended to take it too seriously and needed to be reminded about the aim of our research at some points during the survey (estimating the welfare impacts of forest conservation on their livelihoods). Such observations concur with other scholars' experiences (Whittington 1998; Glenk 2006).

The more seriously respondents consider the valuation scenario, the less hypothetical they are, and this is likely the case for large environmental or development interventions. A lack of understanding of the hypothetical nature of the valuation scenarios may result in much confusion and the spread of misinformation which raise many critical ethical issues (Whittington 2004). Despite the efforts we put into explaining the hypothetical nature of the DCE survey, a few respondents still felt confused or misled. In a few cases, the household heads were trembling because of the fear of potential prosecution, there was a risk that they would take actions that would harm them or others. In other cases, the scenarios may also influence their behaviour and encourage them to clear as much forests as they could after the survey period because they believed that restrictions will soon be strictly enforced. The rapid spread of confusion may also have unintended consequences on policy making which can result in repressive actions against the community and thus harm the respondents or others. While these ethical risks are much higher for sensitive goods, they are issues that need to be given careful considerations in any cross-cultural context or in remote and impoverished areas of developing countries where illiteracy rate is high.

While it was important that respondents do not take the survey too seriously, they also need to value the good we wanted them to value and consider the impacts of attribute changes on their welfare. Another related issue we faced while designing and piloting the scenarios was the framing of the strict enforcement of the ban on forest clearance. Where clearance is prohibited, we originally included an attribute specifying fines payable if people were caught clearing forests to ensure strict enforcement. However this lacked realism given the very high corruption rate in Madagascar. We also tested different durations of prison sentences but faced a similar issue, and respondents could not distinguish between different levels. We eventually used gendarme dolls to convey the strict enforcement of forest protection. Finding the right balance between managing expectations and ensuring workable and realistic scenarios requires careful adjustments of the survey as well as extensive piloting (ideally interspersed across sequential stages).

- **Ensuring a balance between neutrality and credibility**

A balance must be struck between emphasizing the neutrality of the research and ensuring the credibility of the scenarios, particularly that of the institutional regime which would deliver the hypothetical good or service (i.e. which institution would manage the cash donations in the WTA scenario or to which institution the payment would be made in the WTP scenario). A few respondents found it implausible that we were not sent by the government or an organisation with a particular agenda or political interest. It was difficult for them to dissociate us from government representatives who may dupe them into taking part and admitting that they are engaging in illegal behaviour. Respondents needed to be repeatedly reminded that the research was not funded by any NGO or Malagasy governmental organisations and we did not have any predefined interests regarding the future of the protected areas. We explained that we neither encouraged them nor stop them from clearing forests. At the same time, it was important that the institutional context described in the valuation scenarios were credible[86]. In the WTP format, we mentioned that they would be paying the government but that an independent institution would also be involved to ensure transparency. Theoretically, it would be impossible to value a policy independently of the institutional regime which would ensure its implementation. As Whittington (1998, p28) put it, “as long as people have

[86] DCE researchers may be concerned that revealing the institutional regime which would deliver the good or the policy would bias the valuation results since they would be measuring respondents’ distrust of the institutions instead of measuring the economic benefits or costs of the proposed policy. I concur with Whittington (1998) that since the institutional context within which respondents’ preferences are likely to be elicited in reality is assumed to be the correct one by most revealed preference approaches, DCE researchers ought to use the most credible institutional regime.

preferences with regard to various aspects of how a project is carried out, such preferences need to be taken into account." However, DCE researchers also need to emphasise the neutrality of the research and finding the right way to convey it to remote communities may be sometimes challenging.

- **Other framing considerations**

In our DCE survey, each choice task was composed of three alternatives including the reference level alternative. DCE surveys usually include a status quo option (or do nothing or opt out) but since a status quo option (households' own current "levels" for each attribute) would vary enormously across respondents[87], we opted for a reference level option where protection is totally lifted. Elucidating a status quo alternative would also have suffered from the problem of requiring respondents to reveal their current participation in forest clearance (which is illegal). Whittington and Adamowicz (2011) strongly advise against the use of hypothetical baselines because of the higher cognitive challenges that they pose to respondents or the difficulties it entails for advising policy makers. I concur with such recommendations but argue that the open or closed forest frontier used as our reference level or "status quo" scenario are not hypothetical to the respondents as they have experienced it in previous forest policies. Using follow-up questions and qualitative debriefing approaches is extremely useful in ensuring that the respondent understands the story in the scenarios about the hypothetical baseline and interprets it in the manner that the DCE researcher intends.

6.5.2 Survey preparation and implementation

6.5.2.1 The importance of visualisations and warm up steps

Visual aids may help reduce the complexity and cognitive challenges inherent in the task of making trade-offs in a DCE survey. They may be valuable tools to mediate information, particularly for respondents in developing countries with low literacy or educational levels. However, DCE researchers should choose visualisations carefully as they can be more influential than oral information and may not convey the exact message that DCE researchers intended them to. It is therefore essential to extensively pre-test what local people associate with the visual aids presented and ensure that their meanings are correctly conveyed orally by enumerators (as the main DCE interviews are almost always conducted in person). In self-administered questionnaires, visual tools should not also substitute

[87] As forest clearance is illegal, respondents' practices would vary enormously depending on the level of enforcement (and hence the opportunities), their labour and capital as well as their behaviour (i.e. whether they are risk loving or averse).

textual information. We used large photographs and dolls to represent the donor and the gendarmes and various colourful background papers to represent the three alternatives in each choice set and succeeded well at facilitating the understanding of respondents as well as in mediating the hypothetical nature of the survey.

We also used lengthy warm-up steps before giving the actual choice sets to give respondents some practice. The warm-ups also helped make respondents comfortable with the interview and establish trust with the enumerators. Given the sensitive nature of the good we valued, warm-up steps were critical in desensitizing forest clearance as well as in ensuring that respondents understood the task of making trade-offs. Although these lengthy explanations considerably eased respondents' comprehension of the valuation exercise, I acknowledge that this may have also biased respondents' choice decisions. Too little information can mean that the respondent has an inadequate understanding of the scenario considered, but too much information may also confuse respondents or distort their true preferences. However, I argue that a key responsibility of the DCE researcher is to provide sufficient information for the respondents to be able to state their preference meaningfully, where the scenarios are understandable, credible, and in as neutral a manner as possible. As a practical matter, a design aspect may need to be sacrificed to mitigate the high cognitive demand of DCE surveys. In our case the interview necessarily lasted on average 1.5 hours, a factor which should be borne in mind by researchers hoping to achieve large sample sizes.

6.5.2.2 The importance of selecting and training enumerators

Selecting good enumerators is of key importance to the quality of the DCE data. They are the bridge between a carefully designed survey and their successful communication to respondents. I would argue that DCEs cannot really be done in low-income contexts by 'enumerators' – at least some if not all of the 'interviewers' have to be researchers - i.e. able and willing to contribute to the (re)design of instruments and interpretation of results. I concur with Whittington's criteria (2002) on what makes a good enumerator. The most important criterion would be the enumerator's willingness and motivation to learn and follow the researchers' instructions. They need to have a great deal of patience in conveying a large amount of information to respondents. Also as neutrality is important, they may not be NGO or government officials who are already engaged in conservation or development interventions and may have predisposed ideas or interests with the issues at stake. Extroverted or extremely self-confident candidates may not necessarily be the best choice. Similarly, the selection process should not hinge too heavily on the interviewing skills and experience. Interpersonal skills such as human dynamics are also equally important as enumerators have to stay

together for a long period of time i.e. how well they interact with each other and mutually help each other.

Once selected, training the enumerators is a major task. They need to understand the objectives of the DCE survey, the theories and concepts underpinning DCEs, the principle of neutrality, and the rationale behind the treatments used[88] (such as the time to deliberate treatment we used or the split sample used to investigate the disparity between the willingness-to-accept and willingness to-pay). The main difficulty that the DCE researcher will face in training enumerators is to explain what the DCE is and what the objectives of the treatments are. I spent a whole week in classroom explaining what the DCE is about, the experimental design, the use of repeated choice sets and the importance of the treatments in examining the reliability and validity of the DCE method. For example, the use of the test-retest method in investigating the effect of more time to deliberate was motivated by the difficulty I (as the researcher) faced while deciding about certain design aspects of the DCE scenarios. As I could not know in advance which alternative research design would work best or yield more accurate or reliable responses, I tested different approaches that would increase the confidence I have in my results as well as improve the DCE method. The enumerators need to be able to rephrase the DCE information if the respondent does not initially understand as well as answer respondents' questions while ensuring consistency in the information provided. The researcher has the responsibility to develop contingencies or any eventualities in the issues or potential questions that may arise and the different ways to deal with them. Classroom trainings or role-playing are not enough, extensive pre-testing is the main component of the training where enumerators first observe the researcher administering the DCE in the field and then interview respondents (while being accompanied by the researcher). It would be legitimate to dismiss enumerators who do not display entirely satisfactory criteria after the piloting. In my experience, training enumerators and piloting often require more time than is available under the pressure of research deadlines or funders. Maintaining the motivations of the retained enumerators is also important by rewarding them for good work and providing them with health kits and any small items that they may need for the field.

[88] Although it might be thought desirable to achieve a degree of "blinding" – but this may usually be impractical

6.5.2.3 Other field practicalities in administering the DCE survey in the field

We first piloted the use of tablets in recording responses while administering the survey. The tablet however attracted too much attention and distracted the household from answering the DCE survey. We therefore decided to use paper and pencil and enter data immediately at the end of the day. This helped detect any errors or missing data early on and promptly correct them or check with the respondents. It also motivated interviewers to input any interpretations or observations while they are still fresh in their minds. I and the enumerators stayed together during the whole period of field survey in each village, which enabled frequent debriefings and constructive work. The field work team's strategy to gather in one single location and survey nearby households within a relatively short time period also helped in limiting communication between respondents and hence avoid contamination bias[89] (where households who had been administered the survey talked to other households about their choices).

6.5.3 Other ethical considerations

Many enumerators or local researchers in developing countries are not familiar with essential ethical principles. Rigorous training, regular debriefing and the researcher's supervision are of key importance in ensuring that enumerators abide by the ethical rules.

6.5.3.1 Informed consent: respecting respondents' rights not to participate and promising anonymity

Getting informed consent is challenging in developing countries where many respondents can neither read nor write. Making them sign a consent form is therefore problematic because of trust issues. Enumerator's training therefore plays a significant role in ensuring that respondents are treated with respect. The promises of anonymity is often the hardest ethical aspect to explain to respondents (and enumerators alike[90]) yet it is vital for respondents to truthfully reveal their preferences for sensitive goods. Respondents often find it hard to believe that their responses will not be linked to their identity, likewise, enumerators often do not understand why respondents' identity must be kept confidential from donors funding the study. In the field, we successfully demonstrated during the interview the need to protect respondents' identity by removing the first page of the DCE questionnaire where we recorded respondents' identity and using codes for the remaining pages. A

[89] In our study sites, contamination bias was further limited by the widely scattered locations of households within the forest landscape, some isolated households taking 4-5 hours of walk from the nearest hamlet.

[90] Or even NGO representatives or government officials who have never had any training in ethics.

particular challenge I faced in the field is also the balance between promises of anonymity and rigorous random sampling procedures (i.e. the equitable selection of respondents) which rely on an accurate sampling frame and the accurate location of each household. Since no map or census of households was available in our study sites (and probably in many remote areas of developing countries), we had to make significant efforts in constructing a reliable sampling frame and recorded households' GPS locations. Obtaining meaningful informed consent is not therefore trivial and requires careful considerations from both donors and DCE researchers.

6.5.3.2 Compensating respondents for their time and the importance of restitutions

Compensating or paying respondents for their time is not straightforward in a cross-cultural context. Local researchers in developing countries are not used to compensating respondents and it often raises some troubling ethical issues. Many would argue against compensations because of the risks of imposing higher costs on other researchers and creating potential jealousies or envy among villagers (i.e. those sampled and not). Compensations may also influence respondents' answers in an unexpected way, for example, they may incentivise respondents to answer in a certain way hoping they will receive more. However, we argue that providing compensations to respondents for their time (in kind or cash, depending on what is most appropriate locally) is essential. Compensations may reduce respondents' fatigue and compensate them for the valuable time they gave up for the interview. We gave participants small gifts like cups, sugar, candles and oil that we presented as a small gesture of appreciation for their time. Respondents really appreciated and valued them. As the success of social research heavily hinges on the relationship between researchers and respondents, compensations may need to become the norm in developing countries. Likewise, communicating back the results of the DCE to respondents is critically important as respondents are entitled to know the research findings. Restitutions would give room to respondents to check that their opinions and preferences have been characterised accurately. Restitutions may also give them the opportunity to ask any questions or raise any issues with regard to the survey. Communicating back the research results to local communities would also increase the social value of the research by embedding local people into the research program and hence promoting inclusivity and participation in policy making.

To conclude, local people in developing countries are entitled to an independent review of the risks and benefits of the research. Such review may be completed by an oversight institution ideally based in the countries where projects are implemented and knowledgeable about the particular cultural norms and contexts in these countries. International organizations that typically use their own resources for funding research activities, such as the World Bank, must create such a regulatory

structure or enable an existing independent institution to scrutinise research projects' ethical proposals and ensure compliance with ethical rules (Whittington 2004).

6.6 Conclusion

Solving the trade-off between conservation and local livelihoods has long been a major concern in the environmental arena. This trade-off is not unique to Madagascar but is shared by other biodiversity hotspots in the tropics where poverty and threats to habitat are most severe. Primary forests provide new agricultural lands to local people and the closing of the forest frontier (as in protected areas) inevitably harms their food provision. While biodiversity should be rightly conserved both for its intrinsic and economic values, concerns for non-human species should not be prioritised over social welfare. The urgency with which protected areas are being established in the tropics to halt biodiversity loss is particularly worrying. While local costs and the need to compensate them are being increasingly recognised, there is a dearth of ex-ante assessments of these local costs and lack of consideration of alternative conservation policy options. Choice experiments have been used to evaluate, ex-ante, the impacts of policy change but my systematic review showed that most of the evidence concerning the validity and reliability of the approach in valuing non-market environmental goods comes from industrialised contexts. I used choice experiments to estimate ex-ante the costs of conservation restrictions limiting the clearance of forests for swidden agriculture in the eastern rainforests of Madagascar. I devoted particular attention to exploring the reliability and validity of this increasingly widely used approach in a challenging low-income context where local people's literacy levels are very low and swidden agriculture is a sensitive issue. I found that experience of conservation restrictions matters, households' stated welfare losses based on their ex-ante preferences (i.e. inexperienced households' preferences) may therefore be biased indicators of compensation. My results suggest that estimating compensations for coercive conservation measures may be impractical. While deliberation may provide less literate respondents more opportunities to reflect on the survey questions, it may promote strategic behaviour. I also found a strong demand for securing local forest tenure and evidence that doing so may better protect forest resources. Achieving both conservation and enhanced local well-being may require a radical change from current policies, including devolution of secure forestland tenure to local people and genuinely negotiating conservation with forest owners.

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APPENDICES

APPENDIX 1: DESIGN OF THE DCE SURVEY: SELECTION OF ATTRIBUTES AND THEIR LEVELS IN THE WTA FORMAT

Before selecting the final attributes, we pretested a complex package of compensation measures as potential attributes of the DCE survey using focus groups (e.g. cash payments, in-kind compensations, different development projects, communal level compensations) but eventually kept a relatively simple CE design with four attributes to avoid overloading and confusing respondents with too many attributes given their low literacy levels (2 years of schooling on average). We also disregarded all communal aspects of compensations as they were not viewed as additional and were considered in the piloting as crowding out government's responsibilities. Also specifying different bundles of compensations packages would not allow us to compare between different communities which might state different bundles of compensations according to their specific situations.

The cash payment attribute was designed to imitate future revenues from REDD+ projects which also aim at improving people's livelihoods in addition to compensating them for foregoing forest use. We instructed respondents that these payments can be used for various purposes, e.g. for buying manure fertilizers, hiring labour, etc. We also converted them in baskets of rice using the paddy fields market prices at the harvest period so that respondents could easily comprehend how much they are worth.

Previous studies estimating the local costs of deforestation aided the selection of the payment levels, these studies' assumptions reveal several inadequacies that are relaxed in our survey. Shyamsundar and Kramer's (1996) major assumption was that the establishment of the national park would result in people losing access to only a given area of forestland (208 hectare) and that only 18% of people's agricultural lands used by the average household lie within the park. Therefore, if one average household produces on average 500 kg of paddy, only 92 kg or approximately 6 baskets of rice (one basket of rice equals 15 kg of paddy yields) would be the average opportunity costs of the establishment of the protected area per household. We however posit that people's foregone benefits are much higher. Opportunity costs of *teviala* can be as high as 100% of the average respondents' income and should also take into account potential forestlands for *teviala* and potential *teviala* doers, i.e. those not currently doing *teviala* but who may do *teviala* in the near future. To set the initial payment level that we used in the piloting, we roughly estimated the net present value (paddy yields) of new *teviala* plots[91]. We also relaxed Ferraro's assumption (2002) which sets to zero the value of any alternative options to *teviala*. We argue that agricultural costs alone do not fully capture the net costs of forest conservation, as people may have other substitutes. Our design also ensured that

[91] Considering how much area an average household is likely to clear per year and over a 20 year project period and using Aubert et al.'s report (2003) on the average agricultural returns on the land for a typical tavy cycle starting with a primary forest taking into account the decrease of productivity of lands. We however note that these agricultural returns (as reported by Aubert et al. 2003) were just used as crude indicators as they depend highly on a set of assumptions on the costs of labour, land productivity, discount rate, land tenure and market properties (whether market is well-functioning or not which is not often the case in a developing country context).

respondents took into account any ecological (indirect or less tangible) benefits of forest conservation. To do so we included a follow-up question to check whether respondents perceived any of these benefits and took them into account in their choice decisions.

There was a risk that framing the DCE survey as a game would undermine the consequentiality of our DCE survey (whether respondents make choices as if they have real economic consequences). However, it helped avoid raising any expectations with regard to any future benefits generated by our survey and also helped desensitize *teviala*. We also avoided mentioning links to policies or real life context as people tended to exhaustively and emotionally discuss issues of property rights, forest protection and its consequence on their livelihoods. We do not have any doubt that the respondents considered the consequentiality of the valuation exercise, in fact, many tend to take it too seriously and needed to be reminded about the aim of our research.

For the *teviala* attribute, to ensure conditionality, we originally included an attribute specifying fines payable if people were caught doing *teviala*. But this lacked realism given the very high corruption rate in Madagascar. We also tested different durations of prison sentences but faced a similar issue, and respondents could not distinguish between different levels. We eventually used gendarme dolls to convey the strict enforcement of forest protection. Since we framed the cash payments as donations to help respondents engage with the valuation survey, the use of the gendarme dolls helped emphasize conditionality (i.e. donations are conditional on forest use restrictions).

While administering the survey, we used simple and natural language and careful wording of the explanations and questions adjusting to the customary way of orally presenting questions in the area. Hence, instead of proceeding directly to the point, we used some perambulations before asking the respondent the actual question. This helped make the respondents comfortable with the interview and establish trust with the enumerator. We also had to use some warm-up steps to give respondents some practice and ensure they understood the task of making trade-offs.

APPENDIX 2: WILLINGNESS TO ACCEPT QUESTIONNAIRE[92]

1. General introduction, consent and anonymity

"We are university students and researchers who aim to better understand your livelihoods. We would like to do a valuation exercise that we framed as a game in which you will be asked to consider different choice scenarios that you are well knowledgeable about. We will ask you to choose what you prefer most among the alternatives i.e. which option you think is best for your livelihoods / welfare. Please kindly note that even if we frame it as a game we ask you to answer as you would in a real situation.

The results of the research can be used by policy makers. But please note that we are conducting an independent research, i.e. we are independent of the government and thus we have neutral views with regard to forest use and forest policies. We also reassure you that your answers will not be communicated to anyone in a form where your reply can be linked to you.

First of all, we would like to kindly ask your consent, i.e. whether you willingly accept to be interviewed. Know that if you are happy to be interviewed, you can still stop us at any time or refrain from answering any question that you are not comfortable with.

2. Introduce the technical and material support for improved rice farming

Please consider a major foreign donor who would like to provide you with some development assistance (we used a doll to represent the donor). The donor would like to help you specifically with the improved rice cultivation technique. It targets rice cultivation both on hills and flat lands and it can be described as a sustainable and modern agricultural package that includes productivity enhancing practices such as the use of fertilisers, insecticides and/or herbicides. It involves digging and possibly the construction of terraces for slopes and precludes the use of fire as a way to maintain fertility while not fallowing the land. Each household will also be provided with some agricultural materials and inputs such as improved seeds, fertilisers, wheelbarrows, spades, etc.

Question: How familiar are you with such improved rice cultivation technique? (coding: 3= I've done it myself, 2=I've seen other people doing it, 1= I've heard about it, 0=never heard nor seen it)

If you were receiving tools for improved rice cultivation for free, how promising that would be for improving your livelihood? "I believe that the technical rice farming would better my livelihoods" - To be measured on a Likert scale from 1 to 5 (1='strongly disagree', 2='disagree', 3= 'neither disagree nor agree', 4='agree', 5='strongly agree')

3. Compare the improved rice cultivation with cash payments

Next consider that the donor lets you freely choose which development assistance you find best for your livelihoods. Therefore, the donor also offers to give you some cash payments that you can invest in any alternative income generating activities of your choice or to purchase materials or fertilizers,

[92] Translated from Malagasy

etc. Such cash payment would be managed by an independent external institution such as a microcredit or an access bank which will provide you with savings accounts.

Now, consider that the donor is asking you to choose between the two following options.

A	B
You will be donated 1,000,000 MGA cash	You will be provided with support for the improved rice cultivation

If the respondent chooses the cash payment, ask him/her, what would s/he do with the money?

In what follows, we will ask other scenarios but you have to look at each independently from each other.

4. Introduce *teviala* on one ha of forestland

Now, please consider that the government would make it possible for you to get a permit to do *teviala* on one hectare of forestland. This would be like a new additional land, still very fertile for which you have a legal title (no risk of being penalized by the government).

What would you cultivate on that land if you were offered it? For how many years would you cultivate there before fallowing the land?

Now, please consider the two following choices and choose which one you like most:

A	B
You will be donated 6,000,000 MGA (6 tapitrisa) cash *You would receive that cash in 10 instalments (i.e. 600,000 per year for 10 years)* *You must not do teviala anymore (forever)*	You will be provided with support for the improved rice cultivation - You will be offered one *teviala* permit on one ha

Please know that the *teviala* permit on one hectare is a one-off opportunity. Likewise, the rice cultivation technique would be a one-off project.

5. Introduce free *teviala*

Next, please consider that the government would make it possible for you to get a permit to do *teviala* on an unlimited forestland (i.e. not limited to one hectare). This would be similar to the former President Ratsiraka's government (1975-1991) where *teviala* permits were formally granted to rural farmers. That would mean that the government would stop enforcing forest protection and you can think of it as an open forest frontier.

So if you were offered the choice below, which one would you choose? I.e. which one is the best option for your livelihoods?

A	B	C
Cash payment: 9,000,000 MGA	-	Free *teviala*
Number of instalment: 10 years (900,000 per year)	-	
No support for improved rice cultivation	Support for improved rice cultivation	
You and your children must not do *teviala* presently and in the future (forever)	*Teviala* permit on 1 ha	
(*need to emphasize here that they can still buy paddy field or borrow or rent lands**)		

Know that in choice A where you must not do *teviala*, protection will be strictly enforced (will need to place some officer dolls in the alternative). If you are caught doing *teviala* even in one small area of forestland, you must face at least 5 years of imprisonment. No one will escape as patrolling efforts will be very stringent.

Also, in alternative B (*we used colourful background paper to distinguish the three alternatives*), you must not exceed one hectare or do *teviala* elsewhere without a permit. The sanction would be the same if you don't abide by the rules.

6. Introduce number of instalments

Next, please consider that in the following choice, you must not do *teviala* anymore. Which one would you choose?

A	B
Cash payment: 9,000,000 MGA	**Cash payment**: 6,000,000 MGA
Number of instalment: 20 years (450,000 per year)	Number of instalment: 10 years (600,000 per year)
No *teviala*	

Ask the respondent if s/he has any questions, then proceed with the first choice card of the DCE survey. The position of the reference level alternative (i.e. the open forest frontier) was alternated in the six choice tasks (across the three columns, A, B, and C).

7. Respondents' rated comprehension (scale 1 to 5) – For the overall CE exercise – to be measured by the enumerator)

1: The respondent doesn't understand the CE valuation exercise at all, all the choices seem inconsistent, barely finished the choice cards

2: Many inconsistencies but at least finished all the choice cards

3: So so, the respondent seems to understand the generalities but still missed the details, still made few inconsistent choices

4: In general, good understanding of the details and generalities, made only one- two inconsistent choices

5: Perfect, understands very well everything

8. Certainty codes (to be scored by the interviewers for each choice card on a scale of 1 to 5)

1: very uncertain, changed his/her choice many times, asked the interviewers to re-explain the choice cards again one or two more time, took ages to finally make up his/her mind

2: uncertain, the respondent took also very long but least a little bit quicker than 1

3: so, so, seems a bit uncertain but at least when s/he made his choices eventually, s/he seemed certain.

4: certain of his/her choice, the respondent was quick and didn't request any additional explanations

5: no doubt at all, relatively quick, made up his mind after the first round of explanation.

9. Follow-up questions

A. Questions about experience of use restrictions (length of restriction and severity of past sanctions)

1. Is there in your family anyone who has been penalized because of *teviala* practices? *(Coding yes = 1, no = 0).* If yes, how many, when and what was the sanctions?

2. In the village territory, do you know anyone who has been imprisoned or who has incurred sanctions because of *teviala* practices? *(Coding yes = 1, no = 0).* If yes, how many, when and what was the sanctions?

3. a. How is the current enforcement level of forest protection? *(This is only a dummy question to introduce 3b and 3c, write down any categorical answer)*. Ask the respondent to give examples of how strong or how weak it is.

b. Compared to now, how stronger or weaker was it in the past (past here depends on the age of the respondent, can be 5 years ago, 10 years ago, prompt the respondent)? (Coding: 3= I don't know, 2=Stronger, 1=weaker, 0=stay the same)

c. In your opinion, do you think the protection will become stronger, weaker, or stay the same in the future, let's say in 5 years' time? (Coding: 2=Stronger, 1=weaker, 0=stay the same, -99= I don't know)

B. Questions related to the valuation exercise

4. Would there be still any traditional rules regulating *teviala* in the free *teviala* scenario where the government is not present? (coding yes = 1, no = 0, -99= I don't know)

   If yes, have these rules changed compared to before? (coding yes = 1, no = 0, -99= I don't know*)*

5. How plausible did you find the idea of a donor who is interested in development that would give you cash instead of usual development projects (e.g. improved rice-growing technique)?

   (to be measured on a Likert scale 1 to 5, 1: very implausible 3= probably (not so sure), 5: very plausible)

6. How much do you trust the independent institution which is to manage your money over time?

(to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)

If his answer is 1, ask him which institution he would trust more and he could suggest to keep his/her money?

7. If the respondent always chose the free *teviala* option, ask why?

8. If the respondent always chose the no *teviala* option, ask why?

C. Questions about beliefs and perceptions

9. Do you perceive any non-use benefits from forest protection? *(coding yes = 1, no = 0,* -99= I don't know*)*

If yes, can you give examples?

When you made your choices, did you take these non-use benefits into account? *(coding yes = 1, no = 0,* -99= I don't know*)*

10. *Consequentiality of the valuation exercise:* Do you believe that the results of this research would be used to inform policy on forest conservation?

(to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)

11. How much do you believe you would be able to negotiate compensations / or any other requests with the government?

(to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)

12. In your opinion, which forest management policy is more legitimate: 1) You do not have the rights to forestlands and pay to be able to do teviala, that is state protection is legitimate) 2) You do have the rights and need to be paid not to do teviala, i.e. state's protection is not legitimate) *(1= State's protection is legitimate; 0= State's protection is not legitimate; -99=I don't know)*

WTA CHOICE CARDS

BLOCK 1

1	A	B	C
Payments (10^6 MGA)	Open forest frontier	6	3
Instalments		10	20
Improved rice project		Yes	Yes
Forest clearance		Closed	Open

2	A	B	C
Payments (10^6 MGA)	12	Open forest frontier	6
Instalments	1		10
Improved rice project	Yes		No
Forest clearance	1 ha		1 ha

3	A	B	C
Payments (10^6 MGA)	0	15	Open forest frontier
Instalments	0	20	
Improved rice project	Yes	No	
Forest clearance	Open	Closed	

4	A	B	C
Payments (10^6 MGA)	Open forest frontier	0	15
Instalments		0	20
Improved rice project		Yes	Yes
Forest clearance		Open	2

5	A	B	C
Payments (10^6 MGA)	3	Open forest frontier	12
Instalments	20		10
Improved rice project	No		No
Forest clearance	Open		1 ha

6	A	B	C
Payments (10^6 MGA)	9	12	Open forest frontier
Instalments	10	1	
Improved rice project	Yes	No	
Forest clearance	1 ha	Closed	

BLOCK 2

1	A	B	C
Payments (10^6 MGA)	Open forest frontier	6	3
Instalments		20	1
Improved rice project		No	Yes
Forest clearance		1 ha	1 ha

2	A	B	C
Payments (10^6 MGA)	0	Open forest frontier	9
Instalments	0		10
Improved rice project	Yes		No
Forest clearance	Open		Closed

3	A	B	C
Payments (10^6 MGA)	15	9	Open forest frontier
Instalments	10	1	
Improved rice project	No	No	
Forest clearance	Closed	1 ha	

4	A	B	C
Payments (10^6 MGA)	Open forest frontier	3	15
Instalments		20	10
Improved rice project		Yes	Yes
Forest clearance		Open	Closed

5	A	B	C
Payments (10^6 MGA)	12	Open forest frontier	6
Instalments	1		20
Improved rice project	No		No
Forest clearance	1 ha		Open

6	A	B	C
Payments (10^6 MGA)	9	0	Open forest frontier
Instalments	10	0	
Improved rice project	Yes	Yes	
Forest clearance	Closed	Open	

APPENDIX 3: WILLINGNESS TO PAY QUESTIONNAIRE[93]

1. Introduction, consent and anonymity

We are university students and researchers who aim to better understand about your livelihoods. We would like to do a valuation exercise related to your livelihoods. The exercise will be framed as a game in which you will be asked to consider different choice scenarios that you are well knowledgeable about. We will ask you to choose what you prefer most among the alternatives i.e. which option you think would be best for your livelihoods / welfare.
Please know that we are conducting an independent research, i.e. we are independent of the government and thus we have neutral views with regard to forest use and forest policies.

First of all, we would like to kindly ask your consent, i.e. whether you willingly accept to be interviewed. Know that if you are happy to be interviewed, you can still stop us at any time or refrain from answering any question that you are not comfortable with.

Be reassured that your answers will not be communicated to anyone in a form where your reply can be linked to you. Consequently your answers cannot be used against you, and we ask you to truthfully reveal your most preferred alternative on the basis of what is your best livelihood option.

2. Introduce the improved rice project

Please consider that you will be given the opportunity to buy a household project which can provide you with alternative livelihood strategies; i.e. a project that you can use to complement your income generating activities, such project will all include technical support from the start till the end production and start up materials. It targets rice cultivation both on hills and flat lands and it can be described as a sustainable and modern agricultural package that includes productivity enhancing practices such as the use of fertilisers, insecticides and/or herbicides. It involves digging and possibly the construction of terraces for slopes and precludes the use of fire as a way to maintain fertility while not fallowing the land. Each household will also be provided with some agricultural materials and inputs such as improved seeds, fertilisers, wheelbarrows, spades, etc.

Please consider that the primary project which you could purchase now is the improved rice cultivation technique. It specifically targets rice cultivation on steep hills and its main objective is to maintain soil fertility.

Question: How familiar are you with such improved rice cultivation technique? (coding: 3= I've done it myself, 2=I've seen other people doing it, 1= I've heard about it, 0=never heard nor seen it)

In your opinion, how promising is such improved rice cultivation with regard to improving your livelihoods? "I believe that the technical rice farming would better my livelihoods" - To be measured on a Likert scale from 1 to 5 (1='strongly disagree', 2='disagree', 3= 'neither disagree nor agree', 4='agree', 5='strongly agree')

[93] Translated from Malagasy

3. Introduce teviala permit on one ha of forestland

Now, please consider that you would also be able to buy a permit to do *teviala* on one hectare of forestland. This would be like a new additional land, still very fertile for which you have a legal title (no risk of being penalized by the government).

What would you cultivate on that land if you were offered it? For how many years would you cultivate there before fallowing the land?

Now, if you could afford both alternatives below, which one would choose?

A	**B**
A *teviala* permit on one ha	Improved rice cultivation project

Please know that the *teviala* permit on one hectare is a one-off opportunity, i.e. your household would be given the chance to buy it only once in your lifetime, likewise, the rice cultivation technique would be a one-off project.

Next, which one would you choose in the following scenario:

A	**B**
You pay: 500,000 Ar in 10 instalments (50,000 per year) You get a *teviala* permit on one ha	**You pay**: 100,000 Ar in 10 instalments (10,000 per year) You get the improved rice cultivation project

Note that you can pay only after harvest. But please we would like to kindly remind you to carefully consider whether you would be really able to afford the one you choose.
Know that you would be paying the government through state agents, and the permit would be legal. Note that the *Fokontany* and independent stakeholders would also be involved to ensure transparency.

4. Introduce free *teviala* and number of instalments

Next, please consider that the government would make it possible for you to buy a permit to do *teviala* on an unlimited forestland (i.e. not limited to one hectare). This would be similar to the former President Ratsiraka's government (1975-1991) where *teviala* permits were formally granted to rural farmers. That would mean that the government would stop enforcing forest protection and you can think of it as an open forest frontier.

So if you were offered the choice below, which one would you choose? I.e. which one is the best option for your livelihoods?

A	**B**
Cash payment (you pay in total) 1,500,000 MGA Number of instalment: 20 years (75,000 per year) Free *teviala*	Cash payment (you pay in total) 1,000,000 MGA Number of instalment: 10 years (100,000 per year)

5. Introduce an example of choice card

Next, which of the alternatives below would you choose? Please note that you need to carefully think of your budget constraints i.e. consider whether you would be really able to afford the one you choose.

A	B	C
You pay in total: 3,000,000 MGA Number of instalment: 20 years (150,000 per year) No project Free *teviala*	You pay in total: 500,000 MGA One instalment You get the improved rice cultivation project You get one *teviala* permit (1ha)	No payment and no *teviala* (forest protection strictly enforced) – (*this must not sound too negative, will need to emphasize here that they can still buy paddy field or borrow or rent lands*)

Know that in choice C where you must not do *teviala*, protection will be strictly enforced (will need to place some officer dolls in the alternative). If you are caught doing *teviala* even in one small area of forestland, you must face at least 5 years of imprisonment. No one will escape as patrolling efforts will be very stringent.

Also, in alternative B (this will be referred to as the colour of the background paper), you must not exceed one hectare or do *teviala* elsewhere without a permit. The sanction would be the same if you don't abide by the rules.

Ask the respondent if s/he has any question, then proceed with the first choice card

6. Comprehension codes (scale 1 to 5) – For the overall CE exercise – to be measured by the interviewer)

1: The respondent doesn't understand the CE valuation exercise at all, all the choices seem inconsistent, barely finished the choice cards

2: Many inconsistencies but at least finished all the choice cards

3: so so, the respondent seems to understand the generalities but still missed the details, still made few inconsistent choices

4: In general, good understanding of the details and generalities, made only one- two inconsistent choices

5: Perfect, understands very well everything

7. Certainty codes (to be scored by the interviewers for each choice card on a scale of 1 to 5)

1: very uncertain, changed his/her choice many times, asked the interviewers to re-explain the choice cards again one or two more time, took ages to finally make up his/her mind

2: uncertain, the respondent took also very long but least a little bit quicker than 1

3: so, so, seems a bit uncertain but at least when s/he made his choices eventually, s/he seemed certain.

4: certain of his/her choice, the respondent was quick and didn't request any additional explanations

5: no doubt at all, relatively quick, made up his mind after the first round of explanation.

8. Follow-up WTP format

A. Questions about experience of use restrictions (length of restriction and severity of past sanctions)

1. Is there in your family anyone who has been penalized because of *teviala* practices? *(Coding yes = 1, no = 0).* If yes, how many, when and what was the sanctions?
2. In the village territory, do you know anyone who has been imprisoned or who has incurred sanctions because of *teviala* practices? *(Coding yes = 1, no = 0).* If yes, how many, when and what was the sanctions?

3. a. How is the current enforcement level of forest protection? *(This is only a dummy question to introduce 3b and 3c, write down any categorical answer)*. Ask the respondent to give examples of how strong or how weak it is.

b. Compared to now, how stronger or weaker was it in the past (past here depends on the age of the respondent, can be 5 years ago, 10 years ago, prompt the respondent)? (Coding: 3= I don't know, 2=Stronger, 1=weaker, 0=stay the same)

c. In your opinion, do you think the protection will become stronger, weaker, or stay the same? (Coding: 2=Stronger, 1=weaker, 0=stay the same, -99= I don't know)

B. Questions related to the valuation exercise

4. Would there be still any traditional rules regulating *teviala* in the free *teviala* scenario where the government is not present? (coding yes = 1, no = 0, -99= I don't know)
   If yes, have these rules changed compared to before? (coding yes = 1, no = 0, -99= I don't know*)*
5. *Likelihood of the State selling permit in the valuation exercise:* How plausible did you find the idea of the government selling you a permit to do *teviala*?
   (to be measured on a Likert scale 1 to 5, 1: very implausible 3= probably (not so sure), 5: very plausible)
6. *If the government had sold you permit, would you trust the government to honour that permit forever?*
   (to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)
7. *If the respondent always chose the free teviala option, ask why?*
8. *If the respondent always chose the no teviala option, ask why?*

C. Questions about beliefs and perceptions

9. Do you perceive any non-use / ecological benefits from forest protection? *(coding yes = 1, no = 0,* -99= I don't know*)*
   If yes, can you give examples?

   When you made your choices, did you take these non-use benefits into account? *(coding yes = 1, no = 0,* -99= I don't know*)*

10. *Consequentiality of the valuation exercise:* Do you believe that the results of this research would be used to inform policy on forest conservation?
(to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)

11. How much do you believe you would be able to negotiate *teviala* permit / or any other requests with the government?
(to be measured on a Likert scale 1 to 5, 1: don't trust at all, 3= probably (not so sure), 5: very high trust)

12. In your opinion, which forest management policy is more legitimate: 1) You do not have the rights to forestlands and pay to be able to do teviala, that is state protection is legitimate) 2) You do have the rights and need to be paid not to do teviala, i.e. state's protection is not legitimate) *(1= State's protection is legitimate; 0= State's protection is not legitimate; -99=I don't know)*

WTP CHOICE CARDS

BLOCK 1

1	A	B	C
Payments (10^6 MGA)	Closed forest frontier	0	3
Instalments		0	10
Improved rice project		Yes	Yes
Forest clearance		Closed	Open

2	A	B	C
Payments (10^6 MGA)	1.5	Closed forest frontier	1
Instalments	10		20
Improved rice project	Yes		No
Forest clearance	1 ha		1 ha

3	A	B	C
Payments (10^6 MGA)	0.5	0	Closed forest frontier
Instalments	10	0	
Improved rice project	No	No	
Forest clearance	Open	1 ha	

4	A	B	C
Payments (10^6 MGA)	Closed forest frontier	1	1.5
Instalments		20	1
Improved rice project		Yes	No
Forest clearance		Closed	Open

5	A	B	C
Payments (10^6 MGA)	3	Closed forest frontier	2
Instalments	20		10
Improved rice project	No		Yes
Forest clearance	1 ha		Closed

6	A	B	C
Payments (10^6 MGA)	2	0.5	Closed forest frontier
Instalments	1	20	
Improved rice project	No	Yes	
Forest clearance	Open	Closed	

BLOCK 2

1	A	B	C
Payments (10^6 MGA)	Closed forest frontier	1	1.5
Instalments		20	1
Improved rice project		No	Yes
Forest clearance		1 ha	1 ha

2	A	B	C
Payments (10^6 MGA)	2	Closed forest frontier	0.5
Instalments	1		1
Improved rice project	Yes		Yes
Forest clearance	Open		1 ha

3	A	B	C
Payments (10^6 MGA)	0	3	Closed forest frontier
Instalments	0	20	
Improved rice project	Yes	No	
Forest clearance	Closed	Open	

4	A	B	C
Payments (10^6 MGA)	Closed forest frontier	3	2
Instalments		20	10
Improved rice project		No	Yes
Forest clearance		1ha	Closed

5	A	B	C
Payments (10^6 MGA)	1.5	Closed forest frontier	1
Instalments	10		20
Improved rice project	No		0
Forest clearance	Open		1ha

6	A	B	C
Payments (10^6 MGA)	0.5	0	Closed forest frontier
Instalments	10	0	
Improved rice project	Yes	No	
Forest clearance	1 ha	Open	

APPENDIX 4: CHOICE EXPERIMENT SURVEY IN PRACTICE

APPENDIX 5: EXPERIMENTAL DESIGN

We combined alternative levels of the four attributes in choice tasks using an efficient design that seeks to minimize the standard error of the coefficients to be estimated (see Ferrini and Scarpa, 2007). The fractional factorial design was optimised for d-efficiency for the multinomial logit model using Ngene 1.1.1, and based on information on the signs of the parameters obtained from the piloting (Scarpa and Rose 2008). The design generated 12 choice tasks which were divided into two blocks; each respondent was presented with six choice tasks. Respondents were randomly assigned one of the two blocks in the experiment. The design with zero priors and adding the reference alternative had an ex-ante d-error of 0.04, and 0.003 when evaluated ex-post.

Each choice task was composed of three alternatives including the reference level alternative. DCE surveys usually include a status quo option (or do nothing or opt out) but since a status quo option (households' own current "levels" for each attribute) would vary enormously across respondents, we opted for a counterfactual where protection is totally lifted. Elucidating a status quo alternative also suffered from the problem of requiring respondents to reveal their current participation in *teviala*. The position of the reference level alternative within each choice card was shuffled to motivate respondents to engage in compensatory decision making (i.e. trade-off attributes and levels across alternatives instead of anchoring on the position of the reference option).

APPENDIX 6: ANALYSIS OF DCE RESULTS – LATENT CLASS MODEL (CHAPTER 3)

The choice experiment method has its theoretical basis in Lancaster's model of consumer choice (Lancaster, 1966), and its econometric basis in the random utility theory (McFadden, 1974). Lancaster proposed that consumers derive satisfaction not from goods themselves but from the attributes they provide. According to the random utility theory, the utility of a choice is comprised of a deterministic

component (*V*) and a stochastic component (*ε*), which are modelled to follow a predetermined distribution. According to this framework, an individual *n* has a utility function (*U*) of the alternative *i*:

$$U_{ni} = V_{ni} + \varepsilon_{ni} = \beta_k X_{ni} + \varepsilon_{ni} \quad (1)$$

Where:
β_k is a vector of fixed preference parameters associated with attribute *k* and X_{ni} is a vector of attributes for alternative *i*.

Assuming that the error terms ε_{ni} are independent and identically distributed (IID) and follow a standard Type I extreme-value distribution, and substituting the attributes associated with each alternative into the deterministic portion of utility V_{ni}, the probability P_{ni} that individual *n* chooses alternative *i* from a set of particular set of alternatives *j* from a choice set C can be expressed by the conditional logit model (CLM):

$$\mathrm{P_{ni}} = \frac{\exp(\mu\beta_k X_{ni})}{\sum_{j \in C} \exp(\mu\beta_k X_{nj})} \quad (2)$$

where μ is a scale parameter that is assumed to equal 1.

The conditional logit model assumes the same preference structure across individuals, it may result in biased estimates and incorrect predictions if heterogeneous preferences exist. The random parameters Logit (RPL) model or mixed logit model allows us to take both observed and unobserved potential determinants of preference heterogeneity (variability) into account. While the random parameter models incorporate and account for heterogeneity mostly at the individual level, they are not well-suited to explaining the sources of heterogeneity. The latent class model (LCM) has been successfully used to identify the sources of heterogeneity at the segment (or group) level, i.e. identify a grouping of respondents with similar preferences. The LCM thus typically assumes that preferences are uniform within groupings of individuals, but vary between these groupings. In many cases the sources of heterogeneity between segments relate to the characteristics of individual consumers such as socio-demographics as well as attitudes and perceptions (Boxall & Adamowicz, 2002). Explaining the sources of heterogeneity at the segment level allows a better understanding of the distribution of the welfare impacts of conservation. We estimated LCM on the pooled data sets (from the two study sites, Ampahitra and Mantadia) and tested models including all permutations of socio-demographic variables to explain class membership.

The LCM assumes that the vector β_k is not specific to an individual but to one of the *S* segments, and that individual *n* belongs to segment s (*s* = 1, …*S*). The utility function can now be expressed as:

$$U_{ni|s} = \beta_s X_{ni|s} + \varepsilon_{ni|s}$$

The probability of a respondent *n* choosing alternative *i* from a particular set of alternative *j* conditional on belonging to class *s* then becomes:

$$\mathrm{P_{ni|s}} = \frac{\exp(\mu_s \beta_s' X_i)}{\sum_{j \in C} \exp(\mu_s \beta_s' X_j)} \quad (3)$$

where β_s' and μ_s are now segment-specific utility and scale parameters respectively.

The probability that an individual *n* belongs to segment *s* can be determined by a multinomial logit process in which individual-specific characteristics (Z_n) rather than attribute (X_i) are used to determine choice probabilities. Assuming the error term to be independently distributed across individuals and segments with a Type I extreme value distribution and a scale factor α, this probability takes the form of:

$$\mathrm{P_{n|s}} = \frac{\exp(\alpha Z_n)}{\sum_{s=1}^{S} \exp(\alpha Z_n)} \quad s = 1, \dots, S \tag{4}$$

Given the choice probability and the class probability, the joint probability that a randomly chosen individual n belongs to segment s and chooses an alternative *i* is given by:

$$\mathrm{P_i(n)} = \sum_{s=1}^{S} \left[\frac{\exp(\alpha Z_n)}{\sum_{s=1}^{S} \exp(\alpha Z_n)} \right] \left| \frac{\exp(\mu_s \beta_s' X_i)}{\sum_{j \in C} \exp(\mu_s \beta_s' X_j)} \right| \tag{5}$$

As different scale parameters may exist for different subgroups of respondents (which can be defined by geographical boundaries in our study context), differences in scale factors should always be accounted for when stacking different data sets. We fix one of the scales to one and rescaled one of the data set (Ampahitra) with a relative scale factor using the Swait and Louviere method (1993). Steps to find the relative scale parameters involved stacking two data sets, Ampahitra and Mantadia (i.e., pooled data), and multiplying the Ampahitra data set by a scalar value with varying levels. The purpose is to determine the value of the scalar that optimizes the log-likelihood of the MNL model fitted to the pooled data sets. If the Ampahitra data set has more random noise (higher variance of the error term) than Mantadia's, the scale parameter ratio is less than 1. The log likelihood value of the MNL model was optimized when scalar assumed a value of 0.45 ($\frac{\boldsymbol{\mu 1}}{\boldsymbol{\mu 2}} = \mathbf{0.45}$), implying that the data set from Ampahitra has larger variance that results in more process heterogeneity with respect to choice strategies than respondents in Mantadia. This is probably not surprising given the more heterogeneous feature of the households' immigration status in Ampahitra.

APPENDIX 7: RANDOM PARAMETER MODEL (CHAPTER 5)

We present below the cumulative percentage of the total sample who chose the reference level alternatives. 70% of the WTA sample never chose the reference level alternative (open access scenario) on any choice card, against 24% for the WTP (strict protection scenario). The share of respondents choosing the reference level in at least one choice set in the WTP sample (75%) is almost three times as high as that of the WTA sample (26%). Less than one percent of the total WTA sample exhibited serial reference alternative choices i.e. systematically chose the reference level in all six choice cards against 7% of the total sample in the WTP format.

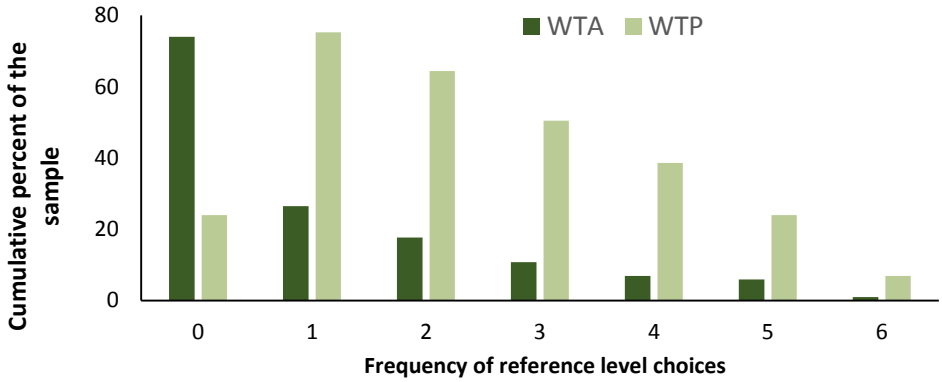

Figure: Reference level alternative choices

As the frequency of reference level choices clearly differs between the WTA and WTP samples (figure 5.4), we decomposed the variance of the ASC (alternative specific constant taking the value of 1 if respondents chose the reference level alternative) by interacting it with respondents' socio-economic and attitudinal characteristics. In particular, serial reference level alternative choices (i.e. the household systematically chose the reference level in all six choice cards) may represent genuine high or low estimates of compensating or equivalent surplus respectively, or budget constraints in the WTP format, however, they may also reflect a refusal to trade off because of ethical belief towards the policy in question, or problematic attitudes towards the survey scenarios. Interacting the ASC with the socio-economic and attitudinal variables would thus indicate what drove the reference level alternative choices.

As a large number of interactions would require a larger sample size, we retained only the candidate explanatory variables that are hypothesised to be most relevant and which result in the best model fit in terms of AIC, log likelihood and McFadden's pseudo R^2. We have, however, tested models including all permutations of socio-demographic variables to allow for preference heterogeneity. The household head literacy which significantly differ between the two samples at 10 % level was also added in the model but was not significant. We used a backwards stepwise approach to iteratively eliminate non-significant explanatory variables Following each iteration, models were compared using AIC, log likelihood and McFadden's pseudo R^2. The RPL estimation results with the interactions are shown in the following table.

The ASC is no longer significant in either format when interacted with other socio-economic and attitudinal variables. In the WTP format, Betsimisaraka households and those who found the state's protection illegitimate were significantly more inclined to choose the reference level alternative of strictly enforced protection. In the WTA sample, none of these variables is significant when interacted with the ASC, only those who exhibited problematic attitudes towards the features of the survey scenarios tended to choose the reference level of open forest frontier.

Table: Random parameters logit model (RPL) results showing the interactions with the ASC. Mean effects show the effects on utility for discrete changes in each attribute for the average respondent away from the same baselines in table 3. Standard deviation parameters show the spread in preferences around this mean effect for each attribute and level change. All parameters are set as random with a normal distribution. Note: *, **, * ➔ Significance at 1%, 5%, 10% level.**

	WTA		WTP	
	Coefficient	**Standard error**	**Coefficient**	**Standard error**
	Random parameters			
Total cash donations (WTA) or payments (WTP)	0.091**	0.037	-1.728***	0.305
Instalment = 10 years	0.778**	0.324	0.392	0.262
Instalment = 20 years	0.248	0.302	-0.183	0.288
Improved rice farming	0.840***	0.165	0.104	0.175
Permit 1ha	1.107**	0.437	-0.380	0.326
Closed forest frontier for WTA or open forest frontier for WTP	-0.433	0.603	-0.900	0.540
ASC (reference level alternative)	3.074	2.296	-1.781	1.293
	Non-random parameters in utility functions			
ASC * Food security	-0.166	0.140	-0.045	0.071
ASC * *Betsimisaraka*	1.005	1.345	.936*	0.498
ASC * Attitude scale	-0.300*	0.122	-0.089	0.068
ASC* Perception of ecological services	-0.216	0.827	-0.003	0.475
ASC * Belief in the legitimacy of state's conservation policy	-0.788	0.711	-1.473**	0.619
	Standard deviation estimates			
Stdev Total cash donations	0.132***	0.042	1.603***	0.254
Stdev Instalment = 10 years	0.531	0.432	0.293	0.661
Stdev Improved rice farming	1.067**	0.497	0.664	0.433
StDev Permit 1ha	0.769***	0.196	0.521***	0.159
Stdev Closed forest frontier for WTA or open forest frontier for WTP	1.746***	0.378	0.604	0.580
Stdev ASC	1.165**	0.567	1.615***	0.450
Log-likelihood	-458.22		-493.28	
McFadden's pseudo R2	.32		.26	
AIC/n	1.56		1.69	
Nobs	612 (N=102)		606 (N=101)	

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APPENDIX 8: CO-AUTHORSHIP STATEMENTS

CO-AUTHORSHIP STATEMENT – CHAPTER 2

All papers/manuscripts with multiple authors which are part of a PhD thesis should contain a co-author statement, stating the PhD student's contribution to the paper

1. PhD student			
Name:	O. Sarobidy Rakotonarivo	UCPH user id:	-
Department:	Department of Food and Resource Economics		

2. Paper/Manuscript *This co-authorship declaration applies to the following:*	
Title:	***1) A systematic review on the reliability and validity of discrete choice experiments in valuing non-market environmental goods***
Authors(s):	Marije Schaafsma, Neal Hockley
Journal:	Journal of Environmental Management
Vol/page:	Status: Accepted
DOI:	

3. Contributions to the paper/manuscript made by the PhD student
What was the role of the PhD student in designing the study?
The PhD student conceived and designed the review in dialogue with co-authors
How did the PhD student participate in data collection and/or development of theory?
The PhD student led and conducted the steps of the systematic review process (development of a protocol to guide the review, screening or inclusion criteria, data extraction, and synthesis)
Which part of the manuscript did the PhD student write or contribute to?
The PhD student wrote the whole manuscript and edited it based on inputs from co-authors
Did the PhD student read and comment on the final manuscript?
The PhD student read, commented and edited the final manuscript

4. Material in the paper from another degree / thesis

Data collected and preliminary work carried out as part of another degree/thesis may be part of the PhD thesis if further research, analysis and writing are carried out as part of the PhD study.

Does the paper contain data material, which has also formed part of a previous degree / thesis (e.g. your master's degree)? [x] **No**

Please indicate which degree/thesis:

Please indicate which specific part(-s) of the paper that has been produced as part of the PhD study:

Signatures

The co-author statement should always be signed by the first author, the corresponding-/senior author and the PhD student. If there are two or three authors the statement must always be signed by them all

Date:	20-04-2016	Name:	O.S. Rakotonarivo	Signature:	
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CO-AUTHORSHIP STATEMENT – CHAPTER 3

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Title:	***2) Qualitative and quantitative evidence on the true local welfare costs of forest conservation in Madagascar: Are discrete choice experiments a valid ex-ante tool?***
Authors(s):	Jette Jacobsen, Helle O. Larsen, , Julia P. G. Jones, Martin R. Nielsen, Bruno S. Ramamonjisoa, Rina Mandimbiniaina, Neal Hockley
Journal:	World Development
Vol/page:	Status: Under review
DOI:	

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The PhD student wrote the whole manuscript, and edited based on inputs from co-authors

Did the PhD student read and comment on the final manuscript?

The PhD student read, commented and edited the final manuscript

4. Material in the paper from another degree / thesis

Data collected and preliminary work carried out as part of another degree/thesis may be part of the PhD thesis if further research, analysis and writing are carried out as part of the PhD study.

Does the paper contain data material, which has also formed part of a previous degree / thesis (e.g. your master's degree)? [x] **NO**

Please indicate which degree/thesis:

Please indicate which specific part(-s) of the paper that has been produced as part of the PhD study:

Signatures

The co-author statement should always be signed by the first author, the corresponding-/senior author and the PhD student. If there are two or three authors the statement must always be signed by them all

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2. Paper/Manuscript

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Title:	***Does more time to deliberate affect respondents' behavior in a discrete choice experiment? Assessing the welfare impacts of forest conservation in Madagascar***
Authors(s):	Jette Jacobsen, James Gibbons, Nilsen Andrianantenaina, Neal Hockley
Journal:	Manuscript submitted for publication
Vol/page:	na
DOI:	

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Did the PhD student read and comment on the final manuscript?

The PhD student read, commented and edited the final manuscript

4. Material in the paper from another degree / thesis

Data collected and preliminary work carried out as part of another degree/thesis may be part of the PhD thesis if further research, analysis and writing are carried out as part of the PhD study.

Does the paper contain data material, which has also formed part of a previous degree / thesis (e.g. your master's degree)? [x] **NO**

Please indicate which degree/thesis:

Please indicate which specific part(-s) of the paper that has been produced as part of the PhD study:

Signatures *The co-author statement should always be signed by the first author, the corresponding-/senior author and the PhD student. If there are two or three authors the statement must always be signed by them all*					
Date:	27-04-2016	Name:	O.S. Rakotonarivo	Signature:	
Date:	30-05-2016	Name:	Jette Jacobsen	Signature:	
Date:	03-06-2016	Name:	Neal Hockley	Signature:	

CO-AUTHORSHIP STATEMENT – CHAPTER 5

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2. Paper/Manuscript *This co-authorship declaration applies to the following:*	
Title:	***Willingness-To-Pay or Willingness-To-Accept? Contested property rights in forest conservation in Madagascar***
Authors(s):	Jette Jacobsen, Mahesh Poudyal, Alexandra Rasoamanana, Neal Hockley
Journal:	Manuscript submitted for publication
Vol/page:	na
DOI:	

3. Contributions to the paper/manuscript made by the PhD student
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How did the PhD student participate in data collection and/or development of theory?
The PhD student collected the data with A. Rasoamanana
Which part of the manuscript did the PhD student write or contribute to?
The PhD student wrote the whole manuscript and edited it based on inputs from co-authors

Did the PhD student read and comment on the final manuscript?
The PhD student read, commented and edited the final manuscript

4. Material in the paper from another degree / thesis

Data collected and preliminary work carried out as part of another degree/thesis may be part of the PhD thesis if further research, analysis and writing are carried out as part of the PhD study.

Does the paper contain data material, which has also formed part of a previous degree / thesis (e.g. your master's degree)? [x] **NO**

Please indicate which degree/thesis:

Please indicate which specific part(-s) of the paper that has been produced as part of the PhD study:

Signatures

The co-author statement should always be signed by the first author, the corresponding-/senior author and the PhD student. If there are two or three authors the statement must always be signed by them all

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Date:	30-05-2016	Name:	Jette Jacobsen	Signature:	
Date:	03-06-2016	Name:	Neal Hockley	Signature: